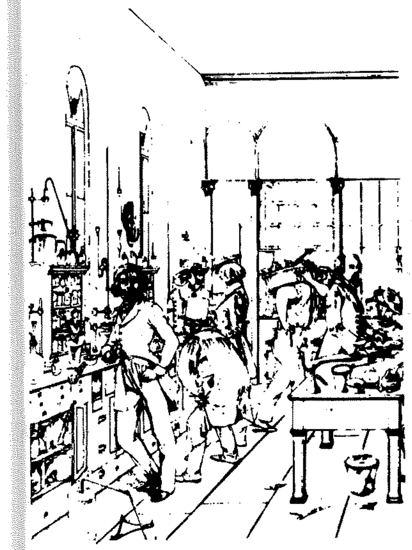
SCIENCE IN CULTURE
PART D

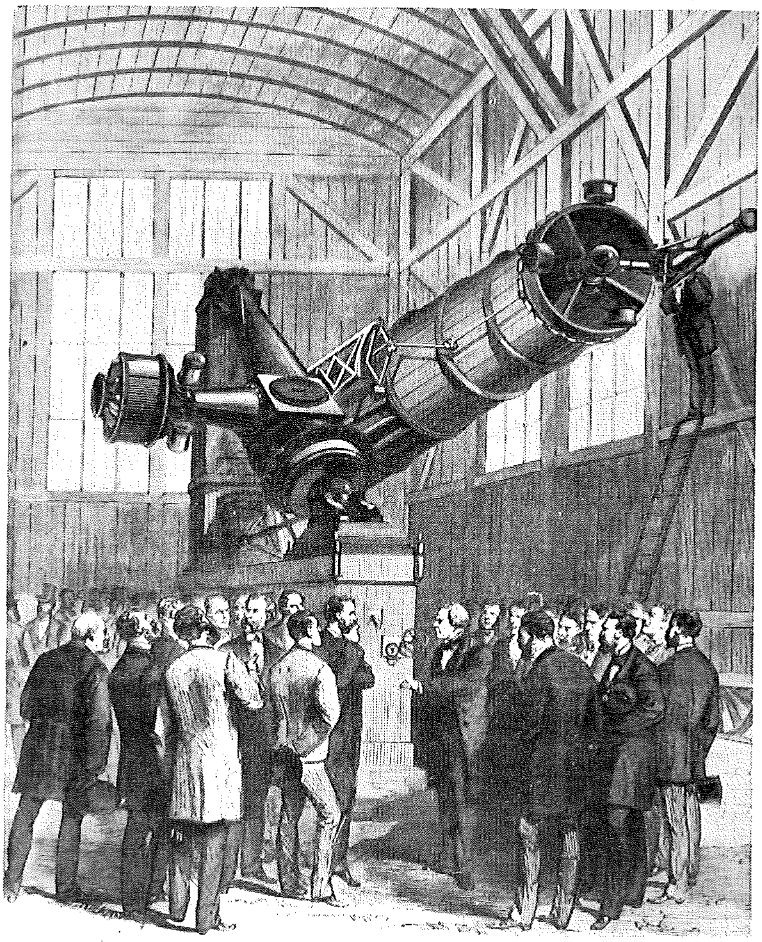
SCIENCE & SOCIETY



DEAKIN UNIVERSITY

SCIENCE & SOCIETY

STUDY GUIDE & READINGS



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The course includes
HUS305 Part A: Medicine & Society
(Study Guide and Readings)

HUS306 Part B: Knowledge Making
(Study Guide and Readings)

HUS307 Part C: Knowledge Using
(Study Guide and Readings)

HUS308 Part D: Science & Society
(Study Guide and Readings)

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STUDY GUIDE

ESSAY QUESTIONS

1

DOING TIME IN MODERN SOCIETY

David Biggins

2

SUPPRESSION IN SCIENCE

Brian Martin

3

HIGH TECHNOLOGY IN AUSTRALIA

4

KNOWING IN CULTURE

Helen Watson

5

AFRICAN IRON TECHNOLOGY

6

THE LESSONS OF LYSENKO

7

BOOK STUDY OF *OUR ORIGINAL AGGRESSION*

ESSAY QUESTIONS

Essay Questions

Doing Time in a Modern Society

A How is the Australian Aborigines' concept of time suited to their way of life, and to their environment.

B Describe and explain the difference in time perspectives between a medieval craftsman and a modern factory worker.

C Our attitude to time contains an important moral component; it reveals what we believe we *should* do with our life. The extracts from Benjamin Franklin put forward one such view of 'morality of time'. Write an essay on your own 'morality of time'. This could include describing your attitude(s) to time, exploring how you acquired these views, and how they affect your life, and assessing the implications of your time morality for other people.

D Write an essay describing the time structure and experience of your work environment.

Suppression in Science

A Looking at a particular suppression case, explain how the different participants develop an understanding of what is going on.

B What sustains hierarchy in science? What forces oppose it? Discuss in relation to suppression in science.

High Technology in Australia

A What potential benefits may be reaped from Australia's high-technology industries? What costs may be involved?

B Why is there so much emphasis on *policy* for high-technology in Australia? Why not simply let the market work?

C Is the Silicon Valley model of high-technology development entirely appropriate for Australia?

Knowing in Culture

A Anthropomorphism implies a certain view of the world. What is it? Logomorphism also enforces a certain view of the world. How would you describe logomorphism? What are its dangers when we come to cross-cultural comparisons of knowledge systems?

B

> The man in the street does not stop to analyse his position in the general scheme of humanity ... If he is an Englishman, he feels himself to be a member of the 'Anglo-Saxon race', the 'genius' of which race has fashioned the English language. (Reference 4.4, p.208.)

Do you agree with the 'Englishman in the street'? Are the 'temperment' of the English, the value system of English society, or metaphysical beliefs of English people related to the types of material objects which speakers of English say are in the world? Summarise Sapir's opinion on the matter. What is your opinion? Defend your ideas about the possibility or otherwise of a such relationship.

C

> It goes without saying that the mere content of language is intimately related to culture ... In the sense that the vocabulary of a language more or less faithfully reflects the culture whose purpose it serves ... But this superficial and extraneous kind of parallelism is of no real interest to the linguist ... The linguistic student should never make the mistake of identifying a language with its dictionary. (Reference 4.4, p.219)

Here Sapir is saying that it is not the biophysical or social features that a language has words for that matters. What matters is the type of object they are construed as in talk. Enlarged upon this difference, giving specific examples to illuminate the difference between 'vocabulary' and 'types of objects referred to'. Why is the type of object which speakers of a language say there is in the world, important?

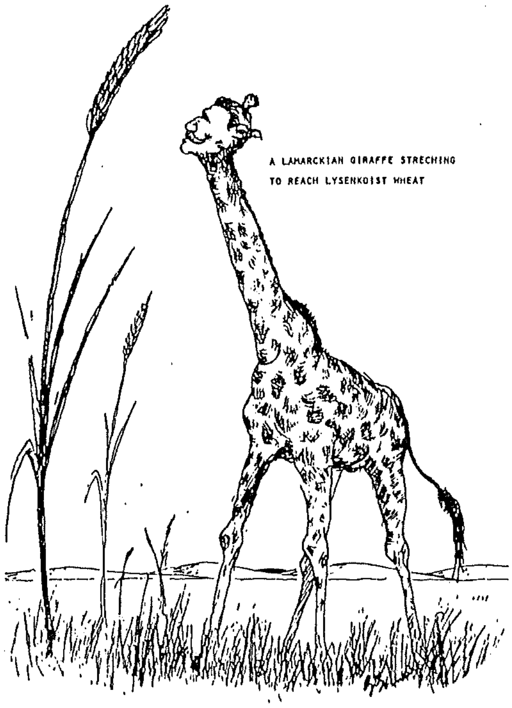
African Iron Technology

A How has archaeological information, published since about 1970, altered perception of the origins and development of iron smelting in Africa?

B Compare and contrast the role and status of iron smelters and blacksmiths in Africa.

The Lessons of Lysenko

A Discuss the role of ideology in science in the light of the struggle for authority which took place in the 1940s-1950s in the general field of heredity, judiciously weighing the various accounts and perspectives that have been considered.



Book Study of '*Our Original Aggression*'

All students must complete Section A and one essay from Section B.

Section A

Write a book review (750-1000 words) of *Our Original Aggression.* The review must give an account of Butlin's main arguments and conclusions. It should also attempt to comment on the quality and significance of his arguments and conclusions.

Section B

A Is relativism an appropiate philosophical framework for analysing the various responses to *Our Original Aggression*?

B What are the strengths and weaknesses of a social contructivist account of the 'struggle for authority' centred on the knowledge claims in *Our Original Aggression*?

1

DOING TIME IN MODERN SOCIETY

Prepared for the course team by David Biggins

Doing Time in Modern Society

What is time? Usually we never bother to think about such a question. Probably, at a semi-conscious level, we tend to accept time as part of the nature of things. This suggests that time is a part of nature—like trees and birds, the sun and the sky. But is it part of nature, or something we have 'put onto' nature to help us make sense of the world?

If we do think about time, we may recall it is a basic scientific category. 'Time'—along with 'matter', 'space', 'motion' and so on—is defined by physics. Deliberations on the nature of time are the business of philosophers who deal especially with the philosophy of science. It's their job to ponder such imponderables, and in their ponderations they pay a great deal of attention to science and the views of the more philosophically inclined practitioners of science. Because time is a *property* of the natural world it seems reasonable to assume that science can tell us just what it is. But can it?

In this unit I want to question those beliefs: that our idea of time is entirely 'natural', and that our science is the best way to understand it. I think we can find evidence that central scientific concepts like 'time', 'space', 'cause' etc. are at least as much social as they are natural. I shall argue that the particular forms such concepts take, and the relative importance they assume in particular sciences and societies, incorporate social values and facilitate certain types of socio-political relations.

Let us then consider the notion of 'time'. In primitive cultures, and in Western culture up to the Middle Ages, the idea of time was directly related to events in everyday human life. Time was *experienced* in the succession of events. For a herdsman, for example, the *passage* of time during the day is primarily the succession of the pastoral tasks of caring for animals. This may be quite detailed: for example, for an African tribe, the Nandi, the day is characterised by events which yield a half-hourly precision: about 5.30 a.m. (our time), oxen go to the grazing ground; 6.00, sheep are unfastened; 6.30, sun has risen; 7.00, it's become warm; 7.30, goats to the grazing ground; etc. In such cultures the *location* of time within the day is also made in terms of events and may be quite precise—for example, E.P. Thompson in his article, 'Time, work-discipline, and industrial capitalism', explains that for monks in Burma the time of rising may be 'when there is light enough to see the veins in the hand' (*Past and Present*, no.38, 1967, p.58). Similarly, the *measurement* of periods of time is conceptualised in terms of events and may be quite precise: e.g. 'a rice cooking', 'the frying of a locust', or, to take a couple of examples with a more medieval flavour, a 'credo', an 'Ave Maria' (when said aloud this provided a measure for cooking an egg), a 'miserere whyle', or 'a pissing whyle' (a somewhat arbitrary measurement).

In feudal societies and in simple farming, hunting, herding or peasant communities all human activities are related to events. Labour is 'task oriented' and work is related to natural work rhythms—both daily and seasonal—of the farming, crofting, fishing or hunting community. 'Natural' work rhythms may extend to secondary industry—for example, charcoal burners sleep beside their fires; ore smelters' activities are similarly regulated by the needs of their furnace. In this situation there is little distinction between 'work' and 'life', and no great sense of conflict between labour and 'passing the time of day'. These are distinctions created during the period of the Industrial Revolution, which did not exist before but which are now deeply embedded in modern social life.

Read Reference 1.1: Evans-Pritchard, 'Time is not a continuum'.

This is a description of the sense of time in the life of the African tribe, the Nuer.

In this extract Evans-Pritchard notes:

'... there are almost as many points of reference between 4 and 6 a.m. as there are for the rest of the day' (p.75) and suggests two possible reasons for this. What are they? What does this suggest about the 'rate' at which time moves? What do you think this would feel like?

To what extent is Nuer time determined by natural events, and to what extent is it determined by social and economic activities? Which of these two is the more fundamentally determining?
Evans-Pritchard later writes:

'Time has not the same value throughout the year' (p.77). What does this mean? What do you imagine such variance would feel like?

The Nuer appear to have only a limited knowledge of their history. Why might this be so?

With the breakdown of agrarian feudal society and the development of modern, industrial, capitalist society, important changes occurred. Three major changes are worth noting. Firstly, there was a shift of most of the population from rural environments saturated with natural cycles, events and the time patterns to *urban*, constructed environments. Secondly, industrialisation brought machines with their own regular, exact, and uniform, *mechanical* pattern of time, and the need for labour to be synchronised with this pattern. Thirdly, the development of *capitalism* created a new social system under which some people sold their labour (which might be measured in time units) to others.

All of these developments—urbanisation, mechanisation and capitalism—are important. But it is the capitalist drive to control all the various factors needed for production, including the labour supply, that is dominant. In this, control of time—by way of exact measurement, regulation and discipline of the labourer's time—came to play a central role.

Under the capitalist system, ownership and/or control of the means of production bestows ownership of the things produced. The labourers no longer own the products of their labour. Also, the division of labour, increasing in scope and intensity in this period, means that the individual no longer produces a complete, meaningful end-product. Work can no longer be seen as meaningfully 'task oriented'.

Under these circumstances the purpose of labour for the labourers cannot be the products of their labour. Rather, what the labourers get is a certain amount of pay, according to the time worked. The product is of no consequence, and the alienation of the labourer from the real product of their labour is inevitable. This separation would be inconceivable in a community of self-employed artisans. For us the important consequence of this separation is that *time spent labouring* and the *activity of labouring* are now two different things, in the same way that 'work' and 'life' are now two different things, and must be conceptually recognised as such. Time spent labouring is no longer the labourer's time.

What labourers now work on is not theirs. Their work is not their life. They don't put their hearts into it. They put in their (employer's) time.

Read Reference 1.2: Marx, annotated by Fischer, 'Some consequences of the division of labour'.

This extract is a description of the effects of the division of labour, mechanisation and capitalist relations of production on the experience of labour.

Try to describe what you imagine the *experience* of working would feel like for a medieval craftsman.

List as many differences as you can between the work situation of a medieval craftsman and a factory worker in a capitalist society.

Discipline of time was rigorously enforced in the new factories by a system of time-keepers, time-sheets, fines and eventually, of course, the practice of clocking in. Time became a battleground. It was common for employers to put the clock forward in the morning and back at night; workers were not allowed to wear watches and were dismissed if they presumed to know too much about the time. The workers countered with struggles to achieve a fair system of time accounting and later with battles over the length of the working day, overtime etc. The historian E.P. Thompson has summed it up thus:

> The first generation of factory workers were taught by their masters the importance of time; the second generation formed their short-time committees in the ten hour movement, the third generation, struck for overtime or time and a half. They had accepted the categories of their employers and learned to fight back with them. They had learned their lesson, that time is money only too well. (Thompson, *Past and Present,* no.38, 1967, p.86.)

Read Reference 1.3: Thompson, 'Time, work-discipline, and industrial capitalism'.

List as many as you can of the ways in which workers in the new factories during the industrial revolution were disciplined using time.

With that nowadays common expression—'time is money'—we come to an aspect of central importance. This was the internalisation of a new concept of time, and the creation of a new attitude to time. Both school and church inculcated this new attitude. Children were taught the value of punctuality, teachers were fined for lateness, schools were a 'spectacle of order and regularity'. The church emphasised the morality of man; pay-day was compared to that 'great and general day of account', the Day of Judgment. The language of 'wasting time', 'saving time', 'using time', 'spending time'—which now seems natural to us—begins here. Moralists exhorted the importance of the careful management of time—probably none more than the Methodists. Wesley expatiated on the need for 'buying up every fleeting moment out of the hands of sin and Satan', and was almost fanatical about the virtues of early rising. He himself rose every day at 4 a.m., and no wonder when you consider the dangers of doing otherwise:

> By soaking... so long between warm sheets, the flesh is as it were parboiled, and becomes soft and flabby. The nerves, in the meantime, are quite unstrung. (Thompson, p.88.)

Benjamin Franklin, a good representative of the new bourgeoisie, summed up the new morality of time thus:

> Since our time is reduced to a standard, and the bullion of the day minted out into hours, the industrious know how to employ every piece of time to a real advantage in their different professions: and he that is prodigal of his hours, is, in effect, a squanderer of money. (B. Franklin, Poor Richard: The Almanacks for the Years 1733-1758, ill. N. Rockwell, Limited Editions Club Edition, Paddington Press, New York, 1976, p.192)

Read Reference 1.4: Franklin, 'A project of moral perfection.'

In Franklin's list of virtues time is explicitly addressed only under 'Industry', but a strong awareness of time permeates many aspects of his scheme for moral education. Read the passage carefully, noting all the ways in which regularity, time discipline, time accounting, management of time etc., are incorporated.

What sort of *concept* of time underlies these attitudes?

How would it feel to live as Franklin suggests? Compare these feelings with those of living in Nuer time.

Read Reference 1.5: Franklin, 'Poor Richard's Time'.

List all the aphorisms dealing with time in the extract. Note the number of occasions in which time is related to money.

What attitudes to time are embodied in the section (p.281) on 'Frugality'?

What is Franklin's concept of leisure? What is your own idea of leisure?

So by all these changes—in the actual environment of most people, in the conditions of their working lives, and in moral inculcation—time came to be conceived of in a different way. Just how profound a change this was is difficult for us to sense because the new notion of time is so thoroughly imbued in us and our way of living. But I think we might recapture some idea of the magnitude of that change if we try to consider what it would be like trying to explain our idea of time to people from the Middle Ages.

We would have to explain that societies would come to exist where virtually everyone walks around with a machine strapped to their wrist which governs nearly all of their everyday activities. What does this machine do? It marks out an entirely abstract sequence, a mathematical system of ... of what? Of non-events. The movements of these machines themselves have no intrinsic importance or meaning. Their significance was solely in terms of the unreal system. But that system is entirely autonomous; it has no real connection with events experienced in the world. This system does *parallel* some events in the experienced world, for example, the rising and setting of the sun—but not really even those; rather it relies upon, and itself supports, idealised abstractions of those events. It has no tangible, necessary connection with the experienced world. It is merely a convenient arrangement, just as the philosopher Descartes had suggested that mental and physical events in the world are not necessarily connected but God had deemed it convenient that they run in parallel.

We would have to explain that almost everyone in this society has one of these machines strapped to them all their waking hours, and even the start and finish of their time awake is announced by these machines, or other versions of the same type of machine. Such machines are placed in conspicuous places in schools, workplaces, all public buildings, and in several rooms of everyone's home. Many versions of these machines incorporate bells, buzzers or other noises, sounding their call entirely according to this sequence and cutting into the flow of real events, enabling these machines to force people to acknowledge the system of which they are custodians. We would have to emphasise that almost everything people do in this society is measured against, even determined by, this entirely unreal system.

Now my feeling is that you simply couldn't explain this, make sense of it, to people from the Middle Ages, sheltering with their sheep from a storm, or snoozing in the shade of a haystack whilst the sheep did likewise as the sun climbed the sky. And the reason why you couldn't is not that they would have been too stupid, nor even that they would find it quite incredible, unbelievable—quite apart from thinking it a crazy way to live. The crux of it is not that they would be unable to *believe* it, but that they would be unable to *understand* it. They could not grasp our concept of time and the sort of life and society that goes with it without experiencing the same social changes and the same socialisation process that we've experienced—which I outlined earlier.

Our concept of time was forged along with the construction of our urban, industrial, capitalist society. This society *needed* an idea of time as something abstracted from nature and experienced events. It needed a concept of time as regular, uniform and exact, and as moving in a linear, unidirectional way.

Now this sort of concept of time is precisely what Newtonian time is. In the Newtonian world-view time is entirely abstracted from real experience and events; it moves independent of these in a regular, uniform and precise way; it is also linear and unidirectional (i.e. non-cyclic). Newtonian time can be accurately measured and accounted for; it can be added, subtracted, divided and multiplied. Any moment of time is equal to and like any other because it bears no necessary relation to experiences or processes in the world. The historian Alexandre Koyre has aptly labelled it 'timeless time'.

Read Reference 1.6: Whitrow, 'Time'.

Included are extracts from the writings of Isaac Newton and his contemporaries, formulating this new concept of time.

What are the main 'properties' of time in this view?

Describe this concept of time as clearly as possible in your own words.

These formulations are by scientists or scientifically minded philosophers, but we can see that this concept of time is profoundly social. It fulfils a real need in a society which is changing in the ways I have described. It is as necessary to the operation of modern society as is modern technology. Our society could not function without it, and we could not function in our society without it.

And, dialectically, the new concept arises from the social, political and material changes that have occurred in society since the Industrial Revolution. Urbanisation reduced contact with, and experience of, natural time patterns. Mechanisation provided human creations, separate from nature, which could serve as models for the idea of abstract, regular, exact time. (These new creations provided the basic metaphorical material not only for the time but for the whole cosmology—the Newtonian 'world machine' or the mechanical world-view.) Capitalism produced psychological and material alienation from work, i.e. it separated people from their interaction with nature and natural time patterns. In short, the new concept of time is a sociohistorical construction—it's a product of the time.

The new time is of *that* society and not of the previous one. The new society is of *that* time, and no longer of the time before.

But this concept is one of the fundamental categories of Newtonian physics, one of the basic concepts of modern science and the scientific world view. 'Time', 'space', 'matter' and 'gravity' are the four basic items from which the whole of the modern scientific explanation of the world is built. From these—and these alone—Newton constructed an immensely powerful account of the workings of nature, a description and explanation of the real world. Surely Newtonian time is a natural category? Yes, but it is also a product of a particular sort of society at a particular time in its history.

What are we to make of this? The lesson is simply that there is no reason to place scientific concepts apart from the rest of human culture. It's more reasonable to assume they are—in some part—social, historical and cultural creations. They may be less obviously so, and the connections between such abstract, fundamental concepts and the nitty gritty of human life may be very complex, but they are most certainly connected nevertheless. I would expect that if we explored other central 'scientific' concepts—such as 'space', 'force' or 'cause'—we could confirm this.

There are, ultimately, no 'correct' natural categories or 'truly scientific' concepts. All explanations of nature are human explanations. But we can *explain* those explanations. This is a very healthy exercise in our present science-dominated culture. It helps to demystify science in a deep way; it helps to free us from our subservience to that view of the world and its possibilities; it awakens us to the role of that world view in the maintenance of contemporary society; it opens our imagination and experience to the possibility of alternatives (and sharpens our awareness of their possible social implications). It is a necessary task in creating a new cosmology needed to replace the existing, restricting one. We need to create fundamentally new concepts as part of creating a new society, in the same way that Newton's society needed to create a new concept of time.

Read Reference 1.7: Mumford, 'The monastery and the clock'.

Summarise the main contributions of the new idea of time in modern society then summarise the main contributions of the new society to the new idea of time.

So far I have argued that our concept of time is a social concept, and dialectically so. It arises from the material, social and intellectual circumstances prevailing, and it assists the further development of a particular sort of society. But does it assist all parts of society equally, or does it favour some groups over others? In other words, is it not only social, but ideological? I think we can begin to see that this view of time does favour some social groups, and their particular interests, over others in a very broad way. (We would, of course, only expect to be able to find very broad relationships, since time is such a fundamental category.)

This sort of view of time is necessary for our idea of progress. The idea that society is progressing requires a particular sort of time—linear, unidirectional and non-repetitive. The modern concept of progress would have been unworkable—indeed inconceivable—within a framework of cyclical, non-progressive time such as formed part of the Aristotelian world-view that was dominant before Newton, or as is common to most 'primitive' cultures. Newtonian time facilitates the development of a progress-oriented society, such as capitalist society, which is centred on private appropriation and increasing accumulation of material wealth. But a relatively small number of people accumulate most of the wealth of this society, while the majority get little in comparison to them, and many people actually suffer as a result of progress. The benefits of 'progress' are very unevenly distributed, but the prevailing ideology suggests that everyone benefits and has an equal chance to improve their situation. The ideology of progress conceals inbuilt inequalities.

Evidence of the role which our concept of time plays in this is provided by the attitude of different social groups to the future. Concern with the future is characteristic of Western, industrial, capitalist societies. Newtonian time is very amenable to a future-oriented society, whereas time experienced in present events is not. Future orientation is central to the psychology of capitalist motivation, vital to the economic rationale of capitalism, and it is given a moral grounding in the Protestant ethic. We are urged to work and forego pleasure now in order to reap our reward later; to save (including saving time) now so that we will have more to spend in the future.

However, in contrast to the capitalist's progressive accumulation of wealth, most members of the working class have little likelihood of accumulating much wealth, little chance of material progress, and thus little to gain by adopting the psychology and morality of future orientation. Of course, they are daily pressured to do so—'to invest in their (sic) future'—but it's generally more realistic to get what enjoyment they can from the present. Most working-class culture is notably more present oriented than the culture of the ruling classes. Moreover, anthropological studies suggest that severely oppressed groups in modern society (e.g. Aborigines in Australia, or ghetto blacks in the USA) don't develop much future-time perspective. It is not only that they do not plan for the future or do not much care about it; they simply do not much conceive of it. They retain a stronger sense of time experienced in the ongoing events and activities of their daily lives. These findings add weight to the contention that Newtonian time is ideological, favouring the interests of some groups in society over others.

As we have seen, people in 'primitive' cultures usually have a very different sense of time from our own. They are often past-oriented, and their sense of history is cyclic, involving the eventual restoration of a prehistoric, timeless paradise. In the contact between such cultures and our own these differences become apparent. Erikson has described the 're-education' of the Sioux Indian into white culture by the US civil service employees responsible for dealing with the Indians. Differences not only in social values, economic means and life-goals, but also in time perspectives were involved:

> In the remnants of the Sioux Indians' identity, the prehistoric past is a powerful psychological reality. The conquered tribe has never ceased to behave as if guided by a life plan consisting of passive resistance to a present which fails to reintegrate the identity remnants of the economic past; and of dreams of restoration in which the future would lead back into the past, time would again become ahistoric, hunting grounds unlimited, and the buffalo supply inexhaustible—a restoration which would permit again the boundlessly centrifugal life of hunting nomads. Their federal educators, on the other hand, preach values with centripetal and localised goals: homestead, fireplace, bank account—all of which receive their meaning from a life plan in which the past is overcome and in which the full measure of fulfillment in the present is sacrificed to an ever-higher standard of living in the future. The road to this future is not outer resoration but inner reform.
> (Erikson, *Identity: Youth and Crisis*, Faber & Faber, London, 1968, p.48.)

Such re-education is, of course, subjugation, and in this process the obliteration by the dominant group of the other's culture is especially powerful. An important aspect of this, here and in many other cases, is the instillation of a new sense of time. This is done by the insistence on adherence to the daily time patterns and regulations of white society, through the imposition of Western religions and the Protestant ethic, by the time-discipline patterns of the white education system, and eventually by assimilation into the workforce of modern society. By these means our view of time is made practical and real. The experience of Western time thus enforced usurps previous time experience. Thus the new concept of time is part of the means used by dominant social groups to enforce their dominance.

Read Reference 1.8: Elkin, 'Aboriginal philosophy'.

This is a description of the sense of time of the Australian Aborigines and how this fits into their general philosophy.

What is 'the Dreaming'?

What is the relationship of 'past' and 'present' in the Aboriginal idea of time?

What is the purpose of performing rituals and re-telling myths for the Aborigines?

Explain the consequence of losing contact with 'the Dreaming'.

Read Reference 1.9: (a) Ball, 'Ernabella keeps up with the times', and (b) Community Advisor, 'Letter'.

Together these extracts provide a description of an attempt to introduce the modern Western concept of time into an Aboriginal community.

Why have the Aboriginal people tended to ignore the Ernabella clock?

Comment on the statement in the Community Advisor's letter that 'aborginal people do not work by time'.

Read Reference 1.10: Whorf, 'The relation of habitual thought and behaviour to language'.

These extracts give a description of the sense of time in Hopi (American) Indian society.

In some contrast to Aboriginal society, Hopi society puts considerable emphasis on the future (e.g. via the idea of 'preparing'). Can you suggest why the two societies might be different in this way?

How is our concept of time antithetical to the Hopi notion of 'preparing'?

How does our concept of time tend to 'routinise' us? What are some effects of this?

Explain how the Hopi attitude to the 'power of thought' favours co-operation and community spirit. Can you think of any instances in our culture which acknowledge the 'power of thought'?

Certain activities are facilitated by the dominant concept of time in our society, others are not, and many are actually hindered by it. Work done in a factory conforms to the necessities of the productive system (the timing of the production line etc.) not those of the worker. Work done within a system of task orientation and experienced time is more comprehensible to the worker: it conforms to the necessities of the task and the requirements in performing it. However, where natural time rhythms or task orientation persists in modern society they are frequently seen as inaccurate or wrong; they are out of time with the established order. Abstract, regular, exact, Newtonian time assists the smooth functioning of *capitalist* society—of those activities in capitalist society necessary for the growth of capital. It does not favour 'unproductive' (non-capital enhancing) activities, such as child rearing, creative writing and music making, not to mention love making and most human relationships. All these and more are distorted by capitalism, and Newtonian time is an agent of this distortion. For us, time conforms to the necessities of the socio-economic system, not the experienced necessities of living. The time sense acquired from working under capitalism comes to condition life activities in general. The time sense of work usurps that of life.

By the mid-nineteenth century English workers were favourably compared to Irish workers for their better punctuality, regularity etc., *and* their repression of the capacity to relax in the old uninhibited ways. Their work had got the better of them. But the subjugation was not complete, as revealed by at least one nineteenth-century moralist's indignation about the way workers spent their few hours of leisure, the last remaining remnants of their own, pre-modern time:

> in what manner ... is this precious time expended by those of no mental cultivation? ... We shall often see them just simply annihilating those portions of time. They will for an hour, or for hours together ... sit on a bench, or lie down on a bank or hillock ... yielded up to utter vacancy and torpor ... or collected in groups by the roadside, in readiness to find in whatever passes there occasions for gross jocularity; practising some impertinence, or uttering some jeering scurrility, at the expense of persons going by ... (Foster, quoted in Thompson, p.90.)

The historian Thompson comments: 'This clearly was worse than Bingo [in Bingo, of course one learns many important aspects of capitalist social relations]: non-productivity, compounded with impertinence. In mature capitalist society all time must be consumed, marketed, put to *use*; it is offensive for the labour force merely to "pass the time"' (Thompson, pp.90-1). Time is no longer 'passed' but 'spent'.

Children can 'pass the time' creatively but once we reach adulthood passing the time of day is considered wasteful. Since nothing is 'achieved' it is assumed nothing is being done. Such an activity is mere inactivity, and it is socially unacceptable. Most adults in our society have almost completely lost the ability to pass the time. But in losing this ability we have lost a way of being in the world. It is a profound loss.

We are prisoners of our time.

Read Reference 1.11: Milne, 'An enchanted place.'

This extract expresses a child's apprehension at the approach of adult time.

Read Reference 1.12: Waters, 'Time'.

This song gives a contemporary description of time in modern society.

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A collection of essays. See in particular R.J. Maxwell, 'Anthropological perspectives', and S. De Grazia, 'Time and work'.

Note: There is a large body of philosophical writings on time. Most of it is very technical and difficult and it is usually related to physics. It does not, in general, deal with the sort of social, cultural and historical issues I have focused on here. A standard source for this approach is:

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2

SUPPRESSION IN SCIENCE

Prepared for the course team by Brian Martin

Suppression in Science

Some people have the idea that science is a holy construction: a body of sacred knowledge which is beyond the petty concerns of humans. But science is not made by angels. Scientists are human with the ordinary defects of mortals, such as seeking power, holding grudges and covering up mistakes. Does doing science make scientists purer than other people? This is what some textbooks might suggest. But most scientists are quite aware of all sorts of nasty behaviours in the practice of science, which often affect the kind of research that is done and the type of theories that are favoured.

In this unit the focus is on one aspect of human behaviour within the scientific community: power struggles which are manifested in the suppression of an individual, a publication or a viewpoint.

Before beginning, consider the following questions:

1. **A scientist submits a paper to a journal, and it is rejected. What is the most likely explanation? What are the *possible* explanations? What would you need to know for an explanation to satisfy you?**
2. **A scientist is sacked from a research job. What is the most likely explanation? What are the *possible* explanations? What would you need to know for an explanation to satisfy you? What difference would it make if you knew the scientist was a women?**
3. **What is your view about how power is exercised between scientists in the scientific community? What do most writings say about this? Are there many writings which say anything about it at all?**

The first reading is a set of source documents about a dramatic conflict in the scientific community involving the exercise of power. The documents are about the dismissal of Dr John Coulter from the Institute of Medical and Veterinary Science in Adelaide in 1980. The documents are newspaper articles and letters to the editor, most of them from the *The Advertiser* (Adelaide).

Read Reference 2.1: Selected articles and letters from *The Advertiser* (Adelaide), *The Canberra Times* and *The National Times*.

Examine Hailstone's second article (Reference 2.1c) and Bonnin's letter (Reference 2.1c) in detail. There are at least five separate reasons given by Bonnin for Coulter's demotion (later to be a dismissal). List all the separate reasons which you think are presented. Do Bonnin's arguments reflect 'proper' scientific criteria?

Next look at the letters in response. There are some specific replies to Bonnin's arguments. List them as a parallel list to your list of Bonnin's reasons. Do Bonnin's arguments stand up? Which respondents have presented the most effective replies? Why are they effective?

Are there also some defences of Coulter which are not just responses to Bonnin's arguments? If so, list these defences.

Look closely at the arguments about obtaining research grants. Bonnin at the end of his letter (Reference 2.1g) says that competent research workers are expected to obtain grants. Ross (Reference 2.1i) says this is an unrealistic expectation. Gouldhurst (Reference 2.1i) says that the lack of support from the management of the IMVS could have jeopardised Coulter's chance of obtaining a grant. What are the assumptions about the workings of the scientific community that underlie each of these views?

Pick at least one other argument by Bonnin, along with the counter-arguments to it by respondents, and list the assumptions about the workings of the scientific community that underlie each of the views presented. How do the arguments stand up?

Contrast the types of arguments presented by:
Bonnin (References 2.1c, 2.1g);
Coulter (References 2.1b, 2.1m, 2.1n);
doctors (References 2.1f, 2.1i, 2.1h, 2.1j, 2.1k, 2.1l);
trade unionists (References 2.1d, 2.1n).
How do the different arguments relate to the personal or occupational interests of those presenting them?

Only some people are concerned enough to write letters to the newspaper. Why do you think individuals wrote letters supporting Coulter? Only some letters are published. Why do you think there are no letters from anyone besides Bonnin representing the IMVS?

Do the supporters of Coulter disagree only about the 'evidence' presented by Bonnin or also about the criteria for dismissal?

Do you think the treatment of Coulter was 'fair'? Why or why not? What would an 'impartial' observer need to know to decide? Are the relevant issues only scientific? What *are* the relevant issues?

Assume that Bonnin and other administrators at the IMVS are completely sincere and well-intentioned. Does this rule out any explanations of the Coulter dismissal? What is the relationship between the individual psychology and the actions of the key IMVS decision-makers?

Consider individuals and groups such as Bonnin, Coulter, the IMVS executive, drug companies, consumers, workers and taxpayers. Which individuals and groups benefitted from Coulter's dismissal? Which ones lost out?

There is very little writing about internal power structures in the scientific community. Most studies simply assume that what exists is functional. A usual assumption is that the power of elite scientists is based on their contributions to knowledge, and that their prestige is due to these contributions.

The sociology of science tends to focus on scientific *knowledge*. When 'values in science' are discussed, this refers to values inherent in the choice of scientific research, scientific concepts and how they are organised, and the use of results. There is little attention to values which are built into the structure of the scientific community. These values can include assumptions about hierarchy, about patterns of communication, about who allocates money, and about who makes decisions about appointments, new departments or new research facilities.

Do you agree with these comments about science and how it is studied?

What have you read that supports or opposes them?

There are many ways to conceptualise the scientific community.
Some possible ways are as follows.

1 *Collegial model.* Scientists treat each other as equals because they are essentially equal in the market-place of ideas. Ideas are scrutinised and taken up because of the usefulness or otherwise of the ideas, not because of who promotes them or opposes them. Scientific research is essentially a co-operative endeavour.

2 *Individualistic model.* There is a hierarchy in the scientific community based on merit in research. Those who develop the best ideas are rewarded appropriately. Ideas are examined independently of who develops them, but those higher in the hierarchy have more say over the direction of research funding and about who is hired. The driving force behind individual achievement is competition and the reward structure. The scientific community is essentially a meritocracy.

3 *Professional model.* The opportunity to do scientific research is restricted to those who have passed the courses and otherwise done what is necessary to be accepted. Potential contributions to knowledge are only considered from members of the profession, and this is usually restricted to the relevant speciality. Control within the scientific community is exercised collectively by professional norms.

4 *Feudal model.* The scientific elite exercises most of the control within the scientific community. Apprentices must find a sponsor, and be initiated into their sponsor's school of thought. The members of the elite run little fiefdoms, controlling appointments, promotions, research opportunities and chances to publish research. The members of the elite know best, and operate like a set of independent and benevolent autocrats.

5 *Bureaucratic model.* The scientific community is structured bureaucratically, with a strong hierarchy and division of labour. Key decisions are made at the top, which may be by the scientific elite or by bureaucratic elites in a corporation or government department. Contributions to knowledge are assessed according to how they serve the interests of the bureaucracy.

Analyse Dr Coulter's dismissal from the point of view of each of these models. Which model makes the most sense?

Are there any other models to explain the behaviour of the scientific community? Which models are most flattering to scientists? Which models are most threatening to the status of scientists?

Can the scientific community be explained by a single model? Is the 'community' really a community in the usual sense? Do different groups of scientists react the same way to different social environments? How can the whole operation be understood coherently?

To be able to understand conflicts such as the Coulter-IMVS case, a picture of the operation of the scientific community is needed. One picture is as follows.

Firstly, within the scientific community, there is an elite group which exercises much more power than most other scientists. These scientific power elites dominate decision making about research priorities, allocation of funds, editorial policies of scientific journals, and appointments and promotions of scientists. Secondly, the scientific power elite has ties to power elites outside science, especially in industry and government. Scientific elites in the areas of geology and geophysics, for example, are likely to have research and career interests tied to corporate elites in the mining industry and in the government bureaucracies which deal with minerals. Thirdly, scientists who threaten the interests of the scientific elites or related elites in industry or government—for example by speaking out in a critical manner about the research that is being done—may be attacked to shut them up or to discredit them. This can be called 'suppression of intellectual dissent'. This perspective is presented in the next reading.

Read Reference 2.2: Martin, 'The scientific straightjacket'.

There are several strands which you can look for:

1 Evidence about the extent of suppression. The Coulter-IMVS case is only one of ten cases (pp.34-5) presented from Australia and New Zealand. The editors of *The Ecologist* have added some US cases on p.39.
2 A discussion of the scientific power elite and its relation to other elite groups (pp.35-6), with the specific example of the forest industry (pp.36-7).
3 Application of the scientific power elite perspective to explain the phenomenon of suppression (pp.38-41).
4 An outline of challenges to the scientific power structure (pp.41-2).

How is the Coulter-IMVS case similar to and different from the other cases outlined in the reading?

What are the characteristics of 'suppression' as presented in the reading? How can these cases be distinguished from the taking of 'proper' measures against incompetent work or people?

To use a term such as 'suppression' is to introduce value judgments, such as the implication that it is illegitimate. What values are incorporated into the concept of 'suppression of intellectual dissent'? Who sets the standard from which 'dissent' is made?

How can the 'cognitive scientific elite' and the 'political scientific elite' be distinguished? Is this a useful distinction? Are there other types of scientific elites?

Does the idea of a 'political scientific elite' really explain the phenomenon of 'suppression'?

Should suppression be considered a normal part of science, occasionally involving an injustice that should be tolerated?

Is there a contradiction between the usual notion that the ideas of scientists should be judged on their merits, and attempts to prevent public comments by scientists? Do the 'norms of science' only apply to 'pure research'?

Is it possible for 'science' to be done without professionals, or without hierarchy? To what degree do scientific specialisation and hierarchy reflect the bureaucratisation of modern industrial society? How much of modern science would survive a switch to science done 'by the people'?

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3

HIGH TECHNOLOGY IN AUSTRALIA

Prepared for the course team by Stuart Macdonald

High Technology in Australia

What is High Technology?

Australia lacks any consensus on the definition of 'high technology'. At the same time, there is a profusion of high-technology policy at Federal, State and even lower levels of government. Policy for what then? Like beauty, Australian high technology is largely in the mind of the beholder. There is as little high technology as would justify prescriptions to encourage high technology, and as much as would prove the success of high-technology policy.

Part of the reason why there is no commonly accepted definition of high technology is because the term is in everyday use and everybody knows instinctively what it is. But instincts vary, and so do definitions. The Australian Department of Science and Technology—which has also been known to rely upon its instincts—considered high-technology industries to be those which indulge in generous research and development (R&D) spending, in which there is interaction between scientific and technical skills that generate new products and processes, and in which there are entrepreneurs with scientific or technological backgrounds. Such indicators catch the flavour of high technology by concentrating on major ingredients, but the recipe remains elusive. When reduced from the theory to the practice of a standard industrial classification, these ingredients yield decidedly unsatisfying fare. For instance, they virtually ignore the whole of the tertiary sector because its investigative activity is scarcely acknowledged to be R&D in Australia. Moreover, even within the manufacturing sector such an approach can produce totally conflicting results. In the USA, where there is now no shortage of lists of these industries, one major study includes rubber and oil among these industries. Another categorically denies the inclusion of these specific industries and views high-technology entrepreneurs as a breed apart from the entrepreneurs of traditional industries.

While such lists fail utterly to be definitive, they do at least discourage more fanciful definitions. Among the several of these with some currency in Australia at the moment are those which emphasise the close affinity of high technology with science and scientific research classifying industries by the purity of their research would pose some fascinating problems.

There is an alternative approach, though still not one which yields a definitive taxonomy of high-technology industries. High technology is high not because it is nearer to God than ordinary technology, but because it involves high risk and possibly high return, a high rate of change, and—especially—high information intensity. That definition is obviously inadequate—it could apply quite nicely to many sophisticated criminal activities—but it does have the merit of stressing the utter dependence of high-technology industry on its ability to handle vast quantities of new information. Such a notion is quite foreign to most definitions of high technology, except in as much as they are preoccupied with what may be a very restricted information activity—research.

This fundamental characteristic is not even considered relevant to much of the high-technology policy currently prevalent in Australia. Yet, the distinctive use of information is the common factor among high-technology firms and this factor is what makes them different from the mass of firms engaged in most other industrial activity. Certainly they have little else in common and to speak of high-technology industry as if it were a single entity is not always helpful. For this reason there may, in fact, be little profit in seeking a single, comprehensive definition of high-technology industry. The industry comprises tiny firms and huge transnationals exploiting different technologies for different purposes, and often in conjunction with established technologies; it can emerge in any part of the spectrum of existing industrial activity and the characteristics of its individual firms may change rapidly. In practice, high-technology firms have few characteristics in common except high information intensity, and that is difficult to measure rigorously, in any case is not exclusive to high technology.

Benefits and Costs of High Technology

High technology—whatever it is—is a rare and precious economic activity in that it is widely assumed to deliver prodigious benefits while imposing almost no costs at all. Unbounded political enthusiasm for high technology is perhaps surprising: political fingers were just about all that was even singed by the white heat of the technological revolution, and it seems only yesterday that technology was firmly cast in the role of despoiler of the workforce. The easy benefits claimed for high technology have proved irresistible, as have the apparently low costs. In fact, the benefits to be gained from high technology have probably been grossly overestimated, and the costs—especially the indirect and hidden costs—may actually be quite considerable.

Employment

Most prominent by far among the benefits claimed for high technology is its apparent ability to create employment in the depths of prolonged recession. In reality, high-technology industry is not a large employer now and there is no prospect of it becoming one in any future that is even dimly foreseeable. Real job growth has been in the tertiary sector generally, and especially in some of the less salubrious service functions. US employment in the fast-food industry is greater than that in high technology and it has grown faster both proportionally and absolutely—all without a hint of policy to encourage the consumption of hamburgers. Moreover, it is in such unglamorous sectors that most future employment growth is forecast. In Queensland, the agriculture, mining and manufacturing sectors combined have been responsible for only five per cent of new jobs over the last ten years.

Of course, it could be argued—and is argued—that low-grade service functions create jobs mainly for the semi-skilled and unskilled, and do not make full use of human capital. Unfortunately, the same is true of much high technology employment. Highly qualified, professional employees may be more common in high-technology than in other industries, but the typical employee in the USA is poorly paid, part-time and female, with the daily prospect of tedious, repetitive work, with no union protection and no career structure.

The products of high-technology industry may, of course, help generate employment in other industries. They may equally well reduce that employment by allowing what has become known as 'jobless growth'. It all depends on how the technology is used and on the facility with which organisations adapt to change.

Read Reference 3.1: Anderson, 'New technology will not provide jobs'.

Read Reference 3.2: Tomaskovic-Devey & Miller, 'Can high-tech provide the jobs?'.

Read Reference 3.3: Riche, Hecker & Burgan, 'High technology today and tomorrow'.

Wealth generation and growth

High technology industry has certainly grown rapidly, but from a small base. While it is incontrovertible that some high-technology firms have flourished, and have made fortunes for their owners, this has not been typical. Many high-technology firms fail, most just linger, and only a very few have instant success. The few, however, are often presented in Australia as if their fate were inevitable, as if a major share of the international market is always assured, as if the generation of spin-off firms somehow defies the commercial laws of natural selection. This is most misleading. Moreover, the wealth that is undoubtedly created in some quantity by these few is likely to be unevenly distributed.

Environmental and health advantages

Unlike the smokestack industries with which they are commonly compared, high-technology industries are supposed to be clean and healthy. In fact, chemical pollution is now a serious problem in the Mecca of high technology, Silicon Valley. So too are all those problems traditionally associated with smokestack industries—overcrowding, traffic congestion, smog, poverty and even hunger. It is quite wrong to imagine that thriving high technology is kind to the environment and gentle with humanity.

Read Reference 3.4: Rogers & Larsen, 'Problems in paradise'.

Restructuring of the economy

Of all the proclaimed benefits of high technology, its role in the restructuring of developed economies is certainly the most important and probably the least contentious. The primary and secondary sectors have long ceased to produce enough wealth, or the right sort of wealth, to satisfy the requirements of modern society. The tertiary sector—the service sector—has grown prodigiously and now dwarfs the other two. Its function is not only to organise the production of wealth, but also to produce its own, though in an intangible form not always familiar as wealth. Much of this wealth, and a good deal of the wealth produced within the secondary sector and even the primary sector, is in the form of information.

The diffusion of information-technology hardware is only an indicator of the information revolution that is taking place; the force of the revolution is information itself. Information workers comprise between a third and a half of the workforce in developed countries. Government policy, however, seems to be crippled by an attitude which still regards the manufacturing sector as the only possible engine of growth, which recognises wealth only in a tangible form, and which values information as just another input to the production of real wealth.

Government high-technology policy in Australia is no exception. High-technology industry uses information resources that many other industries have either neglected or used inefficiently. Other parts of the economy—perhaps all other parts—must acquire the same facility, and must undergo the reassessment and reorganisation resulting from the new utilisation of a major resource. High-technology industry, by example of its own growth, and by the diffusion of its products through an economy, can play an essential leadership role in what is proving to be a difficult and painful restructuring process.

Neglect of comparative advantage

Governments have simply assumed that their constituencies have an obvious comparative advantage in high technology and—attracted by its assumed benefits—are anxious to direct resources towards that activity. Too often the information requirements of high-technology industry are either ignored in policy, or are not afforded the central position they deserve. Too often those responsible for policy have only a vague idea of what high technology is. High-technology industry is seen to be whatever fills Silicon Valley and so the policy task becomes the replication of Silicon Valley in all sorts of unlikely spots. It is, of course, possible to mimic the obvious characteristics of the Valley elsewhere; to construct what are really no more than Hollywood film sets of Silicon Valley. Those characteristics can hardly have been missed by the dozens of policy makers from Australia who have been conducted through the Valley, though its less desirable attributes do seem to have been overlooked.

What are also missed, because they are intangible and no one is really looking for them anyway, are the intricate networks of surging information channels which supply high-technology industry with its basic requirement. Key personnel in high technology know other industry experts and mobility is high. For such people, work frequently substitutes for religion, and success for heaven. Success is attained through a vicious dedication to acquiring and using information. In Australia, such matters are not considered to contribute to comparative advantage in high technology and are not thought relevant to high-technology policy.

It is axiomatic that the many regional claims of comparative advantage for high-technology industry cannot all be correct, and to the extent that they are wrong, costs are likely to be incurred. Indeed, it is unlikely that Australia offers any overall comparative advantage to many high-technology firms, and incentives should be seen for what they often are—government compensation for lack of comparative advantage. Beyond the direct—and probably fairly moderate—cost of such incentives, there are likely to be further and greater costs associated both with high-technology industries encouraged to locate in the wrong place, and with the neglect of less glamorous industries.

Read Reference 3.5: Industries Assistance Commission, 'Encouraging industries with growth prospects'.

Inappropriate policy

There are costs associated with ill-conceived policy, and probably particularly with the general assumption that high technology is a single industry with uniform requirements. Federal policy to encourage high-technology firms may be frustrated by State policy to attract firms to areas of, for example, high unemployment, however inappropriate the location. Similarly, there are obvious costs when one region or one city competes for high-technology industry by out-bidding the incentives of others. That is parish-pump mercantilism, and literally a policy for high technology at any cost. In Australia, there are even competing policies within governments as individual departments seek to exploit their own high-technology policies to further their cause in internecine power struggles.

Read Reference 3.6: Tempest, 'Texas pays high price for high-tech'.

Read Reference 3.7: Office of Technology Assessment, 'Analysis of census results'.

Market distortion

Government incentives for high technology are subsidies and—much like tariff protection—help to shelter that industry from competition. The spectre of highly protected high technology looms large in Australia and that sort of industry would undoubtedly impose severe costs on the rest of the economy. Not only would high-technology purchases be more expensive, but high technology's leadership role in restructuring would be substantially weakened, and a new distortion in the market would deprive alternative activities of resources that they would otherwise have attracted.

To the extent that policy distorts what is already a highly imperfect high-technology market, it makes more confusing the market signals which influence business decisions. Similarly, the rampant speculation in high-technology shares now occurring in Australia is not unlike the oil and mineral boom of the early seventies and is not altogether a cause for celebration; it confuses the market and makes investment hazardous.

Read Reference 3.8: *The Economist*, 'Wall Street's tiff with high tech'.

Requirements of High-Technology Industry

There are now a number of studies—most of them very recent—which have sought to determine which locational factors are most conducive to high-technology industry: it would seem that a variety of factors are important and that there is little consistency in the variety. For some firms, the availability of venture capital may be crucial; for others, government procurement will be essential; and for yet others, a competent supply industry is a prerequisite. Consequently, any high-technology policy based on the simple assumption that there are just two or three universal prerequisites for all high-technology firms—say, a convenient airport, a nearby university and pleasant surroundings—should be treated with considerable suspicion.

Read Reference 3.9: *The Economist*, 'Planting science parks in Britain',

If only high-technology industries had conventional locational requirements, if only they would demand nearby markets and a local labour supply and proximity to raw materials, then conventional industry policy would be appropriate. But the factors that conceived and nurtured smokestack industry will not bring forth high-technology industry; on that agreement is unanimous. Concomitantly, conditions may be thought to be conducive to high technology simply because they are the very opposite of those required by smokestack industry—the green and pleasant high technology land distant from the dark Satanic mills. Most fascinating of all is the common assumption that, because conventional location criteria are irrelevant, high-technology is a footloose and fancy-free sort of activity that can be made to thrive anywhere under any conditions, if only there is sufficient political will, if only the policy is right.

What is so commonly forgotten is that high-technology industry's main requirement is information, and that it can thrive only where vast quantities of appropriate information are readily available. Much of this information is not technical at all, but the commercial, production and marketing information necessary to effect successful innovation. While high technology is commonly seen as a scientific activity dependent on basic research, high-technology industry is really almost non-scientific in its desperation to avoid undirected inquiry. It is a 'black box' activity; the object is to make the box work for someone without ever having to open it. The means by which information flows—the information channels—are crucial to all high-technology industries. Of all the ways in which information can flow, personal and informal contact is by far the most useful for high-technology. Though formal channels, such as licensing and journal articles, supplement the informal, only information 'on the hoof' provides the industry with the comprehensive, specific and immediate information it requires. That is why so many surveys of the requirements of high-technology industries reveal the vital importance of qualified personnel, though this is often interpreted by those responsible for policy as an indication of the need for more graduates or better workforce planning. They miss the point altogether; what is in demand is not anyone with good qualifications, but specific information that is embodied in key individuals. High-technology policy must permit these prima donnas to be treated as very special people. Pleasant parkland is unlikely to provide sufficient inducement; stock options and the lures of headhunters might.

Read Reference 3.10: Macdonald, 'The need to succeed'.

High-Technology Policy and Information

There are just two basic issues which should be considered by those who would shape policy for high-technology industry. The second is whether the policy will satisfy the information requirements of the industry. The first is whether there should *be* any policy at all. High-technology industry has a strong entrepreneurial tradition in its heartland, the USA, and it could be argued that government interference is more likely to stifle than promote individual vigour. Moreover, governments are not renowned for the intimacy of their relationships with small new firms rather than with corporate giants, they are not good at picking technological winners, they are not at ease with great uncertainty, and they can move frustratingly slowly when speed is of the essence. Even so, it is naive to think that there is any area of economic activity in a mixed economy independent of government influence. The only way to have no high-technology policy would be to have a policy to have no high-technology policy.

Read Reference 3.11: OECD, 'Technology policies and programs in Australia'.

Infrastructure for High Technology in Australia

Australia's reputation overseas as a land of sturdy individualism, a last frontier where the entrepreneur may thrive, a place sufficiently developed to exploit massive undeveloped resources, is largely undeserved and thoroughly misleading. Odd attitudes are nurtured and harboured here, strange situations have developed through isolation and have become acceptable. This distinctive culture is not entirely conducive to the development of high-technology industry.

Perhaps the main impediment to high-technology growth in Australia is the distortion in the rest of the economy created by extensive government involvement. Traditional industries in the manufacturing sector are heavily protected, and the attitude that it is the responsibility of the government to ensure industrial prosperity is widespread. High-technology industry has not been immune from the contagion and has argued for assistance on the grounds of government support for high technology overseas, the importance of high technology for Australia's defence, the need for a high-technology infrastructure for the rest of the economy; it has even used traditional 'infant industry' arguments. Much less evident is the notion that high-technology industry in Australia should be based on some comparative advantage and should aim to be internationally competitive. That sort of attitude has helped create an interesting breed of Australian entrepreneur, one which—quite naturally—is as dedicated to exploiting the opportunities presented by government as those presented by the market.

The attitude that the government should be responsible for whatever has to be done extends even to research and development (R&D). In Australia, the government not only pays for nearly eighty per cent of all R&D, it actually performs that proportion. The measured R&D performed by the private sector is tiny and has actually been contracting in recent years despite, or perhaps because of, increased government incentives. CSIRO alone now performs more R&D than all industries in the manufacturing sector combined. Despite frequent exhortation to undertake more research, the private sector remains stolidly unenthusiastic.

Supporting, perhaps explaining, this unusual Australian research structure is a widespread faith in the validity of the linear model of science policy. The innovative process is imagined to start with invention and to proceed in an orderly sort of way to innovation; basic research is followed by applied research and then by development and marketing; science produces technology. Such a model may be convenient, but it is not realistic. The main consequence of tacit, and sometimes abject, adherence to the linear model is over-concentration on the earlier parts of the innovative process in the belief that once the ball is set rolling its own momentum will carry it some distance. Much less interest is shown in the later stages of the innovative process.

The dynamic nature of high-technology industry demands mobility of resources. Australia lacks that mobility, especially in the most essential high-technology resource—personnel. Over three-quarters of the nation's R&D personnel are accustomed to high status, superannuated job security and to respectable, slowly evolving research programs. This is not the obvious raw material for risky, cut-throat and volatile high-technology industry. There is little evidence in Australia of the personnel mobility—of headhunting, for example—vital in high-technology industry for the transfer of information 'on the hoof'. Consequently, information remains isolated: it is quite clear from extensive surveys of the private sector that no part of government or university research is regarded as an important source of technological information. There is no concentration of high-technology industry in Australia to make mobility easier, it is difficult to transfer superannuation entitlements, and employee stock options—which are essential elsewhere in attracting talent to new firms—are hardly thought relevant to high-technology policy in this country.

The complacency of the Australian private sector was well matched by the almost inert interest in high technology shown by the conservative Fraser government between 1975 and 1983. Those years saw a succession of Industry ministers dedicated mainly to preserving the established structure, and of Science and Technology ministers most notable for their ignorance of scientific or technological matters. Not surprisingly, the importance of information to high technology required a conceptual leap entirely beyond that government. Only the threat of defeat in the elections of March 1983 prompted the Fraser government to take a sudden, belated, interest in high technology—in January. By that time the Labor opposition had exploited for more than a year the considerable electoral capital in high technology as a panacea for all manner of economic maladies. To Australians, discontented with a government which persisted in its conviction that high unemployment was a remedy for high inflation, high technology was an attractive alternative.

The Labor incumbent to the Science and Technology portfolio was Barry Jones, a knowledgeable and passionate advocate of high technology. The new Minister seized every opportunity to bring to public attention the need for high-technology industry in Australia, the importance of recognising the transition to an information economy and the fundamental inability of the existing national infrastructure to support high-technology industry.

Initially, the general economic strategy of the Labor government was well able to accommodate the enthusiasm with which Jones threw himself into his task. The stagnant economic policy of the previous conservative government had offended the electorate and radical alternatives were welcomed. But change always threatens established interests and is particularly threatening when it involves the re-allocation of resources. Neither existing manufacturing industry, heavily dependent on assistance from Canberra, nor the scientific establishment, reliant on government for nearly all its funding, shared the Minister's conviction that urgent and drastic action was required. Pressure mounted for some tempering of the Minister's immoderate and impolitic stance. In late 1984, the Technology portfolio was taken from Jones and given to the Minister for Industry and Commerce, a clever shuffling which put both Jones and Technology in their places. High-technology matters are now the concern of a department which has previously shown only the slightest interest in any sort of technology, and high technology's champion retains responsibility for only Science, an area which has been denied the patronage to which it feels traditionally entitled.

Read Reference 3.12: Jones, 'Sunrise industries: Leading the revolution'.

Jones' contribution to Australian high-technology policy has been fundamental—literally in the sense that he attacked the existing scientific and industrial structure of the country. He went so far as to identify sixteen specific 'sunrise' industries and confessed that government targeting of resources was both necessary and desirable. That was not popular among those unlikely to be targeted, nor probably with those public servants left wriggling with the argument that in choosing sunrise industries the government was merely finding 'key industries' to fortify Australia's technological infrastructure, and was not actually picking winners. In order to use resources more efficiently, Jones directed that portions of some existing budgets be diverted to high technology. That was not popular among those with existing programs and accustomed to greater independence in resource allocation. He even went so far as to criticise the effectiveness of CSIRO research in a bold attempt to revitalise that sacrosanct institution. He recognised some of the main characteristics of high-technology industry, particularly its information intensity, and was consequently forced to acknowledge that the existing Australian infrastructure was quite unsuited to such an activity. The high public profile he gave high technology was his attempt to gain acceptance that the basic infrastructure required change. That, of course, was not allowed to happen, though the scheme he implemented to establish management investment companies, to provide venture capital for high-technology firms, still remains. Ironically, the public awareness of high-technology aroused by Jones to challenge the existing infrastructure has helped generate a profusion of state-based, high-technology programs so superficial that they pose no threat at all to that infrastructure. Similarly, his declared belief that in the area of high-technology industry comparative advantage is not bestowed, but rather created seems to have fuelled the assumption that comparative advantage can be created instantly through high-technology policy.

Read Reference 3.13: Macdonald, 'Review of *Developing High Technology Enterprises for Australia (Espie Report)*'.

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4

KNOWING IN CULTURE

Prepared for the course team by Helen Watson

Knowing in Culture

> It has often been said that man in the past saw nature, and God, in his own image. It now also appears that he saw things in the image of his own language. So the overcoming of logomorphism supplements the overcoming of anthropomorphism. (E. Gellner, *Words and Things*, Victor, Gollancz, London, 1959, p.27.)

In this section we are seeking a better understanding of the ways in which we come to know about the world. Talking is our way of communicating knowledge about the world. Thus analysis of language and the ways people use it will be a useful tool in our endeavour. In this unit we will be looking at how people talk of things in the world. Perhaps through understanding the ways people construct their talk of the world we shall come to a deeper understanding of how they structure their knowledge of the world.

Language analysis is not a popular methodology with those who seek to compare the different ways in which people know about the world, and it is useful to ask why this is so. It seems to me that there are two reasons. First, since the method is concerned with the structural details of statements, it must necessarily be a painstaking examination of syntax and the grammatical functions of the various elements of statements. All too often attempts to detail the constructural system of talk become linguistic exercises in which investigators lose sight of the forest whilst amongst the trees. (See M. Mathoit's book *Ethnolinguistics, Boas, Sapir and Whorf Revisited,* for an account of the pitfalls of using language analysis to provide empirical data for cross-cultural comparisons of knowledge systems.) Readers for whom the primary question is 'how and why do systems of knowing about the world differ?', find it tedious to be given details of syntax and grammar. Consequently, the significance of such differences usually passes them by. In introducing you to the ideas and techniques of language analysis I am aware that I too am in grave danger of losing your interest as soon as I start talking of nouns, verbs, predication and reference.

The second reason for unpopularity of language analysis has, to my mind, more serious implications. It relates to tenacious and pervasive assumptions about language—assumptions that are often unexamined by those anthropologists and philosophers who work in the field of cross-cultural comparisons of knowledge systems. The assumptions centre around our natural inclination to believe that the world is the way we say it is, or in other words, that our talk of the world mirrors the reality of the world. It is this assumption that Gellner characterises as 'logomorphism'. He recognises the fetters that this perspective imposes on our understanding of how we know about the world, and he compares this 'blind spot' to that other pervasive 'morphism', anthropomorphism. Of course our belief that we are talking about the world as it really is, is soundly practical from the utilitarian point of view of living in the world. But it does not serve as a sound beginning for comparative studies, where greater detachment is needed. Nevertheless, most comparisons of knowledge systems are underwritten by following a string of presuppositions: the world is the way we say it is; we all live in the same objective world of material things; thus the physical things we all talk about as being in the world must be the same type of things. (I am thinking here of articles like those of Horton, 'African traditional thought and western science' and 'Tradition and modernity revisted', Hallpike's *The Foundations of Primitive Thought*, and articles in *Universals of Human Thought, Some African Evidence* [edited by Lloyd & Gay], to mention just a few.) As a technique for comparing knowledge systems, language analysis specifically rejects these presuppositions. It attempts detachment from our routine ways of perceiving and interpreting the information we receive from stimulation by the outside world, and report with our ordinary talk of the world. But the pervasiveness of the assumption that the world is the way we say it is, often makes this detachment hard to achieve.

Gregory Bateson is a well known anthropologist and a founder of cybernetics who died in 1980. His book *Mind and Nature* is a challenge to our presuppositions about how we look at the world. The first extract for this unit is the first part of the second chapter of this book. Bateson is sometimes considered difficult to read because he draws analogies from very diverse fields of human endeavour, but this reading should help you to adopt a disinterested attitude to the relation between language and the world.

Read Reference 4.1: Bateson, 'The nature of science'.

Read subsections (2), (4) and (5) particularly carefully and think about just what it is that we do when we talk about the things in the world.

Bateson's second subsection is trying to make us aware of what it is to name; it is a form of classification. *But* the criteria upon which we do so are almost unknown to us. The 'rules of the world' we think we reflect exactly in our language are almost built into our processes of perception. Is it then not likely that they may be rules of perception and of our language? Bateson's fifth subsection may also seem a bit obscure. It may be paraphrased as follows: 'The world outside us is the way it is, but nothing outside constrains us to understand this world as divided in any particular way. We may take any criteria of division of matter in the world. We will obviously choose criteria which suit our purposes. It seems, in practice, that we have at least two criteria to classify bits of the world, and so to name it. They derive from ideas of the inherent characteristics of matter in the world, and our ideas of space-time. Perhaps these ideas are analogies of basic features of our world. The important point is that people speaking different languages appear to use these criteria in different ways'. As Bateson points out in his fifth subheading

> The division of the perceived universe into parts and wholes is convenient, and may be necessary, but no necessity determines how it shall be done. (Bateson, *Mind and Nature: A Necessary Unity*, E.P. Dutton, New York, 1979, p.38.)

Are you beginning to see why logomorphism is so hard to control? Yet at the same time, the recognition of logomorphism is essential in any enterprise of comparative conceptual analysis.

Read Reference 4.2: Watson, 'The types of objects that English speakers and Yoruba speakers talk about'.

In this study I compare the conceptual system which underlies ordinary number usage by speakers of English with that of speakers of the Yoruba language. Yoruba is a language spoken by around 30 million people in West Africa. Most Yoruba-speaking people live in the south-western region of Nigeria, although Yorubaland extends through the southern section of the Republic of Benin into Togo. The extract I have provided constitutes the final section of chapter 3 of my book and is a summary of what has been found through detailed linguistic analysis of English-language and Yoruba-language utterances. The rest of the book goes on to explain just how quantification in English and in Yoruba involve different conceptual processes.

Quantification is an important abstraction process in all languages. The conceptual processes inherent in Western European languages have become the quantification processes of science. A common assumption of anthropologists is that languages which do not share the quantification processes of science are 'primitive'. That is, they represent the type of system out of which 'modern' Western quantification developed. This point of view can be recognised as both ethnocentric and logomorphic once one realises that the form of the conceptual process of quantification will necessarily be related to the types of objects which speakers of a language say there are in the world. If two languages postulate different types of objects in the world, then obviously the abstraction processes in quantification will also differ.

Read Reference 4.3: Whorf, 'Science and linguistics'.

Benjamin Whorf was writing in the early 1940s. He was a student of the linguist Edward Sapir. Whorf, who worked as an industrial chemist, had no formal training in linguistics. His work is interesting because his speculation grew out of both linguistic insights and a good understanding of the nature and forms of scientific theories. He does not give details of the types of objects which speakers of North American Indian languages say there are in the world, but these may be fairly easily inferred from the examples of carefully translated utterances which he does give us. Reference 4.3 comes from the book of his collected writings, *Language, Thought and Reality*, which was edited and published after his death. In another article in the same collection he gives detailed examples of Nootka language utterances. A small extract from this second article has also been provided as an addendum to 'Science and linguistics'. A careful reading of this additional extract will help you understand the nature of the 'objects' which Nootka speakers postulate as comprising the world. They are particular isolates in space-time; for example, *tlih* (an event in canoe motion) and *ista* (a state of being a canoe crew). 'Objects' are operational or functional durations.

I interpret Whorf's ideas as a thesis of language-based *conceptual* relativism. This is a different position to most commentators on Whorf, who interpret him as proposing a thesis of *perceptual* relativism. Most of the empirical studies which are said to test the so-called Whorf-Sapir Hypothesis take it to be a hypothesis of perceptual relativism. I have not included a sample of this work here because it is not related to our primary aim. It is, however, of general importance to the questions of relativism. (See Lukes' article 'Relativism in its place', for a useful short summary of such studies.)

You started off this unit by reading about perception. Given what you understand about perception from Bateson, does it surprise you that different language communities postulate the existence of different types of objects in the world when they talk of it? Or does it now seem much more plausible that different language groups would devise different linguistic conventions about what 'a thing' is?

Having reached this point we are faced with two 'big' questions. First, what factors in a language community determine selection of the types of objects which a language postulates as comprising the world? Or, to put it another way, are the cultural traits, social values and environmental conditions of a community related in a causal manner to the types of objects which that group says are in the world? The second 'big' question is: what are the consequences of the fact that different language communities postulate different types of objects as constituents of a physical world? If different language communities talk of the world as being constituted by different sorts of objects, how will knowledge systems expressed in different languages relate to each other? Think about these questions while you read the next reference.

Read Reference 4.4: Sapir, 'Language, race and culture.'

The book was written in 1921 so you may find Sapir's expository style a little old-fashioned.

Now reconsider the first question. Could it be that certain values of a group will predispose it to postulate certain types of objects as comprising the world? But is it not just as likely that a language which postulates certain types of objects will predispose its speakers to adopt a certain value system?

Both these suggestions would require some preliminary empirical evidence that values and language are related. Is there such evidence? What is Sapir's opinion on this matter?

Alternatively, a utilitarian might suggest that efficacious functioning as a human being in a certain type of biophysical environment will predispose certain communities to select certain types of objects to talk about. Is there evidence to support this view?

The second question concerns the consequence of different language groups talking of different types of things as being in the world. This two-dimensional question is probably more important, partly because it is more likely that we can answer it. The first dimension concerns the nature of human beings. The second dimension concerns the nature of the 'what-is-outside-humans', the material world. In the first case we may ask: is there one kind of human being or many different types? Different types of people would think with linguistic symbols which would have different reference to the physical world. Would this then mean that different conceptual systems are incommensurate? Could it be that people use the information they receive from their sense organs differently because the symbolic categories with which they report their experience differ?

In the second case we must ask: is there one world or many? If there is one world can we judge which way of reporting it is correct? On what criteria should we judge claims that one conceptual system reports the world better than another? If we cannot agree upon what objects are in the world, nor adjudicate upon the issue, how can we agree or adjudicate upon the bigger question?

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5

AFRICAN IRON TECHNOLOGY

Prepared for the course team by David Dorward

African Iron Technology

What were the Origins of the Iron Age in Africa?

Whereas in most of Euro-Asia the Iron Age built upon an earlier Bronze Age technology, Sub-Saharan Africa was long thought to have passed directly from the Neolithic or Late Stone Age into the Iron Age. Copper mining and smelting in South-Central Africa had been shown to have evolved subsequent to the advent of iron working. Unfortunately archaeological evidence for Sub-Saharan Africa was patchy, generating heated and often highly speculative debate regarding the origins of early African iron working. This first section therefore addresses the question of the origins of the Iron Age in Africa.

Before we begin, however, it is important that you have an understanding of the distinctions between *iron smelting* and *iron forging* or *blacksmithing*. Smelting is the process whereby iron ore, which bears no resemblance to iron or steel, is converted in a furnace to produce a *bloom* or mass of concentrated metallic iron which has a greater or lesser quantity of impurities. Smelting requires very high temperatures (up to 1150°C) with the iron ore in combination with oxygen and carbon: oxygen passes through the burning carbon, heating the iron ore which gradually concentrates in the bottom of the furnace to produce the bloom. The iron yield can be increased by the addition of a *flux* which combines with the various impurities to form *slag*. Ideally the furnace should have a means of *tapping off* the slag during the process of smelting. The slag is a discarded by-product of smelting which is normally accumulated in the vicinity of the furnace—it provides sure archaeological evidence of smelting. Euro-Asian iron-smelting appears to have evolved from earlier copper smelting and developed in association with the use of high-temperature pottery kilns in Anatolia, from whence it spread to the Middle East. *Forging* or *blacksmithing* is the secondary stage whereby the bloom is reheated and beaten repeatedly to drive out the remaining impurities and the iron is then fashioned into the desired shape or product.

An older school of thought viewed Black African iron-working as derived from Egypt or Nubia, a conviction in accord with a general theory of cultural diffusion which permeated writing on Africa during the colonial era and for a considerable time thereafter. The ancient city of Meroe in the Nubian Nile (in what is today the Sudan) was commonly regarded as the point of diffusion, rather than Egypt proper. Its massive slag heaps, evidence of a major iron-smelting industry, had led Sayce to christen Meroe the 'Birmingham of Africa' (Sayce 1912). The frequency of narrow shaft furnaces with forced draft bellows found throughout Africa, and similar to one discovered at Meroe, was seen as pointing to a Nubian origin for African iron-working. Surely here was the source of inspiration and diffusion?

However, more recent research would indicate that iron *smelting*, as distinct from smithing, at Meroe only dated from about 500 B.C. Before then, iron working in Nubia and Egypt had been based on iron derived from meteoric sources or imported from the Middle East. The Assyrians, who sacked Thebes in 663 B.C., were responsible for introducing iron tools and weapons into Egypt on any scale. Moreover, the early furnaces at Meroe were crude affairs with no facility for tapping off the slag, while flux does not appear to have been employed to raise the iron yields. The narrow shaft furnaces which produced the impressive Meroe slag heaps and which were so similar to those in Sub-Saharan Africa do not appear until the first century A.D. (Trigger 1969). The origins of iron smelting at Meroe were contemporaneous with the occurrence of iron smelting among the Nok culture of central Nigeria in West Africa. Furthermore, iron working in the Lake Chad region, which lies between Nubia and the Nok sites, across the logical path of possible diffusion, did not commence until relatively late, in the 5th century A.D. Ethiopia, although closer to Meroe, obtained its iron technology from southern Arabia in the 4th to 5th centuries B.C. Thus there is no archaeological evidence to support the theory of diffusion of iron working from Meroe to West Africa, as espoused by Arkell and others (Arkell 1961; Huard, 1960, 1964, 1966). Nor does a Merotic origin accord with the known archaeological evidence from East and Central Africa.

The theories of diffusion from Nubia have been questioned by a group of scholars who raise the possibility of indigenous development of iron smelting in West Africa and elsewhere (Diop 1967). They have been particularly intrigued by the discovery of Early Iron Age sites at Rutare in Rwanda dating from the 3rd century B.C., and at Katuruka Buhaya near Lake Victoria (the iron smelting remains here have been radiocarbon dated from the 5th century B.C.); there have been equally early datings from Taruga in Nigeria. The thirteen furnaces at Taruga, a Nok site which has been radiocarbon dated from 440 to 280 B.C., appear to have a narrow shaft design without facilities for tapping off the slag, nor did chemical analysis indicate the use of flux (Fagg 1968, 1969; Tylecote 1975a). The furnaces were older than those at Meroe and of a rudimentary design consistent with an emergent indigenous technology. That early iron-working sites in West Africa contain Late Stone Age artefacts would arguably indicate that iron technology evolved within a Neolithic culture; Stone Age and Early Iron Age technologies co-existed over a long period throughout large areas of Africa. It has been suggested that the knowledge of smelting might have come from the experience in pit firing of pots in the highly ferric lateritic soils of West Africa. However, Africa was not noted for high (temperature) fired pottery, and the direct evolution of iron smelting from the Neolithic Age would represent a phenomenal technological breakthrough.

If there are major unanswered problems presented by the indigenous evolution hypothesis, and Nubian chronology does not accord with Sub-Saharan archaeological evidence, then what of North Africa as the source of iron technology? It is known that the Phonecians carried the knowledge of iron working from the Middle East to North Africa in the 1st millenium B.C. Rock paintings of chariots from the central Sahara point to contact with Sub-Saharan Africa by the 5th century B.C. Might not the culture bearers of the new iron technology have been the Libyco-Berber raiders from the desert who pillaged neolithic African agricultural communities? Herodotus, writing in the 5th century B.C., referred to the horse-using Garamantes of Saharan Africa who hunted the Ethiopians of the South (Macaulay 1904). Such dating would seem to concur with the radiocarbon dating from the Nok sites and the presence of iron objects from the Jenno-jeno site in the inland delta of the Niger River in Mali (Sutton 1982). Moreover, it was long known that a localised copper culture had flourished in the Akjoujt region of western Mauritania between the 9th and 5th centuries B.C., a development generally attributed to technological transfer from North-West Africa, but one which seemed too isolated to have much bearing on the wider incident of iron-working in Sub-Saharan West Africa (Lambert 1971).

Recent research at Azelik in the Air Mountains of central Niger have revealed copper furnaces dating from the middle of the 1st millenium B.C. There are also reports of unpublished archaeological discoveries of copper furnaces near Agades in Niger reported to date from around 2000 B.C. which, if confirmed, would constitute a Saharan 'Copper Age' contemporary with the Egyptian Middle Kingdom. However, there are problems arising from this very early date, since 4000 years ago this part of

Africa was experiencing a period of extreme desiccation which would have made central Niger very inhospitable and copper smelting logistically difficult (Calvocoressi & David 1971). Nevertheless, it is increasingly apparent that a Copper Culture existed which could have provided background in metallurgical skills for the more complex process of iron smelting. When taken in conjunction with the discovery of iron-smelting sites in south-eastern Niger dating from the early and middle part of the 1st millenium B.C., the copper sites would appear to support the argument for the diffusion of metallurgy from the Sahara. Yet, if this should prove to be the avenue of diffusion, it was no simple process of Libyco-Berber iron-using aliens imposing themselves and their technology on an indigenous neolithic populace. The ancient copper smelting of Niger was integral with a Late Stone Age culture. There is no archaeological evidence from Sub-Saharan early iron-working sites to indicate direct North African influence, while stylistically the terra cotta figurines and other objects from the Nok sites are uniquely African.

Recent archaeological evidence has dramatically altered our perceptions of the prehistory of African iron-working. However, much is yet to be done. If the oldest iron-working sites were in West Africa and the trans-Sahara proves to be the avenue for diffusion of iron-smelting from the Middle East, then the technology spread very rapidly, reaching Lake Victoria and central Africa by 300 B.C. (Sutton 1972; De Maret et al. 1977; Maggs 1977; Hall & Vogel 1980; Mgomezulu 1981; De Maret 1982). Early Iron Age sites dating from about 300 A.D. have been found in Zaire, northern Tanzania, Zambia and even Zimbabwe.

What was the Impact of Iron Technology?

The advent of iron led to an agrarian revolution. The domestication of animals and the development of agriculture in Sub-Saharan Africa predated the Iron Age by several millenia and there is little doubt that the evolution of crops and animals specific to Africa was indigenous. However, iron gave the African farmer a much more effective tool for clearing the bush. With iron tools, the agricultural frontier was expanded from the more open-treed savanna zone into the forest margins, beginning the process of transformation of the West African 'middle belt' from forest into derived savanna. It also facilitated one of the major demographic epochs of African prehistory, the Bantu migrations into East and Southern Africa.

Early Iron Age societies, with their distinctive pottery, metalworking and village culture, spread across East and Central Africa in the form of the Bantu migrations during the 1st millenium A.D., absorbing the indigenous Late Stone Age peoples and obliterating their cultures in all but a few pockets in South and Central Africa. They in turn, by the end of the 1st millenium, were largely overtaken by a second wave of Middle or Late Iron Age societies with their more complex political institutions of the state. As iron tools facilitated the generation of greater surplus, this so surplus made possible socio-economic differentiations, specialisation and the perpetuation of stratification with the emergence of the state (Sutton 1981). Of course, this is to gloss the myriad histories of pre-contact Africa and the reconstruction of the epoch events which impinged upon various peoples of Sub-Saharan Africa, shaping their distinctive cultures.

In his analysis of archaeological evidence from southern Mauritania and western Mali, Munson has argued that a powerful chiefdom or proto-state had already emerged by the Arriane Phase of the Late Stone Age, with cultivators living in well-organised stone masonry villages (Munson 1980). This society was destroyed, sometime after 600 B.C., by Libyco-Berber Iron Age raiders. In other words, in this instance at least, African political evolution had developed to a quite sophisticated level before the advent of iron-using aliens. The advent of iron appears to have been marked by devastation rather than sociopolitical and cultural advancement. Yet it was in this same region that the Ancient Ghana Empire, one of the great historic states of West Africa, was to emerge by the 8th century A.D., in a manner, as

Munson argues, that built upon the pre-existing patterns of a much more ancient proto-state. Moving forward several centuries, iron working increasingly appears to be linked to the demise of Ancient Ghana and therein lies a fascinating tale of the impact of technology.

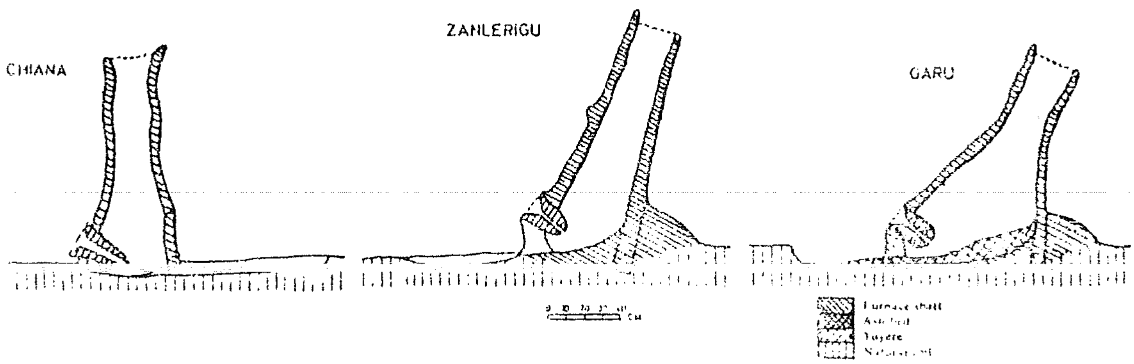
The area of Ancient Ghana spread across what is today a fairly arid region north of the Niger River. Yet Mema, which was part of Old Ghana, is depicted in the legend of Sundiata, an ancient Mali epic, as a prosperous and well-watered land (Niane 1965). It has been argued on the basis of research undertaken by Dr Randi Haaland, a Scandinavian archaeologist working in Mema, that:

> ... a thousand years ago the local environment was more attractive, with the Molodo branch of the Niger [River] still flowing northward from Segu, so that it would have compared with the present inland delta of the Niger to its east. Numerous occupation mounds, iron-working and signs of ancient agriculture are recorded ... It is arguable therefore that Mema was then a more populous area and, more than that, an essential food- and iron-producing province of the Ghana empire. Its decline, Haaland surmises, may have been due to ecological deterioration, caused both by excessive wood-cutting for the iron industry and by the silting of the Molodo channel. (Sutton 1982, p.305.)

It is a fascinating historical theory, in accord with other technological and historic evidence.

How did Iron Smelting Technology of Ancient Africa Compare with Contemporary European Iron-Working?

As the art of ironworking spread across Africa, smelters mastered the art of furnace construction in a profusion of local designs and learned to utilise a vast array of additives as flux.



Smelting furnaces from upper region of Ghana (Pole 1975 p.16.)

Moreover, it may come as a surprise to learn that African smelting technology was superior to that of Europe until the modern era. Most African iron-smelting furnaces were more efficient and produced a higher grade of near-steel or steel than was commonly to be found outside ancient China. African smelters evolved a system of using specially constructed elongated *tuyere* pipes through which the forced air from bellows was preheated before entering the furnace, thereby achieving a very high temperature while economising on fuel (Van Der Merwe 1980).

Read Reference 5.1: Schmidt & Avery, 'Complex iron smelting and prehistoric culture in Tanzania'.

As Schmidt and Avery stress in their conclusions, it was fuel which served as the crucial constraint on African iron production. They calculated that it would take approximately 5000 pounds of wood to produce the 500 pounds of charcoal required for a single firing of the Haya smelting furnace which they observed in operation. Candice Goucher has calculated that to produce the twenty-six slag heaps at Dapaa in Ghana, which was an iron-smelting centre between about 1400 and 1700, would have necessitated the felling of more than 300 000 trees (Goucher 1981). Moreover, not just any wood was suitable for the production of the type of slow-burning charcoal used in smelting and smithing. The required timber needed to be from the dense, slow-growing species, preferably with a high silica and alkali content, which helped stabilise the furnace temperature.

Iron tools not only made it easier for prehistoric farmers to clear the land for agriculture, but the production of iron to make those tools further contributed to the process of deforestation which appears to have taken place in the Early Iron Age. Regarding smelting technology, Schmidt & Avery propose 'one possible hypothesis is that the heavy exploitation of forests (for charcoal and for agricultural purposes) may have triggered the development of an efficient, fuel-economising technology', (Schmidt & Avery 1978, p.1089). The use of elongated *tuyere* extending into the furnace firebed, thereby preheating the bellows-forced air before it entered the furnace, was an innovation born of necessity. In this context it should be noted that 'the technique of preheating the air blast was patented in Great Britain only in the second quarter of the 18th century' (Goucher 1981, p.181). That was in response to a mounting charcoal fuel crisis which was only overcome with the innovation of coal-fired blast furnaces of the Industrial Revolution. To reiterate the conclusions of Schmidt & Avery:

> One of the more profound implications of the West Lake discoveries is that we are now able to say that a technologically superior iron-smelting process developed in Africa more than 1500 years ago. This knowledge will help to change scholarly and popular ideas that technological sophistication developed in Europe but not in Africa. (Schmidt & Avery 1978, p.1089.)

What Price 'Progress'?

Of even more profound significance for Africa and its history has been the process of deforestation and ecological denigration which accompanies smelting and expanding agriculture. Mention has already been made of the transformation of a broad belt of forest fringe into 'derived savanna'. In ecologically more fragile regions, such as the West African Sahel or the Fouta Djallon, deforestation resulted in devastatingly permanent environmental changes. Given the slow rate of natural regrowth of sahel and savanna species, the felling of trees resulted in effectively permanent changes in many areas, since the removal resulted in a higher rate of evaporation, lower soil humidity and soil deterioration. These changes consequently slowed the rate of natural regeneration, while the absence of trees would have led to increased wind and water erosion and a decreased overall rainfall in the longer term. In other words, the type of ecological deterioration which Haaland postulated as contributing to the decline and eventual abandonment of Mema. Even in the wetter forest regions, deforestation could have a permanent impact, such as the spread of trypanosomiasis and related tsetse-fly borne diseases in the Upper Guinea Coast in the 19th century (Dorward & Payne 1975).

From geological and other evidence, it is clear that Africa has passed through prolonged periods of greater and lesser rainfall. Even wetter phases have been punctuated by periodic droughts of localised severity. Large areas of once inhabited land have become desert or near desert. Only further study will reveal the extent to which these conditions were due to climatic changes and/or human action (Nicholson 1979).

What Has Been The Impact of European Trade on Indigenous African Smelting and Smithing?

Candice Goucher argues that the advent of European traders with their relatively cheap trade iron coincided with a mounting charcoal fuel crisis in the indigenous African smelting industry.

Read Reference 5.2: Goucher, 'Iron is iron 'til it is rust'

As Goucher has noted, the introduction of low-carbon, often high-sulphur, European pig iron was inferior to the traditional African smelters' near-steel and required the development of new techniques by African blacksmiths to 'recarbonize' the trade iron before it could be fashioned into tools suited to the African market. Even then, the finished product was often inferior to the traditional one since 'steeling' made the surface harder but more brittle. Farmers recognised that tools made from imported iron were of a poorer quality and nowhere near as durable. L.M. Pole (1982) has calculated that '... it was not until imported iron was cheaper than local iron by a factor of six that the preference for the local product was outweighed... because of the qualitative differences' (pp.507, 509). These and other factors have resulted in the survival of African smelting, as well as smithing, until recent years, not as part of an anachronistic 'museum' activity, but as part of the living local socio-economy.

Read Reference 5.3: Pole, 'Iron production in West Africa from the seventeenth to the twentieth centuries'.

What are the factors which have lead to the survival of African iron smelting and blacksmithing despite prolonged competition from cheap imported iron? ... Producer preference? ... The social organisation of labour? ... Marketing factors? ... Ritual attitudes?

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For a more current overview see:

Van Der Merwe, N.J. (1980), 'The advent of iron in Africa', in T.A. Wertime & J.D. Mulby (eds), *The Coming of the Age of Iron*, Yale University Press, New Haven.

Unesco General History of Africa, (1981), vol.2, Unesco, London.
The relevant regional chapters of this work include especially ch.24, by B. Wai Andah, who leans toward an indigenous African development of iron smelting, and ch.21 by M. Posnansky, which takes an opposing position. See also chs.23 on East Africa by J.E.G. Sutton and 25 on Central Africa by F. Van Noten.

The earlier theses of iron-working diffusion from Meroe, espoused by Arkell, Huard and others, has been effectively undermined by the work of Tigger and Tylecote (see works cited in the text).

Arguments for indigenous African evolution of iron smelting are propounded in:

Davies, O. (1967), *West Africa Before the Europeans*, Methuen, London.

These arguments have been countered by:

Shaw, C.T. (1969), 'On radiocarbon chronology of the iron age in Sub-Saharan Africa', *Current Anthropology*, vol.10, pp.226-31.

See also Diop (1968) for a further elaboration of these arguments.

The reconstruction of the Early African Iron Age outlined in this unit is based on a series of reports on archaeological findings and radiocarbon dates from the *Journal of African History* cited in the text, (Calvocoressi & David 1971; Sutton 1972; De Maret et al., 1977; Maggs 1977; Hall & Vogel 1980; Mgomezulu 1981; De Maret 1982; Sutton 1982) as well as articles on the Taruga Nok site authored by B.E.B. Fagg and R.F. Tylecote. For information on the early West African copper cultures, see the article by Lambert (1971) and the report of recent research in Niger recounted in Calvocoressi & David (1971). Finally, there are the fascinating historical interpretations of archaeological research articulated in the work of both Munson (1980) and Haaland (1980).

Technical aspects of African iron smelting are dealt with by Pole (1975), Schmidt & Avery (1978), and Van Der Merwe (1980) Goucher (1981). For the implications of deforestation and the charcoal crisis, see Darward & Payne (1975), Nicholson (1979), Haaland (1980) and Goucher (1981), while Goucher (1981) and Pole (1982) focus on the impact of European trade.

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6

THE LESSONS OF LYSENKO

Prepared for the course team by Wade Chambers

The Lessons of Lysenko

New
sermon
on the mount! ...

I do not roar to you about Christ's paradise,
Where Lenten fasters lick at sugarless tea.

I roar about genuine
earthly heavens ...

There the sun pulls off such stunts
that every step is drowned in seas of flowers.
Here for ages gardeners drudge
with glass-topped frames and heaps of dung,
but in my land
on parsley roots
six times a year pineapples grow ...

My heaven is for all
except the poor in spirit ...
Throw off nature's insolent yoke!

V.V. Mayakovsky.

Poems, Progress Publishers, Moscow, 1976, tr. W. Chambers.

Introduction

> Skinny, with prominent cheekbones and closecropped hair ... this Lysenko gives one the sensation of a toothache. God grant him health, he is a man of doleful appearance. Stingy of words and insignificant of face, except that you remember his morose eye crawling along the earth as if he were at least getting ready to kill someone. He smiled only once, this barefoot scientist, and that was at the mention of Poltava cherry dumplings with sugar and sour cream. (V. Fedorovich, 'Winter fields', *Pravda*, 7 August 1927, tr. D.W. Chambers.)

In the fullness of late summer, 1927, a feature article appeared in *Pravda* describing the brilliant agricultural results of a young Ukrainian, Trofim Denisovitch Lysenko. Although this rather severe verbal profile portrayed a dour and even intimidating personality, it also suggested qualities of determination, much needed in a country besieged, without and within; a country about to undertake complete reorganisation of its agricultural sector with the dual aim of modernisation and collectivisation. The newspaper story, which gave a glowing account of Lysenko's scientific work, demonstrated that, though not yet thirty, he had a way with the media which would stand him in good stead as he rose within a decade to succeed the distinguished scientist, N.I. Vavilov, as President of the Lenin All-Union Academy of Agricultural Sciences (LAAAS).

Within a few short years this agricultural scientist was to become a national celebrity with monuments erected in his honour and popular songs praising him:

> Merrily play on, accordion,
> With my girl friend let me sing
> Of the eternal glory of Academician Lysenko.
>
> He walks the Michurin path
> With firm tread;
> He protects us from being duped.
>
> (by Mendelist-Morganists; quoted in Z.A. Medvedev, *The Rise and Fall of T.D. Lysenko*, tr. I.M. Lerner, Columbia University Press, New York, 1969, p.132.)

Even after he had attained the presidency of the LAAAS Lysenko's name was little known in the West, except among plant physiologists who considered his work on vernalisation (a technique to speed plant development) to be a 'contribution of some importance' (N. Roll-Hansen, 'A new perspective on Lysenko?', *Annals of Science*, vol.42, no.3, 1985, p.272). However, by the year 1949, his fame in the West had spread due to the publication by a number of Western biologists of books and articles attacking the influence he was having on Soviet biology. The portrait these observers painted was exceedingly less flattering that the image of Lysenko in the Soviet press. His work was subjected to a scathing critique on a scale perhaps unprecedented in the annals of science. For example, Julian Huxley's book *Soviet Genetics and World Science*, published in 1949, which was more restrained in tone than most, considered the new LAAAS president a sincere but fanatical and ignorant peasant. Others called him a charlatan, a crank, and a pseudo-scientist; while the Australian biologist Eric Ashby (in his book *Scientist in Russia*), who had met Lysenko, referred to his work as the product of a 'medieval mind' using a 'medieval technique'.

Although Lysenko's detractors did not spare any aspect of his work, denouncing him as a pure ideologue and scientific ignoramus, their ire focussed not on his plant physiology but on his interpretations relating to the two great generalisations of 20th-century biology: the closely related theories of heredity and of evolution.

Lysenko's critics did not disagree with the view that the environment could induce alterations in the development of a plant or animal. However, Lysenkoists went beyond this and asserted that the induced alterations in development were inherited by later generations. This claim was in direct conflict with the doctrines of Mendelian genetics and natural selection. Mendelian geneticists and neo-Darwinians claimed that an undirected hereditary change, a genetic mutation, was the only change required to fuel the process of evolution. They assumed that gene mutations occured randomly in a population, the role of the environment being only to select the hereditary variations which were most favourable to the survival of species and to the individual. The environment could not, in their view, direct hereditary changes, and all the well-known environmental modifications acquired in the life history of individuals could not be passed on to future generations.

In direct conflict with this view, Lysenkoists believed that acquired characteristics were heritable, endorsing the view articulated by neo-Lamarckian biologists. The neo-Lamarckian tradition and the neo-Darwinian tradition struggled throughout the first half of the 20th century in the Soviet Union as elsewhere. While neo-Darwinian biologists claimed that gene mutations and natural selection alone sufficed to account for the origin of species and the course of evolution, neo-Lamarckian biologists had traditionally claimed that gene mutations controlled only trivial characteristics such as eye colour, hair colour, wing shape, and so on, which did not exceed the boundaries of the species. The fundamental characteristics of the organism, common to higher taxonomic groups, they claimed were due to Lamarckian principles in evolution governed by extra-chromosomal inheritance, through the cytoplasm of the cell. Indeed, many geneticists are now beginning to concede that the cytoplasm has a parallel though weaker role to play in the transmission of characters, together with the chromosomes.

However, here too, Lysenkoists went a step further and denied the evidence for the existence of genes. They claimed that heredity could not be based on any special discrete bodies such as chromosomes. They denounced Mendelian genetics, calling it abstract, idealist, fascist, racist and incompatible with Soviet science and dialectical materialism. As we shall see, name-calling was a tactic relentlessly employed by both sides of the debate.

That a great many of the strongest attacks on Lysenko in the West were to appear in 1949 was no coincidence. They were written primarily in response to a remarkable scientific conference held in Moscow in 1948. The meeting was to be a forum for Lysenko's theories in line with the objective of the socialist transformation of Soviet agriculture. From the verbatim account of the proceedings, published in the following year, it is possible to learn much about the political strategies, the intellectual style, and the scientific and philosophical content of the Lysenkoist movement.

The conference has the feel of being carefully stage-managed. Beginning with Lysenko's opening address, 'On the situation in biological science', with its scathing attacks on the Soviet Union's leading geneticists, continuing with their responses (complete with Lysenko's barbed interjections), we are led to the grand finale: that hushed moment after Lysenko asks the rhetorical question, 'What is the attitude of the Central Committee of the Party to my report?' The answer: 'The Central Committee of the Party examined my report and approved it'. The minutes record the response which came from the audience of nearly a thousand people: 'Stormy applause. Ovation. All rise.' (*The Situation in Biological Science: Proceedings of the LAAAS*, Foreign Languages Publishing House, Moscow, 1949, p.605.)

Had Lysenko announced in his opening, rather than closing, remarks that Stalin had read and approved his address, the tenor of the conference that followed would likely have been very different indeed. It would have taken a brave individual to oppose a Lysenko who had the full backing of Stalin. But although the geneticists knew they were fighting for their careers, perhaps even their lives, so too they believed was Lysenko. In the months before the August meeting, Lysenko's political power had been at a low ebb. His presidency and his science had been subjected to severe criticism by Yuri Zhdanov, whose credentials included being head of the science department of the government's Central Executive Committee. Perhaps of even greater significance were Yuri Zhdanov's family connections: he was the son of Andrei Zhdanov (a powerful member of the Politburo and specialist in all cultural matters) and he was married to Stalin's daughter.

Andrei Zhdanov, one of Stalin's chief aides, had for years been waging a campaign against bourgeois and foreign influences in the arts and in certain areas of scholarship, but he had not yet turned his attention to the natural sciences. If his highly placed son could criticise Lysenko, then perhaps it would be possible to balance the scale against the president of the LAAAS and against his scientific practices. At least, so it must have seemed to the geneticists, many of whom spoke eloquently of their opposition to Lysenko and his reliance on what had come to be called Michurinist biology. But their tone was largely defensive and laced with calls for compromise between the two schools of thought, which may indicate that they suspected what proved to be true: the geneticists had been set up for an ambush. Indeed, the issue had been decided several weeks before the conference opened!

The first public clue as to the eventual outcome had been the announcement, three days before the meeting opened, that unexpectedly thirty-five voting members of the Academy had been *appointed* by the Council of Ministers, in addition to those vacancies which were to be *elected* from a list of candidates nominated by the scientific community. As it turned out, the appointees had been handpicked by Lysenko, giving him majority support of the membership. Furthermore, on the last day of the conference, *Pravda* published a letter to Stalin, dated early July, from the younger Zhdanov, confessing mistakes in his earlier attack on Lysenko and promising future support. This had been written presumably when Zhdanov learned that Stalin had approved Lysenko's speech. Lysenko later boasted that Stalin had amended in his own hand the original draft of his speech, offering advice on its oral delivery.

In the words of Zhores Medvedev, 'this rout was brilliantly organised' (Medvedev, 1969, p.113). The final session concluded with a parade of geneticists obviously in some anguish, promising to review their anti-Michurin, anti-Lysenko activities and calling on their comrades to do likewise. They retired thoroughly discredited, which of course had been Lysenko's object in orchestrating the conference from beginning to end. From that moment until Stalin's death in 1953 Lysenko reigned supreme, not just in agronomy, but throughout the biological sciences. Medvedev, one of the more reliable of the many chroniclers of this episode, gives voice to a liberal Soviet scientist's view of the period of Lysenko's dominance:

> Many excellent genetical, cytological and physiological laboratories of the country were shut down during this period. The scientific and political prestige of our fatherland, and the immortal cause of socialism suffered greatly from these sense-less persecutions. The bourgeois press was provided with material for anti-Soviet propaganda. But most important was the damage to agriculture, medicine, and many brances of the national economy. (Medvedev, p.104.)

Not only did Medvedev, a Russian socialist, deplore Lysenko's, predominance in science, but also many socialists in the West disassociated themselves from what they considered the disastrous policies of the Stanlinist period.

Because many observers attributed Lysenko's preeminent position in Soviet science as due strictly to Stalin's patronage, it was widely anticipated that Stalin's death in March 1953 would bring an end to Michurinist biology. For those interpreters who see the rise and fall of any scientific theory as bound up in complex ways with social and economic factors, it was not so surprising that Lysenko, while not above criticism, retained wide credibility within the Soviet Union. His fall from scientific grace came over a long period of time. During the 1950s Lysenko energetically supported the government's agricultural policy and his fall came only after it became clear that this policy had failed.

After June 1957, Khruschev singlehandedly held the reins of power, and Lysenko, who had close relations with Khrushchev, continued to exert great influence in the LAAAS. However, within the Academy of Sciences (AS) Lysenko's power had begun to wane. Because of strong support from Soviet physicists, chemists and mathematicians, molecular biology was taken up in various institutes of atomic physics and organic chemistry. For these reasons, DNA research proceeded apace after 1959, in spite of Lysenko's position as Director of the AS's Institute of Genetics.

Finally, after Khrushev was ousted in October 1964, vehement criticisms of Lysenko began to appear in the press; his colleagues began to lose their posts' and in February 1965 Lysenko was dismissed as Director of the AS's Institute of Genetics. He died in November 1976, at the age of 78.

Read Reference 6.1: Lenin Academy of Agricultural Sciences of the USSR, 'Selections from the proceedings of the LAAAS'.

Note that the original document from which this short reading was taken totals more than 600 pages. Because almost all technical detail and many lines of argumentation were necessarily eliminated, the point of the extract is only to provide an example of some of the principal issues discussed, to show something of the intellectual style of the participants, and to underline the emotional tone of the exchange.

Questions for discussion

1 **In these extracts identify, and where possible assess, the relative importance of the following factors in the overall discussion: scientific ideas, experimental evidence, theory versus practice, ideological considerations, the Party, agricultural needs, the personalities involved, careerist ambitions, disciplinary rivalries, Russian nationalism and suspicion of hostile foreign elements, institutional reforms of science, suppression of free speech, democratisation of science, and the domination of nature. (Note: At the end of this unit you will be asked to reconsider this question. Therefore you may wish to take notes on your preliminary assessment in order to see how your thinking may change.)**

2 **Vituperation is not unknown at Western scientific meetings, especially when exponents of rival schools of thought confront each other directly. From what you know of scientific meetings, which elements of the LAAAS sessions, if any, do you presume to be uncharacteristic of normal scientific exchange?**

What was Lysenkoism?

In the readings of this section we shall consider a three-fold answer to the above question: Lysenkoism was (1) a theory of inheritance and evolution, (2) a systematic approach to the transformation of plant and animal heredity to meet perceived human need (i.e. what we now call genetic engineering), and (3) a movement for institutional reform in science.

In the course of investigating these three aspects of the Lysenkoist program, we hope to learn something about the reasons behind both Lysenko's ascendancy and his fall from status and power. At the same time we must also try to understand what lay behind the unprecedented and abusive vehemence which characterised the writings of Lysenko's Western critics. Was the geneticists emotional fervour a simple manifestation of righteous indignation? Whatever one may think of the Lysenkoist methods, their ideological perspective, or their scientific conclusions, it is important to remember that the nearly universal condemnation in the West of the Lysenkoist movement was itself couched in remarkably intemperate and transparently ideological language.

In this light we see how important it is to analyse Lysenko's ideas clearly, rationally and in an appropriate context. We must learn the right lessons from this sad episode. Dismissing Lysenko as a complete fraud, crank, charlatan, or irrational brute (terms which have been consistently used by commentators and which in the context of Stalinist terror tactics have a certain attraction) may be dangerously inadequate. Jean Rostand, a French biologist and forceful critic of Lysenko, who nevertheless believed that Lamarckian principles might be valid, made this point when he cautioned that we should not use this affair to attack 'under a scientific cloak' ideas which in another context may be 'perfectly respectable' (J. Rostand, *Error and Deception in Science*, Basic Books, New York, 1960, p.48). Most commentators have been so intent on using Lysenko as a club to hammer socialism or Lamarckism, that their desire actually to understand these events seems to have vanished long ago. Assuming that one's opponents are irrational, primitive and cruel is a Cold War game which we must sooner or later outgrow if the world is to survive.

1 Theory of inheritance and evolution

Lysenko's now discredited theories, though never as fully detailed and extrapolated as were the theories of genetics to which they were set in opposition, must nevertheless be seen as an internally consistent body of scientific ideas. These ideas have a long and respectable history quite separate from, and in some cases prior to, the corpus of Marxist thought. Furthermore, they were still the subject of serious international scientific debate when first embraced by Lysenko. However, the fact that he wore his ideology on his sleeve, so to speak, was held against him in conventional scientific circles. Why? Firstly, because any ideological discussion in science is seen to offend the much-debated ethos of the scientific community which maintains that scientific work is by definition ideologically neutral. Secondly, because Lysenko's Marxism itself was violently in contradiction with the tacit ideological commitments of most Western geneticists. All these issues need further examination and will be considered below under three subheadings: (a) the historical roots of Michurinism, (b) the demarcation of science from non-science, and (c) the play of ideology.

(a) *The intellectual roots of Lysenkoism*
In the first place we must situate Michurinist biology with regard to the development of genetics and evolutionary thought. At the time of Lysenko's training and early scientific formation, evolutionary theory had not yet reached its modern formulation. This was achieved in the mid 1940s, nearly ninety years after the publication of Darwin's *The Origin of Species*. In other words, during the 1920s, 1930s and 1940s, the mix of acceptable evolutionary ideas was much more fluid than it was soon to become.

When Lysenkoists emerged on the scene in 1932, Mendelian genetics and natural selection were just beginning to be constructed into the 'evolutionary synthesis' with the rise of population genetics. Although Lamarckian sympathies persisted in non-genetics circles, there was very little experimental evidence in its favour, and what there was, was discounted or ignored by Mendelian geneticists. Geneticists working on fruitflies and corn had demonstrated that with few exceptions all hereditary traits could be traced to the action of genes. Nevertheless what chromosomal genes were, and how they directed the formation of the characteristics said to be under their control, remained unknown.

This knowledge came latter, from the work in molecular biology and microbial genetics, which emerged during the 1940s and 1950s when micro-organisms were first domesticated for genetic use. During the late 1940s, when Lysenkosists were rising to prominence in the Soviet Union, genetics in the West was changing rapidly and the gene concept itself was being altered. Many phenomena in micro-organisms were found involving the relationship between the environment and heredity which could not be readily accomodated within the classical conception of what the gene was and how it functioned. Many new cases of non-Mendelian inheritance were reported and many geneticists in the West, non-Marxist and Marxist alike, viewed them as representing a Lamarckian principle in evolution. Thus genetics itself was undergoing a crisis in a Kuhnian sense which would lead eventually to a new concept of the gene in terms of information and to new concepts of genetic regulation.

August Weismann in the late 19th century had cut off the tails of rats to demonstrate that taillessness was not inherited in an attempt to discredit the inheritance of acquired characteristics. He assumed a germ line independent of somatic or body cells. However, Lysenkoists did not believe that all acquired characteristics could be inherited, only those which stemmed from environmental influences. In micro-organisms there was no distinction between germ plasm and somatoplasm. In plants the seed arose late in development and no major separation was clear. Lysenko was a botanist and knew perfectly well that by simply sticking a broken twig into the ground a new plant could develop from the body cells. Lysenkoists kept a close watch on non-Mendelian inheritance and the revolutionary genetics in the West and saw *ad hoc* hypotheses and a major crisis which they believed would lead to a wholesale rejection of the classical gene concept.

Read Reference 6.2: Mikuluk, 'Lysenko's theory of inheritance'.

Historians are often called on to explain why a particular idea emerges in one place and not in another, at one time and not at another. Similarly, it is not entirely unusual to find that ideas may have a differential life-span in different cultural contexts. Social and cultural factors are usually at the heart of both sorts of explanation.

Questions for discussion

1 **With hindsight, can we say that Lysenko's rejection of the existence of the gene was not timely (i.e. that it was not scientifically progressive)? What criteria does one bring to bear in making such a judgment?**

2 **History gives us many examples of major and minor scientists who cling to an old idea when most others have moved on. Can you name any? On what basis can such an action be construed as rational or irrational?**

3 **Later in this unit we shall consider some social and cultural reasons why Lamarckian ideas were particularly attractive in the context in which Lysenko worked, and why they were less so in the capitalist West. Can you speculate about what these reasons might be?**

We said above that scientific ideas about heredity and evolution were still 'on the boil' in the 1930s and 1940s when Lysenko came to power. Another extremely important ingredient in the brew that was to produce the modern evolutionary synthesis was an unresolved struggle among the various relevant academic disciplines. Evolutionary theory cut across the boundaries of many scientific fields of specialisation by raising general questions of growth and development, morphology and physiology, variation and inheritance. These issues were addressed by disciplines as distinct as embryology, paleontology, biochemistry, plant breeding, cytology and many others. In other words, the field of heredity was not at this time the sole province of the science of genetics as is generally the case today.

Read Reference 6.3: Sapp, 'The struggle for authority in heredity'.

Questions for discussion

1 **Which factors are cited by Sapp as especially important in the triumph of geneticists over other disciplines for control of the field of heredity in the USA?**

2 **A similar struggle for authority among disciplines in the USSR was resolved in a different manner with Michurinist biology emergent in the 1930s and triumphant in the 1940s. Identify those factors which explain this differential result in the two countries. (More light will be shed on this question in the readings below.)**

3 **During this same period eugenics, the science concerned with the genetic improvement of the human race, lost some of its status as a legitmate field of scientific inquiry. Can you account for this result? Were the reasons primarily scientific or primarily social?**

Another point to keep in mind as you consider the nature of Michurinist biology is its firm foundation in a variety of scientific traditions. Within the USSR, we note in particular the work late last century of Timiriazev, Darwin's Russian bulldog, who as a champion of Darwin lived long enough to deplore the rise of Mendelism, which he saw as a threat to Darwinian theory. He also was a vigorous opponent of Weismann's notion of the immortal germ plasm, which similarly distressed the Lysenkoists. Furthermore, he supported Lamarck's focus on the role of the environment in evolution and distrusted the growing emphasis on statistical analysis. In other words, much of Lysenko's theoretical program stemmed directly from Timiriazev, whom he frequently cited.

Of course, the person most often cited by Lysenko was I.V. Michurin, who stands in much the same relation to Lysenkoism as Mendel does to Western genetics. Just as Mendel himself did not formulate the whole of Mendelian genetics, so Michurinist biology was not the sole contribution of Michurin, who was after all not a theoretical biologist but a practical horticulturalist preoccupied with transforming nature. Michurin's work is itself based in a Russian school of biological thought that extends back a generation before Michurin's birth. In Weiner's carefully documented study (most of which is too detailed to be included here), an authentic intellectual lineage within the Russian intellectual tradition is clearly established for Lysenko.

Read Reference 6.4: Weiner, 'The roots of Michurinism'.

Questions for discussion

1 **What do you think Weiner means to imply by characterising Lysenko's references to Michurin as 'a *post hoc* attempt by Lysenko and his supporters to establish an intellectual pedigree for their program' and in another section of his paper as a 'gambit calculated to enhance his legitimacy'?**

2 **The purpose of citing another scientist's work is at least twofold: to give credit of priority where it is due and to connect one's own work with its intellectual sources. In citing Michurin, was Lysenko in any way departing from this tradition? Or to put it another way, does Weiner's characterisation apply equally well to all scientific citation?**

3 **Why might Weiner be unwilling to admit that Lysenko had authentic intellectual forbears?**

4 **What ideological presuppositions are betrayed by Weiner's use of words like 'Soviet propagandists', delirious apotheosis of Lysenkoism', and 'anti-scientific defence of pseudo-scientific ideas'?**

5 **The scholarly research reported in Weiner's article is exclusively confined to 19th century-figures and events. Why does he bring Lysenko into the picture at all?**

6 **Weiner regards Lamarckian doctrines as 'progressive' in the 19th century but 'pseudo-scientific' in the 20th. To the extent such a distinction is valid, how can one determine precisely when such a change has occured? In this example, what criteria do you think Weiner has applied?**

One might point to other influences on Lysenko which are not generally acknowledged, such as the Russian nobleman Kropotkin whose evolutionary ideas focused on co-operation in place of the Darwinian emphasis on competition. Kropotkin's emphasis on what he called 'mutual assistance' is echoed in Lysenko's approach to 'cluster planting' of trees. Kropotkin, who as an anarchist opposed the Bolsheviks, called for a new approach to agriculture in which work-by-head and work-by-hand would be united, an idea shared with Marxist thought and central to Lysenkoism.

The intellectual style and content of Michurinism also show clear parallels with a long-standing 'romantic' dimension within the scientific tradition. This orientation emerged with Paracelsus, with the Jacobins in revolutionary France, and to a lesser extent with the German 'nature philosophers', who explored biological problems from a consciously philosophical perspective. J.R. Ravetz summarises this tradition as incorporating 'a stress on craftsman's manipulation, a personal involvement in the work, a democracy of participation, and a distrust of abstract or mathematical reasoning.' (*Scientific Knowledge and its Social Problems*, Oxford University Press, New York, 1971, pp.392-3.) All of these are factors found in the Lysenkoist movement.

Ravetz takes the historically debatable position that this tradition has been largely barren in the production of scientific knowledge, though others have argued that such a perspective has positively influenced the work of scientists in a number of disciplines. For our purposes, it only matters that, while this connection is never acknowledged, Lysenkoism is clearly identified with such an intellectual mode.

Thus, Lysenko's view of evolution and heredity grew out of several traditions of biological thought: evolutionary theory with a Lamarckian slant, a 'romantic' philosophy of nature, natural history, horticulture and agronomy, 'transformist' biology in 19th-century Russia, utopian and populist traditions within Russia, and of course Marxism. The influence of Marx on Michurinist thought, which has been systematically exaggerated both by Lysenko and his opponents, will be discussed in greater detail below.

All the intellectual linkages discussed above make it very clear that critics who dismiss Lysenko as an ignorant peasant are wide of the mark. He first gained credibility in the scientific community through his early work in plant physiology which was taken seriously both in his own country and in the West (see Roll-Hansen, pp.266-72). When he went on to postulate a new amalgam of hereditary and evolutionary ideas, his work was received, at home and abroad, with much greater scepticism, and was never given much credence in the West. However, in the Soviet Union his general theory of heredity became the received view for at least ten years and arguably for twenty years.

For our purposes, the important question is not whether Lysenkoist theories were 'right' or 'wrong', but rather why they were so successful for a generation in the USSR. Similarly, we need to know why Mendelian geneticists won the day in the West and ultimately in the USSR as well.

What is the difference between the claim that Lysenkoism lacks intellectual substance and the claim that it is not scientifically progressive?

Describe the differences and the similarities in the rise of Mendelian genetics and the rise of Michurin biology.

(b) *Demarcation of science*
Before undertaking this section students may wish to review Section 1 and 2 of HUS306 *Knowledge Using* Science in Culture, Part B. Most scholars who have written about Lysenko have been at pains to insist that Lysenko was not really a scientist at all, and that Michurinist theory was in no sense 'scientific'. For example, Garrett Hardin, an American biologist, in his widely read book *Nature and Man's Fate* (Rhinehart, New York, 1959) suggests that 'to discuss Lysenko's beliefs as if they were scientific questions would be to mistake the nature of science itself' (p.239). Again, Loren Graham writing in the book *Science and Philosophy in the Soviet Union* (Alfred A. Knopf, New York, 1972), argues that 'the Lysenko episode was a chapter in the history of pseudoscience rather that the history of science' (p.195). Finally, Julian Huxley says '[Lysenkoism] is less a branch of science comprising a basis of facts, than a branch of ideology, a doctrine which it is sought to impose upon facts' (p.viii). Such comments have been almost universal among Western writers. Unfortunately, the boundary between science and non-science is much less easy to define that has been commonly realised.

The question of differentiating science and non-science will recur throughout your reading on Lysenkoism. For a number of very concrete reasons, it is essential to treat Michurin biology and Mendelian genetics as contending schools of scientific thought. Such a position of course says absolutely nothing about the validity of Lysenkoist theories. The history of science is filled to overflowing with rejected theories, and these are as important to an understanding of how science works as are those theories which are widely accepted.

Why should *we* regard Michurin biology as scientific when so many geneticists and historians of science do not? Because Lysenkoists laid claim to scientific status; because they chose to work within scientific institutions and managed to gain their support and even acclaim; because in the wider Russian community Lysenko was generally considered to be an important scientist; because Lysenkoists attempted to develop the intellectual links of their ideas with those of such important earlier scientists as Darwin and Lamarck, while distinguishing their theories from other scientists such as Mendel and Morgan; because their ideas may be considered a straight-line development from a number of pre-Soviet natural historians, biologists, horticulturalists and philosophers; because they insisted that their ideas were constrained by experience of the natural world; because their departures from strict experimental controls and quantitative analysis of their data were not arbitrary, but were defended as holistic and non-reductionist approaches to the subject, accomplished in a real-world setting (unlike the fruitfly experiments of the geneticists)—for all of these reasons, Lysenkoist theoretical pronouncements must stand or fall as scientific theories.

To dismiss Lysenkoism as 'non-science' would require a definition of science that is clearer and more unambiguous than any that scientists or philosphers of science have so far been able to produce. A definition which excluded Lysenko would necessarily exclude a great many other thinkers and practitioners in the history of science, both major and minor figures. What science is, is one of the issues in science itself.

None of the reasons for excluding Lysenkoists from the scientific fold stand up to analysis. That their theories were never universally accepted and have now been almost universally rejected can also be said of hundreds of faithful scientific workers and even of some of the great. It has been suggested that the work of the Lysenkoists was not scientific because they did not perform experiments, which is untrue, and in any case not all scientists work in laboratories. Perhaps the major accusation against Lysenkoism as a scientific school has been its ideological associations. This point needs further clarification.

(c) *The play of ideology*
The relationship of science and ideology is so very multifaceted and complex that, in the brief compass of this unit, many dimensions of the problem must be neglected. We shall not, for example, raise the vital demarcation question as to the possibility of drawing a line that separates ideology from science, i.e., can there be science that is ideology free? Nor shall we consider how scientific knowledge itself may function with the force and effect of ideology. We cannot look at the historical role ideology has played in determining the direction and pace, and even the content, of scientific change. It will not be possible to analyse in depth the specific role that Dialectical Materialism may have played in the formation of Michurinist biology or the role capitalist modes of thought may have played in the development of Mendelism. Finally, it is beyond our scope here even to identify the many ideological strands which pervade both sides of the Lysenko debate. All of these issues, and more, constitute important dimensions of the problem, both generally and in particular relation to Lysenkoism.

Students in this course will be very familiar with general questions of science and values, science and social interests, and science and politics which have been raised throughout the Deakin major in Social Studies of Science. In particular it may be useful to consult the paper of Bob Young, 'Evolutionary biology and ideology,' (read in the course HUS203 *Nature and Human Nature*). Drawing on this background, it is possible to make the following statement about the role of ideology. In an important sense all science is infused with ideology. This is not to suggest that it is distorted or untrue, but rather that its knowledge claims, while directly constrained by the material world, also carry value components based on ideological presuppositions. These underlying views may be unconscious and implicit or they may be explicitly argued for.

An important part of the geneticist's case against Lysenkoism has been the explicitly ideological baggage it carried. In the words of Jean Rostand, 'Michurin biology was not just an ordinary error. It was a delusion *based on ideological indoctrination*' (J. Rostand, *Error and Deception in Science*, Basic Books, New York, 1960, p.42, his emphasis). Similarly, the Lysenkoists labelled Mendelist teaching as idealist, metaphysical and unscientific: 'The true ideological content of Morgan's genetics has been well revealed', said Lysenko (LAAAS, p.20).

Both parties were right to discern ideological components in their rivals' position, but this in itself was not so unusual as each seemed to believe. In any case, the particular connections assumed were not very astutely judged, and, at least in the case of Lysenko's Marxism, amounted to little more than window dressing. The much discussed connection between Dialectical Materialism and Michurinist biology has been shown to be much weaker than either Lysenko or his Western opponents would ever admit. There is no logical necessity for Marxists to support neo-Lamarckism, for example; indeed some modern Marxist biologists in the West have argued that neo-Lamarckian evolution is actually incompatible with Marxist interpretations.

One of the first Western commentators to realise that Marxist ideology as such was not a major 'cause' of Lysenkoism was the historian David Joravsky. The opening words of his major study of Lysenko make this point: 'When other efforts fail, tired minds invoke the magic name of "ideology" to explain the behaviour of Soviet Communists' (*The Lysenko Affair*, 1970, p.1). But Joravsky still attempts to maintain a hard and fast distinction between science and ideology which considerably muddles his explanatory framework. This point is taken up by another historian, Gary Wersky, in his review of Joravsky's book.

Read Reference 6.5: Wersky, 'Science and ideology in the Soviet Union' (pp.240-3).

Questions for discussion

1 **What does Wersky mean when he suggests that Joravsky misunderstands the nature of science?**

2 **Compare Wersky's and Joravsky's positions on the science/ideology question.**

3 **If Lysenkoism was official Soviet doctrine, to what extent was gene theory official doctrine in the West?**

What can we say of the ideological dimensions of Mendelian genetics? Lysenkoists were quick to point out the close association between genetics and the racist eugenic theories of Nazi Germany. Much more might be said about this historical association, but the mainstream of geneticists disavowed such obvious Fascist connections. Their most important philosophical presuppositions were often hidden or at least undisclosed.

Read Reference 6.6: Lewontin & Levins, 'The ideological implications of genetics'.

Questions for discussion

1 **Do you think it purely coincidental that a science which emphasised co-operation and the role of the environment in heredity should grow up in an avowedly socialist country, while a science that emphasised competition, fatalism and the role of an individual's genes in heredity should grow up in an avowedly capitalist country?**

2 **Consider Lewontin & Levins' distinction between the minimal theoretical structure and the ideological superstructure of a science. Apply the distinction to some other scientific theory, for example Darwinism.**

Because ideological commitments are systematically masked in scientific papers, and thus more difficult to tease out, it is helpful to turn to the geneticists' popular writings, particularly their comments on the Lysenkoist movement. We have seen hints of this in the vehement language used in various quotes cited above. For a fuller illustration of the ideology that lay behind the Mendelian position, consider the following brief selection from the book *Nature and Man's Fate* by Garrett Hardin, an American microbiologist. This book is one of the earliest and most comprehensive popular statements of the position of biological determinism referred to in the last reading.

Read Reference 6.7: Hardin, 'The ideological disease of denying competition'.

Questions for discussion

1 **What can you say about Hardin's own ideological commitments? Do you think Hardin would acknowledge that his own views might contain the germ of an ideological 'infection'?**

2 **Hardin sees brotherly love, music, co-operation etc., simply as further manifestations of the competitive urge. Comment.**

3 **More than any other country, USA played the dominant role in the rise of the new discipline of genetics. In what ways might the ideological environment, so aptly described and embodied in Hardin's essay, have contributed to that development? What parallels do you find with the USSR during the same period?**

In conclusion, Lysenkoism, viewed as a theory of inheritance and evolution, constituted a synthesis of several distinct schools of thought, which developed at precisely the same time that a quite different evolutionary 'synthesis' was emerging in the West. The hegemony of the former synthesis in the USSR was marked by the 1948 conference of the LAAAS. That of the latter was marked by the publication in the mid-1940s of books by Huxley, Mayr, Simpson and others which brought together three separate modes of evolutionary thought: genetics, natural history and biometrics. (It should be mentioned that in the USSR, an emerging synthesis of the Western type was represented in the 1920s and 1930s by a school of population genetics which is now recognised as having made important scientific contributions—see '*Towards a synthesis: population concepts in Russian evolutionary thought*' by Adams.

In this fashion the stage was set for a great struggle between two contending paradigms (to employ the Kuhnian term), a struggle which took place both within the USSR and also between scientists of East and West. At every stage of the confrontation the issue would be resolved in the arena of 'practice', i.e. the test would be: Which theory seems to work best in the real world?

2 A system for transforming nature

More than anything else, Lysenkoism was a scheme for bending nature to the human will. If there is any single ideology which dominates the Lysenkoist movement, it is not Marxism as such but rather the ideology of 'the exploitation of nature', an ideology common to both capitalist and Marxist modes of thought. (William Leiss 1972, writes about the domination of nature as a specie of ideology.) Genetics, too, promised to transform nature' a promise which has been kept in agriculture and also, a generation later, in the form of genetic engineering. But the practical achievements of genetics in the agriculture of 1948 were few and far between, as was painfully obvious in the chiding by the Lysenkoists of the geneticists in the 1948 meeting of the LAAAS. Even the few hybrids that Mendelists had developed by that time did not always suit Soviet conditions.

The spirit that pervaded the Lysenkoist movement was utterly consonant with the mood of transformation which enabled the USSR to industrialise its economic base in a fraction of the time it had taken any Western country. This spirit was given a name: Stakhanovism, after Alexei Stakhanov, a coal hewer who had carved out 102 tons of coal in one shift, overfilling his quota by 1400 per cent.

Read Reference 6.8: Young, 'Agricultural Stakhanovites'.

The agricultural problems of the Soviet Union, which will be considered below, were staggering. To overcome them would require all the knowledge and skill that science could provide. But there were two contending and mutually contradictory schools of science: Mendelian genetics and Michurinist biology. Each claimed to speak with authority; each claimed the other was ridden with pseudo-science and ideology; each claimed certain successes. The geneticists, speaking a complex and jargon-filled language, told of success in the laboratory and test-tube; their authority was backed by their professional colleagues in the hostile countries of the capitalist West. The Lysenkoists, speaking the direct language of the farmer, told of near miraculous results in the field which had already been accomplished; their authority was backed by the personnel of agricultural stations across the USSR. Who was to be believed? Remember that for Marxists the final test of any theory is the criterion of practice.

If practical results in the field were the test, then why wasn't the issue resolved in short order, especially since reports suggest that Soviet agricultural results were often disastrous? The answer is multifold but quite straightforward. Firstly, one has to understand the marginal nature of much Soviet agriculture (soil, temperature and rainfall). These geographic conditions produce vastly fluctuating agricultural yields, so that in the best of times the criterion of practice was difficult to put to the test, especially when enemies of the regime, within and without, were wont to sabotage agricultural reorganisation. The rapid collectivisation of agriculture would prove difficult in the best of circumstances. Thus, technical failures might be swallowed up in social and political crises. There have also been documented cases of fraudulent reportage of agricultural results, which are not surprising considering the political pressures which prevailed in the country.

Secondly, the actual agricultural production figures did not always confirm reports of disastrous yields. Problems of agricultural distribution may have been greater than those of production. Furthermore, there were undoubted agricultural successes. Whatever may be said of Lysenko's techniques, he was apparently extremely energetic. Successes may have been due in part simply to the introduction into a near feudal agriculture of new agronomical techniques, which had nothing to do with either Mendel or Michurin.

Finally, we must not forget that not all of Lysenko's techniques have been discredited. Vernalisation and specific techniques in plant physiology were quite favourably reviewed in East and West early in his career, and even today it is unclear just how valuable these techniques may be with regard to specific plants in specific conditions.

Read Reference 6.9: Lewontin & Levins, 'The conditions creating Lysenkoism'.

Questions for discussion

1. **Compare Lewontin & Levins' explanation of the rise of Lysenko with the more generally accepted view, i.e. that Lysenko was a charlatan foisted on the scientific community for ideological reasons.**

2. **How do you account for the failure of Western geneticists to accept or even to consider the sorts of issues raised by Lewontin & Levins?**

Even as late as 1964 Khrushchev evinced his own belief in the effectiveness of Lysenkoism in the field: 'Who wishes to use Lysenko's method cannot lose. Go this year and look at his wheat. I am sure that as always he will have a good crop. Look at the corn on his farm, look at the sugar beets ... It is from such scientists that we can learn' (Khrushchev, Feb. 1964, quoted in Dobzhansky, 'Soviet biology and the powers that were', *Science*, June 1969.) A few months after making these remarks, Khrushchev was to fall from power, due in significant measure to the perceived failure of his agricultural policies. In the next reading Lecourt shows how apparent success in the field helped Lysenko's rise just as apparent failure in the field ensured his fall.

Read Reference 6.10: Lecourt, 'The criterion of practice'.

Questions for discussion

1 **Lecourt's ideological position is described in this section's bibliography. Before looking there, can you speculate on what his general commitments are likely to be? Do you find Lecourt's account to be 'balanced' in spite of his acknowledged commitments? Consider the same question for other authors you have read on the subject.**

2 **What factors does Lecourt cite as significant in understanding the rise of Lysenko, his continued status and his ultimate fall?**

3 **A movement for the institutional reform of science.**

In the early days of the Lysenkoist movement, it embodied a program for the reform of scientific institutions which stemmed from what Ravetz (p.10) described as the 'Romantic' dimension of the scientific tradition: anti-elitest, participatory, suspicious of experts, focused on practice, and decentralised. This interesting phase with its 'hut labs' and 'barefoot professores' has never found a definitive historian. A similar movement during the Cultural Revolution in China was much more wide ranging and systematically applied than it had been in the Soviet Union. (See the book *Red and Expert*, HUS101 Knowledge and Power.) Most importantly, by the time Lysenko came to power, all vestiges of a Russian cultural revolution in science seemed to have been left behind. Lysenkoists simply took over the old hierarchical institutions which wielded power through the scientific and agricultural bureaucracies.

In thinking about this failed and very preliminary attempt to institute a cultural revolution in Soviet science, it is useful to keep in mind the three model scientifc communities that we examined in *Red and Expert*. The first, the Mertonian mode, approximates the view of science traditionally held by scientists in an academic context and given lip service by most working scientists. This mode is generally prescriptive rather than descriptive, but comes closest to realisation in the institutional setting of the university. Even there, actual behaviour of scientists often departs significantly from the normative standards laid down.

The second organisational mode, the bureaucratic, is found in labs associated with government and industrial hierarchies. Soviet science, as it has come to be, is perhaps the classic expression of this approach. This mode also may be seen to apply in significant and increasing degree to science in the West, even to that small rump of science which is still done in the universities. (For further reading on this 'industrial' or bureaucratic mode, see Ravetz's *Scientific Knowledge and its Social Problems*, 1971, and Dickson's work *The New Politics of Science*.)

Finally, the cultural reform mode is that envisaged, but never fully practiced, during the ten years of Cultural Revolution in China. The cultural reform of Chinese science of course failed, in large part due to the insensitive and inflexible leadership provided by the 'Gang of Four', whose excesses have been rightly criticised by the current Chinese regime.

The following reading, the final section of Wersky's review of Joravsky, gives a brief and tantalising sketch of some of the reform ideas proposed in the USSR, especially in the period sometimes called the second revolution, commencing in 1928.

Read Reference 6.5: Wersky, 'Science and ideology in the Soviet Union.' (pp.243-5)

Questions for discussion

1 **Review the detailed description of the three modes (discussed in *Red and Expert*, pp.24-8). Do you believe that Lysenko was a representative of the bureaucratic or of the reform mode?**

2 **Which mode was envisioned by Western geneticists? Describe ways in which their own activity fell short of the ethos to which they subscribed.**

3 **Compare the moves toward cultural reform in China and the USSR.**

As a final reading for this section, two biologists with Marxist commitments discuss the prospects for integrating their science and their Marxism, having learned the 'lessons of Lysenko'.

Read Reference 6.11: Lewontin & Levins, 'Can there be a Marxist science?'

Questions for discussion

1 **Several writers of both left and right, which you have encountered in the readings, have ridiculed the idea that there could be 'two sciences': bourgeois and proletarian. Discuss criticisms that you believe those writers would make of Lewontin & Levins' proposals.**

2 **Can you relate the 'two sciences' idea to the three modes discussed above?**

3 **Return to the first discussion question on p.5. Reassess your response in the light of the reading you have completed.**

ANNOTATED BIBLIOGRAPHY

Note on background reading

Although there exists a vast amount of secondary literature on the rise of Lysenko under Stalin (much of it written in a period of somewhat frenzied anti-Stalinist sentiment which inhibited proper weighing of the social and economic realities), events and developments of the last two decades have highlighted much of importance about the Soviet system, about socialist theory, and the nature of science itself. These new perspectives have been offered by a series of commentators with varying interests, competence, and political points of view. No one of these treatments offers a fully satisfactory account of the Lysenkoist movement; neither do they address all of the questions of relevance to students of this course. The readings printed with this unit attempt to overcome this problem, but nothing can take the place of a good general and detailed account of the chronological events. Therefore, the student is strongly advised, though not required, to obtain and keep at hand for reference one of the several general histories of the period noted by asterisk in the list below.

Adams, M. 'Towards a synthesis: population concepts in Russian evolutionary thought'. *Journal of the History of Biology*, Spring 1970, pp.107-29.

Adams, M. 'Biology after Stalin'. *Survey*, vol.23, 1977/78, pp.53-80.

Ashby, E. *Scientist in Russia*. Penguin, Harmondsworth, 1947.

Dickson, D. The New Politics of Science. Pantheon Books, New York, 1984.

Dobzhansky, T. 'Soviet biology and the powers that were'. *Science*, June, 1969, pp.1507-09.

Graham, L. *Science and Philosophy in the Soviet Union*. Vintage Books, New York, 1974.

Graham, L. *Between Science and Values*. Columbia University Press, New York, 1981.

Hardin, G. *Nature and Man's Fate*. Rinehart, New York, 1959.

Hudson, P.S., & Richens, R.H. *The New Genetics in the Soviet Union*. Imperial Bureau of Plant Breeding and Genetics, Cambridge, UK, 1946.
One of the earliest Western attempts to report fully and critically on the theories and techniques of Lysenko and Michurin.

Huxley, J. *Soviet Genetics and World Science*. Chatto & Windus, London 1949.

*Joravsky, D. *The Lysenko Affair*. Harvard University Press, Cambridge, Mass., 1970.
This book is the best researched, most scholarly and the most comprehensive telling of the Lysenko story. Joravsky is, however, wildly and unrelentingly anti-Marxist and anti-Soviet; furthermore, he is fully as guilty of name-calling and invective as are the Lysenkoists whom he so deplores. Unfortunately, Joravsky's naively positivist view of the nature of science and his simplistic theoretical analysis of the interactions of science and society leave him, as he himself admits, unable to provide rational explanations for the phenomena he describes so well. This is all the more remarkable in a book which so definitively provides the historical details

relevant to such explanation. Indeed, few books written in the history of science have better brought to light the interpenetrations of the scientific and the social spheres. More on Joravsky's book appears in Gary Wersky's review. (see Reference 6.5).

*Lecourt, D. *Proletarian Science?: The Case of Lysenko.* New Left Books, London, 1976.
This is the history of the Lysenkoist movement told from a Marxist standpoint. Obviously the Lysenko case presents particular difficulties for Marxists, and Lecourt faces many of these directly. On the other hand, the author fails to confront basic issues relating to the nature of science and technology, though his text provides a view of the political economy of the period in an ideological context especially germane to those issues. It is not recommended for students without some background or interest in Marxist ideology.

Leiss, W. *The Domination of Nature.* Braziller, New York, 1972.

Lenin Academy of Agricultural Sciences of the USSR. *The Situation in Biological Science.* Foreign Languages Publishing House, Moscow, 1949.

*Medvedev, Z.A. *The Rise and Fall of T.D. Lysenko.* Columbia University Press, New York, 1969.
As a primary source this is the most useful of all the general histories. It was written by a Soviet biologist now living in exile, who, though never in the camp of the Lysenkoists, retains some sympathy for and understanding of socialist goals and objectives. For this reason, he is less inclined to dismiss the controversial events which he witnessed simply as defects of the socialist system, as irrational ideological fervour, or as the evils of totalitarianism. He provides a clear chronological account of the personalities, their actions, and their political stratagems; his analysis of the issues relating to the nature of science is lacking in sophistication, though his understanding of the specific biological theories and principles is unfailingly clear and easily accessible.

Ravetz, J.R. *Scientific Knowledge and its Social Problems.* Oxford University Press, New York, 1971.

Roll-Hansen, N. 'A new perspective on Lysenko?'. *Annals of Science*, vol.42 no.3, 1985, pp.261-78.

Rostand, J. *Error and Deception in Science.* Basic Books, New York, 1960.

Vucinich, A. *Science in Russian Culture, 1861-1917.* Stanford University Press, Stanford, 1970.

Vucinich, A. *Empire of Knowledge: The Academy of Sciences of the USSR (1917-1970)*, University of California Press, Berkeley, 1984.

Weiner, D.R. 'The roots of "Michurinism": transformist biology and acclimatisation as currents in the Russian life sciences'. *Annals of Science*, vol.42, no.3, 1985, pp.243-60.

Wersky, G. *The Visible College.* Allen Lane, London, 1978.

Young, B. 'Evolutionary biology and ideology: then and now'. *Science Studies*, vol.1 1971, pp.177-206.

Young, B. 'Getting started on Lysenkoism'. *Radical Science Journal*, 6/7, pp.82-105.

Zirkle, C. (ed.). *Death of a Science in Russia*. University of Pennsylvania Press, 1949.
This book, and a number of others of this vintage (e.g. Huxley's *Soviet Genetics and World Science*), are by now very dated in their information and perspective. This volume provides translations of various documents relevant to the 1948 meeting, which though highly selective are considerably more useful than Zirkle's commentary.

7

BOOK STUDY OF *OUR ORIGINAL AGGRESSION*

Prepared for the course team by Lyndsay Farrall

Book Study of '*Our Original Aggression*'

Introduction

Noel Butlin's book, *Our Original Aggression* (George Allen & Unwin, Sydney, 1983) is an important study of the size of the Australian Aboriginal population at the time of Britain's colonisation of Australia. The book is concerned with two major questions:

1 What was the 'size of the Aboriginal population in 1788'?
2 What was 'the process of depopulation of Australian blacks' in south-eastern Australia during the period 1788-1850? (see Butlin p.5).

Butlin concludes that the Aboriginal population of south-eastern Australia in 1788 was at least five times greater than the most common previously accepted estimate. He also argues that the depopulation of blacks took place at a faster rate and in a more devastating fashion than most previous writers have concluded. Although these may seem to be reasonably obscure questions of interest only to scholars and experts, Butlin's findings have provoked heated debate in the daily newspapers.

Your first task in this 'book study' is to write a 750-1000 word book review of *Our Original Aggression* (see Essay Questions at the beginning of this unit). The major aim of this review should be to describe the arguments and conclusions of the book. Secondary aims should be to indicate the significance of the book and to assess the quality of its arguments and conclusions. The book review will contribute towards your assessment for this section. In addition, assessment for this book study will be based on one of the essays listed at the conclusion of the study guide. It is suggested that you turn to these questions to familiarise yourself with possible assignment topics so that you will have these in mind as you read the book and write your book review.

Reading *Our Original Aggression* as Part of HUS308 *Science in Culture.*

We are frequently told not to judge a book by its cover. What about its title? Note the title of the book—'*Our Original Aggression*'—followed by the subtitle—'*Aboriginal Populations of Southeastern Australia 1788-1850*'. What are your reactions to the title and subtitle? You may want to note down these reactions now so that you can compare them with your views when you have finished reading the book and then ask yourself how apt the title and subtitle are.

Our Original Aggression has become the subject of a certain amount of public controversy. Before you read the book try to imagine why this should be so. How would a book about 'Aboriginal Population of Southeastern Australia 1788-1850' become controversial? When you read the book you may care to keep a list of points which you think are potentially controversial, together with a list of the kinds of reader to whom they would appear controversial.

Butlin's book is a suitable case study for students of *Science in Culture* for a variety of reasons. It is directly related to a number of the questions raised in *Interpreting the Australian Environment*, the last book of HUS204 *Nature and Human Nature.* In particular, it examines the relationship between Australian Aborigines and their natural environment and asks how that relationship was changed by the arrival of white settlers. So the book can be seen as a follow-up to some of the materials that many of you will have studied before. However, in this section, the book is treated more as an example in the making and using of knowledge, themes which are the central concern of *Science in Culture.*

Our Original Aggression is the outcome of research originally initiated because its author could not find a satisfactory estimate of the Aboriginal population in the period before 1860 (see Butlin, p.xii). This research, in turn, was part of an overall project in Australian economic history to estimate national income during the period 1788-1860. The collection of such economic statistics needs the best possible estimate of population sizes and their rates of change. For an economic historian, knowledge about population size is a crucial aspect of the knowledge with which they work. In this context, Butlin's book can be viewed as an attempt to make a major revision to Australian economic history.

One of the major theses examined in *Science in Culture* is the thesis that (scientific) knowledge is socially constructed. (See 'Knowledge Making: An Introduction', (Section 1 in HUS306 *Knowledge Making*, Science in Culture.) In this section you are asked to look at the making of *Our Original Aggression* and at the controversy surrounding it from the point of view of the social constructivist thesis.

The social constructivist thesis can be thought of as involving:

1. scientists or scholars putting forward knowledge claims which arise out of the particular circumstances and lives of their authors;
2. the knowledge claims being put forward becoming one among a variety of knowledge claims competing for recognition as authoritative knowledge;
3. knowledge claims being tested against supposedly universal criteria, methods or values which will influence whether or not the claims are accepted and become authoritative in either the scholarly (scientific) or general community.

The social constructivist thesis of knowledge can be applied to *Our Original Aggression* by looking at the particular circumstances in which it was written, by examining the various contexts in which its claims about Aboriginal population are being debated, and by referring to the criteria, methods and values brought to bear on its knowledge claims.

It has already been noted that Noel Butlin is an economic historian engaged in research on Australian economic history. Hence it is not surprising that he should be interested in knowledge about the size and changes in Australian Aboriginal populations. Nor is it surprising that Butlin should use both data and arguments from fields which have direct bearing on knowledge about populations, such as history of medicine, demography, economics and ecology. In fact, establishing the best estimate of the Australian Aboriginal population in 1788 or 1850 is a good example of a problem best solved by drawing on insights from a number of different disciplines and by using the writings of scholars with quite different backgrounds and interests.

Given the range of subject areas and disciplines from which Butlin draws, there is considerable room for dispute about his findings. Not only might his knowledge claims be of considerable interest to workers in the fields already mentioned, but others in Aboriginal studies, anthropology, archaeology, Australian history and sociology might also be interested in the claims made and the implications of such claims for their own understanding of Australian Aboriginal peoples.

All of these academic considerations need to be put into two more general contexts. Firstly, knowledge claims in any area are characteristically judged by theories and ideas about knowledge which are the subject matter of the academic speciality known as 'epistemology'. Your earlier studies in the social studies of science will have made you familiar with the fact that there are several such competing theories. Secondly this pluralism in epistemology is matched by a pluralism in values, systems of belief and political ideologies in contemporary Australian society.

The historical study of Aboriginal population size is important for many Australians who might not otherwise be interested in such a scholarly activity because the findings of such studies can be used in political, economic, ethical and religious debates about Aboriginal land rights and related issues. The pluralist political and value commitments of contemporary Australians combine with the pluralist epistemological commitments of contemporary Australian scholars to provide plenty of scope for discussion, debate, argument and controversy relating to this particular 'struggle for authority' among competing knowledge claims.

Our Original Aggression and The Struggle For Authority

Butlin's challenge to accepted authority

Butlin's book is a challenge to the previously accepted estimates of Aboriginal population and of the rate and process of depopulation. A widely accepted estimate of the Australian Aboriginal population before 1788 has been that of A.R. Radcliffe Brown who summarised his reasons for arriving at an estimate of 250 000–300 000 in the *Australian Year Book* of 1930. One of Butlin's reasons for reassessing Radcliffe Brown's estimate was his knowledge of the debate about American Indian population estimates before European settlement in the Americas. References 7.1 and 7.2 should be read to get a sense of what acceptable knowledge claims in this field were before Butlin's work.

It is worth noting that Butlin's questioning of a previously accepted estimate is in accord with the scholarly and scientific precept that all accepted knowledge claims should be examined or tested and, if found wanting, replaced by more acceptable knowledge claims. However, it should also be noted that in this case an economic historian is challenging the findings of an anthropologist on the basis of work done in a variety of disciplines both inside and outside Australia. When you read these three accounts about Aboriginal population estimates, try to establish for yourself why the three authors would see this problem as an important one and why they would regard themselves as having sufficient expertise to throw light on the problem. Apart from questions of personal motivation, think about the social and intellectual contexts in which the various authors lived and worked. Ask yourself how a social constructivist might account for the origins of the knowledge claims being made in Butlin's book given its explicitly acknowledged connections to the prior accounts of Radcliffe Brown and Dobyns.

Read Reference 7.1: Radcliffe Brown, 'Former numbers and distribution of the Australian Aborigines'.

Read Reference 7.2: Dobyns, 'An appraisal of techniques with a new hemispheric estimate'.

The struggle for authority among scholars

The study of Australian Aboriginal culture has probably increased significantly in the last twenty to thirty years. It has certainly become of interest to various academic disciplines which previously ignored it. So, for example, historians of all kinds have become much more interested in Aboriginal history, an interest which has seen the recent establishment of a journal entitled *Aboriginal History*. Similarly, prehistorians and archaeologists who previously might have concentrated their research on the ancient civilisations of the Middle East, Europe and Asia have now published a significant number of studies on Australian Aboriginal cultures. Aboriginal studies have come to interest scholars across the range of the humanities, social sciences and natural sciences.

This increase of scholarly interest in Aboriginal studies has meant that there is a new pluralism in Aboriginal studies. Researchers trained in different disciplines bring different methods and different commitments not only to different problems but also to the same problems. The total size of the Aboriginal population in the 18th century and the size and causes of the subsequent depopulation are topics of crucial interest to many scholars in many different areas. As you read Butlin's book you might like to make a list of all the different disciplines from which he draws or to which he refers.

The scholarly reaction to Butlin's book is represented in the readings by a dispute between him and Charles Wilson, professor of history. You should work your way through References 7.3, 7.4 and 7.5 with the social constructivist thesis and the associated concept of struggle for authority in mind. However, References 7.6, 7.11 and 7.12 show that there are also scholars who accept Butlin's conclusions quite wholeheartedly. As you read these references, try to discern, as social constructivism suggests, the appropriate contexts in which knowledge claims are made, disputed and accepted. Try also to identify the particular interests and backgrounds against which the knowledge claims of individuals need to be understood.

Read Reference 7.3: Wilson, 'Smallpox and Aboriginal genocide',

Read Reference 7.4: Butlin 'Aboriginal depopulation'.

Read Reference 7.5: Wilson, 'A Rejoinder'.

Read Reference 7.6: 'More Evidence of Aboriginal "Settlements'".

The struggle for authority in the public arena

Butlin's findings have been challenged in the public arena by Mr Hugh Morgan, executive director of Western Mining Corporation. It is clear from Morgan's statements that he connects Butlin's historical knowledge claims with contemporary political issues, particularly Aboriginal land rights claims.

As you read References 7.7, 7.8, 7.9 and 7.10, you should ask what connections Hugh Morgan sees between the *domain of meaning* and the *domain of power*. Why does he think that certain knowledge claims about the history of Aboriginal depopulation (claims in the domain of meaning) have direct connections with contemporary political issues (activities in the domain of power)? Think of two or three other individuals or groups who are likely to take up a particular stance on land rights and ask yourself whether they would see similar connections to those seen by Hugh Morgan.

Read Reference 7.7: Flanagan, 'Our past being falsified, mining chief claims'.

Read Reference 7.8: Morgan, 'A day to remember realities of history'.

Read Reference 7.9: Morgan, 'Hugh Morgan's reply'.

Read Reference 7.10: Sheehan, 'The right strikes back'.

The Age found no difficulty in providing its readers with views which strongly opposed those put forward by Hugh Morgan. References 7.11 and 7.12 provide examples of such views. Reading these references should make it clear that the Australian academic community is not insulated from the debates that go on in our society about politics, ethics and human values. In one sense the pluralism of the scholarly community in Australia is a microcosm of the pluralist nature of Australian society. As you read these debates ask yourself whether the pluralism of society needs a matching relativism in scholarly debates.

Read Reference 7.11: Flanagan, 'Historians disagree with Morgan'.

Read Reference 7.12: Attwood, 'Phillip's conduct towards blacks was not exemplary'.

READINGS

1

DOING TIME IN MODERN SOCIETY

Karl Marx

1.2

SOME CONSEQUENCES OF THE DIVISION OF LABOUR

In the towns, the division of labour between the individual 40
guilds was as yet quite natural, and, in the guilds themselves, not at all developed between the individual workers. Every workman had to be versed in a whole round of tasks, had to be able to make everything that was to be made with his tools. The limited commerce and the scanty communication between the individual towns, the lack of population and the narrow needs did not allow of a higher division of labour, and therefore every man who wished to become a master had to be proficient in the whole of his craft. Thus there is found with medieval craftsmen an interest in their special work and a proficiency in it, which was capable of rising to a narrow artistic sense. For this very reason, however, every medieval craftsman was completely absorbed in his work, to which he had a contented, slavish relationship, and to which he was subjected to a far greater extent than the modern worker whose work is a matter of indifference to him.

The personal relationship of the craftsman to his product first began to change with the manufacturing systems, in which division of labour began to predominate. But the later introduction of machinery led to a radical depersonalization of the worker.

In the first place, in the form of machinery, the implements of labour become automatic, things moving and working independent of the workman. They are thenceforth an industrial *perpetuum mobile* that would go on producing forever, it did not meet with certain natural obstructions in the weak bodies and the strong wills of its human attendants.

Along with the tool, the skill of the workman in handing it
passes over to the machine. The capabilities of the tool are 41
emancipated from the restraints that are inseparable from human labour-power. Thereby the technical foundation on which is based the division of labour in Manufacture, is swept away. Hence, in the place of the hierarchy of specialized workmen that characterizes manufacture, there steps, in the automatic factory, a tendency to equalize and reduce to one and the same level every kind of work that has to be done by the minders of the machines, in the place of the artificially produced differentiations of the detail workmen, step the natural differences of age and sex.

In handicrafts and manufacture, the workman makes use of a tool, in the factory, the machine makes use of him. There the movements of the instrument of labour proceed from him, here it is the movements of the machine that he must follow. In manufacture the workmen are parts of a living mechanism. In the factory we have a lifeless mechanism independent of the workman, who becomes its mere living appendage . . .

At the same time that factory work exhausts the nervous system to the uttermost, it does away with the many-sided play of the muscles, and confiscates every atom of freedom, both in bodily and intellectual activity. The lightening of the labour, even, becomes a sort of torture, since the machine does not free the labourer from work, but deprives the work of all interest... The special skill of each individual insignificant factory operative vanishes as an infinitesimal quantity before the science, the gigantic physical forces, and the mass of labour that are embodied in the factory mechanism and, together with that mechanism, constitute the power of the 'master'...

The technical subordination of the workman to the uniform motion of the instruments of labour, and the peculiar composition of the body of workpeople, consisting as it does of individuals of both sexes and of all ages, give rise to a barrack discipline, which is elaborated into a complete system in the factory, and which fully develops the beforementioned labour of overlooking, thereby dividing the workpeople into operatives and overlookers, into private soldiers and sergeants of an industrial army.

K. Marx, annotated by E. Fischer, in collaboration with F. Marek, *Marx in His Own Words*, tr. A. Bostock, Penguin Books, Harmondsworth, 1970, pp. 40–1.

Benjamin Franklin 1.4

A PROJECT OF MORAL PERFECTION

It was about this time I conceiv'd the bold and arduous project of 78
arriving at moral perfection. I wish'd to live without committing any fault at any time; I would conquer all that either natural inclination, custom, or company might lead me into. As I knew, or thought I knew, what was right and wrong, I did not see why I might not always do the one and avoid the other. But I soon found I had undertaken a task of more difficulty than I had imagined. While my care was employ'd in guarding against one fault, I was often surprised by another; habit took the advantage of inattention; inclination was sometimes too strong for reason. I concluded, at length, that the mere speculative conviction that it was our interest to be completely virtuous, was not sufficient to prevent our slipping; and that the contrary habits must be broken, and good ones acquired and established, before we can have any dependence on a steady, uniform rectitude of conduct. For this purpose I therefore contrived the following method.

In the various enumerations of the moral virtues I had met with in my reading, I found the catalogue more or less numerous, as different writers included more or fewer ideas under the same name. Temperance, for example, was by some confined to eating and drinking, while by others it was extended to mean the moderating every other pleasure, appetite, inclination, or passion, bodily or mental, even to our avarice and ambition. I propos'd to myself, for the sake of clearness, to use rather more names, with fewer ideas annex'd to each, than a few names with more ideas; and I included under
thirteen names of virtues all that at that time occurr'd to me as 79
necessary or desirable, and annexed to each a short precept, which fully express'd the extent I gave to its meaning.

These names of virtues, with their precepts, were:

1. TEMPERANCE.

Eat not to dullness; drink not to elevation.

2. SILENCE.

Speak not but what may benefit others or yourself; avoid trifling conversation.

3. ORDER.

Let all your things have their places; let each part of your business have its time.

4. RESOLUTION.

Resolve to perform what you ought; perform without fail what you resolve.

5. Frugality.

Make no expense but to do good to others or yourself; *i. e.,* waste nothing.

6. Industry.

Lose no time; be always employ'd in something useful; cut off all unnecessary actions.

7. Sincerity.

Use no hurtful deceit; think innocently and justly, and, if you speak, speak accordingly.

8. Justice.

Wrong none by doing injuries, or omitting the benefits that are your duty.

9. Moderation.

Avoid extreams; forbear resenting injuries so much as you think they deserve.

80 10. Cleanliness.

Tolerate no uncleanliness in body, cloaths, or habitation.

11. Tranquillity.

Be not disturbed at trifles, or at accidents common or unavoidable.

12. Chastity.

Rarely use venery but for health or offspring, never to dulness, weakness, or the injury of your own or another's peace or reputation.

13. Humility.

Imitate Jesus and Socrates.

My intention being to acquire the *habitude* of all these virtues, I judg'd it would be well not to distract my attention by attempting the whole at once, but to fix it on one of them at a time; and, when I should be master of that, then to proceed to another, and so on, till I should have gone thro' the thirteen; and, as the previous acquisition of some might facilitate the acquisition of certain others, I arrang'd them with that view, as they stand above. Temperance first, as it tends to procure that coolness and clearness of head, which is so necessary where constant vigilance was to be kept up, and guard maintained against the unremitting attraction of ancient habits, and the force of perpetual temptations. This being acquir'd and establish'd, Silence would be more easy; and my desire being to gain knowledge at the same time that I improv'd in virtue, and considering that in conversation it was obtain'd rather by the use of the ears than of the tongue, and therefore wishing to break a habit I was getting into of prattling, punning, and joking, which only made me acceptable to trifling company, I gave *Silence* the second place. This and the next, *Order,* I expected would allow me more time for attending to my project and my studies. *Resolution,* once become habitual, would keep me firm in my endeavors to obtain all the subsequent virtues; *Frugality* and Industry freeing me from my remaining debt, and producing affluence and independence,
81 would make more easy the practice of Sincerity and Justice, etc., etc. Conceiving then, that, agreeably to the advice of Pythagoras in his Golden Verses, daily examination would be necessary, I contrived the following method for conducting that examination.

I made a little book, in which I allotted a page for each of the virtues. I rul'd each page with red ink, so as to have seven columns, one for each day of the week, marking each column with a letter for the day. I cross'd these columns with thirteen red lines, marking the beginning of each line with the first letter of one of the virtues, on which line, and in its proper column, I might mark, by a little black spot, every fault I found upon examination to have been committed respecting that virtue upon that day.

Form of the pages.

TEMPERANCE.							
EAT NOT TO DULNESS; DRINK NOT TO ELEVATION.							
	S.	M.	T.	W.	T.	F.	S.
T.							
S.	•	•		•		•	
O.	• •	•	•		•	•	•
R.			•			•	
F.		•			•		
I.			•				
S.							
J.							
M.							
C.							
T.							
C.							
H.							

• • •

The precept of *Order* requiring that *every part of my business should have its allotted time,* one page in my little book contain'd the following scheme of employment for the twenty-four hours of a natural day: 83

THE MORNING. *Question.* What good shall I do this day?	5, 6, 7	Rise, wash, and address *Powerful Goodness!* Contrive day's business, and take the resolution of the day; prosecute the present study, and breakfast.
	8, 9, 10, 11	Work.
NOON.	12, 1	Read, or overlook my accounts, and dine.
	2, 3, 4, 5	Work.
EVENING. *Question.* What good have I done to-day?	6, 7, 8, 9	Put things in their places. Supper. Music or diversion, or conversation. Examination of the day.
NIGHT.	10, 11, 12, 1, 2, 3, 4	Sleep.

B. Franklin, 'The Autobiography of Benjamin Franklin', in C. Eliot (ed.), *The Autobiography of Benjamin Franklin, The Journal of John Woolman, Fruits of Solitude: William Penn*, P. F. Collier & Son, New York, 1973, pp. 78–83.

Benjamin Franklin
POOR RICHARD'S TIME

1.5

People were collected at a Vendue of Merchant Goods. The Hour of 278
Sale not being come, they were conversing on the Badness of the Times, and one of the Company call'd to a plain clean old Man, with white Locks, *Pray, Father* Abraham, *what think you of the Times? Won't these heavy Taxes quite ruin the Country? How shall we be ever able to pay them? What would you advise us to?* – – – – Father *Abraham* stood up, and reply'd, If you'd have my Advice, I'll give it you in short, for a *Word to the Wise is enough*, and *many Words won't fill a Bushel*, as *Poor Richard says.* They join'd in desiring him to speak his Mind, and gathering round him, he proceeded as follows;[46]

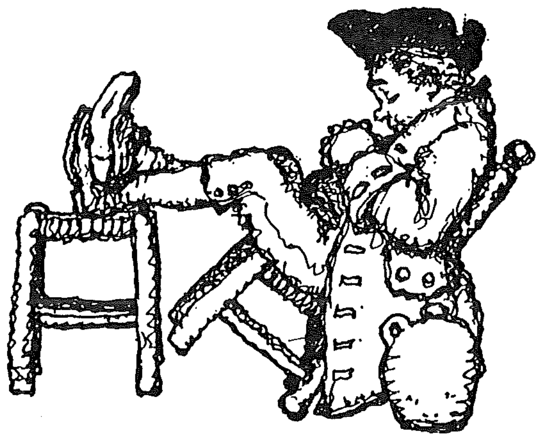
"Friends, says he, and Neighbours, the Taxes are indeed very heavy, and if those laid on by the Government were the only Ones we had to pay, we might more easily discharge them; but we have many others, and much more grievous to some of us. We are taxed twice as much by our *Idleness*, three times as much by our *Pride*, and four times as much by our *Folly*, and from these Taxes the Commissioners cannot ease or deliver us by allowing an Abatement. However let us hearken to good Advice, and something may be done for us; *God helps them that help themselves*, as *Poor Richard* says, in his Almanack of 1733.

It would be thought a hard Government that should tax its People one tenth Part of their *Time*, to be employed in its Service. But *Idleness* taxes many of us much more, if we reckon all that is spent in absolute *Sloth*, or doing of nothing, with that which is spent in idle Employments or Amusements, that amount to nothing. *Sloth*, by bringing on Diseases, absolutely shortens Life. *Sloth, like Rust, consumes faster than Labour wears, while the used Key is always bright*, as *Poor Richard* says. But *dost thou love Life, then do not squander Time, for that's the Stuff Life is made of*, as *Poor Richard* says. – – – – How much more than is necessary do we spend in Sleep! forgetting that *The sleeping Fox catches no Poultry*, and that *there will be sleeping enough in the Grave*, as *Poor Richard* says. If Time be of all Things the most precious, *wasting Time* must be, as *Poor Richard* says, *the greatest Prodigality*, since, as he elsewhere tells us, *Lost Time is never found again*; and what we call *Time-enough, always proves little enough.* Let us then up and be doing, and doing to the Purpose; so by Diligence shall we do more with less Perplexity. *Sloth makes all Things difficult, but Industry all easy*, as *Poor Richard* says; and *He that riseth late, must trot all Day, and shall scarce overtake his Business at Night.* While *Laziness travels so slowly, that*

279 *Poverty soon overtakes him*, as we read in *Poor Richard*, who adds, *Drive thy Business, let not that drive thee*; and *Early to Bed, and early to rise, makes a Man healthy, wealthy, and wise.*

. . .

232



. . .

So what signifies *wishing* and *hoping* for better Times. We may make these Times better if we bestir ourselves. *Industry need not wish*, as *Poor Richard* says, and *He that lives upon Hope will die fasting. There are no Gains, without Pains*; then *Help Hands, for I have no Lands*, or if I have, they are smartly taxed. And, as *Poor Richard* likewise observes, *He that hath a Trade hath an Estate*, and *He that hath a Calling hath an Office of Profit and Honour*; but then the *Trade* must be worked at, and the *Calling* well followed, or neither the *Estate*, nor the *Office*, will enable us to pay our Taxes. - - - - If we are industrious we shall never starve; for, as *Poor Richard* says, *At the working Man's House* Hunger *looks in, but dares not enter*. Nor will the Bailiff or the Constable enter, for *Industry pays Debts, while Despair encreaseth them*, says *Poor Richard*. - - - - What though you have found no Treasure, nor has any rich Relation left you a Legacy, *Diligence is the Mother of Good-luck*, as *Poor Richard* says, *and God gives all Things to Industry*. Then *plough deep, while Sluggards sleep, and you shall have Corn to sell and to keep*, says *Poor Dick*. Work while it is called To-day, for you know not how much you may be hindered To-morrow, which makes *Poor Richard* say, *One To-day is worth two To-morrows*; and farther, *Have you somewhat to do To-morrow, do it To-day*. If you were a Servant, would you not be ashamed that a good Master should catch you idle? Are you then your own Master, *be ashamed to catch yourself idle*, as *Poor Dick* says. When there is so much to be done for yourself, your Family, your Country, and your gracious King, be up by Peep of Day; *Let not the Sun look down and say, Inglorious here he lies*. Handle your Tools without Mittens; remember that *the Cat in Gloves catches no Mice*, as *Poor Richard* says. 'Tis true there is much to be done, and perhaps you are weak handed, but stick to it steadily, and you will see great Effects, for *constant Dropping wears away Stones*, and by *Diligence and Patience the Mouse ate in two the Cable*; and *little Strokes fell great Oaks*, as *Poor Richard* says in his Almanack, the Year I cannot just now remember.

Methinks I hear some of you say, *Must a Man afford himself no Leisure?* - - - - I will tell thee, my Friend, what *Poor Richard* says, *Employ thy Time well if thou meanest to gain Leisure*; and, *since thou art not sure of a Minute, throw not away an Hour*. Leisure, is Time for doing something useful; this Leisure the diligent Man will obtain, but the lazy 280
Man never; so that, as *Poor Richard* says, a *Life of Leisure and a Life of Laziness are two Things*. Do you imagine that Sloth will afford you more Comfort than Labour? No, for as *Poor Richard* says, *Trouble springs from Idleness, and grievous Toil from needless Ease. Many without Labour, would live by their* WITS *only, but they break for want of Stock*. Whereas Industry gives Comfort, and Plenty, and Respect: *Fly Pleasures, and they'll follow you. The diligent Spinner has a large Shift*; and *now I have a Sheep and a Cow, every Body bids me Good morrow*; all which is well said by *Poor Richard.*

70

He that riseth late must trot all Day, and shall scarce overtake his Business at Night.

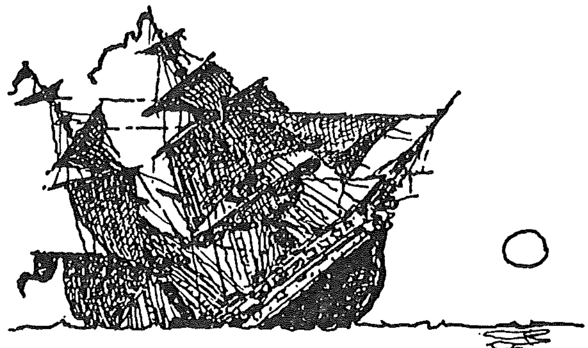
So much for Industry, my Friends, and Attention to one's own 281
Business; but to these we must add *Frugality*, if we would make our *Industry* more certainly successful. A Man may, if he knows not how to save as he gets, *keep his Nose all his Life to the Grindstone*, and die not worth a *Groat* at last. *A fat Kitchen makes a lean Will*, as *Poor Richard* says; and,

Many Estates are spent in the Getting,
Since Women for Tea forsook Spinning and Knitting,
And Men for Punch forsook Hewing and Splitting.

If you would be wealthy, says he, in another Almanack, *think of Saving as well as of Getting: The* Indies *have not made* Spain *rich, because her* Outgoes *are greater than her* Incomes. Away then with your expensive Follies, and you will not have so much Cause to complain of hard Times, heavy Taxes, and chargeable Families; for, as *Poor Dick* says,

Women and Wine, Game and Deceit,
Make the Wealth small, and the Wants great.

And farther, *What maintains one Vice, would bring up two Children.* You may think perhaps, That a *little* Tea, or a *little* Punch now and then, Diet a *little* more costly, Clothes a *little* finer, and a *little* Entertainment now and then, can be no *great* Matter; but remember what *Poor Richard* says, *Many a* Little *makes a Mickle*; and farther, *Beware of* little *Expences*; *a small Leak will sink a great Ship;* and again, *Who Dainties love, shall Beggars prove*; and moreover, *Fools make Feasts, and wise Men eat them.*



A small Leak will sink a great Ship.

. . .

283 And again, *Pride is as loud a Beggar as Want, and a great deal more saucy.* When you have bought one fine Thing you must buy ten more, that your Appearance may be all of a Piece; but *Poor Dick* says, *'Tis easier to* suppress *the first Desire, than to* satisfy *all that follow it.* And 'tis as truly Folly for the Poor to ape the Rich, as for the Frog to swell, in order to equal the Ox.

Great Estates may venture more,
But little Boats should keep near Shore.

'Tis however a Folly soon punished; for *Pride that dines on Vanity sups on Contempt*, as *Poor Richard* says. And in another Place, *Pride breakfasted with Plenty, dined with Poverty, and supped with Infamy.* And after all, of what Use is this *Pride of Appearance*, for which so much is risked, so much is suffered? It cannot promote Health, or ease Pain; it makes no Increase of Merit in the Person, it creates Envy, it hastens Misfortune.

What is a Butterfly? At best
He's but a Caterpillar drest.
The gaudy Fop's his Picture just,

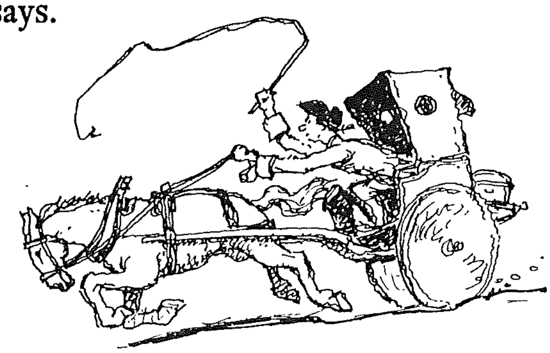
as *Poor Richard* says.

But what Madness must it be to *run in Debt* for these Superfluities! We are offered, by the Terms of this Vendue, *Six Months Credit*; and that perhaps has induced some of us to attend it, because we cannot spare the ready Money, and hope now to be fine without it. But, ah, think what you do when you run in Debt; *You give to another Power over your Liberty.* If you cannot pay at the Time, you will be ashamed to see your Creditor; you will be in Fear when you speak to him; you will make poor pitiful sneaking Excuses, and by Degrees come to lose your

Veracity, and sink into base downright lying; for, as *Poor Richard* says, *The second Vice is Lying, the first is running in Debt.* And again, to the same Purpose, *Lying rides upon Debt's Back.* Whereas a freeborn *Englishman* ought not to be ashamed or afraid to see or speak to any Man living. But Poverty often deprives a Man of all Spirit and Virtue: *'Tis hard for an empty Bag to stand upright*, as *Poor Richard* truly says. What would you think of that Prince, or that Government, who should issue an Edict forbidding you to dress like a Gentleman or a Gentlewoman, on Pain of Imprisonment or Servitude? Would you not say, that you are free, have a Right to dress as you please, and that such an Edict would be a Breach of your Privileges, and such a Government tyrannical? And 284
yet you are about to put yourself under that Tyranny when you run in Debt for such Dress! Your Creditor has Authority at his Pleasure to deprive you of your Liberty, by confining you in Gaol for Life, or to sell you for a Servant, if you should not be able to pay him! When you have got your Bargain, you may, perhaps, think little of Payment; but *Creditors*, *Poor Richard* tells us, *have better Memories than Debtors*; and in another Place says, *Creditors are a superstitious Sect, great Observers of set Days and Times.* The Day comes round before you are aware, and the Demand is made before you are prepared to satisfy it. Or if you bear your Debt in Mind, the Term which at first seemed so long, will, as it lessens, appear extreamly short. *Time* will seem to have added Wings to his Heels as well as Shoulders. *Those have a short Lent*, saith *Poor Richard*, *who owe Money to be paid at Easter.* Then since, as he says, *The Borrower is a Slave to the Lender, and the Debtor to the Creditor*, disdain the Chain, preserve your Freedom; and maintain your Independency: Be *industrious* and *free*; be *frugal* and *free*. At present, perhaps, you may think yourself in thriving Circumstances, and that you can bear a little Extravagance without Injury; but,

For Age and Want, save while you may;
No Morning Sun lasts a whole Day,

as *Poor Richard* says.



Lost Time is never seen again.

• • •

B. Franklin [Richard Saunders], *Poor Richard: The Almanacks for the Years 1733–1758*, ill. N. Rockwell, Limited Editions Club Edition, Paddington Press, New York, pp. 278–80, 281, 283–4.

G.J. Whitrow 1.6

TIME

THE INVENTION of mechanical clocks which could, 98
if properly regulated, tick away continually for years on end greatly influenced belief in the uniformity and continuity of time. These characteristics were implicit in the idea of physical time adopted by Galileo in the dynamical part of his famous *Discourses on Two New Sciences*, published in 1638. Although he was not the first to represent time by a geometrical straight line, he became the most influential pioneer of this idea through his theory of motion expounded in this book.

For the first explicit discussion of the concept of mathematical time we must go to the *Geometrical Lectures* of Isaac Barrow, written about thirty years after the publication of Galileo's book. His views on the nature of time are not only of great interest in themselves, but are important because of their influence on Newton, who succeeded him in the Lucasian chair of mathematics at Cambridge in 1669. Barrow maintained that, because mathematicians frequently make use of time, they ought to have a clear idea of the meaning of the word, for otherwise, he said, they are quacks! Although time is measurable by motion, he was careful to distinguish the two:

> Time denotes not an actual existence but a certain capacity or possibility for a continuity of existence;
> just as space denotes a capacity for intervening length. 99
> Time does not imply motion, as far as its absolute and intrinsic nature is concerned; not any more than it implies rest; whether things move or are still, whether we sleep or wake, Time pursues the even tenour of its way. Time implies motion to be measurable; without motion we do not perceive the passage of Time. We evidently must regard Time as passing with a steady flow; therefore, it must be compared with some handy steady motion, such as the motion of the stars and especially of the Sun and Moon.

Barrow pointed out that

> strictly speaking the celestial bodies are not the first and original measures of Time; but rather those motions, which are observed round about us by the senses and which underlie our experiments, since we judge the regularity of the celestial motions by the help of these. Not even is Sol himself a worthy judge of Time, or to be accepted as a veracious witness, except so far as time-measuring instruments attest his veracity by their votes.

Barrow regarded time as essentially a mathematical concept that has many analogies with a line, for it has length alone, is similar in all its parts and can be regarded either as a simple addition of successive instants or as the continuous flow of one instant. He thought it could be represented by, to quote his own words, 'either a straight or a circular line'. Although the reference here to 'a circular line' shows that Barrow was not completely emancipated from traditional ideas, his statement goes further than any of Galileo's, for Galileo used only straight-line segments to denote particular intervals of time. Barrow took care, however, not to push the analogy between time and a line too far. Time, in his view, was 'the continuance of anything in its own being', and in a passage to which reference will be made later, he remarked, 'nor do I believe there is anyone but allows that those things existed equal times which rose and perished together'.

Barrow's idea that, irrespective of 'whether things move or are still, whether we sleep or wake, Time pursues the even tenour of its way' is echoed in the famous definition at the beginning of Newton's *Principia* of 1687. 'Absolute, true and mathematical time', wrote Newton, 'of itself and from its own nature, flows equably without relation to anything external.' Newton admitted that in practice there may be no such thing as a uniform motion by which time may be accurately measured, but he thought it necessary that, in principle, there should exist an ideal rate-measurer of time. Consequently, he regarded the moments of absolute time as forming a continuous sequence like the points on a geometrical line, and he believed that the rate at which these moments succeed each other is independent of all particular events and processes.

Newton's idea of absolute time existing in its own right accords with the commonsense idea that most of us nowadays accept automatically when we try to think about time. We feel that time is something that can have neither beginning nor end and must continue independently of whatever happens. Newton's views made a great impression on the philosopher John

Locke, whose *Essay Concerning Human Understanding* was published in 1690, only three years after Newton's *Principia*. In it we find the clearest statement of the scientific conception of time that was evolved in the seventeenth century. Locke wrote:

> Duration is but as it were the length of one straight line extended *in infinitum*, not capable of multiplicity, variation or figure, but is one common measure of all existence whatever, wherein all things, whilst they exist, equally partake. For this present moment is common to all things that are now in being, and equally comprehends that part of their existence as much as if they were all but one single being; and we may truly say, they all exist in the same moment of time.

G. J. Whitrow, *What is Time?* Thames & Hudson, London, 1972, pp. 98–101.

Robert Ball

1.9(a)

ERNABELLA KEEPS UP WITH THE TIMES

SA's remote north-west corner is hardly the place one expects to find a giant revolving electrically operated clock atop a hill.

But, if the clock's soiltary existence is considered in the context of the local Pitjantjatjara people's needs, there is little reason for surprise.

The hill, is near the town centre of Ernabella in the Musgrave Ranges, about 1500 kilometres north-west of Adelaide and 450 kilometres south-west of Alice Springs.

The clock was erected about a year ago after the town's council — Pukatja — decided that the community needed a timepiece.

The clock has two basic roles.

The first is to educate people about time.

Knowledge of time is vital at Ernabella. Once nomadic, with little need for timetables, the inhabitants now run a productive town with pursuits ranging from market gardens to motor mechanic training all accompanied by well-defined work periods.

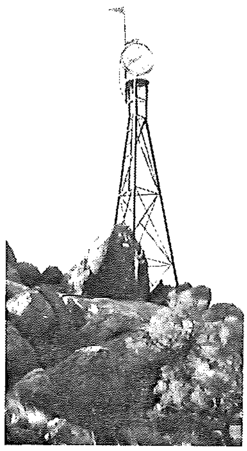
And the clock also serves, as do thousands of clocks in thousands of town squares around the world, simply to "tell the time."

Pukatja council commissioned a contractor from Moss Vale, NSW, to build the clock.

However, out in the bush time is, well, a "timeless thing."

Ernbella's "Big Ben" isn't quite in the same league as Adelaide's GPO clock with regular accuracy checks.

Last week "Big Ben" was running 20 minutes slow but nobody missed the bus.



Ernabella's "Big Ben."

R. Ball, 'Ernabella keeps up with the times', *The Advertiser*, 19 June 1978, p. 1.

Community Advisor, Pukatja Community

LETTER

1.9(b)

ERNABELLA
via Alice Springs
N.T. 5751

Telegrams. O.P.R. Alice Springs
Telephone. R.T. 367

24.6.84

Re: your request for information concerning the Ernabella Clock.

The clock is a waste of time! The idea seems to have been one of those "good ideas" that we white advisers have for Aboriginal communities. The clock was installed by a Uniting Church volunteer who brings work parties to Ernabella to fulfil projects that local expertise cannot handle. This project may have been one of those.

I have asked local people what they think of the clock. They look at me imploringly, saying with their eyes 'what do you think, is it a good idea?' If I indicate 'yes' by nuance of facial expression, they say, 'yes a good idea.' But if I do the opposite they say 'no, not a good idea.' The fact is that nobody looks at the clock. The clock has not been working for months. No-one knew that it was not working. The main reason for them not looking was because of unreliability. The clock runs on electricity which means that when we have a power failure the clock loses time. No-one has the time to correct the time and so it soon runs hours late.

For years the community used a siren system to tell people when it was work time or cup of tea time, etc. This was also found to be useless. Aboriginal people do not work by time. The imposition of European concepts of time in a traditional setting does not seem to work. People respond to needs and work by the sun and the climate; hot, windy, cold, humid, etc.

European staff use time and watches to regulate their activities but often they also work until the job is completed or it is too hot or cold or dark to continue.

This local adaptation to time is still going on. Things aren't as efficient now as they were in 1978. But that is because in those days the influence of the mission was much greater. Today as the rhetoric of self-management comes to mean more, Aboriginal people are increasingly adapting their style of work and life to the unique conditions prevailing in tribal traditional outback Aboriginal communities.

<u>Community Advisor</u>

Community Advisor, Pukatja Community, Letter concerning the Ernabella clock, 24 June 1984.

A.A. Milne

AN ENCHANTED PLACE

• • •

all – because we've heard, I mean we all know – well, 168
you see, it's – we – you – well, that, to put it as shortly as possible, is what it is." He turned round angrily on the others and said, "Everybody crowds round so in this Forest. There's no Space. I never saw a more Spreading lot of animals in my life, and all in the wrong places. Can't you *see* that Christopher Robin wants to be alone? I'm going." And he humped off.

Not quite knowing why, the others began edging away, and when Christopher Robin had finished reading POEM, and was looking up to say "Thank you," only Pooh was left.

"It's a comforting sort of thing to have," said Christopher Robin, folding up the paper, and putting it in his pocket. "Come on, Pooh," and he walked off quickly.

"Where are we going?" said Pooh, hurrying after him, and wondering whether it was to be an Explore or a What-shall-I-do-about-you-know-what.

"Nowhere," said Christopher Robin.

So they began going there, and after they had walked a little way Christopher Robin said:

"What do you like doing best in the world, Pooh?"

"Well," said Pooh, "what I like best —" and then he had to stop and think. Because although Eating Honey *was* a very good thing to do, there was a moment just before you began to eat it which was better than when
you were, but he didn't know what it was called. And 169
then he thought that being with Christopher Robin was a very good thing to do, and having Piglet near was a very friendly thing to have; and so, when he had thought it all out, he said, "What I like best in the whole world is Me and Piglet going to see You, and You saying 'What about a little something?' and Me saying, 'Well, I shouldn't mind a little something, should you, Piglet,' and it being a hummy sort of day outside, and birds singing."

"I like that too," said Christopher Robin, "but what I like *doing* best is Nothing."

"How do you do Nothing?" asked Pooh, after he had wondered for a long time.

"Well, it's when people call out at you just as you're going off to do it, 'What are you going to do, Christopher Robin?' and you say 'Oh, nothing,' and then you go and do it."

"Oh, I see," said Pooh.

"This is a nothing sort of thing that we're doing now."

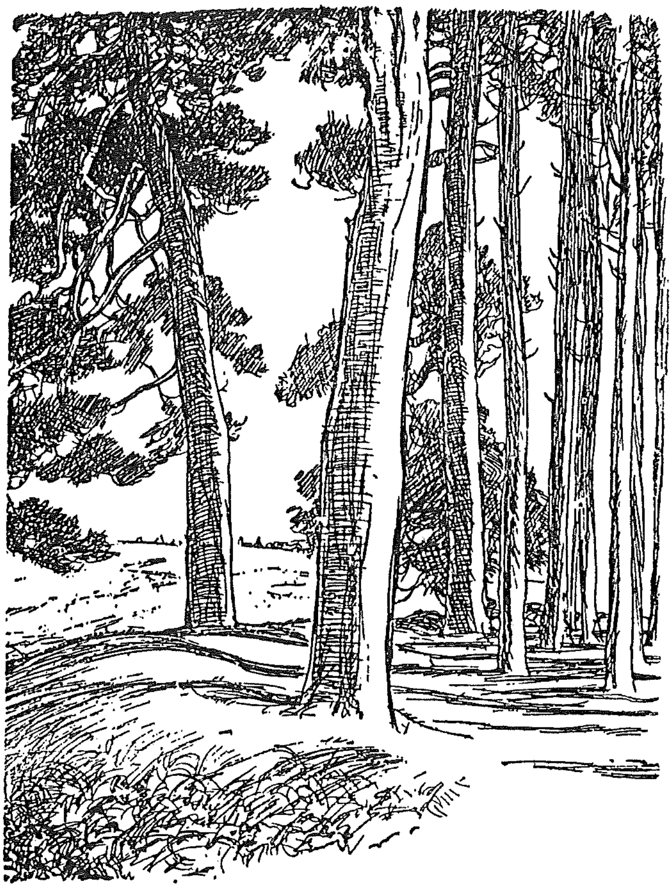
"Oh, I see," said Pooh again.

"It means just going along, listening to all the things you can't hear, and not bothering."

"Oh!" said Pooh.

They walked on, thinking of This and That, and by-and-by they came to an enchanted place on the very top of the Forest called Galleons Lap, which is sixty-something trees in a circle; and Christopher Robin

knew that it was enchanted because nobody had ever been able to count whether it was sixty-three or sixty-four, not even when he tied a piece of string round each tree after he had counted it. Being enchanted, its floor was not like the floor of the Forest, gorse and bracken and heather, but close-set grass, quiet and smooth and green. It was the only place in the Forest where you could sit down carelessly, without getting up again almost at once and looking for somewhere else. Sitting there they could see the whole world spread out until

 171

it reached the sky, and whatever there was all the world 172
over was with them in Galleons Lap.

Suddenly Christopher Robin began to tell Pooh about some of the things: People called Kings and Queens and something called Factors, and a place called Europe, and an island in the middle of the sea

where no ships came, and how you make a Suction Pump (if you want to), and when Knights were
Knighted, and what comes from Brazil. And Pooh, 173
his back against one of the sixty-something trees, and his paws folded in front of him, said "Oh!" and "I don't know," and thought how wonderful it would be to have a Real Brain which could tell you things. And by-and-by Christopher Robin came to an end of the things, and was silent, and he sat there looking out over the world, and wishing it wouldn't stop.

But Pooh was thinking too, and he said suddenly to Christopher Robin:

"Is it a very Grand thing to be an Afternoon, what you said?"

"A what?" said Christopher Robin lazily, as he listened to something else.

"On a horse?" explained Pooh.

"A Knight?"

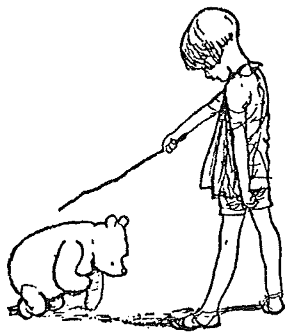
"Oh, was that it?" said Pooh. "I thought it was a — Is it as Grand as a King and Factors and all the other things you said?"

"Well, it's not as grand as a King," said Christopher Robin, and then, as Pooh seemed disappointed, he added quickly, "but it's grander than Factors."

"Could a Bear be one?"

"Of course he could!" said Christopher Robin. "I'll make you one." And he took a stick and touched Pooh on the shoulder, and said, "Rise, Sir Pooh de Bear, most faithful of all my Knights."

174



So Pooh rose and sat down and said "Thank you," which is the proper thing to say when you have been made a Knight, and he went into a dream again, in which he and Sir Pump and Sir Brazil and Factors lived together with a horse, and were faithful knights (all except Factors, who looked after the horse) to Good King Christopher Robin . . . and every now and then he shook his head, and said to himself, "I'm not getting it right." Then he began to think of all the things Christopher Robin would want to tell him when he came back from wherever he was going to, and how muddling it would be for a Bear of Very Little Brain to try and get them right in his mind. "So, perhaps," he said sadly to himself, "Christopher Robin won't tell me any more," and he wondered if being a Faithful Knight meant that you just went on being faithful without being told things.

175 Then, suddenly again, Christopher Robin, who was still looking at the world with his chin in his hands, called out "Pooh!"

"Yes?" said Pooh.

"When I'm – when — Pooh!"

"Yes, Christopher Robin?"

"I'm not going to do Nothing any more."
"Never again?"
"Well, not so much. They don't let you."

Pooh waited for him to go on, but he was silent again.
"Yes, Christopher Robin?" said Pooh helpfully.
"Pooh, when I'm – *you* know – when I'm *not* doing Nothing, will you come up here sometimes?"
"Just Me?"
"Yes, Pooh."
"Will you be here too?"

A. A. Milne, *The House at Pooh Corner*, Methuen Children's Books, London, 1978, pp. 168–75.

Roger Waters

TIME

REFERENCE 12

R. Waters, "Time", Dark Side of the Moon, Pink Floyd, EMI, 1973.

A contemporary expression of feelings about time is given by Roger Waters in Pink Floyd's song "Time". Try to get the record and listen to it.

Ticking away the moments that make up a dull day
You fritter and waste the hours in an off hand way
Kicking around on a piece of ground in your home town
Waiting for someone or something to show you the way

Tired of lying in the sunshine staying home to watch the rain
You are young and life is long and there is time to kill today
And then one day you find ten years have got behind you
No one told you when to run, you missed the starting gun

And you run and you run to catch up with the sun, but it's sinking
And racing around to come up behind you again
The sun is the same in the relative way, but you're older
Shorter of breath and one day closer to death

Every year is getting shorter, never seem to find the time
Plans that either come to naught or half a page of scribbled lines
Hanging on in quiet despiration is the English way
The time is gone the song is over, thought I'd something more to say.

R. Waters, 'Time', performed by Pink Floyd, *Dark Side of the Moon*, EMI, 1973.

2

SUPPRESSION IN SCIENCE

2.1(a)

WRIT FROM SCIENTIST

Prominent SA environmentalist Dr. J. R. Coulter has issued a summons against the SA Institute of Medical and Veterinary Science claiming wrongful dismissal.

The summons was served yesterday on the institute director, Dr. J. A. Bonnin.

The action has been set down for conference in the SA Industrial Court Chambers on Friday.

Dr. Coulter, who has been told by the Institute that his laboratory will be closed on June 30 and that his pay and status will be reduced, has issued a qualified claim which questions whether the SA Executive Council has sanctioned the action.

If the Executive Council has not sanctioned the action, Dr. Coulter is claiming that he has been dismissed contrary to Section 15 (1) (e) of the Industrial Conciliation and Arbitration Act and that his dismissal is "harsh, unreasonable or unjust."

Dr. Coulter has been employed at the institute as a surgical research officer since 1959.

He said yesterday he had been told by Dr. Bonnin that he would be transferred to the clinical microbiology section on a salary $10,000 less than he now received.

The Advertiser (Adelaide), 26 March 1980, p. 3.

Barry Hailstone

2.1(b)

SACKED FOR SPEAKING OUT - SCIENTIST

A prominent SA environmental research scientist claims he has been "virtually sacked" by the SA Institute of Medical and Veterinary Science because he is outspoken on environmental issues.

He is Dr. J. R. Coulter, whose future is still in doubt after a meeting in chambers at the SA Industrial Court on Friday.

The court meeting resulted from a summons being issued against the IMVS claiming wrongful dismissal of Dr. Coulter.

Dr. Coulter said he had been told by the institute's director, Dr. J. A. Bonnin, that, from June 30, his environmental mutagen testing laboratory at the institute would be closed and that he (Dr. Coulter) would be transferred to the microbiology section on a salary $10,000 a year less than he now received.

The summons was issued on the ground that the SA Executive Council presided over by the Governor, Mr. Keith Seaman, must sanction moving Dr. Coulter and, if it had not, then his dismissal was not valid.

Dr. Coulter's lawyer said yesterday that in the conference in chambers on Friday the summons against the IMVS had been withdrawn by agreement with the Crown Law Department (representing the IMVS) on an assurance from the Crown that the Executive Council had not ratified the IMVS action in moving Dr. Coulter.

Dr. Bonnin said yesterday he would seek legal advice this week to see what the IMVS's next step would be.

This advice would include whether the Executive Council should be asked to sanction the move.

Dr. Coulter said he would also discuss with his lawyer the significance of the present situation.

Later, his lawyer had said they would either be seeking an ultimatum from the IMVS that it would not take the matter to the Executive Council, or they would seek an injunction in the Supreme Court to stop the IMVS going to the Executive Council.

In his present position, Dr. Coulter is paid $31,223 a year. The position offered, that of a trained pathologist, has a salary of $21,444.

Dr. Coulter said he was classified as a specialist pathologist in his present job.

He was not only trying to keep his present job for his personal security, but important work on mutagens in the environment should be continued.

"I have always believed that it is a scientist's responsibility to be available to the public and to speak out on issues where the scientist has a specialist knowledge and not just publish results in learned journals," he said.

"Being outspoken is my crime.

"As a doctor I see my duty to the community to do, and say, those things which are intended to improve the health of the community.

"With cancer now the second most common cause of death and the fact that 60 p.c. to 90 p.c. of cancers are caused by exposure to environmental agents, I believe I have a clear responsibility to make facts and consequences known."

Dr. Coulter, a strong opponent of the nuclear industry, said there had been a serious attempt to reduce his standing in the community so the issues he was involved in would be downgraded.

He and a colleague had been the first in the world to show that a drug, tinidazole, was mutagenic and potentially capable of causing cancer in human beings.

Dr. Bonnin said yesterday he would like to comment on Dr. Coulter's remarks, but would not do so until he had seen them and until he had taken legal advice.

Probably this would be done today.

The Advertiser (Adelaide), 31 March 1980, p. 12.

Barry Hailstone

2.1(c)

DIRECTOR GIVES REASONS FOR SCIENTIST'S MOVE

The SA Institute of Medical and Veterinary Science could not afford to have SA environmentalist Dr. J. R. Coulter continue working in his own laboratory, the director, Dr. J. A. Bonnin, said yesterday.

He said the institute had to find about $85,000 a year from its own resources to support Dr. Coulter in his work.

Dr. Bonnin, who was replying to a statement by Dr. Coulter that he had been "virtually sacked" by the institute because he was "outspoken on environmental issues," said there were several aspects to Dr. Coulter's work which had to be put into perspective.

Dr. Coulter has been told in a letter from Dr. Bonnin that from June 30 his environmental mutagen testing laboratory at the institute will be closed and that he (Dr. Coulter) will be transferred to the microbiology section on a salary $10,000 a year less than he now receives.

Dr. Bonnin said Dr. Coulter was employed as a research worker and until recently had one research scientist and two other scientific assistants.

"However, he started on his own initiative to provide a routine service, testing various substances for mutagenic (potentially cancer-causing) properties," he said.

"The financial situation of the institute makes it necessary to reduce the size of this unit.

"The Institute is charged with providing pathology services to the public and with research related to these services.

"It has neither the accommodation nor the available finance to branch out into other services."

Dr. Bonnin said that if such a testing unit was thought desirable in Adelaide it would be better placed in another service department such as the chemistry division of the Department of Services and Supply.

"Many drugs and chemicals are now tested by, or for, their manufacturers and there is little need for this work in Adelaide, which manufacturers almost none of these substances," he said.

Dr. Bonnin said Dr. Coulter may well have been able to continue in medical research had he been successful in attracting research grants from appropriate bodies such as the Anti-Cancer Foundation or the National Health and Medical Research Council.

Dr. Coulter had been prematurely promoted to medical specialist status and salary on his undertaking to write up earlier work in the form of a thesis for his degree of doctor of medicine.

"The consequences of not obtaining this degree were fully explained to him," he said.

Dr. Bonnin said there was now a salaried medical officers award in SA which took medical specialist and other qualifications into account.

"The SA Health Commission had insisted that specialist salaries could be paid only to those who were registrable as medical specialists or to those who held an appropriate postgraduate qualification.

"Dr. Coulter still has only his basic medical degree," he said.

"In moving him into a service position in microbiology he is no longer eligible to be paid as a specialist.

"This is what Dr. Coulter regards as demotion.

"The institute council feels obliged to move Dr. Coulter into a less costly aspect of the institute's work."

The Advertiser (Adelaide), 1 April 1980, p. 8.

Bill Rust

2.1(d)

UNIONS IRATE AT 'SACKING' OF SCIENTIST

SA's top trade union official yesterday strongly criticised the "virtual sacking" of a prominent SA environmental research scientist.

The United Trades and Labor Council of SA secretary, Mr. R. J. Gregory, said the trade union movement was "extremely concerned" that Dr. J. R. Coulter, a man known for speaking his mind about the effects of chemicals on workers, now found himself unable to continue that research.

Dr. Coulter has been told by the SA Institute of Medical and Veterinary Science director, Dr. J. A. Bonnin, that from June 30 his environmental mutagen testing laboratory at the Institute will be closed and that he (Dr. Coulter) will be transferred to the microbiology section on a salary reduced by $10,000 a year.

Dr. Bonnin said last week the institute had to find about $85,000 a year to support Dr. Coulter and could not afford to have him working in his own laboratory.

He had been employed as a research worker, but on his own initiative had started to provide a routine service testing substances for mutagenic (potentially cancer-causing) properties.

Finance had made it necessary to reduce the unit's size.

Mr. Gregory said yesterday:

"The trade union movement is most concerned that many new chemicals and processes are arriving on the market and are constantly being introduced into industry, when in many cases it is not known until 10, 15 or even 35 years later whether they are harmful to the people who will have to work with them.

"We believe scientific and medical research should be continued to determine whether chemicals and new processes are carcinogenic or harmful in other ways.

"We understand that a very simple test can determine whether a chemical is likely to be carcinogenic, allowing scientists then to carry out more extensive work which will prove conclusively whether or not it is.

"This work is very important because it would help to remove from our factories one of the most difficult hazards to detect."

Mr. Gregory said that where chemicals were suspect or proved to be unsafe, alternative chemicals could be found.

The SA union movement was very conscious of Dr. Coulter's good work in isolating carcinogens and drawing public attention to the chemicals and substances harmful to human beings.

Such work would be increasingly important with investigations taking place into effects of radiation on people who had worked at Radium Hill in SA "and other activities which the SA Government is going to want Australian workers to participate in."

"We are very concerned that a scientist working in this area is suddenly being pushed to one side," he said.

The Advertiser (Adelaide), 8 April 1980, p. 6.

D. Cole

CANCER RESEARCH WORK

Sir — I am dismayed at the recent remarks of Dr. Bonnin of the Institute of Medical and Veterinary Science regarding the research into cancer-inducing agents carried out at the Institute by Dr. John Coulter.

Dr. Bonnin should be aware that the incidence of cancer is rising significantly on a world-wide basis. It is now suggested by medical authorities that between 70 p.c. and 90 p.c. of all cancers are caused by environmental factors, including industrial pollution and the indiscriminate use of various chemicals.

While there may be some dispute as to what constitutes a pathology service, I would have been far more impressed with Dr. Bonnin's approach to the matter if he had indicated that the IMVS recognised the value of Dr. Coulter's work and that it would, despite limited resources, assist him to continue what is a necessary service.

Dr. Bonnin suggests that in relation to the cancer-inducing properties of certain products, reliance may be placed on data and information provided by manufacturers. This disregards the fact that manufacturers have a responsibility to shareholders to sustain and, if possible, increase profits. It is naive to suggest that manufacturers of chemical products, for example, are satisfactory guardians of public health, even though they may share concerns as to the possible impacts of their products.

Dr. Bonnin's remarks encourage the conclusions either that the IMVS possesses an extremely parochial view of public health or that it is experiencing great difficulty in publicly justifying the removal of Dr. Coulter from his original position.

D. A. COLE

St. Peters.

The Advertiser (Adelaide), 9 April 1980, p. 5.

P. Gouldhurst

2.1(f)

CANCER RESEARCH

Sir — In his interview with your medical writer ("The Advertiser," 1/4/80) the director of the Institute of Medical and Veterinary Science made two claims, neither of which will bear close examination. He said "the financial situation of the institute makes it necessary to reduce the size of Dr. Coulter's) unit," that is to zero, and that "there is little need for this work in Adelaide which manufactures almost none of these substances," that is drugs and novel industrial chemicals. Elsewhere in a television interview he had similarly said that the institute did not have the finance to continue this work and that Dr. Coulter's laboratory was simply repeating work which had been done overseas.

On March 26, 1980, the IMVS had a credit balance $882,000 above its budget expectations for this point in the year, despite Dr. Coulter's laboratory having been funded in the present year.

On the second point, Dr. Coulter's laboratory was the first in the world to show that the drug tinidazole was mutagenic and therefore posed a potential cancer risk for patients. This drug, although manufactured by an overseas company, was being introduced to the Australian market before registration was sought in the US. Drs. Coulter and Turner published this work in "Mutation Research," 57:97 (1978), a fact which your readers may check for themselves.

As public accountability seems very much at issue in this matter it seems fair to comment that the public are not well served if Dr. Bonnin, the director of a publicly funded institute, does not adequately research his facts before making public statements. The statements made by Dr. Bonnin in "The Advertiser" article quoted above are clearly misleading.

DR. F. R. S. GOULDHURST
Rostrevor.

The Advertiser (Adelaide), 10 April 1980, p. 5.

J. Bonnin

2.1(g)

CANCER RESEARCH WORK

Sir — The statements of Mr. R. J. Gregory, as related by your Industrial Reporter, Mr. Rust, and the letter of D. A. Cole ("The Advertiser," 9/4/80) concerning Dr. R. J. Coulter need to be answered.

The main reasons for the Institute Council's decision with regard to Dr. Coulter, have become confused with finance. These reasons are:

●His broken agreement to submit his work for an appropriate postgraduate qualification. It was on the basis of this agreement that he was promoted. Senior research workers are required to obtain such qualifications, which he could easily have done He has for many years been paid as a specialist pathologist for which he has no qualification.

●His failure to publish the results of past research. All research workers are expected to publish their work in reputable scientific journals and, despite repeated requests and written direction, this has not been done.

●His markedly low productivity as a full-time research worker, for which he is employed and which has been drawn to his attention. He has published only three papers in recognised journals in more than two years. The institute is criticised for allowing this state of affairs to continue for as long as it has. Several of the institute's routine service personnel are far more productive, fitting in their research studies along with their other duties.

●His failure to comply with the Regulations under the Institute of Medical and Veterinary Science Act with respect to submitting any manuscript for approval before sending it for publication.

Most of Dr. Coulter's public statements have come from his private interest in environmental matters and have not resulted from his own research at the institute. He will no doubt continue to make these statements and play a prominent part in environmental affairs irrespective of the institute.

Dr. Coulter has not justified, scientifically or practically, some of his statements about hazardous environments. Every reputable scientist acknowledges that one cannot extrapolate what he finds in an artificial "test tube" situation to what occurs in an animal or human being. The human body has many protective mechanisms. The test he uses is a useful screening test only, and much more work in animals is necessary before claiming that any substance is cancer-producing.

Dr. Coulter was authorised to establish the Ames mutagenicity test as part of an approved research project. He was not authorised to establish a large routine testing service of drugs and chemicals, most of which are now being submitted for testing in the country of manufacture, not necessarily by the manufacturers themselves. Much of Dr. Coulter's recent work has merely reproduced the findings of others.

His claim to be the first in the world to demonstrate the mutagenicity of Tinidazole, repeated by Dr. Gould-Hurst ("The Advertiser," 10/4/80) is not correct as this observation was made by Drs. Lindmark and Muller in a paper published two years earlier.

The institute does have an interest and a role in cancer research. Excellent research work into cancer is being undertaken in three other areas which is well supported by recognised research grants. Those responsible for recommending research grants (a form of peer review) have not recommended support for his present work. A competent research worker in any scientific organisation is expected to attract financial support in the form of research grants.

It is not the role of this institute to establish a large routine testing service for the testing of chemical substances for cancer-producing properties.

J. A. BONNIN
Director,
Institute of Medical
and Veterinary Science.

The Advertiser (Adelaide), 12 April 1980, p. 5.

Richie Gun

2.1(h)

CANCER RESEARCH WORK

Sir — The closure of the Environmental Mutagen Testing Unit at the Institute of Medical and Veterinary Science (IMVS) is a retrograde step.

The unit has provided an important service through a laboratory test which gives a valuable guide to the likelihood of any substance causing mutations or cancer in exposed populations.

Without this test reliance must be placed on human population studies. This means waiting years while people are exposed to a particular substance and then seeing how many people develop cancer or have stillborn or deformed offspring.

In contrast, the environmental Mutagen Testing Unit could give a guide within 48 hours as to whether or not an agent is potentially harmful.

The Port Adelaide Occupational Health Service through this test has been able to have one substance withdrawn from use in a particular workshop because of its demonstrated potential as a mutagenic and carcinogenic agent. On other occasions we have been able to obtain assurances that particular agents were safe to handle.

In seeking to justify closure of the unit, the director of the IMVS, Dr. Bonnin, says many drugs are tested by or for their manufacturers.

Unfortunately, many others are not.

Perhaps Dr. Bonnin will advise the Port Adelaide Occupational Health Service and other concerned bodies what they should do in future when they wish to investigate the safety of any substance workers may be asked to handle.

RICHIE GUN
Acting Medical Director,
Port Adelaide Occupational Health Service

The Advertiser (Adelaide), 12 April 1980, p. 5.

Michael Ross

RESEARCH WORK

Sir — Without wishing to become involved in the specific situation of Dr. Coulter, I would take issue with two comments made by Dr. J. A. Bonnin ("The Advertiser," 12/4/80) regarding the standards set for satisfactory performance of research scientists.

The publication of three papers over two years as an unsatisfactory standard disturbs me. Surely publications should be measured by their impact on the scientific community, the status of the journal in which they are published and the length of time and complexity of the research. Dr. Bonnin's statements suggest an emphasis on the American ethic of "publish or perish," which has led to the discarding of long-term and carefully considered research in favor of pointless and minor projects for the sake of a publication credit.

Second, in the current economic climate, failure to gain a research grant cannot be considered unsatisfactory. I am well aware of many exceptional projects submitted to bodies such as NH & MRC and ARGC which cannot be financed because the funds provided can barely guarantee maintenance for presently funded projects, let alone new ones. Failure to gain funds cannot be seen as indictment of a researcher's ability in the present economic circumstances.

the employment of any researcher.

DR. MICHAEL ROSS
Stirling

The Advertiser (Adelaide), 18 April 1980, p. 5.

P. Gouldhurst 2.1(j)

BONNIN 'ATTACK'

Sir — I would like the opportunity to answer, point by point, Dr. J. A. Bonnin's long, serious and personal attack on Dr. John Coulter (*The Advertiser*, 12/4/80).

● Dr. Bonnin cannot reasonably complain that this issue has "become confused with finance" when he was the first to introduce financial considerations and when he has now been shown to have made a misleading public statement in relation to IMVS finances.

● While there may now be a requirement, under an award, for senior research workers to obtain a higher qualification, when Dr. Coulter began at the IMVS in 1959, there was no such requirement. Dr. Coulter was paid for about 10 years as a specialist pathologist before the present award was introduced about three years ago. Dr. Bonnin clearly recognises the quality of Dr. Coulter's work in relation to these qualifications for he says Dr. Coulter "could easily have obtained" them.

● Dr. Coulter tells me he has kept Dr. Bonnin fully informed of the progress of papers for publication and was informed, in writing, by Dr. Bonnin as recently as July 1979 that this progress was satisfactory. A paper on the past research referred to by Dr. Bonnin was submitted to him for publication in December, 1979.

● It is untrue that Dr. Coulter's productivity has been markedly low. Studies of research productivity in Australia and New Zealand show an average production of 1¼ papers a scientist a year. By this standard and using Dr. Bonnin's own figure, Dr. Coulter's research productivity has been above average. Moreover Dr. Coulter has had a further four letters published or accepted for publication in scientific literature and last year was twice invited to deliver paid lectures on his work in Victoria and once in NZ. This is not the record of an unproductive scientist.

● Dr Bonnin does not say in what way or when Dr Coulter did not comply with IMVS regulations with respect to submitting manuscripts for approval before sending them for publication. It is true that Dr Coulter makes many public statements as a spokesman for the conservation movement. He has been scrupulous in making these statements as a private individual.

● Dr. Bonnin's criticism of the mutagenicity test used by Dr. Coulter illustrates an alarming and perhaps wilful ignorance of this important work going on in his own institute. Both the US Occupational Safety and Health Administration and the UK Health and Safety Commission have endorsed this test. Moreover, even if this test showed no correlation with carcinogenicity (in fact it shows better than 90 p.c.) it would still provide an extremely valuable method of identifying genetically toxic agents which may increase abortion and birth defect rates and lead to damage to the human gene pool.

● Applications for research grants are made, not for basic support but for specific research proposals. Grant-giving bodies enquire closely into whether an applicant already has the basic support of his institution. Dr. Bonnin's attack on Dr. Coulter illustrates how little support the latter has from IMVS management, and this factor must have weighed heavily against a favorable assessment of his grant applications. It is also relevant that last year Dr. Coulter was made to apply for twice as much money as he had asked for in his draft grant application, thus further prejudicing his chances in a situation in which more than seven out of every 10 applications were unsuccessful.

● Finally Dr. Bonnin has not addressed the central matter raised by Dr. Coulter, namely that the latter was made to withdraw a section of a paper because of fear that it may have jeopardised money for research from drug companies. Dr. Bonnin's remarks so far have exhibited a greater concern for the welfare of drug companies than the public interest. He has said several times that there is no need for these tests in Australia as drug firms carry them out overseas and has referenced the work of Lindmark and Muller on the drug, tinidazole.

Perhaps he would care to explain why the company launching this drug on the Australian market did not mention Lindmark and Muller's work even though the same company had supplied tinidazole to these workers and must have known of these results. Either the company did not regard this as a proven demonstration of mutagenicity or it was withholding this information from Australian medical practitioners. Either way, it argues strongly for an independent test facility in Australia.

DR. P.R.S. GOULDHURST

The Advertiser (Adelaide), 22 April 1980, p. 5.

J. Potter

WORK HAZARDS

2.1(k)

Sir — It has come to our notice recently that some organisations including semi-governmental organisations are reluctant to allow workers access to information regarding industrial hazards to which they may be exposed.

This policy is often on the basis that the information is too complex to be readily understood by them. Such attitudes, even were they justified (in fact they frequently are not) are markedly paternalistic and destructive of trust in communication.

The Doctors' Reform Society is firmly committed to the principal of free exchange of scientific information throughout the society and particularly where the information has direct and practical consequences for good or ill.

We believe that occupational health entails active participation of all parts of the workforce. Such participation is not possible if information is not freely exchanged.

Dr. J. D. POTTER,
President,
Doctors' Reform Society.

The Advertiser (Adelaide), 12 May 1980, p. 5.

Ian Maddocks

HEALTH RISKS

Sir — Your thoughtful editorial Acceptable Risk (17/5/80) is a useful reminder of a concept with which we should all be familiar.

It is often not appropriate in medicine to speak of "cause and effect." We cannot say that smoking "causes" heart attacks, or that exposure to radiation or asbestos "causes" cancer.

But the risk of developing these diseases is clearly increased by such factors, and we have to decide, as individuals and as communities, what risks we are prepared to accept for ourselves.

As you clearly state, it is not satisfactory to leave those decisions to experts.

For one thing, we are each, individually, most responsible for our own personal well-being.

For another, the experts are often in disagreement.

Very few of the chemicals to which we are exposed in daily life have been fully assessed for their dangers to man — you quote a figure of 100 out of a total of 70,000.

I am not prepared to be told by the manufacturer of a chemical that it presents me with an "acceptable risk."

I want to know of an assessment done outside the industry, and, if possible an assessment which takes into account the Australian situation and the way that chemical is used here.

It is a little ironic, therefore, that at the very time when your comment appears, we are facing the closure of one of the few such laboratories in Australia — the unit under Dr. John Coulter which assesses chemicals for their mutagenicity, their likelihood of increasing the risk of cancer.

Dr. Coulter is plainly out of favor with his Director.

The reasons which Dr. Bonnin has stated in your columns for Dr. Coulter's demotion do not seem to justify such extreme action. I hope that Dr. Bonnin will not force the closure of the unit — one of the few planks we have in our pitifully inadequate defense against the subtle and insidious chemical dangers of our modern world.

IAN MADDOCKS
North Adelaide.

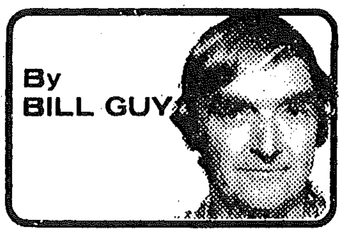
The Advertiser (Adelaide), 28 May 1980, p. 11.

Bill Guy

2.1(m)

CONSERVATION CAMPAIGNER IN CRISIS

Dr. John Coulter believes passionately that every scientist has a moral obligation to use his expertise for the benefit of society.



This belief has made him one of Australia's foremost conservation campaigners; it is also responsible, some say, for the professional crisis he now faces.

For almost his whole working life, Dr. Coulter, 49, has been a researcher at the SA Institute of Medical and Veterinary Science.

He was once described as "the conscience of the institute."

It is the function of conscience to prod, to nag, even to goad. Dr. Coulter has perhaps done his share of all that.

It is often the effect of conscience to create inner conflict; and it is conflict within the institute that now threatens Dr. Coulter's career.

Unless a decision of the institute's governing council is rescinded, Dr. Coulter by the end of this month will suffer a loss of status, a loss of $10.000 in annual salary — and the loss of his research unit at the institute.

It will be a crushing anti-climax to the dreams and ambitions he took with him when he joined the institute in 1959.

Dr. Coulter went to the institute with a medical degree after a brief spell in general practice. His early work included research into hospital cross-infection and the effects of DDT.

For the past few years he has concentrated on the identification of mutagens in the environment. Mutagens are chemical agents which cause cell mutations. Many mutagens have been found to be carcinogenic — that is, cancer-inducing.

Early in March, Dr. Coulter was told in a letter from the institute's director, Dr. J. A. Bonnin, that from June 30 his mutagen testing unit would be closed and he would be transferred to the microbiology section.

He was also told that he would lose his specialist status and his salary would be reduced from $31,223 to $21,444.

These decisions have stirred a public debate which last week prompted questions in the SA Parliament. The controversy raises three main issues:

- Is Dr. Coulter the victim of professional injustice?
- Is there a continuing need in SA for his mutagen testing unit?
- Is Dr. Coulter being penalised by the scientific Establishment because of his forthright championing of environmental causes?

On this last point, an academic who has made a special study of what he calls "the suppression of scientists pursuing environmental research" believes the Coulter case could well fall into this context.

In a paper submitted last month to the journal "Social Studies of Science," Mr. Brian Martin, of the Department of Applied Mathematics at the Australian National University, writes:

"Environmental scholarship is often seen as linked to the 'politics' of the environmental movement; environmental scholarship often presents a challenge to established practices and policies of powerful organisations; and environmental scholarship often challenges the dogmas of various scientific disciplines."

Mr. Martin, who spoke on this subject at last month's ANZAAS conference in Adelaide, writes elsewhere in his paper of the "political scientific elite" ...

..."that group of scientists with the greatest political power, both within and without the scientific community, to influence government and corporate policies and to influence developments in the scientific community.

"The power of this elite is manifest in the promotion of research in certain areas and its restraint in others, in the creation or dismantling of research institutions, in the hiring or dismissal of staff, in the allocation of funds from specific research projects and in the establishment of and setting of policies for scientific journals and tests."

I contacted Mr. Martin after his ANZAAS address and asked him whether he believed Dr. Coulter might have suffered on account of his conservation work. He replied:

"Dr. Coulter's case is indeed serious. There are many people in Adelaide and around Australia concerned about it. I certainly believe that a full, open investigation is called for."

Dr. Coulter's conservation crusade began in the mid-1950s.

He was an early opponent of water fluoridation and uranium mining, he has voiced deep misgivings about the Redcliff petrochemical project, he was recently caught up in the controversy over the Maralinga nuclear tests.

B. Guy, 'Conservation campaigner in crisis: does Dr Coulter have to go?', *The Advertiser* (Adelaide), 17 June 1980, p. 5.

Deborah Smith

2.1(n)

LABOR PROMISES AN INQUIRY INTO SA MEDICAL INSTITUTE

By DEBORAH SMITH

THE OPPOSITION in South Australia has effectively served notice on one of the State's scientific establishments – the Institute of Medical and Veterinary Science – that Labor would begin an investigation into the structure and functions of the IMVS.

Labor wants to ensure that the activities of the scientific institute are related to the State's contemporary needs, particularly in preventative medicine – an area in which, it considers, the institute has fallen down recently.

Labor's call for a public inquiry into the affairs of the IMVS has been in response to the dismissal from the IMVS of Dr John Coulter, a surgical research officer, and to the closure of his research unit.

For the past ten years Dr Coulter has been establishing a bacterial test for the identification of substances which can cause mutations or genetic damage to cells. Such mutagens can be responsible for increasing abortion and birth defect rates and may initiate cancers.

Using this method, called the Ames test, Coulter's unit has been routinely screening substances to see if they are mutagenic. Some of the chemicals submitted for testing have come from concerned groups outside the scientific community, in particular from workers' health organisations. Only two other units, one in Melbourne and one in Sydney, routinely accept samples for mutagen screening.

An irate United Trades and Labor Council of SA is providing the focus of union concern at the closure of Coulter's laboratory with a call for all unions to support the council's submission to the Minister of Health for the reopening of the mutagen testing unit. The council claims that Coulter's work is particularly valuable to the unions because it helps remove from factories one of the most difficult hazards to detect – harmful chemicals.

The Opposition spokesman on health in SA, Terry Hemmings, told The National Times of two major concerns – that the reason for Coulter's dismissal might have been influenced by his outspokenness on environmental hazards and that SA has lost a valuable unit for the testing of hazardous substances.

Dr John Coulter. . . "the responsibility of the IMVS is to defend the public — not the private interest of drug and chemical companies."

Before and during the 21 years Dr Coulter has worked at the IMVS he has become well known as an outspoken conservationist. In particular, his forthrightness over issues of chemical hazards to the environment has led him into conflict with some commercial interests. Both the Bayer drug company and Velsicol Australia Ltd have taken issue with public statements Coulter has made regarding the companies and their products.

Dr Coulter told The National Times that he believes the comments he has made in a private capacity but which have led drug and chemical companies to complain to the IMVS have embarrassed the institute to the extent that the action of his dismissal has now been taken.

"It is to the present Government's advantage not to have me in a situation where I can make announcements and do work which is inimical to the drug and agricultural chemical companies," he said.

The Institute of Medical and Veterinary Science has received, on occasions, research grants from drug companies, but the director of the IMVS, Dr Jim Bonnin, has always stressed that the decision to dismiss Coulter is based only on financial and professional considerations.

Bonnin points out that Coulter was not authorised to establish this routine testing service and as such the institute could not afford to support the laboratory with its public finances.

His environmental interests and his communication of his environmental concern to the public are very much part of his concept of the scientist's role in society.

He does not see science as an ivory tower pursuit; he is not primarily interested in the intellectual satisfactions if offers; he is concerned that scientific knowledge be used for the benefit of the community.

And if the applications of science pose dangers to the public, he believes the public ought to be fully alerted to those dangers.

This philosophy has inevitably led to collisions with commercial interests and to consequent disputes within the IMVS.

In 1978 the Bayer drug company objected to comments Dr. Coulter made about one of its products in an ABC "Four Corners" program on pesticides. And last year, Velsicol Australia Ltd. took issue with Dr. Coulter over his reference to the firm's parent company in the US during a Melbourne seminar on pesticides.

The IMVS from time to time, has been awarded research grants by drug companies, and no doubt it would welcome more.

But any suggestions that Dr. Coulter's clashes with Bayer and Velsicol or his environmental work generally have contributed to his demotion are strongly denied by Dr. Bonnin.

Dr. Bonnin justifies Dr. Coulter's downgrading on purely professional grounds.

About 13 years ago when Dr. Coulter was given specialist status, says Dr. Bonnin, he was promoted on a promise that in that same financial year he would prepare his work for a thesis for his doctorate of medicine.

"He renewed that promise several times; to this day he still hasn't fulfilled it," Dr. Bonnin said.

"Early on, he was working very well and his work was very highly regarded," he said, adding that if Dr. Coulter had written up that work as a thesis it might well have secured him the necessary postgraduate qualification.

Dr. Bonnin also complains about an alleged paucity of published papers by Dr. Coulter.

"You assess a research worker's work," said Dr. Bonnin, "by reading his published work. If that published work is quoted by others you know that people are accepting that work.

"All we know is that Dr. Coulter has published very little indeed that is refereed . . . and basically he isn't quoted by other people."

Dr. Bonnin said he did not doubt Dr. Coulter's competence and went on:

"I have read Dr. Coulter's papers on things like carbon dioxide in the atmosphere. They are most interesting. He has a good knowledge of conservation and this is his passionate hobby and interest.

"I really respect both Dr. Coulter and his views but the point is that if he is going to do that at the expense of his official duties I have got to be responsible for this."

When Dr. Coulter became a specialist pathologist there was no official requirement that he should hold a postgraduate qualification. That came in 10 years later under a professional award.

But Dr. Bonnin disputes that the award is being applied retrospectively. The award has become relevant, he says, because of the need to give Dr. Coulter a new appointment following the closure of the mutagens testing unit.

But why close the unit? Is the service it provides of no value?

Dr. Bonnin recognises that mutagenic tests serve as an early-warning system in the identification of possible carcinogens.

But he believes that the testing can be done on a bigger and better scale elsewhere.

A national testing service has, in fact, been offered by the unit at the Sydney School of Public Health and Tropical Medicine, Dr. Bonnin says.

But local trade unions, in particular, are not swayed by this argument. Some of their workers have benefited from Dr. Coulter's tests and they believe SA should retain its own mutagen test unit.

It is this feeling that has prompted the United Trades and Labor Council to appoint a delegation to the Minister of Health, Mrs. Adamson, to voice its concern at the proposed closure of the Adelaide unit.

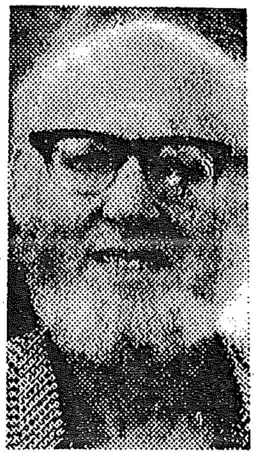
I asked the Minister what action she was taking in the Coulter case and was told she was awaiting advice from the IMVS council on its course of action.

Did this mean, I asked Dr. Bonnin, that the council could rescind its decision?

He agreed that the council, if it wished could call a special meeting before June 30 to review the case.

Is it too much to hope that a reconciliation can be arranged between Dr. Coulter and the institute he has served for 21 years?

Even if all the rights in the case do not lie with Dr. Coulter, does that justify the infliction on him of what seems to many a savage wrong?



Dr. Coulter . . . a victim of professional injustice?

"He has a good knowledge of conservation and this is his passionate hobby and interest. I really respect Dr. Coulter and his views but . . . if he is going to do that at the expense of his official duties I have got to be responsible for this."

Dr. J. A. Bonnin, Director of the Institute of Medical and Veterinary Science

Other reasons that have been given by Bonnin for the decision relate to Coulter's failure to submit work for postgraduate qualifications, the number of scientific papers Coulter has published – "only three in more than two years" – and a failure to comply with the regulations of the IMVS Act with respect to submitting any manuscript for approval before sending it for publication.

Hemmings, the Opposition spokesman on Health, told The National Times that a report which Coulter made on Ethylene Oxide in April might have influenced the decision to dismiss Coulter: "It may have been the one that crowned it all."

Coulter had been commissioned to study the hazard to staff of exposure to ethylene oxide, a potent mutagen, which was being used in one of the laboratories as a sterilising agent. As a result of releasing his report simultaneously to the staff in the laboratory as well as to the official safety committee, Coulter was severly rebuked by Bonnin.

". . . the staff concerned received a document which they could not completely understand, therefore becoming unduly frightened and concerned. You were obliged to report your findings to the Fire and Safety Committee," Bonnin wrote to Coulter.

Coulter sees the issue as one of freedom of information, particularly that of the right of workers to information about substances to which they are occupationally exposed.

The mutagen testing unit in SA was a very small part of a worldwide effort to gather information on hazardous substances. Overall the task is a formidable one with as many as 50,000 chemical substances in common use and between 700 and 1000 new ones being marketed each year.

The Sydney unit which also provides routine mutagen tests is at the Commonwealth Institute of Health and the head of the laboratory, Dr Robert Baker, explained the status of the three types of evidence used to identify possible carcinogenic substances.

He pointed out that there is still a paucity of data on human cancers from studies of exposed populations; and that animal cancer tests are very expensive, take several years to complete and even then cancers which arise in animals are not always predictive for humans. No animal tests are carried out in Australia.

Baker stressed that short-term laboratory tests, the third method, are very valuable as a quick and inexpensive but only preliminary way of screening chemicals. The Ames test studied by both Coulter and Baker is the most widely used and validated of these short-term tests and provides a good indicator of a potential hazard with an 80 to 90 per cent correlation between a substance which is a mutagen and its chance of causing cancer.

Does SA need its own mutagen testing unit? The stance of the SA Government has been one of support throughout for the IMVS Council's decision. Jennifer Adamson, the Minister of Health, has emphasised that it is not the IMVS's function to screen chemicals routinely for their carcinogenicity and has justified the closure of the unit on grounds of its size.

She contends that the identification of carcinogens can only be done effectively on a grander scale with a national testing facility able to carry out toxicological studies as well as a number of short-term tests and able to provide an information bank on hazardous substances.

In this context she names the proposed Environmental Toxicology Unit which is to be set up by the Commonwealth Institute of Health in Sydney as such a national laboratory. "There is no useful purpose in individual States duplicating aspects of the work which will be carried out effectively and on a comprehensive basis by a national laboratory," she said.

On the other hand Coulter and some of the groups who have used his facilities argue that the value of the laboratory in SA has already been well established. As a local unit it has been able to monitor work environments where known mutagens are being used as well as detecting new ones, and reports from the unit have led to safer conditions for some workers.

A commonly cited case is Coulter's discovery that the drug tinidazole is mutagenic and so poses a potential cancer risk for patients. He later found out he was not the first to discover this. Two years previously workers with samples of tinidazole supplied by the pharmaceutical producers had obtained the same results. The question remains why this work was not mentioned by the company when tinidazole was first launched on to the Australian market.

Coulter is convinced that, even if a national laboratory is set up, the duplication of the results of short-term mutagen tests is both scientifically and politically valuable. He argues that the consequences of the tests are far-reaching in terms of the health of workers and the potential profit of companies. This makes it essential that there be a number of independent laboratories both for corroboration of scientific results and to guard against political pressure.

The proposed national centre of environmental toxicology in Sydney will be headed by Dr Alistair Thom, who is at present on a related study trip overseas. Research will be undertaken into the effects of environmental poisons using epidemiological or population studies, and a computer link-up with US toxicological data is planned. The Institute of Health, with its other facilities to study birth defects and possible mutagens, hopes progressively to expand all its abilities to monitor the environment for hazards.

Meanwhile, the groups concerned with occupational hazards in SA are upset at having lost a unit which could give a guide within a couple of days as to whether or not a particular substance is potentially harmful.

It may be that the House of Representatives Standing Committee Inquiry into Hazardous Chemicals will recommend the setting up of more mutagen testing units, but it may not. Either way the prospect is a long-term one since the inquiry is as yet only receiving submissions.

The conflict between Coulter and the Institute of Medical and Veterinary Science is a much more immediate problem and Coulter is as outspoken as ever.

"As a public institution the responsibility of the IMVS is to defend the public – not to defend the private interest of drug and chemical companies," he said. "This is the fundamental difference between us."

The National Times, 20-26 July 1980, p. 36.

South Australian correspondent

2.1(o)

ONE MAN'S WORK UNDER FIRE

A MINI war is raging within the hallowed halls of the South Australian Institute of Medical and Veterinary Science.

Tucked away inside is Dr John Coulter — scientist, environmentalist, and major victim of the current battle. Time is running out for this prominent South Australian, for, on June 30, his laboratory will be closed and his status in science circles will be all but lost, along with a $10,000 drop in annual salary.

The predicament in which Dr Coulter finds himself is now well known to scientists all over the country and support for his cause is widespread, ranging rrom that of doctors and educators to trade unions and politicians. But Dr Coulter has no doubts about the reason for his demotion — he talked too much.

For almost his whole working life, Dr Coulter, 49, has been a researcher at the IMVS, taking up his position with a medical dedree after a short time in general practice.

His early work in the institution involved research into hospital cross-infection and the effects of DDT — a time when, according to the institute director, Dr J. A. Bonnin, he was working very well and was highly regarded. But for the past few years, Dr Coulter has made quite a name for himself by using his laboratory to identify mutagenic (potentially cancer-causing) properties in the environment — and that, it would appear, is when things started to go wrong for him.

Early in March, Dr Bonnin informed Dr Coulter of the pending closure of his laboratory, and the reclassification of his specialist pathologist position to trainee pathologist in the microbiology section. De Coulter's shocked response was to issue a summons against the IMVS claiming wrongful dismissal, saying, virtually, that he had been sacked because of his outpoken views on environmental issues. The summons was withdrawn, with both sides getting themselves into something of a legal mess over the issue, and Dr Coulter started talking again.

"Being outspoken is my crime", he told the Press. "I have always believed that it is a scientist's responsibility to be available to the public and to speak out on issues where the scientist has a specialist knowledge and not just publish results in learned journals. As a doctor I see my duty to the community to do, and say, those things which are intended to improve the health of the community. With cancer now the second most-common cause of death and the fact that 60 per cent to 90 per cent of cancers are caused by exposure to environmental agents, I believe I have a clear responsibility to make facts and consequences known".

Dr Bonnin replied that finances were not available to a degree enabling the continuation of Dr Coulter's unit, adding, "I really respect both Dr Coulter and his views, but the point is that if he is going to do that at the expense of his official duties I have got to be responsible for this".

He said many drugs and chemicals were tested now by their manufacturers, and that there was little need for this kind of work in Adelaide, where almost none of the substances were manufactured.

When the United Trades and Labour Council of South Australia pitched its weight behind Dr Coulter and his work, expressing its concern that he was "suddenly being pushed to one side", Dr Bonnin pointed out that the institue had to find about $85,000 a year to support Dr Coulter. Then Dr Bonnin proceeded to explain also that Dr Coulter was rather naughty in that he had been prematurely promoted to medical specialist status on his undertaking to write up earlier work in the form of a thesis for his degree of doctor of medicine.

"He renewed that promise several times — to this day he still hasn't fulfilled it", Dr Bonnin said. "The consequences of not obtaining this degree were fully explained to him".

Dr Bonnin did not leave his schoolmasterly attack there — Dr Coulter had "markedly low productivity" for a full-time research worker, he said, having published only three papers in recognised journals in more than two years.

"You assess a researcher's work", continued Dr Bonnin, "by reading his published work. If that published work is quoted by others, you know that people are accepting that work. All we know is that Dr Coulter has published very little indeed that is referred . . . and basically he isn't quoted by other people".

While Dr Bonnin's observation may be correct in terms of the "publish or perish" medical journals, it is not true of Dr Coulter's impact over the years on the various media outlets. His conservation campaigning began a long time ago when he opposed the fluoridation of w ater. Since then, he has spoken out and been widely quoted as the voicepiece of anti-uranium mining groups, has talked against the Redcliff petrochemical project in this State, and was more recently involved in the controversy surrounding the Maralinga nuclear tests. Dr Coulter made himself unpopular with the Bayer drug company in 1978 because of comments he made on ABC television about pesticides, and he ran into trouble last year with Velsicol Australia Ltd over his reference ot the firm's parent company in the United States during a Melbourne seminar on pesticides.

Although D-Day has almost arrived for Dr Coulter, the union movement is not allowing the matter to rest. The United Trades and Labour Council has appointed a delegation to the Minister of Health, Mrs Adamson, who is, in turn, awaiting advice from the council of the IMVS.

As UTLC secretary, Mr R. J. Gregory, said, "The trade union movement is most concerned that many new chemicals and processes are arriving on the market and are co nstantly being introduced into industry, and in many cases it is not known until 10, 15 or even 35 years later whether they are harmful to the people who will have to work with them. This work is very important because it would help to remove from our factories one of the most difficult hazards to detect".

The State Opposition spokesman on health, Mr Hemmings, called last week for an immediate public inquiry into the downgrading of Dr Coulter. It just remains to be seen whether or not he left his run a little too late.

The Canberra Times, 27 June 1980, p. 2.

2.2

Brian Martin

THE SCIENTIFIC STRAIGHTJACKET

Dissident scientists in communist countries receive wide publicity for their causes. But what of cases of suppression in the West? How do those who challenge the scientific establishment fare? And why have environmentalists become the chief target of those who seek to preserve the status quo?

Inscribed across the facade of the Sydney University School of Physics are the names of twenty or so famous scientists: Archimedes, Roger Bacon, Copernicus, Kepler, Galileo, Newton and others. As a result of their scientific achievements, such illustrious forebears commanded respect; through their authority and prestige in scientific matters, they influenced the direction of scientific research. Or so the standard image of scientific 'greats', as portrayed in textbooks and the media, would suggest.

But what is the relation of the image of the eminent scientists of past eras to the present generation of scientific elites who hold positions of power in large research organisations around the world? Setting aside the question of the actual status of past elites, there is no doubt that a vast change in the organisation of scientific research has come about in the past few decades. This transformation may be called bureaucratisation, industrialisation, or the shift from 'little' science to 'big' science. Even if it were ever the case in the past, it is doubtful that the leaders of the scientific community today exert power primarily through their authority on scientific matters alone.

Suppression of Scientists

Table 1 lists a number of instances of suppression from Australia and New Zealand involving individuals who have been engaged in research or teaching relating to environmental issues. There is little documentation of the scale of suppression in the scientific and academic communities, and most of the cases came to my attention through personal contacts. For example, within the Australian National University, where four of the ten cases originated, there is no straightforward or easy way to determine the existence of academic suppression. However, there are several reasons to believe that cases such as those in Table 1 are only the tip of an iceberg.

In a survey of evidence about suppression of dissident scientists, Manwell and Baker conclude that such suppression is much more widespread in the west than generally acknowledged.[15] But, they note, cases in the west receive very little publicity compared to the great attention focussed on dissidents in communist countries. For example, it was only as a result of his personal case and the publicity it received that Manwell was informed of over one hundred cases of suppression in the English-speaking world.[16]

It is well known that there were wide-scale sackings and harassment of scientists and academics in the 1940s and 1950s, especially in the US.[17] The large scale of this activity is often forgotten, as are the long term effects of this attack on nonconformist scholarship. Just as important is the low level of awareness of the political suppression which has continued since then.[18]

As well as political beliefs, suppression is often closely connected with struggles with organisational vested interests, and with disputes over the validity of different types of knowledge and ways of acquiring it — that is, paradigm disputes.[19] A mixture of political, organisational and paradigm aspects in suppression cases is quite common.[20]

Most scientists prefer to avoid public controversy concerning their own research and teaching. This means that it is difficult to find individuals willing to have their cases presented as in Table 1. I know of several other suppression cases in which those involved do not wish publicity for personal or career reasons. There are also many cases in which suppression is a likely possibility but in which there is insufficient evidence to make a firm public case.

For these reasons it seems reasonable to infer that publicised cases are a small fraction of total cases.[21] Furthermore, since some types of suppression receive more publicity than others, it is highly likely that outright attempts to sack dissidents (as in the cases of Coulter, Evans and Manwell) are greatly outnumbered by non-tenured positions not being renewed; by failures to hire and promote; and by particular types of environmental research and teaching simply not being initiated in the first place.

At a more fundamental level, suppression merges with inhibition. As clearly expressed by C. Wright Mills years ago in relation to university teachers, "the deepest problem of freedom for teachers is not the occasional ousting of a professor, but a vague general fear — sometimes politely known as 'discretion', 'good taste', or 'balanced judgment'. It is a fear which leads to self-intimidation and finally becomes so habitual that the scholar is unaware of it. The real restraints are not

Table 1: Cases

Table 1. Instances of suppression from Australia and New Zealand involving individuals engaged in environmental research or teaching.

CASE 1: *Name* Dr John Coulter
Position Surgical Research Officer, Institute of Medical and Veterinary Science, Adelaide (1959-).
Background (a) Outspoken on numerous and diverse environmental issues, such as the impacts of environmental chemicals (1956-).
(b) As a researcher in IMVS, started on his own initiative (1977-) a routine service for testing substances for mutagenic properties.
(c) Prepared a report on the hazards of ethylene oxide (ETO) as a sterilant, and gave this to ETO workers as well as to the appropriate IMVS Committee (16 April 1980).
(d) Posted on IMVS noticeboards copies of the ETO report and related correspondence with the Director of IMVS (8 May 1980).
Action (a) Letters of complaint to IMVS from chemical companies. .
(b) Environmental mutagens testing unit closed by IMVS on 30 June 1980.
(c) Letter of rebuke from Director of IMVS for releasing ETO report to workers (23 April 1980).
(d) Instruction from Director of IMVS to not make available material dealing with the affairs of IMVS to any staff member without express approval from the Director (9 May 1980).
(e) Coulter dismissed from IMVS (30 June 1980).
Status Unresolved (September 1980).
Reference (1)

CASE 2: *Name* Dr Jeremy Evans
Position Senior Lecturer, Human Sciences Program, Australian National University (1973-).
Background Taught in environmentally oriented Human Sciences Program (1973-).
Action Reappointment and review committees recommended that tenure be denied (1979).
Status Tenure decision postponed until 1982.
References (2), (3)

CASE 3: *Name* Dr John Hookey
Position Senior Lecturer, Faculty of Law, Australian National University (1971-1974).
Background Introduced (1972) and taught first Australian undergraduate course in Environmental and Natural Resources Law, at Australian National University.
Action Indication that tenure would be denied (1973).
Status Resigned (1974) pending completion of internal appeal to take up appointment as Public Hearings Commissioner in Federal Department of Environment and Conservation; subsequently Commissioner, Redcliff Environmental Inquiry, and Presiding Commissioner, Fraser Island Environmental Inquiry.
Reference (4)

CASE 4: *Name* Dr Philip Keane
Position Lecturer in Botany, La Trobe University (1975-).
Background Published an article (5) in a national weekly newspaper (January 1977) about the spread of cinnamon fungus in Victorian forests.
Action Chairman of the Forests Commission of Victoria applied great pressure on the University's Chancellor, Acting Vice-Chancellor and the Deans of Science to take action — nine letters written and hand-delivered between 3rd and 24th February 1977 (6).
Status Unchanged by events. The University Council was informed of the attacks and the appropriate officers (Chairman of Department, Dean of School of Biological Sciences) resisted all pressures and strongly rejected the allegations made. The Chairman of the Forests Commission was further informed that all Australian University Statutes are framed to allow staff to speak publicly on controversial issues thereby preserving academic freedom.
Reference (7)

CASE 5: *Name* Dr Robert Mann
Position Senior Lecturer, Department of Biochemistry, on secondment to Centre for Continuing Education (1976-), University of Auckland.
Background A founding teacher (1974-) of the Environmental Studies programme; publicly active on issues of nuclear power, nuclear weapons, 2,4,5-T, etc.
Action Dismissal proceedings initiated (1977) by University of Auckland after letter to Vice-Chancellor

so much external prohibitions as control of the insurgent by the agreements of academic gentlemen".[22]

The incidence of suppression in the environmental area is almost certainly greater in government and industry than in academia, especially when cases of inhibition are included. Academics generally have much greater freedom — in that their jobs are less immediately threatened — to carry out research on and speak out on controversial topics. Because of this, academics are also more likely to speak up when attempts at suppression are made, though this is seldom enough. Dissidents in government or industry generally keep quiet, learn a new set of standards, or quietly exit. Especially in industry where few voice criticisms and stay around to tell about it.

Incompetence Rarely a Factor

Is there an underlying reason for suppression in the environmental and other areas? One answer is that the grounds given for dismissal, non-renewal and the like are themselves valid. A detailed assessment of this would require full documentation of each case, hardly possible here. Suffice it to say that purely academic or scientific judgements are almost always insufficient as an explanation. In almost every case in Table 1, the research output or teaching performance of the individual under threat was well above average, and in several cases the research or teaching records were outstanding.

For example, the outstanding teaching performance of Evans has been widely acknowledged[3]; Manwell's publication record placed him in the top one per cent of comparable scientists[10]; Smith, in the few years since submitting his Ph.D. thesis, has an enviable publication record. A similar pattern has been noted in cases of political suppression, in which shortcomings of ability, competence or performance have been sufficient to justify suppression in only a tiny proportion of cases[23]. Indeed a study of all contested dismissals in the period 1916-1970 reported in the *American Association of University Professors* Bulletin found that "in only 13 of the 217 dismissal cases was there even a suggestion of incompetence in either their teaching or research".[24,25]

The cases as listed in Table 1 are only outlines. In almost every case, further details and information show even more clearly that the suppression is illegitimate by normal scientific and academic criteria, and that efforts at suppression are more systematic and sustained than first meets the eye. For example, at the time of the Routley case, several scientists in different organisations were threatened with dismissal or other reprisals for merely giving the Routleys publicly available information and references to public documents.

from Head of Department of Biochemistry (no grounds given).
Status Dismissal efforts renewed 1979; University Council proposed further probation and implied cancellation of accumulated leave entitlement.
Reference (8)

CASE 6: *Name* Professor Clyde Manwell
Position Professor of Zoology, University of Adelaide (1970-).
Background Sent letter (co-author, C.M.A. Baker) to newspaper criticising aspects of the South Australian government fruit-fly spraying programme (1971).
Action (a) Dismissal proceedings initiated by senior Professor of Zoology (1971).
(b) Australian Research Grants Committee grant cut off (1972).
Status (a) Proceedings dropped (1975).
(b) Grants not resumed (1980).
References (a) (9); (b) (10)

CASE 7: *Name* Mr Peter Rawlinson
Position Senior Lecturer, Zoology Department, La Trobe University (1967-).
Background Involved in a series of radio and television interviews critical of the activity of the Forests Commission of Victoria, especially with regard to the spread of cinnamon fungus (January and February 1977), at the time being an elected member of the Conservation Council of Victoria Executive and their spokesperson on forestry issues.
Action Chairman of the Forests Commission of Victoria applied great pressure to the University's Chancellor, Acting Vice-Chancellor and the Deans of Science to take action: nine letters written and hand-delivered between 3rd and 24th February 1977.
Status Unchanged by events. The University Council was informed of the attacks and the appropriate officers resisted all pressures and strongly rejected the allegations made. The Chairman of the Forests Commission was further informed that all Australian University Statutes are framed to allow staff to speak publicly on controversial issues thereby preserving academic freedom.
Reference (7)

CASE 8: *Name* Mr Richard Routley
Position Senior Fellow, Philosophy Department, Research School of Social Sciences, Australian National University (1971-).
Background Wrote a book (co-author, Val Routley), *Fight for the Forests* (11), which was critical of Australian forestry planning and practice. Publication by Research School of Social Sciences arranged (1972).
Action (a) ANU Vice-Chancellor suggested that printing should not proceed unless book was given to the Head of the Forestry Department at ANU, to be revised in accordance with his comments (1973).
(b) R. Routley barred from using Forestry Department library (1974).
Status (a) Three editions (1973, 1974, 1975) of book published and sold out; strong interest in book continues (1980), but funding for further editions or reprints unavailable.
(b) Bar dropped (1974).
Reference (12)

CASE 9: *Name* Mr David Smith
Position Ph.D candidate, Forestry Department, Australian National University (1974-).
Background Ph.D thesis showed inadequacies in current procedures for evaluating effectiveness of pesticides (1977).
Action Two of three examiners rejected Ph.D thesis (1978).
Status Unresolved (1980); working elsewhere (1978-).
Reference (13)

CASE 10: *Name* Dr Peter Springell
Position Principal Research Scientist, Commonwealth Scientific and Industrial Research Organisation (1953-1976).
Background Scientific research undertaken and published on environmental topics; criticism of CSIRO for lack of environmental research (1974-1976).
Action Refusal to allow papers to be published through CSIRO; attempts at dismissal and transfer (1974-1976).
Status Resigned from post (1976), working elsewhere.
Reference (14)

Environmentalists Singled Out?

In cases in which no clear reasons for the suppression action were given, there was a lack of scientific or academic justification for the action. But there are suggestive alternative explanations. It is reasonable in Mann's case to imagine that his public activities as an environmentalist played some role in the initiation of dismissal efforts. And those familiar with Hookey's career would be disinclined to accept that the action taken against him was a result of poor teaching or research, given his initiative in introducing the first Australian undergraduate course in environmental and natural resources law, his publication of papers on Aboriginal land rights and his participation in Papua New Guinea land rights cases.[26,27]

It might be claimed that suppression in the area of environmental research and teaching is not unusual, since suppression is common in all areas of research and teaching. No doubt this is true in a general sense. But as noted before, available evidence suggests that suppression is closely connected with political beliefs, organisational vested interests, paradigm disputes and combinations of these. Each of these factors helps make the environmental area a prime one for suppression. Environmental scholarship is often seen as linked to the 'politics' of the environmental movement; environmental scholarship often presents a challenge to established practices and policies of powerful organisations; and environmental scholarship often challenges the dogmas of various scientific disciplines.

The data presented here suggest an explanation for suppression of scientists based on an understanding of the power structure of science. Suppression *does* occur in a wide range of areas of scientific research and application, from anthropology to engineering to zoology. Tellingly, it occurs most frequently in areas such as environmental studies where opportunities arise for teaching and research which provides a threat to vested interests either inside or outside the scientific community.

The Scientific Elite

There is a considerable literature documenting the existence of an elite group within the scientific community which is characterised by high productivity in scientific research, a high degree of professional recognition of its intellectual achievements, a high degree of internal interaction and clustering at a few select institutions, and a high degree of influence over the professional activities of non-elite scientists[28,29]. This group may be called the *cognitive* scientific elite, because as usually studied it is concerned mainly with academic scientists and with activities relating to the production of scientific knowledge.

It is also possible to focus on a group which may be called the *political* scientific elite, that group of scientists with the greatest political power (both within and without the scientific community) to influence government and corporate policies and to influence developments in the scientific community. The power of this elite is manifest in the promotion of research in certain areas and its restraint in others; in the creation or closing of research institutions; in the hiring or dismissing of staff; in the allocation of funds from specific research projects and in the setting of policies for scientific journals and texts[28,30]. It is clear that there is a considerable degree of overlap between the membership of the cognitive and political scientific elites, as well as a degree of overlap between the activities and characteristics of the two[31]. The cognitive and the political scientific elites are linked in another important way. Leaders of the scientific research community often attempt to use their political power to control what counts as scientific knowledge and how it may validly be obtained and verified, and vice versa.[32,33]

Most studies which treat aspects of the political scientific elite either do not address the question of the advantages and disadvantages of the existence of an elite, or tend to emphasise the functional uses of the elite, as presently constituted, to the scientific community and society as a whole[34]. Few studies challenge the view, common among scientists who support the present organisation of the scientific community, that leading scientists are in positions of power due to their superior scientific abilities and achievements, and therefore are the best people to make decisions about the functioning and development of scientific research and the scientific community.

In one of the few studies of the political scientific elite, Mulkay[28] argues that scientific elites mediate between working research scientists and powerful groups, notably in government and industry, which have an interest in influencing the direction and content of scientific research. In Mulkay's view, then, the scientific elites serve to protect the working scientists from these outside, non-scientific influences.

The object here is not to provide a detailed critique of these views, but rather to present an alternative, partly supplementary perspective. But it may be useful to mention some inadequacies of the views referred to above. First, it has not been demonstrated that there is a correlation between rising within the scientific hierarchy and the making of productive and equitable decisions about scientific priorities. Second, the positions of political power accessible via a scientific career often attract individuals interested in personal aggrandisement[35]. Third, the strong vested interests which most scientific elites have in their reputations and in the perpetuation of particular types and styles of scientific research often lead to scientific* or public interest[36]. These second and third points are seldom taken into account in studies of the scientific elite. Finally, attention mostly has been focussed on justifying, explaining or at most reforming the structure of the current scientific elite. Little attention has been focussed on the possibilities for alternative structures.

According to Mulkay's view, the main source of direct suppression of scientists would be from government and industry. Although this is indeed a primary source of suppression, I argue below that leading scientists and academics have taken an active role in many suppression efforts. More generally, it may be argued that the politics of the scientific community is characterised by what Haberer calls 'prudential acquiescence'[37]. For example, German scientific leaders under Nazism adopted a course of accommodation rather than opposition to outside political direction.

Rather than speak of the scientific elite structure, it is a useful generalisation to speak of the scientific power structure, recognising that a power structure has many levels rather than a single elite versus all the rest. Springell in Table 1 was fairly senior in the scientific hierarchy and hence suppression came from scientists and administrators who might reasonably be called elites. But in the case of Ph.D. candidate Smith, suppression was initiated by lower level scientists, though higher in the hierarchy than Smith.

Three main aspects of the scientific power structure will be briefly discussed here: its relation to powerful groups outside the scientific community, its relation to the scientific community and its relation to scientific knowledge.[38] (see insert on page 43a)

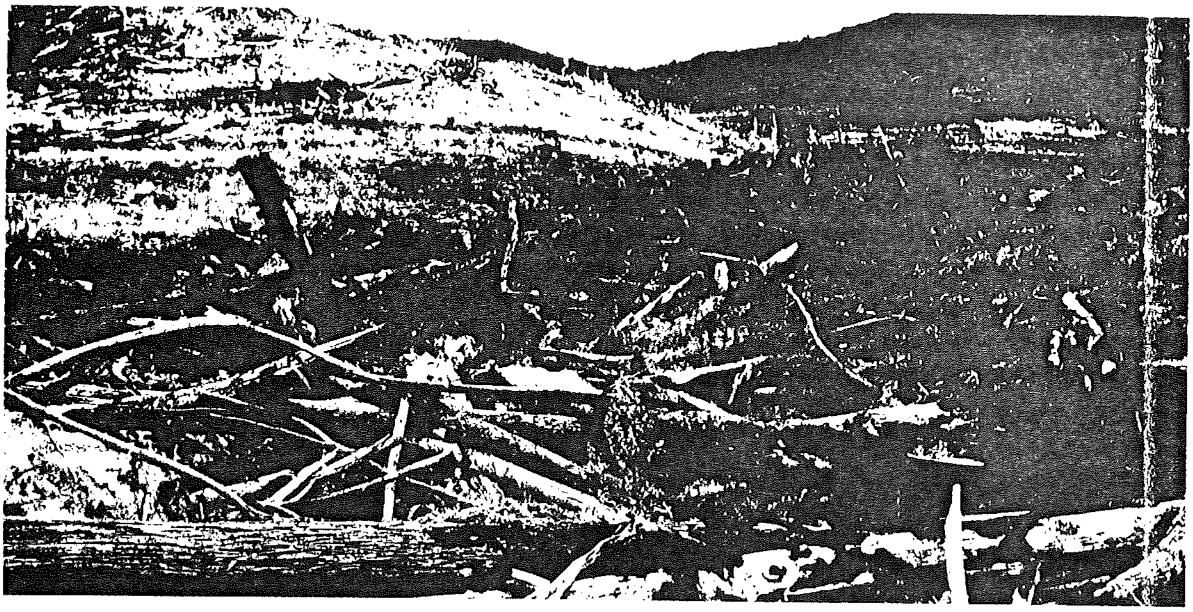
The influence of political and economic interests on the giving of scientific advice, which frequently ends up serving to justify particular policies and practices that promote the interests of powerful groups, is well documented.[36]

The patronage of leading scientists and scientific organisations by powerful non-scientific individuals and organisations is threatened when issues are taken into the domain of public debate, since the legitimacy conferred by the stamp of unanimous scientific approval is undermined. For this reason there is a strong preference among politically powerful scientists for patterns of closed decision-making[37,39]. Secrecy in scientific decision-making is the norm in the processes of allocating research grants, filling posts and making organisational policies — all areas where the influence of, and service to, powerful political and economic interests is crucial. When issues are taken to the public by concerned scientists, this often is seen as inappropriate and even contrary to proper scientific behaviour. Examples can be found in many areas, such as debates relating to nuclear power and nuclear weapons.[40,41]

The Forest Industry

The link between powerful interest groups inside and outside the scientific community helps to explain several of the cases in Table 1. In the forestry area in Australia, there appear to be strong links between university forestry departments, government forest services and research organisations, and the forest industries (timber, pulp, wood chip and other industries based on forest products). These links include informal networks of communication, professional and commercial organisations, clubs, joint conferences, consultation concerning appointments, planning and the like. These social and organisational links lead to the sharing of values and goals, which in turn influence patterns of interaction.

* priorities aligned to sources of patronage rather than in the scientific



Logged eucalyptus forest. The Australian forestry industry exercises a powerful grip on forestry research.

One example of the link between forestry researchers and personnel in the forest industries is the international organisation called the Concatenated Order of the Hoo-Hoo[42]. In Australia, the members of this social and 'service' organisation are "limited to male persons of the full age of 21 years, of good moral character and engaged in forestry, sawmilling, the manufacture of timber products, wood pulp and insulation materials derived from forest products, officials of the forestry service, forest commissions and boards, officers of timber organisations and makers of the allied industries."[43] Despite its name and associated rituals, the Hoo-Hoo plays an important role not only in generally promoting the forest industries but in helping attune forest regulatory agencies and certain forest researchers to the interests of the forest industries.

The movement of key persons between posts in forest industries and government forest services also plays a key role in strengthening the links between the forestry industry and those who conduct forestry research. In particular there are quite a few leading figures in the government forest services who on retirement have taken positions with forest industries.[44] This interaction by personnel interchange is common in many fields beside forestry, such as nuclear power, armaments and agriculture.

In many Australian states, the link between government forest services and the forest industries operates through the structure of the state and federal government bureaucracies. The state cabinets appoint senior officials in the bureaucracy, including the departmental head responsible for forestry. Due to the political influence of industry in lobbying, creating jobs locally and supplying election funds, most departmental heads are chosen to be acceptable to industry. The powers of the departmental heads are considerable. For example, in Victoria, public service regulations make it punishable with dismissal for a state government scientist to criticise the departmental head or departmental policies to three or more people or to make critical comment regarding matters outside one's field. Even if such regulations are seldom applied in practice, their presence is a strong deterrent to the voicing of dissent.

The Dangers of Speaking Out

Although in principle one can speak out in one's 'private capacity', in practice it is easy to get into trouble doing this. For example, John French in September 1976 spoke out as a member of the Native Forests Action Council about the spread of cinnamon fungus in Victorian forests, but was reported in a newspaper article as speaking in his capacity as a government scientist in the CSIRO. A correction was later published, but French also received a letter of concern from the Chairman of CSIRO about the newspaper article.[45]

The basic orientation of the government forest services and many forestry academics is to promote the exploitation of forest resources for the purposes of production and profit.[46] This orientation carries over into the research of government and university foresters, where the criteria for valid and useful knowledge, and how it may be obtained, are influenced by the interests of the forest industries. In other words, the paradigm for many forest researchers is, to put it bluntly, based around ensuring that forests exist primarily for the forest industries.

Anyone who challenges this view — who criticises the way foresters manage the forests, or who promotes an alternative use of forests — is apt to be attacked by the powerful forestry interest group. Indeed, I have been informed of a considerable number of cases of suppression in the forestry area. Access to most jobs in forestry work is directly influenced by the powerful forestry interest group; hence most of those suppressed are hesitant to have their cases publicised. Indeed, the only cases which could be presented in Table 1 — Keane's, Rawlinson's and Routley's — involve individuals not working directly in the forestry area.

Patterns of Suppression

Cases of suppression seem to follow a typical pattern. A person makes a public criticism, a critical analysis in a research document, or some other 'threat' to the forestry establishment. Leading foresters, for example in the government forest services, then apply pressure on the individual's boss to have the criticism stopped, for example by making complaints in person or by telephone, or by sending letters of complaint. Steps taken to prevent recurrence of criticism include informal comments about the individual's competence and motivations, hindering of research, blocking of appointment or promotion, and threats of dismissal. Such efforts (even when immediately unsuccessful as in the cases of Keane and Rawlinson) can by setting an example serve to reduce the future likelihood of research in sensitive areas or of public comment by others.

Besides the forest industries, some other prime sources of suppression — either directly, or indirectly via subservient government and academic bodies — are chemical industries, pharmaceutical industries, electrical industries, mining industries and automotive industries.

The Smear Campaign

One method of suppression deserves special mention: the smear campaign or the threat of it. The following excerpt from a letter by a 'distinguished organic chemist' speaks for itself:

> "I appreciate your views that it would be desirable to have independent tests on water and plants in the area to see if residues of 2,4,5-T are present.
>
> "Regretfully, however, I feel that I should not at any price undertake such tests, or indeed direct anyone in the department . . . to conduct such tests.
>
> "My reasons for this stem from my complete lack of faith in certain government people who, in conjunction with their confraternity in the commercial sphere, tried very hard in a thoroughly despicable way last year to bring discredit upon me, following my criticisms of spraying activities in SA with 2,4,5-T and with amitrole.
>
> "If any tests conducted by me or anyone in my department yielded positive results of an embarrassing nature to the same people, I fear that another smear campaign would be implemented and that rumours would be concomitantly circulated to the effect that we had 'cooked' our findings.
>
> ". . . I trust that you will understand my point of view."[47]

The links between powerful interest groups inside and outside the scientific community can also help explain the cases of Coulter, Manwell and Smith in Table 1. On a number of occasions pressure was brought to bear on Coulter because of activities undertaken in his 'private capacity'.

In 1978 the Bayer company brought an action against the Australian Broadcasting Commission, partly over remarks Coulter had made on a television programme regarding one of its products containing the mutagenic pesticide dichlorvos. The action was subsequently dropped about two years later but in the interim pressure was brought to bear on Coulter through the Agricultural Chemical Trade Association and the Director of the Institute of Medical and Veterinary Science, where Coulter worked.

In 1979 Velsicol Australia complained to the Director of the IMVS about a lecture Coulter had given, in a private capacity, to a Melbourne seminar on pesticides. Coulter had mentioned the way the parent company in the US had handled the information on the carcinogenicity of two of their products, chlordane and heptachlor[48]. By dismissing Coulter, the managers of the IMVS, whatever their reasons, certainly served the interests of corporate and government bodies which produce and regulate the use of chemicals such as dichlorvos, chlordane and heptachlor.

Concerning Manwell's case, speaking out against aspects of a government fruit fly spraying programme would hardly seem grounds for great concern. Indeed, a number of individuals had done this in Adelaide prior to the publication of Manwell's letter — but none were scientists. Manwell was a scientist working in a relevant field. His letter threatened the rationale for an existing government programme benefiting various political and administrative figures, a programme which previously had had the scientific stamp of approval. It is noteworthy that Manwell's writing of a letter to the newspaper was fiercely criticised by certain conservative South Australian parliamentarians prior to the attempt of academic suppression[9]. (In the forestry area also, criticisms by scientists, and criticisms presented in rigorous technical fashion, have induced much stronger responses than less technical criticism by non-scientists.)

In the case of Smith, the suppression took place at a much lower level in the scientific hierarchy. Here a vital factor seems to be the prevalence of a perspective underlying much research on pests, which is based on the extension of engineering concepts and linear analysis. In crude terms, this particular pest control paradigm is essentially 'biocidal': the only solution perceived to the problem of pests is applying pesticides. In this view everything else is treated as an externality of secondary importance. The generation and maintenance of this paradigm is strongly influenced by industries profiting from chemical methods of pest control and by government policies legitimising these methods[49]. Scientists steeped in this pest control paradigm may not have any direct contacts with the chemical and other industries which support and benefit from research done within the paradigm. But such scientists might well be unsympathetic to research, such as presented in Smith's thesis, which questions commonly held beliefs about effectiveness of the biocidal approach.

Seen from the point of view of the interests of powerful non-scientific groups, the scientific power structure serves a valuable function of social control. The patronage of politically powerful scientists provides a ready means for outside interests to influence the direction of scientific research, and to obtain scientific legitimisation for preferred policies. If patterns of control over scientists were less hierarchical, such outside influence would be less easily exercised and less effective.

Areas of Suppression

Karl Z. Morgan. Although not opposed to nuclear energy, Morgan has been consistently outspoken in his criticisms of the nuclear industry's safety record. Highly regarded in the scientific community as the 'Father of Health Physics', he was part of the team which developed the atomic bomb. Later he became a director of the prestigious Oak Ridge National Laboratory. His first brush with the authorities came after he wrote a speech critical of the liquid metal fast reactor. Advance copies were siezed and replaced by an edited version. In 1980, he was relieved of his post as professor at the Georgia Institute of Technology. According to the World Information Service on Energy, "Sources close to Morgan claim that his dismissal is most probably linked to his continuing criticisms of the nuclear industry."

John Goffman and Arthur Tamplin. Presented a paper in 1969 challenging current radiation exposure standards. Attempts were made to censor a subsequent report to the American Association for the Advancement of Science. Goffman's research grant was terminated. Tamplin stayed on at the Lawrence Livermore Laboratory, "essentially a non-person." Later he resigned and moved to the Natural Resources Defence Council.

Thomas Mancuso. Regarded as one of America's most outstanding epidemiologists, Mancuso was awarded a contract by the Atomic Energy Commission in 1964 to study the effects of low-level radiation on the health of workers at the Hanford reprocessing plant in Washington State. In 1974, pressure was put on Mancuso to refute the findings of an independent study which revealed that cancer rates at the plant were five times higher than expected. Mancuso refused and his grant was terminated. His own report estimated that workers at Hanford had 26 per cent higher risk of dying from cancer and that the risks of dying from cancer of the bone marrow was increased by 107 per cent.

Irwin Bross. Funds cut off after publishing the results of a survey showing that children x-rayed in the womb had a three to four times higher chance of developing leukemia than those who had not been x-rayed.

Milton Zaret. First experienced attempts to suppress his research when he reported that microwaves, well within the current exposure limits, could adversely effect the behaviour of rats. Later he established a link between microwave exposure and the development of cataracts. His research grant from the Department of Defence was terminated and he was brusquely told that there is 'no such thing as a microwave cataract'. Zaret now alleges that he is blacklisted from receiving funds from the Department of Defence, the Food and Drug Administration and the Environmental Protection Agency. In an interview with the environmental magazine *Commonweal*, he described most government-sponsored research on the effects of microwaves as 'intelligent looks in the wrong direction'. (See *The Ecologist*, Jan-Feb 1979).

Robert Van Den Bosch. Outspoken critic of the pesticide industry. As a result he suffered frequent attempts to discredit him and to oust him from his post at the University of California — none of which were successful. In his book, *The Pesticide Conspiracy*, he described the pesticide industry as a 'mafia' with "its own lobbyists, front organisations, PR apparatus, and 'hit men'". He also accused the industry of owning "politicians, bureaucrats, researchers, administrators, and elements of the media" and of being quite capable of breaking those who do not conform to its rules. Tragically he died in 1979.

Specialisation and Suppression

Those who rise within the scientific power structure often do so via a successful research career following orthodox research channels in a fairly narrow specialisation. The bases on which power and prestige rest within the hierarchy depend therefore on the status of specialised research within a recognised discipline. In other words, empire-building in scientific organisations tends to follow disciplinary lines[50]. It is no coincidence that the elite body the Australian Academy of Science is a group of specialists.

Disciplines and specialism themselves should not be seen as 'natural' divisions of knowledge, but as socially constructed divisions which are established, maintained or altered on the basis of social conventions and institutional arrangements[51]. Power struggles within scientific organisations thus have several facets. They involve positions within the hierarchy: struggles for appointment, promotion and research grants. They involve the nature of the hierarchy: struggles over specialism and discipline boundaries, as in the setting up of departments and courses. And they involve the standards and frameworks for knowledge: struggles over paradigms and struggles using paradigms as resources.[52]

The status of specialised research within a recognised discipline depends in part on the discipline in question being off limits or opaque to non-specialists and to the public. Only to the extent that the essence of the work in a discipline and its specialities is either a special preserve or else not readily grasped by outsiders is it possible for members of the discipline to claim exclusive rights to judge the importance of work in the discipline.

I have suggested that specialisation and disciplinary exclusiveness serve the interests of many who work in traditional disciplines, especially those who rise to positions of power within these disciplines. With this perspective, it is understandable that many scientists in traditional disciplines would be antagonistic to potentially substantial programmes relating to science which are either truly interdisciplinary or popular with students or the public. Interdisciplinary research and teaching is, by its nature, subversive to that portion of the scientific power structure which is founded on narrow disciplinary research and teaching. Likewise, scientific programmes or ideas that involve the public in active understanding or participation are also a threat to the power structure of science, since the exclusive judgement rights over the development of the discipline are potentially challenged.

The Human Sciences Program

In recent years the environmental area has been a source of scientific research and teaching which is potentially threatening to many parts of the traditional power structure of science. By its nature much environmental research is interdisciplinary. The results of this research often offer a challenge to existing policies and practices of government and industry, and the area is one of high public concern. Such research can thus provide a threat to the hierarchical power structure of science.[53] The same strictures apply to the achievement of successful environmental education.

These points help to explain the cases of Evans, Mann and Springell (Table 1). On the basis of information available to me, the case of Evans provides the best illustration. The Human Sciences Program in which he works has been under attack by various people, especially people in positions of power within traditional departments, from the time it was first proposed in 1970, although the Program has been vindicated in several reviews. The Program is the only one of its kind in Australia to espouse clearly the ideal of holistic education in which a number of different possible approaches to knowledge and understanding (of which science is only one) are studied, with special application to environmental issues. Predictably, some scientists have criticised the lack of a disciplinary base for the Program. Their commitment to specialist, discipline-based perspectives helps to explain their negative evaluation of Evans' research, which is actually above the average for his faculty in terms of quantity.

Antagonism to the Program was strong from some sections of the University even in the days when there was plenty of government money for nearly everyone in the universities; therefore departmental competition for resources cannot be the sole explanatory factor. The recent years of increasingly tight university budgets seem to have provided the extra pressure which led to the attempt to deny tenure to Evans, the only potentially tenurable member of the Program.

A study of environmental programmes in U.S. universities concluded that two features were necessary, though not alone sufficient, for their success:

"1. Substantial or complete control of the faculty reward structure and

2. Freedom to be innovative in introducing course material, educational programs, work study programs, and curriculum requirements for degrees."[54]

Such requirements obviously conflict with the maintenance of the current scientific power structure. Therefore, it is not surprising that decision-making groups within the Australian National University have maintained a tight rein over the academic staff of the Human Science Program with regard to each of these two features.

Institutionalised Suppression

Links between powerful groups inside and outside the scientific community, and vested interests in disciplinary exclusiveness within scientific organisations, are two major features of the power structure of science which lead to suppression of dissident viewpoints. These two features should not be seen simply as bases for overt attempts at suppression such as blocking publications or appointments. The basis for suppression is institutionalised in science through the very nature of scientific research and scientific organisations.

Corporate and government bodies have an important direct influence on the nature of scientific research. This influence operates through funding of research, through availability of jobs in particular areas and industries, and through the general benefits to scientific elites for setting up research bodies that are designed not to challenge the status quo. Many funds are available for studying fossil fuels and nuclear fission, few for studying the conserver society. Lots of investment is made in microprocessors and other labour-saving technology, little is put into industrial democracy. There is plenty of research into how to make war, almost none into how to make peace. In short, existing patterns of funding for science, existing orientations of scientific organisations and current scientific paradigms all tend to discourage or suppress views contrary to the interests of powerful groups in society.

I have argued that the scientific community is based on hierarchies of power as well as status, on specialised disciplinary research and teaching and on the separation of scientific work from the public. Most leading scientists have vested interests in these aspects of science, and this can lead to suppression as in the case of Evans. But it is important to note that hierarchy, specialisation and exclusiveness in science are also valuable to powerful groups *outside* the scientific community. Specialised research is selectively useful to powerful groups who have the resources to hire experts to study and apply it; hierarchically organised scientific organisations mesh well with hierarchical organisations in government and industry; and the chopping up of the learning experience into specialist bits tends to produce scientists who do not question the premises underlying their work. Hierarchy, specialisation and the separation of scientific work from public scrutiny thus help retain the patronage of powerful non-scientific groups.

Specialised research and teaching is much less likely to lead to or serve public campaigns which might damage the interests of groups in government and industry. For example, a great deal of Japanese research of the orthodox, large-scale, discipline-based type — mostly funded by government and industry — was unable to determine the cause of Minamata disease. On the other hand, the local groups of concerned scientists, school teachers and citizens who carried out simple but insightful and wide-ranging experiments were able to trace the disease to mercury poisoning caused by industrial effluents.[55] Ui argues on the basis of cases such as this that scholars working in specialisms in traditional organisations *necessarily* stand on the side of the institutions which produce environmental problems.[56]

In the case of the Centre for Resource and Environmental Studies (CRES) at the Australian National University, the traditional disciplinary approaches used and the traditional hierarchical structure of the organisation make it a very inadequate base for getting to the roots of environmental problems, as I have argued previously[57]. CRES, which was set up by elites in the academic and wider community, was from the beginning strongly oriented towards government and industry rather than to community groups or the general public. That it was also set up by these elites to carry out discipline-based research in a traditional hierarchical structure seems no coincidence.

The Human Sciences Program in which Evans teaches is an example of the holistic approach to knowledge, which involves integrating knowledge, perspectives and methods from different disciplines and world views into a unified framework. By contrast, much multidisciplinary research and teaching such as in CRES involves merely the collection together of narrow specialists from different fields. Multidisciplinary interaction of this latter type poses relatively little threat to traditional patterns of power and professional control.[57,58] This is especially the case when, as in many problem-oriented projects, the work is carried out under the aegis of one particular discipline or approach.[59]

In the above examples a strong connection can be seen between the scientific power structure's links with powerful groups outside the scientific community and the pressures within the scientific community for specialisation and discipline-based research and teaching. Indeed, it may be argued that many characteristics of the scientific community have evolved out of the community's history of interaction with government, industry and other groups. For example, the tendency of academics to avoid the limelight[60], which is part of the scientific ethos, can be interpreted as an adaptive response to avoid alienating potential sources of patronage. More generally, the process of professionalisation of science can be seen as a process of transforming the special knowledge and skills of scientists into social and economic rewards[61]. The scientific power structure would seem to be an important component in this process.

Challenges to the Scientific Power Structure

I have described how the power structure of science is sustained by links with powerful non-scientific groups, by the self-interest of those high in the scientific hierarchy and by the vested interests of the scientific community in specialised, discipline-based research and teaching. However, by no means all scientists acquiesce in these arrangements. Especially in the past decade or so, there have been increasing challenges to the scientific power structure.

One potent challenge to the links between powerful scientists and powerful non-scientific groups is simply public exposure of these links. Such exposure has occurred especially in controversies over issues such as the supersonic transport aircraft, nuclear power, food additives and pesticides[36]. When the public is made aware of conflicts of interests in the roles of scientists[62,63] and becomes aware of the existence of value assumptions underlying statements and advice by scientists, the ability of scientific experts to legitimise policies 41
and practices of government and industry is greatly reduced.

Another development in this area is the creation of scientific research groups which are committed to 'public interest science' and hence less susceptible to pressures or cooption by powerful special interest groups. Examples are the Union of Concerned Scientists in the U.S., the Science Shop at the University of Amsterdam[64] and the citizen-based environmental research groups in Japan[55]. Such initiatives also tend to emphasise interdisciplinary approaches and to modify or replace the traditional hierarchies in science. One model for such efforts is the high level of community involvement in scientific research, reduction in scientific training and orientation of research to practical problems in China in the early 1970s, as described by *Science for the People*[65].

Also important in challenging the power structure of science is challenging traditional paradigms, especially when established ideas and ways of doing research and teaching are clearly linked to vested interests. Challenging paradigms is especially significant when the dispute is taken outside disciplinary boundaries and involves non-scientists.

Another set of challenges to the scientific power structure arises from attempts to change scientific organisations from within, for example, by introducing innovative interdisciplinary research and teaching programmes in areas such as alternative technology, women's studies or participatory democracy. This approach may be one of the most difficult to bring off.
The existing emphases in universities — not to mention 42
government and industry — are predominantly in traditional subject areas, using traditional methods in traditional organisational structures. As a consequence, there is an in-built resistance to changes in such institutions from the usually narrow purposes for which they were designed.

In the area of energy and environment in the US, no holistic study programmes were established at universities before 1971. Thus the programmes followed rather than preceded the development of widespread public interest and definition of the main problems[58]. This suggests that the generation of public interest in issues and the creation of independent, citizen-oriented research groups may have a larger impact on existing scientific institutions than isolated attempts for change from within.

In the meantime, struggles do continue within scientific organisations, of which the cases in Table 1 are a sample. As noted before, many such encounters are hushed up by all concerned. However, the general interests of environmental scholarship are more likely to be served by publicity in at least some cases. Publicity is usually avoided by the individuals and groups carrying out the suppression, especially when their side of the case cannot be openly or readily justified in scientific or academic terms. Also, publicity threatens to expose the existence and methods of operation of the vested interests involved in the suppression effort. In the case of Hookey, no information about the case reached the general university community or the wider community; the individuals and organisational interests which led to the suppression were relatively undisturbed. However, the later case of Evans has generated a number of newspaper articles and letters, petitions, and support groups of staff and students. Even unpublicised cases, such as those of Hookey and Smith, can cause embarrassing divisions and conflicts within the university hierarchy. Publicity and staff or student action as in the cases of Coulter, Evans, Mann, Manwell and Springell can be a real threat to business as usual.

Incidentally, it would seem unwise for those involved in suppression cases to put heavy reliance on staff or professional organisations for support. In many suppression cases such organisations have been conspicuous by their absence. For example, when an engineer who worked for the Electricity Trust of South Australia provided public information to a newspaper, he was "severely criticised by the Institution of Engineers for saying things critical of other engineers"[66]. It seems better to look for support from individuals and groups which are in no way beholden to the groups attempting the suppression.

It would be unrealistic to expect all suppressed scientists to speak out about their cases. Those in the middle stages of a scientific career often have heavy financial or family commitments and can ill afford risking job security or promotion prospects. Those just beginning a career or with a well-established reputation often are in a better position to take risks — both in making scientific innovations[67] and in speaking out and so inviting or challenging suppression — though sacrifices may be entailed in these cases also, for example by jeopardising job prospects or losing pension benefits. But then, power structures of any kind have seldom been reformed without risk or sacrifice.

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44. A few examples:
    Mr C.W. Elsey, a Commissioner of the Forests Commission of Victoria when he retired in January 1973, became a Director of Timber Holdings Limited within two months.
    Dr F.R. Moulds, Chairman of the Forests Commission of Victoria, after retirement became a forestry consultant and also Chairman of Victoria's Timber Promotion Council.
    Mr N. Humphreys as Harvesting Officer of the NSW Forestry Commission in 1979 went on a world trip with a manager of Australian Newsprint Mills Limited to pick out a logging system for their Albury newsprint mill, and shortly after joined ANM as a manager.
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 Material to be inserted after paragraph 3, column 2, page 36

Powerful groups outside the scientific community

Those who are high up within the scientific power structure have considerable interaction with people and organisations outside the scientific community, particularly with those in positions of power[62]. This is inevitable given the sizeable fraction of social resources devoted to science and the important role given to leading scientists in deciding its allocation. A second major source of interaction arises out of the scientific advisory apparatus, again dominated in terms of influence by the most politically powerful scientists[68]. Informal interactions are at least as important as the formal ones, though much harder to document.

Many individuals in the scientific power structure have had little or no scientific training or experience, or have lost touch with what training or experience they once had. Such individuals may suitably be called administrators. Yet many such administrators are in key positions of power concerning scientific matters, especially in government and industrial research bodies. So in actuality there is a meshing of occupational backgrounds as well as of contact and interest between individuals inside and outside the scientific power structure.

The results of the interaction between scientific and non-scientific power brokers can be seen as a quid pro quo. From powerful non-scientists, scientists receive funding, a source of jobs and some prestige. From powerful scientists, the non-scientists receive help in directing scientific research through channels selectively useful to the latters' interests. This is fairly clear in the case of government and industry, under whose direct auspices the bulk of scientific research takes place. It also applies to university research, where the influence of grant money, future job prospects and possible applications of research help channel research into areas selectively useful to powerful groups[69]. For example, Weart has concluded that leading French nuclear scientists after World War II only controlled events so long as matters moved in the directions desired by more powerful, non-scientific groups; in this context, scientists who failed to attract and maintain outside support were shunted aside for the purposes of decision-making about directions for scientific research and development[70].

68. Philip H. Abelson, "The President's science advisors", Minerva, 3, Winter 1965, pp. 149-158.
69. David Elliott and Ruth Elliott, The control of technology (London: Wykeham, 1976), in a careful analysis of models of the status of technocracy, conclude that the model of technocrats as servants of power is most appropriate. Their analysis clearly suggests that the same conclusion applies to scientific elites. The classic statement of the view that universities are structured to serve outside vested interests is Thorstein Veblen, The higher learning in America: a memorandum on the conduct of universities by business men (New York: B. W. Huebsch, 1918).
70. Spencer R. Weart, Scientists in power (Cambridge, Massachusetts: Harvard University Press, 1979).

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3

HIGH TECHNOLOGY IN AUSTRALIA

Ian Anderson

3.1

NEW TECHNOLOGY WILL NOT PROVIDE JOBS

TECHNOLOGY as a provider of jobs is being oversold, according to two researchers from Stanford University. Not only will technical innovations displace workers, but the industry itself will employ comparatively few people. And only a small percentage of these will require advanced technical and scientific skills.

Between now and 1995, the researchers say, less than six per cent of all new jobs in the US will be in technically based occupations. None of the 10 occupations with the largest expected growth in new jobs will be related to technology. Only two of these—teaching and nursing—will require any higher education. Advances in technology will actually lower the job skills required by most workers in the US.

These are controversial conclusions, especially for the nations and cities of the world that are trying to attract and develop the so-called high-technology industries.

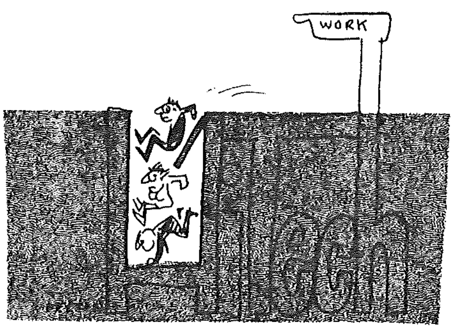
They have been drawn by Henry M. Levin and Russell W. Rumberger from Stanford's Institute for Research on Educational Finance and Governance, which is part of the School of Education.

Levin, a professor of education and economics, and Rumberger, a senior research associate, analysed data from the Bureau of Labour Statistics (BLS), the National Science Foundation, and the Institute for Economic Analysis at New York University.

Their report "Forecasting the Impact of new technologies on the future job market" was released last week by Stanford.

"We are saying," said Levin, "that technology has a place, but by no means a dominant one, in the job market of the future."

"People are confusing the percentage growth in occupations created by new tech-



nology with the total number of jobs created", Levin and Rumberger say.

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While employment in jobs related to technology are expected to increase 46 per cent by 1995, this will account for only about six per cent of all new jobs in the economy.

For example, openings for computer system analysts will increase by 85 per cent, but it is not even among the top 20 occupations when the total number of jobs is tallied.

Almost 14 times as many caretakers (779 000), cashiers (774 000) and secretaries (710 000), will be needed as computer service technicians (53 000) between 1984 and 1995.

Another false assumption is in the type of jobs that new technology will create. "It is misleading to automatically equate new technology with high-level jobs," Rumberger said. "Most of the jobs are in production and office areas that pay below average wages, and require little or no higher education."

Industries that manufacture computers, electronic components, and other technical devices employ 15 per cent or less of the US work force. Also, less than a quarter of the jobs in these industries require substantial knowledge of technology. (This view is supported by recent reports from Sweden that the introduction of numerical control equipment into industry has reduced markedly the level of skills required from experienced operators.)

Nevertheless, Levin and Rumberger stress that more, not less, education is needed in science and technology. But it needs to be a broad education so that retraining is possible. "A narrow, specialised education is more dangerous now than it ever has been," Rumberger says. Moreover, the average citizen needs a broad science background to help them understand many of today's issues.

The work of Levin and Rumberger has already made ripples in some high places. They are working with fellow academics and government representatives in both France and Australia to organise meetings on technology and employment.

But not everyone agrees with their conclusions. They have been criticised in Silicon Valley for being too dependent on projections by BLS, which have been labelled as "notoriously bad" in the area of technology. Also, critics have suggested their work ignores the influence that the boom in technology is having on the overall economy. □

I. Anderson, 'New technology will not provide jobs', *New Scientist*, 10 May 1984, p. 24.

Donald Tomaskovic-Devey & S. Miller

3.2

CAN HIGH-TECH PROVIDE THE JOBS?

In his 1983 State of the Union address, President Reagan asserted that he expected high-technology industries to expand and absorb a rapidly increasing percentage of the labor force. He is not alone in this belief. It has been commonly bruited about in the mass media, and in many political circles, that the glamorous high-technology industries are the key to the resurgence of the American economy. Growing high-tech exports, it is believed, will offset the declining exports and falling employment rate in the mass production smokestack industries. Technological advances and entrepreneurial drive will supposedly solve the twin problems of slow economic growth and unemployment.

What is high-tech?

We decided to examine the record of "high-tech" employment to see whether it might prove to be the panacea so many people claim it is. Before we could do that we needed a definition of "high-tech," which we naively thought would be easy to obtain. We soon discovered that there is no agreed-upon definition of what comprises this important group of industries. Of the several definitions we located, none is compelling and all are suspect. We have come to the conclusion that the phrase "high technology" is not merely a categorization of industries or technologies; it is also political advertising hype for some U.S. industries.

High technology as a gleaming phrase is not dissimilar to the familiar advertising tags of "new" or "new-improved." In an environmentally conscious era marked by the decline of traditional manufacturing industries and the high quality of Japanese products, U.S. technology (and industry) has an unfavorable image. Now in the early 1980s political rhetoric and economic analysis are pointing to two kinds of technology: plain, old, dirty smokestack *technology*, and new, improved, clean *high-technology*. Great political importance has been attached to productivity as a solution and high technology as its wellspring. Industries lucky enough to be tagged with the name "high-technology" are likely to be the main beneficiaries of reindustrialization policies targeted at productivity and exports.

We do not assess the reality or integrity of any single definition of high technology. Rather, we recognize that some combination of industries will benefit from government policies directed toward somebody's conception of the term. Which combination will receive the accolade is uncertain. Senator Paul Tsongas, Democrat from Massachusetts, introduced three bills in January of 1983 to "protect" the development of American high-technology industries. Many states, including Massachusetts and New York, have plans to forge a government-business-academic partnership around high-tech research and development. Which industries will be included, protected, and nurtured in the government courtship of high-tech?

In the uncertain terrain, we located and named three definitions of high-technology and decided to test them by seeing how they affected employment. Each definition uses a different criterion.

Table 1 **Twenty-five High-Technology Industries Classified by Three Definitions (the all-inclusive list).**

Def.	SIC[1]	Name
ab	281	Industrial Chemicals
ab	282	Plastics
abc	283	Drugs
b	287	Agricultural Chemicals
c	348 (19)	Ordnance[2]
ab	351	Engines and Turbines
abc	357	Office and Computing Machines
ac	361	Electrical Distribution Equipment
ac	362	Electrical Industrial Apparatus
c	363	Household Appliances
c	364	Electric Lighting & Wiring Equipment
bc	365	Radio & TV Equipment
abc	366	Communications Equipment
abc	367	Electronic Components
c	369	Misc. Electrical Equipment, Supplies
ab	372	Aircraft and Parts
ac	376	Space Vehicles & Guided Missiles[3]
c	379	Misc. Transport Equipment
abc	381	Engineering and Scientific Instruments
abc	382	Measuring and Control Instruments
abc	383	Optical Instruments
bc	384	Medical Instruments
bc	385	Ophthalmic Goods
abc	386	Photographic Equipment and Supplies
bc	387	Watches, Clocks

[a]Mass definition.
[b]R&D definition.
[c]Sci-Tech definition.
[1] For more on definitions, see Table 2 and text. SIC or Standard Industrial Codes are used in various Commerce Department statistics to identify industries by product.
[2] The SIC code for ordnance was changed from 19 to 348 in 1972.
[3] Data not available prior to 1977.

The "Sci-Tech" definition includes those industries with high levels (13.7 percent or more) of scientists, engineers, and technicians in their labor force.

The "R & D" definition concentrates on industries with an above-average intensity (9.6 percent of investment) of research and development.

The "Mass" definition (so-called because it originated in the Massachusetts Division of Employment Security, and does not have the quasi-formal status of the other two) combines two criteria: a high proportion of scientists and technicians, and a highly skilled labor force.

We defend none of these definitions; the first and last are particularly suspect because changes in employment patterns would affect their validity. All we are doing is using the various criteria that are offered by government and business groups. As we shall see, these criteria lead to different lists of supposed high-technology industries.

If the three definitions are taken together, they cover a group of 25 industries, which are listed in Table 1. From the aggregate list, we have isolated two more categories: (1) the "24-industries" list, which excludes one industry on the roster of 25, space vehicles and guided missiles, for which data are not available prior to 1977; and (2) the "nine-industries" list, comprising the eight industries common to all three of the definitions, plus ophthalmic goods, which in recent years split off from one of the other eight industries in the U.S. Commerce Department's Standard Industrial Codes (SIC).

Part of projecting the future is examining the past. Therefore, Table 2 compares the number of jobs created between 1969 and 1979 in the various lists of high-technology industries with the employment experience in the manufacturing sector, the private sector, and the overall economy.

How many jobs?

Strikingly few of the total number of new jobs in the seventies were created by the three sets of high-technology industries. The number ranges from a low of 71,600 for the "Sci-Tech" industries—employment growth of only 2 percent to a high of 169,000 jobs in the "Mass" industries—a growth of 5.1 percent. But during the 1970s the economy as a whole showed an increase of 19 million jobs—growth of 27.6 percent in employment. Compared to that, high-technology was a very minor job generator.

All in all, the high-technology industries produced jobs at about the same slow rate as manufacturing in general. The process of

Table 2

Employment Levels in High-Technology Industries (various definitions), Manufacturing, and the Private Sector, U.S. 1969-1979.

	(000's) 1979	1974	1969	1969-79 absolute change	% change 69-79	% change 74-79
9-industries[1]	2132.8	1623.2	1688.8	443.9	26.3	28.6
R&D def.	3622.8	3269.3	3467.9	154.9	4.5	10.2
Sci-Tech def.	3645.9	3288.5	3574.3	71.6	2.0	9.5
Mass def.	3492.0	3325.4	3322.8	169.2	5.1	5.0
24-industries	4694.1	4596.0	4834.0	−139.9	−2.9	2.1
All manufacturing	20972	20016	20121	851	4.2	4.8
All private	73870	64050	57914	15956	27.6	15.3
All employment	89482	78334	70141	19341	27.6	14.2

Source: *Employment and Earnings* Vol. 27,3 (March 1980) pp. 58-70. Vol. 16,9 (March 1970) pp. 44-58. Vol. 21,9 (March 1975) pp. 52-64.

[1] The three definitions of high-tech include a total of twenty-five industries. For one of them (SIC 376) data are not available prior to 1977. Thus, "24-industries" is the largest sample of high-tech industries. "9-industries" is made up of the eight that overlapped on all three definitions plus SIC 385 in the 1969 and 1974 data sets.

job generation by high-tech firms is, however, more complex than this statement suggests. When we contrast the "all-inclusive" list (24 industries) with the "all-exclusive" list (the nine industries shared by all three definitions), we find a much more variable employment picture.

The all-inclusive classification suffered a *net loss* of about 140 thousand jobs in the seventies. A tremendous drop in employment in a few industries produced this contraction. Industrial chemicals (SIC 281) declined by 142.9 thousand jobs, ordnance (SIC 19 in 1969 which was changed to SIC 348 by 1979) decreased by 267 thousand jobs, and aircraft and parts (SIC 372) lost 200 thousand jobs over the ten-year period. In addition, plastics (SIC 282), electrical distribution equipment (SIC 361), household appliances (SIC 363), radio and TV equipment (SIC 365), engineering and scientific instruments (SIC 381), and watches and clocks (SIC 387), all reduced employment over that period. Employment gains in the other fifteen industries did not offset the losses in these nine. Explanations for this net loss include the decline in war purchases for SIC 348 and 372 (ordnance and aircraft) by the U.S. government after Vietnam. For the others, some combination of loss of demand to foreign competition (for example, 365, radio and TV equipment) and automation (281, industrial chemicals) led to a decline in employment.

The all-exclusive nine-industry classification suggests a much rosier employment potential. These nine industries added 443.9 thousand jobs. Although this is a small proportion of total U.S. employment growth (2 percent) over the ten-year period, the growth rate of these nine was similar to that of the economy in general and much higher than that for manufacturing as a whole. This finding should be understood in terms of the *variability* of manufacturing employment. While a limited set of industries grew quickly (the nine-industry definition) the broad pattern (24-industry definition) of high-technology employment growth was one of stagnation or decline. As some sectors of "high-technology" manufacturing add employees, others contract. For example, office and computing machines (SIC 357) and communications equipment (SIC 366) increased their labor force, while industrial chemicals (SIC 281) and ordnance (SIC 348) decreased theirs.

High-tech employment growth is at least partially tied to military spending. Much of the growth in high-tech employment was concentrated in the most recent post-Vietnam years. In the late seventies high-tech job creation accelerated in all except the "Mass" industries. This growth is only now catching up with the high levels of the Vietnam period. Certainly, it may be fueled further by the inflated military budgets of the Reagan administration.

Demand and employment

The growth in demand for high-tech goods has far outstripped the growth of high-tech employment. In the seventies, according to the most restricted and favorable definition (nine-industries), the output (value added) of high-tech goods grew by 144 percent, while its employment grew by 26 percent over the same period. Using the most inclusive definition ("24 industries"), value added grew more slowly by 99 percent, while employment actually declined. High-tech was not more efficient in producing employment from increased demand than were manufacturing industries in general. For the en-

Table 3 **Growth in Value Added and Employment in U.S. High-Technology Industries (various definitions) and Manufacturing Sector.**

	1967-77 percent growth value added	1969-79[1] percent growth employment
9-industries[2]	144	26
R&D def.	97	5
Sci-Tech def.	132	2
Mass. def.	88	5
24-industries	99	–3
All manufacturing	123	4

Source: Computed from *Statistical Abstract of the United States 1980.* Table 1451, pp. 814-819 and Table 2 above.

[1] Value added data are not presently available for all industries in the various high-tech definitions after 1977. The 1967-77 and 1969-79 data for Value Added and Employment should be treated as rough comparison.

[2] See Table 2 for explanation of various high-technology definitions.

tire manufacturing sector it took a growth in demand of about 30 percent to produce employment growth of one percent. The "24-industry" and "Sci-Tech" lists of high-tech industries were substantially worse than manufacturing in general at increasing employment. The other definitions ("R&D" and "Mass") fared slightly better than manufacturing as a whole in converting demand into jobs. Only the "nine-industry" definition produced relatively large employment growth with growing demand. But that growth is not reassuring, if it is compared to growth in the economy as a whole.

Of the more than 19 million jobs added to the U.S. economy between 1969 and 1979, only 2 percent (440,000 jobs) are attributable to employment expansion in high-tech occupations (using the fastest growing, most favorable "nine-industry" definition). Even during the period from 1974, a trough year in high-tech employment, to 1979, these industries still furnished less than 5 percent (510,000 jobs) of the growth of total employment. If that figure were doubled to a million jobs in ten years, it still would fall far short of solving problems of unemployment and unevenness in labor supply.

Of course, the employment picture depends to a great extent on which high-tech industries are favored by general economic conditions or specific economic policies. More jobs will be created if those enterprises in the "nine-industry" definition are favored than if a more global "24-industry" grouping is the target of economic incentives. Since the decision is a political one, dependent on industry lobbying, it is more likely that the all-inclusive list will be favored, with its lower employment gain.

A spokesperson for high-tech industries, such as the High-Tech Council in Massachusetts, might argue that much high-tech employment is generated indirectly. Software companies, keypunchers, people who work on computers or word processors, and many other employment slots are created outside of high-tech industries. There is some truth in the suggestion that new employment, particularly software employment, has been generated by high technology. The occupations of computer programmer and systems analyst are growing, spawned by the advent of computers. But as these fields grow, more traditional occupational fields diminish. Programmers often replace bookkeepers and accountants. Increased secretarial efficiency through the use of word processors reduces the need for secretaries. Certainly, robotics on the assembly line and "intelligent" cash registers reduce both the skill and number of required workers. Individual employers selectively reduce their labor forces by using high-tech equipment. The general point is that the loss of positions in firms purchasing high technology more than offsets the employment gain in firms producing high-tech equipment.

Who gets the jobs?

Half the people employed in high-tech industry are women. This large percentage is not the result of affirmative action hiring. Most of these women are not engineers or computer programmers. Rather they are production workers, whose jobs Richard Howard has described in these terms: "low-wage, dead-end jobs, unskilled, tedious work, and exposure to some of the most dangerous occupational health hazards in all of American industry" ("Second

Table 4 **Production Workers, Women, Minorities as a Percent of High-Technology Industries (9-industry definition). Manufacturing Sector, and Total Employment.**

	Percent[1] production workers	Percent[2] women	Percent[2] minorities
High-tech	50	36	8
Manufacturing sector	72	31	11
Total employment	82	41	11

Source: *Employment and Earnings*, March 1980, Table B-2, pp. 58-66. *Employment and Earnings*, January 1979, Table 30, pp. 179-80.

[1] Data for 1979.

[2] Data for 1978.

Class in Silicon Valley," *Working Papers*, August 1981).

High-tech firms employ proportionally fewer production workers and members of minority groups than do manufacturing firms and the private sector in general (see Table 4). High-tech industry employment is no more (in fact, it is less) attractive to production workers than other manufacturing industries. Not only are the wages low and the work hazardous, but employment is not stable. As the low percentage of production workers in Table 4 indicates, these industries are among the most automated in the U.S. economy. We can expect further loss of production jobs as these industries use their own products and engineering talent to limit their work forces further or, as in the recent case of Atari, move their production to low-wage export platforms like Singapore. For these reasons the potentially de-skilled low-wage production jobs that now furnish some employment in high-tech firms will probably not increase as quickly as the sales of the industries as a whole.

Regional issues

Table 5 shows the regional distribution of high-tech employment, according to the most restricted definition, now down to seven industries (see footnote to table). The table shows that the older industrial areas, with their high unemployment rates, actually lost high-tech jobs between 1972 and 1976, the latest year for state level data. Two contrasting developments are immediately apparent from the table. The first is that much high-tech employment is in the older industrial states. The second is that as high-tech employment expanded, those states suffered a net loss. If the pattern of regional employment growth between 1972 and 1977 continues, significant increases in employment generated by high-tech will not occur in the older industrial states. Can those areas, with their expensive urban infrastructure, high wages, and large poor populations, compete with the union-free, low-wage, low-tax South and West?

New England is usually cited as an example of how a region that once was declining can compete successfully for high-tech jobs. Massachusetts, it is said, improved the "business climate" for high-technology firms by limiting property and business taxes as well as by spending a good deal of money on advertising designed to attract firms. Massachusetts was, however, in a special position to succeed in this because it already had a reputation as a high-tech state, and it offered both a highly skilled labor force and the amenities of comfortable living.

In general, however, slow growth in overall demand and competition from other parts of the United States and from abroad causes high-tech firms —like other manufacturing industries—to flee high-cost older industrial states in search of cheaper production costs. Clearly, high-technology industries will not produce many jobs for the regions of the United States that most need those jobs.

The regional distribution of high-technology growth demonstrates the limited usefulness of expansion in these industries. High-tech growth has taken place in areas of low wages and low unionization. The boom in high-tech production and employment offered by Secretary of Defense Caspar Weinberger in the form of greater military spending will not evenly benefit the dif-

Table 5 High-Technology Employment (7-industry definition) by Region, 1972-77.[1]

Region[2]	1977 (000's)	1972 (000's)	Absolute change	Percent change
New England	166.3	136.9	30.6	21.4
Older Industrial	447.1	459.7	–12.7	–2.8
South	99.3	78.4	20.9	26.6
West	351.6	261.6	90.0	34.4
U.S.[3]	1590	1396	194	13.9

Source: U.S. Census of Manufactures, 1977.

[1] This definition of High-Technology included SIC codes 283, 357, 366, 367, 382, 383, 386. 381 was omitted because employment was so low in surveyed states that it was not included in the census' regional tables.

[2] A sample of states was taken for each region. The sample included all states that had high-technology industries among their top five manufacturing industries plus a few large states with substantial employment. New England includes Massachusetts, Connecticut, and New Hampshire. The Older Industrial region includes Michigan, New Jersey, Indiana, Illinois, and New York. The Southern region includes Georgia, West Virginia, North Carolina, Florida, Tennessee, and Alabama. The Western region includes the states of Arkansas, Louisiana, Texas, Arizona, Colorado, California, and Washington.

[3] Data for U.S. are for the whole country.

63 ferent regions of the country.

The political decision to favor high-technology, high-productivity, high-export industries is only beginning to be discussed. Those industries which benefit from targeted economic incentives may grow and provide increased employment. Unfortunately, that employment is unlikely either to be substantial or to benefit those regions or workers in the greatest need of a job boost. High-tech is not an economic panacea.

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D. Tomaskovic-Devey & S. Miller, 'Can high-tech provide the jobs?', *Challenge*, vol. 26, no. 2, 1983, pp. 57–63.

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3.3

HIGH TECHNOLOGY TODAY AND TOMORROW

High technology enjoys high visibility. Industry developments are tracked closely in the United States and abroad; and the implications for productivity, international competition, national defense, and the general standard of living are of increasing interest. Many States and some major cities have established task forces to assess the potential of high technology to provide employment opportunities and to develop incentives to attract high tech industries.

Although industries that manufacture computers and office equipment, electronic components and new drugs and medicines generally are among those classified as high tech industries, experts differ as to the makeup of the high tech group. There is no widely accepted definition of high technology industries, and they have been defined in many ways. In this article, we set forth various concepts of high technology and consider its effect on employment during the 1970's and through the mid-1990's.

The criteria generally used to classify high tech industries are research and development (R&D) expenditures, the use of scientific and technical personnel relative to total employment, and product sophistication. Employing these criteria, we developed three definitions of high tech to analyze employment trends in these industries. Our analysis indicates 50
that:

- Employment in high tech industries increased faster than average industry growth during the 1972–82 period.
- High tech industries accounted for a relatively small proportion of all new jobs nationwide, but provided a significant proportion of new jobs in some States and communities.
- About 6 out of 10 high tech jobs are located in the 10 most populous States.
- States with relatively high proportions of employment in high tech industries are generally small; most are in the Northeast.
- Through 1995, employment in high tech industries is projected to grow somewhat faster than in the economy as a whole.
- High tech industries, even broadly defined, will account for only a small proportion of new jobs through 1995.
- Scientific and technical workers, while critical to the growth of industry and the economy, will account for only 6 percent of all new jobs through 1995.

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A look at the concepts

Our examination of published reports on high technology prepared by private organizations and Federal and State agencies indicates a variety of approaches to identifying high technology industries. One approach used by a State agency, for example, involved a review of the U.S. Government's Standard Industrial Classification (SIC) manual in which 20 industry groups were designated as high tech based on the perceived degree of technical sophistication of the products.[1] One limitation of this method, and others which focus on the nature of the product, is that it is highly subjective. Moreover, as Robert Vinson and Paul Harrington point out in an article on high technology industries in Massachusetts, the degree of technical sophistication of the product is of less significance than the complexity of the production process for those interested in the implications of high tech for capital and labor force requirements.[2]

A concept of high technology included in a document prepared by the Congressional Office of Technology Assessment illustrates a much broader and complex approach in which a series of factors are considered in developing a concept of high tech firms and industries.[3] The office describes high technology firms as ". . . companies that are engaged in the design, development, and introduction of new products and/or innovative manufacturing processes through the systematic application of scientific and technical knowledge . . .". It points out that these companies typically use state-of-the-art techniques, have a high proportion of R&D costs; employ a high proportion of scientific, technical and engineering personnel; and serve small, specialized markets. The report goes on to say, "A high technology industry is a group of firms, producing similar or related products, that includes a high proportion of high technology firms."

As suggested earlier, definitions of high technology vary considerably. Federal agencies, including the Department of Defense, the Securities and Exchange Commission, and the Department of Commerce have formulated definitions of high technology to suit their own particular research needs.

An example: the set of definitions included in a report by the International Trade Administration, Department of Commerce, which examines U.S. competitiveness in high technology industries.[4] Four techniques for defining technology intensive trade are presented; one identifies industries and three focus on products.

The industry-based definition of technology intensive trade, developed by Michael Boretsky, uses the two measures frequently employed in examining high technology: R&D expenditures as a percentage of industry value added, and industry employment of scientists, engineers, and technicians as a proportion of the industry work force.[5] He identified two groups of industries based on the magnitude of R&D expenditures and employment of scientists, engineers, and technicians: technology intensive industries and high technology industries. Technology intensive products and others are not separately identified. The three product-based definitions also help in evaluating competitiveness in high technology industries. In the mid-1970's, Regina Kelly used R&D expenditures by product field and value of product shipments to develop intensity ratios.[6] She ranked products by R&D "intensity" and classified them by technology. Kelly designated the first quartile of R&D intensities as high technology goods. Subsequently, she refined her analysis and considered product groups with above average R&D intensities as technology intensive. In 1980, C. Michael Aho and Howard Rosen basically used the Kelly methodology to identify technology-intensive product groups.[7] These researchers used more recent data and the Standard International Trade Classification. More recently, Lester Davis used input-output analysis and R&D expenditure and shipment data by product group to develop an index of technological intensity.[8] Using an input-output matrix, Davis determined the value of R&D embodied in the various inputs used to make the products and the percentage of R&D embodied in the final product. He then arrived at total R&D by combining the indirect R&D (R&D contributed by inputs) with the value of direct R&D (R&D expenditures on product development). Davis ranked product groups according to total R&D to shipments intensity, with only those goods showing a significant R&D intensity (rather than simply above average) designated as high tech products.

A definition by Ann Lawson in an article in the Department of Commerce's *Industrial Economic Review* includes industries "possessing above average levels of scientific and engineering skills and capabilities, compared to other industries; and currently experiencing the accelerating technological growth associated with the germination and evolution stages along their respective S-curves."[9]

Selecting three groups of industries

Because there is no widely accepted definition of high technology industries, we believe it is useful to illustrate employment trends under a range of concepts. As indicated, the concepts underlying most definitions of high technology use one or a combination of three factors (1) the utilization of scientific and technical workers, (2) expenditures for research and development, and (3) the nature of the product of the industry. We have selected three groups of high technology industries based on these concepts.

We have defined industries according to the Standard Industrial Classification (SIC) at the 3-digit detail. We would have preferred to use 4-digit detail, but data were not available. We made an exception for R&D laboratories (SIC 7391), because, for this industry, data were available, and the other industries in SIC 739 have high levels of employment but little or no involvement with high technology. We defined scientific and technical workers as engineers, life and physical scientists, mathematical specialists, engineering and science technicians and computer specialists. We refer to these workers as *technology-oriented workers*. We excluded government, colleges, and universities, although some of their activities are no doubt high tech-oriented, such as some research conducted in higher educational institutions and in some government agencies. There was no realistic way to estimate the small proportion of employment associated with these activities.

Data on research and development expenditures are compiled annually through surveys conducted by the National Science Foundation. The most recent data available are for 1980. Statistics on employment of scientific and technical workers by industry are presented in the Bureau's national industry-occupation matrix. The most current matrix available presents data for 1982.

Group I. The criterion for inclusion in this group is solely the utilization of technology-oriented workers. We included an industry if technology-oriented workers accounted for a proportion of total employment that was *at least one and a half times the average for all industries.* (See table 1.)

To provide a reasonable definition but very broad coverage, we set the cutoff at 5.1 percent of total employment.

Table 1. Employment in high technology industries, 1972, 1980, and 1982
[In thousands]

SIC	Industry	High-tech group[1]			Employment			Percent change	
		I	II	III	1972	1980	1982	1972–80	1972–82
131	Crude petroleum and natural gas	X			139.3	219.6	281.7	57.7	102.2
162	Heavy construction, except highway and street	X			495.1	658.5	633.9	33.0	28.1
281	Industrial inorganic chemicals	X		X	141.2	161.1	153.5	14.1	8.7
282	Plastic materials and synthetics	X		X	228.7	204.8	182.7	-10.0	-20.1
283	Drugs	X	X	X	159.2	196.1	199.8	23.2	25.5
284	Soaps, cleaners, and toilet preparations	X		X	122.4	140.9	145.3	15.1	18.7
285	Paints and allied products	X		X	68.6	65.1	59.7	-5.1	-13.0
286	Industrial organic chemicals	X		X	142.8	174.1	174.3	21.9	22.1
287	Agricultural chemicals	X		X	56.4	72.0	67.1	27.7	19.0
289	Miscellaneous chemical products	X		X	90.0	93.3	91.5	3.7	1.7
291	Petroleum refining	X		X	151.4	154.8	169.0	2.3	11.6
301	Tires and inner tubes	X			122.1	114.8	101.9	6.0	-16.5
324	Cement, hydraulic	X			31.9	30.9	28.5	-3.1	-10.6
348	Ordnance and accessories	X		X	81.9	63.4	71.4	-25.6	-12.8
351	Engines and turbines	X		X	114.6	135.2	114.8	18.0	0.2
352	Farm and garden machinery	X			135.0	169.1	130.8	25.3	-3.1
353	Construction, mining, and material handling machinery	X			293.7	389.3	340.9	32.6	16.1
354	Metalworking machinery	X			286.0	373.1	320.3	30.5	12.0
355	Special industry machinery, except metalworking	X		X	176.9	207.3	179.4	17.2	1.4
356	General industrial machinery	X			267.5	323.7	283.2	21.0	5.9
357	Office, computing, and accounting machines	X	X	X	259.6	432.2	489.7	66.5	88.6
358	Refrigeration and service industry machinery	X			164.4	174.2	161.3	6.0	-1.9
361	Electric transmission and distribution equipment	X		X	128.4	122.5	110.1	-4.6	-14.2
362	Electrical industrial apparatus	X		X	209.3	239.9	211.8	14.6	1.2
363	Household appliances	X			186.9	163.2	142.0	-12.7	-25.0
364	Electric lighting and wiring equipment	X			204.4	209.2	186.9	2.4	-8.6
365	Radio and TV receiving equipment	X		X	139.5	108.8	94.6	-22.0	-32.2
366	Communication equipment	X	X	X	458.4	541.4	555.7	18.1	21.2
367	Electronic components and accessories	X	X	X	354.8	553.6	568.7	56.0	60.3
369	Miscellaneous electrical machinery	X		X	131.7	152.1	141.3	15.5	7.3
371	Motor vehicles and equipment	X			874.8	788.8	690.0	-9.8	-21.1
372	Aircraft and parts	X	X	X	494.9	652.3	611.8	31.8	23.6
376	Guided missiles and space vehicles	X	X	X	92.5	111.3	127.3	20.3	37.5
381	Engineering, laboratory, scientific, and research instruments	X		X	64.5	76.8	75.7	19.1	17.4
382	Measuring and controlling instruments	X		X	159.6	245.3	244.3	53.7	53.1
383	Optical instruments and lenses	X		X	17.6	33.0	32.5	87.5	84.7
384	Surgical, medical, and dental instruments	X		X	90.5	155.5	160.4	71.8	77.2
386	Photographic equipment and supplies	X		X	117.1	134.6	138.3	15.0	18.1
483	Radio and TV broadcasting	X			142.7	199.6	216.4	39.9	51.6
489	Communication services, n.e.c.[2]	X			29.7	66.1	91.0	122.6	206.4
491	Electric services	X			312.0	391.0	415.1	25.3	33.0
493	Combination electric, gas and other utility services	X			183.4	196.7	198.4	7.3	8.2
506	Wholesale trade, electrical goods	X			331.2	421.4	434.9	27.2	31.3
508	Wholesale trade, machinery, equipment, and supplies	X			868.6	1,307.7	1,344.9	50.6	54.8
737	Computer and data processing services	X		X	106.7	304.3	357.5	185.2	235.1
7391	Research and development laboratories	X		X	110.7	163.1	162.7	47.3	47.0
891	Engineering, architectural, and surveying services	X			339.3	544.9	568.7	60.1	67.6
892	Noncommercial educational, scientific and research organizations	X			111.8	113.5	117.8	1.5	5.4

[1] *Group I.* Includes industries with a proportion of technology-oriented workers (engineers, life and physical scientists, mathematical specialists, engineering and science technicians and computer specialists) at least 1.5 times the average for all industries.

Group II. Includes industries with a ratio of R&D expenditures to net sales at least twice the average for all industries.

Group III. Includes manufacturing industries with a proportion of technology-oriented workers equal to or greater than the average for all manufacturing industries, and a ratio of R&D expenditures to sales close to or above the average for all industries. Two nonmanufacturing industries which provide technical support to high tech manufacturing industries also are included.

[2] Not elsewhere classified.

However, we excluded industries with fewer than 25,000 workers. A total of 48 industries makes this the broadest of the three groups. As indicated in table 1, manufacturing industries account for 3 of every 4 industries in this category, with the remainder in mining, construction, transportation and public utilities, and trade and services.

Group II. R&D expenditures were the factor used to select this group of industries. We included an industry if its *ratio of R&D expenditures to net sales was at least twice the average for all industries.* The cutoff point, 6.2 percent, was set high to capture only those industries, such as drugs and communication equipment, heavily involved in developing new products. Because the National Science Foundation data show little R&D outside of manufacturing, we excluded other industries. This group, with only six industries, is the narrowest of the three groups of high tech industries. The industries, as expected, fall into all three groups.

Group III. The criteria for this group are both the utilization of technology-oriented workers and R&D expenditures. In addition, we excluded some industries based on their major products.

We included manufacturing industries if the proportion of technology-oriented workers relative to total employment in the industry was *equal to or greater than the average for all manufacturing industries (6.3 percent) and the ratio of R&D expenditures to sales was close to or above the average for all industries (3.1 percent).* We added two industries which provide technical support to manufacturing industries, computer and data processing services (SIC 737) and R&D laboratories.

Group III, with 28 industries, provides a scope of coverage between groups I and II. It excludes most nonmanufacturing industries that are in group I but which have little R&D activity (and therefore little new product development), such as engineering and architectural services and radio and TV broadcasting. The exclusion of nonmanufacturing industries is common in definitions of high tech industries. Group III also excludes some manufacturing industries found in group I, such as motor vehicles, which did not meet both criteria, and certain machinery industries, which met the criteria, but whose products we did not consider high technology. However, using both criteria, we included some manufacturing industries not in group II, such as those in the instruments, chemicals, and electrical equipment groups, industries with moderately high R&D to sales ratios that appear on many lists of high technology.

Employment trends during 1972–82

Employment in high technology industries, no matter which of the three definitions is used, increased faster than all wage and salary employment between 1972 and 1982. (See table 2.) Group II employment, however, increased significantly faster, 39.8 percent, nearly twice as fast as the 20.1-percent increase in total employment. Group III employment increased 27.3 percent and group I, only 23.6 percent. Over the period, each group increased slightly as a percentage of total wage and salary employment, group I from 13.1 to 13.4 percent, group II from 2.4 to 2.8 percent, and group III, from 5.8 to 6.2 percent.

The contribution of high tech industries to total employment growth over this period, no matter how high tech is defined, was relatively small. Group I accounted for 15.3 percent of new wage and salary jobs, group II, 4.7 percent, and group III, 7.9 percent.

Growth was not steady. For example, when wage and salary employment declined below its 1980 level during the 1981–82 recession, employment in group I, which includes some *cyclical* industries, also declined. During this period, employment in group III held steady, and group II continued to grow, despite the recession.

Among the industries included in the high technology groups, growth rates varied widely during 1972–82. Computer and data processing services had the fastest growth, 235.1 percent, followed by communication services, 206.4, crude petroleum and natural gas extraction, 102.2, office,

Table 2. Employment in three groups[1] of high technology industries, 1972, 1980, 1982, and projected 1995
[In thousands]

Employment grouping	Employment			Projected 1995 employment alternatives			Percent change								
	1972	1980	1982	Low	Moderate	High	1972–80	1972–82	1980–95 Low	1980–95 Moderate	1980–95 High	1982–95 Low	1982–95 Moderate	1982–95 High
All wage and salary workers	76,547.0	92,611.2	91,950.1	115,382.9	117,744.9	120,531.1	21.0	20.1	24.6	27.1	30.1	25.5	28.1	31.1
Group I	9,989.7	12,550.1	12,349.6	16,260.7	16,612.9	16,931.6	25.6	23.6	29.6	32.4	34.9	31.7	34.5	37.1
Percent of total employment	13.1	13.6	13.4	14.1	14.1	14.0	—	—	—	—	—	—	—	—
Group II	1,819.4	2,486.9	2,543.0	3,517.5	3,409.6	3,452.9	36.7	39.8	41.4	37.1	38.8	38.3	34.1	35.8
Percent of total employment	2.4	2.7	2.8	3.0	2.9	2.9	—	—	—	—	—	—	—	—
Group III	4,468.9	5,694.8	5,691.1	7,746.6	7,719.8	7,890.0	27.4	27.3	36.0	35.6	38.5	36.1	35.6	38.6
Percent of total employment	5.8	6.2	6.2	6.7	6.6	6.5	—	—	—	—	—	—	—	—

[1]Each group equals the sum of employment in detailed industries listed in Table 1

computing, and accounting machines, 88.6, and optical instruments, 84.7. Radio and TV receiving equipment declined by 32.2 percent, household appliances by 24.0, motor vehicles by 21.2, and plastic materials and synthetics, by 20.1 percent. Some of the declines in employment are directly attributed to the 1981–82 recession.

Employment through 1995

Every other year, the Bureau prepares employment projections of roughly 12 years by industry under alternative scenarios. The latest projections of moderate, high, and low growth extend through 1995.[10] Because of employment declines in certain industries in 1981 and 1982, projected growth in wage and salary employment and employment in groups I and III is actually greater from 1982 to 1995 than from 1980. In group II, which had increasing employment from 1980 to 1982, this is not the case. For each of the three groups, using either 1980 or 1982 as a base, high tech employment is projected to grow somewhat faster than total wage and salary employment under all three alternatives. (See table 2.)

For group II, the low growth alternatives shows higher 1995 employment than the moderate alternative. This is because higher defense spending is assumed in the low alternative than in the moderate alternative, and group II has a high proportion of its employment in three defense-related industries, communication equipment, aircraft and parts, and guided missiles and space vehicles. In addition, these projections indicate that certain industries which grew very rapidly over the 1972–82 period, including computer and data processing services and office, computing, and accounting machines, will grow at a slower rate over the 1982–95 period, although still well above the average for all industries.

High tech and displaced workers. The Bureau's projections indicate that between 23.4 and 28.6 million new wage and salary jobs will be created between 1982 and 1995. We estimate that between 1.0 and 4.6 million of these jobs will be in high technology industries. Growth in group I will account for 16 or 17 percent of all new jobs, depending on the projection used, while growth in group II will account for 3 or 4 percent and group III, 8 or 9 percent. The great majority of new jobs will be in industries other than high technology. Therefore, displaced workers and others seeking jobs, and governmental and community organizations seeking to attract jobs to their regions, would be well advised not to limit their search to high tech industries only.

Table 3. Occupational distribution in selected *rapidly growing* high-technology industries and the motor vehicle manufacturing and blast furnaces and basic steel industries, 1980

[In percent]

Occupation	Office, computing, and accounting machines	Electronic components	Computer and data processing services	Blast furnaces and basic steel products	Motor vehicles
Total	100.0	100.0	100.0	100.0	100.0
White-collar	66.3	37.7	96.0	17.7	20.2
Tech-oriented	27 3	15.0	26.0	3.9	5.9
Engineers	11.9	7.2	1.7	1.8	3.4
Life and physical scientists	.1	.2	.1	.2	.1
Mathematical specialists	([1])	([1])	(.3)	([1])	(.2)
Engineering and science technicians	8.8	6.4	2.7	1.5	1.7
Computer specialists	6.5	1.2	21.2	.4	.5
Blue-collar	32.7	61.0	3.4	80.2	76.8
Service	1.0	1 3	.6	2.1	3.0

[1]Less than 0.1 percent.

Table 4. Projected 1982–95 growth in technology-oriented occupations

[In thousands]

Occupational group	Employment				Change 1982–95					
	1982	Projected 1995			Number			Percent		
		Low	Moderate	High	Low	Moderate	High	Low	Moderate	High
All occupations	101,510	124,846	127,110	129,902	23,336	25,600	28,392	23.0	25.2	28.0
Professional	16,584	21,545	21,775	22,325	4,961	5,191	5,741	29.9	31.3	34.6
Technology oriented	3,287	4,777	4,795	4,907	1,490	1,508	1,620	45.3	45.9	49.3

One additional factor may have a negative effect on the ability of high tech industries to save economically depressed industries and provide jobs for displaced workers. The occupational composition of many rapidly growing high tech industries is significantly different from other manufacturing industries that have suffered in recent years. For example, about three-fourths of the workers in the blast furnaces and basic steel industry and the motor vehicles industry are blue-collar workers. These are the workers who have been displaced. However, many high tech industries, especially those projected to grow the fastest, have a much smaller proportion of their workers in these occupations. (See table 3.)

High technology occupations

High technology occupations have also been the subject of much concern recently, although here too data on current and projected employment and clear definitions of what occupations are included have been lacking.

Occupations which clearly meet the definition of high technology workers are engineers, life and physical scientists, mathematical specialists, engineering and science technicians, and computer specialists. Most workers in these technology-oriented occupations are directly involved in developing or applying new technologies.[11] Their work requires in-depth knowledge of theories and principles of science, engineering, and mathematics underlying technology—a knowledge which distinguishes them from computer operators, computer service technicians and other high tech machinery repairers, or workers in a wide range of occupations who use word processing machines, computers or other high technology products, but rarely have—or need—such in-depth knowledge. Workers in these technology-

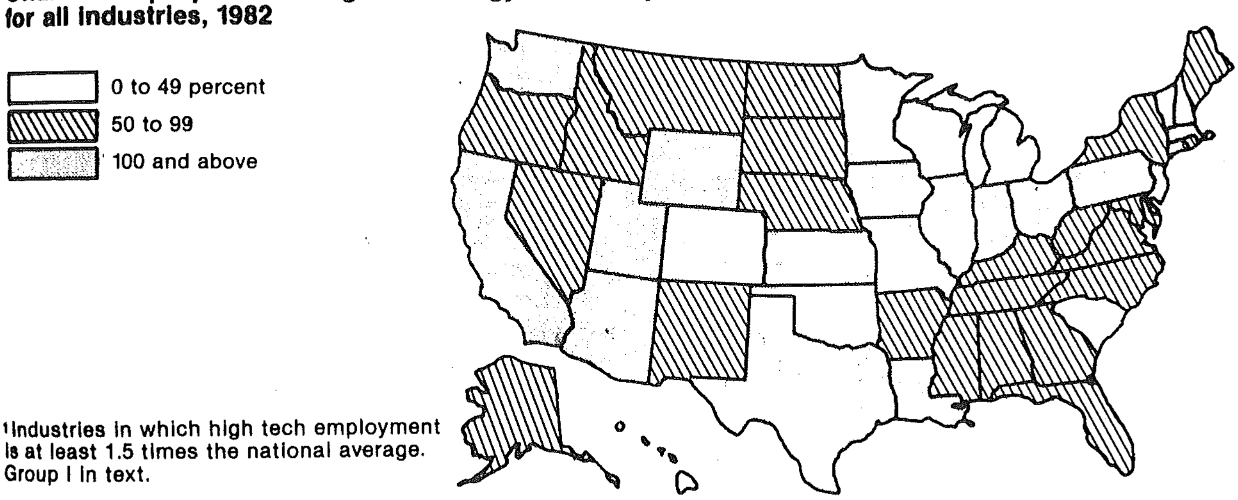
55 -oriented occupations generally need specialized post-high school education in some field of technology—ranging from an associate degree or its equivalent to a doctorate—education with a thorough high school preparation in science and mathematics as a prerequisite.

Technology-oriented workers, while essential to the development of technology, are relatively few in number and will account for a relatively small proportion of new jobs through 1995. In 1982, technology-oriented employment totaled 3.3 million, or about 3.2 percent of total employment. (See table 4.) Through 1995, this employment is projected to show growth ranging from 45.3 to 49.3 percent, much faster than the 23- to 28-percent increase projected for all wage and salary workers. This growth is expected to generate between 1.5 and 1.6 million new jobs over the 13-year period. These occupations are projected to account for 6 percent of all new jobs in the economy, roughly the same proportion as during the 1970's.

Local employment levels

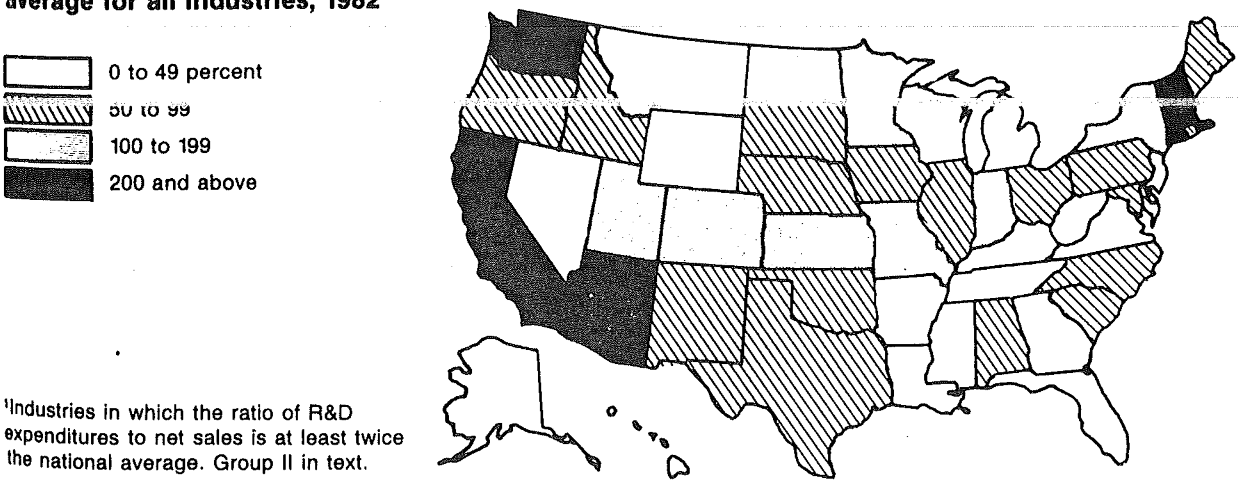
High technology employment is not expected to take up the slack in job generation caused by the long-term decline in heavy durable goods industries, including those we have defined as high tech. What is true for the Nation as a whole of course, does not hold for certain States and areas. (See charts 1 and 2.) High technology employment can have a

Chart 1. The proportion of high technology workers by State in 48 industries[1] compared with the average for all industries, 1982



[1] Industries in which high tech employment is at least 1.5 times the national average. Group I in text.

Chart 2. The proportion of high technology workers by State in six industries[1] compared with the average for all industries, 1982



[1] Industries in which the ratio of R&D expenditures to net sales is at least twice the national average. Group II in text.

Table 5. Metropolitan areas ranked by high technology employment levels and percentages of total nonagricultural employment in three States, September 1982

[In thousands]

State	Group I			Group II			Group III		
	SMSA[1]	Number of employees	Percent	SMSA[1]	Number of employees	Percent	SMSA[1]	Number of employees	Percent
California, total		1,523.3			616.3			930.0	
Top 5 areas, total		1,321.1			574.5			848.4	
Top 5 areas as percent of State's high tech employment		86.7			93.2			91.2	
	Los Angeles	606.3	17.2	Los Angeles	259.5	7.4	Los Angeles	365.0	10.4
	San Jose	261.3	37.5	San Jose	169.5	24.3	San Jose	227.7	32.7
	Anaheim	175.7	20.9	Anaheim	78.4	9.3	Anaheim	121.3	14.4
	San Francisco	173.0	11.1	San Diego	45.1	6.8	San Francisco	67.4	4.3
	San Diego	104.8	15.8	San Francisco	22.0	1.4	San Diego	67.0	10.1
Texas, total		1,016.8			154.4			362.3	
Top 5 areas, total		739.2			134.0			286.3	
Top 5 areas as percent of State's high tech employment		72.7			86.8			79.0	
	Houston	349.1	22.0	Dallas	102.0	6.6	Dallas	140.9	9.1
	Dallas	284.5	18.4	Houston	10.4	.7	Houston	86.9	5.5
	San Antonio	36.4	8.7	Austin	10.4	3.8	Beaumont	24.0	16.2
	Beaumont	35.3	23.8	San Antonio	7.1	1.7	Austin	21.6	8.1
	Austin	33.9	12.6	Lubbock	4.3	4.8	San Antonio	12.9	3.1
Michigan, total		623.4			28.8			118.4	
Top 5 areas, total		490.3			24.5			88.3	
Top 5 areas as percent of State's high tech employment		78.6			85.1			74.6	
	Detroit	325.5	21.0	Detroit	11.7	.8	Detroit	48.1	3.1
	Flint	59.2	33.9	Kalamazoo	7.9	7.5	Grand Rapids	15.8	6.0
	Ann Arbor	37.4	28.5	Muskegon	2.2	3.9	Kalamazoo	10.6	10.0
	Grand Rapids	34.9	13.3	Grand Rapids	1.4	.6	Ann Arbor	9.5	7.2
	Lansing	33.3	18.6	Benton Harbor	1.3	2.4	Muskegon	4.3	7.6

[1]Standard Metropolitan Statistical Area.

large impact on a local economy. Local success stories include California's Silicon Valley and the Route 128 area in Massachusetts and New Hampshire.[12] In a relatively short period, these areas have developed substantial industrial bases built on high technology industries.

We analyzed data on the distribution of high technology employment in three States—California, Michigan and Texas. The results are shown in table 5.[13]

Regardless of the definition used, we found most employment to be located in the largest metropolitan areas. The top five areas in each State accounted for between 72.7 and 93.2 percent of the high tech jobs, depending on the State and definition used. Nonagricultural employment in these areas ranged from 63.7 to 74.2 percent of all employment in each State. Thus, the distribution of high technology employment appears to be concentrated within the States.

In California, the Los Angeles area, with a large aerospace industry, shows the highest level of high technology employment by a large margin over San Jose. However, the San Jose area, which contains "Silicon Valley," has the highest proportion of high tech jobs in California, regardless of definition. In the San Jose area, from a quarter to more than one-third of the jobs are in high tech industries.

Texas ranked second, third, and fourth in the *number* of high technology jobs. Because of its size and large employment base, however, it ranked no higher than eighth in the *proportion* of workers in high tech jobs. When scrutinized at the metropolitan level, however, several Texas areas emerge as high technology centers.

Dallas provided over 100,000 high technology jobs, regardless of definition. The Houston area is also a major source of jobs, while Beaumont shows a large proportion of high tech jobs in groups I and III, primarily because of its chemical and petroleum refining industries.

Michigan has a high proportion of high technology jobs in group I, which includes auto manufacturing. (See table 6). With groups II and III, Michigan ranks 14th and 39th among all States. Detroit, under the group III definition, shows almost 50,000 high technology jobs, and the Kalamazoo area displays a smaller proportion of high tech workers (7.5 and 10.0 percent in groups II and III).

Outside of those two areas, high technology industry does not appear to be a major factor in the Michigan economy unless auto manufacturing remains in the high technology definition.

If we look at the nonmetropolitan proportion of high tech employment in the three States, we find that California has 1.6 percent in group I, .4 percent in group II, and .5 percent in group III; Texas, 10.4, 4.0, and 5.8; and Michigan, 9.5, 7.8, and 15.6.

Few counties outside metropolitan areas have many high tech jobs. (Hutchinson County in Texas is an exception, with more than 5,000 in group I, and almost 2,500 in group III.)

Employment by State

In 1982, the share of the Nation's high technology employment in the 10 States with the highest levels of high tech employment ranged from 57.4 to 66 percent among

57 our three groups, while these States had only 54.1 percent of the total U.S. nonfarm employment. (See table 6.) Eight States—California, New York, Texas, Massachusetts, New Jersey, Florida, Illinois, and Pennsylvania, appear on all three lists. All were also among the 10 States with the most nonagricultural employment in 1982. Only two States not among the top 10 in employment appear on the three lists—Washington and Connecticut—largely because each had more than 10 percent of the national employment in aircraft and parts (SIC 372), which appears in all three high technology definitions.

California not only heads each list but does so by a large margin. New York's total nonagricultural employment was 74 percent of California's in 1982, but it had only half of California's high technology employment in group III, and about a third of its group II employment, illustrating the importance of definitions.

Has the concentration of high tech employment within the larger States increased over the last several years? The following shows the percentage of total U.S. high technology employment in the top 5 States under each definition for selected years from 1975 to 1982:

	1975	*1977*	*1979*	*1982*
Group I	38.4	37.8	38.3	37.4
Group II	46.7	47.1	47.6	47.5
Group III	41.6	40.9	40.4	40.7

The concentration of high technology employment in the largest States does not appear to be increasing, regardless of the definition used..

As we have seen, comparing a State's high technology employment to its total nonagricultural employment produces a much different picture than looking at absolute levels. Small States appear on these lists, as a broad spectrum of industries in large States tends to overshadow small groups of emerging industries. Only under the broadest definition—group I—do as many as 5 of the 10 States with the most nonfarm employment qualify. Under the most restrictive definition—group II—only two large States are included.

Table 6. Employment in three groups of high technology industries in 10 States with highest levels of high technology employment, annual averages,[1] 1982

[In thousands]

Group I		Group II		Group III	
Total, U.S.	13,038.3	Total, U.S.	2,633.7	Total, U.S	5,943.4
Top 10 States	7,489.5	Top 10 States	1,737.4	Top 10 States	3,566.6
California	1,527.5	California	610.6	California	933.1
Texas	1,068.4	New York	205.3	New York	493.4
New York	924.0	Massachussetts	160.7	Texas	372.0
Ohio	683.0	Texas	157.6	New Jersey	316.8
Illinois	672.0	New Jersey	116.9	Massachussetts	305.5
Michigan	651.0	Florida	108.1	Pennsylvania	277.0
Pennsylvania	615.4	Connecticut	98.5	Illinois	261.5
New Jersey	521.7	Illinois	96.2	Ohio	247.8
Massachusetts	450.0	Pennsylvania	93.3	Connecticut	185.8
Florida	376.5	Washington	90.2	Florida	173.7

[1]Because fourth quarter 1982 data were not available at the time of publication, a 9-month average was used.

Table 7. High technology employment as a percent of total nonagricultural employment in top 10 States under three definitions, 1982 annual average[1]

Group I		Group II		Group III	
Total, U.S.	13.4	Total, U.S.	2.8	Total, U.S.	6.2
Delaware	24.0	New Hampshire	7.2	Delaware	16.2
New Hampshire	21.0	Vermont	7.0	Connecticut	13.0
Michigan	20.4	Connecticut	6.9	New Hampshire	12.5
Connecticut	20.3	Arizona	6.8	Vermont	11.7
Vermont	18.9	California	6.2	Massachussetts	11.7
Indiana	17.6	Massachussetts	6.1	New Jersey	10.3
Massachussetts	17.2	Washington	5.7	California	9.5
Texas	17.0	Kansas	4.7	Arizona	9.0
New Jersey	16.9	Utah	4.2	Washington	8.2
Kansas	16.5	Colorado	3.9	Kansas	7.8
Ohio	16.5				

[1]9 month average.

It is noteworthy that Massachusetts, despite its size, is on all three lists. (See table 7).

Turning again to group I, we find 46 States had 10 percent or more of their nonagricultural employment in high technology industries. However, in group II no State had more than 7.2 percent of high tech employment.

The performance of Delaware under the three definitions is quite interesting. It tops groups I and III with 24.0 and 16.2 percent of its nonfarm employment in high technology. In group II, however, Delaware places 42nd in the Nation, with only .8 percent. Groups I and III both include the entire chemical manufacturing industry (SIC 28). Group II only includes drug manufacturing (SIC 283). Because more than 10 percent of the total employment in Delaware is in chemical manufacturing (about 10 times the national proportion), any high technology definition which includes the entire chemical industry places Delaware at or near the top in the proportion of high tech employment.

A regional pacesetter

The relative importance of high technology among States, however, no matter how defined, shows that the New England States lead other regions in the proportion of high technology employment. The New England area has provided the ideal environment for these industries. Preeminent educational institutions provide the needed skilled workers. Also, for many decades the area has had a decaying industrial base. In 1947, Massachusett's leading nondurable manufacturing industries were textiles, apparel, and leather, with a total employment of almost 250,000 workers. In 1982, employment in those industries totaled slightly more than 75,000 workers. The departure of the textile and apparel industry to the South and overseas left behind an industrial infrastructure, coupled with an awareness of the need to attract and foster industrial development. New England States (with the exception of Massachusetts) also tend to be small, making, as noted, the impact of high technology employment more noticeable.[14]

Although for the Nation as a whole, high technology industries generated only between 4.7 and 15.3 perçent of the new jobs in the United States during 1972–82, several

States showed greater growth. Even in narrowly defined group II, nine States saw high tech jobs account for 10 percent or more of the rise in their total employment between 1975 and 1982. In Massachusetts, growth exceeded 18 percent. (See table 8.) Maine, absent from the top 10 in percentage of high tech employment, appears to have experienced significant job generating effects from high tech expansion under the group II definition.

However, care must be used in analyzing the impact of high technology growth in a State. A State may register a large increase in high tech jobs in a generally expanding economy, or a modest gain in a stagnant economy. Examples of both situations appear in all three groups of high tech industries. Massachusetts, which tops groups II and III and ranks fourth in group I, is an example of the first situation. Massachusetts ranked 10th in total job creation between 1975 and 1982 and depending on definition, 3rd, 2nd, or 4th in high tech job generation. South Dakota, which ranks 1st, 8th, and 3rd in percentage growth of high tech jobs, added a total of only about 20,000 new jobs, one of the smallest increases in the country. However, a large proportion (10.0 to 49.1 percent—according to definition) were high tech, such as those within electrical and nonelectrical machinery manufacturing (SIC 35 and 36).

Table 8. High technology employment growth as a percentage of total nonagricultural employment growth in top ten States, 1975–82, under three definitions

Group I		Group II		Group III	
Total, U.S.	21.0	Total, U.S.	5.8	Total, U.S.	11.3
South Dakota	49.1	Massachussetts	18.3	Massachussetts	30.0
New Hampshire	43.1	New Hampshire	15.8	Vermont	26.9
Vermont	38.7	Vermont	11.5	South Dakota	25.1
Massachussetts	35.2	Arizona	10.6	New Hampshire	25.0
Nebraska	33.1	Maine	10.1	Connecticut	21.4
Rhode Island	32.6	California	10.0	Idaho	19.9
Idaho	32.4	Oregon	10.0	Maryland	19.9
Montana	31.5	South Dakota	10.0	District of Columbia	19.8
Delaware	30.7	Washington	10.0	Rhode Island	19.2
Colorado	30.3	Rhode Island	9.1	Oregon	18.0

IT SHOULD BE REITERATED that even when high tech is very broadly defined, as in group I, it has provided and is expected to provide a relatively small proportion of employment. Thus, for the foreseeable future the bulk of employment expansion will take place in non-high tech fields. □

—FOOTNOTES—

[1] Robert Vinson and Paul Harrington, *Defining High Technology Industries in Massachusetts* (Boston, Mass., Department of Manpower Development, September 1979.)

[2] *Ibid.*

[3] *Technology, Innovation, and Regional Economic Development* (Washington, U.S. Congress, Office of Technology Assessment, Sept. 9, 1982). This 14-page report describes a project to assess the implications of high technology to include factors which promote the development of high technology industries in States and localities.

[4] *An Assessment of U.S. Competitiveness in High Technology Industries* (Washington, U.S. Department of Commerce, International Trade Administration, February 1983), 68 pp. See, particularly, Appendix A, "Defining Technology Intensive Trade," pp. 33–37.

[5] Ibid. See also Michael Boretsky, "Concerns About the Present American Position in International Trade," *Technology and International Trade* (Washington, National Academy of Sciences, 1971), and "The Threat to U.S. High Technology Industries: Economic and National Security Implications," draft (Washington, U.S. Department of Commerce, International Trade Administration, March 1982).

[6] Ibid. See also Regina Kelly, "Research and Development in U.S. Trade in Manufactures," paper prepared for International Economics Course, George Washington University, 1974, and "The Impact of Technological Innovation on International Trade Patterns," *Staff Economic Report*, (Washington, U.S. Department of Commerce, Office of Economic Research, December 1977).

[7] Ibid. See also C. Michael Aho, and Howard F. Rosen, "Trends in Technology-Intensive Trade," *Economic Discussion Paper 9* (Washington, U.S. Department of Labor, Bureau of International Labor Affairs, October 1980).

[8] Ibid. See also Lester A. Davis, "Technology Intensity of U.S. Output and Trade," (Washington, U.S. Department of Commerce, International Trade Administration, July 1982.)

[9] Ann M. Lawson, "Technological Growth and High Technology in U.S. Industries" (Washington, U.S. Department of Commerce, Bureau of Industrial Economics, *Industrial Economics Review*, Spring 1982), p. 12.

[10] See Arthur J. Andreassen, Norman C. Saunders, and Betty W. Su, "The economic outlook for the 1990's: three scenarios for economic growth;" Valerie A. Personick, "The job outlook through 1995: industry output and employment projections," and Howard N Fullerton and John Tschetter, "The 1995 labor force: a second look," elsewhere in this issue.

[11] Some managerial jobs also involve the development and application of technology, and many of these jobs are filled by workers transferring from these "technology-oriented" occupations. Data are not available to identify this group.

[12] "America Rushes to High Technology for Growth," *Business Week*, March 28, 1983, p. 87.

[13] The industry employment statistics cited in this study are from two Bureau of Labor Statistics payroll employment programs. The industry classifications are taken from the 1972 Standard Industrial Classification Manual, Office of Management and Budget.

Employment estimates for the Nation were compiled from the Current Employment Statistics program. These data are produced from employer payroll records reported to the Bureau on a voluntary basis each month. Self-employed persons and others not on a regular civilian payroll are outside the scope of the survey.

State and county data were compiled from the ES-202 program, which collects information on the employment and wages of workers covered by unemployment insurance programs. Each quarter all covered employers submit mandatory reports of employment and wages to the appropriate State Employment Security Agency. These reports are edited and summarized by county, State, and detailed industry, and forwarded to the Bureau. Self-employed persons are not covered in this statistical program.

[14] For more on the factors which enabled New England to become a leading area in high technology, see Lynn E. Browne and John S. Hekman, "New England's Economy in the 80's," *New England Economic Review*, January/February 1981, pp. 5–16.

R. W. Riche, D. E. Hecker, & J. U. Burgan, 'High technology today and tomorrow: a small slice of the employment pie', *Monthly Labor Review*, vol. 106, no. 11, 1983, pp. 50–8.

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3.4

PROBLEMS IN PARADISE

Silicon Valley has a two-class structure, with almost no mobility across class lines. The upper-class segment, the highly paid engineers, reside in North County. The other half of Silicon Valley's population lives in South County, concentrated in San Jose and its suburbs of Milpitas and Gilroy. The skilled manual workers of Silicon Valley's microelectronics industry live here, the proletariat of the information society. These workers make up almost half of the total Silicon Valley workforce. The 1980 median family income in San Jose was $22,886 and in Gilroy, $19,139, compared to a median family income of $48,000 in North County.[12] About 7 percent of South County residents live in poverty; they are most often families headed by single mothers.

Historically Silicon Valley began in the Stanford Industrial Park and gradually spread south toward San Jose. The North County communities of Palo Alto, Mountain View, Sunnyvale, and Cupertino got first choice of the microelectronics firms. It was like an industrial development smorgasbord, with each city filling up its plate. Gilroy and San Jose were at the end of the line and what was left was to house the manual workers. From the viewpoint of a city's property tax base, providing housing for the poor is a losing proposition; costs of government services like education, welfare, police and fire protection are high relative to taxes paid per person. In short, North County profits at the expense of South County due to the segregation of Silicon Valley.

The lower-class resident will not see much of the fabled Silicon Valley lifestyle ballyhooed in the mass media. "For the production workers who make up half the electronics industry workforce, Silicon Valley means low-wage, dead-end jobs, unskilled, tedious work, and exposure to some of the most dangerous occupational health hazards in all of American industry. It is a dark side to the sparkling laboratories that neither barbecues, balloons, nor paid sabbaticals can hide."[13] Race and gender are part of the proletarian lifestyle of South County people. To simplify, white males in Silicon Valley hold the engineering and managerial positions with the highest incomes and greatest power.

Non-white men (Hispanics, Vietnamese, Filipinos, and blacks) and white women fall lower in the socioeconomic status hierarchy of Silicon Valley. Minority women are at the bottom of the occupational structure.[14]

The salary for assembly-line workers is the lowest in the industry. Starting pay is $4.50 per hour, close to the minimum legal wage. Because electronics jobs usually do not require English language ability and because of the high turnover rates (about 50 percent a year), these jobs are relatively easy to get. Therefore companies can pay low salaries, but be assured, by the pool of cheap labor available in South County, of a constant supply of employees who want jobs.

To most electronics executives unionization is anathema. Presently none of the Silicon Valley firms are unionized. Companies often warn their employees about the dangers of labor unions, anti-union literature is distributed, and managers and executives are given training courses (by the American Electronics Association, for example) on how to prevent unions from gaining a toehold. The last union election in Silicon Valley was held in 1980 at Raytheon's semiconductor plant in Mountain View; the International Brotherhood of Electrical Workers lost.[15]

A union organizer told of cases of harassment against organizing efforts. An employee in one firm passed out union literature to her co-workers and she was fired immediately, charged with slashing the tires on her supervisor's car parked in the company parking lot. The pro-union employee claimed this was a trumped-up accusation.

At a 1982 public hearing of disgruntled Fairchild employees which was conducted by the Santa Clara County Commission on Human Relations, a Filipino stated that she had been fired for reading a union leaflet, printed in English, to a Tagalog-speaking fellow worker (who also was fired).[16] Several other ex-Fairchild employees at this hearing described anti-union activities by their supervisors and managers.

Nevertheless, the threat of possible unionization may have the indirect effect of causing improved working conditions and better treatment for Silicon Valley's manual workers. The American Electronics Association's anti-union seminars preach that a preventive approach to union busting is the most effective. Silicon Valley firms treat their employees to generous benefits and give their workers a good deal of respect and responsibility. In part this policy coddles brainpower, the scarcest resource in Silicon Valley. The other reason for treating employees like human beings, particularly important regarding manual workers, is to prevent unionization.

Bob Noyce of Intel said: "Remaining non-union is an essential for survival for most of our companies. If we had the work rules that unionized companies have, we'd all go out of business. This is a very high priority for management here. We have to retain flexibility in operating our companies. The great hope for our nation is to avoid those deep, deep divisions between workers and management, which can paralyze action."[17] The captains of

Silicon Valley are convinced that unionization is an enemy only slightly worse than Japanese competition in microelectronics. As long as these leaders are so convinced, Silicon Valley firms will continue to treat their employees with a high degree of paternalistic care.

About 2,000 Filipino immigrants work for National Semiconductor, one-quarter of National's total workforce in Silicon Valley. Similar concentrations of Third World nationals are found in other companies—for instance 4,000 Vietnamese work at Hewlett-Packard. Filipinos are dominant on the AMD semiconductor assembly lines, and Spanish-speaking workers are numerous at several other Silicon Valley firms. These ethnic concentrations in various companies stem from the interpersonal networks through which people find jobs. A Filipino working at AMD encourages her cousin, newly arrived in San Jose from Cebu City, to apply at the AMD personnel office. This cousin, once hired, can speak in Tagalog with her supervisor and fellow workers in an AMD cleanroom.

As a rule the newest ethnic group to arrive in Silicon Valley is closest to the bottom of the occupational class structure. Currently this group is Vietnamese refugees. Several hundred new immigrants arrive in San Jose each month, many "boat people." South County is one of the two main concentrations of Vietnamese in the United States (San Diego is the other).

There are certain disadvantages to ethnic diversity among the line-operators in Silicon Valley companies. One problem is communication among different minority groups. At its wafer fab-

A Day in the Life of a Semiconductor Line-Operator

Maria wakes at 6:00 A.M. so that she has time to prepare breakfast for her three children and get them dressed before leaving for work. Driving up Highway 101 from her apartment in East San Jose, she drops her smallest child, a baby of six months, at a day-care center run by her aunt. Then back into the slow-moving traffic to Mountain View, a 15-mile commute requiring 45 minutes in the morning rush. By the time she pulls her old 1968 Buick into the huge parking lot behind the semiconductor fabrication plant it is almost 8:30. She arrives breathless at the locker room, where she hurriedly changes into her cleanroom bunny suit.

Maria then faces a seemingly endless line of silicon wafers which she checks for defects under a microscope. By her 10:00 coffeebreak, Maria's neck and shoulders hurt. She knows that by the end of the workday, the pain will have settled into a dull ache. But complaining to her supervisor does little good, and anyway the plant has recently announced threatened layoffs. A semiconductor fab worker would have a difficult time getting a new job.

So Maria copes as best she can. She complains to Isabella, her co-worker at a neighboring microscope. As the workday wears on, they talk, *sotto voce,* about what they saw last night on TV, their fellow employees, their children. The company provides a volleyball court, swimming pool, and sauna, but their 30-minute lunch break hardly allows an opportunity to use the facilities. At 4:30 Maria heads her old Buick (she affectionately calls it "Blunderbuster") back down 101 toward home, picking up her baby on the way.

She fixes dinner for the kids, and then talks with a neighbor (another single parent) while half-heartedly watching Spanish-language TV. By 10:00 Maria is yawning. As she goes to bed, she thinks, "Now, if I just don't get laid off."

192 rication plant in Mountain View, which is so old it practically has a dirt floor, Fairchild Semiconductor achieves a very high yield of about 70 percent (that is, about 30 percent of the semiconductor chips must be rejected). But at its spiffy new wafer fab in San Jose, Fairchild's yield hovers around 10 percent.[18] The difference in quality of production seems to be due to the diversity of languages and cultures at the San Jose plant—most line-operators are Vietnamese refugees, Taiwanese, or Mexicans. Company orders and instructions in English are not understood by many workers.

193 The Black Market Garage Operations

One often associates Silicon Valley garages with such successful entrepreneurs as David Packard and William Hewlett, or Steve Jobs and Steve Wozniak. But garages today also have another association, one which is illegal, unsavory, and illustrates the exploitation of Third World women.

"Beneath Silicon Valley is an underground of cheap labor in which housewives, aliens, refugees, welfare recipients, and others struggling to make ends meet earn less than the minimum wage and do without Social Security and workers' compensation benefits."[19] These "sweatshops" take advantage of the thousands of illegal aliens, many of them recent immigrants from Mexico or Vietnam, who cannot accept legal employment. The black market garages operate in a cash market, thus eliminating the 10 percent of labor costs that would otherwise go for payroll deductions, and the 15 or 20 percent ordinarily paid by the employee as income tax and Social Security. Thus a Silicon Valley company contracting with a sweatshop can get a $9-an-hour job done for only $6 or $7 per hour.[20]

Board-stuffing is a kind of work that is easy to subcontract to a garage-type operation in Silicon Valley. Let's say that a microcomputer firm wants 50,000 circuit boards stuffed, and will pay fifty cents per board. The boards and the semiconductors that are to be inserted are delivered in an unmarked car via an intermediary (who is likely to extract a 10 percent fee). Nothing is put in writing and all transactions are handled in cash. The garage sweatshops are not a small-time operation; knowledgeable sources say that thousands of people and millions of dollars are involved. One Silicon Valley executive estimates that 200 sweatshops are presently assembling circuit boards.[21]

As with any illegal operation involving weaker sectors of the population, exploitation occurs. A state investigator for the California Division of Labor Standards said: "Most people who work at home are not paid the minimum wage. The laborers suffer. It also creates unfair competition. In home work, the pervasive violation of minimum wage and overtime laws is chronic."[22] In some cases, illegal aliens from Vietnam must pay an "entry fee" of $150

to $200 in order to get jobs as black market home assemblers. 194
Kickbacks are also paid by sweatshop bosses to company officials who direct business their way.

The cutthroat competition of the Valley, which encourages using any means of reducing labor costs, and the quick service sweatshops provide, ensure their existence. Consider the following: In a rundown section of San Jose, a Vietnamese woman invites two well-dressed men into her home. Soup bubbles on the stove and children play in a back room. Using hand signs and fractured English, a deal is concluded in which the woman, her aged relative, and a woman down the street will assemble printed circuit boards for fifty cents apiece. "We work faster, we get more. Slower, we get less," says the sweatshop boss.[23]

Why don't state and federal officials crack down on the garage sweatshops in Silicon Valley? In the first place they are hard to identify. Everyone involved cooperates in hiding their location. Most are operated by ethnic minorities, and their neighbors and friends (fellow Mexicans or Vietnamese or Filipinos) will not inform government authorities. But everyone knows they exist and do a flourishing business. The sweatshops are part of the seamy side of Silicon Valley, operating outside the law as part of the information economy's underground.

Assembly Operations in the Third World

In addition to the garage operations in Silicon Valley are the semiconductor assembly plants in Asian nations, employing hundreds of thousands of young women. "Wages vary from about 5 percent of the U.S. norm in Indonesia to nearly 25 percent in Hong Kong."[24] Almost every major semiconductor firm in Silicon Valley, and many of the computer companies, own "offshore" assembly plants in Asia.

This unique international assembly line got started when Fairchild Semiconductor built an assembly plant in Hong Kong in 1963. In the 1960s the final testing of silicon chips was done in Silicon Valley. Companies shipped the chips to the offshore lo- 195
cations in Asia, they were assembled and encapsulated in plastic protective cases, then airfreighted back to California for final testing. Now most off-shore facilities are self-contained, carrying out all operations through final test and "mark and pack." The original engineering design work is all done at the companies' headquarters in Silicon Valley.[25] Fairchild alone operates assembly plants in six Asian nations, plus Brazil and Scotland.

During the 1960s and early 1970s the Silicon Valley semiconductor companies became a worldwide industry. Asian governments invited these corporations into their countries with hopes of gaining technological expertise, obtaining foreign exchange in

U.S. dollars, and providing employment for their huge and growing populations. The U.S. semiconductor industry is highly competitive and one can hardly blame it for seeking to cut labor costs, although the transfer of assembly operations to Asia means fewer jobs in the U.S. Labor is much cheaper in Asia compared to the 1983 average Silicon Valley microelectronics worker's wage of $9.20 per hour (including fringe benefits). In Mayalasia it is 60 cents per hour; the Philippines, 50 cents; and in Hong Kong, $1.20.[26]

During the past five or six years, Silicon Valley firms have opened newer production facilities in a dozen or so locations around the U.S. These newer "Silicon Valleys" share one important characteristic with the offshore locations in Asia: the availability of relatively cheap, non-unionized labor. The majority of this workforce is young women between the ages of 15 and 25, mostly unmarried, and fresh from the countryside.

Silicon Valley as an Unhealthy Place

According to the California Division of Labor Statistics and Research, the electronics industry has illness rates much higher than those of U.S. industry as a whole.[27] On the surface, the microelectronics plants look startlingly attractive. But inside Silicon Valley, important health problems occur.

One of the most visible and talked about problems in Silicon Valley is air pollution. Because the area is a kind of geological valley, pollutants become trapped. The chief source is auto exhausts. As Silicon Valley exploded into its present crowded condition, no one thought much about how people would transport themselves to work. Today everyone pays for this negligence. The main expressways connecting the South County workforce to the North County places of work are congested daily with traffic jams. The worst cases of mile-long "parking lots" take place heading north in the morning and south in the late afternoon. About 90 percent of Silicon Valley residents go to work by private auto; no one thought to plan for mass transportation by bus or commuter train. The average auto commute of 15 miles today will go up to 22 miles in the next few years, an increase of 50 percent.[28] The trend is to longer home-to-work travel as the North County/South County separation of the workforce from its employment places becomes more pronounced. The time wasted while sitting in a car stalled in traffic continues to increase; today average commuting time approaches 46 minutes per day.[29] If one has a mental picture of Silicon Valley that only includes the manicured campuses of the high-tech firms, the miles-long "parking lots" of the main expressways should also be part of the picture.

What can be done about the traffic problems of Silicon Valley? The construction of new freeways between North County/South County is not feasible, as previously available space is gone. Improved bus service, light rail (that is, modern streetcars), and commuter trains could help, as could the organization of more carpools. Silicon Valley firms could contribute to a public transportation tax, levied on the basis of number of employees, as has been proposed recently by the City of Palo Alto. Flexible working hours already have been instituted by many companies. They help somewhat, but the basic transportation problem goes deeper, back to the 1960s and 1970s when the Silicon Valley complex was dividing into a North County and South County.

Also guilty of causing pollution problems are the microelectronics firms. Even though these are presumably "clean" industries, not characterized by belching smokestacks, Silicon Valley's semiconductor firms blow 25 tons of precursor organics into the air each day. These materials chemically react with sunlight to form smog.[30] A recently implemented regulation requires the semiconductor firms to filter (or "scrub") their exhaust fumes to remove the pollutants, at a cost of $37 million over the next 10 years.

Silicon Valley can be a dangerous place in which to live, especially if one has a semiconductor company as a neighbor. This realization began to crystallize on December 4, 1981.[31] Workmen were digging a hole for a new water tank at the South San Jose plant of Fairchild Semiconductor. They noticed strange, rust-colored earth around a chemical storage tank that had been in use since 1977. They dug deeper and found that industrial chemicals were seeping through the bottom of the old fiberglass tank. The gauge that was supposed to measure the volume of liquid in the tank had failed. The stain in the earth was extensive, and Fairchild notified the Great Oaks Water Company, whose wells were nearby, that there might be a problem.

The well nearest the leaky tank, about 1,500 feet away, was contaminated with 1–1–1 trichloroethane (TCA), with concentrations nearly 29 times higher than levels considered acceptable by the state health agency. About 40,000 gallons of TCA had leaked out of the defective underground tank beginning in May 1980.[32] The well was closed the next day, but neither the water company nor Fairchild disclosed the problem to the public for 50 days.

Housewife Lorraine Day opened the *San Jose Mercury News* on January 20, 1982, and read about the leak. She could look out her window and see the Fairchild plant, within a quarter mile of her home. Her family got its water from the Great Oaks Water Company. Her smallest child had multiple congenital heart defects, had almost died, and needed open-heart surgery. Ms. Day recalled that her neighbors had complained that their water tasted funny. Within the past three years on her block, four children had been born with birth defects, two miscarriages had occurred, and one stillbirth had happened. Lorraine Day wrote to the president of the water company, demanding an investigation. Her action attracted considerable media attention; the *San Jose Mercury News* wrote an article about her and she began receiv-

198 ing phone calls from the Associated Press and the *National Enquirer.* Within days 31 birth disorders were reported to the Great Oaks Water District. As the alarm about the water supply spread 36 other leaks of hazardous chemicals were found in the Bay Area, 7 in Santa Clara County that involved chemical solvents used by semiconductor plants such as Intel, Hewlett-Packard, Signetics, and AMD. Leaks at other Fairchild plants in Los Altos, Mountain View, and Santa Clara were also found.[33] Suddenly, Silicon Valley did not seem to be such a healthy place in which to live. Eventually 11 tanks were found to be leaking chemicals.[34]

Lorraine Day and 265 of her neighbors sued Fairchild, Great Oaks Water Company, and various government agencies, charging negligence. Lawyers for the accusers expect about a hundred more people to file suits. While legal actions are still pending, the TCA incident set off public concern about the handling of chemicals used in the semiconductor industry. Fairchild has removed the faulty tank and is digging up its other underground storage tanks and placing them in cement storage vaults that will contain future leaks if they occur. The company has spent $12 million in excavating contaminated soil and has sunk more than 100 wells in the area around their plant site to pump out the contaminated water.[35] Health authorities are investigating the water pollution problem, monitoring chemical leaks, and developing a code to prevent such problems from happening again.

Perhaps the TCA incident and its attendant publicity have raised the consciousness of industry officials, government figures, and the public. They will no longer be lulled into complacency about the seeming "cleanliness" of the semiconductor industry in Silicon Valley.

Housing and traffic have been acknowledged as Silicon Valley problems that got out of control, a lesson of history. Ironically, the same thing is likely to occur again, but this time it will be energy and water that are in such short supply as to limit further activity in the Valley.

For computer and electronics manufacturers an absolutely essential element is the availability of reliable electrical power. Any fluctuations in voltage or the temporary loss of power will shut down a computer and dump its data, or will cause interruptions in the thin-film manufacturing processes of semiconductor
199 chips. Erosion in the quality of electrical power represents a serious financial threat to high-tech manufacturing. According to Intel's Gordon Moore, "We're very susceptible to power interruptions. As the area gets near to maximum generating capacities, the chance of rolling blackouts increases and they just scare the heck out of us."[36]

One of the most far-reaching exposés of 1982 involved plans for the San Jose sewage treatment plant. While plans for sewage treatment plants are usually not at the top of most people's concerns, treating chemicals produced by Silicon Valley manufacturers is hardly a typical problem. The cost of improving the sewage treatment plant in San Jose alone is estimated at more than $138 million.[37] Chemicals used in high-technology companies must be flushed down a drain or carried away somehow, and indeed they are—in large amounts. Waste removal and treatment are serious problems in Silicon Valley. Avoiding repeats of chemical leaks from underground tanks and limiting the discharge of potentially dangerous chemicals into water and air is an issue likely to gain even greater importance in the next decade.

The Tragedy of the Commons

The problems of Silicon Valley might seem to be separate and distinct. Actually, they are all interrelated to the way in which high-technology industry developed in the Valley. There are sixteen local governments involved; each looked out for itself over the past twenty or thirty years, encouraging firms to locate within city limits, but at the same time pursuing policies of self-interest regarding housing of company employees. Palo Alto, Mountain View, Sunnyvale, and the rest of the North County cities re-zoned residential area for industrial use. The net result was a severe housing shortage, a lengthy commute by workers, and creation of a bimodal North County/South County workforce characterized by socioeconomic inequality.

Unfortunately this pursuit of municipal self-interest means that no one, or at least no one with any clout, looked out for the overall welfare of Silicon Valley as a total system. Each city, each firm, and each entrepreneur looked out for good old Number One. Unfettered free market forces in Silicon Valley worked to the considerable advantage of the strong and powerful, but unfortunately most of the benefits obtained were at the cost of creating disadvantages for the poorer and weaker sectors of the system. Lack of a strong and public-spirited centralized government in Silicon Valley meant a vigorous knees-and-elbows kind of industrial development that ignored public benefits, creating a socioeconomically unbalanced high-technology system. Today we belatedly realize there are many problems in the "paradise" of Silicon Valley. The other "Silicon Valleys" springing up around the United States want to avoid these problems. How can they?

The recent decades of Silicon Valley history represent a classic case of "the tragedy of the commons." This concept was coined by Garret Hardin, an ecologist, to describe the situations when what an individual wants to do conflicts with the needs of the social system to which the individual belongs.[38] If individual independence is maximized in these situations, the common good is sacrificed to the detriment of the system, and ultimately, of its individual members. The tragedy of the commons derives its name from the historical case of the commons pastures in medieval England. The tragedy occurred because each farmer increased the number of his cows grazing in the commons pasture of his village, leading to disastrous overgrazing and eventually to erosion and the demise of the commons. Each herdsman acted rationally in adding yet another animal to his herd because he figured that the additional cow would only be a small increment to the total herd grazing on the commons. The societal tragedy occurs because each herdsman does not consider the societal consequences when his individual actions are aggregated for the system.

"Each man is locked into a system that compels him to increase his herd without limit—in a world that is limited. Ruin is the destination toward which all men rush, pursuing his own best interests in a society that believes in the freedom of the commons."[39] We see contemporary examples of the commons tragedy
201 on all sides: overpopulation, industrial pollution, and the global military race. A lack of concern for the welfare of society has a very high cost. The bottom line is the kind of social problems that exist today in Silicon Valley.

As a much-envied center of microelectronics high technology, Silicon Valley is, and will be, much copied. Those who mimic Silicon Valley owe it to themselves to learn a crucial lesson from the experience gained in the past thirty years: *That the same factors and forces encouraging the individualistic entrepreneur, if not understood and managed effectively, can lead to the tragedy of the commons evinced today by Silicon Valley's problems.* This is a sobering, cautionary lesson, one that has not received much attention in previous accounts of the Silicon Valley story.

If Silicon Valley had been better planned there would be more high-rise housing in downtown San Jose and along the central transportation corridor, which would carry a mass transportation system with feeder buses, designed to move commuters to and from jobs. Taxes might be shared among the individual cities, which would cooperate with each other in planning a logical combination of housing and industry. Certainly there should be a strong regional government looking out for the overall welfare of the entire system, a counterbalance to the greedy, self-serving forces of the sixteen communities, the thousands of companies, and the legions of on-the-make high-tech entrepreneurs. The microelectronics firms would pursue enlightened policies of social responsibility, thinking of the system and society as well as of their net worth and annual profits.

The Cost of Paradise

The problems in a high-tech paradise are to some extent inevitable; some may even be functional. In a free enterprise system, manufacturing looks for a base of cheap labor. The availability of cheap labor in Silicon Valley attracted high-tech industry to the area. The problem—a large group of low-paid workers—is symbiotic.

Other problems are also inevitable accompaniments to paradise. The pervasive wealth of Silicon Valley is an invitation for crime. When the valued object is as small and light as a chip, and when security is more cavalier than secure, the only question is how long until the next heist occurs. Considering the amount of wealth in Silicon Valley, both personal and corporate, crime is actually rather low. The point is that one cannot separate the benefits of Silicon Valley's microelectronics industry—like wealth and employment—from the disadvantages of crime, pollution, and inequality.

Some of Silicon Valley's problems are shared by other metropolitan areas. The absence of coordinated regional planning has resulted in traffic congestion and accompanying pollution, as well as housing dilemmas, in other American cities and cannot be attributed to high-tech industry alone. But the problems happened more quickly in Santa Clara County than elsewhere. Other problems have an obvious link with the get-rich-quick, exploitative approach of the aggressive entrepreneur. Black market board-stuffing operations exist because profit sheets encourage them; computer crime flourishes since companies don't want to invest adequate resources to hinder it; patent violations continue as long as almost everybody benefits.

Perhaps the greatest threat of all—which continues to be ignored—is the single-minded devotion to self-interest at the expense of the common good. Unless high-tech industry and Silicon Valley acknowledge this tragedy, it may end up being the problem that destroys paradise.

E. M. Rogers & J. K. Larsen, *Silicon Valley Fever: Growth of High-Technology Culture*, Basic Books, New York, 1984, pp. 189–202.

Industries Assistance Commission

ENCOURAGING INDUSTRIES WITH GROWTH PROSPECTS

Proposals to encourage the development of new technology industries are usually supported by the observations that, for the last decade, the Australian manufacturing sector has been in a state of decline and that in Japan and the United States, high technology industries have experienced high rates of growth despite the severity of the world recession. Growth and employment prospects of Australian industry are seen to depend on the development of similar industries. In addition to the use of the forms of assistance discussed above, some have advocated a more directive approach by government through selective assistance for the development of those industries considered to have the best growth prospects. 32

In this section, three of the most common proposals to encourage the development of new technology industries are examined. They are the identification and encouragement of a number of 'sunrise' industries and technologies, the development of technology parks and the provision of venture capital.

Sunrise industries and technologies

As a result of changing economic circumstances, at any point in time some industries will be in a state of decline while other new industries will be emerging and expanding. Some of the new industries that emerge and grow may do so as a result of new advances in technology. Others, particularly service industries, may emerge as a result of changes in leisure preferences and lifestyles. The fact that some industries emerge while others decline has given rise, particularly in US literature, to the concept of 'sunrise' and 'sunset' industries, the sunrise industries often being seen as those which arise from the development of new technology.

The continual emergence of new industries is an important ingredient in the evolution of the industrial structure and results from the dynamic process of economic growth. During this process, it is important for resources to flow into those activities in which Australia has a comparative advantage in order to take maximum advantage of the opportunities for growth. In this context, an issue is whether new activities in which we have a comparative advantage will develop in a neutral assistance environment, or whether government support is appropriate.

The argument that government should nominate certain sunrise industries for the provision of selective assistance is based on the propositions that the growth and employment prospects of Australian industry, and thereby the welfare of the community, depend on the development of such industries, and that government is capable of picking the 'winner' industries. Two relevant considerations are thus whether government has the capacity to 'pick the winners', and to what extent the development of such industries would contribute to the employment and growth prospects of Australian industry.

The growth prospects of a firm are largely determined by the extent to which its activities are suited to the features of the environment in which it must operate (ie. the structure of relative prices, patterns of demand

33 and resource endowments). As this economic environment is continually changing, a firm's growth prospects will also depend upon the extent to which it can adjust its operations to suit these changes so as to either improve, or at least maintain, its competitive position on domestic and international markets.

To successfully identify those areas of Australian industry that are likely to experience the best rates of growth, the government would therefore have to anticipate market developments. But it is very difficult for anyone to predict with any accuracy those industries which are likely to grow rapidly in the future. Each entrepreneur has a different perception of the prospects of various industries and invests his own funds and those of his shareholders accordingly. Government can be seen as one of those entrepreneurs with no better perception or market intelligence than the others. Moreover, the insulation of government employees from the risks of the market and the financial penalties associated with wrong decisions may place them in a worse position than other entrepreneurs. Government decisions are likely to represent a concensus and may devote too many resources to an unsuccessful area. This has been noted by the OECD (1979, p. 105):

> "the danger is that the government's project selection will reflect a common assessment of future developments, while private sector investment will reflect a variety of views manifesting themselves through competing projects".

It has been suggested that the government could use the growth performance of overseas industries as a guide to facilitate the selection of 'sunrise' industries in Australia. However, although an industry may have experienced rapid rates of growth in other countries, it does not follow that the industry would also exhibit rapid rates of growth if it was established in Australia. The Australian economic environment differs in many respects from the environments that prevail in other countries. As a result, the growth rates experienced by overseas industries are unlikely to be the same as the growth rates these industries would experience if they were located in Australia, and are unlikely to provide the government with a reliable indication of those types of industries that would experience rapid rates of growth in Australia.

Regardless of the way in which 'sunrise' industries are selected, once the government starts to encourage the development of these industries, the development of other industries that use similar resource inputs will be affected. The ability of alternative emerging activities to attract resources and grow will be reduced as a result of the government nominating particular industries for special encouragement. Therefore, if the government nominates ultimately inappropriate or unsuitable activities the community will be worse off because resources will have been allocated inefficiently and the growth of other suitable activities will have been retarded. Even if the nominated activity proves to be suitable to the domestic economy, too many resources may be devoted to that activity as a result of its selection for special encouragement. Also, within a nominated industry, government support may not permit resources to flow into those areas in which the domestic economy can most efficiently specialise.

34 A requirement for obtaining consistently good growth rates is for resources to be capable of being relocated between different activities as economic conditions change. Thus, the firm or economy which is sufficiently flexible to be able to reallocate resources to take maximum advantage of changing conditions will be most successful. However, once having made the decision to encourage a given activity, the government is likely to come under pressure to maintain support for that activity, even if commercial indications are that it is not viable. In fact, history has shown that as such activities became less profitable, the pressure for government to accept responsibility for maintaining them mounts. This has the effect of locking resources into inefficient areas of the economy.

A more general argument in support of assisting sunrise industries to develop is that directive government policies have been successful in other countries. A frequently cited case is that of the Ministry for International Trade and Industry (MITI) in Japan, with that country's rapid growth being attributed to directive industry policies. However, the extent to which that growth can be attributed to MITI is unclear. As well as its support for some successful industries, MITI supported others which proved to be unsuitable to the Japanese economy and failed to support some which later showed remarkable growth. Furthermore, the economic and cultural environment in which MITI operates differs significantly from most western-style countries, including Australia, and it is questionable whether the powers exercised by MITI in pursuit of its industry policies would be appropriate for Australia.

The extent to which the development of nominated sunrise industries could contribute to the employment and growth prospects of Australian industry depends as much on their absolute size as it does on their prospective rates of growth. It is to be expected that new enterprises will record the highest rates of growth, but that does not mean that they will make the greatest contribution to the growth of the economy. A very low rate of growth in a large established industry is likely to make a greater contribution. Some perspective of the size of the proposed sunrise industries in relation to other industries, sectors of the economy, and the economy as a whole, is important to an evaluation of their potential to promote growth and employment. In this context, since it is the community's resources and welfare which are at issue in industry development, it is incumbent on those who wish to back their judgment about which of the potential activities competing for its resources should be nominated as 'sunrise' industries, to explain to the community, in the clearest possible terms, the factual basis for their judgment and choice.

An argument which is complementary to proposals to develop sunrise industries is to provide incentives to Australian industry to increase its utilisation of new technology. Active government encouragement for the utilisation of new technology may, however, encourage firms to increase their levels of investment in the adoption of new technology at the expense of their levels of investment in other projects that are potentially more profitable. New technology is not always more profitable than older forms of technology. Similarly, to the extent that these incentives only apply to the utilisation of 'new' technology, firms may also be encouraged to adopt forms of technology that they would otherwise consider to be inappropriate or utilise particular technologies at a time earlier than they would normally consider to be desirable.

The provision of government incentives to encourage the utilisation of new technology is also likely to have an undesirable effect on the international competitiveness of domestic industry to the extent that it might result in a government commitment to maintain the commercial utilisation of certain 'key technologies'. Such a commitment could inevitably result in repeated claims by industry for increases in assistance to ensure their continued utilisation of these technologies in the face of mounting pressures for adjustment to advances in technology or other changes in the economic environment. If these claims for increased assistance were met, then the industries concerned would be encouraged to maintain their utilisation of these 'key technologies' beyond their true economic lives. Other firms might also be encouraged to utilise these 'key technologies' as a means of securing access to government assistance to insulate them from the need to adjust to change.

Technology parks

In recent years, a number of State governments and instrumentalities have announced plans for the establishment of 'technology parks' in their respective regional areas. These plans seem to have been stimulated by the same considerations that underlie arguments for the provision of assistance to encourage the development of new technology industries in Australia. In this case, however, the concern is for regional rather than national growth and employment prospects. This desire to establish a technology park may also be prompted by observations of the apparent success overseas of technology regions such as 'Silicon Valley' in California.

The main argument on economic grounds which has been advanced to support government action to establish technology parks relates to the existence of externalities associated with such projects. The externalities postulated in the case of technology parks have been termed 'economies of association' and include access to better information, greater employment opportunities for staff and exchange of ideas, increased potential for specialisation and the establishment of highly specialised educational facilities.

However, it is not clear that government action is necessary, or could, ensure the establishment of a successful technology park. The success of California's 'Silicon Valley' can be attributed to the combination of entrepreneurial, financial, and technical skills, existing research institutions and a supply of labour adept at performing the intricate tasks required for the assembly of electronic products. It is questionable whether government action could produce such a combination of factors in a particular region. If such a combination does evolve in Australia, it is likely to do so naturally rather than by government direction.

Since the promotion of technology parks is aimed at encouraging regional economic growth, the problems resulting from directive government policies and the inability of government bodies to determine which industries are most appropriate for local conditions will also apply to technology parks. In addition, technology parks may be inimical to national economic efficiency in that competition by regions for new firms and plants may lead to greater industry fragmentation. The fragmentation of the
36 Australian steel industry has been attributed in part to State authorities insisting on local downstream production facilities being installed in return for access to raw materials.

Different regions tend to compete for different industries by offering incentives such as offsets, purchasing preferences and direct subsidies to attract firms to their particular region. If this leads to fragmentation of industry and the inappropriate location of firms it is clearly counter to national and regional interests and is contrary to any justification of such government involvement on the grounds of economies of association. As a result, the establishment of technology parks by governments, which will be on a regional basis, may be an inappropriate policy for achieving national goals of growth and efficiency.

Venture capital

Many new business ventures whose future is subject to a high degree of risk require the injection of capital in the early stages of their development. Such high risk capital is obtained from a variety of sources ranging from friends and relatives for small businesses to the share market for larger ventures such as mineral exploration. Finance provided for a narrowly defined section of this market for high risk funds has been described as 'venture capital'. Such finance has a number of distinguishing features.

First, it is usually equity rather than loan finance. In the United States, venture capital companies retain an equity interest in the enterprises in which they invest. Second, venture capital is long term finance. The venture capital company will only obtain a return on its equity investment when the enterprise is sold or merged or when it can realise a capital gain on its shareholdings in that enterprise. Finally, when a venture capital company provides venture finance to an enterprise, it normally becomes actively involved in the management of that enterprise. Venture capital companies also have staff specialised in assessing the risk associated with the type of venture being considered. Conventional finance institutions usually do not have the expertise to assess this risk. The venture capital company typically monitors the performance and participates in the management of the enterprise in which it invests.

Venture capital therefore has two basic advantages over alternative forms of finance for a new technology enterprise. If the enterprise funds its operations with venture capital, it can avoid the cash flow problems associated with short term loan finance and gain access to experienced management guidance.

In some countries, notably the United States of America and Canada, an identifiable market for venture capital has been established to the extent that both government supported and privately owned companies provide the high risk finance described as venture capital. Attempts to operate similar companies in Australia have so far proved to be unsuccessful. Consequently, the market for such finance remains small and informal.

37 Many entrepeneurs regard the small size of the venture capital market in Australia as an impediment to the development of new technology industries. The absence of a larger venture capital market in Australia may place Australian enterprises at a disadvantage relative to their overseas counterparts to the extent that they rely on loan finance and the limited management skills of entrepreneurs with essentially science or engineering backgrounds. However, the identification of a cost disadvantage relative to overseas counterparts for an input to a process does not usually constitute a sufficient condition for government intervention. In this case the input is quite specialised - a high degree of financial, management and market expertise embodied in high risk equity capital - and it is possible that the demand for and supply of such a product in Australia are insufficient to support a larger market.

The small size of the venture capital market in Australia is therefore not necessarily proof of market failure, but it is possible that certain features of the Australian financial system have restricted the formation of local venture capital companies. This question was addressed by the Campbell Committee of Inquiry into the Australian Financial System.

The Campbell Committee identified a range of government actions that have tended to increase the cost and limit the availability of finance to domestic producers. These include the bias in the present Company Taxation provisions towards debt financing and away from equity financing, as well as the tendency for existing lending and interest rate controls to increase bank interest rate charges for larger borrowers and ration funds to smaller borrowers. Concern was also expressed for the constraints that are imposed on the range of financial choice available to business by bank portfolio restrictions, exchange controls and restrictions on forward cover facilities. It considered that its recommendations for the deregulation of the financial system would enable institutions to adopt a more flexible approach to risk.

However, the Campbell Committee attributed the failure of new ventures and innovation proposals to gain equity and debt financing to three main factors. These were a lack of commercial viability, shortcomings in the financial management skills of the entrepreneurs, and disinclination, partly arising out of high relative costs, on the part of investors to evaluate untested ventures and products.

In its discussion of venture capital and innovation finance for small business, the Campbell Committee argued that these three factors did not provide sufficient grounds for government intervention and expressed the view that, given the venture character of the finance sought, there is no substitute for the market-place. In particular, the Committee was critical of the Myers Committee (Technological Change) recommendation for the establishment of a Venture Capital Corporation financially assisted by the Commonwealth Government. It also said that it was least well disposed to government financial assistance for loan insurance/loan guarantee schemes, specialist small business financing institutions, and over-the-counter markets for unlisted shares.

The Campbell Committee did not distinguish between high technology and other new ventures and expressed its preferred approach to be to rely on its deregulation proposals, but that if the Government, on social or other grounds, wished to support new ventures it should do so on a comparable basis to its proposals for small business. Those proposals were for fiscal means in the form of relief of personal tax given to subscribers to Small Business Investment Company shares.

More recently, the High Technology Financing Committee of the Australian Academy of Technological Sciences (Espie Committee) was asked to examine the issues, and report on appropriate initiatives which might be taken by government to assist the development of Australian high technology enterprises. That Committee identified essentially the same factors as Campbell for the inability of new ventures to attract venture finance. However, the Espie Committee recommended government intervention for the purpose of supporting a larger venture capital market in Australia. The Committee regarded the lack of such a market as a major inhibiting factor to the development of new technology enterprises. Consequently, the Espie Committee recommendations conflict with those of the Campbell Committee.

Both committees also recognised that deregulation of the financial system would be an insufficient incentive for the development of a venture capital market. Again, where the Campbell Committee preferred to let a deregulated market determine the allocation of finance, the Espie Committee proposed a range of further interventions.

The question of whether government intervention is warranted for the provision of venture capital would thus seem to depend on whether considerations of national prosperity and efficiency have precedence over improving the environment for the growth of high technology enterprises.

For the Campbell Committee, considerations of national prosperity and efficiency were paramount, whereas the Espie Committee was concerned with the development of a particular industry sector.

Conclusion

In some circumstances, government intervention can lead to improvements in the efficiency of resource use. In other circumstances, intervention may occur for the purpose of pursuing objectives other than economic efficiency. In either case, it is important for the implications of the intervention to be understood.

Assistance to any activity benefits that activity and imposes costs on others. Those costs can impinge heavily on specific activities by raising the cost of an input to a production process, and can have wide-reaching effects by increasing general price levels or by imposing additional charges on general revenue. Sometimes government intervention can have unintended but nevertheless quite detrimental effects on large sections of industry. For example, there is currently a high cost in providing protection for the motor vehicles industry and difficulty in now specifying an assistance package which will encourage an orderly move towards a more efficient industry structure. This is the consequence of previous changes to the objectives of the manufacturing plans and associated revisions of the assistance arrangements. If similar problems are to be avoided in the future, it is essential for the economy-wide effects to be understood before any proposed intervention is implemented.

Industries Assistance Commission, *New Technology and Industry Assistance*, Discussion Paper, July 1983, AGPS, Canberra, 1983, pp. 32-8.

Rone Tempest

TEXAS PAYS HIGH PRICE FOR HIGH-TECH

AUSTIN, Tex.—When this city beat out San Diego and other rivals last spring as the site for a highly publicized consortium of electronics companies determined to race the Japanese to the next generation of computers, it was heralded as a victory for the state of Texas.

But now that Microelectronics & Computer Technology Corp., a joint venture by 14 high-technology companies, has settled into this serene city in the Texas hill country, the question of "Who really won?" is still up in the air.

Texas won the prize, in part by offering many more financial incentives than any other state. But now the Lone Star State must pay for it. Texas also faces several tricky legal problems, including tax and antitrust questions.

California, Georgia and North Carolina were all finalists in the contest to land the unique company. But each state has managed to parlay its "loss" into substantial gains in funding for higher education and new business for state-supported research facilities.

California Gov. George Deukmejian, stung by criticism of his handling of the bid and California's loss of the project, last month proposed a record 30% funding increase for the nine-campus University of California system, including a 16.5% boost in faculty salaries.

Kirk West, secretary of the California Business, Transportation and Housing Agency, said the loss of the venture, known as MCC, has been followed by "an unprecedented boom in employment in overall electronics industries, with approximately 50,000 new jobs."

"We've had nothing but a high-tech boom ever since MCC," West said. "California hasn't been hurting."

Atlanta, the consortium's second choice, used the publicity it gained from the competition to lure several new companies. Georgia Gov. Joe Frank Harris accelerated by one year the state's plans to develop its own consortium of research institutions.

North Carolina's state-supported Research Triangle Park, which balked at committing public money to attract the private industry consortium, has gained new companies representing an investment of more than $250 million. North Carolina Microelectronics Center President Donald S. Beilman said: "Our experience since that time has provided us with a very substantial increase in the number of high-technology industries here. There have been no negative effects."

To win MCC, Texas Gov. Mark White pledged more than $60 million in public and private support, including nearly $40 million from the state's two major universities for expanded electronics and computer science programs. He promised an additional $23.5 million in private donations to build a new research building for MCC and to lease a private jet for the venture's executives.

Now, while the state's oil-industry economy is in a severe slump, White must go on the stump to raise the $23.5-million portion of his pledge.

Last month, in a private meeting at the Houston Petroleum Club, White asked a roomful of wealthy Texans to make donations. A series of fund-raisers are scheduled in other Texas cities. Letters have been sent to potential donors appealing to their Texas patriotism. "Now we must make good on our governor's pledge," the letters say.

White and others claim that the fund-raising is going well. One foundation gift exceeding $5 million is expected to be announced soon. But officials say it still may take three years before they come up with the cash that has been promised.

Defenders of the massive financial commitment to the small experimental company contend that the state of Texas will not lose much in the deal, even if MCC fails. All but about $4 million of the money, they say, will go to building up important programs in the university system. Even the new MCC building, constructed with $15 million in private donations and $5 million in university funds, will revert to the University of Texas if MCC is not a success.

But the massive commitment to the electronics and computer science departments at the university, which includes endowed professorships that pay more than $100,000 a year, concerns faculty members in other areas, who are scheduled to receive a meager 2% raise in the coming year.

'Mixed Feelings'

"I have mixed feelings about it," said American studies professor William H. Goetzmann. "While the MCC thing generates intensity and excitement, it is somewhat schizophrenic. Most of the faculty are not direct beneficiaries. I feel it should be a Big Bang expansion so they are not robbing Peter to pay Paul."

The money appeal is complicated by questions about the tax status of the contributions. MCC is a private corporation. The governor is asking donors to give to a fund set up by the University of Texas that will in turn benefit a private corporation.

Are the contributions tax deductible? Officials in the fund-raising drive have asked the Internal Revenue Service for a ruling. Meanwhile, pledges are being made "contingent" on a favorable ruling by the IRS, said Lee Straus, executive assistant to fund-drive chairman Ben Love.

"I've been in a lot of fund-raising drives. I've never been in anything quite like this before," said Love, chairman of Houston's Texas Commerce Bank. "The governor of the state, in order to attract industry, felt it essential to commit $23 million from the foundations and private sector. In doing so, he staked his reputation and the reputation of the state of Texas."

"I laid the cards down," White said. "But I had the people backing me up. Several of them could have written a check for it that day if they had had the desire." Prominent Texans supporting the MCC bid include Dallas computer millionaire H. Ross Perot, developer Trammell Crow and oilman Bum Bright.

Executives Seem Unconcerned

MCC executives do not seem concerned about money.

"Clearly, if Texas were not to live up to its commitment, that would have a ricochet effect all over the country on other people who looked down here," said MCC President Bobby Ray Inman, former director of the National Security Agency and deputy director of the Central Intelligence Agency.

Many consider Inman, 52, a retired admiral who gained a reputation in Washington for handling complicated intelligence problems, as an ideal choice for the top job at MCC. Among other things, Inman must deal almost daily with the independent whims of each of the 14 companies that form the consortium. Each company has a liaison stationed at the temporary headquarters in a suburban Austin office building.

"There are days I tell my old colleagues in my past life that I do feel I'm back dealing with NATO again," Inman said in a recent interview with The Times.

"It's as though there are 14 different cultures out there that are bound together only by the sense that there is a common danger and that if they don't stay bound together they won't be able to deal with it," Inman said.

Details of how the various companies will use the scientific breakthroughs made by MCC, should any be made, are still to be worked out.

Antitrust Questions Raised

So are the fine points of reconciling the open scholarship undertaken by professors at the University of Texas and Texas A&M University, with which MCC has a working agreement, and the secret research to be conducted by the private company.

But the biggest obstacles facing the optimistic and supremely confident Inman have to do with the antitrust questions and the recruitment of scientific talent.

MCC is a pooling of resources by American corporations, along a Japanese model, to achieve shared technological breakthroughs in new computer systems, including the so-called expert computers that use "artificial intelligence" to solve complex problems.

The companies in the MCC consortium are: Allied Corp., Advanced Micro Devices Inc., National Semiconductor Corp., Control Data Corp., Motorola Inc., Digital Equipment Corp., RCA Corp., Harris Corp., Honeywell Inc., NCR Corp., Sperry Corp., Rockwell Corp., Martin-Marietta Inc. and Mostek Corp.

Each company is supposed to contribute an annual fee ranging from $1 million to $8 million to participate.

Japanese companies several years ago combined in a similar effort. Other American corporations not part of MCC, including IBM and AT&T, are working separately to achieve the same goal, as are the national research laboratories in Livermore and Los Alamos, N.M.

It is widely believed that whichever country reaches this new microelectronic goal first will have enormous trade advantages.

Predetermination Sought

Unlike the Japanese effort, MCC's venture faces potential antitrust challenges from the companies, large and small, not participating in the consortium. Bills have been proposed in Congress tailored to protect MCC from antitrust actions but are not expected to be enacted before the end of the current session.

"The odds of their doing legislation are dropping daily," Inman said.

The Justice Department, like the IRS in the tax issue, has been asked to make a predetermination that MCC does not violate antitrust laws. But Justice, while studying the issue assiduously, has been reluctant to take a stand.

"I would love a nice, crisp decision," Inman said. "But I don't think we'll get one." Inman has described the Justice Department attitude as an "amber light."

At its peak, MCC is expected to have no more than 300 employees. But the very difficult process of recruiting top scientists to lead the company's pioneer research is going more slowly than hoped, according to Inman.

"Assembling the talent and getting the research under way has not been going as fast as I would like to have it go," Inman said. "But it is awfully early for a report card. I'm a good five months ahead of my worst case and I'm about four months behind my fastest track."

Inman, counting on his fingers to get a precise number, said he had offered jobs to only four persons who turned the jobs down. But all four, he said, were for the key program-manager jobs he needs to fill to begin research.

Berkeley Staff Approached

"Four of the seven program managers have been on board and working since the first of the year. We have 64 people here as of today. The cast of characters continues to surge," Inman said.

In some of the areas where MCC intends to do research, including the field of artificial intelligence, the number of scientific experts is very limited, as few as 400 to serve all of industry and higher education. Inman, with an unproven company involved in uncharted research, must compete with established programs.

"MCC people and University of Texas people and Texas A&M people have all been calling and trying to get people to come down there," said David Hodges, chairman of the microelectronics department at UC Berkeley, one of those approached. "They are not yet in our league but they would like to be, quickly. Many of us are skeptical that MCC will succeed."

Hodges estimated that one-fourth of the Berkeley microelectronics faculty of 70 had been approached either by MCC or one of the two Texas state universities. He said that so far no one has accepted the offer.

Ironically, one of the reasons Hodges said the Texas group may have trouble is that "morale is very high" at the university after the large budget and salary increases proposed by Deukmejian.

The highly publicized loss of MCC helped Deukmejian justify his budget request for the university system.

Texans Still Euphoric

"One of the big issues addressed by MCC was the state's commitment to education, and we've seen in the governor's new budget a substantial increase in funds especially for the state college and university system," said Lt. Gov. Leo McCarthy, who made the MCC case into a major political issue.

"Look at the California budget," Inman said. "I noted it with delight. I don't want to take any credit."

The problems faced by MCC have done little to dampen the euphoria of Texans involved in bringing the company to Austin.

Some contend that the Texas offer was a high price to pay for such little direct benefit to the state.

[illegible] environment," said Jared Hazleton, president of the Texas Research League, a respected independent governmental study organization. "Having MCC here will make us more attractive. But new businesses are going to come here for different reasons. MCC will have very little direct employment benefit."

Most, however, have attitudes similar to that of Gov. White, who still swells with pride at any mention of his big catch for the state capital.

"I was in Paris two weeks ago," White said in a recent interview. "A guy said to me, 'Oh, you're from Texas. Isn't that where MCC is located?' That was Paris, France, not Paris, Tex. They're not sure about MCC yet in Paris, Tex. They're not sure it's a good idea yet."

R. Tempest, 'Texas pays high price for luring high-tech venture', *Los Angeles Times*, Part 4, 21 February 1984, pp. 1–2, 11.

Office of Technology Assessment

3.7

ANALYSIS OF CENSUS RESULTS

OTA's census identified over 200 State and local level economic development initiatives with at least some features directed toward high-technology development. The appendix contains detailed descriptions of 150 of these initiatives that were launched by State governments; table 2 shows the distribution of these programs by type and State. Using the narrower definition of a "dedicated" high-technology development (HTD) program—chartered and at least partially funded by the State government, and specifically targeted on the creation, attraction, or retention of high-technology firms—OTA identified a total of 38 programs in 22 States.

Most of these HTD initiatives have been launched within the last 3 years (see table 3). Few of them have been in existence long enough to produce measurable results, and in most cases there has been no systematic evaluation of their effectiveness. In fact, their effectiveness will be difficult to measure—many of these States had experienced a considerable amount of high-technology development prior to any intervention by the State government, and the impact of the dedicated program on further development has yet to be demonstrated. In other cases, furthermore, relatively mature State programs have been very slow to produce any appreciable results,

Table 2.—State High-Technology Programs by Type[a]

State	HTD	TF	HTE	LTA	CPA	GID
Alabama	—	—	—	—	—	1
Alaska	—	—	—	—	2	—
Arizona	—	—	1	1	1	1
Arkansas	—	—	—	2	1	—
California	1	—	1	1	—	—
Colorado	1	1	—	1	—	1
Connecticut	3	—	—	1	1	1
Delaware	—	—	—	—	—	1
Florida	3	—	1	—	—	3
Georgia	1	—	2	—	—	—
Hawaii	1	—	—	—	—	—
Idaho	—	—	—	2	—	1
Illinois	2	1	—	2	1	—
Indiana	1	—	1	—	—	—
Iowa	—	1	—	—	—	—
Kansas	—	1	—	—	—	1
Kentucky	—	—	—	1	1	1
Louisiana	—	1	—	—	1	—
Maine	—	—	—	—	2	2
Maryland	—	1	—	2	3	—
Massachusetts	1	—	1	1	1	1
Michigan	8	—	—	1	1	—
Minnesota	—	—	1	—	—	1
Mississippi	1	—	—	1	1	1
Missouri	1	—	1	2	2	2
Montana	—	—	—	—	—	1
Nebraska	—	—	—	1	—	1
Nevada	—	—	—	—	—	1
New Hampshire	—	—	—	—	—	1
New Jersey	—	1	—	—	—	—
New Mexico	2	—	1	—	—	—
New York	2	—	—	1	2	—
North Carolina	1	—	2	—	—	1
North Dakota	—	—	—	—	1	1
Ohio	1	—	—	—	2	1
Oklahoma	—	—	—	—	—	1
Oregon	—	—	—	—	—	1
Pennsylvania	2	—	1	1	1	—
Puerto Rico	—	—	—	2	1	—
Rhode Island	1	—	—	1	1	—
South Carolina	—	1	—	1	—	—
South Dakota	—	—	—	—	—	1
Tennessee	2	—	1	—	—	—
Texas	1	—	—	1	—	2
Utah	—	—	—	—	—	1
Vermont	—	—	—	—	1	—
Virginia	1	1	—	—	—	1
Washington	1	—	1	—	—	—
West Virginia	—	—	—	1	—	1
Wisconsin	—	—	—	—	—	3
Wyoming	—	—	—	—	—	1
Totals	38	9	15	27	27	37

[a]HTD = High-technology development.
TF = Task force.
HTE = High-technology education.
LTA = Labor/technical assistance.
CPA = Capital provision assistance.
GID = General industrial development.

SOURCE: Office of Technology Assessment.

Table 3.—Establishment of State High-Technology Programs by Year

1959	North Carolina Research Triangle Park
1964	Mississippi Research and Development Center
1968	New York Science and Technology Foundation
1975	Connecticut Product Development Corp.
1978	Florida Research and Development Commission
	Massachusetts Technology Development Corp.
1979	Hawaii Venture Development Fund
	North Carolina Board of Science and Technology
1980	Georgia Advanced Technology Development Center
1981	California Innovation Development Loan Program
	Florida Technical Entrepreneurship Program
	Indiana Corp. for Science & Technology
	Missouri Office of Science and Technology
	New York Corp. for Innovation Development
	Pennsylvania Industrial Development Authority
	Tennessee Department of High-Technology, Finance, and Service Sales
	Washington Research Foundation
1982	Connecticut Innovation Development Loan Fund
	Connecticut Science Park
	Illinois Biomedical Research Parks
	Illinois Research Assistance to the State
	Michigan High-Technology Development Corp.
	Michigan High-Technology Resource Center
	Michigan Industrial Technology Institute
	Michigan Innovation Center
	Michigan Molecular Biology Institute
	Rhode Island Strategic Development Corp.
	Tennessee Technology Corridor Foundation
	Texas Institute for Ventures in New Technology
	Virginia Science, Engineering, and Technology Advisory Service
1983	Florida Interagency High-Technology Committee
	Michigan High-Technology Equity Loans
	New Mexico Economic Development Division
	Ohio Industrial Technology and Enterprise Board
	Pennsylvania Ben Franklin Partnership Fund

SOURCE: Office of Technology Assessment.

while more recent programs in other States are already considered successful. Admittedly, some programs are designed to achieve long-range objectives, while others are intended to show short-term results; but there has yet to be any systematic comparison of the effectiveness and benefits of different program types. Finally, some States report that, even without a dedicated effort, they have nevertheless experienced a great deal of high-technology industrial development.

It was also found, however, that different States define "high-technology development" in different ways, and that in most cases their high-technology initiatives are an extension of their overall economic development strategies. States with HTD initiatives, for example, tend to be those that had a sophisticated research base and considerable high-technology industry even before these programs were established; their objective is in part to strengthen and retain what was already there. In States where the economic base consists primarily of "sunset" industries, on the other hand, the "high-technology" strategy tends to emphasize economic diversification and the application of new production technologies to traditional manufacturing processes. Still other States, notably those that are not yet highly industrialized, base their strategies on the aggressive pursuit of the production facilities of expanding high-technology firms as part of a broader effort to bolster their industrial base and build the foundation for future development.

These patterns suggest that, for most States, attention to high-technology industrial development is not distinct from economic development in general. They also suggest, however, that in launching their initiatives, the States have given attention both to the special needs of technology-based enterprises and to their own comparative advantage vis-a-vis the basic stages of technological innovation and commercialization. This attention, in turn, results in part from the ways in which these programs have been created.

Creation of State High-Technology Programs

Initiatives to promote high-technology industrial development usually come about in one of two ways: 1) as a natural, evolutionary outgrowth of the State's ongoing economic development efforts; or 2) as the result of a special effort to identify and mobilize the appropriate State resources. Both routes lead to programs that are based on the needs of technology-based enterprises, whether perceived or projected, and on strategies to mobilize the resources or provide the services that will encourage or attract their growth within the State. In many cases the programs are based on models elsewhere: several States cited the National Research Development Corp. (created by the British Government in 1949 to commercialize new products), and many other strategies are described in terms of "making Silicon Valley happen here."

General Industrial Development Programs

In the first instance, the dedicated high-technology program results from a need perceived by the State's department of economic development. This office already provides a wide range of assistance to industry in general, such as locating plant sites or identifying an appropriate labor pool. Many but not all these services are also helpful to technology-based businesses. As more and more special requests are received from high-technology firms, or as this sector becomes more important to the State's industrial base, individuals or offices within the existing State agency are designated to concentrate on meeting this increasing demand.

OTA found that almost all States have "general industrial development" programs that can also assist or influence the creation and growth of high-technology businesses. The same can be said for programs in the "capital assistance" and "labor and technical assistance" categories. These programs rarely exclude any specific type of business that needs their services, and the services they offer to new, expanding, or relocating high-technology industry are often not much different from services offered to more traditional industry.

For this reason many States that do not have dedicated HTD programs can and do encourage this kind of development through the services offered by their general, capital, and technical assistance programs. In the States where dedicated programs do exist, they usually work closely with these general programs to help their high-technology clients. In addition, in several States, an existing State agency or representative assumed the job of encouraging high-technology industrial growth in the State.

As a result, however, it was difficult in most States—even those with dedicated programs—to determine precisely where to make the cutoff between "high-technology" programs, on the one hand, and those whose mission is more general but who nevertheless provide the special services demanded by a growing high-technology sector.

High-Technology Task Forces

The second method of creating a high-technology program is more of a "supply-side" tactic. The Governor or legislature appoints an ad hoc task force to examine the State's resources and recommends initiatives that will encourage the development of high-technology industries. These task forces usually represent all sectors of the State's economy, and they address such issues as the proper definition of "high technology," the special needs of high-technology firms, and the question of loyalty to traditional industry versus the appeal of emerging technologies.

OTA's census identified nine existing task forces, and similar task forces in a number of other States have already disbanded after reporting to the Governor or legislature. In many cases the task forces, after presenting their recommendations, are succeeded by permanent advisory committees that coordinate subsequent efforts. In other cases they are transformed into nonprofit, semiprivate corporations or foundations that administer or provide funding for the mechanisms created to implement task force recommendations. These organizations often provide the basis for the State's HTD programs.

High-Technology Development Programs and Services

OTA's census identified only 38 State initiatives that met the criteria for the "high-technology development" category—that is, a dedicated State government program or agency whose specific mission is the promotion of high-technology industrial development in the State, whether by attracting branch plants of expanding firms elsewhere or by encouraging the creation and retention of indigenous high-technology businesses. In addition, OTA identified 15 "high-technology education" programs—initiatives undertaken by States in conjunction with their universities, and dedicated to equipping inventors or entrepreneurs with the skills needed to create firms that will develop or commercialize emerging technologies. In many cases, however, it is difficult to draw the line between these two categories, particularly where the school involved was a State university and thus funded by the State. (In addition, the 15 education programs included here are only a fraction of the HTD programs that have been launched by colleges and universities, both public and private, throughout the Nation.)

The services most frequently offered by these 53 programs involve information dissemination—17 programs link industry and university resources, and 8 others involve promotional activities aimed at advertising the State's resources and opportunities for high-technology firms. Almost half of the programs also offer some form of financial assistance—nine programs assist entrepreneurs in locating venture capital, another nine deal with industrial revenue bonds, eight provide grants for research and development, and four provide loans to high-technology firms. Other services commonly offered include: market development assistance (seven programs); product development assistance (four programs); and incentives or assistance in training technical personnel (five programs). More unique services include helping inventors to acquire patents; providing laboratory or office space for new and growing businesses; and investing public pension funds in high-technology business.

Unsuccessful High-Technology Programs

In the course of its census, OTA also identified several high-technology industrial development programs that have not succeeded. It is unclear whether the reasons for failure are unique to each program or State, or the result of program design flaws that other States should avoid. Several examples follow:

- The Maine Capital Corp. (MCC) was established by the legislature in 1978, with capitalization encouraged through a 50-percent credit against State income tax for investments in MCC. Since that time, MCC has funded only one project—a manufacturer of electronic parts—and that project was unsuccessful. Although MCC has advertised its services, the fact that it could invest only in Maine corporations, and the fact that it is so close to Boston (a center of competing private venture capital activity), may have rendered the program ineffective.
- The Michigan Business Development Corp. (MBDC) was authorized by the legislature in 1979 to promote the growth of small high-technology businesses in Michigan by channeling private venture capital to existing firms. However, the legislature did not reach final agreement with the financial community about stock and royalty rights before the legislation was passed. As a result, the private sector was unwilling or unable to provide funding to businesses through this program, and MBDC never got off the ground. It was replaced in 1981 by the Michigan Economic Development Authority, which has established several programs for financial assistance to high-technology industry.
- The New Jersey Office for Promoting Technical Innovation (OPTI) was set up in 1979 to encourage economic development through technical, business, and financial assistance to technology-based enterprises. OPTI had a broad mandate but little flexibility or funding. It attempted to encourage everything from "basement inventors" to sophisticated licensing arrangements, but its greatest success may have been in involving the private sector in screening and financing promising projects. The program lost its funding on September 31, 1982, but a source close to the program characterized it as a useful experiment whose errors were taken at a low cost. A proposal to create a similar mechanism, with these errors corrected, has been introduced in the New Jersey Legislature.

High Technology in Overall State Strategies

Most States report that "high-technology development" is part of their overall strategies to increase economic growth, create new jobs, and enhance the standard of living of their people. (These strategies are described in the appendix.) As part of this effort, most States seem to be assessing their strengths and capitalizing on them in order to develop, attract, or retain high-technology industry. The resulting programs therefore appear to target different phases in the development and commercialization of new technologies, according to each State's comparative advantage. These phases correspond roughly to basic stages of industrial innovation: 1) initial research and product/process development; 2) commercialization and firm creation; and 3) expanding production or application by established firms. These patterns are illustrated below with examples identified by the OTA census.

Strategies Focused on Research and Development

States whose high-technology strategies emphasize basic and applied research in emerging technologies tend to focus on the resources and facilities of their university systems, and on the importance of cooperation between industry and university activities. Several States are working to improve or expand the university faculty, curriculum, and research in relevant disciplines. To encourage these efforts they often provide R&D tax credits, offer challenge grants for university research, seek out Federal R&D contracts, and even support the creation of independent centers of research and development.

- Michigan has set up several research institutes with State funding to conduct research and development in biotechnology and robotics.
- Illinois and Utah both have a biomedical research park connected with the State university.

Strategies Focused on Commercialization and Firm Creation

Some States encourage the development and commercialization of new technologies by providing their inventors and entrepreneurs with the services they need to create new firms and bring new products to market. These services include providing product and market development assistance, finding capital assistance for new products and young companies, and in some cases establishing "incubator facilities" for high-technology business starts.

- Georgia's Advanced Technology Development Center provides technical and business assistance, helps firms to find venture capital, and provides incubator space for new businesses.
- The Massachusetts Technology Development Corp. provides venture capital for firms and products that would usually be overlooked by traditional sources of capital, and it also provides assistance with business plans, management, and marketing.
- The Hawaii Venture Development Fund has a special "Inventor's Fund" for the development of new product ideas.
- The Connecticut Product Development Corp., which makes equity investments in existing firms, has recently set up an Innovation Development Loan Fund to fund the development of innovative projects.

Strategies Focused on Expanding Production and Mature Industries

Instead of targeting the early stages of the innovation and firm creation processes, some States concentrate on attracting the assembly facilities of expanding or relocating high-technology firms, or on transferring and applying new production technologies to help firms in mature industries.

Attracting Production Facilities.—Some States, aware of their limited R&D capability or skilled labor pool, are instead trying to attract the production of more standardized high-technology goods in the State. These States pursue expanding and relocating high-technology firms in much the same way that they recruit more mature industries, through promotional programs and through location assistance and tax incentives. Alabama's New and Expanding Industry Program is an example of such an initiative; similar strategies to attract high-technology expansion and relocation are being used in Delaware, Idaho, and Puerto Rico.

Process Development and Application.—The "high-technology" strategy in many States emphasizes technology transfer—the application of new production technologies to the manufacturing processes in mature industries already located in the State. By increasing the efficiency and productivity of the existing industrial base, these programs may strengthen and retain facilities and jobs that might otherwise leave the State or the country. Maine's New Enterprise Institute, for instance, helped to introduce technologies like computer-assisted design and manufacturing to the shoe industry, and Michigan has established an Industrial Technology Institute to promote the development and application of robotics in the automotive and other mature industries. Similar programs are underway at the Mississippi Research and Development Center, the Arkansas University-Industry Experimental Center for Small Manufacturers, and the Texas Engineering Experimental Stations.

Other Approaches

Some State strategies have aimed at developing integrated markets within the State, thereby providing the opportunity for new high-technology industry to produce for and obtain inputs from existing industry. Examples of this approach are Michigan's emphasis on robotics, both as a new manufacturing sector and as an input to the automotive industry, and Arizona's attempts to encourage the growth of high-technology support industries.

A few States have placed their general industrial development emphasis on technology appropriate to unique State needs, and are not making a concerted effort at this time to attract or develop high-technology industry. Alaska, for example, has a limited manufacturing base and special application needs in most technologies.

Finally, several States have attracted "spillover" high-technology industry without a concerted effort by their governments. These States include Colorado, Oregon, Arizona, Washington, and New Hampshire. Each of them may be a desirable place to live and do business, but in each case they also have the advantage of proximity to a growing center of high-technology development—apparently a great advantage in attracting branch plants.

Office of Technology Assessment, *Technology, Innovation, and Regional Economic Development*, Background Paper, Census of State Government Initiatives for High-Technology Industrial Development, Congress of United States, Washington, D.C., 1983, pp. 7–12.

The Economist

WALL STREET 'TIFF' WITH HIGH-TECH

3.8

Wall Street's tiff with high-tech

NEW YORK

The prices of many computer shares are collapsing on Wall Street as the long boom in America's office-equipment sales falters (see chart). The news of lower profits for this quarter knocked 14% off the value of Wang's shares on March 12th. On the following day, shares of Computervision, a fast-growing maker of computer-aided manufacturing gear, lost 25% of their value within a few minutes when the company said it would make no money this quarter. In the final quarter of 1984, its earnings had been 43% higher than in the same period in 1983. Newport Corporation, a high-technology laser-research company, has suffered a similar fate.

Other victims include Data General, Digital Equipment Corporation (DEC) and Apple. Data General's share price is down to about $46 from a peak of $76 early this year. It lost about $14 in a single day when the company said orders were soft. Share prices of DEC and Apple are at or near their low points for the year so far, after hints of a dull first quarter at DEC and Apple's decision to halt production for several days (see page 66). Even IBM's share price dipped 2.7% on a single day in mid-February following predictions of flat first-quarter earnings. It has slipped further since.

Hambrecht & Quist's technology stock index, which recovered to 600 by the end of January from a low of 470 last July, is back down to 555. Broader market measures have lost much less ground than that since Wall Street peaked in mid-February. The Standard & Poor's 500 index is down a mere 2% from its 1985 high.

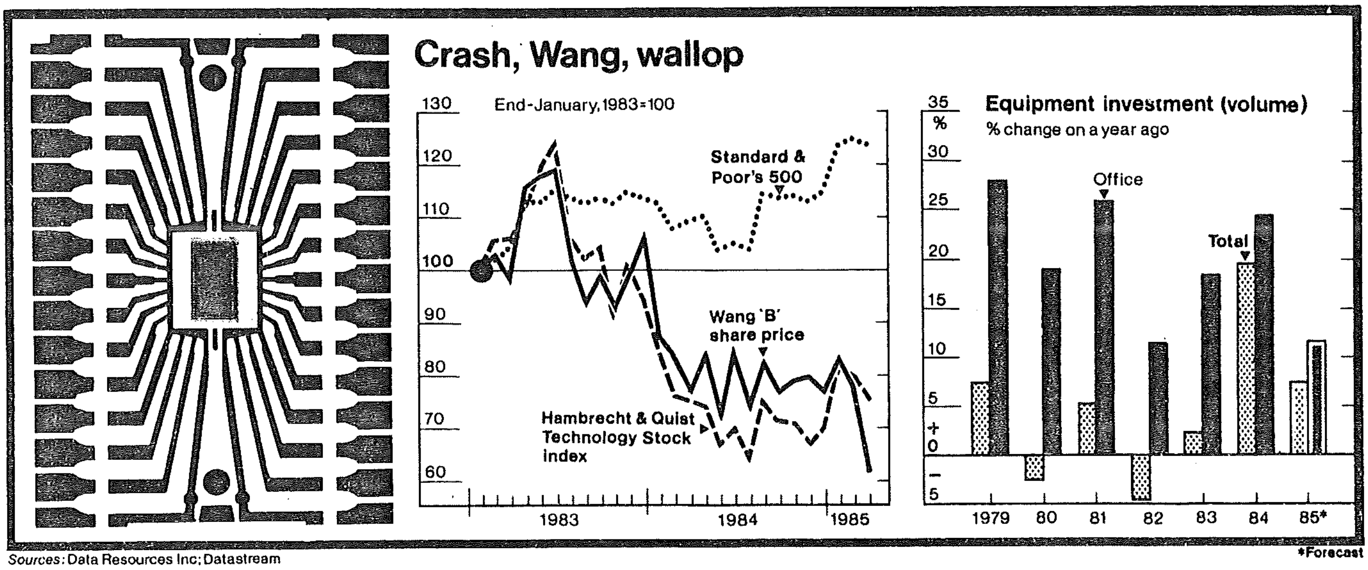
Slower capital spending has hit high-technology companies hardest. Spending on office equipment, which has accounted for nearly a third of the rise in America's capital outlays in the past two years, was up by 24% in real terms last year. Data Resources estimates that this growth has slowed to 8-10% in the first quarter (always the industry's most vulnerable) of this year.

The high dollar and strong competition further undermined profits at many high-technology companies. IBM says its gain in foreign earnings last year was cut from 32.4% to 19.6% by the dollar's strength. Wang also blames the dollar, though critics of both Wang and Apple say their products are falling behind. Strong competition for market share in a young industry has kept margins low despite strong demand. The bewildering range of new computers has encouraged some buyers to delay their purchases.

Semiconductor companies were the first to feel the chill. Orders slumped after the late summer, and so did the share prices of big companies in the industry. At mid-week, Texas Instruments was standing at $107½, little higher than its low point for 1984. National Semiconductor, at $10⅝, was also near last year's low. Now small companies have also lost favour. Micron Technology, one of the hottest new issues last year, is a notable example. Its share price has fallen from a peak of $40 in 1984 to around $12.

The fall in orders, which hit Data General and other big computer makers a month ago, is now affecting more specialist companies like Computervision. Software and data-services firms have been spared so far. The value of these companies' shares rose on average by more than a quarter in January, though this growth rate dwindled to 2% last month. Mr Stephen McClellan, the computer analyst at Salomon Brothers, argues that software and data firms are insulated from swings in the dollar and capital spending. But then the same used to be said of computers.

How bad is America's high-technology hiccough? The semiconductor industry association reports a small rise in orders



Sources: Data Resources Inc; Datastream

*Forecast

76 in February, but it guesses that orders in 1985 will stagnate at last year's level. Data Resources predicts a 12% increase in real spending on office equipment in 1985. That is roughly half the increase last year, but still better than the 7.4% gain expected in industry's total spending on equipment. Which is why Wall Street may keep returning to high technology despite high risks of sudden shocks.

'Wall street's tiff with high-tech', *The Economist*, 16 March 1985, pp. 75–6.

The Economist

3.9

PLANTING SCIENCE PARKS IN BRITAIN

Planting science parks in Britain

The university-based science park is an American idea that is catching hold in Europe. Britain has 13 of them (including some creatures called "innovation centres") with another 15 on the way. West Germany is fast catching up. Its first innovation centre opened for business in November, 1983; seven are now fully operational and another 40 are planned. Will they spawn the high-technology enterprises and jobs their sponsors hope for?

Nobody agrees on what exactly a science park is. Britons think of them as sites for high-tech factories that feed off the talent of a nearby university. Often the British university itself provides the land. But the university connection need not remain close. Americans no longer distinguish between Silicon Valley and the Stanford University Industrial Park which spawned it. In Britain the same fuzziness is apparent at Cambridge (see box on the next page).

Science parks began to sprout in America 30 years ago. Some, like the constellation of firms around Boston's route 128, grew haphazardly as university scientists formed or joined companies founded to exploit their ideas commercially. Others, like North Carolina's Research Triangle, were carefully-planned nurseries for new technology. A few just happened: Silicon Valley was the by-product of the happy match of a great land-rich university (Stanford) and a hungry new industry (electronics).

Not knowing exactly what science parks are has not stopped governments wanting them. State governments in America continue to invest exuberantly in science parks in the hope that they will attract new high-tech firms: in 1983 the National Governors' Association identified new research parks in 18 states. In Europe, too, science parks are seen as a way to create expanding businesses and new jobs. As an answer to some of Europe's problems with low growth and high unemployment they provoked near euphoria at a meeting on science parks sponsored by the EEC commission in West Berlin last month. But beware of the hyperbole: it takes more than a university and some land to grow a Silicon Valley.

Esse quam videri

Not all science parks bloom; none does so overnight. Many new parks in Europe, especially those started in derelict areas, take heart from North Carolina's Research Triangle. Founded in 1956 to revive an industrial backwater, it now covers about 5,500 acres. Companies and laboratories within the triangle employ nearly 20,000 and it claims one of America's highest per capita concentrations of PhDs.

Part of the triangle's success is attributable to three goodish research universities in the neighbourhood—Duke University, the North Carolina State University and the University of North Carolina. But success was not inevitable. The triangle's would-be imitators tend to forget that the park grew feebly at first. On its ninth birthday, in 1965, it housed only nine laboratories employing fewer than 1,000 people. Impressive expansion did not begin until IBM decided to buy a 400-acre site, and two federal laboratories followed its lead.

Many similar American projects, including those associated with Cornell and the University of Missouri, have failed. Others have limped on without ever looking like fulfilling the high hopes of their founders. So why is Europe so uncritically enthusiastic about science parks?

Bringing inventions to market

One reason is a changing diagnosis of what ails European technology. Ten years ago, Europe's disappointing performance was usually blamed on poor scientific research. Today, Europeans worry more about the sluggish rate at which technology moves from laboratories to the marketplace: one reason why the Welsh Development Agency, for instance, has founded WINtech as a sort of walking, talking and videotexing version of the yellow pages to help those involved in new technologies to know what others are doing. Science parks are seen as another spur to innovation.

Britain is particularly keen to jump on the bandwagon. Most European countries (West Germany is an exception), are treading warily. France is concentrating on a few well planned parks, notably the Sophia Antipolis development on the Cote d'Azur; Holland plans a substantial science park at Groningen.

British universities, on the other hand, have rushed during the past three years to emulate the science parks started in the 1970s at Cambridge University and Heriot-Watt University in Edinburgh (see table). The ventures in this second wave vary widely in size, scope and management. Some (eg, in Leeds, Hull and Merseyside) are short of space and can offer little more than "incubator" sites for start-ups. Others are on greenfield sites with ample room to grow, and can try to woo established high-tech firms or even the research units of big multinationals.

Some examples give the flavour of this second wave:

Aston science park is managed by Birmingham Technology Limited, a company owned jointly by the City of Birmingham, the University of Aston and Lloyds Bank. The park got started with the aim of creating wealth and employment in the Birmingham area. Its first phase, a £2.5m development for start-up companies, has 14 tenants. If all goes to plan, a second phase this year will add nine "venture" units of 5,000

British university science parks and innovation centres

Location	First tenant	Number of tenants at end 1984	Total employment at end 1984	Source of Venture Funds: Regional development agencies	Local government	Universities[3]	Banks	Property developers	Private firms
Heriot-Watt	1972	19	380	★[1]		★			
Cambridge	1973	40	1300			★			
Aston	1982	12	100		★	★	★		
Merseyside	1982	10	200		★	★			
Warwick	1983	19	100		★	★	★		
Bradford	1983	18	130					★[2]	
West of Scotland	1983	4		★		★			
Leeds	1984	7						★[2]	
Hull	1984	7	Each					★[2]	
Manchester	1984	2	less		★	★			★
Surrey	1984	3	than			★			
Loughborough	1984	10	100		★				
Brunel	1985	0							

Notes: 1. The Scottish Development Agency has been involved only since 1983.
2. English Estates, the property developers involved here, is a government-backed agency.
3. University involvement ranges from providing land only to infrastructure, buildings and even venture capital.
Source: Jones & Dickson, Technical Change Centre, 1985.

81

The Cambridge phenomenon

In the 1960s, the entrepreneurialism of the Massachusetts Institute of Technology turned Cambridge, Massachusetts, into a mecca for high-tech start-ups. Something similar has at last happened to Cambridge, England. A study published last month by Segal Quince and Partners, a management consultancy, found that by the end of 1984 there were some 350 high-tech companies around the city, compared with 50 around Oxford.

This flowering of enterprise was helped by Trinity College's decision to start the Cambridge Science Park (CSP). Since opening for business in 1973 it has become the most successful in Europe. Even so, the CSP is far from being the main cause of Cambridge's high-tech boom. By American standards, its growth has been modest. It took 10 years to fill its first 24 acres, and by the time it began to attract bigger firms (eg. Napp Laboratories in 1981) it had become as much a beneficiary as a cause of the so-called Cambridge "phenomenon".

So why has high-tech come to Cambridge? First because of the lure of Cambridge University itself: being Britain's best technological university (Oxford graduates read on) helps. Academic excellence, though, is not the whole story. Cambridge would not have spawned so many firms had it not also:

- changed its attitude towards industry. In the 1960s Cambridge discouraged industrial development: even IBM was turned away. In 1969, Sir Nevill Mott, head of the Cavendish Laboratory, changed the university's mind. From then on, links with industry were actively encouraged;
- given its scientists a free hand in business ventures. Many universities try to help their dons become entrepreneurs—in exchange for a share of profits. Cambridge believes the risks and the rewards should belong to the individual scientist. Its best gift to entrepreneurial dons is lots of free time;
- kept a high proportion of untenured staff on short-term contracts. At the end of 1982, nearly half of the university's 1,734 staff were in this category. Many of these people are reluctant to leave Cambridge: joining or starting a local business is often their only chance to stay.

If Cambridge, why not Oxford? Oxford has a bigger ratio of arts to sciences and its science tends to be "pure" rather than "applied". Oxford has never overcome its inhibitions about links with industry. The fate of the motor industry in nearby Cowley has reinforced fears that industrial development will undermine Oxford's character as a cosy university town.

square feet each for successful spin-offs from established firms as well as the start-up companies.

Manchester science park is a joint venture of the City of Manchester, Manchester University and four companies—Granada Television, Fothergill and Harvey, Ciba-Geigy, and Ferranti. Its aim is to exploit the resources of local universities and polytechnics to lure high-tech firms to the area. A first building of 24,000 square feet was opened in December, 1984.

Merseyside's innovation centre is a company owned by the Merseyside County Council, Liverpool University and the Liverpool Polytechnic. One aim is to provide "nursery" accommodation and expert consultancy to new high-tech firms. Ten small companies operate from MIC's first 10,000 square foot building.

A science park is growing fast at **Warwick** University. The first "incubator" building of this joint venture between the university and three local authorities—Coventry, West Midlands and Warwickshire—opened in February, 1984. Within seven months it was fully occupied. The park's unusually fast take-off is attributed to two things: the offer of free management advice by several financial institutions to start-ups; and the way the university's growing reputation for advanced research on automation has attracted Computervision, Boston's Automatix and other established firms.

It is probably no coincidence that universities began to rediscover science parks in 1981, shortly after the government's University Grants Committee began selective cuts in their income. Desperation also helps explain the participation of many local authorities. The danger is that too-high hopes are riding on too slender an investment. Only a tiny fraction of industrial R&D is ever likely to be performed in science parks.

The assumption that science parks will generate lots of jobs looks certain to be frustrated. The planners of the early science parks thought that entrepreneurs with good ideas would get started at science parks, develop their businesses and then move out to establish factories. Something like that did happen at Stanford (the classic example is the company created in a garage by two bright students, Mr William Hewlett and Mr David Packard). this has not happened in Britain: some companies grow, but the hoped-for transition to mass manufacturing has not occurred. In the past two years, British science parks have created fewer than 2,000 jobs directly.

British universities note that high-tech businesses in Britain lack the cornucopia of America's defence and space budgets. Without more public investment science parks in Britain are unlikely to develop a critical mass of scientists and technologists.

British parks have particular difficulty in persuading propertymen to build on spec the offices they need to attract mobile, and impatient, high-tech firms. Many British parks are in the depressed north of England where property companies are loth to build. Many science parks also lack the other ingredients that American experience suggests are essential for success. Mr Russell Cox, president of America's Resort Management Incorporated, told the meeting in Berlin that these included:

- **Being a nice place to live.** In America, attractive homes, cultural activities and good schools and universities have proved a big plus for science parks. High marks go to Boston and San Francisco; lower ones to Knoxville, Tennessee; Huntsville, Alabama; and Rome, New York.

● **A good university** nearby, like Stanford or MIT, is invaluable. It can provide services, consultants and bright graduate students. High-tech firms are attracted by the prospect of sending their employees back to university for part-time training or research.

● **Research facilities** on or near to a science park help to get things snowballing—other laboratories and support services follow the first, leading to a critical mass of scientists who can spin off small startups of their own.

● **A skilled labour force**, which does not mean just boffins. In the United States science parks have benefited from the dexterity of the disciplined workforce laid off by such declining industries as shoes and textiles.

Mr Cox reckons that communities lacking one or more of the above will find it hard to develop science parks. If he is right, many British ventures—and particularly those in the derelict areas that yearn most for an industrial renaissance—will have to prepare themselves for disappointment.

The only ingredient with which Britain is well endowed is publicly-funded universities and polytechnics. But exploiting this resource properly does not mean copying American approaches. British universities differ in three important ways:

● **Scale.** British universities are smaller and there are fewer of them. For example, the Merseyside Innovation Centre (MIC) is trying to do in Liverpool what Philadelphia did in 1965 when it launched a science centre to arrest years of decline in old industries. Whereas MIC is relying on two higher education institutions, the Philadelphia science centre drew on the resources of 28 local universities and colleges. They included the University of Pennsylvania, one of America's leading research campuses.

● **Management.** Britain's small government-supported universities are run by academics; many of America's bigger ones by professional managers with experience of fund-raising and business. To compensate for their lack of expertise, some British universities have set up formal mechanisms—eg, arms-length limited liability companies—to manage relations with industry.

● **Ethos.** Entrepreneurialism does not come naturally to British dons. Two of the main reasons for this—tenure and secure public funding—are gradually being eroded. Salford, a university savaged by the government's penny-pinching on education, has pioneered some of British higher education's most imaginative links with business. Shock treatment, though, can only go so far: bigger cuts in government money for universities risk destroying the good research and researchers that attract risk capital to their science parks.

'Planting science parks in Britain', *The Economist*, 16 March 1985, pp. 80–1.

Stuart Macdonald
THE NEED TO SUCCEED

Some countries seem to attach particular importance to the role of the individual, and especially the entrepreneur, in industry. His success may be viewed as no more than just reward for personal effort, but the provision of a suitable climate in which to make that effort may well be seen as something of a national responsibility. If a man succeeds, he may have both himself and the country to thank; if he fails, he can have only himself to blame. The United States is, perhaps, the outstanding example of a country in which the entrepreneur has achieved a degree of national veneration. Much of this mystique has its roots in the entrepreneurial origins of many established American companies. Yet, it is questionable how much opportunity for individual impact now remains in many modern industries most associated with such activity in the past; the automobile, chemical or computer industries, for example. Emerging industries are likely to provide most scope for entrepreneurial activity and individual impact for they have no established structure or hallowed traditions. Such a new and essentially American industry has formed since the Second World War to make semiconductor devices.

It may well be that the part played by the individual in the American semiconductor industry is only an extreme example of what is going on in other industries and in other countries. On the other hand, it may be that this particular industry has established its own distinctive tradition for, even after thirty years, the individual still seems to have an extraordinarily important role to play.

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The rapid pace of change in the semiconductor industry has meant that success has often gone to those companies which have either initiated or been quickest to respond to technological and commercial developments. Those companies which have been slow to respond to change have often not just failed to prosper, but to survive. Success for the company has been closely associated with success for the individual, working either within the company or as an entrepreneur. In a climate of constant change, the entrepreneur and the individual can thrive, especially when the incentives are great. Those incentives, upon which the success of individuals and, consequently, of companies is dependent, are interesting in themselves, but if they give some insight into the ways in which this particular industry works that might be of relevance in other industries, then their consideration should be especially worthwhile.

The Impact of Semiconductors

Most people know very little about semiconductors. A relatively informed opinion would be simply that they are the materials at the heart of transistor radios and pocket calculators. In fact, semiconductors are, as the name implies, materials that are half way between metals and insulators; they are materials whose ability to conduct electricity is dependent on the presence of tiny concentrations of impurities. Subtle control of the impurities determines how and when electricity may flow through the semiconductor, so that a small effort produces a very much greater result. Consequently, semiconductors used for such purposes as amplifying current, require little power to operate and also have the advantage of being robust, tiny and ridiculously cheap.

The predecessor of the semiconductor amplifier (the transistor) is the thermionic valve, which also amplifies, but is comparatively expensive, uses much more power, wears out and is fragile, hot and inconveniently large. For example, nearly all the electronics in a pocket calculator are contained in one solid 'chip' of the semiconductor silicon, measured in millimetres rather than centimetres and costing as little as 50 pence.[1] The price, the size, the performance and the reliability of the calculator would be inconceivable with valves and are all consequences of semiconductor electronics. So too are modern computers, electronic telephone exchanges, most of the latest weapons, satellites, new television sets and radios, much medical and domestic electronics, police radios, burglar alarms, automation in factories, modern commercial stock control methods and a great deal more. It is hard to imagine that there is anyone who has not been affected by the use of semiconductors in electronics and most of us have had our lives altered more radically than we probably realise.

The Semiconductor Industry

There are several unusual features about the industry which has done so much with semiconductors. Though it now has annual world sales of about $7000 million, the industry is comparatively new. Until the invention of the transistor in 1947, there was really no semiconductor industry at all. Secondly, the industry has always been dominated by the Americans.[2] The United States has led the way in the invention, development and production of semiconductor devices[3] and now accounts for over half the world's semiconductor consumption.[4] Many of the companies in the British electronics industry are American owned and, in the semiconductor business, the largest British firm has about a quarter of the market share of the largest American firm in Britain. In integrated circuits, one of the most advanced and important of semiconductor developments, American firms hold about three-quarters of the British market. In 1968, 48% of British semiconductor consumption was imported, half from the United States.[5] This powerful American industry has grown up largely outside the established electronics industry. There are many small new firms, but many of the largest companies, if not actually formed specifically to make semiconductors, had little previous experience of electronics. Lastly, the industry has often been called immature because it is constantly introducing new techniques and products. These have precipitated massive and regular price cuts and have made it difficult to automate production lines. The industry remains volatile because it is dominated by change.

For some, these characteristics have helped make an attractive industry in which to work. It is hard to imagine the majority of semiconductor workers finding much interest in their jobs at all for the industry is highly labour intensive and has always been characterised by the cheapest possible labour performing the most mundane and repetitive of tasks, assembling, testing and sorting components. Physical conditions are good because the materials demand extreme cleanliness, but the work is incredibly boring. There are actually more people employed by the American semiconductor industry in 'cheap labour' countries than in the United States[6]. But the situation is very different among the technical and managerial ranks.

There the industry is regarded as being dynamic and exciting. This paper is concerned with the attitudes of this latter group towards this extraordinary American industry, and is based upon a series of interviews with some of those most intimately concerned with the industry.

Growing Pains in the New Industry

The transistor, the device which launched the new semiconductor industry, was invented by Bell Laboratories, the massive research arm of the American Telephone and Telegraph Company. In 1952, all Bell's transistor knowledge was made available to firms wishing to purchase a licence and such early commercial products as transistorised hearing aids and radios appeared soon after. There were then eight large electronics companies in the United States manufacturing receiving valves, the component which the transistor, with its ability to amplify, seemed to duplicate[7]. All these firms took an early interest in transistors and led the way, along with Bell Laboratories, in further research and development. Yet, by 1959, these large established companies had lost much of the semiconductor market to new companies[8].

The large companies seem to have made several mistakes. The fairly static field of valve development did nothing to encourage a feeling that an apparently similar device would be any more dynamic. In its 40 years of development, the valve had become the concern of technology entirely so that, by the early fifties, there was no valve science of commercial interest. The transistor, on the other hand, was a product of science[9]. To understand how it worked required a knowledge of quantum mechanics; to make it work demanded the purest materials then known to man. Not surprisingly, there were those with careers steeped in valve technology who could not or would not understand the new science, who even felt threatened by it. This is important, for in nearly every one of the old electronics companies, transistor development was organised within existing valve departments because both devices performed the same amplifying function. So great was the logical appeal of similarity, that the much more important differences of the transistor — its small size, tiny power requirements and totally distinctive way of working — were conveniently subsumed. Separate semiconductor departments were not generally formed until the mid-fifties[10].

The established electronics companies failed to appreciate how rapid would be the rate of semiconductor progress. In the traditional electronics industry, change took place over years and could be adequately managed by corporate machinery. Considering the immense early difficulties in manufacturing reliable transistors at a yield which would permit a realistic price, there was some justification for thinking that semiconductor development would take some time[11]. The established industry failed to recognise the importance of the personal factor, which, in semiconductors, has always been crucial and has generated change in months rather than years.

It was very much easier to produce one or two transistors in the research laboratory than it was to produce them in quantity in the factory. The industry was plagued by problems of low yield, of making devices with the required characteristics, problems of poor reliability and high costs. The processes were highly labour intensive with lines of girls using tweezers to assemble transistors under microscopes. Mysterious impurities such as the fabled 'deathnium' would unaccountably ruin transistors and processes such as 'forming' (the injection of a high current) would, equally unaccountably, improve their performance. There were no proven ways to make transistors and no companies to supply the necessary equipment. Most companies made their own furnaces and crystal-pullers and many handled the entire process from refining the ore containing the semiconductor element, to selling the finished electronic component. Valve technology was understandably used to overcome some of the problems and many early transistors appeared sealed in glass or cans to protect them from contamination.

There was no guaranteed formula for success and there was a scarcity of men with even a few years experience in the field. The first university course on semiconductors, for example, did not begin until 1951. Consequently, those few who understood the science of semiconductors and who had some knowledge of those empirical methods which brought some success in the factory, found themselves in a peculiarly powerful position. Not only were they working near the frontiers of science, but they also held the key to the successful commercial development of semiconductor devices.

The Early Importance of the Individual

Many of these men were, of course, at Bell Laboratories and many continued to work in that quasi-academic atmosphere which produced so much of the semiconductor innovation of the fifties[12]. Good scientists were attracted to Bell by its reputation and Bell became something of a school in which good solid state scientists became semiconductor experts. This role was also played by the older electronics companies with their semiconductor research and development units, for there simply was nowhere else where one could learn how to make semiconductor devices[13]. But, having mastered aspects of the new semiconductor technology, many of these men were reluctant to remain with either Bell or the large electronics firms. They found it frustrating to be aware of the potential of semiconductor devices and yet be subject, in the case of the large electronics companies, to a distant authority which regarded semiconductors as a very minor and uncertain portion of electronics.

Existing management often did not appreciate the essential differences of semiconductor electronics and, within a valve department, the attitude towards the interloper could be indifferent or even hostile. But the objections of the semiconductor men were more fundamental. They were generally young men, unversed in other skills, who had suddenly and almost unexpectedly found themselves at the forefront of both scientific and technical knowledge. While they fully realised their own importance, it seemed that this realisation was shared by few others in the large electronics firms. A young man with a degree in a semiconductor department was paid much the same and generally had the same sort of career prospects as any other young graduate anywhere else in the company, even though he might have been one of only a handful of men who knew the secret of a particular semiconductor process. Consequently, when he was offered more money and prestige elsewhere, he left.

However, it was not just money that motivated these men. It was a heady experience to be at the frontier of what they knew to be an important branch of knowledge. Discoveries, which in themselves would make a man's reputation if not his fortune, seemed to be just waiting to be made. There was also a peculiar satisfaction in exploring a new frontier, and a feeling that that frontier would not remain accessible for ever. It was perhaps inevitable that semiconductor men should have felt baulked when those who were not familiar with that frontier failed to understand the individual's hurried pace or comprehend the direction of his exploration. Consequently, there was real incentive for semiconductor experts to take their expertise elsewhere.

"In Westinghouse, as in probably every major older company, semiconductor activity started in an applications sense . . . in the division that was making the tubes for the same purpose . . . It handicapped semiconductors for a bit because it made the transistor look like a replacement for the tube and it was perhaps some years before people started to look and see what it could really do in its own right."

"They put managers in the early days in charge of some of their semiconductor operations that didn't know a blessed thing about a semiconductor device . . . For about a year we had a going-away party every Friday."

Many semiconductor experts of the fifties took their expertise to new firms. They went to companies which, though they had little experience of electronics, thought sufficiently highly of the potential of semiconductors to investigate them thoroughly, either for their own use or for sale on the open market. They also went to new firms founded specifically to make semiconductor devices, and sometimes they started their own semiconductor companies(14). That this happened as part of the organising process of a new industry will cause no surprise, but in the semiconductor industry the mobility continued and is still a crucial factor. With it is associated an enthusiasm for the semiconductor industry that, in some, approaches obsession. The strength of that feeling of involvement may account for much of the success of the American semiconductor industry.

Individual Motivation

There is no doubt that one of the main attractions of the semiconductor industry is the money that can be earned from it. Personal fortunes have certainly been acquired and these lure others. Most money is probably to be made by an individual starting his own company, but the risks are correspondingly great. The company might do well enough catering for small specialised markets, by providing a second source for products produced by bigger companies, or simply by establishing semiconductor capability to be bought by a large company wanting to join the industry. But the aim is often to form a new company to take advantage of an idea no one else has considered or thought worthwhile. The incidence of failure among such companies is uncertain, but may be high. What is quite certain is that many of the leading companies in the American industry started in just this way(15).

"The top few people in the company have the opportunity of making a great deal of money if they are successful, and I think this provides the motivation to work night and day to really just out-perform the competition . . . I wouldn't have left Fairchild except for the prospect of making some money. I just wouldn't have done it because that was interesting too. I had a good job and a good future there."

More common is the desire to become rich by means of stock options. To procure a man or reward him over and above his salary, a company will often offer a share in the firm. The stock is cashable at some future date and also has the effect of encouraging an interst in the firm's prosperity. It would be typical for a firm to have about a quarter of its total shares allotted to favoured employees and some companies have replaced even employee's pension funds with stock awards. The transfer from one company to another of leaders in the industry has normally been accompanied by enormous allocations of stock(16). The move of Lester Hogan from Motorola to Fairchild in 1968 has become legendary because of the generosity of the stock options involved. Hogan was promised a salary of $120,000, an interest-free loan of $5.4 million to buy 90,000 Fairchild shares at $60 each, and the chance to buy another 10,000 shares, then worth $80 each, for $10 share. His paper profit then approached $2.5 million[17]. The value of stock naturally depends upon the fortunes of each company, but there have been times when semiconductor shares have been considered glamour issues, when virtually any semiconductor stock was valuable[18]. There have also been times when much stock was worth little or nothing.

> "The semiconductor world is populated by almost rich professional managers who took jobs with a nice salary expecting to become millionaires with the stock option they got and every one of us has somewhere in a dresser drawer stock options worth garbage and not that much money in the bank."

Yet, there seems to be more than money attracting men to the industry and stimulating them in it. Though the semiconductor industry is now a generation old, much of its technology is very new. There has been constant technological change and, though the direction of future change is far from certain, it seems that future technology will be the product of work done in industry rather than in universities or government research laboratories. Industry has always carried out the mass of semiconductor research and development and there is no indication that this will alter[19].

> "I've never been bored in this industry — never . . . This is a state-of-the-art type of industry and the state-of-the-art is what kind of a recipe you can put together to do it right. That's why you never get bored."

Inseparable from rapid change, and perhaps a direct result of it, has been regular and massive price cutting associated with furious learning economics. Products, processes, companies and markets have changed as rapidly as technology[20]. Rarely do individuals have the opportunity to participate in both the technical and commercial aspects of an organisation. It is absolutely necessary that there be this dual activity when products have both a brief technical lifetime and an equally temporary commercial existence. If there is a shorter road to disaster in the semiconductor industry than attempting to make a product without understanding the difficulties of selling it, it is selling a product without realising how difficult it is to make. In the past, the industry has probably had men with more technical expertise than commercial, a factor which may explain why some think that, taken as a whole since the invention of the transistor, the American semiconductor industry has operated at a loss. There is now a proven need in this industry for individuals possessing both technical and commercial expertise and there is no doubt that many welcome this dual challenge to their abilities.

> "Many of us, at least in this industry, start out as technologists or engineers and then we become more and more interested in the business or economic aspects of any particular corporation. Semiconductors allows you to do both."

> "Whereas most industries are strongly influenced by marketing considerations, the semiconductor industry is still basically run by technically-trained people who think they can pull off anything . . . The thirteen years I spent in the semiconductor industry I never saw a marketing forecast that came true . . . They are really technical people who figure if they can make it, somebody will buy it, rather than marketing people who figure there's a need and I'm going to fill it."

> "If the semiconductor managers weren't such idiots, we might not have integrated circuits where they are today, because somebody would have said, 'Look, I can't manufacture them at the cost I've got to sell them'. It was the wrong decision, but it has benefited us all."

Many also find satisfaction in the use made of semiconductor products. Modern electronics has radically altered the organisation of society and it seems inevitable that with more factory automation, more computers, with electronics in the home, the car and in telecommunications, its influence will increase. It is important to many working in the industry that they are playing a part in creating this influence. This feeling of involvement in matters of consequence is even more marked when the individual has played a personal role — as many have — in some of the more dramatic exploits made possible by semiconductor electronics. To have helped put a man on the moon, for example, is deemed something to have been well worth working for.

'We knew what we were using our devices for . . . It was quite exciting when the picture was coming back from the Moon, to say to my friends and my kids 'I helped bring back that picture'. I found that very motivational and that's been the story of my career. I've always been tied to something and known where the devices were going to be used and I've found that rather exciting."

"I haven't the least idea whether I could make a great income doing something else, but this is a lot of fun and there's a lot of satisfaction in the sense that you have changed the future of the world . . . I had the people who did the solar cells for Telstar."

"Most good people like to see their ideas go someplace besides into a memo. They like to see it come out as a product . . . You would be surprised at the number of professional people who do not work for the almighty dollar, I mean that really work for self-satisfaction."

"I was absolutely amazed the number of engineers who were interviewed for jobs on the development side who wanted to meet the marketing guy. They wanted to have some assurance that their zippy ideas that were running around in their head were going to in fact get marketing push. Now where do you find that elsewhere?"

"Semiconductors are fun . . . The fun part of it is that you are really using your brain . . . The industry is young enough so that all solutions aren't tried and true . . . I can't work 8 to 5. I come in here at 7 to 7.30 in the morning and I leave here anywhere between 6.30 and 8 o'clock at night and I can't imagine where the day went . . . The personal achievement I feel in the whole thing is that in some ways I have contributed to a technology that is really serving man. I have had a little piece of the action."

The Entrepreneur and the Continuing Importance of Mobility

In a field in which rapid technical and commercial change is the norm, and in which this change is so often the product of individual inspiration or endeavour, it is not surprising that many try to strike out on their own. There is certainly financial motivation for a man to start his own company, but there are also rewards associated with being your own boss. In such a volatile industry in which change may go in any direction, it is often difficult to take a real interest in the job and at the same time work for someone with a completely different view of what can and should be done. It is an industry of experts at all levels and no natural hierarchy. If a man feels that he is not being allowed sufficient latitude to influence personally the direction of change, then one option open to him is to go into business either on his own or with a group of friends with similar convictions. The complexity of modern semiconductor technology now makes this a more expensive operation than it has been in the past, but there are still many small firms to confound the frequent prediction that the industry must eventually be controlled by a handful of large corporations. Successful innovation from new and small firms has become almost a tradition in the industry that goads men on to try for themselves. The willingness to take the financial risk and to regularly work a 12 hour day is a product of the future individuals see in the industry, of confidence in themselves and of the satisfaction they feel in being able to control the direction of their own work.

"I had a tremendous rapid change from being the president of a little company that was totally entrepreneurial to selling that to . . . and becoming part of the . . . organisation and you really can see why these companies just cannot be successful because they insist that the semiconductor operation operate in exactly the same manner as they run a steel mill . . . Large companies' control mechanisms don't let them do something until it's obvious and by that time it's too late."

"As long as there are possibilities for major new breakthroughs, there is going to be a role for small, new firms, I would argue, coming in and taking advantage of that, because the big, established firms are going to want to concentrate more on the proven technology where their scale had some sort of advantage."

It is much more typical for semiconductor experts to move from one company to another rather than start their own. If the rewards — in the full sense — in one company seem inadequate, then it is normal for the individual to look elsewhere. When new companies enter the field, or when existing firms seek to improve their capability, they do so by obtaining experts from other companies rather than by attempting to train their own. Many men in the industry have worked for an extraordinary number of companies. Generally, there is no stigma attached to this mobility and it is frequently held that much of the rapid pace of semiconductor development is the consequence of the ready diffusion of knowledge by this means. But while companies are usually eager to learn of new processes in this way, most are keen to dissociate themselves from its extreme and disclaim any interest in 'turn-key' knowledge. It is apparently possible to survive, and perhaps even prosper for a short while, with little knowledge of semiconductors, by passing from company to company, selling to each the knowledge procured from the last. Such itinerant 'job-hoppers' tend to rapidly outlive their value and are hardly typical of the real mobility within the industry. It is significant that a system which allows a man to make what he considers to be the best use of his knowledge and abilities has become acceptable and even commendable in the semiconductor industry when, in most others, it would be considered hazardous or even treacherous.

"We all know each other. It's an industry where everybody knows everybody because at one time or another everyone worked together."

"Most of the intelligence interplay that goes on in the industry is through guys quitting and whoring themselves to competitors."

"You better watch out who you talk to and what you say to every person because he may be your boss some day."

Coping with the Individual

Much of the American semiconductor industry has organised itself to help meet the demands of the individual. In the late fifties and early sixties, the Boston area, Route 128 in particular, was the home of many semiconductor companies. At least part of the attraction was the proximity of such centres of learning as MIT and Harvard and the cultural attractions of Boston. Since then, the heart of the industry has moved to the San Francisco region of California. Again, the attractions include universities active in semiconductor research, such as Berkeley and Stanford, and the relatively civilised atmosphere of San Francisco. But probably of greater importance are such factors as a pleasant climate and the availability of risk capital to start new ventures. Semiconductor companies and ancillary industries abound in the region as one company has 'spun-off' individuals to start another company which has, in turn, spawned yet more companies. Movement of personnel from one company to another is made easy by the concentration of the industry and information flow, upon which so much of the prosperity of companies and of individuals is deemed to depend, encounters the minimum of obstacles[21].

Large companies, particularly those with major interests in fields other than semiconductors, may have some difficulty in providing the adventure and prospects present in smaller companies. Those who require such conditions will leave, but not all semiconductor men feel the need for such aggressive involvement in the industry. It seems to be quite possible to enjoy the greater security of corporate employment and yet still feel sufficiently involved in the importance of the semiconductor product to want to spend long hours at work. Contact with individuals in other companies remains close, often despite geographic isolation, and much important technical and commercial information is acquired by informal contacts, particularly at conferences. It is quite usual in this industry for employees of rival companies to consult each other on technical, though not proprietary, matters.

"I have people call me quite frequently and say, 'Hey, did you ever run into this one?', and you say, 'Yeah, seven or eight years ago. Why don't you try this, that or the other thing?'. We all get calls like that."

The Great Game

There is an atmosphere in which everyone wishes to succeed, to win the same game. Yet, for any individual or company to succeed requires the participation of, and regular contributions from, others. For both companies and individuals, the semiconductor business is a continuous game in the sense that the real rewards depend upon individual achievement, the efforts of colleagues and the standard of the opposition. The challenge lies in mastering not just a technology, but a rapidly changing technology with profound social and economic implications. It would be surprising if the men engaged in such an industry did not feel a desire to succeed, but it is this very determination to win the game which spurs on the competition, encourages further technological change, and results in yet greater social and economic consequences and, ultimately, yet more commitment to the industry. The pace of change is the essence of the American semiconductor industry and it is both the cause and effect of not simply a desire to succeed, but a positive need.

"I like to participate in the game in order to see if I can win."

The intention of the National Enterprise Board in Britain, declared in June of 1978, to stimulate the British semiconductor industry by means of enticing individuals from the American industry to establish a factory in the north of England is fascinating. The firm is planned to be a high-risk, high-reward venture concentrating on producing an advanced memory chip. That such a scheme should be considered worth an expenditure of £50 million — indeed, that it should even be contemplated in Britain — seems ample testimony to the importance of the role played by the individual in a thriving semiconductor industry [22].

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Organisation for Economic Co-Operation and Development

TECHNOLOGY POLICIES AND PROGRAMS IN AUSTRALIA

3.11

2 POSTSCRIPT

In June 1983 Ministers for Industry and Technology from the Australian Commonwealth, State Governments came together for the first time to consider issues directly concerned with technology. This paper (modified) formed the basis for that discussion.

As part of a move towards the development of a national technology strategy Ministers agreed to establish a Standing Working Party of Officials. The initial tasks given to the Working Party call for an examination of areas in which competition exists between Governments for technology-based industry in which co-operation between Governments might be encouraged to assist technology development. Ministers also approved the establishment of a Sub-Committee to consider human aspects of technological change.

INTRODUCTION

One of the first initiatives of the new Minister for Science and Technology, the Hon Barry O Jones, was to seek to establish closer cooperation between the Commonwealth and State Governments on technology-related issues.

One of the current problems is the degree of competition between the State and Territory Governments in Australia to attract new, advanced technology activities. The obvious danger in this situation lies in the fact that the attraction of technology-based activities to any one area, without consideration of the effects on others, is at times counterproductive.

It must be clearly recognised that State Governments in Australia have sovereign rights in respect of technology issues. Thus, the development of any national strategy must depend upon the achievement of a consensus between States and the Commonwealth. The history of Commonwealth/State relationships has been clouded by a reticence to work together when there was a possibility of States' powers being constrained.

The foregoing is the background against which responsibility for science and technology (S&T) issues in Australia is shared between the Commonwealth and State Governments. Assistance measures for technology have evolved according to particular requirements which has resulted in an array of assistance measures:

- The Commonwealth has supported S&T generally through the higher education sector and specifically through government research organisations (Commonwealth Scientific and Industrial Research Organisation (CSIRO) and the Defence Science and Technology Organisation (DSTO)) and incentives directed at particular areas (energy, agriculture and industry).

- State Government support for S&T has been largely directed to agriculture and other primary industries ranging from 60% (Victoria) to 88% (Queensland) of total R&D expenditure in 1978/79.

While technology is important to all sectors of the economy, the main current concern at both the Commonwealth and State level has been in industrial technology.

- this is the area which has been largely neglected by Governments
  - this is illustrated by Australia's relatively low level of resources devoted to R&D in manufacturing technology;
- this appears to be where Australia has its greatest problems;
- there are some technologies (micro electronics and materials) which have applications in many areas of the economy and which cannot be ignored.

Renewed interest in S&T over the last few years has led to increased Government involvement.

- resulting in some increase in funding
- establishment of special advisory organisations (eg. S&T Councils and Technology Review Committees), which appear to lack interaction
- appointment of Ministers for Technology.

Most countries provide at least a minimum level of support for technology. That is, its development is not left to market forces alone, but is subject to some form of Government intervention. While government support is essential Australia should not attempt simply to copy overseas approaches, but should develop policies to meet its particular requirements (eg Australia's federal structure of Government and raw material exports).

AUSTRALIA'S GENERAL TECHNOLOGICAL POSITION

If gross R&D expenditure figures are examined, the most striking difference between Australia and most other OECD countries is industry's low level of R&D funding and performance. The technological base of much of Australian industry has remained relatively unchanged since the 1950's while other industrialised countries have undergone a revolution.

- industry's expenditure on R&D is very low and falling (Table A)
- Australia is in the middle rank of OECD nations in terms of government expenditure on R&D. However, it falls near the bottom of the table of OECD nations in terms of gross expenditure on R&D. This is due to the very small contribution by industry which spends only 25% of the total R&D budget in contrast to 50-80% in countries such as US, Japan, UK, Italy, France, Germany, Sweden and Switzerland.

The balance of trade in high technology products is also poor (Table B). Of the 24 OECD countries, Australia ranks 22nd on a combination of two indices: the per capita value of high technology exports and the ratio of exports to imports in high technology products.

Recent figures show a further decline in industry involvement in R&D.

- R&D manpower fell by 6% from 1978/79 to 1981/82
- R&D manpower in the electronics industry fell by almost 30% from 1976/77 to 1981/82.

4 With the current concern with unemployment it is important to note that the decline in total employment in R&D given in the Australian trend figures of Table A represents a loss in employment in this area alone between 1973-74 and 1981-82 of approximately 30 000.

This poor performance and continuing decline in R&D investment by industry has been at a time when both levels of government have maintained funding.

Australia's R&D performance is, however, a cause of even greater concern if gross R&D expenditure is broken down into two components, agriculture and industry (including Defence expenditure) and expressed as a percentage of GDP (see Table C). On this basis Australia's performance in agriculture is much higher, relatively, than most other OECD nation and Australia has been well served by this expenditure as its competitiveness in agricultural products attests.

In contrast, Australia's R&D performance in manufacturing industry is very much worse than gross R&D statistics would indicate. In respect of percentage GDP expenditure, Government funding is significantly less than that in the major industrialised countries. Industry funding is low compared to almost all OECD countries.

PHILOSOPHY

Agreement has been reached between the Commonwealth and State Governments that technology is essential to industrial development and restructuring, improved international competitiveness and economic growth. There is also a general belief that, in contrast to agricultural technology, manufacturing technology's contribution to economic growth in Australia has been small compared to other countries.

It is considered important that the Commonwealth and States work together on technology issues to develop advanced technology and to avoid undue social disruption.

- The Commonwealth's predominant role should be the development of the technology infrastructure and broad support mechanisms essential to industrial development.

- The States' predominant role should be the development of tactical programs which match the particular needs and opportunities of their State.

POLICY

An important feature of the present situation in Australia is that Commonwealth and State Governments are developing new policies to promote greater competitiveness and where possible new industries. The methods used to achieve this objective vary among Governments but the end result sought is often similar.

The objectives of these new policies might be summarised as:

- to assist the restructuring and revitalisation of established industry;

- to create new growth industries;
- to provide technology-based support to industry as a means of improving productivity and developing new products.
- to develop community awareness and involvement in the selection and design of technology decisions including issues such as training and education;
- to coordinate these policies with the policies of other Governments.

At the Commonwealth level, there is a recognition that macro-economic, industrial, manpower and education policies all have an influence on efforts to upgrade Australia's technological base.

- there are specific policies to promote the development, diffusion and utilisation of industrial technology.
- recent developments include the desire to encourage specific technologies, proposals to amend the Industrial Research and Development Incentives Act, raising community awareness and increased support for research, particularly applied research.

State Governments are similarly in the process of developing new proposals

- some have announced new initiatives to promote technology (eg. Advanced Technology Corporation and Technology Parks)
- most have foreshadowed plans for many more initiatives in this area. These plans concentrate on helping firms to obtain technology, technical information services, technology transfer, improving interaction between researchers and industry, training and education.

Within an agreed Australia-wide technology strategy, undoubtedly different detailed policies will emerge between Governments

- maximum coordination between the Commonwealth and States is supported but it should be recognised that each State has its own requirements;
- consultation is required among States as well as between the Commonwealth and States.

ORGANISATION

No Government has developed a single, centralised organisation to coordinate S&T, or established a single budgetary channel. All governments seem to rely on various advisory and government agencies to coordinate their efforts.

- the organisational arrangements for co-ordination of technology vary among Governments.
- The Commonwealth Government has well established organisations undertaking R&D, (e.g. CSIRO and DST)

6 State Governments concentrate less on technology development and more on the commercial application of technology. They place considerable emphasis on Small Business Advisory Groups (SBAG). While SBAG's are involved in technology issues, these form a minor part of their total activities.

Social impact issues are also not well coordinated.

- most Governments have some form of advisory body on social impact issues.
- some consultation between the Commonwealth and States takes place through the Labor Ministers' Advisory Conference (DOLAC).

TECHNOLOGY PROGRAMS

Table E provides a summary of assistance measures for industrial technology provided by various governments. However, aggregation of the information in this way fails to provide a perspective on how effective the programs are and whether they are fully in place or developmental. The following discussion is designed to expand on the information in Table D.

Development

The Commonwealth provides general and specific support for technology development.

- general support through taxation measures and the tertiary education.
- specific support through the Industrial Research and Development Incentives Act, programs of the Department of Science and Technology, CSIRO and, to a lesser extent, other government research organisations.

State Governments provide grants for technology development.

Transfer

State Governments are moving towards a much greater involvement in technology transfer and technical information activities.

- some are developing policies to provide better information to industry.
- some resources of tertiary institutions are being directed towards industry problems.
- State Agents-General are already involved in technology transfer activities.

The Commonwealth is involved in technology transfer through

- the Technology Transfer Council which is supported by the Commonwealth and industry
  - it has branches in each State

- the special technology transfer mechanisms established by Government research organisations (e.g. CSIRO).

- a technical information unit which provides US S&T information (NTIS).

- the Patent Information Service.

Industrial Development

State Governments provide a wide range of specific policies such as subsidised buildings and land, market advisory services, Government purchasing and export promotion.

This is a main area of interest to State Governments.

- it is through these policies that they currently provide their major support for technology development.

The Commonwealth relies primarily on broadly based measures to improve the efficiency of industry.

- measures such as investment allowance, depreciation, technology development programs, a strong commitment to export promotion and strengthening of its purchasing policy.

Venture Capital

This appears to be an area of concern to all Governments.

- there is a general belief that there is a shortage of venture capital exists in Australia.

All State Governments provide some form of loan and/or loan guarantee facilities to small business.

- some technology-based firms receive support in this way.

  - however, loan finance is generally unsuitable for new technology-based firms. Equity finance is a preferred route.

The Commonwealth already provides some financial assistance through the Australian Industry Development Corporation and the Commonwealth Development Bank.

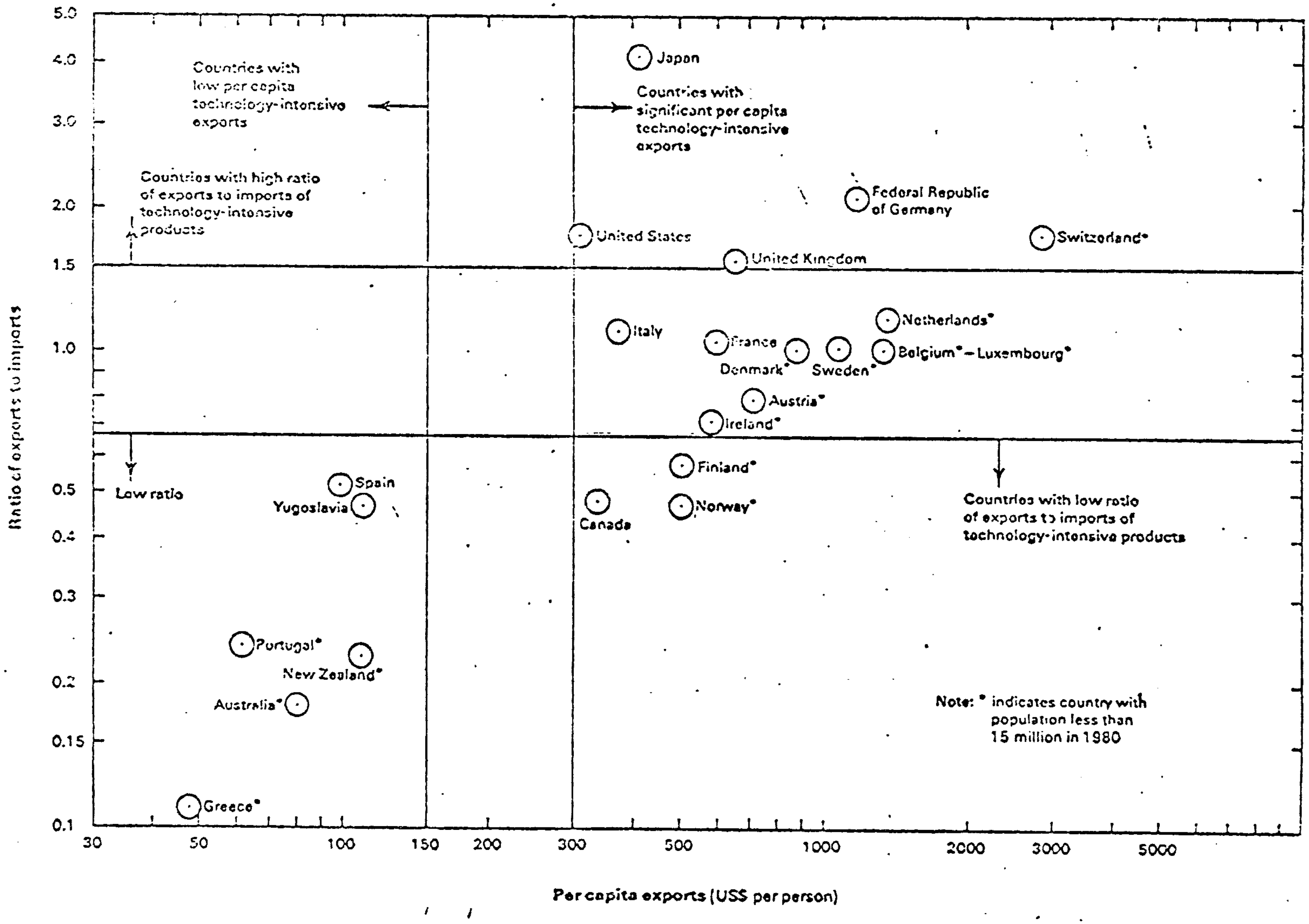
R&D Employment as % Total Workforce and Gross Domestic Expenditure on R&D (GERD) as % GDP (1979)#

Country	R&D Employment as % total workforce	% GERD/ GDP	R&D in Sector* as % GDP				
			Source of Funds*		Performance*		
			Business Enterprise	Government	Business Enterprise	Government	Higher Education
Australian Trends							
Australia (1968-69)	0.82	1.34	0.48	0.79	0.49	0.53	0.32
Australia (1973-74)	0.85	1.26	0.42	0.79	0.42	0.50	0.33
Australia (1976-77)	0.67	1.05	0.24	0.78	0.24	0.50	0.29
Australia (1978-79)	0.63	1.03	0.21	0.79	0.24	0.46	0.31
Australia (1981-82 est)	0.57	1.01	0.21	0.78	0.23	0.47	0.30
Medium R&D Performers							
Switzerland	1.23	2.41	1.81	0.58	1.81	0.14	0.43
Netherlands	1.09	1.99	0.94	0.96	1.03	0.42	0.50
Sweden	0.85	1.88	1.13	0.71	1.32	0.17	0.41
(Group Median, excluding Australia)	0.78	1.40	0.92	0.62	0.98	0.20	0.33
Belgium	0.78	1.40	0.92	0.43	0.98	0.13	0.29
Canada	0.52	1.10	0.40	0.62	0.46	0.30	0.33
AUSTRALIA	0.63	1.03	0.21	0.79	0.24	0.46	0.32
Italy	0.42	0.84	0.57	0.46	0.49	0.20	0.15

\# The international comparisons are based on OECD "Science and Technology Indicators I". The data differ slightly from those presented earlier in the OECD "Science Resources Newsletter".

* In OECD data the Business Enterprise sector includes both private and public business enterprises. The General Government sector includes federal and provincial or State governments and their agencies excluding public business enterprises. "Government" as a source of funds includes the "Own funds" of public universities.

TABLE B Trade per head of population in broad product groups with high average research intensity



Source: Science and Technology Statement 1982-83

Note:
- Turkey has an export/import ratio of 0.04
- There is insufficient information to include Iceland on the chart.

R&D Employment in Manufacturing as % Total Manufacturing Workforce and Sources of Funds for Manufacturing R&D in Business Enterprises as % GDP (1979) - Comparisons with OECD and with Agricultural R&D*

Country	Manufacturing R&D Employment as % total Manufacturing Workforce	R&D as % GDP for funding Sources: Manufacturing R&D in Business Enterprises funded by: Industry	Gov.**	Total	Agric. R&D Total
Australian Trends					
Australia (1968-69)	0.98	0.46	n.a.	n.a.	n.a.
Australia (1973-74)	0.89	0.34	n.a.	n.a.	n.a.
Australia (1976-77)	0.53	0.15	0.08	0.23	n.a.
Australia (1978-79)	0.52	0.14	0.09	0.22	0.18#
Australia (1981-82 est)	0.46	0.12	0.10	0.21	n.a.
Medium R&D Performers					
Switzerland	2.76	n.a.	0.11	n.a.	n.a.
Netherlands	2.51	n.a.	n.a.	n.a.	0.08
Sweden	2.20	1.05	0.20	1.25	0.02
(Group Median excluding Australia)	2.00	0.48	0.09	0.52	0.03
Belgium	1.80	n.a.	n.a.	n.a.	0.03
Canada	0.99	0.31	0.07	0.38	0.09
AUSTRALIA	0.52	0.14	0.09	0.22	0.18#
Italy	0.73	0.48	0.04	0.52	0.02

* These data represent a preliminary analysis by DST (Indicators and Resource Analysis Section) based on OECD material available as at May 1983. While they are broadly indicative and suitable for comparisons where there are large differences, they will be subject to revision. In particular, they are likely to be significantly refined following publication of OECD Science and Technology Indicators I later in 1983.

** Components performed in business enterprises of government support for both industrial growth and defence R&D. The figures do not include aerospace R&D support performed in business enterprises. Including aerospace would raise the figures for USA and UK, among others.

\# About 10% is provided by industry, about 50% by State Governments, and the balance mainly from the Commonwealth.

TABLE E: AUSTRALIAN GOVERNMENTS: TECHNOLOGY-RELATED ASSISTANCE MEASURES

Type of Assistance	COMM	NSW	VIC	QLD	SA	WA	TAS	NT	ACT
TECHNOLOGY DEVELOPMENT									
. Basic Research Support	e		e						
. IR&D Grants	e		e						
. Technology Parks		p			e	e			p
. Government Research Organisations	e	e	e						
. Innovation Centres		e	e			e			
. Assistance to Inventors Scheme	e	e	e			e		e	
. Technology Advisory Centres	e	e	p	p					
. Technology Demonstration Centres	e	e							
. Research Associations	e								
. Selecting Technology Policy									
TECHNOLOGY TRANSFER									
. Technology Transfer Organisations	e	e		p					
. Technical Information Services	e	e	e	e	e	e	e		
. Subsidies for Technology Consultants		e	e		e	e			
INDUSTRIAL DEVELOPMENT									
. Payroll Tax Rebates		e	e	e	e	e	e		e
. Expansion or Establishment Grants or Subsidies					e	e	e		
. Re-location and Removal Assistance		e	e	e	e		e		e
. Feasibility Studies		e	e				e		e
. Market Research Reports	p	e	e	e	e	e	e		
. Purchasing Preference	e	e	e	e	e	e	e		
. Off-sets	e	e	e	p					
. Taxation Concessions	e								
. Advisory Bodies to Government	e	e	e		e	e			
. Industrial Design Council	e	e	e	e	e	e	e		
. Productivity Promotions Council of Australia	e								
FINANCE									
. Loans and Loan Guarantees	e	e	e	e	e	e	e	e	e
. Equity Finance	e		p		p				

Type of Assistance	COMM	NSW	VIC	QLD	SA	WA	TAS	NT	ACT
INTERNATIONAL ISSUES									
. Foreign Investment	e								
. Technology Transfer Through Joint Ventures		e	e	e	e	e	e		
. Export Promotion	e	e	e	e	e	e	e		
. S&T Agreements	e								
. OECD - CSTP	e								
SOCIAL ASPECTS									
. Studies of Particular Technologies	e	e	e		e				
. Consultative Mechanisms	e	e	e	e	e	e	e	e	
. Education and Awareness Raising	p	e	e						
. Monitoring Effects of Tech Change Overseas	e	e	e						
. Government Advisory Bodies on Tech Change (incl Statutory Authorities)	e	e	e		e	e			
. Training Grants and Other Support	e	e	e						

e = existing; p = planned/proposed

Case studies: Technology policies and programs in Australia, Research, Technology and Regional Policy, Workshop, Paris, 24–27 October 1983, OECD, pp. 1–12.

Barry O. Jones

3.12

SUNRISE INDUSTRIES: LEADING THE REVOLUTION

Since the mid-1970s there has been a growing recognition that Australia's economic base has been undergoing a fundamental change — a decline in employment in manufacturing and a growth in employment in services (many increasingly marginal and peripheral), altered patterns in consumer demand generally ('from hardware to software'), the development of technologies which are far less dependent on cheap raw materials and abundant energy and far more on access to high and rising levels of skill. 42

State of Australian Technology

Australia has been passing through a Post-Industrial Revolution which began here about 1965-66. Our decline in manufacturing employment is not a temporary anomaly to be rectified by tariffs, wage freezes, bounties, quotas and other fiscal measures. There will be no return to what we have come to think of as 'normality' i.e. a continuation of the development patterns of the 1950s and 1960s.

The fall in manufacturing employment is a normal part of a development cycle, although Australia's decline has been sharper and more painful than in most other technologically developed countries. Between 1965 and 1982 there were 2,060,000 new jobs created in Australia, of which the contribution of manufacturing was negative, a loss of 150,000 or -7.3%.

In 1965 manufacturing accounted for 27.6% of our labour force. In 1983 the figure is 16.5%. This fall will continue.

Changing patterns of demand in the world economy, e.g. falling requirements for aluminium and other commodities produced by huge amounts of cheap energy, will alter the whole concept of 'comparative advantage' away from physical resources and towards human resources.

In Australia barely 15% of the active population is currently employed in the production of necessities while the remaining 85% work in producing services and commodities which we choose to have to improve the amenity of life but which we could provide for ourselves if we wished, or (like cars, boats, holidays and entertainment) go without or postpone acquiring.

A Paradigm Shift

If employment is falling in manufacturing and new technology is now biting into services generally, how are we to tackle this problem of declining industries and unemployment? First, we need to understand what the problem actually is. Second, we need to radically change our perception of what is going on — a paradigm shift. The word paradigm is taken from the Greek for 'pattern' or 'framework' and the concept of paradigm shifts was proposed in 1962 by the American physicist Thomas S. Kuhn in his book *The Structure of Scientific Revolutions.* For example, the view of the universe accepted as a paradigm for 1500 years was Ptolemy's model. It was based on common place observation: the sun goes around the earth, the earth is flat, forces don't operate at a distance. Copernicus, Galileo and Newton proposed a new paradigm: a clockwork universe in which the sun is central, the earth is round and forces such as gravity do operate at a distance. This change involved a paradigm shift. Anomalies which would not be explained by the old paradigm were explained by the new (and better) paradigm. The shift was not just a change in perspective — it involved a change in value systems as well.

We are undergoing a shift of this type in Australia now. What we are experiencing is not simply an industrial society decline and needing temporary support to restore, but a new type of society based on different economic bases (knowledge and skills rather than raw materials and muscle power) — that is, a post-industrial or 'information' society. The rate and diffusion of technological change is now more rapid than in any time previously. For example, it has been estimated that in medieval times it took 40 years for the invention of the (optical) spectacle to spread across Europe. Today equally major inventions will spread in as many months across the globe. This situation poses a very great threat to our traditional industries. However, considerable benefits can be gained from this situation if we stress the encouragement of new technologies and industries as well as the rapid diffusion of new technologies.

We need the 'shock of recognition' to grasp this paradigm shift — that we have a post-industrial or information society which is operating pretty much as one would expect at an early stage of development but which is capable of dramatic expansion.

Threats and Opportunities

Technological change offers both threats and opportunities. Australia is not well prepared to get the best out of new emerging technologies. The level of research and development expenditure by industry is inadequate, management attitudes are not conducive to technological change and investment in new high technology ventures and participation rates in education are low.

Australia is in the middle rank of OECD nations in terms of government expenditure on R & D. However, it falls near the bottom of the OECD nations in terms of gross expenditure on R & D (GERD) on account of the very small contribution by industry.

In Sweden, funding of R & D expressed as a percentage of GDP **by business** is over five times greater than in Australia while **Government** funding of R & D as a percentage of GDP is slightly higher here than there. It would seem obvious that R & D should be one of the last areas cut back by managers when things get tough but this is not the way Australian managers see it. If these trends continue, our industrial base will be weakened, it will be more difficult for industry to develop new technologies and to successfully adapt overseas technologies to Australian conditions. R & D is an investment in the future in the same way that education is.

Education an Essential Area

Education is an essential area if we are to have the necessary skills to develop new industries. Australia can learn much from the example of Japan and the United States towards education. In Japan, at the age of 17 years, 88.1% of Japanese are still at school compared with only 31.7% of Australians. In the U.S., there are, pro rata, three times as many young people studying science as there are in Australia, and of these, 28% go on to take out PhDs compared with 5% in Australia. On March 2 this year, the U.S. House of Representatives approved a bill that would provide about $US425 million for science and mathematics instruction in the 1984 fiscal year — to rise to $1 billion by the 1988 fiscal year. On a per capita basis for Australia, this is equivalent to an additional investment of about $30 million for science and mathematics instruction alone (*Business Week*, 29 March 1983).

Australian management has shown that it is not very aware of the importance of technology. Previous reliance on tariff protection and liberal foreign investment policies have encouraged a situation where Australian industry manages to live behind a sheltered wall. There is little incentive to adopt or develop new technologies, invest in R & D or new high-technology ventures. We have encouraged an inward-looking manufacturing sector. Heavy concentration of foreign ownership in Australia has contributed to this. Foreign ownership is also one reason why R & D in industry is low — parent companies prefer to carry out their R & D at home rather than in their Australian subsidiaries. We need to devise policies which encourage such firms to contribute more to Australia's technological development.

These problems in management, R & D and education will need to be overcome if we are to hope to benefit from new technological opportunities. Government has a very important role to play in overcoming these problems. However, we still need that 'shock of recognition' — a realisation that we are undergoing a paradigm shift into a new regime.

Unfortunately, there are many who do not recognise this. These include free marketeers and traditional conservatives who insist that Australia's natural advantages lie in the agricultural and mining industries, and that we should let our economic fate be decided by a withdrawal to these areas.

Resource-based Industry Vulnerable

An economy so dependent on resource-based industries is highly vulnerable to economic fluctuations. In the past 25 years there has been a significant shift in the pecking order in per capita national income among industrialised nations. The falls, generally, have been in resource rich nations such as the U.S., Canada, Australia and New Zealand. **The most spectacular rises have been in nations with very limited raw materials and lacking cheap abundant energy — Sweden, Japan, the Netherlands, Denmark, Singapore and the Republic of Korea.**

Australia has a population of 15,000,000, one of the richest natural resource bases of any nation, abundant cheap energy and a strong agricultural base. Its Gross Domestic Product in 1980 was $140 billion (U.S.), or $9.59 billion per 1,000,000 of population.

By comparison the Netherlands has a population of 14,000,000 (93% of Australia's), very limited access to raw materials other than some North Sea oil and gas. However, its GDP (1980) was $169 billion (U.S.), or $12.07 billion per 1,000,000 population — 25.8% higher than Australia.

The case of Sweden is even more striking. It has a population of 8,300,000 (55% of Australia's), rather limited raw materials, and a very short industrial working week of 30.2 hours. Nevertheless, its 1980 GDP of $123 billion amounted to 88% of ours. This figure represented $14.82 billion per 1,000,000 of population — 54.5% higher than Australia. Sweden has penetrated world markets - including Australia — with brand names such as Volvo, Saab, Scania, SKF, LM Ericsson, ASEA and Electrolux. **They sell us $5 worth of finished goods for every $1 in raw materials we sell to them.**

Intellectual Input

The major factor in the rise of resource-poor nations has been the proportion of export earnings directly attributable to intellectual input: invention, research and development, product innovation, design, patents, royalties and copyright. More than 60% of the annual export earnings of Japan, France, Sweden, the Netherlands and Italy depend on 'brain-based' industries, while Australia's figure is less than 5%. The skill-based earnings of our industrial contemporaries will grow. Ours will contract unless we have a fundamental change in economic direction.

A table prepared by my Department for a recent undelivered paper indicated that on two industries, technology-intensive exports per capita and the ratio of exports over imports in high technology products, Australia ranked overall 23rd of 24 nations in OECD. Only Greece was lower.

If we are to create growth, we must avail ourselves of the high growth opportunities of high technology industries. High technology goods are better able to compete in terms of quality, design and sophistication as well as prices. They also diminish the impact of high transport costs. In principle therefore, Australia could offset the traditional disadvantages of a comparatively small, domestic market by taking advantage of global markets and in particular, the lucrative Asian markets.

Adherence to the doctrine of free markets neglects the important role that overseas governments have had in assisting the development of high technology industries. It also neglects the reality that in the area of high technology industry, comparative advantage is not bestowed, rather created. The inaction of the previous Government in this regard, by letting our technological capability decline, has been very damaging and makes our task of implementing our National Recovery and Reconstruction Plan all the more difficult.

Important Role of New Technology

However, in advocating a transition to 'sunrise industries' we must recognise the important role that new technology has to play in improving the products and processes of existing industries. Of course, the gradual reduction of tariff protection would greatly assist industrial restructuring, the diffusion of new technologies and the reorientation of management attitudes. **But we must remember that the new sunrise industries, while offering enormous opportunities for wealth generation will not be significant employers.** The transition to sunrise industries represents one aspect of the Government's approach to restructuring and revitalising Australian industry. Other aspects will be the retention of protection for existing industries as well as the application of new technologies to improve the competitiveness of traditional industries. Technology transfer will have an important role in this but we just make the best use of overseas technologies. Australian industry must not become dependent on new technology but if it is used widely, it can be a very valuable source of productivity improvement. This can, of course, only be successfully done if Australia has a strong scientific and technological infrastructure.

The point is that time is running out for Australia. We cannot rely on our natural resources to save us. Rapid technological change and the nature of new technologies is causing a fundamental change to our industrial structure. Australia **must** be in a position to participate in these changes, and get the best out of them. Failure to do so will cause serious problems.

Before outlining some initiatives which could be taken to improve our prospects of generating sunrise industries, I would like to comment on some of the technological opportunities which these industries offer.

New Technological Opportunities

The ALP Science and Technology Policy identifies sixteen 'sunrise' industries. It could fairly be said that we did not choose these sixteen: essentially they chose themselves. They fall into two categories:

- areas where Australia was at the leading edge of research e.g. in biotechnology, solar energy, industrial ceramics, or custom-designed chips, and where an international market niche seemed likely,

or

- areas where Australia would itself provide a major market e.g. personal computers, communications technologies.

The list comprises:

- biotechnology — in at least nine different major areas
- personal computers
- software writing for computer operations
- VLSI computer chip industry designing custom-made products for specific markets
- scientific instrumentation
- medical technologies
- lasers
- communications technologies
- industrial ceramics, especially partially stabilised zirconia (PSZ)
- solar energy cells, collectors and other equipment
- development of shape-memory alloys, increasingly important in high precision engineering, microsurgery and space research
- plasma-physics research (including fusion)
- robots
- intermediate technology projects
- hydrogen generation and storage
- biomass

Biotechnology the Leader

Biotechnology — genetic engineering — seems certain to be the 'lead technology' of the 1980s. It involves the application of biological organisms, systems or processes — for example splicing genes from 'recombinant DNA' into plasmids from specific bacteria which are then able to replicate to 'manufacture' the desired substance. Biotechnology has a vast potential, e.g. leaching minerals from ores; turning waste materials into animal food; finding new forms of usable energy (biomass); making new plant strains capable of resisting salt, using less water and fixing nitrogen; low energy techniques in industrial processes; producing new medical technologies (e.g. making interferon for use in cancer treatment) or old therapeutic substances in new ways (e.g. manufacturing human insulin, instead of extracting it from animals). **Of all the 'sunrise' industries, biotechnology has the greatest potential for wealth creation and here Australia has**

great comparative advantages in skills — a strong agricultural base, expertise in plant and animal genetics, microbiology, biochemistry and medical research.

At a recent meeting Cabinet adopted a National Biotechnology Programme, following a recommendation by the Australian Science and Technology Council (ASTEC) in November 1982. The Programme will bridge the gap between research in the laboratory and the implementation of new discoveries in new industrial applications, in mining, agriculture, chemicals, food processing and medicine. We must move quickly to end the brain-and-ideas-drain from our universities and colleges of advanced education.

The precise level of funding will have to be determined in a Budget context.

A number of computer-related areas, for example, computer applications technology such as CAD/CAM, software production, custom-design chips and personal computers, also have excellent prospects. These technologies will be essential if we are to participate in the emerging information society. If we cannot do this, then we will be committing our economy and society to a purely peripheral role with all its dire consequences. Existing industries such as steel and motor manufacturing will continue to decline as employers and we must have new industries to develop, not as replacements, but to be complementary.

Allocation of Resources

As the identification of sunrise industries becomes more refined we will have to make well-informed decisions about where our resources will be allocated. As I have said, we put emphasis on high potential sectors where we have a comparative and competitive technological advantage or where we have an established or emerging technological capability. While I recognise that productivity gains can be substantial from imported technology, technological self-reliance provides the basis for innovative success and the efficient use of imported technology. The important point to remember is that if we are to return to a sound rate of economic growth, we will need to develop and exploit high yield areas of activity. Sunrise industries hold out this prospect. The important immediate task will be to determine the crucial role that government has in encouraging not only sunrise industries but also improving our technological base in general. It must not be forgotten that Government will also need to ensure that the adverse consequences of rapid technological change are evenly shared and minimised.

Funding Sunrise Industries

In the past twelve months there has been a growing consciousness of the economic and political problems Australia faces in the high technology area. The question of providing venture capital for new high technology firms and inducements for existing firms to invest more heavily in R & D — or in some cases for firms to **begin** investing — has attracted more interest in the past twelve months than ever before.

In June 1980 the Australian Science and Technology Council (ASTEC) produced a report which recommended that expenditure specifically earmarked for R & D should attract tax deductibility of 125%, that is a 25 cent premium on every dollar spent by existing or new entities. The report was ignored by the Fraser Government.

In February 1983, the Austalian Scientific Industry Association (ASIA) published a report *High and New Technology: Needs of Australian Industry* which went further than ASTEC, recommending 150% deductibility for R & D expenditure.

Neither ASTEC or ASIA specifically addressed the question of the provision of venture capital.

The Espie Report

In April 1983 the Australian Academy of Technological Sciences produced a report commissioned by my predecessor as Minister for Science and Technology, the Hon David Thomson, entitled *Developing High Technology Enterprises for Australia.* This report was prepared by a ten man High Technology Financing Committee chaired by Sir Frank Espie.

The Espie Report recommends 100% tax deductibility for new high technology enterprises and proposes a monitoring system which will preserve elements of free market choice and flexibility while ensuring that funds are not dissipated ineffectively through hundreds, if not thousands, of 'no hoper' companies.

The Espie Report proposes setting up a Licensing Board which will approve the setting up of a number of Growth Business Investment and Management Companies (GBIMC). The name is a bit of a mouthful but we will learn to live with it. Full tax deduction would be received for equity capital paid to a GBIMC and the maximum amount to be approved by the Licensing Board would be set by the Budget each year. GBIMC licences would be issued to applicants offering technological expertise, finance and management ability, appropriate investment strategy and capital backing. Each GBIMC would invest in a portfolio of eligible businesses, thus spreading the risk.

Other Proposals for Urgent Consideration

Other proposals which must be considered urgently in a Budget context are possible amendments of Division Seven of the Income Tax Act, which would give private companies power to reinvest profits instead of being compelled to distribute them, and a tax credits scheme which would provide rich rewards for successful enterprises while retaining the traditional penalties for failure.

The Government will, I expect, be in a position to announce specific measures early in the budget session. There is a need for speedy determination because a prospective high technology investor with $1 million at his disposal is not going to rush into investing in June if by September his investment will be tax deductible. This combination of great expectations and temporary uncertainty has created an unavoidable and unfortunate hiatus in the investment market.

In addition, proposed amendments to the Australian Industries Development Corporation (AIDC) and the Australian Industrial Research and Development Incentives Board (AIRDIB) would give government a greater international role in identifying and assisting promising 'sunrise' enterprises.

No one approach will occupy the whole field. I expect that the Government will adopt a 'menu' approach, to use the latest buzz word. Different items on the menu will be offered for hors d'oeuvres, main course and afters. Inventors and enterprises will be able to choose the items which have the most appeal with the Espie recommendations standing out as the 'roast of the day'.

The Government's Role

The Government recognises that, as a matter of urgency, Australia must develop new, high technology 'sunrise' industries as wealth generators, and to compensate for the long-term decline in employment in our traditional manufacturing industries.

In encouraging sunrise industries, and in ensuring that new technology is applied in traditional industries, we will have to do more than just ensure that the economic climate is right. Even if we are to remain competitive in areas such as primary production and mining we will need to apply new technologies in these areas in order to do things better and more efficiently. Problems facing Australian industry are long-term in nature and effective government intervention will be required.

The Report of the Working Group on Technology, Growth and Employment which was set up following the Versailles Summit Conference in 1982 recommended that:

> "Special attention should be paid to the rejuvenation of mature industries through the use of science and technology".

I believe we cannot neglect this advice even considering the important growth opportunities offered by sunrise industries.

An example of this comes from the Kawasaki Steel Corporation in Japan. The Corporation stands to make savings of hundreds of millions of dollars through the introduction of a new process in iron-making from fine ores. In full commercial use, this new process could cut operating costs of the entire Japanese iron-making sector by 15 per cent. (*Inside R & D*, March 16, 1983).

Importance of Technological Base

As a first step, it will be important to build-up our technological base. Key areas which need attention are research, scientific and technical manpower, the commercialisation of research and that multinational corporations should act in a way which enhances Australia's technological strengths. Technology transfer is another area where intervention is needed to ensure that Australia gets the best deal.

The transition to sunrise industries, and I stress the word transition, will take time. The extent of the problem we are facing here can be gauged from the following example. In 1980 Australia exported US$81 per capita of goods with a high research component. This makes a startling comparison with Switzerland, for which the figure is US$2584, the Netherlands (US$1378), Belgium (US$1352) and overall OECD countries (US$468 per capita). We set a goal of increasing our exports in these goods to the level over all OECD countries, we would have to increase our export performance by about six fold. That is from about US$1.2 billion to about US$6 billion. The task looks difficult if we consider that in 1980-81, Australia ran a trade deficit of some A$6billion in goods, with a high research component.

The Government has a commitment to retaining programmes of protection in the current economic climate. Industry policy needs to strike a balance between easing the adjustment problems of our traditional industries and the encouragement of new technology transfer.

Selectivity in Research Support

My Department has mentioned the need for greater selectivity in the support of research. This concept is very much like the need to encourage sunrise industries. In fact, it forms a very fundamental and integral part of encouraging sunrise industries. We cannot spread our scientific and technological resources too thinly. We need to concentrate these resources in the most promising areas. Of course, I am thinking here of technologies which are fundamental to a number of sunrise industries, microelectronics and biotechnology are examples. But in doing this, we must recognise that we cannot duplicate all the initiatives underway worldwide.There must be realistic choices made of what products can be successfully made in Australia. One of the best ways to do this is to give encouragement to Australian industry to seek out market niches in high technology products. Industry will probably be the best judge of new opportunities but I don't think this should prevent the Government from intervening in the market should this be in the national interest.

In laying this foundation for new sunrise industries, it will be important not to repeat mistakes of the past where we concentrated too many resources within the government sector. Greater emphasis will have to be placed on encouraging industry to develop, commercialise and market the results of successful research.

Incentives

The Government, for its part, can assist in many ways. One of the most significant is by making appropriate and effective incentives available. Inappropriate incentives can be very damaging and it is also likely the influence of incentives will be reduced if the general economic environment is not in their favour. In particular, the behaviour of multinational corporations and the effective use of overseas technology by Australian firms will be a necessary and essential part of getting this environment right.

It is not sufficient to concentrate only on technology. It is vitally important to get the commercial aspects right as well.

Just recently I announced the formation of a new company to commercially develop Australian biomedical inventions for the world market. This company, Australian Biomedical Corporation Ltd., is a joint venture between Wormald International and the Australian Industry Development Corporation (AIDC).

The overall objective of the Biomedical Corporation is to identify inventions and innovations which have potential, then to assist with their development and marketing on the domestic and international markets.

Conclusion

As I said in my National Science Forum Speech on 5 April, I have often drawn attention to the close parallels between encouraging the growth of 'sunrise' industries and the way in which we revive the Australian feature film industry in the early 1970s. The challenges, both external and domestic, were analogous and I believe that the Australian Industrial Research and Development Incentives Board will play a similar role to that played by the Australian Film Development Corporation between 1970 and 1975. There are five major priorities:

- Identifying areas of strength and weakness in our research base and looking for niches in the international market.
- Raising levels of awareness of the importance of high technology as a factor in economic growth — 'talking up' high technology.
- Creating a climate of opinion in which politicians, bureaucrats, the media and the community generally, accept that the risks of high technology investment are worth taking.
- Stimulating investor confidence by providing competent risk assessment and bringing appropriate people into a skill network, as MITI has had in Japan and as the AFDC did in films. But I would be opposed to having this done by some large bureaucratic monster.
- Provision of appropriate financial incentives for investors.

Finally, **I want to provide a reassuring note for people who are involved in traditional industry,** some of whom have felt somewhat threatened by the recent media emphasis on 'sunrise' industries.

"What about us?" they say. "Will the sun go down for us forever as we approach the 'brave new world' of the sunrise industries". The use of the new technologies, with their forward and backward linkages, can revolutionise much existing industry.

The technology changes with bewildering speed. Social attitudes, however do not. The fundamental conservatism of Australian management and Australian investors is a major obstacle to change. To paraphrase the Duke of Wellington: "I have seen the enemy — and it is us". You can lead an Australian manager to the technology, but you can't make him adopt it. As Professor Ken Taylor of the Department of Physics at the University of New South Wales told me, sadly, he has shown new laser applications to many Australian managers, showing them how their firms can use new technology to improve the quality and range of products at lower cost. They look at what lasers can do, express amazement and say: "That would go down very well in Sweden or the U.S. — but in Australia? It just doesn't seem to fit in here".

This is the defeatist attitude that we have to overcome in Australia and I take it that by your presence here today you are prepared to join me in leading the revolution.

ABOUT THE AUTHOR

The Hon. Barry O. Jones MP, Minister of Science and Technology, Member for Lalor, has been a Labor Member of the House of Representatives since 1977. Formerly a public servant, high school teacher, university lecturer, and lawyer, he has worked extensively on radio and television. As a Member of the Victorian Parliament 1972-77 he served as Shadow Minister for Social Welfare, Aboriginal Affairs, Transport and the Arts. He was Deputy Chairman of the Australian Council for the Arts 1969-73. He took a leading role in reviving the Australian film industry as a member of the Australian Film Development Corporation 1970-75, foundation Chairman of the Australian Film and Television School 1973-75 and Chairman of the Australian Film Institute 1974-80. He has been active in penal reform and as a successful campaigner against the death penalty. His interests include films, music, travel, collectng autographed documents, antique terracottas and paintings and reading.
His best seller *Sleepers Wakes!: Technology and the Future of Work* was published by Oxford Univesity Press in 1982; A Japanese and Swedish edition will appear in 1983. He has written five other books.
He became Minister for Science and Technology in March 1983.

B. O. Jones, 'Sunrise industries: leading the revolution', *Human Resource Management Australia*, vol. 21, no. 3, 1983, pp. 42–7.

Stuart Macdonald

3.13

REVIEW OF *DEVELOPING HIGH TECHNOLOGY ENTERPRISES FOR AUSTRALIA (ESPIE REPORT)*

Developing High Technology Enterprises for Australia (Espie Report) *by the Australian Academy of Technological Sciences* (Australian Academy of Technological Sciences, Canberra, 1983) pp. ix + 132, $10.00. 389

Just three years ago was published the Myers Report, *Technological Change in Australia*, a set of four sizeable volumes displaying such striking incompetence that they have had no impact whatsoever on the process of technological change in Australia. In contrast, *Developing High Technology Industries for Australia*, the Espie Report, is a slim volume and may well have a profound effect on Australia's technological future. This is not to say that Espie has all the answers — the Report candidly admits that so much is wrong in Australia that a universal solution is impossible — but Espie has at least come to grips with one issue. Everything was beyond the grasp of Myers.

The Espie Report deals with venture capital, the absence of which is seen by many to have stunted the development of high technology in Australia. It recommends the formation of new venture capital companies, called Growth Business Investment and Management Companies (with the unpronounceable acronym GBIMC), licensed by government and affording their investors 100 per cent tax deduction on investments. These venture capital companies are to select a portfolio of promising high technology companies, are to take equity in these companies and play an active part in their management. The recommendations of the Espie Report have been warmly welcomed by those interested in the development of high technology in Australia as an obvious first step in that development, so obvious, in fact, that one wonders why a prestigious committee had to deliberate for two years before they could be suggested. The answer probably has little to do with high technology *per se* and more to do with the threat it poses as a claimant in the redistribution of resources that will accompany any restructuring of the economy. Despite resolute opposition from some government departments, Cabinet has, at the time of writing, accepted the Espie recommendations in principle, and the Minister for Science and Technology has promised an announcement accepting the bulk of the proposals in September.

While the conclusions of the Espie Report are fuel to warm the hearts of those anxious to stimulate change in the Australian economy, the means by which the Report reaches those conclusions will chill the soul of those with an old-fashioned attachment to intellectual rigour. The Report was prematurely delivered — in draft form — just before the last election to a government suddenly desperate to show an interest in high technology, and its ultimate birth is hardly likely to have been assisted by that earlier, hasty delivery. But even the Report's conception is shrouded in mystery: the Academy was apparently invited to examine "the issues" by the previous Minister for Science and Technology and there are indications that his Department executed some of the survey work, but no terms of reference are presented and whatever there might have been were apparently broadened during the investigation.

The Academy started its work from the premise that there is nothing wrong with Australian invention or technology; innovation, however, is weak and the problem lies in financial and managerial inadequacies (pp. 41-2). Some support of that stance would have been welcome, as would further investigation of just why it is that community attitudes are not conducive to technological change in Australia (p. 43). Without presenting a shred of evidence, the Report declares that government technology policy has "certainly" been successful (p. 26), but that more policy is

390 needed. Indeed, the whole Report is woefully lacking in anything smacking of evidence, and is at its strongest and most honest when it simply reproduces opinions. This reviewer can cope with the regurgitated, and apparently unpublished, convictions of Stanley Pratt, President of the Capital Publishing Corporation, and of Norman Fast, MBA (New York) PhD (Harvard). He can even tolerate

> "A US venture capitalist stated to the Committee that . . . The Committee wishes to emphasise the importance of these findings . . ." (p. 24)

without so much as a name being given in evidence. But he will not stomach the detailed analysis of studies by American chartered accountants (p. 25) or the University of New Hampshire (p. 108) without adequate citation to permit verification. Such a serious failing in the Report can mean only that the Academy has been inept or has sought to deceive.

Much more serious is a conceptual problem. Although the Academy is considering the problem of venture capital for high technology enterprises in Australia, it is clearly thinking mainly — and often only — of those in manufacturing industry. This is a Report "to develop a more competitive and forward looking manufacturing industry" (p. iii), concerned with "the decline in Australian manufacturing" (p. 26) and recommending incentives for enterprises which "are mainly engaged in manufacturing" (p. 73). The engineers of the Academy have a strange notion of high technology limited largely to the manufacturing sector, especially strange as the survey of 255
391 Australian high technology firms carried out for the Espie Committee showed by far the largest group to be in the computer software industry (p. 93).

The survey itself provides more grounds for concern. It is mandatory upon those responsible for such a Report to provide some survey methodology: this Report leaves the reader foundering. Details of the survey appear in a lengthy Attachment 2. No mention is made of "the 18 surviving enterprises examined in detail by the Committee" (p. 26) from which examination unfettered generalisations are made in the text. Have these 18 anything in common with the "21 high technology case studies" (pp. 70, 84) mentioned, in passing, elsewhere in the text? The whole sample of 255 had apparently experienced a 12 per cent growth in employment between June 1981 and June 1982. Although no figures are presented for previus years, it is admitted that "the employment growth record could only be described as moderate" (p. 32), which suggests a rather selective use of figures. The Report is determined to show that high technology enterprises create lots of jobs (at all skill levels) and in its enthusiasm falls into the trap, already occupied by several American studies, of comparing employment generation in all established firms with that in only surviving high technology firms. Inevitably, the conclusion is that "nearly all the net new jobs in the private sector are created by young, high growth companies" (p. 24).

There are also some oddities in the detail of the report's recommendations. New Australian high technology companies are not only to use high technology, but will preferably develop it in-house (p. 67). New venture capital companies are not to be really new at all, but rather specialised offshoots of existing financial institutions (pp. 56, 75). Nor does the Report really consider how its recommendations might fit in with other developments in the conservative confusion of technology and industry policy in Australia — the Campbell Report, for example, is not mentioned until page 40. What, one wonders, will be the impact of tax concessions for venture capital companies on the most common sources of start up capital for high technology entrepreneurs — friends, family and acquaintances (p. 126)? Time will tell, and in a very short time the amateurish scribblings of the Espie Committee will be forgot. Its recommendations seem destined to leave a more enduring mark, but they should be seen for what they are — good ideas derived from instinct and experience, not the result of research and logic.

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S. Macdonald, 'Review of *Developing High Technology Enterprises for Australia (Espie Report)*, by Australian Academy of Technological Sciences', Prometheus, vol. 1, no. 2, 1983, pp. 389–91.

4

KNOWING IN CULTURE

Gregory Bateson 4.1

THE NATURE OF SCIENCE

Science, like art, religion, commerce, warfare, and even sleep, is based on *presuppositions*. It differs, however from most other branches of human activity in that not only are the pathways of scientific thought determined by the presuppositions of the scientists but their goals are the testing and revision of old presuppositions and the creation of new. 25

In this latter activity, it is clearly desirable (but not absolutely necessary) for the scientist to know consciously and be able to state his own presuppositions. It is also convenient and necessary for scientific judgment to know the presuppositions of colleagues working in the same field. Above all, it is necessary for the reader of scientific matter to know the presuppositions of the writer.

I have taught various branches of behavioral biology and cultural 26
anthropology to American students, ranging from college freshmen to psychiatric residents, in various schools and teaching hospitals, and I have encountered a very strange gap in their thinking that springs from a lack of certain *tools* of thought. This lack is rather equally distributed at all levels of education, among students of both sexes and among humanists as well as scientists. Specifically, it is lack of knowledge of the presuppositions not only of science but also of everyday life.

This gap is, strangely, less conspicuous in two groups of students that might have been expected to contrast strongly with each other: the Catholics and the Marxists. Both groups have thought about or have been told a little about the last 2,500 years of human thought, and both groups have some recognition of the importance of philosophic, scientific, and epistemological presuppositions. Both groups are difficult to teach because they attach such great importance to "right" premises and presuppositions that heresy becomes for them a threat of excommunication. Naturally, anybody who feels heresy to be a danger will devote some care to being conscious of his or her own presuppositions and will develop a sort of connoisseurship in these matters.

Those who lack all idea that it is possible to be wrong can learn nothing except know-how.

The subject matter of this book is notably close to the core of religion and to the core of scientific orthodoxy. The presuppositions—and most students need some instruction in what a presupposition looks like—are matters to be brought out into the open.

There is, however, another difficulty, almost peculiar to the American scene. Americans are, no doubt, as rigid in their presuppositions as any other people (and as rigid in these matters as the writer of this book), but they have a strange response to any articulate statement of presupposition. Such statement is commonly assumed to be hostile or mocking or—and this is the most serious—is heard to be *authoritarian.*

It thus happens that in this land founded for the freedom of religion, the teaching of religion is outlawed in the state educational system. Members of weakly religious families get, of course, no religious training from any source outside the family.

Consequently, to make any statement of premise or presupposition in a formal and articulate way is to challenge the rather subtle resis-
27 tance, not of contradiction, because the hearers do not know the contradictory premises nor how to state them, but of the cultivated deafness that children use to keep out the pronouncements of parents, teachers, and religious authorities.

Be all that as it may, I believe in the importance of scientific presuppositions, in the notion that there are better and worse ways of constructing scientific theories, and in insisting on the articulate statement of presuppositions so that they may be improved.

Therefore, this chapter is devoted to a list of presuppositions, some familiar, some strange to readers whose thinking has been protected from the harsh notion that some propositions are simply wrong. Some tools of thought are so blunt that they are almost useless; others are so sharp that they are dangerous. But the wise man will have the use of both kinds.

It is worthwhile to attempt a tentative recognition of certain basic presuppositions which all *minds* must share or, conversely, to define mind by listing a number of such basic communicational characteristics.

1. SCIENCE NEVER PROVES ANYTHING

Science sometimes *improves* hypotheses and sometimes *disproves* them. But *proof* would be another matter and perhaps never occurs except in the realms of totally abstract tautology. We can sometimes say that *if* such and such abstract suppositions or postulates are given, *then* such and such must follow absolutely. But the truth about what can be *perceived* or arrived at by induction from perception is something else again.

Let us say that truth would mean a precise correspondence between our description and what we describe or between our total network of abstractions and deductions and some total understanding of the outside world. Truth in this sense is not obtainable. And even if we ignore the barriers of coding, the circumstance that our description will be in words or figures or pictures but that what we describe is going to be in flesh and blood and action—even disregarding that hurdle of translation, we shall never be able to claim final knowledge of anything whatsoever.

28 A conventional way of arguing this matter is somewhat as follows: Let us say that I offer you a series—perhaps of numbers, perhaps of other indications—and that I provide the presupposition that the series

is ordered. For the sake of simplicity, let it be a series of numbers:

2, 4, 6, 8, 10, 12

Then I ask you, "What is the next number in this series?" You will probably say, "14."

But if you do, I will say, "Oh, no. The next number is 27." In other words, the generalization to which you jumped from the data given in the first instance—that the series was the series of even numbers—was proved to be wrong or only approximate by the next event.

Let us pursue the matter further. Let me continue my statement by creating a series as follows:

2, 4, 6, 8, 10, 12, 27, 2, 4, 6, 8, 10, 12, 27, 2, 4, 6, 8, 10, 12, 27 . . .

Now if I ask you to guess the next number, you will probably say, "2." After all, you have been given three repetitions of the sequence from 2 to 27; and if you are a good scientist, you will be influenced by the presupposition called *Occam's razor*, or the *rule of parsimony:* that is, a preference for the simplest assumptions that will fit the facts. On the basis of simplicity, you will make the next prediction. But those facts—what are they? They are not, after all, available to you beyond the end of the (possibly incomplete) sequence that has been given.

You *assume* that you can predict, and indeed I suggested this presupposition to you. But the only basis you have is your (trained) preference for the simpler answer and your trust that my challenge indeed meant that the sequence was incomplete and ordered.

Unfortunately (or perhaps fortunately), it is so that the next fact is never available. All you have is the hope of simplicity, and the next fact may always drive you to the next level of complexity.

Or let us say that for any sequence of numbers I can offer, there will always be a few ways of describing that sequence which will be simple, but there will be an *infinite* number of alternative ways not limited by the criterion of simplicity.

Suppose the numbers are represented by letters:

x, w, p, n

and so on. Such letters could stand for any numbers whatsoever, even fractions. I have only to repeat the series three or four times in some verbal or visual or other sensory form, even in the forms of pain or kinesthesia, and you will begin to perceive pattern in what I offer you. It will become in your mind—and in mine—a theme, and it will have aesthetic value. To that extent, it will be familiar and understandable.

But the pattern may be changed or broken by addition, by repetition, by anything that will force you to a new perception of it, and these changes can never be predicted with absolute certainty because they have not yet happened.

We do not know enough about how the present will lead into the future. We shall never be able to say, "Ha! My perception, my accounting for that series, will indeed cover its next and future components," or "Next time I meet with these phenomena, I shall be able to predict their total course."

Prediction can never be absolutely valid and therefore science can never *prove* some generalization or even *test* a single descriptive statement and in that way arrive at final truth.

There are other ways of arguing this impossibility. The argument of this book—which again, surely, can only convince you insofar as what I say fits with what you know and which may be collapsed or totally changed in a few years—presupposes that science is a *way of perceiving* and making what we may call "sense" of our percepts. But perception operates only upon difference. All receipt of information is necessarily the receipt of news of *difference,* and all perception of difference is limited by threshold. Differences that are too slight or too slowly presented are not perceivable. They are not food for perception.

It follows that what we, as scientists, can perceive is always limited by threshold. That is, what is subliminal will not be grist for our mill. Knowledge at any given moment will be a function of the thresholds of our available means of perception. The invention of the micro-

scope or the telescope or of means of measuring time to the fraction of a nanosecond or weighing quantities of matter to millionths of a gram—all such improved devices of perception will disclose what was utterly unpredictable from the levels of perception that we could achieve before that discovery.

Not only can we not predict into the next instant of the future, but, more profoundly, we cannot predict into the next dimension of the microscopic, the astronomically distant, or the geologically ancient. As a method of perception—and that is all science can claim to be—science, like all other methods of perception, is limited in its ability to collect the outward and visible signs of whatever may be truth.

Science *probes;* it does not prove.

2. THE MAP IS NOT THE TERRITORY, AND THE NAME IS NOT THE THING NAMED

This principle, made famous by Alfred Korzybski, strikes at many levels. It reminds us in a general way that when we think of coconuts or pigs, there are no coconuts or pigs in the brain. But in a more abstract way, Korzybski's statement asserts that in all thought or perception or communication about perception, there is a transformation, a coding, between the report and the thing reported, the *Ding an sich.* Above all, the relation between the report and that mysterious thing reported tends to have the nature of a *classification,* an assignment of the thing to a class. Naming is always classifying, and mapping is essentially the same as naming.

Korzybski was, on the whole, speaking as a philosopher, attempting to persuade people to discipline their manner of thinking. But he could not win. When we come to apply his dictum to the natural history of human mental process, the matter is not quite so simple. The distinction between the name and the thing named or the map and the territory is perhaps really made only by the dominant hemisphere of the brain. The symbolic and affective hemisphere, normally on the right-

hand side, is probably unable to distinguish name from thing named. It is certainly not concerned with this sort of distinction. It therefore happens that certain nonrational types of behavior are necessarily present in human life. We do, in fact, have two hemispheres; and we cannot get
away from that fact. Each hemisphere does, in fact, operate somewhat 31
differently from the other, and we cannot get away from the tangles that that difference proposes.

For example, with the dominant hemisphere, we can regard such a thing as a flag as a sort of name of the country or organization that it represents. But the right hemisphere does not draw this distinction and regards the flag as sacramentally identical with what it represents. So "Old Glory" is the United States. If somebody steps on it, the response may be rage. And this rage will not be diminished by an explanation of map-territory relations. (After all, the man who tramples the flag is equally identifying it with that for which it stands.) There will always and necessarily be a large number of situations in which the response is not guided by the logical distinction between the name and the thing named.

3. THERE IS NO OBJECTIVE EXPERIENCE

All experience is subjective. This is only a simple corollary of a point made in section 4: that our brains make the images that we think we "perceive."

It is significant that all perception—all conscious perception—has image characteristics. A pain is localized somewhere. It has a beginning and an end and a location and stands out against a background. These are the elementary components of an image. When somebody steps on my toe, what I experience is, not his stepping on my toe, but my *image* of his stepping on my toe reconstructed from neural reports reaching my brain somewhat after his foot has landed on mine. Experience of the exterior is always mediated by particular sense organs and neural pathways. To that extent, objects are my creation, and my experience of them is subjective, not objective.

It is, however, not a trivial assertion to note that very few persons, at least in occidental culture, doubt the objectivity of such sense data as pain or their visual images of the external world. Our civilization is deeply based on this illusion.

4. THE PROCESSES OF IMAGE FORMATION ARE UNCONSCIOUS 32

This generalization seems to be true of everything that happens between my sometimes conscious action of directing a sense organ at some source of information and my conscious action of deriving information from an image that "I" seem to see, hear, feel, taste, or smell. Even a pain is surely a created image.

No doubt men and donkeys and dogs are all conscious of listening and even of cocking their ears in the direction of sound. As for sight, something moving in the periphery of my visual field will call "attention" (whatever that means) so that I shift my eyes and even my head to look at it. This is often a conscious act, but it is sometimes so nearly automatic that it goes unnoticed. Often I am conscious of turning my head but unaware of the peripheral sighting that caused me to turn. The peripheral retina receives a lot of information that remains outside consciousness—possibly but not certainly in image form.

The *processes* of perception are inaccessible; only the *products* are conscious and, of course, it is the products that are necessary. The two general facts—first, that I am unconscious of the process of making the images which I consciously see and, second, that in these unconscious processes, I use a whole range of presuppositions which become built into the finished image—are, for me, the beginning of empirical epistemology.

Of course, we all know that the images which we "see" are indeed manufactured by the brain or mind. But to know this in an intellectual sense is very different from realizing that it is truly so. This aspect of the matter came forcibly to my attention some thirty years ago in New York, where Adalbert Ames, Jr., was demonstrating his experiments on how we endow our visual images with depth. Ames was an ophthalmologist who had worked with patients who suffered from anisoconia; that is, they formed images of different sizes in the two eyes. This led him to study the subjective components of the perception of depth. Because this matter is important and provides the very basis of empirical or experimental epistemology, I will narrate my encounter with the Ames experiments in some detail.

Ames had the experiments set up in a large, empty apartment in
33 New York City. There were, as I recall, some fifty experiments. When I arrived to see the show, I was the only visitor. Ames greeted me and suggested that I start at the beginning of the sequence of demonstrations while he went back to work for awhile in a small room furnished as an office. Otherwise, the apartment contained no furniture except for two folding deck chairs.

I went from one experiment to the next. Each contained some sort of optical illusion affecting the perception of depth. The thesis of the whole series was that we use five main clues to guide us in creating the appearance of depth in the images that we create as we look out through our eyes at the world.

The first of these clues is size;* that is, the size of the physical image on the retina. Of course, we cannot *see* this image so it would be more exact to say that the first clue to distance is the angle which the object subtends at the eye. But indeed this angle is also not visible. The clue to distance which is reported on the optic nerve is perhaps *change in angle subtended.*† The demonstration of this truth was a pair of balloons in a dark area. The balloons themselves were equally illuminated, but their air could be passed from one balloon into the other. The balloons themselves did not move, but as one grew and the other shrank, it appeared to the observer that the one which grew, approached, and the one which shrank, retreated. As the air was shifted from one balloon to the other and back again, the balloons appeared to move alternately forward and back.

The second clue was contrast in brightness. To demonstrate this, the balloons stayed the same size and, of course, did not really move. Only the illumination changed, shining first on one balloon and then on the other. This alternation of illumination, like the alternation in size, gave the balloons the appearance of approaching and retreating in turn as the light fell first on one and then on the other.

Then the sequence of experiments showed that these two clues, size and brightness, could be played against each other to give a contradiction. The shrinking balloon now always got the more light. This

* More precisely, I should have written: "The first of these clues is *contrast* in size . . ."

† I observe not only that the processes of visual perception are inaccessible to consciousness but also that it is impossible to construct in words any acceptable description of what must happen in the simplest act of seeing. For that which is not conscious, the language provides no means of expression.

combined experiment introduced the idea that some clues are dominant 34
over others.

The total sequence of clues demonstrated that day included size, brightness, overlap, binocular parallax, and parallax created by movements of the head. Of these, the most strongly dominant was parallax by head motion.

After looking at twenty or thirty such demonstrations, I was ready to take a break and went to sit in one of the folding deck chairs. It collapsed under me. Hearing the noise, Ames came out to check that all was well. He then stayed with me and demonstrated the two following experiments.

The first dealt with parallax (see Glossary). On a table perhaps five feet long, there were two objects: a pack of Lucky Strike cigarettes, supported on a slender spike some inches from the surface of the table and a book of paper matches, similarly raised on a spike, at the far end of the table.

Ames had me stand at the near end of the table and describe what I saw; that is, the location of the two objects and how big they seemed to be. (In Ames's experiments, you are always made to observe the truth before being subjected to the illusions.)

Ames then pointed out to me that there was a wooden plank with a plain round hole in it set upright at the edge of the table at my end so that I could look through the hole down the length of the table. He had me look through this hole and tell him what I saw. Of course, the two objects still appeared to be where I knew them to be and to be of their familiar sizes.

Looking through the hole in the plank, I had lost the crow's-eye view of the table and was reduced to the use of a single eye. But Ames suggested that I could get parallax on the objects by sliding the plank sideways.

As I moved my eye sideways with the plank, the image changed totally—as if by magic. The Lucky Strike pack was suddenly at the far end of the table and appeared to be about twice as tall and twice as wide as a normal pack of cigarettes. Even the surface of the paper of which the pack was made had changed in texture. Its small irregularities were now seemingly larger. The book of matches, on the other hand, suddenly ap-
peared to be of dollhouse size and to be located halfway down the length 35
of the table in the position where the pack of cigarettes had formerly been seen to be.

What had happened?

The answer was simple. Under the table, where I could not see them, there were two levers or rods that moved the two objects sideways as I moved the plank. In normal parallax, as we all know, when we look out from a moving train, the objects close to us appear to be left behind fast; the cows beside the railroad track do not stay to be observed. The distant mountains, on the other hand, are left behind so slowly that, in contrast with the cows, they seem almost to travel with the train.

In this case, the levers under the table caused the nearer object to move along with the observer. The cigarette pack was made to act as if it were far away; the book of matches was made to move as if it were close by.

In other words, by moving my eye and with it the plank, I created a reversed appearance. Under such circumstances, the unconscious processes of image formation made the appropriate image. The information from the cigarette pack was read and built up to be the image of a distant pack, but the height of the pack still subtended the same angle at the eye. Therefore, the pack now appeared to be of giant size. The book of matches, correspondingly, was brought seemingly close but still subtended the same angle that it subtended from its true location. What I created was an image in which the book of matches appeared to be half as far away and half its familiar size.

The machinery of perception created the image in accordance with the rules of parallax, rules that were for the first time clearly verbalized by painters in the Renaissance; and this whole process, the creating of the image with its built-in conclusions from the clues of parallax, happened quite outside my consciousness. The rules of the universe that we think we know are deep buried in our processes of perception.

Epistemology, at the natural history level, is mostly unconscious and correspondingly difficult to change. The second experiment that Ames demonstrated illustrates this difficulty of change.

This experiment has been called the *trapezoidal room*. In this case, Ames had me inspect a large box about five feet long, three feet high,

and three feet deep from front to back. The box was of strange trapezoidal shape, and Ames asked me to examine it carefully in order to learn its true shape and dimensions.

In the front of the box was a peephole big enough for two eyes, but before beginning the experiment, Ames had me put on a pair of prismatic spectacles that would corrupt my binocular vision. I was to have the subjective presupposition that I had the parallax of two eyes when indeed I had almost no binocular clues.

When I looked in through the peephole, the interior of the box appeared to be quite rectangular and was marked out like a room with rectangular windows. The true lines of paint suggesting windows were, of course, far from simple; they were drawn to give the impression of rectangularity, contradicting the true trapezoidal shape of the room. The side of the box toward which I faced when looking through the peephole was, I knew from my earlier inspection, obliquely placed, so that it was further from me at the left end and closer to me on the right.

Ames gave me a stick and asked me to reach in and touch with the point of the stick a sheet of typewriting paper pinned to the left-hand wall. I managed this fairly easily. Ames then said, "Do you see a similar piece of paper on the right-hand side? I want you to hit that second piece of paper with the stick. Start with the end of your stick against the left-hand paper, and hit as hard as you can."

I smote hard. The end of my stick moved about an inch and then hit the back of the room and could move no farther. Ames said, "Try again."

I tried perhaps fifty times, and my arm began to ache. I knew, of course, what correction I had to impose on my movement: I had to pull in as I struck in order to avoid that back wall. But what I *did* was governed by my image. I was trying to pull against my own spontaneous movement. (I suppose that if I had shut my eyes, I could have done better, but I did not try that.)

I never did succeed in hitting the second piece of paper, but, interestingly, my performance improved. I was finally able to move my stick several inches before it hit the back wall. And *as I practiced and improved my action,* my image changed to give me a more trapezoidal impression of the room's shape.

Ames told me afterward that, indeed, with more practice, people 37
learned to hit the second paper very easily and, at the same time, learned to see the room in its true trapezoidal shape.

The trapezoidal room was the last in the sequence of experiments, and after it, Ames suggested that we go to lunch. I went to wash up in the bathroom of the apartment. I turned the faucet marked "C" and got a jet of boiling water mixed with steam.

Ames and I then went down to find a restaurant. My faith in my own image formation was so shaken that I could scarcely cross the street. I was not sure that the oncoming cars were really where they seemed to be from moment to moment.

In sum, there is no free will against the immediate commands of the images that perception presents to the "mind's eye." But through arduous practice and self-correction, it is partly possible to alter those images. (Such changes in *calibration* are further discussed in Chapter 7.)

In spite of this beautiful experimentation, the fact of image formation remains almost totally mysterious. How it is done, we know not—nor, indeed, for what purpose.

It is all very well to say that it makes a sort of adaptive sense to present only the images to consciousness without wasting psychological process on consciousness of their making. But there is no clear primary reason for using images at all or, indeed, for being *aware* of any part of our mental processes.

Speculation suggests that image formation is perhaps a convenient or economical method of passing information across some sort of *interface.* Notably, where a person must act in a context between two machines, it is convenient to have the machines feed their information to him or her in image form.

A case that has been studied systematically is that of a gunner controlling antiaircraft fire on a naval ship.* The information from a series of sighting devices aimed at a flying target is summarized for the gunner in the form of a moving dot on a screen (i.e., an image). On the same screen is a second dot, whose position summarizes the direction in which an antiaircraft gun is aimed. The man can move this second dot

*John Stroud, personal communication.

by turning knobs on the device. These knobs also change the gun's aim. 38
The man must operate the knobs until the dots coincide on the screen. He then fires the gun.

The system contains two interfaces: sensory system–man and man–effector system. Of course, it is conceivable that in such a case, both the input information and the output information could be processed in digital form, without transformation into an iconic mode. But it seems to me that the iconic device is surely more convenient not only because, being human, I am a maker of mental images but also because at these interfaces images are economical or efficient. If that speculation is correct, then it would be reasonable to guess that mammals form images because the mental processes of mammals must deal with many interfaces.

There are some interesting side effects of our unawareness of the processes of perception. For example, when these processes work unchecked by input material from a sense organ, as in dream or hallucination or eidetic (see Glossary) imagery, it is sometimes difficult to doubt the external reality of what the images seem to represent. Conversely, it is perhaps a very good thing that we do *not* know too much about the work of creating perceptual images. In our ignorance of that work, we are free to *believe* what our senses tell us. To doubt continually the evidence of sensory report might be awkward.

5. THE DIVISION OF THE PERCEIVED UNIVERSE INTO PARTS AND WHOLES IS CONVENIENT AND MAY BE NECESSARY,* BUT NO NECESSITY DETERMINES HOW IT SHALL BE DONE

I have tried many times to teach this generality to classes of students and for this purpose have used Figure 1. The figure is presented to the class as a reasonably accurate chalk drawing on the blackboard, but without the letters marking the various angles. The class is asked to

* The question of formal necessity raised here might have an answer as follows: Evidently, the universe is characterized by an uneven distribution of causal and other types of linkage between its parts; that is, there are regions of dense linkage separated from each other by regions of less dense linkage. It may be that there are necessarily and inevitably processes which are responsive to the density of interconnection so that density is increased or sparsity is made more sparse. In such a case, the universe would necessarily present an appearance in which wholes would be bounded by the relative sparseness of their interconnection.

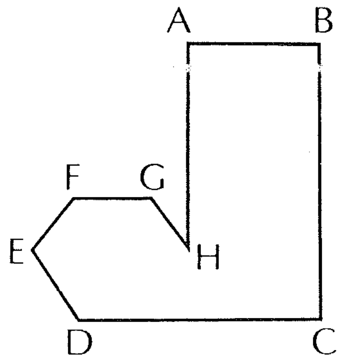

Figure 1

describe "it" in a page of written English. When each student has finished his or her description, we compare the results. They fall into several categories:

a. About 10 percent or less of students say, for example, that the object is a boot or, more picturesquely, the boot of a man with a gouty toe or even a toilet. Evidently, from this and similar analogic or iconic descriptions, it would be difficult for the hearer of the description to reproduce the object.

b. A much larger number of students see that the object contains most of a rectangle and most of a hexagon, and having divided it into parts in this way, they then devote themselves to trying to describe the relations between the incomplete rectangle and hexagon. A small number of these (but, surprisingly, usually one or two in every class) discover that a line, *BH*, can be drawn and extended to cut the base line, *DC*, at a point *I* in such a way that *HI* will complete a regular hexagon (Figure 2). This imaginary line will define the proportions of the rectangle but not, of course, the absolute lengths. I usually congratulate these students on their ability to create what resembles many scientific hypotheses, which "explain" a perceptible regularity in terms of some entity created by the imagination.

c. Many well-trained students resort to an operational method of description. They will start from some point on the outline of the object (interestingly enough, always an angle) and proceed from there, usually clockwise, with instructions for drawing the object.

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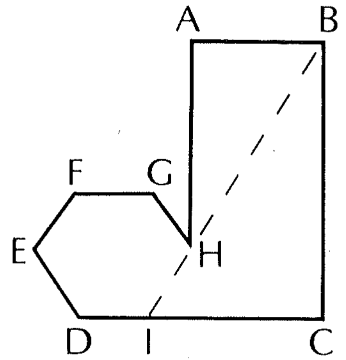


Figure 2

d. There are also two other well-known ways of description that no student has yet followed. No student has started from the statement "It's made of chalk and blackboard." No student has ever used the method of the halftone block, dividing the surface of the blackboard into a grid (arbitrarily rectangular) and reporting "yes" and "no" on whether each box of the grid contains or does not contain some part of the object. Of course, if the grid is coarse and the object small, a very large amount of information will be lost. (Imagine the case in which the entire object is smaller than the grid unit. The description will then consist of not more than four nor less than one affirmation, according to how the divisions of the grid fall upon the object.) However, this is, in principle, how the halftone blocks of newspaper illustration are transmitted by electric impulse and, indeed, how television works.

Note that all these methods of description contribute nothing to an *explanation* of the object—the hexago-rectangle. Explanation must always grow out of description, but the description from which it grows will always necessarily contain arbitrary characteristics such as those exemplified here.

G. Bateson, *Mind and Nature: A Necessary Unity*, E. P. Dutton, New York, 1979, pp. 25-40.

Helen Watson

THE TYPES OF OBJECTS THAT ENGLISH SPEAKERS AND YORUBA SPEAKERS TALK ABOUT

Talking about the world necessarily involves postulating the types of physical objects that constitute the world. It is an initial step in quantification. We have been considering the types of objects that English speakers and Yoruba speakers say comprise the world and admit into quanitification. Linguistic analysis of English and Yoruba utterances has led me to a conclusion that some may find rather startling. It is, that English speakers and Yoruba speakers usually talk of different types of things when they refer to the physical world. The type of object that an English speaker starts with in quantification is different from the type of object that a Yoruba person takes as the starting point for that process.

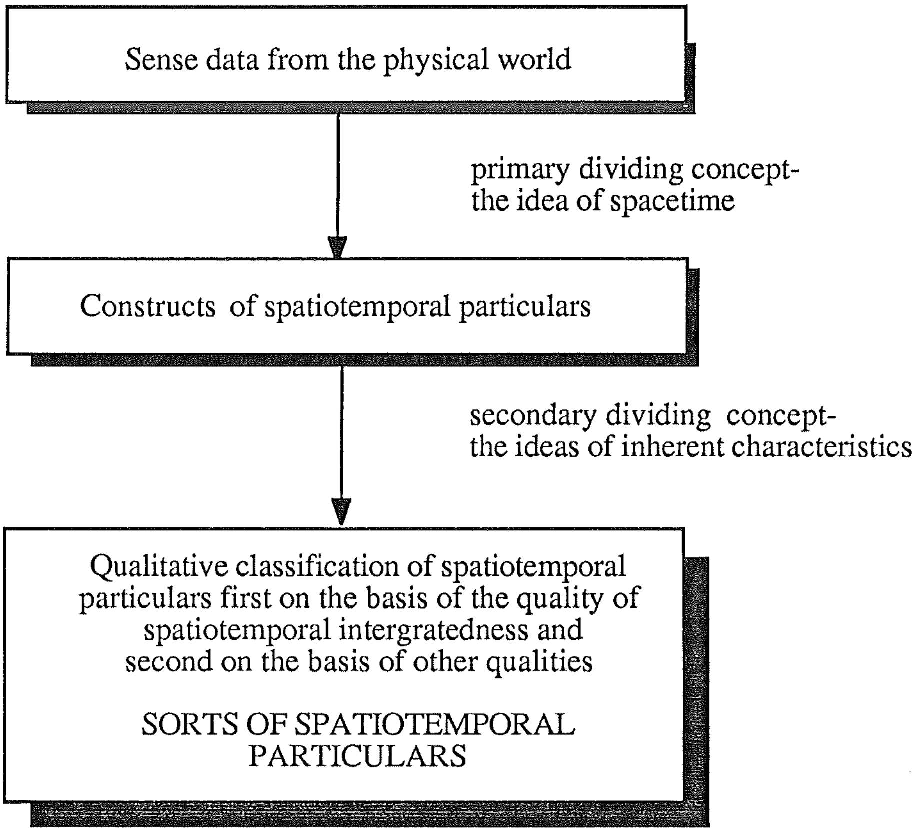
I suggested that there is a significant difference in meaning when a person is informed in English "It is a hoe" or "It is water" and when someone is told in Yoruba **"Okò ni ó jé"** or **"Omi ni ó jé"** albeit that the hoe and the glass of water are to be seen and felt by all. Better English translations of **okò ni ó jé** and **omi ni ó jé** are "Hoe-matter manifests" and "Water-matter manifests". The difference in meaning between the English and the Yoruba utterances is a difference in the type of object which is being referred to. When it comes to quantification, this difference is important.

Using the ideas of the inherent characteristics of matter and ideas of spacetime, ideas which we may take as analogies to the basic nature of the physical world, speakers of both languages synthesise ideas of physical objects to talk about and to provide the conceptual starting point for quantification. But, it seems that the types of physical objects they end up postulating are different. It seems that Yoruba speakers use these critieria in the reverse order to English speakers. This reversal of the order applying critieria through which the world is symbolically sliced up, produces ideas of different types of physical objects. So, the things which a Yoruba speaker says are in the world are different from the things which an English speaker says are in the world.

In Figure 1 I have summarised the sequence of procedures which I suggest produce the basic physical object, the object which provides the starting point for quantification in the English language. Using first the concept of spacetime to think about the world, the idea of spatiotemporal particulars is generated. These may be subsequently classified according to ideas of inherent characteristics, to produce the idea that physical objects are **different sorts of spatiotemporal particulars.** In Figure 2 I have summarised the sequence of conceptual treatments of sense data adopted by Yoruba speakers. The primary dividing idea is that of inherent characteristics of physical matter. This produces constructs of sortal particulars. These sortal particulars may be constructs as subdivided according to ideas of spacetime, producing the idea of physical objects as **spatiotemporally situated sortal particulars.** I suggest that this is the basic type of object which Yoruba speakers start with in the process of quantification.

Figure 1

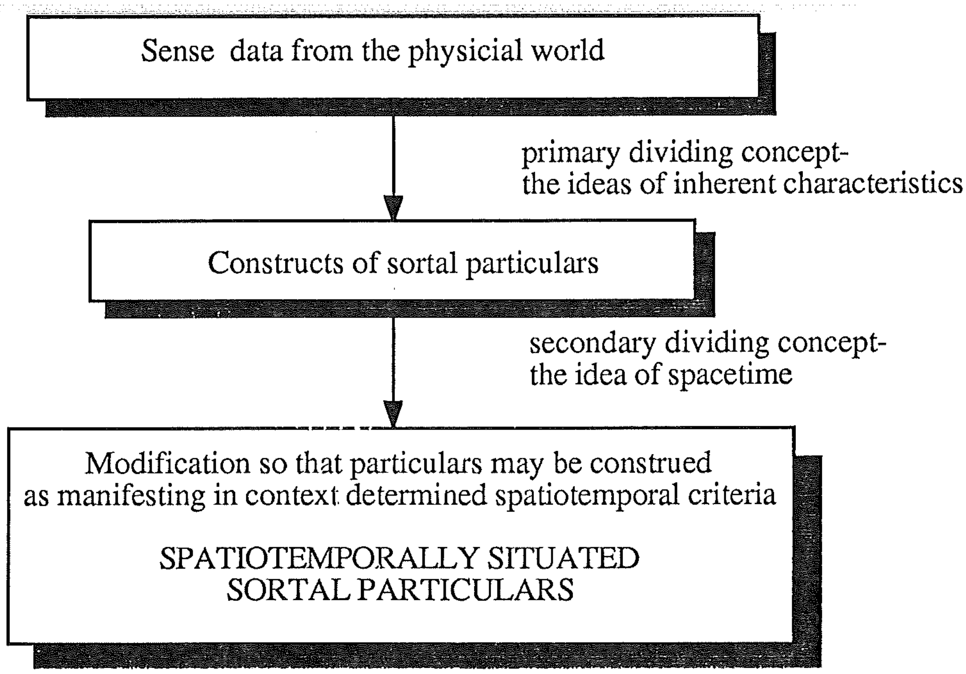
The conceptual scheme by which objects to talk about are synthesised by English speakers



The ideas of spacetime and inherent characteristics are not symmetrically related ideas. Thus there is a lack of symmetry in the types of 'objects to talk about' produced when these ideas are used as critieria of classification of matter, in a different order. To understand this lack of symmetry, let us consider two metaphors.

Figure 2

The Conceptual Scheme by which objects to talk about are synthesised by Yoruba speakers



My first metaphor concerns a sequence of two physical operations; the second I take from arithmetic. The procedures which may be adopted to prepare eggs for eating are: 1. denaturing the protein of the egg through immersion in boiling water, and 2. breaking and removing the shell of the egg. We often carry out these procedures in the sequence 1 then 2. That way we produce a boiled egg. But, we may also carry out the procedures in the sequence 2 then 1. That way we produce a coddled egg. In the first place the object is a white oblate spheroid with a yellow core. In the second instance the object produced is a network of yellowish white strands. Each are equally nutritious and tasty as prepared eggs, but they are different types of prepared eggs. Now, consider the arithmetic processes of division and addition. Quite different algorithms result when these procedures are applied in a different sequence. For example: $16 \div 8 + 20 = 22$ and $(16 + 20) \div 8 = 4.5$. In each of these operational sequences different objects are produced from the same material or information by using the same set of procedures in a different order. In the same way it seems that the objects which Yoruba speakers talk of and which enter quantification in the Yoruba language will differ from the objects which English speakers talk of and the English language.

I have represented the process of perception, the synthesis of objects to talk about, and quantification as a sequence of transformations of data information. Each process is an act of logical typing, producing at each stage ideas of 'objects' which, as the sequence progresses, become increasingly removed from sense data. I have taken the naming of physical objects as the first degree of abstraction since this is the first transformation which is accessible to our minds. I have shown how this transformation is language specific and how it leads speakers of different languages not only to speak of different types of physical objects but also to admit into their talk different types of abstract objects. Objects produced in a subsequent transformation of information. But, to suggest that people who speak different languages talk of the world as constituted by different types of physical and abstract objects is not to suggest that such people cannot communicate.

Saying there are different types of things in the world does not necessarily imply that other aspects of peoples' behaviour towards the 'stuff' of the world will differ. People walk around, or sit upon chairs and put their arms around people irrespective of what they may *say* is in the world. Probably too, they have similar pictorial images of sections of physical matter in their minds. To these physical bodies that they sit upon, or put their arms around and construe with pictures in their minds, young children learn to impute permanence across space and time, no doubt linking current perceptions to memories of past perceptions. Through such inductive procedures sense data is construed to mean permanent physical objects. The child learns to construct an acceptable basis for a conceptual system. Later when language develops, the child learns too that the abstract objects related to the physical objects s/he talks of, are assigned a similar permanence.

People create pictorial images in their minds of what they take to be the permanent objects of the world, and to the extent they use these pictorial images to think about the world, this far what people *say* is in the world will not affect how they think about the world. Further, if we speak only of physical objects in overt stimulus situations it will matter little that we say there are different types of things in the world when we speak English and Yoruba. Often when we are using the linguistic form of symbolic representation of the world (that is, we are talking), we abstract from the physical objects we are talking about and start to talk of abstract objects, just as we have abstracted from sense data to arrive at the physical objects we talk of. It is becasue we often talk of abstract things, and use these things in various further conceptual processes that it does become important to note that we talk of different types of physical objects when we speak either English or Yoruba.

A Yoruba speaking person and an English speaking person handle a cup of water in the same way and they may wield a hoe in a similar fashion. They may have similar pictorial images of these objects in their minds. But, they *talk* of them in different ways. A Yoruba first mentions the sort of object that it is. Its spatiotemporal mode of manifestation can secondarily be mentioned if the context demands it. The English speaking person primarily situates the object spatiotemporally and comments on the sort of object that it is. And, because they allocate primacy to different aspects of the world when they talk of it, Yoruba speaking and English speaking people end up talking of different types of objects. After the first level of abstraction of sense data English speakers and Yoruba speakers find themselves in different positions. And this matters when it comes to futher degrees of a abstraction. Quantification is a series of abstractions, and that Yoruba speakers and English speakers say there are different types of things in the world will have profound consequences for that series of abstractions. We may guess too that other matters of abstraction will be similarly affected. For, a linguistic conceptual system sits upon the base of the things that speakers of that language *say* there is in the world.

H. Watson, *Numbers and Things*, unpublished manuscript.

Benjamin Lee Whorf
SCIENCE AND LINGUISTICS

Every normal person in the world, past infancy in years, can and does talk. By virtue of that fact, every person—civilized or uncivilized—carries through life certain naïve but deeply rooted ideas about talking and its relation to thinking. Because of their firm connection with speech habits that have become unconscious and automatic, these 207
notions tend to be rather intolerant of opposition. They are by no means entirely personal and haphazard; their basis is definitely systematic, so that we are justified in calling them a system of natural logic—a term that seems to me preferable to the term common sense, often used for the same thing.

According to natural logic, the fact that every person has talked fluently since infancy makes every man his own authority on the process by which he formulates and communicates. He has merely to consult a common substratum of logic or reason which he and everyone else are supposed to possess. Natural logic says that talking is merely an incidental process concerned strictly with communication, not with formulation of ideas. Talking, or the use of language, is supposed only to "express" what is essentially already formulated nonlinguistically. Formulation is an independent process, called thought or thinking, and is supposed to be largely indifferent to the nature of particular languages. Languages have grammars, which are assumed to be merely norms of conventional and social correctness, but the use of language is supposed

* Reprinted from *Technol. Rev.*, 42:229–231, 247–248, no. 6 (April 1940).

to be guided not so much by them as by correct, rational, or intelligent THINKING.

Thought, in this view, does not depend on grammar but on laws of logic or reason which are supposed to be the same for all observers of the universe—to represent a rationale in the universe that can be "found" 208
independently by all intelligent observers, whether they speak Chinese

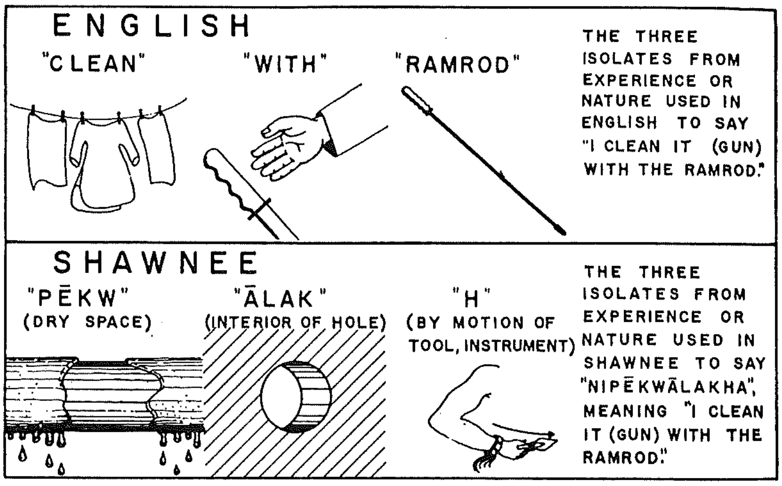


Figure 9. Languages dissect nature differently. The different isolates of meaning (thoughts) used by English and Shawnee in reporting the same experience, that of cleaning a gun by running the ramrod through it. The pronouns 'I' and 'it' are not shown by symbols, as they have the same meaning in each language. In Shawnee ni- equals 'I'; -a equals 'it.'

or Choctaw. In our own culture, the formulations of mathematics and of formal logic have acquired the reputation of dealing with this order of things: i.e., with the realm and laws of pure thought. Natural logic holds that different languages are essentially parallel methods for expressing this one-and-the-same rationale of thought and, hence, differ really in but minor ways which may seem important only because they are seen at close range. It holds that mathematics, symbolic logic, philosophy, and so on are systems contrasted with language which deal directly with this realm of thought, not that they are themselves specialized extensions of language. The attitude of natural logic is well shown in an old quip about a German grammarian who devoted his whole life
209 to the study of the dative case. From the point of view of natural logic, the dative case and grammar in general are an extremely minor issue. A different attitude is said to have been held by the ancient Arabians: Two princes, so the story goes, quarreled over the honor of putting on the shoes of the most learned grammarian of the realm; whereupon their father, the caliph, is said to have remarked that it was the glory of his kingdom that great grammarians were honored even above kings.

The familiar saying that the exception proves the rule contains a good deal of wisdom, though from the standpoint of formal logic it became an absurdity as soon as "prove" no longer meant "put on trial." The old saw began to be profound psychology from the time it ceased to have standing in logic. What it might well suggest to us today is that, if a rule has absolutely no exceptions, it is not recognized as a rule or as anything else; it is then part of the background of experience of which we tend to remain unconscious. Never having experienced anything in contrast to it, we cannot isolate it and formulate it as a rule until we so enlarge our experience and expand our base of reference that we encounter an interruption of its regularity. The situation is somewhat analogous to that of not missing the water till the well runs dry, or not realizing that we need air till we are choking.

For instance, if a race of people had the physiological defect of being able to see only the color blue, they would hardly be able to formulate the rule that they saw only blue. The term blue would convey no meaning to them, their language would lack color terms, and their words denoting their various sensations of blue would answer to, and translate, our words "light, dark, white, black," and so on, not our word "blue." In order to formulate the rule or norm of seeing only blue, they would need exceptional moments in which they saw other colors. The phenomenon of gravitation forms a rule without exceptions; needless to say, the untutored person is utterly unaware of any law of gravitation, for it would never enter his head to conceive of a universe in which bodies behaved otherwise than they do at the earth's surface. Like the color blue with our hypothetical race, the law of gravitation is a part of the untutored individual's background, not something he isolates from that background. The law could not be formulated until bodies that always fell were seen in terms of a wider astronomical world in which bodies moved in orbits or went this way and that.

Similarly, whenever we turn our heads, the image of the scene passes
210 across our retinas exactly as it would if the scene turned around us. But this effect is background, and we do not recognize it; we do not see a room turn around us but are conscious only of having turned our heads in a stationary room. If we observe critically while turning the head or eyes quickly, we shall see, no motion it is true, yet a blurring of the

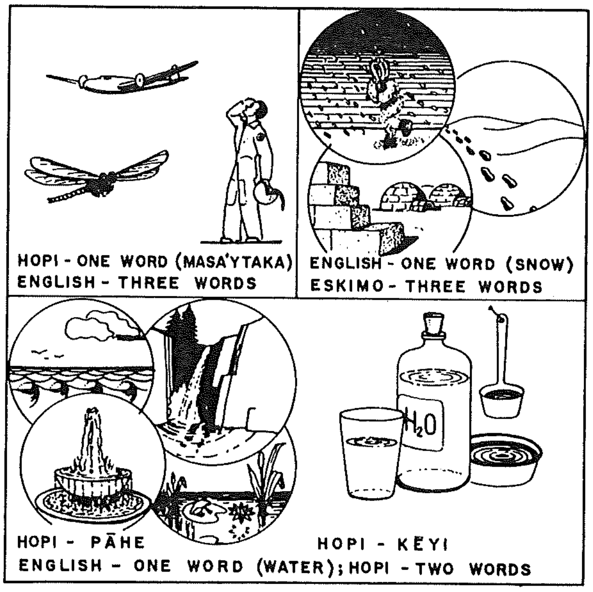


Figure 10. Languages classify items of experience differently. The class corresponding to one word and one thought in language A may be regarded by language B as two or more classes corresponding to two or more words and thoughts.

scene between two clear views. Normally we are quite unconscious of this continual blurring but seem to be looking about in an unblurred world. Whenever we walk past a tree or house, its image on the retina changes just as if the tree or house were turning on an axis; yet we do not see trees or houses turn as we travel about at ordinary speeds. Sometimes ill-fitting glasses will reveal queer movements in the scene as we look about, but normally we do not see the relative motion of the environment when we move; our psychic makeup is somehow adjusted to disregard whole realms of phenomena that are so all-pervasive as to be irrelevant to our daily lives and needs.

Natural logic contains two fallacies: First, it does not see that the phenomena of a language are to its own speakers largely of a background character and so are outside the critical consciousness and control of the speaker who is expounding natural logic. Hence, when anyone, as a natural logician, is talking about reason, logic, and the laws of correct thinking, he is apt to be simply marching in step with purely grammatical facts that have somewhat of a background character in his own language or family of languages but are by no means universal in all languages and in no sense a common substratum of reason. Second, natural logic confuses agreement about subject matter, attained through use of language, with knowledge of the linguistic process by which agreement is attained: i.e., with the province of the despised (and to its notion superfluous) grammarian. Two fluent speakers, of English let us say, quickly reach a point of assent about the subject matter of their speech; they agree about what their language refers to. One of them, A, can give directions that will be carried out by the other, B, to A's complete satisfaction. Because they thus understand each other so perfectly, A and B, as natural logicians, suppose they must of course know how it is all done. They think, e.g., that it is simply a matter of choosing words to express thoughts. If you ask A to explain how he got B's agreement so readily, he will simply repeat to you, with more or less elaboration or abbreviation, what he said to B. He has no notion of the process involved. The amazingly complex system of linguistic patterns and classifications, which A and B must have in common before they can adjust to each other at all, is all background to *A* and *B*.

These background phenomena are the province of the grammarian—or of the linguist, to give him his more modern name as a scientist. The word linguist in common, and especially newspaper, parlance means something entirely different, namely, a person who can quickly attain agreement about subject matter with different people speaking a number of different languages. Such a person is better termed a polyglot or a multilingual. Scientific linguists have long understood that ability to speak a language fluently does not necessarily confer a linguistic knowledge of it, i.e., understanding of its background phenomena and its systematic processes and structure, any more than ability to play a good game of billiards confers or requires any knowledge of the laws of mechanics that operate upon the billiard table.

The situation here is not unlike that in any other field of science. All
212 real scientists have their eyes primarily on background phenomena that cut very little ice, as such, in our daily lives; and yet their studies have a way of bringing out a close relation between these unsuspected realms of fact and such decidedly foreground activities as transporting goods, preparing food, treating the sick, or growing potatoes, which in time may become very much modified, simply because of pure scientific investigation in no way concerned with these brute matters themselves. Linguistics presents a quite similar case; the background phenomena with which it deals are involved in all our foreground activities of talking and of reaching agreement, in all reasoning and arguing of cases, in all law, arbitration, conciliation, contracts, treaties, public opinion, weighing of scientific theories, formulation of scientific results. Whenever agreement or assent is arrived at in human affairs, and whether or not mathematics or other specialized symbolisms are made part of the procedure, THIS AGREEMENT IS REACHED BY LINGUISTIC PROCESSES, OR ELSE IT IS NOT REACHED.

As we have seen, an overt knowledge of the linguistic processes by which agreement is attained is not necessary to reaching some sort of agreement, but it is certainly no bar thereto; the more complicated and difficult the matter, the more such knowledge is a distinct aid, till the point may be reached—I suspect the modern world has about arrived at it—when the knowledge becomes not only an aid but a necessity. The situation may be likened to that of navigation. Every boat that sails is in the lap of planetary forces; yet a boy can pilot his small craft around a harbor without benefit of geography, astronomy, mathematics, or international politics. To the captain of an ocean liner, however, some knowledge of all these subjects is essential.

When linguists became able to examine critically and scientifically a large number of languages of widely different patterns, their base of reference was expanded; they experienced an interruption of phenomena hitherto held universal, and a whole new order of significances came into their ken. It was found that the background linguistic system (in other words, the grammar) of each language is not merely a reproducing instrument for voicing ideas but rather is itself the shaper of ideas, the program and guide for the individual's mental activity, for his analysis of impressions, for his synthesis of his mental stock in trade. Formulation of ideas is not an independent process, strictly rational in the old sense, but is part of a particular grammar, and differs, from slightly to
213 greatly, between different grammars. We dissect nature along lines laid down by our native languages. The categories and types that we isolate from the world of phenomena we do not find there because they stare every observer in the face; on the contrary, the world is presented in a kaleidoscopic flux of impressions which has to be organized by our minds—and this means largely by the linguistic systems in our minds.

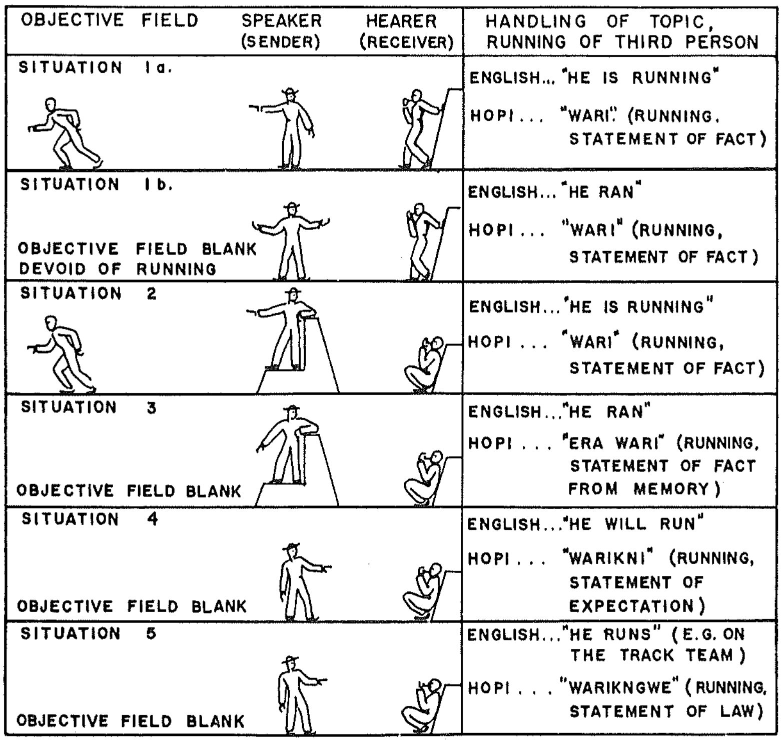


Figure 11. Contrast between a "temporal" language (English) and a "timeless" language (Hopi). What are to English differences of time are to Hopi differences in the kind of validity.

We cut nature up, organize it into concepts, and ascribe significances as we do, largely because we are parties to an agreement to organize it in this way—an agreement that holds throughout our speech community and is codified in the patterns of our language. The agreement is, of course, an implicit and unstated one, BUT ITS TERMS ARE ABSOLUTELY OBLIGATORY; we cannot talk at all except by subscribing to the organi- 214
zation and classification of data which the agreement decrees.

This fact is very significant for modern science, for it means that no individual is free to describe nature with absolute impartiality but is constrained to certain modes of interpretation even while he thinks himself most free. The person most nearly free in such respects would be a linguist familiar with very many widely different linguistic systems. As yet no linguist is in any such position. We are thus introduced to a new principle of relativity, which holds that all observers are not led by the same physical evidence to the same picture of the universe, unless their linguistic backgrounds are similar, or can in some way be calibrated.

This rather startling conclusion is not so apparent if we compare only our modern European languages, with perhaps Latin and Greek thrown in for good measure. Among these tongues there is a unanimity of major pattern which at first seems to bear out natural logic. But this unanimity exists only because these tongues are all Indo-European dialects cut to the same basic plan, being historically transmitted from what was long ago one speech community; because the modern dialects have long shared in building up a common culture; and because much of this culture, on the more intellectual side, is derived from the linguistic backgrounds of Latin and Greek. Thus this group of languages satisfies the special case of the clause beginning "unless" in the statement of the linguistic relativity principle at the end of the preceding paragraph. From this condition follows the unanimity of description of the world in the community of modern scientists. But it must be emphasized that "all modern Indo-European-speaking observers" is not the same thing as "all observers." That modern Chinese or Turkish scientists describe the world in the same terms as Western scientists means, of course, only that they have taken over bodily the entire Western system of rationalizations, not that they have corroborated that system from their native posts of observation.

When Semitic, Chinese, Tibetan, or African languages are contrasted with our own, the divergence in analysis of the world becomes more apparent; and, when we bring in the native languages of the Americas, where speech communities for many millenniums have gone their ways independently of each other and of the Old World, the fact that languages dissect nature in many different ways becomes patent. The relativity of all conceptual systems, ours included, and their dependence
215 upon language stand revealed. That American Indians speaking only their native tongues are never called upon to act as scientific observers is in no wise to the point. To exclude the evidence which their languages offer as to what the human mind can do is like expecting botanists to study nothing but food plants and hothouse roses and then tell us what the plant world is like!

Let us consider a few examples. In English we divide most of our words into two classes, which have different grammatical and logical properties. Class 1 we call nouns, e.g., 'house, man'; class 2, verbs, e.g., 'hit, run.' Many words of one class can act secondarily as of the other class, e.g., 'a hit, a run,' or 'to man (the boat),' but, on the primary level, the division between the classes is absolute. Our language thus gives us a bipolar division of nature. But nature herself is not thus polarized. If it be said that 'strike, turn, run,' are verbs because they denote temporary or short-lasting events, i.e., actions, why then is 'fist' a noun? It also is a temporary event. Why are 'lightning, spark, wave, eddy, pulsation, flame, storm, phase, cycle, spasm, noise, emotion' nouns? They are temporary events. If 'man' and 'house' are nouns because they are long-lasting and stable events, i.e., things, what then are 'keep, adhere, extend, project, continue, persist, grow, dwell,' and so on doing among the verbs? If it be objected that 'possess, adhere' are verbs because they are stable relationships rather than stable percepts, why then should 'equilibrium, pressure, current, peace, group, nation, society, tribe, sister,' or any kinship term be among the nouns? It will be found that an "event" to us means "what our language classes as a verb" or something analogized therefrom. And it will be found that it is not possible to define 'event, thing, object, relationship,' and so on, from nature, but that to define them always involves a circuitous return to the grammatical categories of the definer's language.

In the Hopi language, 'lightning, wave, flame, meteor, puff of smoke, pulsation' are verbs—events of necessarily brief duration cannot be anything but verbs. 'Cloud' and 'storm' are at about the lower limit of duration for nouns. Hopi, you see, actually has a classification of events (or linguistic isolates) by duration type, something strange to our modes of thought. On the other hand, in Nootka, a language of Vancouver Island, all words seem to us to be verbs, but really there are no classes 1 and 2; we have, as it were, a monistic view of nature that gives us only one class of word for all kinds of events. 'A house occurs' or 'it houses'
216 is the way of saying 'house,' exactly like 'a flame occurs' or 'it burns.' These terms seem to us like verbs because they are inflected for durational and temporal nuances, so that the suffixes of the word for house event make it mean long-lasting house, temporary house, future house, house that used to be, what started out to be a house, and so on.

Hopi has one noun that covers every thing or being that flies, with the exception of birds, which class is denoted by another noun. The former noun may be said to denote the class (*FC–B*)—flying class minus bird. The Hopi actually call insect, airplane, and aviator all by the same word, and feel no difficulty about it. The situation, of course, decides any possible confusion among very disparate members of a broad linguistic class, such as this class (*FC–B*). This class seems to us too large and inclusive, but so would our class 'snow' to an Eskimo. We have the same word for falling snow, snow on the ground, snow packed hard like ice, slushy snow, wind-driven flying snow—whatever the situation may be. To an Eskimo, this all-inclusive word would be almost unthinkable; he would say that falling snow, slushy snow, and so on, are sensuously and operationally different, different things to contend with; he uses different words for them and for other kinds of snow. The Aztecs go even farther than we in the opposite direction, with 'cold,' 'ice,' and 'snow' all represented by the same basic word with different terminations; 'ice' is the noun form; 'cold,' the adjectival form; and for 'snow,' "ice mist."

What surprises most is to find that various grand generalizations of the Western world, such as time, velocity, and matter, are not essential to the construction of a consistent picture of the universe. The psychic experiences that we class under these headings are, of course, not destroyed; rather, categories derived from other kinds of experiences take over the rulership of the cosmology and seem to function just as well. Hopi may be called a timeless language. It recognizes psychological time, which is much like Bergson's "duration," but this "time" is quite unlike the mathematical time, *T*, used by our physicists. Among the peculiar properties of Hopi time are that it varies with each observer, does not permit of simultaneity, and has zero dimensions; i.e., it cannot be given a number greater than one. The Hopi do not say, "I stayed five days," but "I left on the fifth day." A word referring to this kind of time, like the word day, can have no plural. The puzzle picture (Fig. 11, page 213) will give mental exercise to anyone who would like to figure out how the Hopi verb gets along without tenses. Actually, the only practical use of our tenses, in one-verb sentences, is to distinguish among five typical situations, which are symbolized in the picture. The timeless Hopi verb does not distinguish between the present, past, and future of the event itself but must always indicate what type of validity the SPEAKER intends the statement to have: (a) report of an event (situations 1, 2, 3 in the picture); (b) expectation of an event (situation 4); (c) generalization or law about events (situation 5). Situation 1, where the speaker and listener are in contact with the same objective field, is divided by our language into the two conditions, 1*a* and 1*b*, which it calls present and past, respectively. This division is unnecessary for a language which assures one that the statement is a report.

Hopi grammar, by means of its forms called aspects and modes, also makes it easy to distinguish among momentary, continued, and repeated occurrences, and to indicate the actual sequence of reported events. Thus the universe can be described without recourse to a concept of dimensional time. How would a physics constructed along these lines work, with no *T* (time) in its equations? Perfectly, as far as I can see, though of course it would require different ideology and perhaps different mathematics. Of course V (velocity) would have to go too. The Hopi language has no word really equivalent to our 'speed' or 'rapid.' What translates these terms is usually a word meaning intense or very, accompanying any verb of motion. Here is a clue to the nature of our new physics. We may have to introduce a new term *I*, intensity. Every thing and event will have an *I*, whether we regard the thing or event as moving or as just enduring or being. Perhaps the *I* of an electric charge will turn out to be its voltage, or potential. We shall use clocks to measure some intensities, or, rather, some RELATIVE intensities, for the absolute intensity of anything will be meaningless. Our old friend ac-

celeration will still be there but doubtless under a new name. We shall perhaps call it V, meaning not velocity but variation. Perhaps all growths and accumulations will be regarded as V's. We should not have the concept of rate in the temporal sense, since, like velocity, rate introduces a mathematical and linguistic time. Of course we know that all measurements are ratios, but the measurements of intensities made by comparison with the standard intensity of a clock or a planet we do
218 not treat as ratios, any more than we so treat a distance made by com-
parison with a yardstick.

A scientist from another culture that used time and velocity would have great difficulty in getting us to understand these concepts. We should talk about the intensity of a chemical reaction; he would speak of its velocity or its rate, which words we should at first think were simply words for intensity in his language. Likewise, he at first would think that intensity was simply our own word for velocity. At first we should agree, later we should begin to disagree, and it might dawn upon both sides that different systems of rationalization were being used. He would find it very hard to make us understand what he really meant by velocity of a chemical reaction. We should have no words that would fit. He would try to explain it by likening it to a running horse, to the difference between a good horse and a lazy horse. We should try to show him, with a superior laugh, that his analogy also was a matter of different intensities, aside from which there was little similarity between a horse and a chemical reaction in a beaker. We should point out that a running horse is moving relative to the ground, whereas the material in the beaker is at rest.

One significant contribution to science from the linguistic point of view may be the greater development of our sense of perspective. We shall no longer be able to see a few recent dialects of the Indo-European family, and the rationalizing techniques elaborated from their patterns, as the apex of the evolution of the human mind, nor their present wide spread as due to any survival from fitness or to anything but a few events of history—events that could be called fortunate only from the parochial point of view of the favored parties. They, and our own thought processes with them, can no longer be envisioned as spanning the gamut of reason and knowledge but only as one constellation in a galactic expanse. A fair realization of the incredible degree of diversity of linguistic system that ranges over the globe leaves one with an inescapable feeling that the human spirit is inconceivably old; that the few thousand years of history covered by our written records are no more than the thickness of a pencil mark on the scale that measures our past experience on this planet; that the events of these recent millenniums spell nothing in any evolutionary wise, that the race has taken no sudden spurt, achieved no commanding synthesis during recent millenniums,
219 but has only played a little with a few of the linguistic formulations and
views of nature bequeathed from an inexpressibly longer past. Yet neither this feeling nor the sense of precarious dependence of all we know upon linguistic tools which themselves are largely unknown need be discouraging to science but should, rather, foster that humility which accompanies the true scientific spirit, and thus forbid that arrogance of the mind which hinders real scientific curiosity and detachment.

B. L. Whorf, *Language, Thought and Reality; Selected Writings*, (ed). J. B. Carroll, MIT Press, Cambridge, Mass., 1956, pp. 207–19.

4.4

Edward Sapir

LANGUAGE, RACE AND CULTURE

Language has a setting. The people that speak it 207
belong to a race (or a number of races), that is, to a group which is set off by physical characteristics from other groups. Again, language does not exist apart from culture, that is, from the socially inherited assemblage of practices and beliefs that determines the texture of our lives. Anthropologists have been in the habit of studying man under the three rubrics of race, language, and culture. One of the first things they do with a natural area like Africa or the South Seas is to map it out from this threefold point of view. These maps answer the questions: What and where are the major divisions of the human animal, biologically considered (e.g., Congo Negro, Egyptian White; Australian Black, Polynesian)? What are the most inclusive linguistic groupings, the "linguistic stocks," and what is the distribution of each (e.g., the Hamitic languages of northern Africa, the Bantu languages of the south; the Malayo-Polynesian languages of Indonesia, Melanesia, Micronesia, and Polynesia)? How do the peoples of the given area divide themselves as cultural beings? what the outstanding "cultural areas" and what are the dominant ideas in each (e.g., the Mohammedan north of Africa; the primitive hunting, non-agricultural culture of the Bushmen in the south;
the culture of the Australian natives, poor in physical 208
respects but richly developed in ceremonialism; the more advanced and highly specialized culture of Polynesia)?

The man in the street does not stop to analyze his position in the general scheme of humanity. He feels that he is the representative of some strongly integrated portion of humanity—now thought of as a "nationality," now as a "race"—and that everything that pertains to him as a typical representative of this large group somehow belongs together. If he is an Englishman, he feels himself to be a member of the "Anglo-Saxon" race, the "genius" of which race has fashioned the English language and the "Anglo-Saxon" culture of which the language is the expression. Science is colder. It inquires if these three types of classification—racial, linguistic, and cultural—are congruent, if their association is an inherently necessary one or is merely a matter of external history. The answer to the inquiry is not encouraging to "race" sentimentalists. Historians and anthropologists find that races, languages, and cultures are not distributed in parallel fashion, that their areas of distribution intercross in the most bewildering fashion, and that

the history of each is apt to follow a distinctive course. Races intermingle in a way that languages do not. On the other hand, languages may spread far beyond their original home, invading the territory of new races and of new culture spheres. A language may even die out in its primary area and live on among peoples violently hostile to the persons of its original speakers. Further, the accidents of history are constantly rearranging the borders of culture areas without necessarily effacing the existing linguistic cleavages. If we can once thoroughly convince ourselves that race, in its only intelligible, that is biological, sense, is supremely indifferent to the history of languages and cultures, that these are no more directly explainable on the score of race than on that of the
209 laws of physics and chemistry, we shall have gained a viewpoint that allows a certain interest to such mystic slogans as Slavophilism, Anglo-Saxondom, Teutonism, and the Latin genius but that quite refuses to be taken in by any of them. A careful study of linguistic distributions and of the history of such distributions is one of the driest of commentaries on these sentimental creeds.

That a group of languages need not in the least correspond to a racial group or a culture area is easily demonstrated. We may even show how a single language intercrosses with race and culture lines. The English language is not spoken by a unified race. In the United States there are several millions of negroes who know no other language. It is their mother-tongue, the formal vesture of their inmost thoughts and sentiments. It is as much their property, as inalienably "theirs," as the King of England's. Nor do the English-speaking whites of America constitute a definite race except by way of contrast to the negroes. Of the three fundamental white races in Europe generally recognized by physical anthropologists—the Baltic or North European, the Alpine, and the Mediterranean—each has numerous English-speaking representatives in America. But does not the historical core of English-speaking peoples, those relatively "unmixed" populations that still reside in England and its colonies, represent a race, pure and single? I cannot see that the evidence points that way. The English people are an amalgam of many distinct strains. Besides the old "Anglo-Saxon," in other words North German, element which is conventionally represented as the basic strain, the English blood comprises Norman French,[1] Scandinavian, "Celtic,"[2] and pre-Celtic elements. If

[1] Itself an amalgam of North "French" and Scandinavian elements.

[2] The "Celtic" blood of what is now England and Wales is by no means confined to the Celtic-speaking regions—Wales and, until recently, Cornwall. There is every reason to believe that the invading Germanic tribes (Angles, Saxons, Jutes) did not

210 by "English" we mean also Scotch and Irish,[3] then the term "Celtic" is loosely used for at least two quite distinct racial elements—the short, dark-complexioned type of Wales and the taller, lighter, often ruddy-haired type of the Highlands and parts of Ireland. Even if we confine ourselves to the Saxon element, which, needless to say, nowhere appears "pure," we are not at the end of our troubles. We may roughly identify this strain with the racial type now predominant in southern Denmark and adjoining parts of northern Germany. If so, we must content ourselves with the reflection that while the English language is

historically most closely affiliated with Frisian, in second degree with the other West Germanic dialects (Low Saxon or "Plattdeutsch," Dutch, High German), only in third degree with Scandinavian, the specific "Saxon" racial type that overran England in the fifth and sixth centuries was largely the same as that now represented by the Danes, who speak a Scandinavian language, while the High German-speaking population of central and southern Germany[4] is markedly distinct.

But what if we ignore these finer distinctions and simply assume that the "Teutonic" or Baltic or North European racial type coincided in its distribution with that of the Germanic languages? Are we not on safe

exterminate the Brythonic Celts of England nor yet drive them altogether into Wales and Cornwall (there has been far too much "driving" of conquered peoples into mountain fastnesses and land's ends in our histories), but simply intermingled with them and imposed their rule and language upon them.

[3] In practice these three peoples can hardly be kept altogether distinct. The terms have rather a local-sentimental than a clearly racial value. Intermarriage has gone on steadily for centuries and it is only in certain outlying regions that we get relatively pure types, e.g., the Highland Scotch of the Hebrides. In America, English, Scotch, and Irish strands have become inextricably interwoven.

[4] The High German now spoken in northern Germany is not of great age, but is due to the spread of standardized German, based on Upper Saxon, a High German dialect, at the expense of "Plattdeutsch."

ground then? No, we are now in hotter water than ever. First of all, the mass of the German-speaking population (central and southern Germany, German Switzerland, German Austria) do not belong to the tall, blond-haired, long-headed[5] "Teutonic" race at all, but to the shorter, darker-complexioned, short-headed[6] Alpine race, of which the central population of France, the French Swiss, and many of the western and northern Slavs (e.g., Bohemians and Poles) are equally good representatives. The distribution of these "Alpine" populations corresponds in part to that of the old continental "Celts," whose language has everywhere given way to Italic, Germanic, and Slavic pressure. We shall do well to avoid speaking of a "Celtic race," but if we were driven to give the term a content, it would probably be more appropriate to apply it to, roughly, the western portion of the Alpine peoples than to the two island types that I referred to before. These latter were certainly "Celticized," in speech and, partly, in blood, precisely as, centuries later, most of England and part of Scotland was "Teutonized" by the Angles and Saxons. Linguistically speaking, the "Celts" of to-day (Irish Gaelic, Manx, Scotch Gaelic, Welsh, Breton) are Celtic and most of the Germans of to-day are Germanic precisely as the American Negro, Americanized Jew, Minnesota Swede, and German-American are "English." But, secondly, the Baltic race was, and is, by no means an exclusively Germanic-speaking people. The northernmost "Celts," such as the Highland Scotch, are in all probability a specialized offshoot of this race. What these people spoke before they were Celticized nobody knows, but there is nothing whatever to indicate that they spoke a Germanic language. Their language may quite well have been as remote from any known Indo-European idiom as are Basque and Turkish to-day. Again, to the

[5] "Dolichocephalic."

[6] "Brachycephalic."

212 east of the Scandinavians are non-Germanic members of the race—the Finns and related peoples, speaking languages that are not definitely known to be related to Indo-European at all.

We cannot stop here. The geographical position of the Germanic languages is such[7] as to make it highly probable that they represent but an outlying transfer of an Indo-European dialect (possibly a Celto-Italic prototype) to a Baltic people speaking a language or a group of languages that was alien to Indo-European.[8] Not only, then, is English not spoken by a unified race at present but its prototype, more likely than not, was originally a foreign language to the race with which English is more particularly associated. We need not seriously entertain the idea that English or the group of languages to which it belongs is in any intelligible sense the expression of race, that there are embedded in it qualities that reflect the temperament or "genius" of a particular breed of human beings.

Many other, and more striking, examples of the lack of correspondence between race and language could be given if space permitted. One instance will do for many. The Malayo-Polynesian languages form a well-defined group that takes in the southern end of the Malay Peninsula and the tremendous island world to the south and east (except Australia and the greater part of New Guinea). In this vast region we find represented no less than three distinct races—the Negro-

[7] By working back from such data as we possess we can make it probable that these languages were originally confined to a comparatively small area in northern Germany and Scandinavia. This area is clearly marginal to the total area of distribution of the Indo-European-speaking peoples. Their center of gravity, say 1000 B.C., seems to have lain in southern Russia.

[8] While this is only a theory, the technical evidence for it is stronger than one might suppose. There are a surprising number of common and characteristic Germanic words which cannot be connected with known Indo-European radical elements and which may well be survivals of the hypothetical pre-Germanic language; such are *house, stone, sea, wife* (German *Haus, Stein, See, Weib*).

213 like Papuans of New Guinea and Melanesia, the Malay race of Indonesia, and the Polynesians of the outer islands. The Polynesians and Malays *all* speak languages of the Malayo-Polynesian group, while the languages of the Papuans belong partly to this group (Melanesian), partly to the unrelated languages ("Papuan") of New Guinea.[9] In spite of the fact that the greatest race cleavage in this region lies between the Papuans and the Polynesians, the major linguistic division is of Malayan on the one side, Melanesian and Polynesian on the other.

As with race, so with culture. Particularly in more primitive levels, where the secondarily unifying power of the "national" [10] ideal does not arise to disturb the flow of what we might call natural distributions, is it easy to show that language and culture are not intrinsically associated. Totally unrelated languages share in one culture, closely related languages—even a single language—belong to distinct culture spheres. There are many excellent examples in aboriginal America. The Athabaskan languages form as clearly unified, as structurally specialized, a group as any that I know of.[11] The speakers of these languages belong to four distinct culture areas—the simple hunting culture of western Canada and the interior of Alaska (Loucheux, Chipewyan), the buffalo culture of the Plains (Sarcee),

[9] Only the easternmost part of this island is occupied by Melanesian-speaking Papuans.

[10] A "nationality" is a major, sentimentally unified, group. The historical factors that lead to the feeling of national unity are various—political, cultural, linguistic, geographic, sometimes specifically religious. True racial factors also may enter in, though the accent on "race" has generally a psychological rather than a strictly biological value. In an area dominated by the national sentiment there is a tendency for language and culture to become uniform and specific, so that linguistic and cultural boundaries at least tend to coincide. Even at best, however, the linguistic unification is never absolute, while the cultural unity is apt to be superficial, of a quasi-political nature, rather than deep and far-reaching.

[11] The Semitic languages, idiosyncratic as they are, are no more definitelv earmarked

the highly ritualized culture of the southwest (Nav- 214
aho), and the peculiarly specialized culture of northwestern California (Hupa). The cultural adaptability of the Athabaskan-speaking peoples is in the strangest contrast to the inaccessibility to foreign influences of the languages themselves.[12] The Hupa Indians are very typical of the culture area to which they belong. Culturally identical with them are the neighboring Yurok and Karok. There is the liveliest intertribal intercourse between the Hupa, Yurok, and Karok, so much so that all three generally attend an important religious ceremony given by any one of them. It is difficult to say what elements in their combined culture belong in origin to this tribe or that, so much at one are they in communal action, feeling, and thought. But their languages are not merely alien to each other; they belong to three of the major American linguistic groups, each with an immense distribution on the northern continent. Hupa, as we have seen, is Athabaskan and, as such, is also distantly related to Haida (Queen Charlotte Islands) and Tlingit (southern Alaska); Yurok is one of the two isolated Californian languages of the Algonkin stock, the center of gravity of which lies in the region of the Great Lakes; Karok is the northernmost member of the Hokan group, which stretches far to the south beyond the confines of California and has remoter relatives along the Gulf of Mexico.

Returning to English, most of us would readily admit, I believe, that the community of language between Great Britain and the United States is far from arguing a like community of culture. It is customary to say that they possess a common "Anglo-Saxon" cultural heritage, but are not many significant differences in life and feeling obscured by the tendency of the "cultured" to take this common heritage too much for granted? In so far as America is still specifically "English," it is only colonially or vestigially so; its prevail-

[12] See page 196.

ing cultural drift is partly towards autonomous and 215
distinctive developments, partly towards immersion in the larger European culture of which that of England is only a particular facet. We cannot deny that the possession of a common language is still and will long continue to be a smoother of the way to a mutual cultural understanding between England and America, but it is very clear that other factors, some of them rapidly cumulative, are working powerfully to counteract this leveling influence. A common language cannot indefinitely set the seal on a common culture when the geographical, political, and economic determinants of the culture are no longer the same throughout its area.

Language, race, and culture are not necessarily correlated. This does not mean that they never are. There is some tendency, as a matter of fact, for racial and cultural lines of cleavage to correspond to linguistic ones, though in any given case the latter may not be of the same degree of importance as the others. Thus, there is a fairly definite line of cleavage between the Polynesian languages, race, and culture on the one hand and those of the Melanesians on the other, in spite of a considerable amount of overlapping.[13] The racial and cultural division, however, particularly the former, are of major importance, while the linguistic division is of quite minor significance, the Polynesian languages constituting hardly more than a special dialectic subdivision of the combined Melanesian-Polynesian group. Still clearer-cut coincidences of cleavage may be found. The language, race, and culture of the Eskimo are markedly distinct from those of their neighbors;[14] in southern Africa the language, race, and

[13] The Fijians, for instance, while of Papuan (negroid) race, are Polynesian rather than Melanesian in their cultural and linguistic affinities.

[14] Though even here there is some significant overlapping. The southernmost Eskimo of Alaska were assimilated in culture to their Tlingit neighbors. In northeastern Siberia, too, there is no sharp cultural line between the Eskimo and the Chukchi.

216 culture of the Bushmen offer an even stronger contrast to those of their Bantu neighbors. Coincidences of this sort are of the greatest significance, of course, but this significance is not one of inherent psychological relation between the three factors of race, language, and culture. The coincidences of cleavage point merely to a readily intelligible historical association. If the Bantu and Bushmen are so sharply differentiated in all respects, the reason is simply that the former are relatively recent arrivals in southern Africa. The two peoples developed in complete isolation from each other; their present propinquity is too recent for the slow process of cultural and racial assimilation to have set in very powerfully. As we go back in time, we shall have to assume that relatively scanty populations occupied large territories for untold generations and that contact with other masses of population was not as insistent and prolonged as it later became. The geographical and historical isolation that brought about race differentiations was naturally favorable also to far-reaching variations in language and culture. The very fact that races and cultures which are brought into historical contact tend to assimilate in the long run, while neighboring languages assimilate each other only casually and in superficial respects,[15] indicates that there is no profound causal relation between the development of language and the specific development of race and of culture.

But surely, the wary reader will object, there must be some relation between language and culture, and between language and at least that intangible aspect of race that we call "temperament." Is it not inconceivable that the particular collective qualities of mind that have fashioned a culture are not precisely the same as were responsible for the growth of a particular linguistic morphology? This question takes us

[15] The supersession of one language by another is of course not truly a matter of linguistic assimilation.

217 into the heart of the most difficult problems of social psychology. It is doubtful if any one has yet attained

to sufficient clarity on the nature of the historical process and on the ultimate psychological factors involved in linguistic and cultural drifts to answer it intelligently. I can only very briefly set forth my own views, or rather my general attitude. It would be very difficult to prove that "temperament," the general emotional disposition of a people,[16] is basically responsible for the slant and drift of a culture, however much it may manifest itself in an individual's handling of the elements of that culture. But granted that temperament has a certain value for the shaping of culture, difficult though it be to say just how, it does not follow that it has the same value for the shaping of language. It is impossible to show that the form of a language has the slightest connection with national temperament. Its line of variation, its drift, runs inexorably in the channel ordained for it by its historic antecedents; it is as regardless of the feelings and sentiments of its speakers as is the course of a river of the atmospheric humors of the landscape. I am convinced that it is futile to look in linguistic structure for differences corresponding to the temperamental variations which are supposed to be correlated with race. In this connection it is well to remember that the emotional aspect of our psychic life is but meagerly expressed in the build of language.[17]

Language and our thought-grooves are inextricably

[16] "Temperament" is a difficult term to work with. A great deal of what is loosely charged to national "temperament" is really nothing but customary behavior, the effect of traditional ideals of conduct. In a culture, for instance, that does not look kindly upon demonstrativeness, the natural tendency to the display of emotion becomes more than normally inhibited. It would be quite misleading to argue from the customary inhibition, a cultural fact, to the native temperament. But ordinarily we can get at human conduct only as it is culturally modified. Temperament in the raw is a highly elusive thing.

[17] See pages 38, 39

interrelated, are, in a sense, one and the same. As there is nothing to show that there are significant racial differences in the fundamental conformation of thought, it follows that the infinite variability of linguistic form, another name for the infinite variability of the actual process of thought, cannot be an index of such significant racial differences. This is only apparently a paradox. The latent content of all languages is the same—the intuitive *science* of experience. It is the manifest form that is never twice the same, for this form, which we call linguistic morphology, is nothing more nor less than a collective *art* of thought, an art denuded of the irrelevancies of individual sentiment. At last analysis, then, language can no more flow from race as such than can the sonnet form.

Nor can I believe that culture and language are in any true sense causally related. Culture may be defined as *what* a society does and thinks. Language is a particular *how* of thought. It is difficult to see what particular causal relations may be expected to subsist between a selected inventory of experience (culture, a significant selection made by society) and the particular manner in which the society expresses all experience. The drift of culture, another way of saying history, is a complex series of changes in society's selected inventory—additions, losses, changes of emphasis and relation. The drift of language is not properly concerned with changes of content at all, merely with changes in formal expression. It is possible, in thought, to change every sound, word, and concrete concept of

a language without changing its inner actuality in the least, just as one can pour into a fixed mold water or plaster or molten gold. If it can be shown that culture has an innate form, a series of contours, quite apart from subject-matter of any description whatsoever, we have a something in culture that may serve as a term of comparison with and possibly a means of relating it to language. But until such purely formal patterns of
219 culture are discovered and laid bare, we shall do well to hold the drifts of language and of culture to be non-comparable and unrelated processes. From this it follows that all attempts to connect particular types of linguistic morphology with certain correlated stages of cultural development are vain. Rightly understood, such correlations are rubbish. The merest *coup d'œil* verifies our theoretical argument on this point. Both simple and complex types of language of an indefinite number of varieties may be found spoken at any desired level of cultural advance. When it comes to linguistic form, Plato walks with the Macedonian swineherd, Confucius with the head-hunting savage of Assam.

It goes without saying that the mere content of language is intimately related to culture. A society that has no knowledge of theosophy need have no name for it; aborigines that had never seen or heard of a horse were compelled to invent or borrow a word for the animal when they made his acquaintance. In the sense that the vocabulary of a language more or less faithfully reflects the culture whose purposes it serves it is perfectly true that the history of language and the history of culture move along parallel lines. But this superficial and extraneous kind of parallelism is of no real interest to the linguist except in so far as the growth or borrowing of new words incidentally throws light on the formal trends of the language. The linguistic student should never make the mistake of identifying a language with its dictionary.

If both this and the preceding chapter have been largely negative in their contentions, I believe that they have been healthily so. There is perhaps no better way to learn the essential nature of speech than to realize what it is not and what it does not do. Its superficial connections with other historic processes are so close that it needs to be shaken free of them if we are to see it in its own right. Everything that we have
220 so far seen to be true of language points to the fact that it is the most significant and colossal work that the human spirit has evolved—nothing short of a finished form of expression for all communicable experience. This form may be endlessly varied by the individual without thereby losing its distinctive contours; and it is constantly reshaping itself as is all art. Language is the most massive and inclusive art we know, a mountainous and anonymous work of unconscious generations.

E. Sapir, *Language: An Introduction to the Study of Speech*, Harcourt, Brace & World, New York, 1949, pp. 207–20.

5

AFRICAN IRON TECHNOLOGY

Peter Schmidt & Donald H. Avery

5.1

COMPLEX IRON SMELTING AND PREHISTORIC CULTURE IN TANZANIA

One of the primary problems in the prehistory of Africa is how iron metallurgy developed in sub-Saharan Africa. Since 1937 when Cline wrote *Mining and Metallurgy in Negro Africa* (*1*) there has been no systematic attempt to understand the technological complexities and developmental history of African iron metallurgy. Cline's study is a continent-wide ethnological survey, which prehistorians, historians of technology, and metallurgists continue to use as their main reference source. These scholars either subscribe to or passively accept the assumption that the sponge iron bloomery process, well known from European archeological contexts, also describes African iron smelting. This view persists despite the great variability known to exist in African iron smelting processes. Yet no one has tried to see whether or not the European bloomery model applies to iron smelting in Africa. This is a particular problem that we address in this article: whether or not the bloomery process as known from European archeology is similar to iron smelting technology in Africa.

It is also apparent that some authorities have overlooked important variables in African smelting furnaces, and consequently have failed to see possible explanations for the production of high-grade steel. For example, when Tylecote (*2*) looks at the evidence from Oyo, southwestern Nigeria, "reported by Bellamy in 1904" (*3*), he fails to attach importance to the production of a high-carbon (1.67 percent) steel in the induced-draught shaft furnace with tuyères (blowpipes) inserted inside the furnace. Tylecote (*2*) attaches little significance to the possibility that preheating occurred in the Oyo furnace: "At first sight it would seem that the long tuyères are designed for efficient preheating of the ingoing air, but a calculation shows that the degree of preheat would only be 10°C." We show here that these statements are based on unrealistic laboratory and theoretical models and, in fact, do little justice to the technological process-

Summary. Western scientists and students of history have long explained the iron bloomery process by evidence available from European archeology. Ethnographic, technological, and archeological research into the technological life of the Haya of northwestern Tanzania show that these people and their forebears 1500 to 2000 years ago practiced a highly advanced iron smelting technology based on preheating principles and, as a result, produced carbon steel. This sophisticated technology may have evolved as an adaptation to overexploited forest resources. These discoveries are significant for the history of Africa and the history of metallurgy.

es which occur in authentic furnaces, such as those operated by the Haya who live in West Lake Region of Tanzania (Fig. 1).

Our current perspective is limited to one culture, but we expect that it can be extended to other areas of the continent as we gain more archeological and ethnographic information about iron metallurgy. In Africa, there are several areas where traditional iron bloomery smelters actively practice their craft, or remember how it is practiced. Among the Haya there is an active blacksmithing tradition in which scrap iron is used, but no contemporary bloomery smelters are to be found. However, some of these smiths and other old men did smelt iron in the traditional way during their youth 50 to 60 years ago; they are still alive, able to smelt, and in many cases are eager to relive the experience once again. The knowledge held by such old men in West Lake, Tanzania, is profound in its implications for the history of iron smelting and for the prehistory of complex African civilizations. But this knowledge is threatened every day by the passage of time, by death, and by age-related infirmities occurring in this quickly shrinking group of expert smelters.

The Haya are a Bantu-speaking agricultural people who live along the western shore of Lake Victoria. The Haya live in densely populated villages while practicing some cattle herding and a banana and bean subsistence agriculture. Coffee and tea are grown as cash crops. In precolonial times, the culture area contained six small kingdoms, three of which were ruled by different branches of the Bahinda clan.

Archeological verification of oral traditions in one kingdom, Kyamutwara, shows that oral traditions accurately document the history of some ancient religious and historical sites seized by the more recent Bahinda dynasty (*4, 5*). Archeological investigations at the Rugomora Mahe site confirmed the accuracy of traditions about iron production there and dated the iron production to 2400 to 2550 years ago. The later occupation of the site by the royal dynasty was dated by the carbon-14 method to 250 to 300 years ago (*4–6*), confirming dates suggested by oral, royal genealogies. Archeological analysis of this site included examination of technological materials, principally slag and fragments of tuyères.

As part of our study of the ancient technology, we attempted to discover the flow temperatures of slag which had been formed in Early Iron Age furnaces. Test results showed a temperature of 1350° to 1400°C. On the basis of European archeological evidence, this is within the highest temperature limits obtained during most experiments performed in bloomeries. In addition, the physical properties of the tuyère fragments suggested that they had come from inside the furnace. From these archeological data we developed the hypothesis that preheating of the air blast had been a prominent feature of African Early Iron Age smelting. We reasoned that higher combustion temperatures achieved by this preheating would have formed slag at temperatures similar to or higher than those indicated by our heat tests on prehistoric slag. Preheating would permit the attainment of much higher furnace temperatures and better fuel economy than was obtainable in cold blast European bloomeries.

This hypothesis was subsequently tested by experiment, which showed that 20th-century metallurgists and anthropologists, given sufficient money, instrumentation, and effort, could preheat air and make some iron in a reconstructed furnace. But we felt that observations in Africa of traditional iron smelters were necessary in order to obtain reliable and authentic results. We knew from previous research, especially from interviews with old men who had smelted, that the Haya had placed their tuyères or

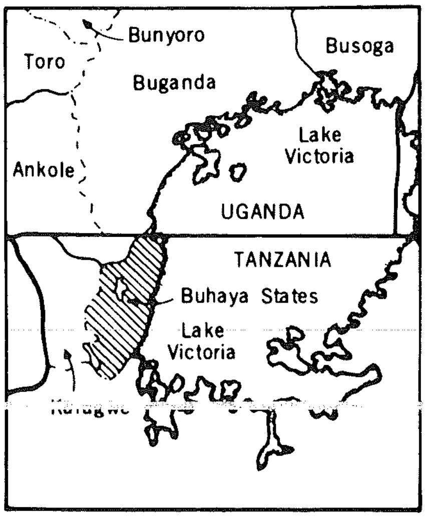


Fig. 1 (left). The traditional kingdoms of the interlacustrine region. Fig. 2 (right). Smelters and assistants place tuyères inside the furnace to variable depths. Tuyères inserted inside the furnace cause preheating of the air blast and result in high temperatures in the combustion zone. The eight tuyères are placed between large blocks of refractory slag, which are used as the foundation for the furnace chimney.

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blast pipes inside their furnaces, thereby creating a situation which would have led to a hot air blast. As Tylecote observes (*2*), enormous gains were made in 19th-century blast furnaces with low degrees of preheat (100°C) brought about by "inefficient [rough] pipe stoves." Although Cline (*1*) mentions several furnace types with tuyères inside furnaces as part of his broad ethnological consideration of iron metallurgy, these examples, along with Oyo, have never been recognized as possible preheating. We presumed that, if a similar technological phenomenon occurred during prehistoric times in West Lake, then there was an excellent possibility that an efficient and complex technology prevailed there during the Early Iron Age.

The implications of our studies suggested that ethnographic research on iron smelting and iron forging, including quantitative technological observations, was critical to testing the hypothesis as well as to understanding the "bloomery" process itself and its associated cultural behaviors. Before we formed our preheating hypothesis, we had assumed—like Maddin, Muhly, and Wheeler (*7*)—that "the highest temperature that could be reached in a primitive smelt appears to have been about 1,200 degrees." That assumption is accompanied by the idea that "smelting iron ore at that temperature yields not a puddle of metal but a spongy mass mixed with iron oxide and iron silicate." These were apparently valid assumptions until our research among the Haya demonstrated at least two unique and important characteristics in this advanced iron technology.

The first demonstrated example of preheating in an African smelting furnace is the Haya process. As oral information had indicated, the Haya do place tuyères inside their furnaces (Fig. 2). The furnace is what we call a shaft-bowl type, with a forced-air draught. A cone-shaped shaft 140 centimeters high (Fig. 3) and constructed with old, refractory slag and mud made from the earth of a termite mound is built over a bowl 50 to 60 centimeters deep and lined with termite mud (Fig. 4). Swamp grass is burned in the bowl until the bowl is filled with the charred swamp reeds. The charred reeds provide a charcoal bed of filamentary alkaline-coated carbon fiber that is readily wetted and infiltrated by the molten iron slag. Eight drum bellows covered with goatskins are used to force air into tuyères, 50 to 60 centimeters in length, which are inserted inside the furnace. The combustion zone just beyond the tuyère develops very high temperatures. The high-temperature products of combustion divide, part passing up through the furnace shaft and part back-flowing along the tuyère; this heats the tuyère and the blast air passing through it. Eventually, external tuyère surfaces are wet with slag and dissolve in it at about 1250° to 1300°C. This means the external temperature of the tuyère clay is in excess of 1250°C. Slag penetrated approximately one-third through the tuyère wall.

The air temperatures inside the tuyère were measured by passing a ceramic-insulated thermocouple down the tuyère. Figure 5 shows typical curves at different times and tuyère lengths. The rap-

Fig. 3. An idealized profile of a Haya iron smelting furnace: this view is before the mixed iron ore and charcoal charge has been added. Note that the tuyères are inside the furnace and therefore are conduits that preheat the air passing through them. Iron ore pockets are added inside the foundation blocks to roast iron ore for the next smelt.

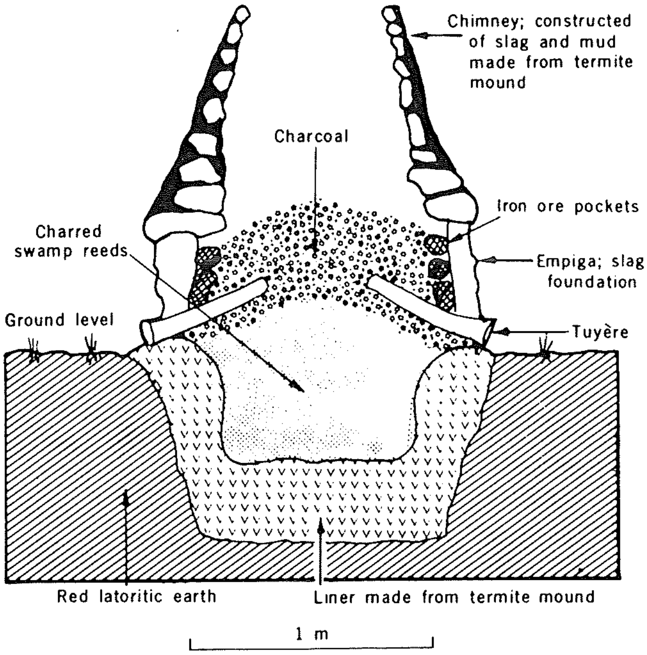
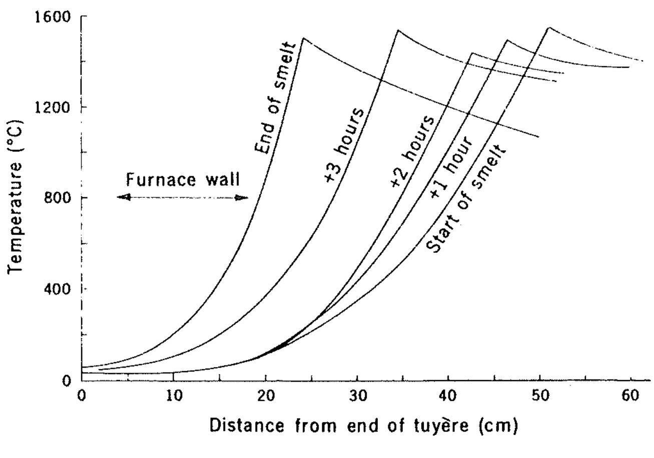


Fig. 4. A melt under way in Nyunge village in the Bukoba area of West Lake Region in Tanzania. Eight double-drum bellows are operated for up to 8 hours in a process that yields a carbon-steel bloom in crystalline form.

Fig. 5. Temperature profiles along a tuyère and into the blast zone. As smelting progresses, the tuyères burn off, the preheating length decreases, and furnace temperatures drop.



id rise in the last 10 centimeters probably represents significant radiation heating of the thermocouple; however, the preheating temperature of the air is clearly in the range of 600°C or more. The preheating efficiency of the tuyère (as reflected in the combustion temperature) is related to its length (Fig. 5). As the tuyère shrinks in size, the temperature in the combustion zone decreases. Combustion zone temperatures were found to vary, but in many cases were above the melting point of a Pt-PtRh (10 percent rhodium) thermocouple, which melts at 1820°C. The Zelechovise I type furnace (Northern Moravia, 8th century A.D.) tested by Pleiner (*8*) in Czechoslovakia during 1964 reached a maximum temperature of 1450°C in its small underground shaft. During tests of an experimental 2nd-century Roman shaft furnace, Tylecote *et al.* (*9*) recorded a combustion zone temperature as high as 1600°C. We feel confident that the high combustion zone temperature in the Haya furnace is caused by preheating of the air blast. The high temperatures achieved through preheating are critical to the Haya process. Given the other unique characteristic of the process—the crystalline formation of the bloom discussed below—the attainment of high temperatures means that iron will be produced more efficiently.

The other remarkable characteristic that makes this process of great interest to science and history is that, in the Haya process, the bloom does not form by the sintering of fine solid particles. Throughout Europe and in many areas of the world, the bloomery process is known as one in which reduction takes place in a CO atmosphere while the bloom forms by sintering of fine solid particles in an iron sponge and by liquation of a fluid slag. But in the Haya smelting furnace of West Lake, iron is precipitated as large crystals growing in a fayalite-wüstite slag; the molten slag undergoes a carbon boil in much the same way that an open hearth furnace has a carbon boil. The carbon boil is caused by an intimate and large contact area with solid carbon (the charred swamp reeds) throughout the molten slag mass. The critical variables in the process appear to be roasting of the iron ore, the use of charred swamp reeds in the furnace bowl, and high temperatures.

"Roasting" of the ore is an important initial step, for then carbon is introduced into the ore. The ore is "roasted" in a reducing atmosphere in a pit with wet wood and limited O_2 access. This reduces the ore, with the ore providing the O_2 for combustion

$$2Fe_3O_4 + 8CO \rightarrow 6Fe + 8CO_2$$

After roasting the ore cools off in a CO_2 atmosphere and is reoxidized with carbon deposited

$$6Fe + 3CO_2 \rightarrow 6FeO + 3C$$

This and the use of charred swamp grass is important: when the iron ore melts above the tuyères, it forms a fayalite-wüstite slag with carbon inclusions from the roasting; it then infiltrates and interacts with the fibrous, carbonaceous interface provided by the burned swamp grass. This, in turn, provides an extremely high carbon-slag contact area. The solid carbon reacts with the slag to form CO bubbles that coalesce and rise to the slag surface, forming large bubbles, 5 centimeters in diameter. As the carbon boil removes oxygen from the slag, the slag becomes supersaturated with iron; iron crystals then precipitate and grow. As the liquid slag changes its composition and loses wüstite (FeO), it becomes increasingly refractory and its melting point rises. Thus, a high temperature allows further and more efficient precipitation of iron crystals.

Metallographic examination of these iron precipitates shows a planar growth interface (Fig. 6a). Here we see the massive iron, white area, growing into the fayalite-wüstite slag, gray area. The growing interface seems always to be pure (carbon-free) iron. The crystal perfection is very good with large straight Neüman bands and large well-formed crystals, as illustrated in the micrographs of a traditional bloom (Fig. 6, b and c). As the iron grows, some slag and carbon are entrapped. The entrapped carbon then locally carburizes the iron surrounding it, producing a highly variable local carbon content (Fig. 6d). If this local carburization were to continue beyond 2.4 percent carbon, then it would result in the formation of cast iron droplets. But this level of carburization is not typical in the Haya process. Our analyses show that the bloom obtained from six smelts in West Lake during 1976 have a carbon content ranging from pure iron to eutectic and hypereutectic microcompositions. Further sampling and testing will determine whether the eutectic compositions predominate.

The assemblage of Early Iron Age artifacts is not complex, being limited to two braceletlike objects from both the Rugomora Mahe and KM2 sites, and one possible knife blade fragment from each site.

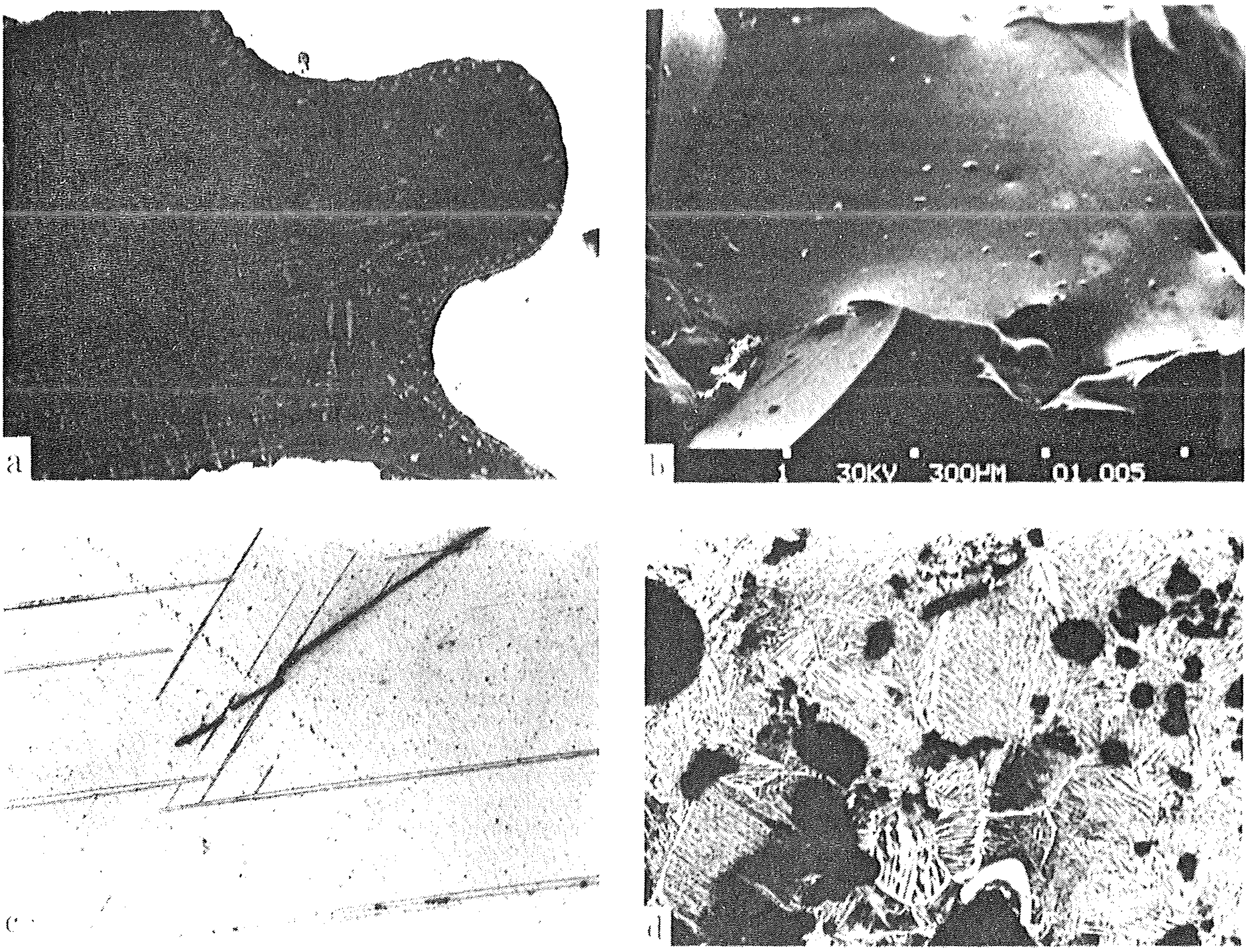


Fig. 6. (a) Bloom from 1976 smelt, showing growth interface between iron (white) and slag (gray) (unetched, ×20). (b) Scanning electron micrograph view of intercrystalline fracture in traditional bloom. (c) Photomicrograph of same bloom illustrated in (b), showing long undistorted Neuman bands (2 percent Nital, ×18). (d) Etched micrograph of bloom from 1976 smelt, showing wide local range of carbon content (×18).

Often the iron is severely oxidized, and therefore we await better archeological evidence for the cultural and technological aspects of prehistoric iron implements.

We find in traditional Africa an Iron Age technological process that was exceedingly complex and, in historical and relative terms, also advanced. There are indications from cultures near the Haya that similar types of furnaces were used in Uganda (*2*). Ethnographic data, although incomplete, indicate that many other neighboring cultures practiced an entirely different kind of technology. However, we suggest that this level of complexity was not limited to East Africa. The probable use of preheating in Yoruba furnaces (*3*) and the high carbon steel produced in them suggests that this and other African smelting technologies may have technological characteristics that are highly advanced and technologically sophisticated according to contemporary historical and scientific values. The insertion of multiple tuyères in large (4-meter-high) shaft-type furnaces such as those among the Wafipa people of southwestern Tanzania (*10, 11*) also suggests preheating and the possible production of steel by a process different from that observed in West Lake.

Our ethnographic study of recent smelting has also provided material evidence that the technological process revealed in 1977 archeological discoveries in the West Lake region has remained unchanged during the last 1500 to 2000 years. Archeological excavations at the KM2 site, an Early Iron Age industrial locale near the Lake Victoria shore (1°28′30″S, 31°44′45″E), have yielded excellent data which demonstrate that similar furnace form and preheating were employed during the Early Iron Age. Furthermore, archeological surveys tentatively show that Early Iron Age peoples lived in large villages where they practiced iron production. It has been suggested (*4*) that large settlements may be linked to localized high population densities caused by an industry "which demanded significant manpower." Our recent observations of the diversity of skills required for preparation of materials, operation of the furnace, and processing of the iron bloom show an organized, highly cooperative labor force. We suggest that the recently observed exploitative behavior is a reasonable, general model for the Early Iron Age. This does not mean that we believe the two are the same, but that the demands of the prehistoric industry were very similar to those of the recent past. It is abundantly clear that the industry is labor intensive, particularly during the direct production phase, during which a minimum of one dozen craftsmen are engaged in the cooperative enterprise. It is also possible that an economically complex and technologically advanced culture with high population densities may be linked to the evolution of politically centralized states in West Lake Region and in neighboring areas such as Rwanda.

Further archeological research is required to discover why such a complex technology grew up along the western shore of Lake Victoria. One possible hypothesis is that the heavy exploitation of forests (for charcoal and for agricultural purposes) may have triggered the development of an efficient, fuel-economizing technology. The widespread distribution of Early Iron Age industrial sites in West Lake, the manner of charcoal production and its 10 to 1 weight ratio of wood to charcoal, and the need for 500 pounds of charcoal, all suggest that, if smelting was widely practiced in prehistoric times, then the impact of the technological system must have been severe. The decline of productivity may be linked to an overexploited forest resource base; the evolution of the fuel-efficient preheated furnace may be an adaptation by the local smelters to that depleted resource.

Preliminary information on prehistoric vegetation obtained through study of prehistoric pollen indicate possible widespread forest clearance during prehistoric times. The enormous drop in productivity since the first colonial government in 1840 is attributed by the smelters to the availability of cheap, imported iron tools and spring steel and to the greater economic rewards of coffee farming.

One of the more profound implications of the West Lake discoveries is that we are now able to say that a technologically superior iron-smelting process developed in Africa more than 1500 years ago. This knowledge will help to change scholarly and popular ideas that technological sophistication developed in Europe but not in Africa. In that respect the ramifications are significant for the history of Africa and her people.

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12. We thank the United Republic of Tanzania, especially the University of Dar es Salaam and the Department of Antiquities for permission to conduct research in West Lake Region. A feasibility study of field research was assisted by a grant (P.S.) from the Wenner Gren Foundation for Anthropological Research. Field research and analyses were supported by NSF grant BSN 75-19611.

P. Schmidt & D. H. Avery, 'Complex iron smelting and prehistoric culture in Tanzania', *Science*, vol. 201, no. 4361, 1978, pp. 1085–9.

Candice L. Goucher

5.2

IRON IS IRON 'TIL IT IS RUST

THE DEMISE of West African iron industries has commonly been explained as the direct result of European technological competition. A succinct summary of the trade-impact model can be found in a discussion of economic change by J. E. Flint in Ajayi and Crowder's *History of West Africa*:

> By 1800 the West African iron mining and smelting industries, upon which the blacksmiths had once had to rely completely for their raw material, were almost at an end, ruined by the competition of cheaper and purer iron bars imported from Europe.[1]

He goes on to suggest that in particular the proliferation of trade products from the industrial revolution, such as iron basins,[2] matchets, knives, and so on provided increased competition to the African smiths throughout the nineteenth century. Another author has perceived the impact and interaction of technologies with an even more sweeping statement:

> But in a thousand years of African 'metallurgy' not only had furnace design remained unchanged, so too had its products: cast iron and steel continued unknown and wrought iron remained insufficient to meet the needs of local populations... The first nail was driven into the coffin of the traditional industry with the introduction of the European iron bar... From the mid seventeenth century the iron bar appears on the Guinea Coast to meet the demands of an iron hunger prevailing there apparently from the earliest times.[3]

This view, which assumes the backwardness and inferiority of West African technology, does not take account of the state of European metallurgy at the time and is in marked contrast to the assessments provided by both contemporary historical accounts and archaeological evidence. It could be pointed out that the European imports, to which firearms could be added, actually required an expansion in the repertoire of West African blacksmiths. Such imports were never so cheaply obtained that they were not repaired by African smiths. Moreover, far from being 'pure', after the eighteenth century much of the European iron had a high sulphur content (due to the use of coal as fuel) which seriously affected the quality of the smelted product and made it a poor substitute for the carbon-steel or pure iron bloom from

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[1] J. F. Ade Ajayi and Michael Crowder (eds), *History of West Africa*, vol. 2 (London, 1974), 387.

[2] To my knowledge basins were never made in iron by West African smiths; presumably such imports might have affected other industries such as calabash carving or potting.

[3] Denis Williams, *Icon and Image* (London, 1974), 69, 72, 86.

 some African furnaces. The present paper, then, is an attempt to provide an alternative model for the decline of West African iron-smelting. Historical, technical, and archaeological evidence is presented in support of an ecological model which suggests that deforestation produced by environmental exploitation (particularly the reliance on charcoal fuels) and climatic change must be taken into consideration in any explanation of the decline of iron industries, and that the trade-impact model must be substantially modified.

Qualitative judgments made by early European travellers along the West African coast provide a positive assessment of African iron technology. At the beginning of the sixteenth century, Sierra Leone was thought to have 'much iron of good quality',[4] probably the same iron which Pereira mentions as being traded at a good profit.[5] Frequent references to a high-quality product, in some cases 'steel', suggest that the voyagers from pre-industrial Europe encountered a highly competitive product.[6] Particularly as it is appearing more probable that iron technology developed independently in West Africa,[7] it is necessary to remove the technological assumptions based on Western models in undertaking a closer examination of the West African industries.

The written descriptions and photodocumentation of iron-smelting furnaces in West Africa reveal a great diversity of types and processes. The location of the smelting site itself was dependent upon the availability of ore, fuel, water, and suitable materials for the manufacture of the furnace and tuyères. The cylindrical shaft furnace[8] has a widespread distribution as recorded in West African ethnographies and historical accounts.[9] Both the induced-draught furnace type[10] and forced-draught type have been reported. While the bellows used for supplying the air blast have frequently been the subject of elaborate typologies, the more important distinctions between smelting and smithing functions have not been adequately considered.

The number and position of the tuyères has also been overlooked. Variations in these technological details may reflect responses to environmental conditions. It has been demonstrated experimentally[11] that the placement of

[4] Valentim Fernandes, *Description de la Côte d'Afrique de Ceuta au Sénégal* (Paris, 1938), 134.

[5] Duarte Pacheco Pereira, *Esmeraldo de Situ Orbis* (London, 1937), 98.

[6] John Matthews, *A Voyage to the River Sierra-Leone, on the Coast of Africa*.(London, 1788), 52.

[7] D. Calvocoressi and Nicholas David, 'A new survey of radiocarbon and thermoluminescence dates for West Africa', *J. Afr. Hist.*, XX (1979), 1–20; J. A. Charles, 'From copper to iron – the origin of metallic materials', *Journal of Metals*, XXXI, VII (1979), [illegible]

[8] This type is usually found with vertical walls, although it has also been reported as inclined. See, for example, Leonard Pole, 'Iron-working apparatus and techniques Upper Region of Ghana', *W. Afr. J. Archaeology*, V (1975), 11–39.

[9] The most comprehensive bibliography remains Walter Cline, *Mining and Metallurgy in Negro Africa* (Menasha, Wisconsin, 1937).

[10] This type has been reported in both Nigeria and Togo where the furnace structure was built on a hill or crevice and tubes placed at the base were reportedly used as outlets for the flow of slag: F. Hupfeld, 'Die Eisenindustrie in Togo', *Mitteilungen a. d. deutschen Schutzgebieten* (1899), 175–94; C. B. Bellamy, 'A West African smelting house', *Journal of the Iron and Steel Institute*, LXVI (1904), 99–126.

[11] Peter Schmidt and Donald H. Avery, 'Complex iron smelting and prehistoric culture in Tanzania', *Science*, CCI (1978), 1085–9; Peter Schmidt and Donald H. Avery, 'A metallurgical study of the iron bloomery, particularly as practised in Buhaya', *Journal of Metals* (1979), 14–20.

tuyères within the furnace, allowing preheating of the air flow, could have constituted a significant technological innovation unique to African industries, with the smelted product being an intentional steel. The technique of preheating the air blast was patented in Great Britain only in the second quarter of the eighteenth century,[12] when the iron industry was provided with significant savings in fuel as higher temperatures produced more complete combustion, raised the potential furnace temperatures, speeded up and, therefore, increased production.

Probably the most crucial factor in the smelting process was the charcoal, because it was involved in the very chemistry of the reduction process. Iron industries in other parts of the world, for example Europe and the United States, also depended upon charcoal fuel until the mid-eighteenth century when the adoption of coal began to revolutionize production. In fact, this innovation rescued a threatened industry which had been concerned about dwindling forest resources for centuries. Coal did not have the same potential in West Africa, since the deposits there are recent and of poor calorific value.[13]

Given the necessity of sustaining the iron industry with charcoal alone, other problems remained to be surmounted. For example, not every tree is suitable for the charring process. Even fewer species provide charcoal suitable for the smelting and forging of iron. For these purposes a slow-burning, dense, hard wood, usually with high alkali and silica contents and a granular structure ensuring the packed charcoal's permeability in the charge, must be utilized.[14] Early historical accounts also provide indications of the selectivity of smiths in their choice of fuel. For example, Jobson writes in the 1620s that there was only one kind of red wood from which the charcoal could be made in the Gambia.[15] Ethnographic research would suggest that the number of preferred savanna species is extremely limited: *Burkea africana*, *Acacia* sp., *Prosopis africana*, and *Zizyphus mucronata* have been widely reported as the most suitable.[16]

A seventeenth-century account of travel and trade on the Gambia describes observations of the production of charcoal:

There is a Tree much like our Cornus[17] in England, but very large, which we felled and made ashift to make Charcoal of, which we did thus – We cut off the Boughs;

[12] Charles K. Hyde, *Technological Change and the British Iron Industry, 1700–1870* (Princeton, 1977), 146–7. On the basis of poorer production in summer months, a cool blast was even thought to be desirable.

[13] R. J. Harrison Church, *West Africa: A Study of Environment and Man's Use of It* (London, 1963), 143–4, 476.

[14] J. Charles, D. Livingstone, L. Pole: personal communications. The charcoal also plays a crucial role in stabilizing the smelting temperature: Roger Dechamps, 'Le Zizyphus, combustible des premiers foyers de fonte du fer du Rwanda', *Africa-Tervuren*, XXIV, iv (1978), 1–4.

[15] Richard Jobson, *The Golden Trade or a Discovery of the River Gambra, and the Golden Trade of the Aethiopians* (London, 1623), 164.

[16] See, for example, Dechamps, 'Le Zizyphus'; F. R. Irvine, *Woody Plants of Ghana* (London, 1961); Candice L. Goucher, 'Notes on Iron-working in the Begho Area', unpublished field notes, University of Ghana, Legon and UCLA; Dennis H. Wood *et al.*, 'The socio-economic and environmental context of fuelwood use in rural communities of developing countries: issues and guidelines for community fuelwood programs', Paper submitted to the U.S. Agency for International Development, Bureau for Program and Policy Coordination, 1979.

[17] Cornus is the botanical genus. Cornel-wood, the wood of *Cornus mascula*, was celebrated for its hardness and toughness.

for we wanted a saw, and therefore cou'd not meddle with the body of the tree, and cut them in short pieces; There was digg'd a good large pit or hole in the ground about a yard wide, and so deep, or deeper; in the bottom we kindled a fire, and filled it with wood, and when it was well burnt threw earth upon it and damp'd it, and when it was cold we took out the Coals.[18]

This description is quite similar to the charcoal-making process observed recently in the village of Hani, Ghana.[19] In this case, two twenty-foot trees (*B. africana*) were felled by two men, with the charring process taking a full ten days, during which periodic monitoring was necessary to maintain the proper conversion conditions, i.e. a constant burning rate.

Of considerable ecological importance is the fact that the species of tree which are suitable for charcoal also tend to be slow-growing. For example, replacement of the two *B. africana* trees will require more than twenty years of growth. The thousands of trees exploited by one iron-smelter probably could not have been replaced during his lifetime.[20] Production estimates from a single industrial site in Ghana (Dapaa, *c.* 1400–1700) require a staggering number of trees to have been used for fuel.[21] The Dapaa site consists of twenty-six slag mounds confined to an area of about one hectare. The arc-like configuration of the mounds conforms to that described by Pole for the Upper Region where it occurs as a result of the placement of several furnaces in a line when smelting.[22]

Using the data obtained from the Dapaa site, calculations for the volume and weight of the slag were made in order to estimate the total iron production and, ultimately, the amount of fuel required. Those estimates are summarized in Table 1 below.

The estimated figure of more than 300,000 trees is considered to be on the conservative side, but in any case the real impact of the technological system on the environment was undoubtedly severe. In medieval Europe, the provision of fuel for a single smelting furnace could level the forest for a radius of one kilometre in a mere forty days of operation.[23] Even in a heavily forested area of Africa, twelve cords of wood (not charcoal) could require the exploitation of more than an acre.[24]

Hopkins considers the dry weight of a savanna ecosystem's total vegetation

[18] San Marino, California, Huntington Library, James Brydges: 'A collection of remarkable papers' (England, *c.* 1735), ST. 28. This transcription appears in several subsequent printed sources: David Gamble, *Bibliography of The Gambia* (Boston, 1979), item 461.

[19] Goucher, 'Notes'. I am grateful to Professor Merrick Posnansky, U.C.L.A., and the National Geographic Society for the opportunity to participate in the 1979 season of excavations at Begho. The experimental production of charcoal would not have been possible without the advice and participation of Mr Amponsah, Hani. All botanical identifications were generously provided by Professor John Hall, University of Ghana, Legon.

[20] Paul W. Richards, 'The tropical rain forest', *Scientific American*, CCXXIX, vi (1973), 58–67, p. 65.

[21] Merrick Posnansky, 'Hani and Debibi', *Nyame Akuma*, VII (1975), 16–23; Goucher, 'Notes', 13–23.

[22] Pole, 'Iron-working'.

[23] Jean Gimpel, *The Medieval Machine* (New York, 1976).

[24] Louis James Mihalyi, 'Charcoal from the Zambian forests', *Geographical Magazine*, XLV, iii (1972), 212–18.

Total volume of slag[a]	1,962·29 m^3
Total weight of slag[b]	209,611·82 kg
Total trees required[c]	307,431 individuals[d]

[a] Estimated volume is based on topographical mapping by M. Posnansky and P. de Barros.

[b] This estimate uses the ratio 106·82 kg/m^3, experimentally determined by measured sampling.

[c] The assumptions involved here include density of species (*Burkea africana* = 60 lb dry weight), identification of species by ethnographic analogy, and estimates of the efficiency of the charring process (10 per cent) and the smelting process (with fuel to slag ratio = 4:1).

[d] Other factors remaining unassessed include: placement of tuyères, number of tuyères, length of smelt, composition of ore used, and size and frequency of charges.

to be about 136 kg (minimum) per acre per year, whereas the number of individual trees (all species) per acre of derived savanna can be as low as 225.[25] This suggests the great vulnerability of forest fringe and savanna systems, as well as the potential magnitude of impact of technology on it.[26] In addition, the overexploitation of resources would not have been limited to enclosed industrial areas but would have been part of a regional phenomenon. Climatic evidence for the later Iron Age centuries suggests the onset of drier conditions by the nineteenth century, preceded by more than a century of severe droughts beginning in the 1680s.[27] An earlier decline may have spanned the fourteenth and fifteenth centuries,[28] while the last millennium should be viewed as a longer period of severe change.[29] Even intermittent centuries of lesser rainfall would have severely slowed the natural rate of replacement of species and re-establishment of the original forest which had been exploited.

The area of most intense iron production[30] includes the region now considered by some scientists to be 'derived savanna',[31] that is, a type of savanna created by human intervention. This pattern of degradation has been identified in the iron-producing areas referred to by several sixteenth-century historical accounts. For example, the area of Fouta Djallon was originally woodland with some inlets of lowland rain forest.[32] Pressures to supply charcoal would have demanded the eventual exploitation of younger species, thus interfering with the replacement of cut trees and leading to rapid deterioration of the soil and to soil erosion problems.[33] Ultimately the entire

25 Brian Hopkins, *Forest and Savanna* (Ibadan, 1974), 81, 105.

26 This may also explain why archaeological sites reflecting transitions from Early Iron Age to Late Iron Age are relatively few. The ecology of an area probably could not sustain a sequence of prolonged Iron Age exploitation.

27 Sharon E. Nicholson, 'The methodology of historical climate reconstruction and its application to Africa', *J. Afr. Hist.*, xx, i (1979), 31–49, p. 47.

28 *Ibid.* 28.

29 M. Talbot, personal communication.

30 See Cline, *Mining*, for widespread distributions in West Africa.

31 Hopkins, *Forest*, 79; Peter M. Ahn, *West African Soils* (Oxford, 1970), 114–17.

32 Church, *West Africa*, 66.

33 Wood, *et al.*, 'The socio-economic', 78.

agricultural community would be made vulnerable to destructive, long-term ecological consequences.[34]

These technological pressures toward deforestation would also have been exacerbated by the European exploitation of forests in West Africa. In particular, camwood (*Baphia nitida*) and the 'African teak'[35] played an important role in the trade and deforestation of the Upper Guinea Coast from the late eighteenth century.[36] In a contemporary account of the area of Sierra Leone, John Matthews suggests that the African communities also depended on the same, increasingly scarce, species for fuel.[37] By the nineteenth century, timber was being collected and transported from areas up to four days' journey from the river.[38] By colonial times, the French were forced to enact legislative protection against further over-exploitation of the environment in Senegal and to provide for systematic replanting.[39] As far as the fate of the African industries is concerned, this attempt at management came too late. A mid-nineteenth-century account of iron-smelting confirms the widespread uncertainty in obtaining charcoal for the furnace.[40] The effects of ecological degradation were being experienced by the African smelting industries, just as they had been by European iron industries before the use of coal.

The presence of the ecological factor in the decline of the smelting industries suggests the need for re-examining the effects of trade. The importance of West African production and trade of iron can be documented in information collected as early as the second half of the fifteenth century. Cadamosto wrote about the region around the Senegal River that the [Jolof] people there '...obtain iron from the kingdom of Gambra of the Blacks beyond, but they cannot make steel'.[41] Hardly the words of a qualified metallurgist, his comments are perhaps more useful as an indication of the economic sensitivity on the part of the early travellers and traders to what was already desired and being traded along the coast. Already by 1519, iron was listed among the trade goods 'best received' as far as the Volta.[42] The

[34] Iron technology is particularly effective for clearing grasslands. The prevention of secondary forest regeneration would aid the spread of wild grasses and create the need for intensification of agricultural practices (burning, shortened fallow, further dependence on iron technology, etc.). See E. Boserup, *The conditions of agricultural growth* (Chicago, 1965), for one model of this interaction.

[35] Woods known as such were provided by *Oldfieldia africana*, among other species. D. C. Dorward and A. I. Payne, 'Deforestation, the decline of the horse, and the spread of the tsetse fly and Trypanosomiasis (*Nagana*) in nineteenth-century Sierra Leone', *J. Afr. Hist.*, XVI, ii (1975), 239–56, p. 247. As timber, such hardwoods found large markets in the greatly deforested Europe; camwood was used in producing dye. Other minor local industries such as keg-making (for the storage of gunpowder) would have added to the demand for hardwood species: W. Rodney, *A History of the Upper Guinea Coast 1545–1800* (Oxford, 1970).

[36] Dorward and Payne, 'Deforestation', 247; Rodney, *History*, 158.

[37] Matthews, *A Voyage*, 39.

[38] Dorward and Payne, 'Deforestation', 248.

[39] It was decreed 20 July 1900: 'Nul ne peut entre prendre une exploitation forestière dans les bois du domaine s'il n'est muni d'une autorisation de Gouverneur ou de son délégué': Jacques Meniaud, *Haut-Sénégal-Niger (Soudan Français)* v. 2: *Géographie Economique* (Paris, 1912), 300–2.

[40] John Duncan, *Travels in Western Africa, in 1845 & 1846* (London, 1847), 2, 132.

[41] G. R. Crone, editor, *The voyages of Cadamosto and other documents on Western Africa in the second half of the fifteenth century* (London, 1937), 33.

[42] P. E. H. Hair, 'Some minor sources for Guinea, 1519–1559: Enciso and Alfonce/Fonteneau', *History in Africa*, III (1976), 19–45: p. 34.

area of Sierra Leone at the beginning of the sixteenth century is described variously as having 'much iron of good quality'[43] and 'the best and mildest iron in all the world'.[44] Richard Jobson, travelling down the Gambia in 1620–1, remarked: '...whereas we thought our Iron would have been greedily desired, we found it not so...'.[45]

It appears that the demand for European iron lessened as one moved inland from the coast.[46] However, even along the coast, Europeans were interlopers of sorts in an established indigenous trade in iron. Portuguese sources record an Upper Guinea iron bar about 9 inches long, two fingers thick at one end, and three fingers at the other.[47] While this product is described as 'forged', not just smelted, the direct production of long, narrow bars using moulds was later observed elsewhere in West Africa.[48]

Subsequent versions of such bars made in Europe would seem to have been remarkably similar in overall dimensions. The Portuguese bar reported in the late seventeenth century was 18 palmos (just over 13 feet in length) and could be divided into eighteen lesser lengths of 9 inches.[49] The French bar traded in the Gambia in 1686 was recorded as being 9 ft long, 10 in. wide, and 4 in. thick.[50] These were cut into fifteen parts, each 7 in. (called 'pattes'). Each of these was subsequently cut into three parts, producing three pieces suitable for working into a spade. In this way, one such bar could produce forty-five spades to be fashioned from a shape of iron, 10 in. by 4 in. by 2·3 in. or 7 in. by 4 in. by 3·3 in., depending upon which way the forms were subdivided. The use of iron 'currency' forms as blanks or templates was widespread.[51] In Togo, where the smelting industry survived well into this century,[52] iron and bloom were traded in the form of roughed-out hoe shapes.

A smaller iron currency, called *nnaabo*, has been reported from Ghana.[53] Like the other small, in some cases miniaturized, forms found in great quantities across West Africa, it would appear to be a rather late development. Dutch correspondence dated 1635 requested 'staven' three fingers thick and, of crucial importance to the request, 11 ft to 12 ft in length. Later, Dapper mentioned 'staven yzer tot de lengte van een, twee, en drie voeten' being traded by the Dutch,[54] although this could be a reference to divided lengths. The seventeenth-century letter above also adds the piece of information that the weight was of concern and probably standardized; about thirty-three of these bars should weigh 1000 lb.[55] Ryder confirms this weight of bar being

43 Pacheco Pereira, *Esmeraldo*, 128.

44 Hair, 'Some minor sources', 29.

45 Jobson, *The Golden Trade*, 119.

46 Rodney, *A History*, 186. This phenomenon may reflect the impact of climatic differences between coastal and savanna regions: Nicholson, 'The methodology', 47.

47 Quoted in Rodney, *A History*, 194.

48 Duncan, *Travels*, 133.

49 Rodney, *A History*, 194; Rodney's conversion of this unit obtains the value 13 ft 6 in., but this cannot have been based on the Portuguese palmo reckoned at 0·22 m.

50 J. B. Labat, *Nouvelle relation de l'Afrique occidentale* (Paris, 1728), 7.

51 Percy Knauth *et al.*, *The Metalsmiths* (New York, 1974), 94.

52 Togo has been particularly well documented by German observers in the late nineteenth century.

53 R. P. Wild, 'Iron disc currency from Ashanti', *Man*, XXXVI, art. 99 (1936), 78–9.

54 Olfert Dapper, *Naukeurige beschrijvinge der Afrikaensche gewesten* (Amsterdam, 1676), 419.

55 Pieter van den Broecke, *Reizen naar West-Afrika, 1605–1611* (The Hague, 1950); A. F. C. Ryder, *Benin and the Europeans* (New York, 1969).

traded in 1646 further down the coast at Benin, although by the eighteenth century a smaller version (weighing 14–18 lb, possibly halved) appears among the goods traded there.[56]

Jobson describes a local smith enlisted to cut bars to the proper length of 12 inches:

> ...and therefore sending for him, he comes to the water side, bringing his shop with him that is his bellowes, and a small Anvill, which hee strikes into the ground under a shady tree...his boy blowing the bellowes, that lye on the ground, the nose of them, through a hard earth, made of purpose with a hole in it, and in this manner with a hammer and a toole, they cut it for us...[57]

This must have been a familiar scene, since smiths were in high demand. Factors' requests for their skills were common by the seventeenth century.[58]

There are several indications that the imported iron required different iron-working techniques.[59] European bar iron was wrought iron with a low carbon content (i.e. less than 1 per cent).[60] This was produced from charcoal pig iron, originally manufactured in blast furnaces and of varied quality, which was then converted in the forge to a higher quality product. This would have been a malleable iron but certainly not a hard iron. On the other hand, West African smelters were producing a product which was steel-like, if not actually a deliberate steel. This conclusion is suggested by the historical accounts which reflect the praise of the earliest Europeans encountering West African technology and is confirmed by archaeological evidence. Examination of a near-complete tuyère excavated at Dapaa revealed that the extent of vitrification (indicating intense heat) was such as to suggest extension of the tuyères more than 10 cm into the interior of the furnace, thus confirming a technology which utilized preheating of the air-blast.[61] Electron probe microanalysis and scanning electron microscopy with energy dispersive X-ray techniques were used to characterize the slag and iron from the site of Atwetwebooso (literally 'the place of iron stones'), an Early Iron Age site in the Dapaa–Begho area. This work has indicated that a liquidus temperature of 1250–1300 °C was reached in the reduction process.[62] In addition, examination of the tuyère ends showed evidence of vitrification, but to a lesser extent than indicated for the later Dapaa material.[63]

Thus, the full development of the preheating (fuel-conscious) innovation in the first millennium may also have been a response to the ecological degradation and deforestation described above. The accomplishment of

[56] Op. cit. [illegible], 333.

[57] Jobson, *The Golden Trade*, 165.

[58] K. G. Davies, *The Royal African Company* (London, 1957), 178–9, quotes a request at Cape Coast for both smiths and bellows, including the hides to mend them. The description of bellows could fit the African types of the region. Bellows for goldsmithing were frequently imported from Europe.

[59] H. A. Gemery and J. S. Hogendorn, 'Technological change, slavery, and the slave trade', in C. Dewey and A. G. Hopkins, eds, *The Imperial Impact: Studies in the Economic History of Africa and India* (London, 1978).

[60] Hyde, *Technological Change*, 7.

[61] Preheating of the airblast has also been reported from other parts of Africa. Schmidt and Avery, 'Complex Iron-smelting', have discussed at length the significance of this technological innovation for East Africa.

[62] Judith Todd, 'Studies of primitive iron technology', Ph.D. dissertation (University of Cambridge, 1976).

[63] M. Posnansky, personal communication.

preheating would not only have allowed for higher, sustained temperatures, but would have conserved fuel. During a stay at Kamalia, Mungo Park observed a tall shaft furnace and the process of smelting iron using twenty-one tubes of clay, the ends of many having been vitrified by the furnace heat.[64] Some of the iron which Park observed being smelted was subsequently hammered into small bars, 'about a foot in length and two inches in breadth', and traded.[65] A high-quality charcoal, a preheated airblast, and an extension of the furnace shaft walls would have created the conditions needed for the production of steel.

The handling of a West African 'steel' and low-carbon European bar at the forge would have required quite different techniques. The harder steel product must be reworked by the technique of annealing, in which the cooling process is slowed, often by burying or covering the heated steel in sand or ashes. Quenching, usually by plunging the heated metal into water, could be used to harden the object; this technique would have been alternated with annealing to achieve the specific combination of qualities needed for the object being made.[66] On the other hand, the bar iron would have been recarburized, i.e. more carbon added to the alloy, by reheating it for several hours at a higher temperature in the presence of charcoal, the iron perhaps packed in a clay envelope with the charcoal.[67] This 'steeling' would have produced a hardened and more brittle surface only.[68] Thus, Matthews, writing in 1786, documents a preferential use of the two kinds of iron: 'In the interior country, south of Sierra-Leone, they have a white iron, very malliable (*sic*), of which they make knives and sabres; and esteem it preferable to European iron for everything but edge tools.'[69]

West African smiths were also sensitive to the variations in the quality of the iron bars. Jan De Paauw, resident at Elmina Castle in 1709, suggested that the imported iron bars which he was requesting would be good merchandise for trade only if of good quality.[70] The quality varied according to ore source (Swedish iron had an excellent reputation) and as a result of the unpredictable performance of the European smelting furnace. As late as 1754, one British smelter lamented 'a furnace is a fickle mistress'.[71]

Finally, the selectivity in consumption of the European bar iron was also a function of seasonal demands. De Paauw added that '... the latter [iron bars] are seasonal commodities, in particular for the months of March and April'. A few years later, a similar observation was recorded elsewhere on the coast:

> Counted the goods in the storehouse and found only 1,400 basins and 39 irons bars. These iron bars and basins are much in demand just before and during the bush clearing season, but now they are not in demand before the next sowing season.[72]

64 Mungo Park, *Travels in the Interior Districts of Africa* (London, 1799), 284.

65 *Ibid.* 348–9, 26.

66 Research conducted in Brawhani near Dapaa indicated a specialized vocabulary for techniques such as quenching, with linguistic borrowings for the non-traditional tools and processes.

67 A temperature differential of at least 250 °C is involved: Nicholas Van der Merwe, *The Carbon-14 Dating of Iron* (Chicago, 1969), 22–4.

68 James Maddin *et al.*, 'How the Iron Age began', *Scientific American*, CCXXXVII, iv (1977), 122–31.

69 Matthews, *A Voyage*, 52.

70 Albert Van Dantzig, *The Dutch and the Guinea Coast 1674–1742: A collection of documents from the General State Archive at the Hague* (Accra, 1978), 141.

71 Hyde, *Technological Change*, 9.

72 Van Dantzig, *The Dutch*, 208.

There exist a number of obstacles to the quantification of iron imports. Portuguese records must remain particularly suspect since shippers were under the Portuguese edicts totally banning the export of iron at times, and even when the privilege of shipping iron was granted, a payment in addition to that for the trading lease was required. Convincing evidence against the claim that European iron imports were uniformly 'cheap' is that the Portuguese lançados were able to purchase West African iron produced inland and resell it at a profit on the Guinea coast.[73] The question of uniformity also presents a possible source of confusion in the utilization of trading accounts, as does the practice of dividing bars along the coast.[74]

Figures compiled by Newbury on the basis of customs records for Great Britain's portion of iron exports in the nineteenth century indicate a steady annual increase from 209 tons in 1812 to 1,667 tons two decades later.[75] By the end of the century, between 5,000 and 10,000 tons of iron were being shipped annually from British ports to 'Western Africa'; compared to Curtin's compilations from eighteenth-century French (Senegalese) sources which indicate a doubling in quantity between 1718 and 1752, the nineteenth-century increases were exponential. The late nineteenth century also witnessed a surge in the diversity of the forms of iron being traded in West Africa.[76] This is reflected in the 1853 fixing of variable French tariffs on iron, previously applied uniformly, according to no fewer than twenty-four categories of metal.[77]

The later centuries also are marked by more effective penetration of imports in the direction of the savanna regions. The scanty archaeological work on coastal smelting sites suggests, however, abandonment centuries earlier.[78] While this has traditionally been explained in terms of a trade-impact model, ecological factors were also operating, perhaps in a variant chronological and climatic framework.[79]

The scarcity of suitable charcoals for smelting demanded from African industries both economic and technological responses. In the scenario presented above, ecological conditions must clearly be considered as a part of the total technological environment. The increased reliance on imported iron bars thus should be viewed as a result of two developments: an increasingly industrialized Europe's ability to compete; and a series of African ecological crises. The correlation of these two trends is substantiated by the evidence of the exploitation of African natural resources, particularly hardwood timber. A series of climatic stresses, and the prolonged over-

[73] Rodney, *A History*, [illegible].

[74] One measure of the centrality and uniformity of the iron bar in the coastal transactions is the adoption of the 'bar' as the currency of account (although the exchange rates between the bars and European currencies were different for each commodity): Philip Curtin, 'Pre-colonial trading networks and traders: the Diakhanke', in Claude Meillassoux (ed.), *The development of indigenous trade and markets in West Africa* (London, 1971), 236.

[75] Colin W. Newbury, 'Credit in early nineteenth-century West African trade', *J. Afr. Hist.*, XIII, i (1972), 83–4.

[76] P. Curtin, *Economic change in precolonial Africa: Supplementary evidence* (Madison, 1975), 92.

[77] Harry Scrivenor, *History of the Iron Trade* (London, 1854), 200.

[78] D. A. Penfold, 'Excavation of an iron-smelting site at Cape Coast', *Trans. Hist. Soc. Ghana*, XII (1972), 1–15.

[79] Nicholson, 'The methodology'.

exploitation of forest areas by smelters relying on charcoal fuel were exacerbated by the disruption of traditional patterns of land use through European intervention, which constituted a kind of ecological imperialism.[80] Initially the European iron import was neither 'cheaper' nor 'purer', rather simply a necessity as the continuation of local smelting faced increasingly severe fuel shortages. The technological responses, such as increased furnace height, adaptive variations in the angle and number of tuyères, and the development of preheating techniques, provided advances in fuel conservation and manufacturing efficiency. When these technical advances could not overcome the ecological challenge, the only viable response by African metalworking industries was an increased reliance on the industrial products of Europe.

SUMMARY

Archaeological evidence and historical accounts have been used to examine the impact of trade and ecology on the decline of West African iron industries. Environmental changes including an increasingly desiccating climatic shift and widespread deforestation as a direct result of fuel procurement over centuries of iron-smelting and European coastal exploitation, seriously affected the survivability of these industries. While the increasing importation of European iron bars and other manufactured goods necessitated a certain amount of technological innovation, the only viable long-term response and adaptation to the ecological devastation became the increased reliance on imported supplies of iron.

[80] Roy A. Rappaport, 'The flow of energy in an agricultural society', *Scientific American*, CCXXV, iii (1971), 116–32.

C. L. Goucher, 'Iron is iron 'til it is rust: trade and ecology in the decline of West African iron-smelting', *Journal of African History*, vol. 22, 1981, pp. 179–89.

Leonard M. Pole

IRON PRODUCTION IN WEST AFRICA FROM THE SEVENTEENTH TO THE TWENTIETH CENTURIES

IN her recent article[1] Candice Goucher shows the need for a more detailed examination of the decline of iron industries in West Africa and advocates a model in which ecological factors are emphasized. These centre on the enormous consumption of charcoal by the iron-smelting process, leading to partial de-forestation, in combination with a 'desiccating climatic shift', seriously endangering the future of fuel reserves. The increasing penetration of European trading institutions from the seventeenth century onwards helped to seal the fate of the industry. While as the outline of a general picture there is little to quarrel with in this model, it would be of little use when applied to a particular local situation. In order to produce a more comprehensive and flexible model of the transition from an industry based on local smelting technology to one dependent on recycled waste products or imported raw materials it is necessary to take a greater range of factors into account. I propose in this paper to discuss the more significant of these factors relating to the competitiveness of locally produced iron vis-à-vis the imported product at the point of contact. This involves consideration of labour input, price, the ritual value of local iron and the social organisation of iron-working groups.

Anybody who has looked in detail at the way iron was produced in West Africa would be likely to agree that it was a complex, skilled, lengthy and very labour-intensive process. No one to my knowledge has attempted to quantify it in detail. Yet I believe some degree of quantification is both possible and useful since it gives some indication of the range of variation of involvement by labour in different kinds of procedure. However, it is not a simple matter to assess how much labour was essential to the work in any one place. Few descriptions in the literature are sufficiently detailed to allow reliable figures to be used. I have tried to obtain figures from my own field-work in northern Ghana and I am therefore aware of the difficulties;[2] these figures are summarized in Table 1. Descriptions of the procedures have been given elsewhere.[3] The figures apply to all activities associated with smelting including building the furnace and other pieces of apparatus, making charcoal, obtaining and preparing the ore as well as smelting itself. In each

[1] C. L. Goucher, 'Iron is iron 'til it is rust', *J. Afr. Hist.* XXII (1981), 179–89.

[2] This field-work was carried out while I was Assistant Keeper at the National Museum of Ghana, Accra. Quantification was made more difficult by the fact that the activities they related to had not been regularly performed for 25 years. Every effort was made by the smiths to repeat exactly what they used to do, but some variation was inevitable. This has been allowed for in the figures.

[3] L. M. Pole, 'Iron-smelting in Northern Ghana', *Nat. Mus. Ghana, Occasional Paper*, VI; L. M. Pole, 'Iron-smelting procedures in Upper Region of Ghana', *J. Hist. Metallurgy*, VIII (1974), 21–31; L. M. Pole, 'Account of an iron-smelting operation at Lawra, Upper Region', *Ghana J. of Science*, XIV (1974), 127–36.

Table 1. *The labour of smelting*

Village	North-west Ghana			South-west Ghana
	Lawra	Tiza	Chiana	Akpafu
Man-hours per smelt	150	178	230	100
Man-hours per load of charcoal	48 (32 %)	75 (42 %)	109 (47½ %)	60 (60 %)
Weight of bloom	*c.* 4·5 kg	*c.* 6 kg	*c.* 4·5 kg	4·5–24 kg
Weight of usable iron per smelt	*c.* 3 kg	*c.* 4·5 kg	*c.* 2·5 kg	3–16 kg
Man-hours per kilogram of usable iron	50	40	93	8–33

of the three villages in north-west Ghana a small shaft between 130–180 cm high was used, with one or two pairs of bellows. It can be seen from the Table that even within one small area within which details of the processes differed little there was considerable variation in labour-input. The figures also emphasize the great proportion of time smiths in this area spent making charcoal. A most important point to make in this context, however, is that the iron-workers themselves did not put any value on their time; most men in this area in the dry season had little to do unless they were full-time specialists (e.g. weavers, xylophone-makers). Only one or two iron-workers could be put into this category. For the majority of those who helped in the process, if they were not doing this work, they would not be doing much else. Smelting was never done in this part of Ghana all the year round. Farm work always took precedence. Although there is some evidence to indicate that the smelting processes which took less labour per kilogram of iron produced cheaper iron, little significance can be placed on the labour element in establishing a price.

What of figures appropriate to other forms of furnace? Since a large amount of labour was spent in attending the furnace, especially operating the bellows, in northern Ghana, a process in which the furnace could be left unattended ought to have been less expensive of labour. Frequent additions of ore and charcoal and the almost continuous tapping of slag, together with pumping the bellows, all contributed to keeping four or five persons busy throughout the day at Lawra, Tiza and Chiana. The procedure was quite different when larger furnaces were used, as at Akpafu in south-eastern Ghana. There, once the charge had been burning for some time the operators could leave the furnace unattended, except for slag-tapping. One person was appointed to keep watch over the furnace, to make sure the air inlet tubes did not become blocked and that the charge was not burning too fiercely. The Akpafu furnaces produced more iron per smelt than those of northern Ghana although there is a wide diversion in the published reports. A single piece of bloom seen in 1972 weighed 24 kg. The amount produced during a demonstration smelting in September 1973 weighed only just over 3 kg, but this reconstruction contained a number of inaccuracies, as discussed elsewhere.[4] Hupfeld's report[5] indicated that at the end of the nineteenth century blooms weighed about $4\frac{1}{2}$ kg. One bloom seen in the neighbouring village of Santrokofi in 1972 weighed 7·2 kg, but that appeared to contain a large amount of slag. To an even greater extent than in northern Ghana a major component of labour input was spent on making charcoal, about 60 per cent. Time spent making the furnace, though extending over a six-month period, was negligible when taken in the context of the overall life expectancy of the structure which was about 20 years. The labour cost per kilogram of usable iron was significantly below that relating to the small furnaces, as the Table 1 indicates.

It is unfortunate that there are few other descriptions which supply quantifiable data. Nadel's account from Nupe gives almost enough information,[6] but some assumptions have to be made. The Nupe furnace was built into the side of a pit and used the long through-the-shaft tuyeres found also in north-eastern Nigeria and adjacent parts of Cameroon.[7] Nadel says one worker was employed as a pumper, but relieved every hour, and two or three others stoked and fed the furnace and removed slag. In addition ore was broken up with hammers. It appears then that five or six men worked at the furnace to produce a bloom 'the size of a large yam fruit'[8] in five or six hours. A bloom that size could have weighed up to 8 kg. Charcoal making was also described, although for the blacksmiths' forge rather than the furnace. Two or three men were involved. Inferring from information from northern Ghana, where the volume of the furnace is similar, I have assumed that this description related to sufficient charcoal for two smelting operations. It occupied the men for two days. In addition, a certain amount of time was taken up with making a new long tuyere for each smelt and a clay plug to fit in the base of the furnace. To make allowance for the inaccuracies consequent upon these assumptions I have allowed for a range of between 50 and 100 man-hours per smelt, which produces a range of six to twenty man-hours per kilogram of iron. These figures contrast sharply with those from northern Ghana, even though both operations utilized bellows. The difference was mainly due to the speed of the smelting in Nupe. Other descriptions of a similar type of shaft in use included even shorter times,[9] down to one hour in one description; this may therefore be a concomitant feature of the design. Although these figures must be used with extreme caution, they do lend support to the impression that the labour consumption of the nothern Ghana furnaces was unusually high and they add to the emerging view of the great variation in standards of activity within this industry.

[4] L. M. Pole, 'Iron-working in the Togo Hills; from technology to oral tradition' (forthcoming).

[5] F. Hupfeld, 'Die Eisenindustrie in Togo', *Mitt. von Forschungsreisenden und Gelehrten aus den Deutschen Schutzgebieten*, XII (1899), 175–94.

[6] S. F. Nadel, *A Black Byzantium* (London, 1942), 259–69.

[7] R. Gardi, *African Crafts and Craftsmen* (New York, 1969), 15–23; H. Sassoon, 'Iron-smelting in the Hill Village of Sukur, North-Eastern Nigeria', *Man* LXIV, no. CCXV (1964); J. H. Vaughan, 'Nkyagu as artists in Marghi society' in W. d'Azevedo (ed.), *The Traditional Artist in African Societies* (Bloomington, Indiana, 1973).

[8] Nadel, *Black Byzantium*, 263.

[9] Sassoon, 'Iron-smelting', 176.

The more labour-intensive procedure may also be a function of the organization of smelting activities on a part-time basis. It would be tempting to suggest that where competition for labour was on the increase it would have affected part-time smelters more rapidly than full-time smelters. But here one point requires some emphasis; the labour was not usually supplied by persons who were able to choose between different kinds of work. In northern Ghana they were usually relatives of those in charge of smelting. They would therefore be constrained to continue to supply labour cheaply for smelting when they could be earning more elsewhere, or indeed participating for the first time in a form of economy which rewarded labour with cash rather than payment in kind.

The way in which labour was organized is of crucial importance in discussing the replacement of local with imported iron, but it has been wholly neglected. It seems obvious that where those who smelted iron were not also the producers of iron objects, the economic pressures brought about by the introduction of an alternate source of iron were different from those where smelting and smithing were performed by the same group. In Nupe, for instance, anyone could smelt iron but only members of certain families were allowed to become blacksmiths. This division also occurred in other places where the same type of furnace was used.[10] Thus smelters were cut off from the marketplace for the finished products to which their labour had contributed. I suggest this tended to make them most vulnerable to competition from other sources of iron. Smiths who took their iron bloom could have switched to buying imported iron as soon as it became advantageous for them to do so. Unfortunately there is no information to indicate to what extent smiths in Nupe were able to decide on a purely economic basis where to obtain their raw material.

This kind of consideration would not have arisen in a community where the smelting and smithing work was performed by the same group. Imported iron would have been seen as a substitute for labour rather than as an alternative raw material. Thus, even if the smelter/smiths had the capital to buy the imported iron, the proposition would not have been attractive to them unless there were some pressure on their labour resources. This pressure could have come from the development of other markets for the labour, which may well have accompanied the arrival of a European presence in the area. Equally well the pressure could have arisen from the consistently high demand for iron which appeared to exist throughout the period – Williams's 'malignant iron hunger'.[11] In such a situation, anything that cut down production time would have increased profit. By giving up smelting work and concentrating only on smithing, the iron workers provided themselves with the time to earn extra income. Since imported iron was generally thought to be of inferior quality, producing tools which lasted less long than those of local iron, its use in itself served to further increase demand.

From an examination of the present-day methods used by smiths in north-west Ghana, the extent of this extra income can be gauged. In 1973 a hoe-blank could be bought in Lawra for one cedi. This was a flat piece of iron with a tang. It required conversion for use with a curved wood shaft, for which work a smith charged 20 or 30 pesewas (0·20–0·30 cedi). Alternatively

[10] Vaughan, 'Nkyagu', 175.

[11] D. Williams, *Icon and Image* (London, 1974), 72.

the smith obtained scrap iron and made hoes which he sold for the same overall price of 1·20–1·30 cedi. In the first instance the work would have taken about thirty minutes, in the second about two hours. In either case the smith could earn about four cedis in a day spent working full-time in the forge. It would have been impossible for them to have earned the equivalent of that kind of money regularly when smelting activities took up at least half the time available in the dry season. In Jefisi, 70 miles to the east of Lawra, one blacksmith claimed to have earned 1,000 cedis in a year, provided he could also work in the wet season. This was much higher than the average yearly wage anywhere in the country in 1973; the labourer's daily rate then was one cedi and a clerk in an office in Accra was earning 600–800 cedis per annum after training.

Another important consideration in determining the earning potential of blacksmithing was the quality of the iron. Tools made from local iron were usually said to be harder and longer-lasting than those made from imported or scrap iron. Farmers in northern Ghana complained that three hoes were needed to complete work on their farms in the early 1970s whereas a hoe made of local iron would have lasted more than two seasons. It was the low quality of imported iron which led the smelters of Sukur in north-east Nigeria to start smelting again in the early 1960s.[12] Fagg also reported that local iron was much harder than imported iron.[13] Bascom indicates that local iron is preferred by carvers at Meko in Yoruba for knife blades.[14] The reason for the widespread preference for local iron lay mostly in its method of manufacture,[15] but there was an element of nostalgia for old traditions involved as well which should not be ignored. Economically the implication of this difference in quality was that up to six times the number of hoes were needed each year for the same population, and this at a time when smiths who had also been smelters had more time at their disposal to make them. This acted as a powerful incentive against taking up smelting again, even if the farmers might have regretted the change. The important point to note is that it was only where the smelting was done by farmers, as in Sukur, and not by the smiths, that smelting was liable to be resurrected. Such resurrection could have only taken place within 50 years at the most; after that some of the specialized knowledge would be forgotten.[16] The choice in this kind of community was between smelting and farming, not between smelting and smithing. The impetus for change would not just have been different in these two communities, but in some cases actually opposed.

Given that there were some groups which were highly resistant to the change in the source of iron, what other factors militated in favour of the imported product? In my opinion, it was not until imported iron was cheaper than local iron by a factor of six that the preference for the local product was

[12] Sassoon, 'Iron-smelting', 174.

[13] W. Fagg, 'Iron-working with a stone hammer among Tula of Northern Nigeria', *Man* LII, no. lxxvi (1952).

[14] W. Bascom, 'A Yoruba master carver: Duga of Meko' in d'Azevedo, *The Traditional Artist*, 78, fn. 5.

[15] N. van der Merwe, 'Production of high carbon steel in the African Iron Age: the Direct Steel Process', *Proc. 8th PanAfrican Cong. of Prehistoric and Quaternary Studies* (1977).

[16] Pole, 'Iron-working in the Togo Hills'.

Table 2. *The prices of iron*

Place	Date	Origin of iron	Weight	Unit	Currency	Bloom price per tonne	Iron bar price per tonne	Hoe price per tonne	Source
N.W. Ghana									
Lobi	1920s	Lobi	3–400 g	Hoe	Cowries	—	—	£100–133	Labouret[20]
Lo-Dagaa	1930s	Lo-Dagaa	—	Bloom	Cowries	£45–51	—	—	Pole
Lo-Dagaa	1930s	—	—	Hoe	Cowries	—	—	£214–400	Pole
N. Togo									
Bassari Bidjabe	1898	Banjeli?	20 kg	Bloom	Cowries	£12–20	—	—	Hupfeld[21]
nr. Banjeli	1910	Banjeli?	500 g	Hoe	—	—	—	£120–140	Klose[22]
Banjeli	1909	Banjeli	—	Bloom	Cowries	£15–30	—	—	Frobenius[23]
Tchitchao	1909	Banjeli	—	Bloom	Cowries	£100	—	—	Frobenius[23]
Binaparba	—	—	3–500 g	Hoe	Cowries	—	—	£130–200	Frobenius
S. Guinea									
Guro, Gagou, Bete	1900–10	Touba	100 g	'Somba'	Francs	—	£20–190	—	Sundstrom[24]
Lola	—	—	2–300 g	Bar	Francs	—	£60–95	—	Sundstrom
Lola	late 19 cent.	—	1–150 g	'Guenze'	Francs	—	£85–125	—	Sundstrom
Konian, Kissi, Toma	late 19 cent.	—	1–150 g	'Guenze'	Francs	—	£46–65	—	Sundstrom
Gbundi, Toma S. Kpelle	—	Konianke	1–200 g	'Kissi penny'	Marks	—	£150–250	—	Westermann[25]
Nigeria									
Oyo	1904	Ola-Igbe	60 lb	Bloom	Shillings	£16–20	—	—	Bellamy[26]
Nupe	1930s	—	4 kg	Bloom	Shillings	£25	—	—	Nadel[27]
Bida	1930s	—	4–700 g	Hoe	Shillings	—	—	£62–95	Nadel
Marghi	1930s	Marghi	2–300 g	Bar	Shillings		c. £500		Vaughan[28]
Sukur	1960	Sukur	[illegible]	Bar	Shillings		£100–150		Sassoon[29]

outweighed in such areas, because of the qualitative differences referred to above. In order to add substance to this opinion it would be necessary to compare the price of various forms of iron at the point of contact in different periods. Unfortunately, the evidence upon which a comparison could be based is not available. In addition to this, however, the forms in which iron was sold make strict comparison very difficult. Where full-time smiths bought in iron smelted by others it was usually in the form of a complete bloom or fragments sufficient to make one tool.[17] As an item in a more widespread network of exchange, iron bars were of course common in Senegambia, eastern Guinea and surrounding areas as well as northern Nigeria and northern Cameroon. Iron in other forms such as hoe-blanks which required the action of the smith before they could be considered as useful tools was more widespread.[18] In Liberia, it has been reported that iron in different forms was exchanged with each other, Mande smiths buying blooms in some form with iron money which the smelters could not manufacture.[19] In northern Ghana smiths manufactured complete tools from iron ore, therefore iron did not enter the marketplace in any other form. Prices of these and other forms of iron have been quoted in the literature in cowries, marks, francs, sterling as well as other commodities. The weights of the units used have seldom been given. In order to make meaningful comparison possible all the following pieces of information would be required – the form in which iron was sold, the venue at which it was sold, the weight of the unit in which it was sold, the origin of the iron, the currency used and the date of the transaction. In no published account that I have come across have all these elements been given. By interpolation from a number of sources it is possible to provide some comparative data, but since it is necessary to speak of a range of prices in each location at different times, when the exchange rate between different currencies was very volatile, the comparison is of limited value. The information is summarized in Table 2. Both the degree of variation in the price of iron from one locality and the price of the finished product were extremely high. Frobenius reported that a bloom selling for 10,000 cowries at Banjeli, the place of production, fetched 40,000 at Tchitchao, some

[17] See for instance, Nadel, *Black Byzantium*, 264; C. V. Bellamy, 'A West African Smelting House', *J. Iron & Steel Inst.* LXVI (1904), 117; R. Cornevin, *Les Bassari du Nord-Togo* (Paris, 1962), 90; S. L. White, 'Iron Production and Iron Trade in Northern and Central Liberia: History of a Major Indigenous Technology', *6th Annual Liberian Studies Conf. Paper* (1974), 10.

[18] For a survey of the evidence see L. Sundström, *The Exchange Economy of Pre-Colonial Tropical Africa* (1974), 206–7.

[19] White, 'Iron Production', 10.

[20] H. Labouret, *Les Tribus du Rameau Lobi* (Paris, 1931), 71.

[21] Hupfeld, 'Die Eisenindustrie', 181.

[22] Cornevin, *Les Bassari*, 89, quoting figures from H. Klose, 'Das Bassarivolk', *Globus* XXXIII (1903).

[23] L. Frobenius, *Und Afrika sprach unter den unstraflichen Aethiopien* (Berlin, 1912) III, 441, 457.

[24] Sundström, *Exchange Economy*, 206, quoting various sources.

[25] D. Westermann, *Die Kpelle* (Gottingen, 1921), 38.

[26] Bellamy, 'West African Smelting House', 118.

[27] Nadel, *Black Byzantium*, 264.

[28] Vaughan, 'Nkyagu', 176.

[29] Sassoon, 'Iron-smelting', 178.

kilometres away.[30] A mark-up of this scale may indicate the extent of demand for iron at Tchitchao; one is tempted to assume that imported iron from Europe would have been readily accepted at such a place as soon as it became available. The increase in the price of iron as it went through the smithing processes was also very substantial, being up to 750 per cent at Binaparba and in northern Ghana.

What comes out of this exercise is the fact that local iron was cheap only in certain centres in West Africa in the late nineteenth century, at Ola-Igbi and Banjeli for instance. Given that there was a preference for local iron, based on a mixture of qualitative and conservative factors, it would have been highly competitive with European iron in the local markets. This competitiveness would have reversed fairly rapidly with the distance from the source of production. Smiths buying iron produced at Banjeli in the market at Kabou close by would have found imported iron expensive. It had to be brought up from the northern terminus of the railway some 120 kilometres to the south. The increase in the price of European iron as it travelled away from the coast was as dramatic as that of local iron as it went away from the point of production. It was reported that an iron bar worth 6*s*. 3*d*. in Lisbon would sell for 11*s*. 3*d*. on the west African coast and twice that inland.[31] Bowdich indicated that in 1819 bars were sold in the markets of Salaga and Yendi at three times their Cape Coast price, which would itself have been twice that in London.[32] In the seventeenth century the cost of iron fluctuated between £13 and £18 per ton,[33] and this range of prices was maintained in the eighteenth century, top quality iron being more expensive.[34] By the middle of the following century the price had dropped to about £7.50,[35] although some steels were still much more expensive at about £13 per ton.[36] Although it would be unwise to infer too much about the price of imported iron in internal markets based on these figures, it is possible to argue that not until the end of the nineteenth century did imported iron become sufficiently cheap and plentiful in these internal markets to outweigh the prejudices against it. Bellamy commented in 1904 that the price of Ola-Igbi iron was approximately six times that of pig-iron in England, yet he could be reasonably optimistic about the short term future of the local industry, even if he regarded the eventual end of smelting as inevitable.[37]

In analysing the elements of competition between the two sources of iron, then, it is important to stress the non-economic components. The preference for local iron, expressed both by those who made the tools and those who

[30] Quoted in Cornevin, *Les Bassari*, 90.

[31] W. Rodney, *History of the Upper Guinea Coast* (Oxford, 1970), 196, quoting from de Andrade.

[32] T. E. Bowdich, *Mission from Cape Coast Castle to Ashantee* (London, 1819), 331.

[33] K. G. Davies, *The Royal Africa Company* (London, 1957), 171, 235; Rodney, *History*, 196; J. van Laun, '17th century ironmaking in south west Herefordshire', *J. Hist Metall. Soc.* XIII (1979), 58.

[34] K. C. Barraclough, 'An eighteenth century steelmaking enterprise: the Company of Cutlers in Hallamshire, 1759–1772', *Bull. Hist. Metall. Group*, VI, 25–9.

[35] C. W. Newbury, 'Prices and Profitability in early nineteenth century West African trade', in C. Meillassoux (ed.), *The Development of Indigenous Trade and Markets in West Africa* (Oxford, 1971), 94.

[36] F. le Play, 'A Report on the Manufacture of Steel in Yorkshire and a Comparison with the Principal Groups of Steelworks in Europe', *J. Hist. Metall. Soc.* VIII (1974), 43.

[37] Bellamy, 'West African Smelting House', 118.

worked with them, in terms of quality, was indicative of an underlying regard for the local material. The difference in quality between the two sources was no doubt not as great as the farmers or the smiths would have had it; part of the antipathy represented an acknowledgement of the ritual significance of local iron.

That iron has always had a significance for West African peoples over and above its usefulness as a material has been long acknowledged. Williams gives a detailed account of the place of the metal in Yoruba metaphysics.[38] It has been suggested that this attitude derives from the insubstantial hold that smelters in west Africa had on the technology of iron production, the mysterious and fitful transformation of earth into metal, but this has not been my experience. Smelters may not have been explorative in their exploitation of the material, but they were in their heyday certainly in control of it. Vaughan is also sceptical of the 'mystery' theory of the position of iron,[39] yet has to agree that blacksmiths were accorded a special status in north-eastern Nigeria by reason of their association with the working of the metal which would be difficult to explain in other terms. Iron has been given special properties by many peoples. Iron tools figured in the myth of foundation of the Hausa states.[40] It was intimately bound up with the burial of chiefs in some states south of Kanuri.[41] In Bamenda locally smelted iron was used in bridewealth payments.[42] There is evidence from elsewhere in Cameroon that only locally smelted iron could be used in ritual contexts, objects made of imported iron not being considered suitable.[43] This was also true in north-western Ghana. During the funerals of blacksmiths the tools and products of their craft were displayed; only locally smelted iron could properly be used.

The respect accorded to iron workers in many areas cannot wholely be put down to the pseudo-magical aura of the material. In some communities it was the smithing group as distinct from the smelters to which special status was attached. In north-eastern Nigeria any one could smelt iron, but the smiths were people apart,[44] and the division was similar in Nupe.[45] In many places the forge fulfilled a religious function in addition to being a place of social focus. It figures prominently in Dogon cosmology.[46] Among the LoWiili in north-western Ghana the forge could be likened to an earth shrine and the smith's status was similar to that of the earth-priest.[47] Smiths in the Limba area of Sierra Leone are also highly respected, partly because they are said to have secret powers, but also because they produced the farming tools.[48] It is this control over the essential tools of other crafts which is in my view

[38] Williams, *Icon and Image*, ch. 12.

[39] Vaughan, 'Nkyagu', 163.

[40] A. Smith, 'The early States of the Central Sudan', in J. Ajayi & M. Crowder, *History of West Africa*, I, 186, fn.

[41] C. K. Meek, *Northern Tribes of Nigeria* (London, 1925), II, 122.

[42] M. D. W. Jeffreys, 'Some notes on Kwaja smiths of Bamenda', *Man* LXII, no. ccxxxvi (1962).

[43] Information relating to iron objects in the Museum of Archaeology and Anthropology, Cambridge.

[44] Vaughan, 'Nkyagu', 167.

[45] Nadel, *Black Byzantium*, 259.

[46] M. Griaule, *Conversations with Ogotemmeli* (Oxford, 1965), 42–6, 84–8.

[47] J. Goody, *The Social Organisation of the LoWiili* (Oxford, 1967), 91.

[48] R. H. Finnegan, *Survey of the Limba People of northern Sierra Leone* (London, 1965), 97–8.

the most significant aspect contributing to the smith's status. It applies principally to the implements necessary to farming but also the hunter's arrow- and spear-heads, the carver's knives and adzes; every craftsman directly or indirectly depends on iron tools. Ogotemmeli of Dogon said '...without the fire of the smithy and the iron of hoes there would be no crops to store'.[49] Although the smelting furnace was often protected by ritual injunctions it could also be treated without any special regard.[50] The respect due to the forge, however, is far more consistent and widespread. Since smithing work is still necessary, since imported iron, even in the form of hoe-blanks still requires reshaping by the smith, this influence is likely to continue. Were the iron-worker's status to be explained on the basis of his association with iron production, one would have expected it to decline with the disappearance of indigenous smelting. While the association with the earth through smelting iron has some relevance, it is the smith's pre-eminent position as the premier artisan that is crucial.

It has usually been a matter for a footnote that iron smelting was formerly practised in most West African communities but has long since died out in all but the most remote areas.[51] My purpose in this paper has been to suggest that the survival of the process over a 400-year period since the introduction of imported iron, and in the teeth of consistent and sometimes direct pressure from European trading and colonial powers, was in itself remarkable and has up to now been little stressed.[52] It is this survival which has to be explained, rather than the eventual decline which, given the pressures I have referred to, was inevitable sooner or later. I have therefore been concerned to emphasize some of the more neglected factors which contributed to the persistence of local smelting practices and may account for the otherwise paradoxical resurrection of smelting in one or two places. These factors principally relate to the organization of the labour involved in the work and the influence that the users of the tools the smith produced have had over their quality. Since, as I have argued, the users comprise the whole community, their demand for the maintenance of quality and fitness for purpose which was inherent in locally-smelted iron was decisive over many decades.

SUMMARY

In the sixteenth century most iron used in west Africa was produced within the region. Extra demand may have been met from the newly established European factors on the coast. By the end of the nineteenth century, in contrast, it was the residue in demand that was satisfied from local sources, the main bulk of iron being imported via the coast and transported inland. For the larger part of this 400-year period imported iron was cheaper than locally-produced iron. What was remarkable then, was not that iron smelting eventually died out, but that it survived for so long and could be studied in detail in the second half of the twentieth century.

It is argued that, although the decline can be related to production constraints

[49] Griaule, *Conversations*, 43.

[50] Pole, 'Iron smelting in northern Ghana', 21, 26–8.

[51] See for instance P. Bohannan, *African Outline* (London, 1966), 50.

[52] But A. Hopkins, *Economic History of West Africa* (London, 1973), 246, refers to the continuity of production and trade over long distances throughout the period of colonial rule.

such as the availability of charcoal, influences originating from the rest of the community can be seen to have prolonged the survival of local iron. The organization of labour of both the iron-smelting and blacksmithing processes, together with the way in which iron was marketed, are central to the analysis. In addition, consumption factors are of the utmost importance. Apart from the prejudice against innovation, the fact that imported iron was plainly not as suitable as local iron for the purposes to which it was put, weakened its impact. Also the ritual attitude to local iron has to be taken into consideration. The present universality of non-local sources has resulted in a change in the regard paid to the metal, but it is argued that the position of the smith is unlikely to alter significantly, since it is more related to his crucial role as supplier of tools for other essential activities such as farming, than to the production of iron itself.

L. M. Pole, 'Decline or survival? Iron production in West Africa from the seventeenth to the twentieth centuries', *Journal of African History*, vol. 23, 1982, pp. 503–13.

6

THE LESSONS OF LYSENKO

Lenin Academy of Agricultural Sciences of the USSR

6.1

SELECTIONS FROM THE PROCEEDINGS OF THE LAAAS

1. BIOLOGY, THE BASIS OF AGRONOMY

Agronomy deals with living bodies—plants, animals, micro-organisms. A theoretical grounding in agronomy must, therefore, include knowledge of biological laws. And the more profoundly the science of biology reveals the laws of the life and development of living bodies, the more effective is the science of agronomy.

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2. THE HISTORY OF BIOLOGY: A HISTORY OF IDEOLOGICAL BATTLE

The appearance of Darwin's teaching, expounded in his book, *The Origin of Species*, marked the beginning of scientific biology.

The leading idea of Darwin's theory is the teaching on natural and artificial selection. Selection of variations favourable to the organism has produced, and continues to produce, the fitness which we observe in living nature; in the structure of organisms and their adaptation to their conditions of life. Darwin's theory of selection provided a rational explanation of the fitness observable in living nature. His idea of selection is scientific and true. In substance, his teaching on selection is a summation of the age-old practical experience of plant and animal breeders who, long before Darwin, produced varieties of plants and breeds of animals by the empirical method. 12

Darwin investigated the numerous facts obtained by naturalists in living nature and analyzed them through the prism of practical experience. Agricultural practice served Darwin as the material basis for the elaboration of his theory of evolution, which explained the natural causes of the purposiveness we see in the structure of the organic world. That was a great advance in the knowledge of living nature.

In Engels' opinion, three great discoveries enabled man's knowledge of the interconnection of natural processes to advance by leaps and bounds: first, the discovery of the cell; second, the discovery of the transformation of energy; third, "the proof which Darwin first developed in connected form that the stock of organic products of nature environing us today, including mankind, is the result of a long process of evolution from a few originally unicellular germs, and that these again have arisen from protoplasm or albumen, which came into existence by chemical means."[1]

The classics of Marxism, while fully appreciating the significance of the Darwinian theory, pointed out the errors of which Darwin was guilty. Darwin's theory, though unquestionably materialist in its main features, is not free from some serious errors. A major fault, for example, is the fact that, along with the materialist principle, Darwin introduced into his theory of evolution reactionary Malthusian ideas. In our days this major fault is being aggravated by reactionary biologists.

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[1] F. Engels, *Ludwig Feuerbach und der Ausgang der klassischen deutschen Philosophie*, Moskau 1946, S. 44.

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13 Biologists should always ponder these words of Engels: "The entire Darwinian teaching on the struggle for existence merely transfers from society to the realm of living nature Hobbes' teaching on *bellum omnium contra omnes* and the bourgeois economic teaching on competition, along with Malthus' population theory. After this trick (the absolute justification for which, as indicated in point 1, I deny, particularly in regard to Malthus' theory) has been performed, the same theories are transferred back from organic nature to history and the claim is then made that it has been proved that they have the force of eternal laws of human society. The childishness of this procedure is *obvious*, and it is not worth while wasting words on it. But if I were to dwell on this at greater length, I should have started out by showing that they are poor *economists* first, and only then that they are poor naturalists and philosophers."[2]

For the propaganda of his reactionary ideas Malthus invented an allegedly natural law. "The cause to which I allude," he wrote, "is the constant tendency in all animated life to increase beyond the nourishment prepared for it."[3]

It must be clear to any progressively thinking Darwinist that, even though Darwin accepted Malthus' reactionary theory, it basically contradicts the materialist foundation of his own teaching. Darwin himself, as may be easily noted, being as he was a great naturalist, the founder of scientific biology, whose activity marks an epoch in science, could not be satisfied with the Malthusian theory, since it is, in fact and fundamentally, at variance with the phenomena of living nature.

Under the weight of the vast amount of biological facts accumulated by him, Darwin felt constrained in a number of

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[1] *The Life and Letters of Charles Darwin*, London 1887, Vol. I, p. 83.
[2] F. Engels, letter to P. L. Lavrov, 12-17 November 1875.
[3] T. R. Malthus, *An Essay on the Principle of Population*, London, New York and Melbourne, 1890, Book 1, p. 2.

cases radically to alter the concept of the "struggle for exist-
14 ence," to stretch it to the point of declaring that it was just a figure of speech.

Darwin himself, in his day, was unable to fight free of the theoretical errors of which he was guilty. It was the classics of Marxism that revealed those errors and pointed them out. Today there is absolutely no justification for accepting the erroneous aspects of the Darwinian theory, those based on Malthus' theory of overpopulation with the inference of a struggle presumably going on within species. And it is all the more inadmissible to represent these erroneous aspects as the cornerstone of Darwinism (as I. I. Schmalhausen, B. M. Zavadovsky, and P. M. Zhukovsky do). Such an approach to Darwin's theory prejudices the creative development of its scientific core.

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15 In the post-Darwinian period the overwhelming majority of biologists—far from further developing Darwin's teaching—did all they could to debase Darwinism, to smother its scientific foundation. The most glaring manifestation of such debasement of Darwinism is to be found in the teachings of Weismann, Mendel, and Morgan, the founders of modern reactionary genetics.

3. TWO WORLDS—TWO IDEOLOGIES IN BIOLOGY

Weismannism, which made its appearance at the turn of the century, followed by Mendelism-Morganism, was primarily directed against the materialist foundations of Darwin's theory of evolution.

Weismann named his conception Neo-Darwinism, but, in fact, it was a complete denial of the materialist aspects of Darwinism. It insinuated idealism and metaphysics into biology.

The materialist theory of the evolution of living nature necessarily presupposes the recognition of hereditary transmission of individual characteristics acquired by the organism under definite conditions of its life; it is unthinkable without recognition of the inheritance of acquired characters. Weismann, however, set out to refute this materialist proposition. In his *Lectures on Evolutionary Theory,* he asserts that "not only is there no proof of such a form of heredity, but it is inconceivable theoretically."[1] Referring to earlier statements of his in a similar vein, he declares that "thus war was declared against Lamarck's principle of the direct transforming effect of use and disuse, and, indeed, that marked the beginning of the struggle which is going on to this day, the struggle between the Neo-Lamarckians and the Neo-Darwinians, as the contending parties are called."[2]

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Weismann asserts that there are "two great categories of 16
living material: the *hereditary substance,* or *idioplasm,* and the '*nutrient substance,*' or *trophoplasm. . . .*"[4] He declares that the bearers of the hereditary substance, "the *chromosomes, represent a separate world, as it were,*"[5] a world independent of the body of the organism and its conditions of life.

Having thus disposed of the living body as being merely a nutritive soil for the hereditary substance, Weismann proclaims that the hereditary substance is immortal and is never generated *de novo.*

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[1] A. Weismann, *Vorträge über Deszendenztheorie,* Bd. 1, S. 277.
[2] *Ibid.*
[4] *Ibid.,* S. 279.
[5] *Ibid.,* S. 239.

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Hence, according to Weismann, there can be no new for- 17
mations of the hereditary substance, it does *not* develop with the development of the individual, and is *not* subject to any dependent changes.

An immortal hereditary substance, independent of the qualitative features attending the development of the living body, directing the mortal body, but not produced by the latter—that is Weismann's frankly idealistic, essentially mystical conception, which he disguised as "Neo-Darwinism."

Weismann's conception has been fully accepted and, we might say, carried further by Mendelism-Morganism.

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Following Weismann, the Mendelist-Morganists contend 19
that the chromosomes contain a special "hereditary substance" which resides in the body of the organism as though in a case and is transmitted to succeeding generations irrespective of the qualitative features of the body and its conditions of life. The conclusion drawn from this conception is that new tendencies and characteristics acquired by the organism under the influence of the conditions of its life and development are not transmissible and can have no evolutionary significance.

According to this theory, characters acquired by vegetable and animal organisms cannot be handed down, *cannot be inherited.*

The Mendelist-Morganist theory does not include in the scientific concept "living body" the conditions of the body's life. To the Morganists, environment is only the background—indispensable, they admit—for the manifestation and operation of the various characteristics of the living body, in accordance with its heredity. They therefore hold that qualitative variations in the heredity (nature) of living bodies are entirely independent of the environment, of the conditions of life.

The representatives of Neo-Darwinism, the Mendelist-Morganists, hold that the efforts of investigators to regulate the heredity of organisms by suitably changing the conditions of life of these organisms are utterly unscientific. They therefore call the Michurin trend in agrobiology Neo-Lamarckian, which, in their opinion, is absolutely fallacious and unscientific.

Actually, it is the other way round.

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20 The true ideological content of Morgan's genetics has been well revealed (to the discomfiture of our Morganists) by the physicist Erwin Schrödinger. In his book, *What Is Life? The Physical Aspect of the Living Cell*, he draws some philosophical conclusions from Weismann's chromosome theory, of which he speaks very approvingly. Here is his main conclusion: "... the personal self equals the omnipresent, all-comprehending, eternal self." Schrödinger regards this conclusion as "the closest a biologist can get to proving God and immortality at one stroke."[1]

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A sharp controversy, which has divided biologists into two irreconcilable camps, has thus flared up over the old question: *can characters and properties acquired by vegetable and animal organisms in the course of their life be inherited?* In other words, whether qualitative variations of the nature of vegetable and animal organisms depend on the nature of the conditions of life which act upon the living body, upon the organism.

The Michurin teaching, which is in essence materialist and dialectical, proves by facts that such dependence does exist.

The Mendelist-Morganist teaching, which in essence is metaphysical and idealist, denies the existence of such dependence, though it can cite no evidence to prove its point.

[1] E. Schrödinger, *What Is Life? The Physical Aspect of the Living Cell*, Cambridge University Press, 1945, p. 88.

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22 Naturally, what has been said above does not imply that we deny the biological role and significance of chromosomes in the development of the cells and of the organism. But it is not at all the role which the Morganists attribute to the chromosomes.

Plenty of examples can be cited to show that our home-grown Mendelist-Morganists accept in its entirety the chromosome theory of heredity, its Weismannist foundations and idealistic conclusions.

23 Academician N. K. Koltsov, for example, asserts: "Chemically, the genoneme with its genes remains unchanged in the course of the entire ovogenesis and is not subject to metabolism—oxidizing and reduction processes."[1] This asser-

tion, which no literate biologist can accept, denies the existence of metabolism in a section of the living and developing cells. It must be obvious to everyone that N. K. Koltsov's conclusion is fully in line with the Weismannist and Morganist idealist metaphysics.

[1] Н. К. Кольцов, „Структура хромосом и обмен веществ в них", *Биологический журнал*, том VII, вып. I, 1938 г., стр. 42.

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We need not continue the list of authors who, like M. M. 25
Zavadovsky and N. P. Dubinin, frankly expound the ABC of the Morganist system of views. In college textbooks on genetics this ABC is called the "Mendelian laws" (dominance, segregation, purity of gametes, etc.). An example of how uncritically our Mendelist-Morganists accept idealistic genetics is the fact that the standard textbook on genetics in many of our colleges has until quite recently been a translated American, strictly Morganistic, textbook—by Sinnott and Dunn.

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Michurin himself and his followers have obtained and are obtaining directed hereditary changes in vegetable organisms
literally in immense quantities. Yet Schmalhausen still asserts 28
that:

"The appearance of individual mutations is by all indications a case of chance phenomena. We can neither predict nor deliberately induce this or that mutation. So far it has been found impossible to establish any causal connection between the quality of mutation and definite changes in the factors of the environment."[1]

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Schmalhausen obviously finds that Michurin's facts do not fit in with his theory of "stabilizing selection." In his book, *Factors of Evolution,* he gets out of the difficulty by making no mention of Michurin's work or of the very existence of Michurin as a scientist. Schmalhausen has written a bulky volume on factors of evolution without ever once mentioning—not

[1] И. И. Шмальгаузен, *Факторы эволюции*, стр. 68.

even in his bibliography—either K. A. Timiryazev or I. V. Mi- 29
churin. Yet Timiryazev bequeathed to Soviet science a remarkable theoretical work bearing practically the same title: *Factors of Organic Evolution.* As for Michurin and the Michurinists, they have put the factors of evolution to work for agriculture, revealed new factors and given us a deeper understanding of the old ones.

Schmalhausen has "forgotten" the Soviet advanced scientists, the founders of Soviet biological science. But at the same time he quotes profusely and repeatedly statements of big and small foreign and native representatives of Morgan's metaphysics and leaders of reactionary biology.

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The Ministry of Agriculture might tell us exactly what the 31
cytogeneticists have offered for practical application, and, if there have been such offers, whether they were accepted or rejected.

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Here is one example which might be cited to show how useless is the practical and theoretical program of our domestic Morganist cytogeneticists.

Professor of Genetics, N. P. Dubinin, Corresponding Member of the Academy of Sciences of the U.S.S.R., who is regarded by our Morganists as the most eminent among them, has worked for many years to ascertain the differences in the cell nuclei of fruit flies in urban and rural localities.

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33 Dubinin, as we see, writes so that outwardly his work may appear to some to be even scientific. As a matter of fact, this was one of the main works on the basis of which Dubinin was elected Corresponding Member of the Academy of Sciences of the U.S.S.R.

But if we were to put it all in plainer terms, stripping it of the pseudoscientific verbiage and replacing the Morganist jargon with ordinary Russian words, we would arrive at the following:

As the result of many years of effort Dubinin "enriched" science with the "discovery" that during the war there occurred among the fruit-fly population of the city of Voronezh and its environs an increase in the percentage of flies with certain chromosome structures and a decrease in the percentage of flies with other chromosome structures (in the Morganist jargon that is called "concentration of inversions" II—2).

Dubinin is not content with these discoveries, "highly valuable" from the theoretical and practical standpoint, which he made during the war. He sets himself further tasks for the restoration period. He writes:

"It will be very interesting to study in the course of several coming years the restoration of the karyotypical structure of the urban population in connection with the restoration of normal conditions of life."[2] (*Animation. Laughter.*)

That is typical of the Morganists' "contribution" to science and practical activity before the war and during the war, and those are the vistas of the Morganist "science" for the restoration period! (*Applause.*)

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[2] *Ibid.*

34 ## 7. MICHURIN'S TEACHING, THE FOUNDATION OF SCIENTIFIC BIOLOGY

Contrary to Mendelism-Morganism, with its assertion that the causes of variation in the nature of organisms are unknowable and its denial that directed changes in the nature of plants and animals are possible, I. V. Michurin's motto was: "We cannot wait for favours from Nature; we must wrest them from her."

His studies and investigations led I. V. Michurin to the following important conclusion: "It is possible, with man's intervention, to *force* any form of animal or plant *to change more quickly and in a direction desirable to man.* There opens before man a broad field of activity of the greatest value to him."[1]

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[1] И. В. Мичурин, *Сочинения*, том IV, стр. 72.

38 According to the chromosome theory of heredity, hybrids can only be produced by sexual reproduction. That theory denies the possibility of obtaining vegetative hybrids, for it denies that the conditions of life have any specific influence upon the nature of plants. I. V. Michurin, on the other hand, not only recognized the possibility of producing vegetative hybrids, but elaborated the "mentor" method. This method consists in the following: by grafting cuttings (twigs) of old varieties of fruit trees on the branches of a young variety, the latter ac-

quires properties which it lacks, these properties being transmitted to it through the grafted twigs of the old variety. That is why I. V. Michurin called this method "mentor." The stock is also used as a mentor. By this method Michurin produced new and improved existing varieties. 39

I. V. Michurin and the Michurinists have found methods of obtaining vegetative hybrids in large quantities.

The vegetative hybrids are cogent proof that Michurin's conception of heredity is correct. At the same time they represent an insuperable obstacle to the theory of the Mendelist-Morganists.

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8. YOUNG SOVIET BIOLOGISTS SHOULD STUDY THE MICHURIN TEACHING 44

Unfortunately, so far the Michurin science has not been taught in our universities and colleges. We Michurinists are greatly to blame for this. But it will be no mistake to say that it is also the fault of the Ministry of Agriculture and the Ministry of Higher Education.

To this day Morganism-Mendelism is taught in the majority of our universities and colleges in the departments of genetics and selection, and in many cases also in the departments of Darwinism, whereas the Michurin teaching, the Michurin trend in science, fostered by the Bolshevik Party and by Soviet reality, remains in the shade.

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Our Academy must work to develop the Michurin teaching. In this it ought to follow the personal example of concern for 49
the work of I. V. Michurin shown by our great teachers— 50
V. I. Lenin and *J. V. Stalin*. (*Loud applause.*)

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SECOND SITTING

Morning, August 2, 1948

We now pass to the discussion of the address. Academician M. A. Olshansky has the floor.

Academician M. A. Olshansky. When judging the correctness of one or another theory, it is important to ascertain to what extent that theory assists practical work. If the Michurin doctrine and Mendelism-Morganism are compared from the standpoint of plant-breeding and seed-growing practice, it will be seen with absolute clarity that Mendelism-Morganism, far from being an assistance, is often a direct hindrance to practice. Michurin genetics, on the other hand, arms plant breeders with effective methods of improving the breed qualities of seed. I shall give a few examples.

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Academician P. P. Lobanov. I call upon Academician 70
I. V. Yakushkin.

Academician I. V. Yakushkin. Comrades, in the latter years of the tsarist regime, just before the October Revolution, it was often said that agricultural science had reached a dead
end. In relation to the impoverished and debt-laden small peas- 71
ant farms of that period this was correct.

From the address of Academician Lysenko we have heard at this session, I think we are absolutely warranted in drawing another conclusion, namely, that we are now living in the great Stalin epoch, in the period of the triumph of the advanced agricultural science which has been built up on the celebrated work of Michurin and Williams, and which assists us in our struggle for a new world.

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75 I cannot help remarking, and those present here will agree with me, that the researches of one of our Morganist-Mendelists on the influence of the Great Patriotic War on the chromosome structure of flies, referred to by T. D. Lysenko in his address, are the utter limit.

At a time when all true sons of the Soviet people were fighting for the honour, independence and liberty of our country and carrying that fight to a victorious conclusion, there were researchers who devoted their time to the study of the influence of the war on flies!

A voice. Flybreeders!

I. V. Yakushkin. Such extremes, it seems to me, only show how wrong it is for any Soviet scientist to isolate himself from the life of his country.

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117 **Academician P. P. Lobanov.** The Presidium has received a note with a question which I consider it necessary to make public: "Why has no one of the adherents of formal genetics taken the floor? Is it because they do not want to speak themselves, or because they are not being given a chance to speak?" I answer: None of them has asked for the floor, but it is to be presumed that they will take the opportunity offered them to speak at this session. It would be incomprehensible and unworthy of the status of scientists if they kept silence while fundamental questions of science are discussed.

Permit me to adjourn until 6 p.m.

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SPEECH BY N. M. SISAKIAN

129 Taking the views of Michurin and Lysenko as our point of departure, and basing our work on the principles of A. N. Bach's school of biochemistry, we in the Bach Institute of Biochemistry of the Academy of Sciences of the U.S.S.R. undertook researches which led to the discovery of a number of new facts that testify to profound changes in the biochemical activity of organisms under the influence of vernalization and vegetative hybridization.

Already in 1936, we were able to establish that in the vernalization of seeds according to T. D. Lysenko's method, fundamental changes of a biochemical character take place in vegetating plants.

The aim we set ourselves in this series of experiments was to trace the changes in the process of enzymatic formation and breakdown of sucrose in the living cell that occur under the influence of vernalization.

Experiments with the winter wheat Ukrainka showed that in non-vernalized plants, the process of enzymatic formation of sucrose predominates over the hydrolysis process, the breakdown of this substance. In vernalized plants, however, we see the very opposite. The process of vernalization conditions the shifting of the enzymatic equilibrium in the living leaves of plants in the direction of hydrolysis.

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130 It must be observed that high productivity in plants is connected with the predominance of hydrolytic reactions in its vegetating organs.

It must be stated that with vernalization, not only does the direction of the enzymatic formation of sucrose change, but so also does the balance of the dissolved sugars. Vernalization leads to an increase in the quantity of monosaccharoids.

To obtain a complete and true judgment of the results arrived at, we conducted experiments similar to those just described on different varieties of cotton, a plant that differs from wheat both in its nature and in the factors required for its vernalization. The results of our experiments on cotton fully confirmed the data we obtained in our experiments on wheat. In cotton, as in wheat, vernalization conditions a fundamental change in the direction of the processes of enzymatic formation and breakdown of sucrose.

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SPEECH BY V. P. BUSHINSKY 148

A large number of experiments and examples of a similar kind can now be quoted. All of them testify to the fact that
science is already able to control life, can control living and 149
dead protein. But science cannot yet say definitely "what protein is," or "what life is" as the derivation of it. Why? Engels in his day put it excellently in *Anti-Dühring* when he said that in order to gain an exhaustive knowledge of what life is, we should have to go through all the forms in which it appears, from the lowest up to the highest.

Consequently, in order to understand and learn "what protein is," it is also necessary to go through all the forms of manifestation, from the lowest to the highest. And for this we need experiment, experiment, and again experiment, i.e., that which the Michurin trend is remarkable for, and not the speculations of formalists, who are to be found in all branches of science.

We must create a big experimental base for the purpose of studying dead and living protein, and enlist experts for this work; and this is what the Academy is doing, and will continue to do.

Long live the Academy which bears the name of mankind's genius—Lenin, the Academy of Michurin, Williams, Ivanov and Timiryazev, the Academy which is directed by the best representative of the Michurin agrobiological trend, Academician T. D. Lysenko, the Academy which is directed by the genius of our great teacher, Comrade Stalin. (*Applause.*)

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SPEECH BY J. A. RAPOPORT 169

Michurin repeatedly pointed to the possibility of extensively utilizing genetics not only in horticulture, but also in field culture. He urged young people to study genetics.

This was long ago. Since then genetics has made much progress, and I cannot agree with those comrades who demand the elimination of the genetics course from the curricula of our colleges and universities and the rejection of principles on the basis of which valuable varieties and breeds have been and are now being produced.

We must not simply ape others, but it is our duty critically and creatively, as V. I. Lenin taught us, to assimilate all that has been done abroad. We must carefully tend the shoots of what is new, and train new cadres who will be able to push science further forward.

Only on the basis of truth, by criticizing one's own mistakes, can we achieve in the future the big successes our country is calling for. (*Some applause.*)

160 **A voice.** What is your present attitude towards the question of the inheritance of acquired characteristics?

J. A. Rapoport. I think that the internal mechanism of genic action consists in that the gene, each gene, in essence, corresponds to one definite enzyme, to one definite enzyme system. This has now been shown by a number of experiments on several organisms of a lower order—bacteria and fungi. These researches are now of great practical significance, and a big step forward has been taken in this respect.

It may be demonstrated that a physiological character also changes as a result of mutation, because there is, of course, no such thing as form divorced from material content. It is possible to produce a change in a definite direction due to the exclusion of this or that enzyme system. It is the enzymes that are directly responsible for the particular modifications. These enzymes are well known to biochemists, with whom geneticists maintain close connection and will undoubtedly maintain closer connection in the future. This is the school of Academician A. N. Bach and Academician A. I. Oparin. Here it is perfectly evident that if we operate on an organism with an enzymatic poison, say, we will get a definite modification which will give rise to a new character. Consequently, the mechanism of modification is the mechanism of the action on the enzymes, or on some other equally important units. It is very easy to obtain these characters, because the molecular bond here is of quite a special kind.

Mutation is another thing, it is an irreversible variation. Here a new molecular bond is established, and the variation that is obtained is transmitted by heredity. In this connection it must be clearly understood that it is possible to operate on the external system, on the membrane, on the enzyme system, and easily obtain variations in characters, the non-hereditary system, but there is no connection between a variation in the gene and modification of this kind, as the Lamarckian theory postulates.

Thus, it must be admitted that there is a separate system of modifications and a system of mutations. We are able to direct both systems, and this will be more fully proved in the future, for genetics is on the threshold of great discoveries.

161 **Academician P. P. Lobanov.** I call on Comrade G. A. Babajanyan.

G. A. Babajanyan (Director of the Institute of Genetics of the Academy of Sciences of the Armenian S.S.R.). Comrades, I am in a better situation than preceding speakers because I do not have to quote from books. I will deal with what Doctor Rapoport said.

Doctor Rapoport said: "Soviet geneticists have never taken an anti-Darwinist stand." What do our Morganists expect to gain by making a statement like that? It is the same as saying that our Morganist geneticists have never taken a Morganist stand. Who, if not Morgan, in his works, regards Darwinism as a system of speculations on problems of evolution, a system devoid of all experimental foundation?

Who does not know that Johannsen, one of the founders of Morganistic genetics, was a most typical anti-Darwinist? In his works, Johannsen quite openly opposed Darwin. It is not what Johannsen said that matters, however, but the substance of his theory. Who is not aware of the nature of Johannsen's metaphysical pure lines theory?

It is impossible to conceive of a more outstanding anti-Darwinist than Weismann. The chromosome theory of heredity is, in substance, Weismann's idealist theory of the immortal germplasm. It was said here that the Morganists do not share Weismann's views, but Rapoport's entire speech was based precisely on Weismann's arguments.

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It was rather difficult to understand some parts of Rapoport's speech. In one part of it he elaborated the idea that the 162
gene is as yet a hypothetical material unit, that the physical existence of this hereditary substance has not been proved. But in another part he said that we have the gene in our hands. This is interesting—the gene is invisible, but the Morganists have it in their hands.... (*Laughter. Applause.*)

Rapoport said that Morganist genetics is hastily criticized, that hasty conclusions are drawn about Morganism. But you know, these people have queer ideas about haste and patience. The character of the two conflicting trends has been discussed in our country for twenty years, and after that people come along and say that "hasty" conclusions are drawn about Morganism-Mendelism. What do they hope to gain by statements like that? They hope to gain time. With this object in view they keep on promising to discover new substances that will produce mutations. One of them, said Rapoport, has already been discovered.

We have been hearing about substances that produce mutations for quite a long time. We remember with what aplomb and cocksureness the Morganists spoke when X-rays, ultra-violet rays, ammonia, formaldehyde, etc., etc., were first employed as mutation producing factors. Do you want us to wait another 20 years to learn the nature of your new chemical mutagenic substance? We are told that this chemical substance is already causing a large number of mutations. That sounds good—a large number of mutations. But what does it really amount to? It would be better if this "large number" did not exist, because the organisms obtained in this way are all trash, freaks! Rapoport could not prove that the new mutations they have obtained differ in any way in principle from the innumerable mutations they obtained before. In his *Factors of Evolution,* Academician Schmalhausen gives a list of an enormous number of unviable mutations. What grounds have we for thinking that the new
mutations obtained under the influence of the new mutagenic 163
substances are of a different nature? On the contrary, we have every ground for thinking that they are of the same nature.

Lastly, let us assume for a moment that a small number of not harmful, not lethal, but useful mutations have indeed been obtained. But who wants them? Who wants what by their very nature are useless *Drosophilas*?

J. A. Rapoport. But there are useful mutations, and many of them. Why do you shut both your eyes to them?

G. A. Babajanyan. Firstly, because they are useful mutations for a useless object. (*Applause.*)

J. A. Rapoport. We have cures for tuberculosis and other diseases.

G. A. Babajanyan. You only make promises.

J. A. Rapoport. And you promise to produce strains in two years, but don't keep these promises, and you don't admit your mistakes.

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Academician P. P. Lobanov. I call upon Professor Z. Y. Beletsky. 189

Professor Z. Y. Beletsky (Head of the Department of Philosophy, Moscow State University). This discussion of Academician T. D. Lysenko's address is an event of great moment. We are here summing up the dispute which has gone on for many years between the two trends in biology—the formal geneticists on the one hand and the Michurinists on the other.

The events taking place in the sphere of biology are to a large extent analogous to the events which took place in the sphere of philosophy. Both in philosophy and in biology we have to deal with phenomena of the same order. A struggle is going on between two trends: the bourgeois, idealist trend and our dialectical materialist trend.

The representatives of the Weismannist trend not only defend a bourgeois theory in biology; they also insinuate the pernicious idea that bourgeois science and Soviet science are identical.

We find that some people among us accept the bourgeois view that our Marxist world outlook, our theory did not spring from the conditions of the new social and material relationships among men, but is the result of a generalization of all preceding intellectual achievements. According to this bourgeois conception, science sets itself the abstract task of comprehending the phenomena of the world in general. This bourgeois conception has it that the various sciences have developed outside of political life, outside the class struggle. From this the conclusion is drawn that only he is a true scientist and really advances the life of society who has mastered all the achievements of bourgeois theory, past and present. There is only one thing that should interest such a scientist, to wit, how ideas are connected with ideas and how they flow from one another. Such a scientist can therefore carry on his work in the quiet of his study. He is a "high priest" of science. He need not worry whether his speculative schemata accord with life or not. The important thing is the theory, not life, not practical activity. The motto of such scientists is: Let life adjust itself to science; if it is unable to do so, so much the worse for life.

That is why our Soviet Weismannists—Schmalhausen, Yudintsev, Alikhanian, Zhebrak, and the others—who assimilated the wisdom of Morganist-Mendelist genetics, have decided that they alone are the genuine scientists, and that our Soviet practice must endeavour to keep abreast of them. And if practice does not bear out their theories, so much the worse for practice. That is why for many years they looked down upon and made light of the practical successes of Michurin biology. That is why the Faculty of Biology at the University of Moscow—a stronghold of Morganist-Mendelist reactionary genetics in our country—has been fighting tooth and nail against the new genuinely scientific biology created by I. V. Michurin and so splendidly carried forward and developed in our day by T. D. Lysenko.

I shall cite a few facts to give you an idea of the doings of the Weismannists in the Faculty of Biology at the Moscow University.

In the past decade the Faculty of Biology has systematically held meetings, academic sessions and conferences devoted to criticism of Academician Lysenko's theoretical views.

Now, don't think that it was anything in the nature of serious scientific criticism. Not at all. Academician Lysenko's views were rejected from the outset as betraying ignorance, as having nothing in common with "genuine" university learning. That is the opinion of Michurin's and Lysenko's teachings

held by the majority of the professors and instructors on the Faculty of Biology, and that is also the opinion inculcated upon the students in that Faculty. Here is an instance. Last February the Faculty held an all-Union scientific conference, which lasted a week. About forty papers were read. But what were the problems the conference discussed? Did it discuss the achievements of biological science in practical farming, or did it demonstrate
the advantages of our biological science as compared with bour- 191
geois science? It did not. Every paper read at the conference, from the first to the last, was an attack on Academician Lysenko's teachings and a defence of bourgeois genetics. It appears that the main task the scientists on the Faculty of Biology set before biological sciences in this year 1948 is to refute the teachings of Academician Lysenko.

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A few words about the students of the Faculty of Biology. 192
The methods of intimidation employed in regard to them are simply incredible. Students of the Faculty of Biology are emphatically told that they must criticize Michurin's and Lysenko's teachings. If individual students nevertheless disagree with the Weismannists, they dare not say so openly. Some of these students, when they come to the Department of Dialectical and Historical Materialism for consultation, insistently request that their names and opinions should not be divulged.

The heads of the Faculty of Biology are doing their best
to eradicate Michurin's and Lysenko's views not only from the 193
minds of the students, but from the minds of the professors as well.

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Many of you must remember the years 1934, 1935, 1936 196
and 1938, when the controversy over fundamental questions of Darwinism raised by Academician Lysenko was at its fiercest. In those years the position of the opponents seemed to be quite strong, their adherents were rather numerous, and they joined forces to give battle to creative Darwinism, the idea and study of which was then put on the order of the day by T. D. Lysenko. They went to the length of bandying unworthy expressions and slanders, browbeating young scientists, warning them that world science would not tolerate Lysenko's denial of the gene as the bearer of the substance of heredity. They had no other name for those who shared Lysenko's teaching and theories than ignoramuses and half-baked scholars. These words have always been used by obscurantists in their effort to smother everything that is fresh and creative. Practice has shown, however, on whose side the truth of life was, who was backed by
the revolutionary theory. But, although a relatively long period 197
of time has elapsed since those years, the propaganda of obscurantism is still going on in the genetics departments of our institutions of higher learning. Propaganda of obscurantism—there is no other name for it.

Voice. Hear, hear!

E. I. Ushakova. Who has given these "scientists" and "teachers" who call themselves Soviet people the right to corrupt and poison the minds and souls of our young specialists! We meet scores and hundreds of young graduates of the Timiryazev Academy. When they come to us, we find that they are opposed to Michurin's teachings. How can they be good agricultural workers, if their attitude to living organisms is based on idealist notions? Comrade Beletsky, too, has told us about the efforts to muddle the minds of students at the University, where some of them are up in arms against the materialist philosophy because this philosophy does not accept the Morganist genetics. It is an intolerable situation that he has described. Students, our future Soviet experts, are being brought up ideologically in a spirit which is alien to Soviet society, to our science and practice! How could things be allowed to come to such a pass? Is it not time somebody was seriously called to account for this?

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203 **Academician P. P. Lobanov.** I call upon G. P. Vysokos.

G. P. Vysokos (Director of the Siberian Scientific Research Institute of Grain Husbandry). In the past century, ever since a beginning was made to bring the vast expanses of the Siberian plains under cultivation, settlers from the Ukraine and Central Russia brought with them winter-wheat seeds. The first attempts to grow winter wheat in Siberia invariably ended in failure. Sown on summer fallow, the wheat perished from the severe frosts. But the Siberian farmers never gave up the idea of growing high-yielding winter wheat in their fields.

It is hard and unprofitable for collective farms and state farms to carry on large-scale grain farming with spring crops alone. This is all the more inexpedient in Siberia, where the summers are short and the harvesting season for cereals is very limited. There can be no doubt that a stable winter wheat crop in Siberia is bound to lead to a large increase in the productivity of labour, making it possible to use tractors and other machines more efficiently and, hence, ensuring the further progress of grain growing on collective farms and state farms.

That is why it is a task of great moment and urgency for Soviet agrobiological science and for the large army of collective-farm Michurinist experimenters to solve the problem of winter wheat for Siberia.

Until recently science was baffled by this difficult problem. Trial sowings of winter wheat on fallow as a rule proved unsuccessful, as the seed perished in the winter. Academician T. D. Lysenko explains the reason why winter wheat
204 sown on fallow in Siberia does not survive the winter; it is due, he says, to mechanical injuries suffered by the underground parts of the plants and their leaves. Observations made by our Institute in the course of six years on winter wheat sown on fallow have fully borne out this explanation. When there is no snow cover from the fall, the soil freezes all through and there appear numerous deep cracks in it, and it becomes considerably deformed. That is the cause of mechanical injuries to the tillering nodes and the roots of winter wheat.

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206 In 1942 Academician Lysenko made a momentous scientific discovery—namely, that winter wheat can overwinter in the steppe part of Siberia if sown in the entirely unploughed stubble of spring crops.

Six years of tests of sowing winter wheat in the stubble on the open steppe fields of our Institute near the city of Omsk have shown that not only varieties like Lutescens 329, Alabasskaya, etc., with a high winter-hardiness, but also the less winter-hardy varieties of winter wheat, such as Ukrainka, Novokrymka, Erythrospermum 015, and the like, can overwinter in Siberia. In the past few years we have tested in our experiment fields about fifty of the most widespread and promising varieties of winter wheat. And they all stand the winter satisfactorily and well. Among the tested varieties there are samples from nearly all the regions in our country where winter wheat is grown—for example, Lutescens 329 and new promising varieties bred in Saratov and Kharkov, Erythrospermum 1160 and other Odessa-bred varieties, promising varieties produced by the Mironovka, Verkhnyachka and Nemercha breeding stations, Kuban winter wheats and promising varieties from the Yaroslavl and Alexandrov breeding stations, etc.

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SPEECH BY M. B. MITIN 273

Dealing with the problem of environment, I must dwell for a moment on the question of Lamarck.

The Mendelist-Morganists are using Lamarck as a bogey; they have converted his name into a word of abuse. It is enough to say "this is Lamarckism" to make them fly like the Devil from holy water. The actual, scientific truth about Lamarck, however, is the following.

As a matter of fact, Lamarck was the first man in the history of the development of science to expound the theory of evolution. He laid down the proposition that the correct classification of organisms is the reflection of the order and evolution of organisms from each other. He pointed to the decisive influence exercised by environment on the development of organisms, proceeding from the proposition that it is not form that conditions the functions of the organism, but, on the contrary, functions, directed by the influence of environment, that condition form.

As is known, Lamarck's theory arose in connection with the ideas of the French encyclopaedists and the French materialists. It reflected the revolutionary epoch of that time. It possessed philosophical content, and was distinguished for its materialistic character. The reaction against the French Revolution also caused a strong reaction against the ideas of Lamarck, and this reaction continued throughout the nineteenth century.

Commenting on the role Lamarck played in the development of the theory of evolution, K. A. Timiryazev wrote: "*The Philosophy of Zoology,* where for the first time the question was raised from the scientific point of view as to whether all the presently existing organisms could not have evolved from each other in the course of time by a gradual and slow process of alteration." In another place he wrote: "Sober-minded Darwinism alone allocates to Lamarckism its proper place in science."

That is how the matter stands with Lamarck.

It must be observed that Darwin himself admitted his error in underrating the influence environment exercises upon the organism. In his letter to Wagner (1876) he wrote:

"... the greatest error which I have committed, has been
not allowing sufficient weight to the direct action of the envi- 274
ronment, i.e., food, climate, etc., independently of natural selection."[1]

Darwin gave a truly scientific explanation of the process of evolution. That is the immortal service he rendered to science, to mankind. But many decades have passed since Darwin's time. Science and life have accumulated a vast number of new phenomena and facts. People like B. M. Zavadovsky, who call themselves "orthodox Darwinists," evidently wish to say that they adhere to Darwin's theory without any modification, i.e., adhere also to his errors (elements of Malthusianism, gradual evolution, repudiation of leaps in variation, etc.), to his obsolete propositions, and refuse to go further forward.

Let them cling to this "general" line. Life and science will pass them and forge ahead.

The Michurin trend in biology represents a qualitatively new and higher stage in the development of Darwinism. After utilizing all the treasures of Darwinism, all that was best in Darwin's theory, I. V. Michurin took an enormous creative step forward in developing this theory.

[1] *The Life and Letters of Ch. Darwin*, Vol. III, London 1888, p. 159.

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285 SPEECH BY E. M. CHEKMENEV

Another example of the good results obtained by employing progressive Michurinist methods, is to be seen on the Sovetskoye Runo State Farm, where the zootechnician S. F. Pastukhov has succeeded in creating a large flock of homebred fine-wooled sheep, which are both highly productive and remarkable for the valuable technological qualities of their wool, a very long, thin, silky and strong wool indeed.

This flock, numbering many thousand animals, ranks among the best sheep flocks of the U.S.S.R.

But what was the attitude of the representatives of formal genetics to these outstanding achievements?

Take, for instance, the book by J. L. Glembotsky, Y. K. Deichman and G. A. Okulichev, entitled *The Stock-Breeding of Fine-Wool Sheep*. There we have the following passage: "Wherever the classing was done systematically by experienced people with a thorough knowledge of the exterior of the sheep and their wool, the result was invariably a considerable
286 increase in the productivity of the flock, and occasionally new breeds or types of sheep were produced. As a recent example of that kind, we may mention the results obtained in the Ipatovsky No. 22 and Sovetskoye Runo No. 11 state breeding farms. There, selection carried on consistently for fifteen years, mainly in respect of the phenotype, has produced splendid flocks of Caucasian Rambouillet, which are to be classed among the best fine-wooled sheep flocks of the U.S.S.R. Nevertheless, we cannot consider this method of selection to be a perfect one because it is based on the conception of the complete identity between phenotype and genotype. Modern genetics has shown that this conception is wrong in principle. . . ."[1]

All one can say is that if the best flock of fine-wooled sheep in the U.S.S.R. is the result of the application of principles that are wrong from the viewpoint of modern genetics, so much the worse for this so-called "modern genetics."

[1] Я. Л. Глембоцкий, Е. К. Дейхман и Г. А. Окуличев, *Племенное дело в тонкорунном овцеводстве*, Москва 1947 г., стр. 163.

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359 **Question.** Do you admit the inheritance of acquired characters? Give a specific answer.

B. M. Zavadovsky. In the diagram which I handed over to the presidium, my answer to this question is the following: As far as Lamarck was concerned all that existed was a scheme of the inheritance of acquired characters. Darwin was not sufficiently precise on this point; he yielded, admitting the inheritance of acquired characters, and thereby, in my opinion, committed a great error.

What has Timiryazev to say on this subject?

I. I. Prezent. Don't tell us about Timiryazev, let us know your point of view.

B. M. Zavadovsky. I am a disciple of Timiryazev and must say this. Timiryazev says that the problem of the inheritance of definite characters requires a differentiated answer. In particular, Timiryazev admitted the inheritance of definite characters in plant organisms, and considered it improbable in animal organisms.

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Academician P. P. Lobanov. Docent S. I. Alikhanian has the floor. 426

S. I. Alikhanian (Genetics Department of the Moscow State University). Comrades, it is very difficult for me, a simple lecturer at the Moscow University, to take the floor after such a fiery speech of the journalist who preceded me, but I shall try my best to lay forth my views on the scientific problems I have been investigating for the past eighteen years.

T. D. Lysenko in his comprehensive survey raised some vitally important problems in the field of contemporary biology. As it is impossible to cover these questions in a short speech, I shall merely express my personal views on heredity and variability, to the study of which I have devoted my life.

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The attacks on the view that genes exist reminds me of the 430
earlier denials of the existence of the atom. Although the atom itself has never been seen, no one doubts its existence today. So it is with the chromosome. There were scientists who vigorously denied the reality of chromosomes. We claim that all plants and animals have specific chromosome complements which vary in number from two or three chromosome pairs to several hundred.

In his speech Academician Mitin has demolished chromosomes and genes with such a ferocity, as though there were nothing more dangerous than a chromosome. But even Academician Lysenko, who is most violently opposed to this theory, wrote the following lines in 1947:

"It is true that chromosomes exist. In the gametes their number is half that of what it is ordinarily. If the gametes bear chromosomal aberrations of any kind they give rise to altered organisms. It is true that the various visible morphological changes of a given chromosome often, and even always, bring about changes in various characters of an organism.

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I therefore once again address myself to Trofim Denisovich, 431
what is idealistic about that? If you consider the existence of the chromosomes and their affinity to the characters of the organism to be a fact, then why not study the structure of these chromosomes? A study of the structure of the chromosome (in your Institute of Genetics chromosomes are studied, aren't they?) discloses its heterogeneity and linear dissimilarity. This has been definitely proved by experiment. I myself have confirmed it experimentally and checked my data cytologically.

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I accept the just reproach that we have insufficiently studied 435
Michurin's heritage and that we have paid but little attention to Michurin's methods. We debate the question who is and who is not a Michurinite, and yet up to this day there is not a single monograph on Michurin published. Why was so little attention paid to the heritage of Michurin, why weren't his works more broadly popularized? The Academy of Agriculture has failed to do this, so have other organizations.

I confess that I am just as much to blame as any of my opponents. Lysenko must be given his due for calling the attention of geneticists to the works of Michurin. I shall not expound upon Michurin's place in biology, I have done that in one of my papers. Here I simply want to stress that a clear-cut summary of Michurin's teachings would do much to advance the application of his ideas to our agriculture. I cannot agree with Lysenko who claims that the cardinal point in the works of Michurin is his teaching about vegetative hybrids. . . .

T. D. Lysenko. Who ever said that? Where did I ever write that?

A voice. Yesterday just the opposite was said.

S. I. Alikhanian. You are always claiming that the teaching about vegetative hybrids is the main point in the works of Michurin.

T. D. Lysenko. You are either intentionally or unintentionally asserting an untruth!

S. I. Alikhanian. I have never done wrong intentionally. I am sincere in everything I do.

T. D. Lysenko. Intentionally or unintentionally you are always putting it so that Lysenko took from Michurin only his teaching about vegetative hybridization. You can find the point anywhere that the cardinal feature of Michurin's teaching (and every Mendelist should get that straight at last) is the role of the environment. That's the point.

A voice. Do you understand now? (*Stir in the audience.*)

S. I. Alikhanian. You interrupted me just when I was about to touch upon the subject. I shall make an effort to answer this point as best as I can.

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448 SPEECH BY I. M. POLYAKOV

In this case, therefore, the creative role of natural selection comes into play, but this is inconceivable if it is divorced from variation and heredity; it cannot be depicted like a sieve which sifts the mutations that arise. Whether the given variations are fit, adapted to life, or whether they will be discarded in the process of evolution is decided by natural selection. This is the fundamental issue between Lamarckists and Darwinists.

If we take it that environment by itself causes adaptive changes in the organism, it will inevitably lead us to theology.

When we say environment we commit a common error; it suggests temperature, the mineral composition of the soil, water, etc. But, as Darwin himself emphasized, the organism's chief environment consists of other organisms, the biological environment. And so, if the external factors, which enter into the process of variation and are an essential condition for the rise of this or that variation, call forth the appearance of diverse characters, the evolutionary value of this diversity may vary considerably in relation to the biological environment.

For example, if certain butterflies acquire a protective colouring to protect them from foes like lizards and birds, the variation in each butterfly is, of course, due to the influence of environmental factors, it is physiological in nature. There can be no doubt about that. Here it is the influence of the external factors. But all this is indefinite. All these variations will be of the indefinite variation type.

T. D. Lysenko. Can it be predicted or not?

I. M. Polyakov. Michurin only raised the question of variation. My idea is perfectly clear. I want to say that, entering the process of hereditary variation, environment causes diversity in the direction of variations. 449

T. D. Lysenko. Can it be predicted or not?

I. M. Polyakov. It is hard for me when I am interrupted.

T. D. Lysenko. It is hard for me when I have to listen to wrong statements. Can it be predicted or not? If, for example, we place a cow under the proper conditions, will her udder grow or not? Can that be predicted or not?

I. M. Polyakov. I say that variations that arise under the elemental conditions of nature proceed in different directions because of the diversity of the organisms and of the conditions surrounding them, and this gives rise to diversity in the direction of the variations. Natural selection decides what is fit and what is unfit.

• • •

Lastly, I want, and feel obliged, to speak about Schmalhausen. The accusations that have been made against me arise from the fact that I have favourably reviewed Schmalhausen's works. I deem it my duty honestly and frankly to say what I think about this now. 452

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There are controversial matters here, and we ought to discuss them. Quotations were read here from his major work, which happens to be the least successful of his books, namely, *Factors of Evolution*. 453

T. D. Lysenko. You wrote a laudatory review of this, in your opinion, least successful of his works, and recommended it for a Stalin Prize!

I. M. Polyakov. Yes, I think that nearly all of Schmalhausen's works are....

T. D. Lysenko. Did you recommend it for a prize?

I. M. Polyakov. It was recommended. It was recommended because there is much that is good also in this book.

A voice. You expressed an official opinion!

I. M. Polyakov. Not only an official opinion....

A voice. Not on all his works, but on *Factors of Evolution*.

I. M. Polyakov. In *Sovietskaya Kniga* I expressed an opinion on all three books of Schmalhausen's including....

A voice. Did you write officially?

I. I. Prezent. Speak more straightforwardly, more precisely.

I. M. Polyakov. What about?

I. I. Prezent. Speak so that everybody may understand what your point of view, your position is.

I. M. Polyakov. My position is this: Schmalhausen's works contain debatable passages, erroneous....

A voice. Which?

I. M. Polyakov. ... but in Schmalhausen's works, on some points, there are a number of ideas important for the theory of evolution, the study of which should not be brushed aside; they are based on an immense amount of biological, embryological and ecological material, and are very important and interesting. Why should we ignore it?

I want to say that there are undoubtedly a number of debatable points in our science and we ought to clear them up by joint, comradely, constructive criticism. In this respect, we must, irrespective of persons, deal sternly with mistakes, with deviations from the main line, and this constructive discussion will help us to formulate the correct point of view. 454

I think it is wrong, and of no benefit to our work, to stick labels on people in lieu of criticism, as is often done. A man may have been fighting Weismannism for fifteen years, but if he happens to disagree with Academician T. D. Lysenko on the question of the struggle for existence, he is labelled a Weismannist. I am not an advocate of "vegetarian relationships" and I am not afraid of sharp talk and criticism, but I think that the task of our science is to unite all that is sound in science around the main, leading trend, and I think that calm, comradely criticism is much more likely to accomplish this than bullying and sticking labels.

• • •

455 A second question. You call for criticism and self-criticism to prevent stagnation, and so forth. That is all very well. Do you know a scientist who only recently said and wrote: I have wavered a long time, not knowing which side to take. Formerly I was closer to the Michurinists, later I firmly decided to support the Schmalhausen side—but now you are with the Michurinists? (*Applause.*)

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456 **Academician P. P. Lobanov.** I call upon Academician P. M. Zhukovsky.

Academician P. M. Zhukovsky. Our disagreements centre mainly around two questions: firstly, the chromosome theory of heredity, and secondly, the influence of external conditions. Trofim Denisovich Lysenko insists on a direct answer to these questions.

As regards the chromosome theory of heredity. It would be deplorable if the entire group of geneticists that has been labelled Mendelist-Morganists were, from this rostrum, to renounce the chromosome theory of heredity. I do not intend to do that. The cardinal fact for me, a student of the plant world, is the alternations of generations in the plant world, accompanied by changes in the nuclear phases.

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459 Thus, the chromosome theory helped us to ascertain the origin of cultivated cotton.

The same applies to cultivated tobacco, which has 24 chromosomes. It has been proved that 12 of these chromosomes belong to the wild variety of Nicotiana sylvestris, while the other set of 12 belongs to the wild variety of Nicotiana Rusbyi or, what is the same thing, Nicotiana tomentosa.

T. D. Lysenko. Has at least one cytologist proved that it is possible to see either the paternal or the maternal chromosome?

P. M. Zhukovsky. I will leave this rostrum if I am interrupted. There are many such cytologists, those who understand cells.

Knowledge of the chromosome theory enabled us to ascertain the origin of many plants, including the origin of such enigmatic plants as maize.

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464 **P. M. Zhukovsky.** Our opponents never mention such terms as vitamins, hormones or viruses. I would advise, not you, Trofim Denisovich, your authority is sufficiently high, but your followers to study, for knowledge is light, while ignorance is darkness. (*Laughter, applause.*)

T. D. Lysenko. Do you apply that to yourself?

P. M. Zhukovsky. I am always studying.

T. D. Lysenko. You don't study very hard.

P. M. Zhukovsky. If you knew how I live, you would know that I do study hard, I study every day....

S. S. Perov. You only read books.

P. M. Zhukovsky. Comrade Dmitriyev, Chief of the Agricultural Planning Administration of the State Planning Commission, said here that there should be no schools.

T. D. Lysenko. Quite right!

P. M. Zhukovsky. I don't know whether Academician Mitin is present, I am afraid he has not displayed proper vigilance. Yesterday's issue of the *Literaturnaya Gazeta* contains an article by Academician Urazov entitled: "Take Care of the Schools. . . ." (*Laughter.*) I think we should take care of scientific schools; we have many in the Soviet Union, and we cannot have only one scientific school.

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I want to make a personal request to Trofim Denisovich. 466
Trofim Denisovich, instruct your organization to issue a comprehensive manual on how to train plants, on how to alter them. Teach us; we too want to learn, and if your methods prove effective, we will accept them. I want agreement. We are just as human as your pupils. You are wrong in sticking labels on us. Trofim Denisovich, this year I lectured to your son. Ask him whether my lectures have spoilt him.

T. D. Lysenko. There is no need to go into family matters.
That is my business as a father. Tell us something that is more 467
important. You complain of being ill-treated. But have you forgotten, Pyotr Mikhailovich, what names I was called in your presence? Did you protest then?

P. M. Zhukovsky. You were never abused in public, at meetings.

T. D. Lysenko. In holes and corners.

P. M. Zhukovsky. That was the gossips. For all that, I want to call for unity. I am one of those who want to work in harmony and not in enmity. We are all Soviet citizens, and we are all patriots. Some of us went personally and others sent their sons to the front. We all fought for our country, and should we really allow things to reach such a pass that people refuse to greet Professor Zhukovsky when they meet him?

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SPEECH BY A. R. ZHEBRAK 474

A. R. Zhebrak. I said that our polyploids have been bred only up to the fifth generation. (*Commotion in the hall.*) We have 96 forms; about fifty of them are promising inasmuch as they excel our standard types.

S. S. Perov. In how many years do you intend to hand them over?

A. R. Zhebrak. Next year we intend to hand over to the State Cereal Variety Trial Commission the material concerning several of our types. This year we shall reproduce them.

T. D. Lysenko. You say that this year you intend to reproduce them; but the year is almost over!

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Contemporary experimental genetics has mastered the 476
means of remodelling the hereditary basis of plants and reconstructing the vegetable world.

Voice from the audience. What has experimental genetics given to industry?

A. R. Zhebrak. In my opinion these investigations raise the level of our Soviet science. They are in unison with Comrade Stalin's instruction that the task of Soviet scientists is not only to overtake the achievements of science of other countries, but to outstrip them.

A voice. What science?

A. R. Zhebrak. The group of investigators that are working under my lead will do its utmost in the field of experimental polyploidy to fulfil the instruction of our great leader and teacher. (*Applause.*)

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486 SPEECH BY N. V. TURBIN

A few words about Comrade Rapoport's interjection about cytogeneticists being aware of useful mutations and their ability to produce them artificially. I do not know what facts he is referring to. I can only recall that in summing up the entire literature on this subject, Academician Schmalhausen arrived at the conclusion that useful adaptive mutations are unknown. Proceeding from this, he built up the theory of stabilizing selection, which is supposed to explain how organic evolution, possessing features of the adaptive process, can take place in the absence of adaptive mutations in nature. We can speak of useful mutations only in one sense, namely, that these mutations are useful to those who are studying them, because, while the mutations that cytogeneticists are studying do not
487 and cannot serve as a source of material for organic evolution, they are quite a reliable source of material for writing dissertations and for the relatively easy receipt of scientific degrees.

J. A. Rapoport. It is a better theory than yours. Obscurantists!

N. V. Turbin. Wishing to sting the Michurinists, Comrade Rapoport said that we must train honest cadres who will look facts straight in the face and not lie to themselves or to others. But the methods to which Comrade Rapoport resorts in defending the theory of the gene—hushing up and shying at facts, distorting well-known facts, offensive interjections and hysterical outcries—all indicate that Comrade Rapoport himself does not belong to the honest cadres.

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499 SPEECH BY I. N. SIMONOV

Let us, comrades, recall the celebrated session of horticulturists that was held two years ago. Many of you present here today witnessed the obstruction that was raised by some of the students of the Timiryazev Academy against Academician P. N. Yakovlev, Michurin's best pupil. You, esteemed Academician Nemchinov, as Director of the Academy, undoubtedly bear moral responsibility for the training of your students in a totally wrong direction. . . .

A voice. Not only moral, but political.

I. N. Simonov. Yes, and political responsibility.

I will quote another example to show in which direction—the Michurin or some other—the Timiryazev Academy is going today. This example is really worth quoting.

Two years ago applications were called for by our Timiryazev Academy to fill a chair that one would think was remote from the present controversy—the chair of the technology of fruit and vegetable plants. One of the applicants was B. A. Rubin, one of Bach's pupils, a biochemist, a professor, and now holder of a Stalin Prize. But he was not elected. His application was rejected not because he lacked the scientific qualifications for a post at the Timiryazev Academy, but because he subscribes to Michurin's teachings. They did not want to "pollute" the precincts of the Timiryazev Academy with persons of that kind.

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I will say more. Last year, with the knowledge of Academician Nemchinov, the last base for practical plant breeding 500
by students was liquidated. ...

Voices. Hear, hear!

I. N. Simonov. This was a collection of European and Russian gooseberries and currants. Material which had just reached the real fruit-bearing stage (ten-year-olds) was sold by auction. Those who witnessed the scene could not leave that spot without emotion. The collection was sold by auction! Perhaps you will say, Academician Nemchinov, that the orchard with fruit trees survived. Yes, that orchard survived by a strange accident, although the fence was already knocked down by a tractor. But permit me to ask Academician Nemchinov, what work is being done in that orchard?

• • •

V. S. Nemchinov.

As the director, I may be reproached with one thing, and 557
that is that I make a distinction between the article of Professor Zhebrak, Member of the Byelorussian Academy of Sciences, and his work. I said that Professor Zhebrak's article to the 558
American journal was condemned not because he adheres to and upholds the chromosome theory of heredity, but because he had committed an unpatriotic act. And so it was.

A voice. Is the chromosome theory part of the gold fund?

V. S. Nemchinov. Yes, I may repeat it, I do consider that the chromosome theory of heredity has become part of the gold fund of human knowledge, and I continue to hold that view.

A voice. But you are not a biologist, how can you judge?

V. S. Nemchinov. I am not a biologist, but I am in a position to verify this theory from the point of view of the science in which I am doing research, notably, statistics. (*Commotion.*)

And it also conforms with my ideas. But that is not the point. (*Commotion.*)

A voice. How is it not the point?

V. S. Nemchinov. Very well, let it be the point. But I must then declare that I do not share the view of the comrades who assert that chromosomes have nothing to do with the mechanisms of heredity. (*Commotion.*)

A voice. There are no mechanisms.

V. S. Nemchinov. You think there are no mechanisms. But this mechanism can not only be seen, it can be stained and defined. (*Commotion.*)

A voice. Yes, stains. And statistics.

V. S. Nemchinov. Nor do I share the view which was expressed here by our respected chairman that the chromosome theory of heredity and, in particular, certain Mendelian laws, are an idealist standpoint, a reactionary theory. I personally consider this opinion wrong, and this is my point of view, although it may not be of much interest. (*Commotion and laughter.*)

A voice. It is of great interest.

V. S. Nemchinov. I have never concealed this point of view. It is my point of view, the point of view of a man who is not a specialist.

A voice. Coming from the director of the Timiryazev Academy, it would be interesting to hear it.

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A voice. Tell us what is your attitude to the principles of 559
the address.

V. S. Nemchinov. My attitude to T. D. Lysenko's address is as follows. His basic precepts and basic ideas, the purport of which is to mobilize agrobiological science in the service of
560 collective farming and to transfer the methods of his work to all the collective-farm fields, I consider correct.

A voice. Theoretically.

V. S. Nemchinov. As to the theoretical side of it, I consider that in the matter of the chromosome theory of heredity Trofim Denisovich Lysenko is wrong.

A voice. But you're not a specialist.

V. S. Nemchinov. No, I am not a specialist, that is why I did not speak until you asked me.

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616 Comrades, our session is drawing to its close. This session has vividly demonstrated the strength and potency of the Michurin teaching. Many hundreds of representatives of biological and agricultural science have taken part in it.

They have come here from all parts of our vast country.
617 They have taken an active part in the discussion on the situation in biological science and, convinced in the course of many years of practical activity that the Michurin teaching is right, are ardently supporting this trend in biological science.

The present session has demonstrated *the complete triumph of the Michurin trend over Morganism-Mendelism.* (*Applause.*)

It is truly a historic landmark in the development of biological science. (*Applause.*)

I think I shall not be wrong if I say that this session has been a great occasion for all workers in the sciences of biology and agriculture. (*Applause.*)

The Party and the Government are showing paternal concern for the strengthening and development of the Michurin trend in our science, for the removal of all obstacles to its further progress. This imposes upon us the duty to work still more extensively and profoundly to arm the state farms and collective farms with an advanced scientific theory. That is what the Soviet people expect of us.

We must effectively place science, theory, at the service of the people, so that crop yields and the productivity of stock-breeding may increase at a still more rapid pace, that labour on state farms and collective farms may be more efficient.

I call upon all Academicians, scientific workers, agronomists, and animal breeders to bend all their efforts and work in close unity with the foremost men and women in socialist farming to achieve these great and noble aims. (*Applause.*)

Progressive biological science owes it to the geniuses of mankind, *Lenin* and *Stalin,* that *the teaching of I. V. Michurin has been added to the treasure house of our knowledge, has become part of the gold fund of our science.* (*Applause.*)

Long live the Michurin teaching, which shows how to transform living nature for the benefit of the Soviet people! (*Applause.*)

Long live the Party of Lenin and Stalin, which discovered Michurin for the world (*applause*) and created all the conditions for the progress of advanced materialist biology in our country. (*Applause.*)

Glory to the great friend and protagonist of science, our leader and teacher, Comrade Stalin! (*All rise. Prolonged applause.*)

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Academician P. M. Zhukovsky. Comrades, late yesterday evening I decided to make this statement. I say late yesterday evening deliberately, because I did not know then of the letter of Yuri Zhdanov which appeared in *Pravda* today. There is therefore no connection between my present statement and Yuri Zhdanov's letter. I think Vice Minister of Agriculture Lobanov will bear this out, since I phoned him yesterday evening and requested permission to make a statement at today's meeting of the session. 618

There are moments in a man's life, especially in our historic days, which are to him of profound and crucial moral and political significance. This is what I experienced yesterday and today. The speech I made the day before yesterday was an unhappy one; it was the last of my speeches against Michurin, as it is said here to have been, although I personally have never before spoken in opposition to Michurin's teachings. At the same time, it was my last speech from an incorrect biological and ideological standpoint. (*Applause.*)

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The speech I made the day before yesterday, at a time when the Central Committee of the Party had drawn a dividing line between the two trends in biological science, was unworthy of a member of the Communist Party and of a Soviet scientist.

I admit that the position I held was wrong. Academician Lobanov's noteworthy speech yesterday, and I esteem P. P. Lobanov as a fine statesman, his words directly addressed to me —our ways must part—moved me deeply. His speech agitated me profoundly. A sleepless night helped me to think over my behaviour.

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Academician P. P. Lobanov. Comrade S. I. Alikhanian wishes to make a statement. 620

S. I. Alikhanian. Comrades, it was not because I had read Yuri Andreyevich Zhdanov's statement in today's *Pravda* that I requested the chairman to allow me the floor. I decided yesterday to make a statement, and Vice Minister of Agriculture P. P. Lobanov can confirm that I spoke to him on the subject yesterday, August 6.

I have followed this session very attentively, and I have lived through a lot in these days. From all that has taken place at this session, from all that I, as a scientist, have pondered over, it behoves me, as a young Soviet scientist, to draw a fundamental conclusion. What is involved, comrades—I here address my like-thinkers....

N. G. Belenky. Past or present?

S. I. Alikhanian. Both past and present. The issue involved is a struggle between two worlds, between two world outlooks, and it would be senseless to cling to the old tenets which were inculcated in us by our teachers.

We strongly succumbed to the polemical passion kindled by our teachers in this controversy. Because of this polemical ardour we were unable to discern the new and growing trend in genetic science—the Michurinian trend. And, as I have said, it is important to realize that we must be on this side of the scientific barricades, with our Party and with our Soviet science.

It would be foolish to think that we are being asked to discard everything good and useful accumulated in the course of the development of science. What we are being asked is to dis-

621 card everything reactionary, false and useless. And we must do so sincerely and honestly, as befits real scientists.

I appeal to my comrades to draw very serious conclusions from these words of mine. I, as a Communist, cannot and must not, in the ardour of controversy, obstinately oppose my personal views and concepts to the onward march of biological science.

When I leave this session, the first thing I must do is to review not only my attitude towards the new, Michurinian science, but my entire earlier activity. I call upon my comrades to do likewise.

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622 **Academician P. P. Lobanov.** Professor I. M. Polyakov has the floor for a statement.

I. M. Polyakov. Comrades, yesterday evening I said in conversation with friends who are present here today that this session was a big event in my life, that it has agitated me deeply and caused me to revalue many of my ideas.

Comrades are familiar with my speeches at numerous scientific congresses and conferences, with my scientific articles and textbooks. I have always striven honestly to analyze and understand the big and important issues in the theory of evolution, in Darwinism and genetics. I have striven to weigh fundamental theoretical principles in our science from the Marxist-Leninist standpoint, to trenchantly criticize the reactionary views of foreign scientists and of some of our scientists. Comrades know that I have done this for many years.

But when the reproach was levelled at me from this platform that my speech was vague, the reproach was justified. Not to go to the end, to take up an intermediate position, is something unworthy of a Bolshevik scientist. One must formulate one's position clearly and distinctly. One must frankly say that the Michurinian trend is the highroad of development of our biological science, and this is the road we must follow. It is the only possible road for Bolsheviks, Party and non-party, who desire to work in the field of our biological science and bring benefit to our Soviet people, to our Country.

623 I should like to remark that for the past eight or nine years I and my close associates have been working on the problem of elective fertilization, one of the most important problems of the Michurinian genetics. We have arrived at a number of interesting conclusions, and these conclusions fully bear out the Michurinian view. I have written about this in works which are now in the press. But one cannot stop there: further conclusions have to be drawn. One must be logical and not try to reconcile irreconcilables.

For the scientist who takes his science seriously and loves it, to change front in short order is hardly a seemly thing. On many issues I still have to do a great deal of serious thinking. On a number of problems of our science we can and should argue fruitfully and constructively. If, for example, we argue over the struggle for existence and selection, there is nothing amiss in that, since comradely disputes among Soviet scientists on concrete scientific problems can be only useful. But it is necessary to understand the chief and fundamental thing, namely, that our Party has helped us to effect a profound and radical reconstruction of our science, has shown us that the Michurinian theory defines the basic line of development of Soviet biological science, and from this we must draw the conclusion and work to promote the Michurinian trend. And this must be demonstrated by one's work, and not simply by words. This must be the program of my work as a Communist scientist. If you do not take this road then, willy-nilly, you will attract people with a penchant for unprincipled factionalism, people who are unable to see behind individual scientific disputes the big and fundamental things that are being done in our country. I urge all our Soviet biologists to come to the same conclusion that I have come to. For many this will not be simple or easy; it will require deep and serious thinking. But, I repeat, it is necessary completely to break with the false views, vigorously to criticize the metaphysical, idealist, Weismannist views of foreign reactionaries in the field of science and the re-echoings of these views in the works of certain Soviet scientists.

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Academician P. P. Lobanov. A motion has been received to send on behalf of the participators at this session a message of greeting to Comrade J. V. Stalin. (*Loud and continuous applause and cheers.*) 624

The draft of the letter will be read by Academician I. D. Kolesnik.

(*Academician I. D. Kolesnik reads the draft of a message of greeting to J. V. Stalin.*)

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From the Session of the Lenin Academy of Agricultural Sciences of the U.S.S.R. to *J. V. STALIN*

Dear Joseph Vissarionovich,

The Academicians, agronomists, animal breeders, biologists, farm mechanization experts and organizers of socialist agriculture present at this session of the Lenin Academy of Agricultural Sciences send you their hearty Bolshevik greetings and best wishes.

Every day and every hour, agricultural scientists and practical workers are conscious of the deep solicitude of the Communist Party and the Soviet Government for agricultural science, and of your constant personal efforts for its promotion and progress.

Science in our country owes a debt of gratitude to you, the great builder of Communism, for having in your works of genius enriched and elevated it in the eyes of the world, for having protected it from the danger of becoming divorced from the needs of the people, for helping it to triumph over reactionary doctrines inimical to the people, and for concerning yourself for the constant widening of the intellectual horizon of our scientific workers.

Continuing the work of V. I. Lenin, you saved for progressive, materialist biology the teachings of that eminent transformer of nature, I. V. Michurin; you held up the Michurin trend in biology to science as the only correct and progressive trend in all branches of biology. This has helped still further to strengthen the natural-scientific foundations of the Marxist-Leninist world outlook, the all-conquering power of which is confirmed by the whole experience of history.

You, our dear leader and teacher, constantly help Soviet scientists to develop our progressive materialist science, which serves the people in their labours and valorous endeavours, the

science which reflects the world outlook and noble aims of the citizen of the new, socialist society.

626 The collective farm system, created under your wise guidance, has opened up boundless possibilities for a powerful expansion of productive forces in all branches of agriculture and has demonstrated its invincible strength. The Party of Lenin and Stalin has reared among the collective farm peasantry splendid fighters for high yields in agriculture and animal husbandry. The Michurinian agricultural science, urged by you more boldly and determinedly to develop research in the active transformation of the nature of plants and animals, arms the practical farmers in their struggle for a highly efficient socialist agriculture. In their turn, the foremost men and women in the collective farms—the agricultural innovators—stimulated by countrywide socialist competition, are enriching our science with new methods and new achievements.

We assure you, dear Joseph Vissarionovich, that we shall bend every effort to assist the collective farms and state farms in securing even higher yields from our socialist fields and higher productivity of collective farm and state farm animal husbandry, in order to ensure an abundance of produce in our country as one of the major conditions for the transition from Socialism to Communism. We see the possibility of achieving this lofty aim in close unity between science and the people, between science and the foremost men and women in the collective farms, as you are constantly teaching us, Party and non-party Bolsheviks. A science which hedges itself off from the people, from practice, is no science.

Our agrobiological science, developed in the works of Timiryazev, Michurin, Williams and Lysenko, is the foremost agricultural science in the world. It is not only the lawful heir of the progressive ideas of the advanced scientists in human history; it represents a new and higher level of development of human knowledge in the realm of agricultural efficiency. The Michurinian doctrine is a new and higher stage in the development of materialist biology. The Michurinian biological science will continue creatively to develop Darwinism, unswervingly and determinedly to expose the reactionary, idealist Weismann-Morganian scholasticism, which is divorced from practice, to combat the servile worship of bourgeois science which is unworthy of Soviet scientists, and to emancipate researchers from survivals of idealist, metaphysical ideas. Pro-
627 gressive biological science repudiates and exposes the false idea that it is impossible to govern the nature of organisms by creating man-controlled conditions of life for plants, animals, microorganisms.

Science should teach the experimenter to be audacious in the search for ways and means of governing nature in the interest of man.

To this we are inspired by the teachings of Marx, Engels, Lenin and Stalin, which have proved so triumphant in science and practice.

To this we are enthused by your words about progressive science, science which serves the people, science which values traditions, but does not fear to raise its hand against all that is obsolete.

Hail the progressive Michurinian biological science!

Glory to the great Stalin, the leader of the people and coryphaeus of progressive science!

(Stormy, prolonged and mounting applause and cheers. All rise.)

LAAAS, *The Situation in Biological Sciences*, Proceedings of the Lenin Academy of Agricultural Sciences of the USSR, Verbatim Report, Foreign Languages Publishing House, Moscow, 1949.

6.2

Maxim W. Mikulak

LYSENKO'S THEORY OF INHERITANCE

Lysenko's Evolution without Genetics

Trofim Denisovich Lysenko, born in 1898 in the Ukrainian village of Karlovka, became the chief beneficiary of Stalin's policy of Bolshevization, as it applied to biology, and a *bete noire* to Soviet geneticists and the survival of their science. His formal training, lasting from 1913 to 1925, was in horticulture, with a heavy accent on practical success in selective breeding. His first claim to national fame was the development of a process of pretreating seeds (vernalization) which shortened the time between seeding and harvesting. Only after Lysenko offered a theoretical explanation of vernalization, which utilized the Lamarckian doctrine of the inheritance of acquired characteristics, did he become involved in the theoretical debates of the biology of heredity. At the meeting of the Seed-Growing Farms Union, held in January 1934, Lysenko's views on heredity and plant development were severely criticized by the scientists. In response to his critics, Lysenko argued in his "Theoretical Principles of Vernalization" that his theory of the phasic development of plants not only conformed to the Marxist criterion of practice but followed Darwin's doctrine of natural selection, K. A. Timiriazev's historical method of biology, and Michurin's teaching that man can control the individual development of organisms. In subsequent encounters with geneticists, Lysenko consistently maintained that his theories were a continuation of the ideas of Marx, Darwin, Timiriazev, and Michurin.

It is not difficult to assemble the objections that Lysenko raised against classical genetics; for the most part he reiterated the arguments of the naturalists, who at the turn of the century saw in the rise of genetics a threat to Darwinian evolution. In Lysenko's judgment the pivotal issue of this controversy with the geneticists, particularly Bateson, Lotsy, Johannsen, and Morgan, was their "denial of the creative role of natural and skillful artifical selection of evolution" which shows "the fundamental theoretical conceptions of genetics are not developing on the plane of Darwin's theory of evolution." He accused the geneticists of divorcing themselves from the real biological laws of development by paying undue attention to hereditary factors as expressions of abstract mathematical probability laws. Finally, he deplored the inability of the geneticists to predict and control the appearance of desired characters in plants and animals. In Lysenko's view, the Lamarckian propositions on the inheritance of acquired characters and on the influence of external conditions on the course of the development of living organisms were "closer to the truth" than the Neo-Darwinists who "indulge in mysticism."

Lysenko was not quite satisfied with some of Darwin's concepts and therefore in the postwar period felt compelled to modify Darwin's general line of thinking – a modification that was to result in "creative Darwinism." Following the arguments of Marx and Engels, Lysenko maintained that the employment of the Malthusian doctrine of over-population was not fruitful in understanding evolution. He also disapproved of intraspecific competition as a contributing factor in the evolution of species, stating that such competition simply cannot be observed in nature. However, he did acknowledge that struggle and competition among species are the motive forces in the process of evolution. The most serious defect that Lysenko saw in Darwinism was its preaching of a "one-sided and flat evolutionism" which concentrated on quantitative changes at the expense of qualitative changes in the transformation of species. Inasmuch as Darwinian evolution is a continuous process, Lysenko noted, Darwin did not accept discontinuities between species in nature and consequently was forced to fall back on the doctrine of intraspecific competition in order to account for the gaps observed between species. To Lysenko, species were a "distinct, qualitatively definite state of living matter" instead of a convenient convention of biological classification. Of special significance is Lysenko's assertion that the itermediate forms of life, which Darwinism calls for, never existed and that new species arise out of older ones without passing through any intermediary stages.

M. W. Mikulak, 'Darwinism and Soviet genetics', *Journal of the History of Ideas*, vol. 31, no. 3, 1970, pp. 369–70.

Jan Sapp 6.3

THE STRUGGLE FOR AUTHORITY IN HEREDITY

In an attempt to break away from the point of view which sees the 312
development of science as the result of intrinsic logical necessity, I shall attempt in this paper to reveal the inseparable social and intellectual determinants which led to the rise of genetics. This task requires the introduction of a different theoretical framework. Before beginning then, I need to introduce some notions of seminal importance to an understanding of my work.

First, I view the rise of genetics not as the result of agreement or consensus, but as a struggle for power and authority. Pierre Bourdieu[4] has characterized the scientific field that is the site of this battle as a system of objective relations involving a competitive struggle between positions already won, in which the particular issue at stake is the monopoly of scientific authority. Scientific authority or competence is understood as the particular agent's socially recognized capacity to speak and act legitimately in scientific matters; it is defined indistinguishably as technical capacity and social power. It should be stressed that when referring to the struggle for scientific authority in the field, it is impossible to differentiate intellectual and social or "political" determinants. The very definition of the field itself cites "the objective space defined by the play of opposing forces in a struggle for scientific stakes."[5]

Second, instead of viewing disciplines as purely intellectual entities, I add a social dimension that involves control and normalization, which are enacted as a strategy to monopolize authority.[6] Genetics, then,

4. Pierre Bourdieu, "The Specificity of the Scientific Field and the Social Conditions of the Progress of Reason," *Soc. Sci. Info.*, *6* (1975), 19–47.
5. Ibid., p. 21.
6. This view of a discipline is informed by the work of Terence J. Johnson, *Professions and Power* (London: MacMillan, 1972). See also Richard Whitley, "Umbrella and Polytheistic Scientific Disciplines and Their Elites," *Soc. Stud. Sci.*, *6* (1976), 471–497.

will not be understood to mean the study of heredity with an origin 313
in antiquity, nor to mean simply Mendelian studies. Rather, genetics will be understood to involve a particular process of social control established as a strategy to dominate the scientific field of heredity.

In this essay I attempt to analyze the emergence of genetics with respect to its power relations with other disciplines. Instead of investigating the conceptual steps leading to a synthesis, I focus on the social and cognitive relations of biological disciplines engaged in a struggle for authority in the field of heredity. At the turn of the century several distinct notions of heredity were produced from within specific disciplines which struggled for authority in the field. The question normally posed, "How did the Mendelian theory of heredity come to be generally accepted?" is replaced here by the question, "How did genetics, based on the Mendelian notion of heredity, come to be the dominant discipline in the field of heredity?"

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THE FIELD OF HEREDITY: 1900–1916

During the first decade of the century, when the Mendelian laws were first proposed, heredity was a central – though extremely vague – concern of biology. Certainly, inasmuch as heredity was thought to be responsible for the similarities and dissimilarities exhibited in successive generations, it was understood to be important to the highly valued problem of evolution. More specifically, biological problems which supposedly relied on heredity were diversified and included such matters as the nature of variations, which in turn were proposed as the basis of the mechanisms of evolution and animal and plant breeding; how an organism grows, develops, and is maintained; why certain parts of the organism are capable of producing the whole (totipotency); how characters are transmitted from one generation to the next; the physical basis of inheritance; and the course of evolution.

314 These problems were the concern of several more or less sanctioned areas of practice, such as cytology, embryology, physiology, practical breeding, natural history, biometry, and so on. Heredity was the "natural" product of different contexts of production. Indeed, how an individual understood the term resulted largely from the methods, theories, explanatory standards, and overall scientific objectives of his particular discipline. Each discipline was characterized by a range of possiblities within which the knowledge of heredity developed. The possibilities were defined not only by the current theories or beliefs about heredity, but by the nature of the objects accessible to investigation, the equipment available for examining them, and the methods of observing and discussing them.

To begin with, Mendelism was based essentially on an experimental and statistical examination of the reappearance of visible differences between individuals of a species. Led until World War I by the British biologist William Bateson, Mendelism supported the notion of discontinuous evolution. Biometry, on the other hand, led by W. F. R. Weldon and Karl Pearson who supported continuous variations, was based on the "Galtonian theory of ancestral heredity" and on statistical examination of visible characters within populations. Heredity for biometricians was a statistical law. "Heredity", wrote Pearson in 1900, "is the law which accounts for the change of type between parents and offspring, *i.e.* the progression from racial towards the parent type."[7]

In addition to investigations grounded in biometric theory, those of paleontology, entomology, systematics, and morphology, here referred to collectively as natural history, were based on a study of the visible characteristics of the organism. While biometricians constructed a statistical conception of heredity, for natural historians of this period heredity was essentially a historical notion investigated by describing the past. Naturalists explored heredity and evolution by compiling data illustrating relationships between new forms of living and extinct organisms brought to light by expeditions sent out from universities and museums. Unlike Mendelian investigators, who were concerned with differences among individuals, naturalists investigated heredity as the link responsible for the similarities and differences which accounted for the relationships among phyologenetic groups. Of course, for many naturalists such as A. Hyatt and E. D. Cope, who believed in

7. Karl Pearson, *The Grammar of Science*, 2nd ed. (London: MacMillan, 1900), p. 474.

315 the inheritance of acquired characteristics, the extraorganismic environment played a direct role in heredity.[8]

In contrast to members of these disciplines, cytologists were concerned with the material link between generations, with what they considered to be "the physical basis of heredity." Cytology was based on microscopic examination of the cell, and since the 1870s cytological investigations had been primarily concerned with the morphological study of eggs and oogenesis, spermatozoa and spermatogenesis, and fertilization. Following this work on the early history of germ cells and fertilization of the ovum, the principal goal of cytology was to reconcile cell theory with evolutionary theory. It became clear that, in the words of E. B. Wilson, "the general problems of embryology, heredity, and evolution are indissolubly bound up with those of cell structure, and can only be fully apprehended in the light of cytological research."[9]

By the last decades of the nineteenth century, with the help of improved staining techniques, cytologists had accumulated evidence for the physical continuity of the cell nucleus and claimed that it played a direct role in heredity.[10] Nonetheless, heredity remained an obscure notion from a cytological point of view. As late as 1914, Wilson wrote: "Our conceptions of cell organization, like those of development and heredity, are still in the making. The time has not yet come when we can safely attempt to give them very definite outlines."[11]

The objective of experimental embryology, beginning in the 1880s, whether *Entwicklungsmechanik, embryologie causale*, or the physiology of development, was to investigate the causes (physical, chemical, physiological, mechanical) of development, which embryologists claimed to be the same as those of heredity. In contrast to Mendelian workers, embryologists were not concerned with the transmission of traits; they viewed heredity as a process, a *production*. To embryologists heredity was concerned with all the morphological and physiological characters which the descendant shared with its parents. The fact that

8. For a list of the views of naturalists on heredity and evolution, see Allen, "Naturalists and Experimentalists," esp. pp. 188–189.

9. Edmund B. Wilson, *The Cell in Development and Inheritance*, 2nd ed. (New York: MacMillan, 1900), p. 6.

10. See William Coleman, "Cell, Nucleus, and Inheritance: An Historical Study," *Proc. Am. Phil. Soc., 109* (1965), 124–158.

11. Edmund B. Wilson, "The Bearing of Cytological Research on Heredity," *Science, 88* (1914), 333–352; quotation on p. 352.

the egg of a rat always gave birth to a rat, and that of a frog a frog, 316
represented a first aspect of the phenomenon of heredity. Heredity was the product of the totality of the structures, of the organs, of the relations between parts and their functioning, which characterized the organized form. On the other hand, Mendelian investigators were confined by the nature of their practice, based on experimental breeding, in viewing heredity not as a production, but rather as a *distribution* and *exchange*.

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In a manner similar to embryologists, physiologists were concerned with the notion of heredity, for it was at the core of their concept of "organism". What physiologists sought in heredity were the "shaping and controlling factors" which gathered materials in such a way that they combined in the proper manner and at the proper time.

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An additional group of practitioners who played a leading role in 317
heredity investigations during the first decades of the century were breeders situated outside the universities. For these individuals neither evolution, development, nor the integrity of the organism were issues. At the time of the rise of experimental breeding based on Mendelian theory in academic institutions, plant and animal breeders joined

318 to view heredity as an important "economic force." The so-called hereditary values of specially bred strains of plants and animals were claimed to be as real as the seemingly more concrete values of land or of goods. The value of the "unseen carriers of heredity" was considered by some in 1910 to be "far above that of gold."[15] Perhaps the notion of heredity for breeders is best represented by the following metaphorical passage: "Heredity is a force more subtle and more marvelous than electricity. Once generated it needs no additional force to sustain it. Once new breeding values are created they continue as permanent economic forces."[16]

Finally, eugenics, "the science that deals with all the influences that improve the inborn qualities of race,"[17] must be considered. Concerned with the improvement of humankind, eugenicists during the first two decades of the century concentrated their efforts in two directions: increasing the reproduction of especially "fit" individuals and reducing the breeding of individuals they viewed as "unfit."

Certainly these groups had different interests and problems. However, I suggest that the differentiation of biologists into sanctioned areas of competence cannot be viewed only in terms of a convenient division of labor. To the degree that members of each practice defined and explored heredity with their own methods and theories, each group claimed the value of its approach to be greater than that of the other. The contending disciplines represented divergent views concerning what questions were important, what answers were acceptable, what techniques were appropriate, what phenomena were interesting. The significance of these diverse biological problems and various views of heredity was not hierarchically ordered within biological research by an intrinsic logical necessity of scientific thought. Rather, their importance to biology depended directly on both the technical capacity and the institutional power of the discipline within which they were produced.

15. These views of breeders are expressed repeatedly in essays and editorials published between 1910 and 1914 in the *American Breeder's Magazine*.

16. "Heredity: Creative Energy," *Am. Breeder's Mag., 1* (1910), 79.

17. This is Galton's definition of genetics; see "Breeding, Genetics, Eugenics," *Am. Breeder's Mag., 3* (1912), 308–309. For brief accounts of eugenics and its relation to genetics in the United States see for example Garland E. Allen, "Genetics, Eugenics, and Society: Internalists and Externalists in Contemporary History of Science," *Soc. Stud. Sci., 6* (1976), 111. See also his "Genetics as a Social Weapon," in Rita Arditti, Pat Brennan, and Steve Cavrak, eds., *Science and Liberation* (Montreal: Black Rose Books, 1980), pp. 48–62.

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334 THE INSTITUTIONALIZATION OF GENETICS IN THE UNITED STATES, 1914–1932

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335 The rapid rise of genetics to a dominant institutional position in American universities was the result of several factors. Certainly, Mendelian analysis and the use of drosophila cannot be underrated as important techniques in producing quick results.[69] Rapid production plays an important role in the recruitment and institutionalization of researchers. With a prolific technique to be exploited, students are attracted into an area where their chances of success are high. On the other hand, a theory with a large explanatory range that lacks a technique capable of producing fast results obviously requires longer to convince as well as to recruit researchers. Quick recrutiment also means being able to take advantage of institutional opportunities as they become available. The alliance between cytology and genetics in the United States, established with the help of E. B. Wilson, must also be considered an important source of authority for genetics.

The rise of genetics was not determined soley by the operation of sources of power and authority internal to an academic "scientific community."[70] Mendelian studies found favor with practical breeders and with government, both of which played significant roles in the origin and maintenance of this differentiation. Mendelian analysis

68. F. B. Sumner to Pearl, Febuary 9, 1928, cited in Provine, "Francis B. Sumner," p. 235.

69. For information concerning the institutional growth of genetics during this period, see Kevles, "Genetics," p. 451. See also Charles E. Rosenbert, *No Other Gods* (Baltimore: Johns Hopkins University Press, 1976), pp. 196–209.

70. For a sociological analysis of the influence of agents outside the "scientific community" on the differentiation of scientific specialities, see Ron Johnston and Dave Robbins, "The Development of Specialities in Industrialized Science," *Sociol. Rev.*, *25* (1977), 87–108.

showed breeders that every organism displaying a specific trait was not necessarily purebred for that trait; that is, with Mendelian theory breeders could detect whether a certain line was purebred or hybrid. Such detection was extremely valuable in crossing plants, where such traits could be followed easily, in order to improve the quantity and/or quality of the yield.[71] Beginning in the first decade of Mendelism, major breeding work aimed at creating improved forms of pedigreed plants and animals was carried out tin the U.S. Department of Agriculture, in state experimental stations, and under other public and private auspices.[72] Clubs devoted to the study of heredity had been organized in centers of learning across the United States, and philanthropists were encouraged to dedicate generous sums of money to endow university chairs of genetics.[73] 336

One can distinguish two phases in the United States between 1900 and 1932, characterized by different relationships between breeders and Mendelians. In a first phase, ending around 1915, there was little distinction between what was known as "genetics" and so-called practical breeding.

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During a second phase, after 1915, genetics in the United States became clearly distinguishable from practical breeding and eugenics. This was when genetics, led by the drosophila group of T. H. Morgan, 337

75. W. Bateson, "Address to the Agricultural Sub-Section," *Brit. Ass. Adv. Sci.* (1911), pp. 1–10: quotation on p. 10.

76. "The New Magazine," *Am. Breeder's Mag.*, *1* (1910), 61–64; quotation on p. 62.

77. "Darwin, Mendel, and Cruikshank," *Am. Breeder's Mag.*, *1* (1910), 14.

took the form of a discipline and attempted to establish a secure position in American universities.[78] The period was characterized by the establishment of university chairs of genetics; by the founding of an academic journal, *Genetics*, distinct from the periodicals of practical breeding work; and by the emergence of a purely academic genetics society, quite separate from the American Genetics Association. The Genetics Society of America was founded in 1932, as an offshoot of the American Society of Naturalists. 338

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78. For a general account of the rise of American universities, see for example Edward Shils, "The Order of Learning in the United States from 1865 to 1920: The Ascendancy of the Universities," *Minerva*, *16* (1978), 159–195.

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CONCLUDING REMARKS 341

The differentiation of science, based on confining the study of heredity to a few specific problems and resulting in a definition of heredity restricted to the sexual transmission of hereditary factors from one generation to the next, was not simply the result of a functional division of labor. The formation of the discipline of genetics, with its own norms, methods, theories, and societies, was a strategy which some biologists supporting Mendelian theory employed to struggle for scientific authority in the field of heredity. With the support of practical breeders and cytologists, genetics in the United States, based on Mendelian analysis and supporting natural selection, worked its way to a dominant position in the academic biological curriculum.

The particular form which this discipline took, as well as the theories and dogmas that it maintained, resulted from the relation of genetics to other disciplines in the field of heredity. The genotype-phenotype distinction, which became the dogma of genetics, was raised as a polemic (albeit based on experimentation) in a particular social and intellectual milieu, against other competitive approaches to the problems of heredity and evolution. It made possible the exclusion of

88. L. J. Cole, secretary of the American Society of Naturalists, to members of the executive committee, October 25, 1929. For dialogue on the suitability of changing the "naturalists" into a "genetics society," see also G. H. Shull to H. J. Muller, October 2, 1929, and H. J. Muller to G. H. Shull, November 2, 1929. Herman J. Muller File, Manuscripts Department, Lilly Library, Indiana University.

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342 naturalists and biometricians from the study of heredity. And it played the tactical role in genetic studies of acknowledging developmental processes; in so doing, it provided the conditions for ignoring them when hereditary characters were being analyzed.

J. Sapp, 'The struggle for authority in the field of heredity, 1900–1932: new perspectives on the rise of genetics', *Journal of the History of Biology*, vol. 16, no. 3, 1983, pp. 311–18, 334–8, 341–2.

6.4

Douglas R. Weiner

THE ROOTS OF MICHURINISM

During the Stalin era, Soviet propagandists never failed to express their debt to Ivan Vladimirovich Michurin for laying the theoretical and methodological groundwork for the ambitious programme of state-directed acclimatization and hybridization that reached its delirious apotheosis in the phenomenon of Lysenkoism. For their part, Western analysts of the Lysenko episode properly recognize that Lysenko's designation of Michurin as his intellectual forbear was a *post hoc* attempt by Lysenko and his supporters to establish an intellectual pedigree for their programme.[1] Michurin's story, that of a modest gardener fighting for recognition against an 'arrogant' and indifferent biological establishment, was ideally suited to serve as a myth to legitimize Lysenko and *his* generation of nature-transformers in *their* efforts to dethrone the biological establishment.[2] 244

In restoring Michurin the man to his modest place as, perhaps, the Luther Burbank of Russia, however, we have yet to identify the scientific and intellectual roots of Michurinism,[3] understood as a set of biological doctrines that (1) hold man's legitimate role as conqueror of nature and master of evolution (2) are based on a mixture of the teachings of Jean-Baptiste Lamarck with those of Etienne and Isidore Geoffroy Saint-Hilaire, and (3) employ the techniques of acclimatization, domestication and hybridization in the pursuit of largely utilitarian objectives. As this study will show, Professor Joravsky's contention that 'in the first stage, the long period from the ... 1870s to the later 1920s, Michurinism was little more than a one-man movement',[1] can only win assent if we narrow our focus to Michurin the man. If Michurin's major contribution to Michurinism was the myth of his martyrdom at the hands of professional scientists and of his subsequent resurrection through the intervention of Party figures, then historians of science have to seek other sources for the transformist biology that characterized Stalin's Russia from the early 1930s. Some accounts of the Lysenko affair, by failing to provide any historical discussion of its background, seem to imply that Soviet biology's twentieth-century excursion into Lamarckian transformist biology was the result merely of the 'hare-brained scheming' of ambitious *praktiki*,[4] cranks and marginal

[1] See especially David Joravsky, 'The First Stage of Michurinism', in *Essays in Russian and Soviet History*, edited by John S. Curtiss (New York, 1962), pp. 120–32; and Dominique Lecourt, *Proletarian Science? The Case of Lysenko* (London, 1977).

[2] In his notorious presidental address of 31 July 1948 to the V. I. Lenin Academy of Agricultural Sciences, published in English as *The Science of Biology Today* (N.Y.: International Publishers, 1948), Trofim Denisovich Lysenko strongly emphasized Michurin's struggle against 'scholasticism' (p. 44) and averred, 'Without the Soviet system I. V. Michurin would have been, as he himself wrote, an obscure hermit of experimental horticulture in Tsarist Russia"' (p. 61).

[3] I. A. Poliakov, a Michurinist, defined Michurinism as a 'teaching [that] asserts the unity of the organism and its environment, the dependence of the hereditary traits of the organism on the latter's conditions of life, and the inheritance of characteristics, acquired by plants and animals in the process of their development under the influence of factors in their environment. The change in organisms' hereditary characteristics is always sufficient to adapt to the changed conditions of their environments'. Quoted from his 'Otechestvennye biologi v bor'be s mendelizmom', Akademiia nauk SSSR. Institut istorii estestvoznaniia. *Trudy*, 3 (1949), 3–27 (p. 3). Joravsky's economic description of Michurinism as 'a willful approach to agriculture' (see Joravsky (footnote 1), 121) has a certain appeal. The pamphlet by Lysenko (footnote 2, *passim*) is essentially one extended official definition of Michurinism.

[4] A *praktik* is someone who learned skills, craft, art, or science on the job without the benefit of formal, academic training.

245 academic types who devised their approaches for transforming nature *ex nihilo*. Others have laid the responsibility at the door of Friedrich Engels or A. A. Bogdanov-Malinovskii.[5]

However, a second look into developments in Russian biology and agronomy in the second half of the nineteenth century strongly suggests antecedents of Michurinism in Russian science itself; these Russian roots constitute a tradition extending from the 1840s through the 1930s of which Michurin himself may be considered a lesser product.[6] Indeed, by the 1860s, when Michurin was scarcely an adolescent, what would later be termed Michurinism had already emerged as a key current in Russian agronomy and the life sciences.

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5. Conclusions

We have suggested that the historical roots of Michurinism reach back to the acclimatization movement of the mid-nineteenth century. This movement, profoundly inspired by an antecedent French zoological tradition and by specific ideas concerning the role of science in society—also exemplified by French science—took hold among academic zoologists, agronomists and breeders, grouped around the Moscow Agricultural Society. Later, with the establishment of the Imperial Russian Society for the Acclimatization of Animals and Plants, this movement became institutionalized. While the Society did not fulfil its promise as the direct initiator of widespread attempts to introduce exotic life forms to Russia, particularly through the agency of the Moscow Zoo, it did disseminate the idea throughout the Russian scientific and agricultural communities generally. In so doing, it inspired local efforts for the acclimatization of everything from African antelopes to sub-tropical fruit trees. An important school in Russian zoology and botany, linked with the figures Rul'e and Bogdanov, also derives from the early days of the movement; this school maintained a belief in the environmental induction of hereditary adaptations, taken from the formulations of Etienne and Isidore Geoffroy Saint-Hilaire and Jean-Baptiste Lamarck. In addition to their role as proponents of acclimatization in the Soviet period, products of this school constituted a significant presence in the Soviet life sciences generally through the 1920s and 1930s, although they have been almost totally slighted by Soviet and other historians of science alike. Although direct, substantiating evidence has yet to emerge, it seems highly suggestive that the proponents of acclimatization of the Soviet era were inspired by the same heady mixture of inductionist theories of heredity, ideas about the role of science in society, and fantasies of becoming 'masters of evolution' that animated their mentors.[89] As I have argued elsewhere, T. D. Lysenko and I. I. Prezent's successful

[87] The reader is referred to Kozhevnikov's enthusiastic review of Ellii Anatol'evich Bogdanov's *Mendelizm ili teoriia skreshchivaniia* (Moscow, 1914). The review, which originally appeared in a 1914 issue of *Estestvoznanie i geografiia*, was excerpted in *Biulleten' Khar'kovskogo obshchestva liubitelei prirody*, no. 6 (1914), 88–89. The fact that the author of Russia's first textbook on Mendelism was the son of A. P. Bogdanov is a delicious irony historians of science may wish to savour—and psycho-historians may assist us to explain.

[88] On the acclimatization campaign, see Weiner (footnote 70), chapters 10 and 11, 483–591, and a forthcoming article.

[89] Although I am aware of the contention of L. V. Shaposhnikov, stated in his 'Anatolii Petrovich Bogdanov i akklimatizatsii zhivotnykh (k istorii voprosa ob akklimatizatsii zhivotnykh v Rossii)' *Biulleten' Moskovskogo obshchestva ispytatelei prirody, Otdel biologicheskii*, 52, pt 4 (1947), 95–103 (p. 101), that, 'With deep regret, we must state that the works of A. P. Bogdanov on acclimatization were later forgotten and did not play a rôle in the development of practical acclimatization ... in the period since the October Revolution', I find it difficult to accept this. Perhaps Shaposhnikov feared to take any of the credit for nature transformation away from Michurin and Lysenko and reassign it to some obscure nineteenth-century professors. This is certainly a problem that invites further study.

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260 bid for leadership of Soviet biology was initially based on exposing and annihilating the opposition by ecologists and Mendelian geneticists to the expanding programme of acclimatization, to which the state was heavily committed.[90] When it became clear during the course of this campaign that Mendelian genetics was not compatible with the new Stalinist goals of acclimatization and the miraculous transformation of nature, it would have been easy for Lysenko and Prezent to reach for the old Rul'e—Bogdanov legacy to fill the vacuum for a theory of inheritance. And that, it seems to me, is what they did. But then why credit Michurin, of all people? In creating his public image, Lysenko sought to portray himself as a man of the people crusading against a hostile biological establishment. If Rul'e and Bogdanov had been matched up against Michurin in Lysenko's mind, in a contest as to which *person* had the greater populist mythic potential, the two Moscow University *professors* would never have stood a chance.

The history of the acclimatization movement in Russia reads like a Marxian dialectic in reverse motion. In the beginning, acclimatization was a movement which, in its efforts to install greater rationality into Russian agriculture, represented a progressive synthesis. It sought to increase the economic potential of the state through the introduction of exotics while at the same time it attempted to prevent the further wanton destruction of Russia's natural resources through wise use and conservation. Affected by internal developments in biological theory and by social and institutional change, however, the synthesis fell apart. By the 1930s, the transformers of nature faced the conservationists as adversaries across a wide divide which was as much philosophical as scientific.

Originally a Westernizing harbinger of increased rationality, acclimatization became an example of an *irrational* approach to resource management, especially during the campaigns, of the Stalinist era, to introduce great numbers of exotics into the wild, where they caused considerable ecological disruption.

Acclimatization was also transformed into its opposite in another way. When the movement was in its youth, its endorsement of the biological doctrines of Lamarck and Geoffroy Saint-Hilaire was a progressive step. Indeed, in the absence of knowledge about chromosomes, genes, and mutations it was the best attempt scientifically to explain the growing evidence of the evolution of life forms on earth. It was a defence of Science against troglodytic Creationists. By the early 1900s, however, and certainly by the mid-1930s, the overly ardent embrace by academic biologists, agronomists, breeders and others of these outmoded theories flew in the face of new evidence and better hypotheses. As such, it constituted an antiscientific defence of pseudoscientific ideas as well as what seems to be part of the context for the triumph of Michurinism.

[90] Weiner (footnote 44), 690–96.

D. R. Weiner, 'The roots of "Michurinism": transformist biology and acclimatization as currents in the Russian life sciences', *Annals of Science*, vol. 42, 1985, pp. 243–5, 259–60.

6.5

Gary Wersky

SCIENCE AND IDEOLOGY IN THE SOVIET UNION

David Joravsky's *The Lysenko affair* reinforces rather than challenges our conventional wisdom about this episode. The value of his work is that it provides us with a wonderfully detailed exposition of the affair's history; reveals some of the hidden social functions that Lysenkoist science successfully fulfilled; and lays bare the intellectual framework that has informed Anglo-American reactions to these strange events. It sounds like a most exemplary piece of scholarship *unless* one rejects, as I do, nearly all of Joravsky's most basic assumptions about the social relations not only of modern science but of industrialized societies as well. The purpose of this review is to make clear what these assumptions are and why they are inadequate.

Joravsky sees the Lysenko affair as a conflict between 'rational' science and 'irrational' ideology. The triumph of Lysenko's 'agrobiology' over orthodox biology is regarded as 'irrational', because: (1) it was achieved by disregarding the norms and results of 'rational' scientific discourse; and (2) it actually made worse the agricultural crisis that it claimed to be able to resolve. Hence, Joravsky argues, Lysenkoism can only be understood in terms of the 'ideological' requirements of bureaucrats, party officials, and, above all, Stalin. All of them wished to consolidate and extend their control over the State. In the context of preserving their collective self-interests, Lysenko's campaign against scientific and agricultural specialists served them well. For it shifted blame for Russia's slow progress in boosting food production away from the bureaucracy and on to the shoulders of the scientific community. Lysenko's attacks also kept Soviet scientists sufficiently off balance to prevent their amalgamation into an independent community capable of seriously challenging the political establishment. Finally, Joravsky concedes that Lysenko and his followers may have provided 'some irrational enthusiasm' which 'the modernization of a backward country seems to require' (p. 305). Naturally, by the end of his narrative, Joravsky is delighted by Lysenko's ultimate defeat (p. 305): 'evidently there is something in plants, soil, farmers and scientists that requires rationality and genuine self-criticism of those who would manage them'. Thus today in the U.S.S.R. the boundaries between science and politics would appear to be not all that different from those that have been established in the West.

The heuristic advantages of Joravsky's dichotomy between the 'rationality' of science and the 'irrationality' of ideology are obvious. Without this distinction, as the author himself admits (p. 4), 'the cognitive enterprise becomes wretchedly inefficient, including that part of cognition that distinguishes between real and imaginary group interests'. With it, Joravsky is able to distinguish 'efficiently' between 'theoretical ideology and genuine knowledge' (p. 7). This also allows him to contrast the 'good guys'—those scientists who defended the methods and results of rational science—with the 'bad guys'—those who destroyed free scientific inquiry in the pursuit of their sordid, self-interested ideological goals. Readers of Joravsky's book are accordingly expected to perceive the outcome of the Lysenko saga as both a notable triumph of rationality over ideology and a vindication of the author's theoretical framework.

Before criticizing that framework, I want to bring out some of the many virtues of Professor Joravsky's scholarship. First, he establishes beyond reasonable doubt the methodological and practical inadequacies of Lysenko's theories. Second, he documents with consummate skill why Lysenko was backed (and eventually opposed) by various sections of the State apparatus. Third, Joravsky determines with great precision the extent not only of Lysenko's power at different periods—he enjoyed absolute dominance only between 1948 and 1952—but also of the 'repression' suffered by scientific specialists—probably no more than five per cent of all biologists. Given the difficulties surrounding research in this field, these are all notable achievements, for which Joravsky deserves to be congratulated. Unfortunately, his views on the social relations of Soviet science under Stalin prevent him from making the most effective use of his magnificent material.

The Lysenko affair is based on theoretical misconceptions concerning the nature of science and of socialism. Some of these difficulties emerge in Joravsky's very muddled discussion of the 'science/ideology' problematic. He recognizes (p. 3) that there is a continuum between 'the rationality of the scientific enterprise' and the 'irrationality of the political enterprise'. A bit later (p. 8) Joravsky rightly points out that 'political and technical issues . . . are deeply and increasingly interdependent', although he eventually goes back on this insight (p. 181) in condemning Stalin for his 'deliberate refusal to distinguish between political and technical authority'. Nevertheless, Joravsky maintains, there is a real difference between the belief systems of scientists and politicians. Scientists deal with 'rationally verifiable knowledge' and have no interest in clinging to 'dogma' once its existence has been exposed. Politicians, on the other hand, orient their ideologies around 'intuitive judgments of practical matters' and, above all, unacknowledged dogma or sacred beliefs that serve their interests and are above discussion. In other words, verified beliefs are the substance of (rational) science; unverified or unverifiable beliefs form the content of (irrational) ideology.

242 Joravsky's positivistic division between science and ideology will not do, for the following reasons. (1) Criteria of rationality are relative to social context. (2) Scientific beliefs can function as political ideologies, and *vice versa*. (3) Scientific theories cannot be separated from scientific practices. (4) The 'science/ideology' dichotomy automatically condemns any effort to create a 'socialist science'. Each of these points is discussed below in some detail.

(1) No belief is 'rational' in and of itself: it is only 'rational' with a set of norms, rules, and values, i.e. a social context. Hence, as Joravsky himself demonstrates, to the extent that Stalin's support for Lysenko was functional to or consistent with the achievement of certain political objectives, the Soviet dictator was acting 'rationally'. Using the same criteria, those Soviet geneticists who accepted State support but opposed Stalin could be said to have been acting 'irrationally'. If this sounds like a language game, then so, too, does Joravsky's labelling of one set of beliefs as 'rational' and another as 'irrational'. One must therefore either, like Max Weber, talk about two competing types of rationality, or drop these labels altogether. But neither of these courses is open to Joravsky, who has set himself the task of deciding 'from the vantage point of genuine knowledge' (p. 10) which side was 'right'.

(2) Joravsky's notion of 'verification' is put forward as a philosophical absolute unique to scientific practice. He neither defines the concept nor suggests that any part of modern science is founded on anything other than verifiable propositions (that have already been verified). Even more problematically, Joravsky offers no reason why beliefs verified by one group of scientists cannot cluster about ideologies or even serve as ideologies themselves. (The example of the Boyle lecturers' use of Newtonian philosophy in the late seventeenth century comes to mind.) But if scientific beliefs can function as (or as a part of) ideologies, then might the reverse be true as well? Before answering that question, it would be well to clarify what Joravsky means by 'ideology'.

(3) For Joravsky, ideology is 'unacknowledged dogma that serves social functions', and 'dogma' is defined as the opposite of 'rationally verified knowledge' (p. 3). Leaving aside the difficulties in Joravsky's use of 'rationality' and 'verification', his conception of ideology still raises considerable difficulties. Why must ideology always be 'unacknowledged'? Cannot a belief based on 'rationally verified knowledge' simultaneously take the form of a firm conviction which is rarely allowed to be challenged? In this connexion, Joravsky's comments (p. 233) about recent opposition to mechanistic biology are instructive: 'in the twentieth century those who are offended by the biologists' mechanistic picture of living things take their complaints elsewhere. There are a few who quarrel with the biologists, but they are generally ignored.' Are not the mechanists operating dogmatically?

Joravsky might reply that the mechanistic approach to biology cannot be regarded as a 'dogma' unless it can be shown to serve unacknowledged social functions. Yet, of course, it does. It is used to define who is inside and outside the pale of scientific respectability: the more such a dogma is successfully enforced the easier it becomes for professional (i.e. mechanistic) biologists to claim that they alone are competent to determine what research should be done and how that research should be evaluated, thereby insuring both their autonomy and their control over the job market. Thus the biologists' defence of mechanism can be seen to be as much rooted in their material as their intellectual interests, principally because their socio-economic position rests upon their claims to esoteric knowledge and expertise. What is frustrating about Joravsky's book is that he provides numerous examples of this interpenetration of, if you like,
243 intellectual and economic rationality, without realizing how such evidence undermines his dichotomy between 'scientific' and 'ideological' beliefs.

(4) Besides the theoretical difficulties inherent in Joravsky's separation of scientific beliefs both from ideological beliefs and scientists' social practices, there is the additional problem that most of the characters in his narrative, including even a few Marxists geneticists, behaved as if that separation were untenable. Whether scientists or non-scientists, Lysenkoites or non-Lysenkoites, they endorsed: the notion of a distinctively 'Marxist' as opposed to 'bourgeois' biology; the end of intellectual autonomy for scientists; and the principle that 'the prime duty of biologists is to serve the country's immediate practical needs' (p. 237). In short, they *said* that they wished to see the emergence within the U.S.S.R. of a scientific community that was, in their terms, genuinely socialist. But instead of taking his subjects' line of thought seriously as a guide to social and intellectual liberation, Joravsky treats it solely as a futile denial of scientific realities and a disastrous concession to bureaucratic domination and anti-intellectualism.

Why is Joravsky so 'external' and unsympathetic to those of his subjects who sought new social relations for science under socialism? A good part of the answer is that Joravsky's own (unstated) political outlook, coupled with the long, demanding years he has spent examining the minutiae of Stalinist atrocities, has led him to deny any possibility that socialism can be made to work. After reviewing the brutal measures that Stalin took in order to bring about industrialization, Joravsky observes (p. 307) that 'this is hardly Marx's notion of liberating revolutionary *praxis*, but let us put off that delicate dream, as the Bolshevik leaders did within their first decade of rule'. Having dismissed the goals of socialism as irrelevant to his study, Joravsky is then able to view Russia's post-revolutionary history as a case study in the 'modernization' of a backward country. Here he is unconsciously relying upon a sociological theory which asserts the existence of a 'logic of industrialism' which ultimately enforces upon all societies (whether 'capitalist' or 'socialist') a common way of life, including widespread adherence to the norms of 'technical rationality'. Such a framework is obviously congenial to Joravsky's general outlook and compatible with the outcome of the Lysenko affair itself.

But aspirations to socialism cannot be shuffled off so easily. Marx's 'dream' was 'delicate' in the Soviet environment of the late 1920s, when a backward economy and widespread opposition from the peasantry rendered the Bolshevik State peculiarly vulnerable to both internal and external threats. Stalin responded with programmes of forced industrialization and collectivization, both of which eventually demanded a combination of political repression, 'bourgeois' expertise, and a strongly hierarchical division of labour. During this period the Russian scientific community was reorganized and greatly expanded; scientists and commissars alike promised overnight breakthroughs in research designed to make agricultural and industrial production more efficient. In return, Soviet researchers received not only high praise but comparatively high salaries as well. Indeed, they soon became, like their Western counterparts, an intellectual élite that was set apart from manual workers. This was merely one of a series of developments that was transforming the Soviet Union into a 'State capitalist' society far removed from Marx's 'delicate dream'.

Yet throughout the 1930s Russia's political authorities, bureaucratic intellectuals, and a distinguished minority of natural scientists publicly agreed that their country's industrialization had to be achieved through the formation of *socialist* relations of production, including efforts to break down the divisions between mental and manual labour. Were these discussions nothing more than mystifying propaganda that served the self-interests of Stalin and his advisors? Joravsky seems to think so: I think there is more to this matter than he allows. For if one takes the Bolsheviks' point of view that socialism could only be achieved through an effective State apparatus, then, *in theory*, there need not have been any long-term contradiction between their socialist ideals and their bureaucratic self-interests. One must therefore ask whether any of their policies —their 'practice'—was designed to bring about not just the consolidation of their power but also the socialism in whose name a revolution had been fought and millions had already died.

Now let us return to the Lysenko affair. It was, as Joravsky has shown, a desperate and inept attempt to solve the agricultural crisis. Lysenko's onslaught on 'bourgeois' biology did make life easier for that part of officialdom that was responsible for food production. And many of those who climbed on to Lysenko's bandwagon in the Thirties and Forties were undoubtedly little more than ambitious, unscrupulous time-servers. But Lysenkoism was also portrayed inside Russia as an attempt to democratize scientific practice, put science into the hands of the people, and thereby lay the basis for new 'socialist' scientific theories. (This view of Lysenko's 'agrobiology' movement comes through clearly in Gennadi Fish's *A people's academy* [Moscow, 1950].)

Although Joravsky ultimately gives little weight to this dimension of his story, he does—to his credit—present ample material about it. Thus *The Lysenko affair* (p. 126) documents the extent to which the battle between Lysenkoites and non-Lysenkoites was waged along class lines. More importantly, Joravsky's evidence can be interpreted as confirming the contention of Lysenko and his followers that the bulk of 'bourgeois' specialists were hostile to the Bolshevik State and devoted themselves to experimental work that was either divorced from or inapplicable to Soviet agriculture.

This 'class grip' on biology was also sustaining, until the mid-1930s, anti-socialist research programmes in eugenics, which assumed the genes of ('bourgeois') scientists to be superior to those of the proletariat. Joravsky (pp. 259-63) dismisses such eugenical tendencies as pure ideology, thereby missing the point that such research was supported by the Soviet biological establishment: in other words, a reductionist form of human genetics in which 'science' and 'ideology' were completely interfused, was for a time validated as legitimate 'scientific' work. In these circumstances one must therefore either say that 'ideology' was operating as 'science', or, more satisfactorily, that here eugenics was simultaneously 'science' and 'ideology'. In any event, this was an indication of the degree to which many 'bourgeois' biologists were alienated, as Lysenko's movement apparently was not, from the notion that labouring men and women had the ability to take control of their own destinies.

Unfortunately, Lysenkoism never became the basis for reinstating Marx's vision of liberating socialist praxis. In the short run, Lysenko and his followers simply took over from the bourgeois specialists the 'traditional hierarchy of [scientific] status': they did not, Joravsky emphasizes, 'destroy the hierarchy itself' (p. 189). Later, in the face of massive setbacks in agriculture brought about by the inadequacies of Lysenko's theories, the facilities of 'hut labs' and the training of a new generation of 'barefoot professors', the last vestiges of the democratization process initiated by the Lysenkoites within science were destroyed.

Today in the Soviet Union the selection, training, and experimental
245 practices of scientists are nearly as élitist as they are in Great Britain or the United States. For Joravsky this represents a small triumph for 'rationality' and 'scientific realism', while I see it as the unhappy outcome of a clash between three equally 'rational' and widely supported trends that proved to be irreconcilable in Stalinist Russia: a theoretical-practical commitment to create socialist relations of production; the political necessity to 'modernize' rapidly and at all costs an economically backward country; and the valid, if self-interested desire on the part of the Soviet intelligentsia to insure the continuance of a relatively autonomous, tolerant, and self-critical intellectual life. Of these, 'modernization' was clearly the victor, socialism the loser.

What of Soviet science? Joravsky implies that it is now rather similar to scientific life in the West. Yet this convergence must be seen as a two-way process. If Russian scientists have gained some autonomy, it is equally true that their Western counterparts have lost much of their independence. Today our research and development effort is overseen by the State, and, as in the U.S.S.R., much of it is directed to military uses. Utilitarian criteria of 'practice', notably profitability, are applied to the work of British and American scientists, many of whom have recently found themselves unemployed. Like Soviet geneticists in an earlier period, these scientific workers are now unable to work in their fields, because State and industry have decreed their skills to be useless. All such unemployment is the result of political-ideological decisions, which emanate from the State in Soviet Russia and from both the State and the market in our own society.

To some readers this comparison might appear far fetched indeed. It would seem to ignore, for example, the Soviet State's recent emulation of our own politicians' historical reluctance to adjudicate scientific disputes. (How absurd it would be to open *The Times* tomorrow morning and read: 'Wilson Backs Koestler; Crick Loses Post'!) But the absence of widespread discussions in East and West about the ideological 'correctness' of different scientific theories may connote something other than a 'rational' recognition that scientists are the best judges of the adequacy of a scientific theory. It could imply that the reigning theories in physics, chemistry, and, above all, biology—far from threatening existing ideologies which rationalize the power of ruling classes-élites—can be easily used to justify the existence of all such hierarchies. At a more abstract level, one can follow Marcuse in noting that the outlook and practices of 'technoscience' have themselves become the dominant ideology of Russia's rulers and ours.

The point of this extended comparison has been to suggest that most of the issues Professor Joravsky raises in *The Lysenko affair* are not unique to the Soviet Union; rather they are common to all advanced forms of 'mixed' capitalism.

Where the Soviet case diverges is precisely when Lysenko and others began to talk and even act as if they were out to achieve socialism. But because Joravsky finds the notion of a socialist society so Utopian, he has to treat it solely as pure 'ideology', i.e. an 'irrational' commitment supportive of narrow self-interest. (Significantly, he does not comment upon recent efforts in China to break down divisions between mental and manual labour.) This leads him to erect as the foundation for his distinguished research a confused, shallow, and misleading dichotomy between 'science' and 'ideology'. Nevertheless, his book retains its inestimable value as one of the few monographs in the history of science that fully documents how intricate and significant are the interrelationships between scientific beliefs and political practices.

GARY WERSKEY

G. Wersky, 'Essay reviews: science and ideology in the Soviet Union. Review of *The Lysenko Affair*, by David Joravsky', *British Journal of the History of Science*, no. 8, 1975, pp. 240–5.

Richard Lewontin & Richard Levins

6.6

THE IDEOLOGICAL IMPLICATIONS OF GENETICS

It is essential to distinguish between what we might call the 'minimal theoretical structure' of a science, which is dependent upon unspoken ideological assumptions, and a kind of ideological superstructure that is built upon but is not logically entailed by the minimal structure. For Mendelian genetics, the minimal structure includes the laws of Mendel and the Weismannian principle that the material substance whose behaviour is formally described by the Mendelian laws cannot be altered

in a directed and adapted way by information from the environment, but that the phenotype of an organism is the outcome of the biosynthetic activities of genes taking place in a particular sequence of external and internal conditions. The ideological superstructure that has been laid on this theory by various geneticists includes notions of the 'limits' set to the phenotype by the genetic 'potential', the notion that what is inherited is somehow fixed and unchangeable, that organisms are 'determined' by their genes. By acting as if this ideological superstructure were in fact, the substance of genetics, geneticists invite a misplaced quarrel with the minimal structure itself. In 1931, Zavadovsky[18] foresaw the inevitable attack on Mendelian genetics that was being invited by the biological and genetic determinism and the pernicious eugenic elitism being read into their science by geneticists. He warned against the extreme environmentalist counter-reaction that would attempt to destroy all of genetics in order to assert the plasticity and perfectability of human society. He was the first, as far as we know, to point out that Lamarckism was anti-progressive since it would imply that centuries of degradation and brutalisation of workers and peasants had made them genetically inferior as well.

Among the issues of superstructure were several prominent ones. In the mid-1920s, most Soviet and Western geneticists propagated an elitist and racist eugenic ideology,* discussing for instance the breeding of superior types from the ranks of the intelligentsia as well as from those members of the lower classes who had been in the vanguard of the revolution.[19] Eugenicists also claimed that the genetically 'best' elements in the population were being outbred by the 'worst' and that this trend might grow worse with population control. This kind of naive genetic determinism of human behaviour naturally invited ideological attack.

The treatment of the gene merely as a cipher, a book-keeping device, uncoupled genetics from physiology. Thus Bateson explained the Mendelian view to the New York Horticultural Congress in 1902 roughly as follows: 'the organism is a collection of traits. We can pull out yellowness and plug in greenness, pull out tallness and plug in dwarfness.' This uncoupling, so attractive to geneticists and to Anglo-American analytical reductionism, was offensive to Lysenko's group who saw heredity as a special (but not too special) case of physiology.

*Rose and Hanmer suggest that the proposals were also sexist. See 'Women's Liberation, Reproduction and the Technological Fix' in the first volume, *The Political Economy of Science*.

49 Mendelian genetics, which made the possibilities of artificial selection depend on the fortunate occurrence of useful genes, a small minority of the mutants, imposed limits to the progress of plant breeding which were socially unacceptable given the needs of Soviet agriculture. On the other hand, a model in which the creation of hereditary variation proceeds apace with its selection promised indefinite progress once physiological knowledge was sufficiently sophisticated.

The traits used by Mendelian genetics to develop and argue its theory are clear-cut mutants of *Drosophila* (fruit fly) and a few other organisms. These mutants are a special kind of variant. They are usually non-viable in nature. They were chosen for their unvarying expression so that they could be followed easily while the complicated processes of variable expression so common to adaptive, quantitative and agronomically important traits were ignored. Finally, many of the mutants and chromosomal abnormalities were artificially induced by radiation at dosages so far beyond those that occur in nature as to make it appear that Mendelian genetics dealt with a special class of laboratory phenomena but could not, in principle, deal with the problems such as adapting fruit trees to the far north.

R. Lewontin & R. Levins, 'The problems of Lysenkoism', in H. Rose & S. Rose (eds), *The Radicalisation of Science: Ideology of/in the Natural Sciences*, Macmillan, London, 1976, pp. 47–9.

Garrett Hardin 6.7

THE IDEOLOGICAL DISEASE OF DENYING COMPETITION

Through the anti-genetic literature of the Communists, as 251
through much of Soviet literature, there runs a thread that is only dimly visible but of prime importance to Communist ideology: fear and hatred of competition. Occasionally it is explicit, as when Lysenko said, "after a deep and comprehensive investigation I have come to the conclusion that there exists no intra-specific struggle but [only] mutual assistance among individuals within a species." The belief that it is possible to escape competition has its roots in orthodox utopian literature. However much the various utopias may differ, almost without exception they sketch a world in which competition is at an end: food is available for the taking, clothes are disbursed from a common wardrobe, no one competes in buying or selling—or in politics, for either all rule all or (probably more significantly) there is an all-powerful, perpetual dictator. A yearning for an end to competition has been a most significant part of the personality of the utopia-makers. The matter deserves more psychological attention than it has yet received.

Can competition be eliminated? A biologist has something to say about this. Competition can be put into a broader framework, that of the struggle for existence, using this phrase, as Darwin did,
"in a large and metaphorical sense," to include both the literal 252
struggles of individuals one with another, and the figurative struggles of individuals to extract a living from the physical environment. *It is certain that the struggle for existence cannot be escaped.* In times of exceptional prosperity, there may be an apparent suspension of the struggle for a while, but this is only a temporary matter. Organisms, if they are to exist continuously in a world of variable risk, must necessarily "tend" to increase geometrically, as Malthus put it. (In mathematical language, the reproductive potential is an exponential function with an exponent necessarily greater than unity.) But the environment is finite. From this, the struggle for existence necessarily follows.

The struggle for existence may take various forms. Under very rigorous conditions, the struggle may be almost entirely against the elements. The twisted cedar near timber line competes perhaps not at all with other living things: its only problem is to withstand the buffeting of the elements, maintaining a tenuous roothold in the rocks and spreading out its photosynthetic surface to catch as much of the sun's energy as it can without being blown from its moorings. Its struggle for existence is with the non-organic world. At the other extreme are the inhabitants of a tropical rain forest, where (for example) 99 seedlings out of 100 may be crowded out of existence by competition among themselves.

It is often useful to subdivide the concept of competition into two sorts: interspecific and intraspecific. Interspecific competition is competition between different species and includes such activities as predation, parasitism and competing for the same food; it is competition in a large, loose sense. Intraspecific competition is competition in a narrower, stricter sense: it is the struggle of individuals of the same species for the same sort of food or *Lebensraum.*

It is a basic axiom of biology that *the struggle for existence cannot be escaped:* it can only be altered in the form it takes. Under severe climatic conditions Arctic lemmings may be "struggling" largely with the severe weather for their existence; under somewhat better conditions, their numbers may be kept in check by
253 Arctic foxes and snowy owls—an example of interspecific competition. Under still better conditions, it is the rivalry of lemming with lemming for food that is decisive: intraspecific competition. The example illustrates the general rule that as a species becomes increasingly "successful," its struggle for existence ceases to be one of struggle with the physical environment or with other species and comes to be almost exclusively competition with its own kind. *We call that species most successful that has made its own kind its worst enemy.* Man enjoys this kind of success. Intraspecific competition may be as crude as cannibalism or infanticide, as "romantic" as chivalrous jousting or dueling, or as subtle as Stephen Potter's "one-upmanship," but it all has the same end in view: the securing of advantage to one's self at the expense of one's neighbor in a world that is not, and cannot be, large enough for the continuously "successful" species. No activity of man is without competitive uses. Even tact is a competitive weapon. From a humane point of view, we may prefer one weapon to another, but let us not deceive ourselves as to their ultimate effect. We are at one with the rest of the living world: either we must struggle with other living species, or we must compete with ourselves. Having subdued the rest of the living world—even, for the most part, the disease organisms that lay low so many proud kings of the forest—we have no choice but to compete with ourselves. This is the spectre that haunts Communism. This is the spectre that haunts most of what has been called "liberalism" in the past century and a half. This is the demon that cannot be exorcised by verbal incantations of "brotherly love," "co-operation," "togetherness," or—that marvelous invention of Marxian apologists—"socialistic competition." (The phrase is never defined, of course, but is shrilly insisted to be quite unlike the evil competition of the Darwinian or capitalistic worlds.)

The biologist who looks at social affairs cannot but reach the conviction that here, too, the principle of the inevitability of competition is observed. Consider, for instance, one of the most conspicuous of men's attempts to do away with competition, the labor-union movement. Few can read the tribulations of laborers in the early nineteenth century without being moved by pity for these
254 wretched pawns of insensitive industrialists. Labor was but a commodity to be dealt with in a strictly economic fashion, "economic" being interpreted in the narrowest way. It is doubtful if the most intelligent "planner" could have devised a kinder solution to the wretched problem than the labor-union movement that actually developed, pockmarked though its history was with lawlessness,

violence and bloodshed. The aim of the movement was to put an end to the competition of laborer with laborer in the labor market, to see to it that a mere surplus of labor did not drive wages down to a subsistence level through the unregulated operation of the Ricardian scheme. The labor-union movement was ultimately successful: but did it put an end to competition? Not at all! It merely changed the locus of the competition, and the pay-off. Now laborers compete with all non-organized groups—office workers, farmers, scientists, clergymen and artists—for shares in the general wealth, to the disadvantage of the non-unionized groups. And if all men were unionized, would this put an end to competition? Certainly not: the competition would take some other form, perhaps for offices of power and wealth within the union to which everyone belonged. It might be polite and lawful competition, or it might be vicious and corrupt, but competition it would be.

No activity of man is without its competitive aspect. Even the activities involved in what Matthew Arnold has taught us to call "culture"—painting, sculpture, music and writing—have their competitive uses. Each youth as he matures casts about for an occupation, trying—overtly, or at a hidden mental level—one after another until he finds his vocation. How does he recognize a "calling"? In part, perhaps, by esthetic means, or by logical analysis; but probably only in small part. To a far larger extent than most young men probably realize, one's occupation is chosen on the basis of a subconscious estimate of one's competitive position in the field. After choosing an occupation, one rationalizes its importance to the whole body politic so as to magnify one's self in the larger competitive scheme. Perhaps it is psychologically essential that he who is choosing a calling not know his own motives. The greatest awards are reserved for those who feel a force larger than themselves, outside themselves. If they knew to begin with that the "call" came really from within, they might never paint pictures, build bridges, study protozoa or write books. This is the surmise that psychiatry has planted in the mind of "rational" man. (There may be some kinds of truth that cannot be faced at first without a disintegration of the self. If this is so, "truth" then takes on new meanings undreamt of in "objective" science. It is for the future to unravel the implications of the psychiatric surmise.)

"The pen is mightier than the sword"—this statement was surely made by a man who was clumsy with the sword but clever with the pen, a man who wished to gain with the cultural weapon the competitive advantage that he could not possibly achieve with the more primitive one. He wished to change the rules of the game—in his favor. In a restricted but real sense even "culture" is only an extension of the means of competition. (Though saying so is not to damn it.)

We can never eliminate competition. We can only change the rules of the game, and the pay-off. Competition is to be found in the subdued and pious Quaker meeting just as surely as it is on the bloodiest of battlefields. The devices of love may be found in its arsenal side by side with weapons of steel. Our problem is not to avoid the unavoidable—competition—but to choose our weapons. In seeking the means that are most commensurate with human comfort, pleasure and dignity we cannot necessarily trust first impressions or traditional moral standards. We will need the deepest insights of psychology and anthropology to enable us to choose well.

Ideas: Infection and Recovery

There is yet one more aspect of the Russian experiment that a biologist would like to point out. We can regard erroneous ideas as infections with which a people may be seized and from which they may recover. There are certain principles connected with bacterial infections that seem to have a parallel in the ideological field.

256 Ideally, a disease infection leads to a critical illness, during which the defense mechanisms of the body are mustered; a phase during which these mechanisms gain the ascendency over the invaders (if we're lucky); followed by a nearly complete immunity to the disease. This is the ideal situation. Sometimes, in some people, the course of events is different. The disease organism may never cause a sharply defined, critical disease state, and the host may never muster vigorous defence measures. Instead, a condition of uneasy and unstable truce may exist between parasite and host for months or years, to the great discomfort and harm of the host. There is reason to think that rheumatic fever is this sort of disease condition.

The ideological disease of denying competition has, in its most virulent form, produced Communism with its dream of the classless society. In its most virulent form, it has produced the fever of Soviet Russia.

G. Hardin, *Nature and Man's Fate*, Rinehart & Company, New York, 1959, pp. 251-6.

6.8

Bob Young

AGRICULTURAL STAKHANOVITES

Inside the Soviet Union a great transformation was going on. The voluntar- 90
ism of the dedicated, hard-working Stakhanovites in coal mining and industrial production invited parallel achievements in altering nature.[8] The Lysenkoists were known as 'agricultural Stakhanovites' and constituted a very special and privileged social stratum—cadres of agricultural production in state farms and breeding stations. Lysenko*ism* became 'the systematic form of the ideology of this social stratum' (Lecourt, p.76). Michurin and Lysenko saw plant experimentation as conscious transformation of nature. Lysenko recited to the Soviet public these words of the man whose name was given to the agro-biological movement—Michurinism: 'It is possible, with man's intervention, *to force* any form of animal or plant *to change more quickly and in a direction desirable to man*. There opens before man a broad field of activity of the greatest value to him.' By the time of Lysenko's ascendancy in 1948, the slogan 'the transformation of nature' became the basis of a whole programme (Graham, 1971, pp.234, 235, 237).[9]

Just how far this went is clear from a remarkable contemporary document which is by turns ludicrous and chilling. *Land in Bloom* by V.Safonov is a popular history of agrobiology from Linnaeus to Lysenko which won the Stalin Prize in 1949 (a copy turned up in a Communist Party book sale in 1977 – I've never seen it mentioned in the literature).[10] It is said that an inspiring slogan of the Chinese revolution (also of 1949) was 'Throw off Nature's insolent yoke!' Remarks on the new Soviet agrobiology strike the same note:

> It was as if the fetters that had bound the ancient science of life had been broken. The profound and exact understanding of living phenomena took the place of lifeless dogmas, reservations and biased interpretations. And so irrefutable was the effective power of this understanding that all the best representatives of biology, irrespective of the opinions they had formerly held, became convinced that the old views could be adhered to no longer.
>
> This is what happened under our eyes. We are proudly conscious of the fact that it could have happened in no other country but ours.
>
> A revolution in the world-wide development of biology has been brought about. We are witnesses of it.
>
> . . . The storm will subside and the new knowledge will then stand out, cast in beauti-
> ful and perfect mold. It will appear majestically calm; and the patina of time will also 91
> cover the struggle and victory of those fearless innovators, the scientists of our day.
>
> We, the Soviet people of the Stalin epoch, have seen how, in one of the biggest ideo-logical battles fought in the history of natural science, a science of unprecedented might was born, and how this might endows man with fabulous power over nature. And all the obstacles that only yesterday had been proclaimed fatally insurmountable, fall before it.
>
> It is the science of life which teaches man how to transform the surrounding world and to re-create living nature. It is Soviet, Michurin agrobiology. Its features are unexampled. It is the science of the people (Safonov, p.13).

The combination of the rhetoric of class struggle, sycophancy and xeno-phobia is a common theme in the representation of the crucial 1948 Session of the Lenin Academy of Agricultural Sciences at which Lysenko achieved the peak of his power:

> "Partisanship in philosophy is the chief orienting factor....Only on the basis of the teachings of Marx, Engels, Lenin and Stalin can science be fully reconstructed . . . . Man is a part of nature, but he must not merely outwardly contemplate this nature . . . . The philosophy of dialectical materialism is an instrument for changing this objective world; it teaches how to influence this nature and to change it; but the proletariat alone is capable of consistently and actively influencing and changing nature—this is what the teachings of Marx, Engels, Lenin and Stalin—those unexcelled titanic minds tell us."

Every Michurinist applies these splendid words directly to himself and to his work, to the struggle he is waging against the advocates of pseudoscientific formal genetics, the offspring of slavish subservience to Western ideological biology, of cosmopolitan worshipping of the idol "world science"(Safonov, pp.529-530).

It is difficult to imagine that there is an honest Soviet scientist today who does not realize the objective significance and ultimate goal of the reactionary, thoroughly idealistic theory of formal genetics that had been imported into our country by the servile worshippers of things foreign (p.539).

The final passage conveys the ghastly apotheosis of the unification of undemocratic centralism and a new, covert elitism with the desire to place science in the service of the people in transforming living nature:

The President of the Academy said: "Progressive biological science owes it to the geniuses of mankind, *Lenin and Stalin* that *the teaching of I.V.Michurin has been added to the treasure house of our knowledge, has become part of the gold fund of our science.*"

He concluded his speech with a tribute to the Michurin science, the science of the transformation of living nature for the benefit of the Soviet people, with a tribute to the Party of Lenin and Stalin which revealed Michurin to the world and created in our country all the conditions for the efflorescence of advanced, materialist science.

And when he uttered his final words: "Glory to the great friend and protagonist of science, our leader and teacher, Comrade Stalin!" the thousand or so people who filled that vast hall rose like one man and stood for a long time clapping, the sound of applause, now rising and now subsiding, only to break out with renewed vigour.

In that same month, August 1948, the Presidium of the Academy of Sciences of the USSR, at a three-day enlarged session, discussed the results of the session of the Lenin Academy of Agricultural Sciences of the USSR: and the supreme scientific body in our country arrived at the conclusion that the development of Michurin science must become the pivot of all the natural sciences.

As a result, the work of our universities, of the vast network of scientific institutes, research laboratories and plant-breeding stations that stretches over our country, was quickly reorganized. An unprecedented wave of enthusiasm swept through the ranks of our agrobiologists, soil scientists, agronomists, zoo technicians, academicians and advanced kolkhozniks [collective farm-workers]. The great festival that had been spoken about at the session appeared to have arrived . . . (pp.541-542).

Not a small group of scientists, but entire country was promoting Michurin science, the science of man's power over the land and of the transformation of the land for the benefit of the people.

It was a revolution in science (p.542).

B. Young, 'Getting started on Lysenkoism', *Radical Science Journal*, no. 6/7, pp. 90-2.

Richard Lewontin & Richard Levins

6.9

THE CONDITIONS CREATING LYSENKOISM

The books of Medvedev and Joravsky show clearly the way in which dogmatism, authoritarianism and abuse of state power can help to propagate and sustain an erroneous doctrine and even establish its primacy for a time. But a theory of 'bossism' is not sufficient to explain the rise of a movement with wide support nor to explain its form and context.

There were a number of streams that converged to give rise to and sustain the Lysenkoist movement. These were: (1) the material conditions of agricultural production in the Soviet Union; (2) the problems of agricultural experimentation under these material conditions; (3) the actual state of genetical theory and practice in the 1930s; (4) the ideological and social implications drawn from Mendelism, including the eugenics movement; (5) the response of the peasants to the collectivisation programme beginning in 1929; (6) the class origins of agronomists and academic scientists in the decades after the revolution and the strong cultural revolutionary movement toward popularisation of scientific understanding and activity; and (7) the growing xenophobia of the 1930s.

(1) *Conditions of Agriculture* 40

There can be no understanding of Lysenkoism that does not begin with the hard facts of climate and soil in the Soviet Union. Since it is usual both within and without the Soviet Union to compare Soviet and American agricultural production, the same comparison of geography is illuminating.

Nearly all of the Soviet Union lies above the latitude of St Paul, Minnesota (40° N) so that its general temperature regime is more like that of Western Canada than the United States. The growing season in the most productive, *chernozem*, belt is short, and contrast between summer and winter temperature is extreme, as compared with Western Europe and the United States. Table 2.1 makes the point clearly, showing the dramatic increase in 'continentality' of the climate moving from West to East in Europe and Asia along the 50° parallel of latitude. While

TABLE 2.1

	Number of frost free days	*°C difference between warmest and coldest month*
Utrecht	196	16·4
Berlin	193	19·3
Kiev	172	25·3
Kharkov	161	28·3
Saratov	151	30·6
Orenberg	147	37·4
Akmolinsk	129	37·3
Irkutsk	95	38·1
Pierre, South Dakota	161	32·6
Hutchinson, Kansas	182	27·8
Ames, Iowa	159	30·4

the Soviet Union has one-third more population than the United States, acreage harvestable per year is the same, about 360 million acres. The rich black *chernozem* soils of the Soviet Union, equivalent to the Great Plains and prairies of the United States and Canada, are in a narrow east-west belt from the Ukraine in the west passing just north of the Black Sea to Akmolinsk in the east, running roughly along the 50th parallel. South of this *chernozem* belt, rainfall is ten inches or less per
41 year and so is much too arid for normal agriculture. North of the *chernozem* belt, rainfall is sixteen to twenty-eight inches per year, quite adequate for agriculture, but the soil is poor, and the growing season short, and the winter frosts very severe so that neither winter nor spring wheat is favoured. The general problem in this region is to plant late enough to avoid killing frosts, yet early enough to get a full growing season. The *chernozem* belt itself, which is the chief agricultural region of the Soviet Union, lies in a band of marginal rainfall of ten to twenty inches per year with frequent droughts that result in catastrophic crop failures. In contrast, the United States' black soil belt runs north-south in the Great Plains, spans a broad range of temperature regimes, mostly milder than in the Soviet Union, and receives fifteen to twenty-five inches of rain per year, reaching thirty inches in the easternmost sections. In addition, the United States has a large central section, just east of the plains, with thirty to forty inches of rain, three to ten feet soils, a long and mild growing season with summer nights not falling below 55° F, that is ideal for maize. This 'corn belt', which is the basis for meat production, is completely absent in the Soviet Union.

Lysenko's rejection of hybrid corn and his insistence on the use of locally adapted varieties is usually offered as a prime example of the counter-productive effects of his unscientific theories, while Khruschev is praised for his adoption of American hybrid corn breeding. Yet hybrid corn has not been a success in the Soviet Union precisely because of the lack of a 'corn belt'. In the United States, outside of the corn belt, in more marginal areas for maize, locally adapted varieties commonly out-perform hybrids.

The generally poor conditions for food crops is matched in other cases. Cotton, which is chiefly produced in the moist warm regions of the south-east of the United States by dry farming, must be irrigated, at considerable expense, in the Soviet Union, since, there, warm temperatures are accompanied by semi-aridity.

The most striking example of the deleterious effect of environment on a staple crop is for sugar-beet, the standard sugar source in Europe. In Germany and France, with high summer rainfall, yields in the mid-1930s were about thirteen tons per acre, of which 34 per cent was sugar content. In the Soviet Union, with dry hot summers, yields were four tons per acre with a sugar content of only 27 per cent.

Another problem for Soviet agriculture is that much of the arable land cannot be cropped annually, nor can it be planted with high yielding varieties that remove moisture and nutrients from the soil at a high
42 rate. For example, 45 million acres in Kazakhstan can be cropped only every second or third year. Soviet agriculture must then be more extensive and less intensive than American both in space and time, although both are non-intensive in comparison with most European practice, but for different reasons. Soviet agriculture is extensive because of the generally severe conditions of climate and soil, while the Americans have sufficient favourable climate and land to make intensive agriculture unnecessary and unprofitable.

Thus, the figures for important food crops shown in Table 2.2, taken from the 1930-5 data, shows the intensive agriculture of Western Europe in sharp contrast to the American and Soviet practice. The yields for the Soviet Union are over-estimates by perhaps as much as 20 per cent since they were normally estimated in the field rather than actually measured after harvest.

TABLE 2.2

Yields in bushels per acre 1930 – 5[10]

Crop	*Germany*	*France*	*United States*	*Soviet Union*
Wheat	29·7	23·0	13·5	10·8
Rye	27·4	18·3	10·7	13·5
Barley	35·9	26·6	20·1	16·0
Corn	–	–	22·1	16·3
Potatoes	226	164	108	120

In general, Soviet agriculture is carried out in conditions that are not only marginal *on the average*, but of much greater *temporal uncertainty and variation*. Periodic catastrophes from drought or severe winter frosts are a regular feature. Two successive years of drought in 1920 and 1921, coming hard on the heels of the Civil War, caused a catastrophic famine in which more than a million perished. Again, in 1924 there was a very severe year which reduced grain supplies by 20 per cent. This variability and unreliability of temperature and rainfall and the imminent possibility of agricultural catastrophe must be regarded as the leading element driving Soviet farm policy. It is no accident that the first wholesale trials of vernalisation[11] were carried out after the two severe winters of 1927 – 8 and 1928 – 9 in which 32 million acres of winter wheat were lost in the extraordinary cold.

(2) *Problems of Experiment and Evaluation* 43

The normal American method of variety testing is to plant a number of varieties in several years at several locations and choose those varieties with the highest average yield over locations and years, with some attention paid to variation between years and locations when average yields are very close. The underlying model is of normal variation around a mean, the coefficient of variation being fairly low so that any sequence of a few years averaged over a few localities will not deviate greatly from any other sequence. This is, in fact, the model that underlies all normal statistical analysis of experimental science. Events are assumed to be regular and drawn from a 'homogeneous' distribution. But the experience of Soviet agriculture has been quite different. There is generally a sequence of 'normal' years punctuated at uncertain intervals by one or more severe crashes. While years and locations could be averaged, the value of such averages as predictors is poor, because the coefficients of variation are so high. An analogy from ordinary experimental science makes the distinction clearer. When a new experimental technique is worked out, there will be a period during which the experimenter has such poor conditions that there will be some replications of the experiment that are clearly deviant and not regarded as part of the normal experimental variation. Not until the experimenter has the system under sufficient control to avoid these deviant cases will data begin to be accumulated to test some hypothesis. The decision that the system has passed from the initial uncontrolled stage of heterogeneous results to the stage of controlled variation is made impressionistically and represents a change in the underlying model of the universe with which he is dealing. In the first stage, averages over all experiments are not appropriate, and, forced to characterise the results, the experimenters would perform some culling, averaging only the 'normal' replications which represent the 'potential' of the experiment.

This is precisely the procedure followed by the Lysenkoists, and by Soviet agriculture authorities even before the Lysenkoist movement, when they reported yields per acre. Obviously, such a culling procedure can be and has been used for self-serving purposes since there is no objective way to decide which cases are 'deviant' and which are 'normal'.

That this 'pathological' model played into the hands of unscrupulous manipulation, or was unconsciously used by wishful thinkers, cannot be doubted. But the conventional statistician's scornful demand that *all* the data be averaged in an 'objective' way will not serve either. The immense variation in results make the averages meaningless as predictors.

44 Lysenkoist recommendations had such wide appeal precisely because they were intended to cope with extreme environments. Vernalisation, for example, was designed to avoid winter killing of wheat, while 'sowing in the mud'[12] was designed to give plants a very early start against the summer droughts. It is revealing that the report on vernalisation of a 1931 drought conference carried a 'warning against drawing hasty negative conclusions from possible individual failures', because 'particular failures are possible, indeed unavoidable . . . as in every experimental search for new pathways.[13] Apparently it was the hope of the conferees that these 'experimental pathways' would soon come under sufficient control to avoid the 'particular failures'.

Normal procedures of variety testing and normal statistical evaluation in which equal weight was given to all observations could not have been applied successfully in the conditions of Soviet agriculture of the 1930s because the level of agricultural technology and husbandry was insufficient to buffer against the extremes of climatic variation. It is not certain that even today conventional plant breeding and evaluation techniques could be successful. What is required is some objective method of dealing with the uncertainties. Perhaps the concepts of *maximum* and *maximax* solutions to the 'game against a capricious nature' could be used, although the irony would be great since games theory is a unique development of bourgeois economics.

(3) *The State of Genetical Theory*

The Lamarckian theory – that characteristics acquired by the organism as a response to the environment during its lifetime may be transmitted to its offspring – had never really been refuted so much as it had been abandoned with the development of modern genetics. The textbook refutation of Lamarck was the work of Weismann.[14] In his classical experiment with mice, the removal of the tail over successive generations failed to produce mice with shorter tails. However, this was in fact irrelevant to the Lamarckian hypothesis, which never claimed that mutilations are heritable. Rather, the claim was that active adaptive responses are transmitted to the offspring; and in support of this there was an impressive body of experimental data.

Among the classical Lamarckian experiments were those of Guyer, who found eye defects in the offspring of rabbits injected with corneal antibodies; the work of Jollos on the transmission of heat resistance and other traits induced by heat treatment in *Drosophila*, Cunningham's
45 arguments on the evolution of the beehive; and MacDougall's behavioural experiments. In plants, Lucien Daniel studied graft hybridisation and Lesage adapted cress to particular conditions and claimed the transmission of the adaptation over six or more generations. Bolley, working with flax in North Dakota, claimed to induce disease resistance which is transmitted. About 1939, Eyster described experiments in which corn was grown in different parts of the United States. The kernels showed different colour patterns, and 'under California conditions more of the color changes extended into the germplasm and this became genetic'. As late as 1944, Reynolds published in the *Proceedings of the Royal Society* a paper entitled 'On the Inheritance of Food Effects in a Flour Beetle, *Tribolium Destructor*' in which the feeding of thyroid extract had a greater effect in the next generation than on the animals fed the thyroid.[15]

Weismann's argument was not based merely on his negative experimental results. Prior to the rediscovery of Mendel's laws in 1900 he had already formulated the distinction between germplasm (or hereditary material) and somatoplasm (the rest of the body) and argued the impossibility of the inheritance of required characters on the basis of the anatomical separation of the two early in development. Reviewing the embryological argument in 1948, Berrill and Liu concluded:

> There is little doubt that he [Weismann] read into his observations ideas that were in a sense already in the air . . . . But it is primarily on the basis of strict recapitulation that Weismann propounded the migration of the primordial germ cells, to which he so stubbornly adhered that he seemed to have defended it to the extent of disregarding the truth. His interpretation of the germ cell origin of *Coryne* serves to illustrate how far imagination can be pushed to suit a preconceived idea . . . . The weight of authority, however, of the Weismann - Nussbaum combination convinced many later workers of the existence of facts they could not observe.[16]

A special form of the inheritance of acquired characters is graft hybridisation, in which grafted plants acquire and supposedly transmit some of the characteristics of their graft partner. This phenomenon likewise has a long pre-Lysenko tradition. In the *Cyclopedia of American Horticulture* (New York: Gordon Press, 1974) Liberty Hyde Bailey discusses the uses of graftage in plant propagation, and adds:

> There are certain cases, however, in which the scion seems to partake of the nature of the stock; and others in which the stock partakes of the nature of the scion. There are recorded instances of a distinct 46
> change in the flavor of fruit when the scion is put upon stock which bears fruit of a very different character. The researches of Daniel (1898) show that the stock may have a specific influence on the scion, and that the resulting [changes] may be hereditary in the seedlings.

Thus, when Lysenko and his followers began to put forward claims of directing hereditary change in the 1930s, Lamarckism was not a dead relic dredged up from the past. It had already been rejected almost universally among geneticists, but was still very much alive in palaeontology and horticulture, and had an extensive literature of experimental results which had never been adequately refuted.

Geneticists were largely unaware of, or indifferent to, the Lamarckian tradition. They regarded it as a carry over of pre-scientific folk science. And in so far as they confronted Lamarckism at all, it was to reject it out of hand because the organisms used were not well characterised, the characteristics supposedly modified were not clear-cut phenotypes like the fruit fly mutants they favoured and the research reports were especially deficient in statistical sophistication. They assumed that Lamarckian results could be explained by hidden selection processes. And, in any case, the impressive successes of Mendelian genetics and the chromosome theory made it simply unnecessary to consider vague allusions to physiological interactions in explanation of dubious claims by not quite respectable authors. (The academic community is as quick as any small town to declare someone a crackpot and not quite believable. The disabilities attached to such a judgement may be anything from smirks to difficulties getting published, even greater difficulties getting read, to unemployment. This is made easier if the person in question lacks formal academic credentials, such as the early twentieth century plant breeders, Burbank or Michurin in the United States and Soviet Union respectively but also applies to wayward colleagues. Thus a whole scientific community may be personally aware and yet intellectually unaware of dissident currents.)

Meanwhile, genetics itself was changing. New phenomena were appearing which were difficult to assimilate. There were the 'dauer-modifications,' changes induced in lower organisms which were transmitted in diminishing degree over as many as twenty generations. New

kinds of material and extra-chromosomal inheritance were being described. Hereditary particles outside the nucleus ('plasmagenes') were postulated, and hints as to the special role of the nucleic acids in here-
47 dity were appearing. The Lysenkoists watched this literature very closely. They saw in it signs of a general crisis in genetics in which *ad hoc* hypotheses and ignored data presaged the final fall of the gene theory.

The contrasting reactions of geneticists and Lysenkoists to the Griffith experiments[17] show how two opposing paradigms can respond so differently to the same experience and each emerge reinforced. The experiment was as follows: a number of different strains of the pneumococcus bacteria exist which can be distinguished by their virulence or non-virulence and by whether the outer capsule is present or absent. Griffith found that live pneumococcus of one variety acquired some of the characteristics of dead bacteria of another strain injected into the same host animal.

From the point of view of genetics, this was an important step in the identification of the genetic material as nucleic acid. From the Lysenkoist point of view, the heredity of one strain of bacteria was transformed by exposure to a specific environment, namely killed bacteria of the other strain. It is therefore by definition the inheritance of an acquired character and the experiment was widely quoted. The important point is that they were formally correct, and that for them this formal precision completely obscured the scientific significance of the experiments. This same approach characterised their treatment of the other anomalies of genetics and cytology. Mendelian genetics asserts that the nucleus controls heredity. But the so-called 'plasmagenes' refuted this. Chromosomes are supposed to be linear arrays of genes, but the best microphotographs of chromosomes showed a distinctly non-linear structure with thousands of loops coming off the chromosomes in a so-called 'lamp-brush' structure.

All of the scientific possibilities opened up by newly discovered phenomena were obscured by a legalistic 'is this or is it not the inheritance of acquired characters?' 'Does this or does it not show extra-nuclear inheritance?' 'Is genetic change directable or not?'

(4) *The Ideological and Social Implications of Genetics*

It is essential to distinguish between what we might call the 'minimal theoretical structure' of a science, which is dependent upon unspoken ideological assumptions, and a kind of ideological superstructure that is built upon but is not logically entailed by the minimal structure. For Mendelian genetics, the minimal structure includes the laws of Mendel and the Weismannian principle that the material substance whose behaviour is formally described by the Mendelian laws cannot be altered
48 in a directed and adapted way by information from the environment, but that the phenotype of an organism is the outcome of the biosynthetic activities of genes taking place in a particular sequence of external and internal conditions. The ideological superstructure that has been laid on this theory by various geneticists includes notions of the 'limits' set to the phenotype by the genetic 'potential', the notion that what is inherited is somehow fixed and unchangeable, that organisms are 'determined' by their genes. By acting as if this ideological superstructure were, in fact, the substance of genetics, geneticists invite a misplaced quarrel with the minimal structure itself. In 1931, Zavadovsky[18] foresaw the inevitable attack on Mendelian genetics that was being invited by the biological and genetic determinism and the pernicious eugenic elitism being read into their science by geneticists. He warned against the extreme environmentalist counter-reaction that would attempt to destroy all of genetics in order to assert the plasticity and perfectability of human society. He was the first, as far as we know, to point out that Lamarckism was anti-progressive since it would imply

that centuries of degradation and brutalisation of workers and peasants had made them genetically inferior as well.

Among the issues of superstructure were several prominent ones. In the mid-1920s, most Soviet and Western geneticists propagated an elitist and racist eugenic ideology,* discussing for instance the breeding of superior types from the ranks of the intelligentsia as well as from those members of the lower classes who had been in the vanguard of the revolution.[19] Eugenicists also claimed that the genetically 'best' elements in the population were being outbred by the 'worst' and that this trend might grow worse with population control. This kind of naive genetic determinism of human behaviour naturally invited ideological attack.

The treatment of the gene merely as a cipher, a book-keeping device, uncoupled genetics from physiology. Thus Bateson explained the Mendelian view to the New York Horticultural Congress in 1902 roughly as follows: 'the organism is a collection of traits. We can pull out yellowness and plug in greenness, pull out tallness and plug in dwarfness.' This uncoupling, so attractive to geneticists and to Anglo-American analytical reductionism, was offensive to Lysenko's group who saw heredity as a special (but not too special) case of physiology.

*Rose and Hanmer suggest that the proposals were also sexist. See 'Women's Liberation, Reproduction and the Technological Fix' in the first volume, *The Political Economy of Science*.

Mendelian genetics, which made the possibilities of artificial selection depend on the fortunate occurrence of useful genes, a small minority of the mutants, imposed limits to the progress of plant breeding which were socially unacceptable given the needs of Soviet agriculture. On the other hand, a model in which the creation of hereditary variation proceeds apace with its selection promised indefinite progress once physiological knowledge was sufficiently sophisticated.

The traits used by Mendelian genetics to develop and argue its theory are clear-cut mutants of *Drosophila* (fruit fly) and a few other organisms. These mutants are a special kind of variant. They are usually non-viable in nature. They were chosen for their unvarying expression so that they could be followed easily while the complicated processes of variable expression so common to adaptive, quantitative and agronomically important traits were ignored. Finally, many of the mutants and chromosomal abnormalities were artificially induced by radiation at dosages so far beyond those that occur in nature as to make it appear that Mendelian genetics dealt with a special class of laboratory phenomena but could not, in principle, deal with the problems such as adapting fruit trees to the far north.

(5) *The Reaction of the Peasants to Collectivisation*

Unlike the Chinese revolution, with its strong political base among the peasants, the Bolshevik revolution could not count on a political and revolutionary peasantry, although 80 per cent of the population was rural. Thus, while Chinese agriculture is rapidly passing from co-operative to collective chiefly by persuasion and local voluntarism, the Russian peasantry, steeped in a petty bourgeois notion of eventual individual land ownership and encouraged in that concept by the market economy of the New Economic Policy, was totally unprepared for the collectivisation that was required by a rational socialist economy. For the Russian and Ukrainian peasants, collectivisation meant appropriation of land and agricultural products by the urban population. It was all one to the peasants whether the product of their labour was taken by a landlord or by a revolutionary government. After all, it was not *their* revolution.

The pressing demand to feed the urban working population resulted in a pace of collectivisation far in excess of what could be supported by

the political state of the countryside. So, when in 1928 the wholesale collectivisation of agriculture began without the long and difficult task of revolutionising the peasants having been accomplished, it was met by
50 forceful resistance and sabotage. Agricultural production was wrecked by ploughing under crops, refusal to sow and harvest, the wholesale slaughter of livestock and attacks on agricultural officials. This force was met with greater and more terrible state force, which eventually won the day for collectivisation but at a great cost in lives, material wealth and political development. Crop yields in 1929 – 30 were 15 – 20 per cent below the pre-collectivisation figures and much more below the optimistic projections of the first five-year plan.[20] Hostile writers like Joravsky and Jasny lay the blame for these losses on the programme of collectivisation, rather than on the peasants' use of force and sabotage to protect their private property. This point of view blinds these authors to the reality of the 'wrecking' and 'sabotage' (which they always put in inverted commas) that characterised Soviet agriculture at the end of the 1920s and in the 1930s. It is entirely reasonable that charges of 'wrecking' levelled by Lysenkoists against their opponents, as an explanation of the failure of proposed methods, should be believed by agricultural officials. An atmosphere of hostility and distrust, grounded in bitter experience, permeated the relations between the state agricultural organs and the mass of farmers. We come here to another aspect of the normal – abnormal model of production discussed in relation to climate. The very real sabotage of agricultural production led to suspicion that instances of failure of Michurinist methods, which, after all, could show striking successes in *some* years and *some* localities, must be the result of abnormal conditions created by the wilful resistance of saboteurs among farmers and agricultural scientists.

(6) *The Class Origins of Scientists and Agronomists*

The suspicion of the more academic 'pure' scientists, including most geneticists, arose in part from their actual histories. Most of the senior scientists of 1930 had been members of the intellectual middle classes of pre-revolutionary Russia. Many had favoured the February revolution but had strongly opposed the Bolsheviks. Men like Vavilov, who was enthusiastic about the socialist revolution from its early days and who displayed a great enthusiasm for the possibilities of science and agriculture in the new society, were the exception. Nevertheless, most agricultural specialists and scientists were kept on in responsible positions because the state seemed to have no choice. Not only in science, but in all branches of technology and management, unsympathetic managers and technicians had to be employed in socialist enterprises if
51 there was not to be a complete breakdown. Soviet authorities were conscious of the difficulties of such a procedure and the position of such pre-revolutionary holdovers was problematical.[20]

In contrast, Lysenko represented the Russian equivalent of the 'horse-back plant breeder', coming from peasant origins and receiving the bulk of his technical training after the revolution. Over and over again the polemic of Lysenkoist and anti-Lysenkoist contrasts are the 'priests' of 'aristocratic and lily-fingered' science with the 'muzhik's son' who is 'illiterate' and 'ungrammatical'. This contest between the effete middle-class intellectuals, and the close-to-the-soil practical agronomists was subtly extended to include a conflict between theory and practice, a vulgarisation of Marxism. In every aspect the conflict in agriculture was a revolutionary conflict, posing the detached, elite, theoretical, pure scientific, educated values of the old middle classes against the engaged, enthusiastic, practical, applied, self-taught values of the new holders of power. That is why Lysenkoism was an attempt at a cultural revolution and not simply an 'affair'.

One of the elements of the cultural revolution was the terror. Joravsky, after a thorough analysis, concludes that: 'Anyway one searches it, the public record simply will not support the common belief that the apparatus of terror consciously and consistently worked with the Lysenkoites to promote their cause.' He points out that the general class divisions between geneticists and Lysenkoists would, in any event, result in more geneticists suffering under a revolutionary terror. While that is undoubtedly true, it must also be the case that the existence of a revolutionary terror, the preponderance of Lysenkoists among state officials, and occasional veiled suggestions by Lysenkoists that they did have access to the organs of terror, would be quite sufficient to inhibit the overt activities of geneticists. Speculation on the way the revolutionary terror might have operated if there had been no historical and class divisions between Lysenkoists and geneticists really misses the point that the struggle *was* in large part a class conflict.

A dispute among plant breeders and geneticists does not invariably become a national *cause célèbre*. However, under Soviet conditions of the 1930s, it quickly became a public issue. One of the early achievements of the Soviet regime was mass publishing. Long before the paperbacks became a lucrative business in the United States, the Soviet Union was publishing world classics, scientific works, poetry and political tracts in cheap editions of tens or hundreds of thousands. The ubiquity of bookstores is a striking feature of socialist cities the world over.

Within this general literacy, science played a special role. There was 52
a widespread consciousness of the relative backwardness of the country and the urgency of rapid technological advance based on the development of science. The development of the Academies of Science of the non-Russian Republics was considered a major step in liquidating the cultural vestiges of Czarist colonialism in central Asia and the Caucasus. This interest in science merged with the older traditional socialist belief that through a scientific understanding of the world, it can be changed for the better – a belief which made at least evolution and cosmology a part of the general liberal education of socialist workers, and before that led Engels into essays in mathematics, tidal friction, human evolution and cosmology.

The Soviet cultural interest in science was especially excited by the broadest, large-scale theories. Vernadsky's concept of the biosphere, Sukachev's biocoenosis (which attempted to treat whole systems, such as forests), Vasili Williams' soil science, which treated the soil as a living system in co-evolution with its vegetation and agricultural practice, Oparin's opening up of the origin of life, Pavlov's exploration of the organisation of behaviour, were both intellectually exciting and aesthetically appealing.

The general alertness to, and interest in, science was heightened by the special practical concern with agriculture and the Soviet food supply.

Here Lysenko had a decided advantage. He was on the offensive, promising advances where geneticists advocated caution. He mobilised large numbers of farmer-innovators whose exploits in plots on collective farms were publicised along with the Stakhanovite innovators in industry. The excitement of bold, sweeping theories, popular inventiveness, the rejection of academic-elitist stodginess in the face of novelty, defiance of the received wisdom, all created an exuberant cockiness described some years earlier by Stalin in his pamphlet *Dizzy with Success*. It was the exultation in the achievements of the early years of the revolution that led to a sense of omnipotence, of daring to do the impossible, of intolerance toward doubters which Stalin was able to perceive, describe, and denounce, but not quite resist.

(7) *Xenophobia*

The Lysenkoist distrust of established academic authority included

both Soviet and foreign geneticists, and was originally part of the iconoclastic exuberance and anti-elitism shared with other sections of Soviet
53 society. But as political and philosophic issues became more prominent in the debate, foreign science was increasingly seen as hostile, as part of the capitalist encirclement. On the naive assumption of a simple one-to-one relation between someone's views in genetics and his or her general political outlook, the anti-Soviet or racist attitudes of foreign geneticists were used to discredit the science. Then sympathy with their scientific views was increasingly assumed to imply political sympathy as well, and close intellectual ties of Soviet with foreign scientists was taken to justify suspicion of disloyalty.

Thus, within a short time, the healthy demand for Soviet intellectual independence was converted into a grotesque xenophobia. It was through this route that Lysenko's opponents were subject to political suppression. The most notorious episode was the arrest of Nikolai I. Vavilov in August 1940. Vavilov, a pioneer in plant genetics and the evolution of cultivated plants, was seized while on a field trip in the western Ukraine and charged with wrecking activities. The particulars included belonging to a rightist conspiracy, spying for England, leadership in the Labour Peasant Party, sabotage in agriculture, and links with anti-Soviet *émigrés*. He was sentenced to death by a military court, and although this was later commuted to ten years' imprisonment, Vavilov died in prison in 1943.

While from the point of view of the Lysenkoists the charges of disloyalty removed leading opponents and silenced other critics, from the viewpoint of the police apparatus their victims' scientific views and international contacts were merely evidence of anti-Soviet activities. Intellectual wrecking – the deliberate making of wrong decisions for the purpose of sabotage – was a respectable accusation in the Soviet Union. In the early 1930s, several British engineers had been convicted, apparently justly, of sabotaging some of the projects of the first five-year plan. Later, in the major purge trials, physicians were accused falsely of murdering the writer Maxim Gorky by deliberate prescribing treatments that would weaken his lungs, already the weak point in his body. This tradition was continued into the post-war period in the infamous doctors' case.

It would not be correct to interpret the anti-foreign hysteria of the late pre-war and early post-war periods as a simple revival of Russian nationalism. Rather, it represented a new, typically socialist form of xenophobia derived from a distorted appreciation of real problems. Scientists in the newly post-colonial countries are very aware of the need for intellectual independence. They recognise that the Western
54 hegemony of science is an instrument of domination. They are aware of the dangers of an excessive regard for established centres of science which leads to the illegitimate transfer of techniques, reinforces the hierarchical, elitist social structure of science, and fosters the ideology of neutral technocracy. In this context, the lesson of socialist xenophobia is not that socialist scientists should return to the fold of the international (largely bourgeois) community of science as the only alternative to a Lysenkoist rampage. Rather, it leads to the demand for a programme of active evaluation and selection of those aspects of foreign science which can be incorporated into the construction of socialist science and a militant resistance to scientific colonialism. This requires a total rejection of the simplistic bureaucratic dogmatic Marxism which sees only the unity of phenomena and therefore equates the philosophy, scientific content, social context and political ideology in foreign science without seeing the heterogeneity and contradictions in it. Ideologically, it means a reaffirmation of dialectical analysis, and this in turn depends on free discussion without administrative fiat.

R. Lewontin & R. Levins, 'The problems of Lysenkoism', in H. Rose & S. Rose (eds), *The Radicalisation of Science: Ideology of/in the Natural Sciences*, Macmillan, London, 1976, pp. 39–54.

6.10

Dominique Lecourt

THE CRITERION OF PRACTICE

From 1948 to 1952, Lysenko's power was absolute: none of his 129
decisions was disputed any longer, none of his theses criticized. Every hostile publication was banned. He undertook to reorganize biology teaching from top to bottom, imposed new textbooks and 'retrained' teachers. There was a systematic purge of the various biological institutes under the umbrella of the Academy of Sciences. As for the Mendelists, their laboratories were confiscated; they were denied the right to pursue their teaching and their research.

But Lysenkoism did not depend for its fate on academic educational institutions. Agricultural technicians had made Lysenko's fortune; he had achieved his fame on the basis of a few spectacular successes obtained in agronomy; he had been able to face up to all his critics and defeat the Mendelist geneticists thanks to the favour his theories enjoyed in the experimental stations. And it was also here that the test of time led to his decline: when the more fantastic applications of the new theory of heredity had ended in spectacular failures that could no longer be concealed.

In 1952 a series of setbacks amounting to a minor disaster in this area made decisive inroads into Lysenko's power. These failures occurred in the implementation of what was then called the 'Great Plan for the Transformation of Nature' or the 'October 20th Plan', laid down in its broad lines by Stalin as a combination of the theses of Vil'yams and Lysenko.

Here are the terms in which Francis Cohen described this plan in *L'Europe* no. 39, March 1949, under the symptomatic title 'The Golden Age: Objective No. 1 in the USSR': 'The Bolshevik Party and the Soviet Government, basing themselves on the experience of scientists and collective-farm workers, have drawn up the "October 20th Plan", whose implementation should in fifteen
years transform the steppe into a veritable park extending over an 130
area of 120 million hectares – twice the area of France. The first point of the plan is to plant a network of windbreaks and forest shelterbelts. These trees will stop the winds, hold in moisture, prevent the snow from melting too quickly and force it to feed the ground with water, suppress surface drainage and hence destructive erosion. Eight main belts will be planted by the state. One, following the Ural River, will stretch from the Caspian Sea to the Ural Mountains. Three others, along the Volga, on the East and the West of the great river, form a second and third barrier. Along the Don, the Donets and between the Don and the Volga, the last four belts will complete the system: in all 5,300 kms in length (about the perimeter of France) and 540,000 hectares in area (about one hundred times the area of the Forest of Fontainebleau).

'At the same time, the state farms and collective farms will plant windbreaks on their lands of a ten times larger total area: 5,700,000 ha. The banks of watercourses will be planted with trees; the slopes of ravines, the edges of ponds will be wooded, the sands of the Caspian region will be fixed. Each "break", i.e., each field containing the same crop, will be separated from its neighbours by trees' (pp. 100–101). The plan included many other measures of the same kind and noted that all this could only be realized by the use of Lysenkoist methods (cluster planting) and Lysenkoist seeds.

It was customary to celebrate this plan as a 'grandiose' initiative. It is clear that this was no exaggeration.

By 1952 the failures had become so significant as to make the Ministry of Agriculture send new directives on shelterbelts which implicitly abandoned the Lysenkoist method of cluster planting of trees.[1] Even before Stalin's death in March 1953, the implementation of the 'Grand Plan' had in fact been abandoned in the form in which it had been announced.

Simultaneously, in 1952, the 'Mendelist' geneticists initiated a counter-attack, based on the failures in practice of the Lysenkoist methods. They began to turn against Lysenko the arguments of effectiveness he had used against them in 1948. One journal provided them with a rallying point: the *Botanicheskii Zhurnal* (*Botanical Journal*). Medvedev well describes the episodes in this campaign, which culminated in 1955 in Lysenko's resignation as

[1] Joravsky, op. cit., p. 154.

131 President of the Academy of Agricultural Sciences and his replacement by Lobanov.

But the essential point is that after 1952, experts and cadres in agricultural production – the backbone of the Lysenkoist troops – began to lose confidence in Lysenko: the crucial element of his strength was thus slipping away. We possess only clues to this withdrawal, but they are irrefutable. The first is a speech to the 19th Party Congress on behalf of the Central Committee by Malenkov in October 1952, which spoke for cadres in agriculture and plainly stated that although 'all anti-scientific, reactionary ideas have been exposed and destroyed in agricultural science, and it is developing now on the only correct, materialist, Michurinist basis . . ., nevertheless it is still lagging behind the requirements of production on the collective and state farms' (cit. Joravsky, op. cit., p. 155). The second is Lysenko's own recriminations; from 1953 on, he constantly complained, with the support of concrete examples, that his directives were only being applied incompletely, slowly and without enthusiasm.

The fact that this retreat was gradual and even very slow is easily explained along the lines of the preceding analyses: Lysenkoism had been organically linked to the political line followed by the Party in agricultural matters for twenty years; precisely to the line which had 'produced' the social stratum of experts, managers and cadres for which Lysenkoist theory had provided an ideological cement.

So there is nothing surprising in the fact that Stalin's death did not lead to Lysenko's fall: only those who see Lysenko as an emanation of Stalin's 'madness' or of the 'cult of personality' and ignore the real historical and social roots of Lysenkoism can find it disconcerting.

Moreover, the situation was complicated by the fact that in 1948 those with ideological and political responsibility in the Party had publicly and unreservedly committed themselves to Lysenko and applied to his doctrine the label 'dialectical materialism'. Hence, without a general re-examination of the question of Lysenkoism, which must have led to an interrogation not only of the nature of Marxist philosophy and its practice in the preceding years, but also that of the question of the forms of ideological struggle and agricultural policy overall since 1917; hence a re-examination which would be inseparable from a critical analysis of all the problems posed by the 'construction of socialism' in the USSR, it was not possible to 'drop' Lysenko. 132

As we know, this great re-examination never occurred; so Lysenko still had to be supported. Given the repeated attacks made on him, the authorities had to commit themselves in his favour once again: on September 29th 1958, *Pravda* announced the award of the Order of Lenin to Lysenko and unreservedly praised his work and doctrine. In December of the same year, a plenary session of the Central Committee of the Party was convened which solemnly reiterated its confidence in Lysenko, reaffirmed its approval for the methods and theories of Michurinist biology and firmly invited Lysenko's opponents to stop their attacks. The editorial board of the *Botanicheskii Zhurnal* were removed and a Lysenkoist team took it over.

For all that, Lysenkoist techniques of selection were being abandoned in practice at the same time. And as a culminating paradox, in consequence of the great revolution which genetics had undergone since the work of Watson and Crick on the structure of DNA and which was beginning to make 'applications' of it actually possible and effective in agriculture, Lysenko's methods were tending to be replaced by openly 'Mendelist' techniques.

Khrushchev's political astuteness is demonstrated by his understanding that there was only one way out of this untenable situation: to get Lysenko to adopt officially agricultural programmes which no longer had anything essential to do with Lysenkoism. Joravsky correctly notes that the famous campaign organized by Khrushchev for the massive planting of maize was based on the use of 'heterosis', a Mendelist method that Lysenko had expressly fought for many years. . . .[2]

Lysenko lent himself to this device with astonishing compliance considering the dogmatic arguments he had previously put forward in favour of his positions: he was rewarded in 1961 when he was restored to the Presidency of the Academy of Agricultural Sciences.

However, it was more and more urgently necessary every day that teaching and research in 'classical' genetics be officially restored. But here too appearances had to be kept up. This was the double

[2] A method which consists, by means of polyploidy, of exploiting hybrid vigour in cases of quantitative characters in order to increase yields. The average increase obtained by this method for maize has been estimated at 37%.

imperative to which the decisions taken by the Central Committee and the Council of Ministers in June 1963 were a response: while emphasizing the interest of Michurinist biology, in fact they restored the compromise situation of before 1948, since they encouraged '*all*' research in biology, medicine and the agronomic sciences. 133

Medvedev's book provides a detailed account of the battle then joined about this decree. Each of the tendencies attempted to exploit it to its own advantage. The important thing, however, is that the process of the re-establishment of genetics was irreversibly under way.

Khrushchev had personally committed himself to Lysenko on several further occasions. Khrushchev's 'resignation' provided the opportunity for forcing Lysenko's retirement at the beginning of 1965.

As the result of a meeting of the Academy of Sciences, he had to leave his post as Director of the Institute of Genetics. At the same time, commissions began to work out new biology textbooks and to organize courses for teachers to bring them up to date with the latest developments of genetics in the West.

The official end of Lysenkoism was occasioned by a symbolic demonstration on the centenary of Mendel's *Memoir* (1865), celebrated with great pomp at Brno in the presence of a large Soviet delegation composed of the most noteworthy of those who had been Lysenko's opponents in 1948. . . .

* * *

On October 10th 1975, *Le Monde* reported on the solemn session of the USSR Academy of Sciences in celebration of its 250th anniversary. The report contained the following lines:

'The Academicians, numbering 245, are elected by secret ballot, which is by no means usual here. More astonishing, Academicians are elected for life and cannot be removed from their work except by a decision of the Presidium of the Academy. Of course, there are a few exceptions to this rule: the most noteworthy was that of Molotov, the former Minister for Foreign Affairs, condemned by Khrushchev in 1957 for anti-Party activities, and expelled from the Academy. On the other hand, Andrei Sakharov is still a member. So, moreover, is Tromfim Denisovich Lysenko, the "charlatan" of genetics, even though he fell into disgrace at the same time as his protector, Mr. "K". Lysenko, who succeeded in getting his absurd theory of the existence of a bourgeois genetics and a proletarian genetics proclaimed as dogma, was also a participant at the Session in the Palace of Congresses.'

A few days later, a speech by the Minister of Agriculture involuntarily revealed the catastrophic figures for cereal production in 1974.

The concatenation of these two facts alone illustrates what I hope has been one of the main lessons of this essay in historical analysis: a politics that retreats from the criticism of its own errors remains willy-nilly subject to the effects of their causes.

D. Lecourt, *Proletarian Science? The Case of Lysenko*, tr. B. Brewster, NLB, Great Britain, 1977, pp. 129–34.

Richard Lewontin & Richard Levins

6.11

CAN THERE BE A MARXIST SCIENCE?

Lysenkoism is held up by bourgeois commentators as the supreme demonstration that conscious ideology cannot inform scientific practice and that 'ideology has no place in science'. On the other hand, some writers are even now maintaining a Lysenkoist position because they believe that the principles of dialectical materialism contradict the claims of genetics. Both of these claims stem from a vulgarisation of Marxist philosophy through delibrate hostility in the one case or ignorance in the other. There is nothing in Marx, Lenin or Mao that is or that can be in contradication with the particular physical facts and processes of a particular set of phenomena in the objective world.

The error of the Lysenkoist claim arises from attempting to apply a 60
dialectical analysis of physical problems from the wrong end. Dialectical materialism is not, and has never been, a programmatic method for the solution of particular physical problems. Rather, dialectical analysis provides us with an overview and a set of warning signs against particular forms of dogmatism and narrowness of thought. It tells us: 'remember, that history may leave an important trace'; 'Remember that being and becoming are dual aspects of nature'; 'Remember that conditions change and that the conditions necessary to the initiation of some process may be destroyed by the process itself'; 'Remember to pay attention to real objects in space and time and not lose them utterly in idealised abstractions'; 'Remember that qualitative effects of context and interaction may be lost when phenomena are isolated'; and above all else, 'Remember that all the other *caveats* are only reminders and warning signs whose application to different circumstances of the real world is contingent.'

To attempt to do more and to try to distinguish competing theories of physical events, or to discredit a physical theory by contradiction, is a hopeless task. For every point of genetics that is supposedly contradicted by dialectical materialism, we can show that, in fact, there is complete support. To the Lysenkoist claim that Mendelism is idealist and formal, we respond that, on the contrary, Mendel solved the problem of heredity precisely by concentrating on the actual pattern of variation among the offspring of a cross, rather than trying to sum up the results in a single idealised description, as others did. Mendel's revolutionary insight was that variation was the thing-in-itself, and that by a study of the pattern of variation he could bring together the two apparently contradictory aspects of heredity and variation under a single explanatory mechanism. The synthesis of the two 'contradictory' elements, heredity and variation, by seeing them as dual aspects of the same phenomenon, was a triumph of dialectical thought. Of course there is a level of abstraction even in Mendel, and he took care to remove some kinds of real variation from his consideration. The reproduction schemes in *Capital* are also abstractions, but in each case the degree of abstraction is appropriate to the problem and does not obfuscate it.

To the Lysenkoist claim that genetics erects the gene as immutable and unchangeable, we reply that, on the contrary, an *essential* feature of genetics is the mutability and variation of genes. If genes were not mutable, genetics could not have been studied, for there would have
61 been no heritable variation.

To the Lysenkoist claim that the template hypothesis of the gene assumes that God must have created the first genes, we reply, 'Remember that the conditions necessary to the initiation of some process may be destroyed by the process itself.' It is, in fact, a triumph of Soviet biology that we begin to understand the conditions for the origin of life and of pre-biotic evolution, and how the evolution of life has destroyed the possibility of present abiogenesis.

To the Lysenkoist claim that genetics erects a barrier between gene, soma and environment, we reply that, on the contrary, developmental and molecular genetics has elucidated the exact material pathway from DNA to protein to environment (the forward path of protein synthesis) and from environment to protein to DNA (the backward path of gene repression and induction), but that such pathways do not happen to include *directed* changes in DNA from environmental contingencies, because there is no material causal pathway for such directed changes, It is pure metaphysical idealism to claim that the dialectical principle of interaction demands that all possible forms of interaction must *ipso facto* exist.

To the claim that genetics does not have a 'correct' view of the internal and external conditions for change, we reply with the metaphor from *On Contradiction* that an egg will not develop into a chicken unless it is placed at the right temperature, but that a stone will never become a chicken at any temperature. That is precisely a paraphrase of the outlook of developmental genetics which asserts that a given phenotype will result only if the genes of the organism are operating in an appropriate environment, but that only some genotypes can operate with that result, no matter what the environment.

There are a number of positive contributions that a dialectical view can make in biology, but these were incompletely pursued by the Lysenkoists, or else applied at inappropriate levels.

Marxism stresses the unity of structure and process. Lysenkoists were justified in rejecting the view which sought explanation in terms of visible structures. It was valuable to expect and investigate the various physiological processes that accompanied the visible fusion of cell nuclei. But in counterposing process to structure, their view was more like that of anarchism, which sees structure as rigidity, death and enemy of process. The emphasis on process resulted in seeing the cell as a blur of interconnection among blurs. In the end, they preserved the structure-process dichotomy.

62 Marxism stresses the wholeness of things, both between organism and surroundings and within organism. Therefore, even among Marxist undergraduates in the 1940s in the United States, there was discussion of the need for feedback from the cytoplasm to the genes in development. But Lysenko did not seriously consider the relative autonomy of sub-systems, while genetic dogma allowed only one-way interaction. It was only much later that the modern genetic view, associated with the work of Jacques Monod, arose in which metabolites combine with some of the genes to regulate the activity of other genes. It is not clear to us whether Monod's own Marxism was relevant to the discovery.

Marxism stresses the integration of phenomena of different levels of organisation. But Lysenkoists saw only the intermediate level of the organism and its physiology. It was a one-dimensional scheme in which the intrusions of molecular events were dismissed as chance, while the level of the population or community was ignored as dynamic entities in genetics or evolution. This despite the pioneering work of Gause

(*The Struggle for Existence*) in Moscow at the same time, which opened up the modern ecology of coexistence.

The view of evolution as the simple consequence of individual genetic modification meant that Lysenkoists in fact had no evolutionary theory distinct from adaptation.

Although Marxism stresses the interpenetration of an object and its surroundings, and although Lysenkoists stressed the importance of environment, they never really took it apart. They did not differentiate among regular and sporadic aspects of environment, local and widespread, short-term and long-term variations, predictable and non-predictable aspects of environment. Therefore they could not separate the different kinds of adaptive responses at the individual and population levels.

Early Marxists had already pointed out the intimate relations of random and determinate events, in which remotely related chains of causality look like chance, random processes have determinate results, and in general the categories are not mutually exclusive. But the linking of the uncertainty principle and indeterminacy to an attack on causality and on the intelligibility of the universe led Soviet Marxists to be hostile to the creative role of random processes in evolution and therefore to be biased both against mutation as a source of evolutionary variation and against the probabilistic models of population genetics. A naive Marxism made Lysenko the enemy of change.

One way in which a Marxist viewpoint can inform scientific work is by encouraging an alternative paradigm to the analytic Cartesian method. Such an alternative stresses system properties as the *primary* objects of study as opposed to the conventional view of the main effect of separate elements, to which are added, as a secondary refinement, the interactions between them. The methodology of the analysis of variance, which separates out main effects and interactions, drives analysis in quite a different direction than does a complex systems analysis. This latter is not the same as an obscurantist holism that denies any possibility of drawing material causal connections. A major success of a complex systems analysis which derives, in part, from a conscious application of a Marxist world view, is the theory of community ecology, with its emphasis on the community matrix and on species interactions.[21]

A more common use of a Marxist approach is in the analysis of how a science has got itself into apparently irresolvable contradictions (but a Marxist analysis is not the *only* way to resolve such contradictions as the history of relativity theory shows). For example, in evolutionary genetics at the present time there are serious contradictions between observations on the genetic variation within species and all standard explanatory theories. But these theories are all equilibrium and steady-state theories which allow no role for historical processes, and they are all theories based on single genes rather than on whole genomes. When an analysis of complex genetic systems is made and when assumptions of equilibrium are relaxed, the contradictions disappear.[22]

We have described the Lysenkoist movement as a failure in several ways. By linking one's stand on scientific issues to basic political partisanship, it brought the whole repressive apparatus into genetics and had disastrous effects on Soviet biology as a whole and on many scientists individually. By depending increasingly on party and administrative support, it undercut its own anti-elitist cultural revolution potential.

It also failed to fulfil its own potential as a scientific revolution and revitaliser of agricultural technology.

The potential of the Lysenkoist movement and its failure can be traced to the same sources: the Marxist philosophical framework, which opened up exciting insights, and the administrative oppression which shut off their creative fulfilment; and behind that the social gap between rural and urban Soviet Union that produced a bifurcation of Marxism into the complex, involuted, dogmatic philosophy of the professional academic Marxists and the common-sense, naive simplistic and often anti-intellectual folk Marxism of the Lysenkoist innovators.

The insights provided by Marxism might have been strengthened and the crudities modified if it were not for the way in which the 'two camps' model was interpreted. The confrontation between socialist science and bourgeois science was seen in the military metaphor as an implacable battle ending with victory or defeat. There was no sense of interaction. Enemy scientific writings consisted of the outrageous or of admissions. We have already pointed out how this prevented any creative assimilation of new developments in genetics. It also made partisanship the test of quality and resulted in a decline in the general level of Lysenkoist research. It established a one-way external interaction between philosophy and science in which the philosophers interpreted, blessed or condemned particular scientific views, but there was never any sense of scientific advances developing the theoretical richness of Marxism. There is some danger that the errors of the Lysenkoist movement, and the recurrent vulgarisations of Marxism that even now repeat those same errors, will inhibit Marxist scientists from making a fruitful use of their world view. We hope that a proper understanding of the history of the Lysenkoist movement will be of some help in bringing the deep insights of Marxism into the practice of science.

R. Lewontin & R. Levins, 'The problems of Lysenkoism', in H. Rose & S. Rose (eds), *The Radicalisation of Science: Ideology of/in the Natural Sciences*, Macmillan, London, 1976, pp. 59–64.

7

BOOK STUDY OF *OUR ORIGINAL AGGRESSION*

A.R. Radcliffe Brown 7.1

FORMER NUMBERS AND DISTRIBUTION OF THE AUSTRALIAN ABORIGINES

FORMER NUMBERS AND DISTRIBUTION OF THE AUSTRALIAN ABORIGINES.* 687

1. General.—Since the white man first began to occupy the Australian continent, the aborigines have very rapidly decreased in numbers. It is, therefore, of some interest to endeavour to form as accurate an estimate as possible of the size of the original population. That is a task, however, that is beset with very great difficulties, for the data are scanty and for the most part unreliable. There have been published since 1788 many estimates of the native population of various parts of the continent. Very frequently, however, the area of country to which the estimate is supposed to apply is not clearly defined, and, generally, the basis on which the estimate is made is not explained.

2. Variation in Density.—It is quite evident that the density of the aboriginal population was different in different parts of the continent, and it seems to have varied fairly closely with the food supply. There is a large area of arid country including part of Western Australia, a large part of South Australia, Central Australia, and small portions of New South Wales and Queensland, which cannot maintain more than a very sparse population. Its area can be roughly estimated at 1,000,000 square miles, or about one-third of the whole continent. On the other hand, there are certain 688
well-watered areas which are better than the rest of Australia in the food supply that they afford for such a hunting, fishing and collecting people as the Australian aborigines. The Murray River for a large part of its course provided one such specially favourable environment. The coastal districts of New South Wales and Queensland seem to have provided another.

3. Method of Estimation.—(i) *Division into Districts.* Any systematic attempt to estimate the former native population of Australia must therefore proceed by dividing the continent into districts and considering each district separately. Further, the territorial areas to be considered must be those recognized by the natives themselves.

(ii) *Aboriginal Territorial Areas.* (*a*) *Tribes.* It would seem that all over the continent the aborigines had the same general territorial organization. We can distinguish three kinds of territorial groups, which will be denoted as "tribe," "sub-tribe," and "horde." A tribe consists of a number of persons who speak one language or dialects of one language and who practise the same customs. The name of the language may be used as the name of the tribe. (*b*) *Sub-tribes.* In some parts of the continent the tribe is subdivided into sub-tribes, which usually, if not always, have differences of dialect within the common language. (*c*) *Hordes.* Everywhere the tribe is divided into hordes. The horde is the land-owning group. Each horde consists of a small body of persons who own and occupy in common a territory of which the boundaries are known. Women enter the horde by marriage from other hordes, but sons belong to the horde of the father for life.

(iii) *Factors to be Determined.* Any accurate estimate of the numbers of aborigines in any district requires a knowledge of the extent (i.e., area occupied) and the volume (i.e., number of persons) of the horde and the number of hordes in the tribe.

4. Western Australia.—(i) *Area north of Gascoyne River.* We may now proceed to consider in order a number of areas beginning at the Ninety Mile Beach, in Western Australia. The first area consists of the country lying north of the Gascoyne River and including the Ashburton, Fortescue, and De Grey Rivers. The total area is about 120,000 square miles or perhaps somewhat more. The area thus defined contains, or formerly contained, not less than 24 tribes, each with its own language. These are the Nangamada at the south end of the Ninety Mile Beach, the Ngerla and Widagari on the De Grey River, the Nyamal on the Coongan, a tributary of the De Grey, the Kariera and Ngaluma on the coast between the De Grey and the Fortescue, the Mardudhunera, Indjibandi, Pandjima, and Bailgu on the Fortescue River, the Noala, Talaindji, Burduna, Binigura, Tjuroro, Djiwali, Tenma, Ina-wonga, and Ngala-wonga on both sides of the Ashburton, and the Baiong, Maia, Targari, and Warienga north of the Gascoyne. There is another tribe on the Upper Gascoyne, and on the upper waters of the De Grey, Oakover, and Fortescue Rivers there are the Ibarga, Targudi, Ngadari and Wirdinya, the exact location of which is not known but whose territory falls wholly or in part within the area we are considering. Thus the average extent of a tribe in this district is under 5,000 square miles. Some of the smaller tribes have considerably less than this.

The native population throughout this area is now very greatly diminished, so that the present number does not give us any indication of the former number.

All these tribes are divided into hordes. There has been no complete survey of even one tribe, but collected data show that the average area of territory occupied by a horde was probably not more than 150 square miles.

It is not easy to obtain accurate information as to the average number of persons in a horde in former times. Some hordes were larger than others. My own inquiries have led me to conclude that the normal or average horde in former times cannot have numbered less than 30 persons, men, women and children.

This would give a density of one person to 5 square miles, or a total population for the whole area of 24,000. The number of persons in a tribe, i.e., speaking one language, would vary from 500 for the smallest tribes up to something over 1,000.

The region is by no means a favourable one. A large part of it is now occupied with sheep stations, but has only one sheep to every 45 acres. It is reckoned that a highly improved station can run one sheep to 10 acres, but this is possible only in the best areas and is quite exceptional. The region was not, therefore, as compared with the rest of Australia, one of dense population.

689 Data that would afford a means of testing this estimate are unfortunately almost non-existent. Charles Harper in Curr ("The Australian Race," I., 287) gives an account of the Ngerla tribe (there spelled Ngurla). The tribe is said to occupy an area of 40 miles by 20 and to have consisted in 1864 of several hundred souls. The tribe certainly occupied a much larger area than this, and Harper's remarks therefore apparently apply to only part of it. A. K. Richardson (Curr, I., 296) estimates the population in 1865 of the Ngaluma tribe as consisting of from 250 to 300 persons. A considerable decrease took place in 1866 as the result of small-pox. The Ngaluma is a small tribe with not more than 2,500 square miles of country. One of Curr's informants (Curr, I., 302) writes of what he calls the Kakarakala tribe as extending from North-west Cape to 30 miles south of the Gascoyne River, and from 30 to 50 miles inland, and estimates the number in this area in 1877 at about 2,000. The area defined actually included four tribes: the Talaindji, Baiong, Maia and Ingarda, and my own estimate would require a population of 2,500 to 3,000 for the four. It may be noted that the estimate was made by Curr's informant about two years after the natives had suffered a very heavy mortality from small-pox.

(ii) *South-Western Area.* We may next consider the south-western portion of Western Australia now occupied as agricultural country. There is here about 50,000 square miles of comparatively well-watered country which provided a fairly favourable environment for the aborigines. We have no information about the territorial divisions of the aborigines (tribes and hordes) that is of any value.

In the early days of settlement, the population of the region of the Swan River settlement was estimated by Sir James Stirling at one native to 2 square miles. Seven hundred and fifty were known to have visited Perth from the district surrounding it, about 40 miles each way. This is probably an over-estimate, but is about the only figure we have.

I believe, however, that we shall be safe in allowing one person to 4 square miles for this region, giving a figure of 12,500.

(iii) *Murchison District and Eastern Goldfields Area.* In addition to the two areas considered, there is an area of about 100,000 square miles, including the Murchison District and the Eastern goldfields, that had a population that I propose to put down provisionally, at 5,000, or one person to 20 square miles.

(iv) *Total for Western District.* Thus for the western part of Western Australia, an area of 270,000 square miles, I propose to assume that there was a native population of 41,500. Excluding about 100,000 square miles of the Kimberley District in the north, which will be treated separately, we are left with an area of 605,920 square mile of arid country almost entirely unoccupied by white settlement and partly unexplored. The whole of this vast region has or had an aboriginal population, but undoubtedly a very sparse one. There are no data whatever on which to base any estimate of their numbers.

(v) *The Kimberley District.* Dr. A. P. Elkin has kindly given me an estimate of the former population of the Kimberley District, based on his recent ethnological investigations in that area. He puts the original population at about 9,700, divided into 26 or more tribes, varying from small tribes of 100 to large ones of 1,000.

(vi) *Total for State.* I estimate, therefore, that Western Australia contained originally not less than 52,000 aborigines, and more probably 55,000, over an area of 975,920 square miles, much of which is desert.

5. **South Australia.**—(i) *General.* Passing to South Australia, a great deal of that State is arid and was very sparsely peopled, and the south-eastern part alone provided a favourable environment. Of the total area of 380,070 square miles, only a little over 60,000 square miles have a rainfall of over 10 inches.

(ii) *Estimates by Moorhouse and Eyre.* Moorhouse in 1843 estimated that there were 1,600 aborigines in regular and irregular contact with the Europeans distributed in the Adelaide district, Encounter Bay, Moorundie, Port Lincoln and Hutt River. If the districts within 120 miles south, 160 miles north and 200 miles east of Adelaide were included, he estimated that the total would be about 3,000. Eyre thought this an under-estimate, and that if the Port Lincoln Peninsula were included the number
690 would be 6,000. Both Moorhouse and Eyre had better opportunities than any one else to form an estimate of the aboriginal population. Nevertheless, I think it can be shown that even Eyre's estimate is too small if we include that part of South Australia through which the Murray River flows.

(iii) *Murray River Area.* The Murray River, from a point westward of the Darling Junction to the mouth, was occupied by two groups of tribes. One group had the word *meru* for " man " or " blackfellow," and included the Ngintaitj, Yuyu, Yirau, Nyauaitj, Ngaiyau, Ngangurukn and Ngaraltu. The other group used the term *ngarindjeri* for " man " or " blackfellow," and hence are frequently referred to by the name Narrinyeri. This group included four or five tribes—the Portaulun, at the entrance of the Murray to Lake Alexandrina; the Yaralde, on the south of Lake Alexandrina and on Lake Albert; the Tanganalun, on the Coorong; and either one or two tribes on the north side of Lake Alexandrina and at Encounter Bay.

A small portion of the area occupied by the Meru tribes belongs to Victoria and New South Wales, but the greater part of it belongs to South Australia. These tribes had suffered a very heavy mortality from small-pox before the white man first came in contact with them.

It would take a good deal of space to discuss critically the evidence relating to these tribes. There is good evidence that the population was, for Australia, a dense one. In 1877 there were still living about 400 of the Yaralde tribe, the names being contained in a list written down by Taplin at that time. The tribe cannot have numbered originally less than 600 and was probably more than 800 before 1820. The tribe was divided into 22 or more large hordes which probably contained not less than 40 persons on the average. Taplin states that " all the Narrinyeri on the southern sides of Lakes Alexandrina and Albert," i.e., the two tribes of Yaralde and Tanganalun, " could muster easily 800 warriors." To provide 800 fighting men a population of 2,400 must be supposed for these two tribes together. This is perhaps an over-estimate. Taplin relates that in 1849 he saw a battle where 500 of the Narrinyeri met some 800 of the Murray natives.

(iv) *Total for South Australia.* Allowing something for the tribes east of the Murray to the Victorian border, we are, I think, keeping quite on the safe side in estimating an original population of 6,000 for the south-eastern portion of South Australia east of the Mount Lofty Ranges. Estimates for the rest of the State are difficult to arrive at with any certainty, but I believe we can quite safely assume one person to 80 square miles. A total population of 10,000 for South Australia as a whole is, therefore, probably well under the true figure.

6. **Victoria.**—(i) *Early Estimates.* For Victoria a number of estimates of population were made in the early days of occupation. E. S. Parker, who was for many years a Protector of Aborigines, and had probably better opportunities than any other person for forming a reliable judgment, estimated that at the foundation of the colony the aboriginal population was 7,500. It is evident also that he tried to make a real estimate and not a mere guess. He said in a lecture given in 1854: "In the year 1843, I endeavoured to take a nominal census of the aboriginal population in the district extending from the Goulburn on the east to the Upper Wimmera on the west, and from the Great Dividing Range between the coast river and the interior waters on the south and the Mallee country on the north. I found and registered by name, in their respective families and tribes, about 1,100 individuals."

A later Protector of Aborigines, William Thomas, after a careful estimate, concluded that the aboriginal population of Victoria before the white occupation could not have been less than 6,000.

These two estimates, by Parker and Thomas, are the most reliable we have for the whole colony.

About 1845, Robinson, Chief Protector of Aborigines at Port Phillip, estimated the population of that district to be at least 5,000.

Brough Smyth in 1878 gives a much smaller number, estimating that the total aboriginal population of Victoria did not number more than 3,000. But the estimate is based on arguments that are open to grave suspicion, and should, I think, be rejected.

We are thus left with three estimates—not less than 5,000 (Robinson), not less than 6,000 (Thomas) and 7,500 (Parker). To them we may add McCombie's statement that Victoria when first colonized contained 7,000 aborigines.

(ii) *Early Estimates for Districts.* We have also a few early estimates of the population of certain parts of the colony. Thomas states that in 1835–6 the aboriginal population of the counties of Bourke, Evelyn and Mornington was 350. He adds that one-half at least of one of the tribes inhabiting these counties had perished in 1834 in a war with Gippsland and Omeo blacks, and that previous to the war the total number was certainly not less than 500. As the three counties mentioned had an area of about 3,000,000 acres, this would give one person to 6,000 acres.

Westgarth in 1848 writes: " The entire area of Australia Felix does not probably contain at present more than five thousand aborigines, or about one aboriginal inhabitant to each nineteen square miles. Of this scanty population about one thousand are in Gipps' Land, two thousand in the Western Port, Murray and Wimmera districts, and two thousand throughout the remainder of the territory."

By 1848 the aboriginal population had been considerably reduced as the result of small-pox, and of the white settlement.

About 1845 an attempt was made by the Aborigines Committee of the Legislative Council of New South Wales to discover the number of the aborigines. Victoria then consisted of five districts. Gipps' Land was estimated by Tyers to contain 1,000 aborigines, the Murray district was estimated by Smyth to contain 200. Fyans estimated the population of the Portland Bay district 3,000, and Wilson gave 300 for Normanby county, which was part of that district. No numbers were obtained for the Wimmera district. For the Western Port district Powlett gave an estimate of 1,000, but within this district Addis gave 200 for Grant county. Thomas gave 165 for Yarra and Western

Port, and Parker gave 302 for the Upper Goulburn and Campaspe Rivers, 200 for the Lower Goulburn, 350 and 670 for the country north and west of the River Loddon. These separate estimates would give considerably more than 1,000 for the whole Western Port district.

We may consider these five subdivisions of Victoria separately. If we accept Howitt's account, Gippsland formerly contained six tribes—Brataualung, Brayakaulung, Brabralung, Tatungalung, Krauatungalung and Bidweli. There were local subdivisions of the tribes, and of these Howitt enumerates twenty for the first five tribes mentioned above. If the total population of the five tribes was 1,000, this would give an average of only 200 per tribe and an average of 50 persons for each local subdivision of the tribe.

The Rev. John Bulmer in 1878 thought that the aborigines in Gippsland could never have numbered more than 1,000 or at most 1,500. Curr (III., 543) estimated the original population at 1,500. This is probably nearer to the truth than 1,000.

If we accept the low estimate of 1,000 for Gippsland this would give a density of only one person to 15 square miles. As the region is of heavy forest, it may well have been only sparsely populated except on the coast. But the figure of 1,000 seems likely to be an under-estimate. We may accept it as the irreducible minimum.

The Murray district was bounded on the north by the Murray, on the south-east by the Australian Alps and on the west by the Goulburn River. The estimate of 200 for the district by Smyth is certainly wrong. A. C. Wills, former Police Magistrate and Warden at Omeo, stated that in May, 1835, there were about 500 or 600 men, women and children resident during a few months of each year at the headquarters of the "Gundanora" tribe on the elevated plain of Omeo. In 1842 they frequently assembled in larger numbers. In 1862 H. B. Lane stated that "the 40 blacks to whom rations, &c., are distributed at Tangamballanga are the sole remnant of three or four once powerful tribes each of which, even within the memory of old settlers, numbered from 200 to 300 souls. These tribes inhabited the tract of country now very nearly described on the electoral map as comprising the Murray district of the Eastern Province, and comprising an area of about 2,000 square miles." He goes on to state that the country was one well suited for the blacks.

For the tribes of some parts of the Murray district we have little information, but for those at the junction of the Goulburn and Murray Rivers we have the probably reliable observations of Edward Curr, who was a pioneer settler there in 1841. His account would show 1,200 aborigines in an area of 3,000 to 3,500 square miles, or one
692 person to 2.5 or 3 square miles. Of these 1,200, 550 occupied a small area of about 1,200 square miles between the Goulburn and the Murray, and belong to the Murray district, the remainder belonging to New South Wales or to the Western Port district of Victoria.

Scanty as these data are, they point very distinctly to the whole aboriginal population of the Murray district, i.e., the region between the Goulburn and Murray Rivers, as having been probably over 2,000. To be on the safe side and keep always to a minimum we may put 1,500.

Turning now to what used to be called the Western Port district, this was occupied by a few large tribes, called by Parker "petty nations." These were the Bunwurung, Woewurung, Tagunwurung, Djadjawurung and Wudjawurung. Each of these tribes was subdivided into local divisions, which we may regard as sub-tribes. Howitt enumerates five such for the Woewurung tribe. Parker gives seven for the Djadjawurung. The sub-tribe was further subdivided into groups which Howitt calls "clans," there being three or four such in the Wurunjeri sub-tribe of the Woewurung. According to Howitt the clans were again subdivided into lesser groups of people, and each had its own definite tract of country and food grounds.

A. C. Le Souef, a good observer with exceptional opportunity, describes what are apparently four sub-tribes of the Tagunwurung tribe. He gives their names as Bootheraboolok, Natrakboolok, Nerboolok and Ngooraialum, and estimates the original numbers of the first two at 100 each and of the last two at 200 each. On Curr's map (III., 566) these groups occupy an area of about 4,500 square miles. Le Souef's estimate therefore gives one person to 7.5 square miles.

Parker describes the Djadjawurung as subdivided into seven parts, which he calls "tribes," and as having "at a remote period numbered about one thousand beings." With a total of 1,000 the average number of a sub-tribe would have been less than 150.

Seeing that each sub-tribe spoke a separate dialect and was divided into hordes, it will seem that we cannot possibly estimate the sub-tribe at less than 100 persons, and for the five tribes mentioned we cannot allow less than 3,000 persons. This figure receives some confirmation from the fact that in 1843 Parker was able to enumerate by name 1,100 individuals between the Goulburn and the Upper Wimmera.

For the Portland Bay district we have Fyans' estimate for 1845 at 3,000, and Wilson's estimate of the same date of 300 for Normanby county. Dawson says that 21 "tribes" used to hold their great meetings at a marsh some miles west of Caramut. His estimate is that each "tribe" mustered 30 fighting men or 120 persons on the average, thus giving a total of 2,500 for the tribes referred to. The coast tribes are not included, as they did not attend these meetings. Dawson adds: "In the estimation of some of the earliest settlers, this calculation of the average strength of each tribe is too low." What Dawson calls "tribes" appear to be sub-tribes. The names of the tribes proper are not known. Tjapwurung seems to be one of them. Dawson writes that at the annual meetings "where sometimes twenty tribes assembled there were usually four languages spoken, so distinct from one another that the young people speaking one of them could not understand a word of the other three." It would seem therefore that there were at least four distinct tribes divided into twenty sub-tribes.

Brough Smyth, on the information of H. B. Lane and Charles Gray, gives an account of the "tribes" of part of the Portland district. These are really sub-tribes, and it would appear that 25 of them occupied an area of about 6,750 square miles, or on the average 270 square miles each. If we take Dawson's estimate of 120 to the sub-tribe, we have a density of one person to $2\frac{1}{4}$ square miles. This would seem to be perhaps too high.

Allowing, however, that Dawson's statements refer to only part of the Portland Bay district, and allowing also for Wilson's estimate of 300 in Normanby county in 1845, we must conclude that the figure of 3,000 given for this district in 1845 is not too high, and that the original population was probably considerably more than that figure.

The Wimmera district falls into three parts. The southern part on the Upper Wimmera was probably well populated. The central portion around Lake Hindmarsh and to Lake Tyrrell had a sparser population. The region bordering the Murray River was inhabited by a number of small tribes, there being seven of them between the Loddon and the Darling junction. These river tribes were enormously reduced by small-pox in the thirties, but even then were numerous, and the evidence is that this 693
was one of the most densely populated regions of the southern part of Australia. Probably this portion of the Murray from Echuca to the Darling junction, and including some part of the Murrumbidgee, originally supported a population of not less than 5,000 or 6,000 aborigines divided into ten or twelve tribes. We may reasonably allot 2,000 of them to Victoria.

An estimate of 1,000 for the southern and central part of the Wimmera district would not be an over-estimate.

(iii) *Total for Victoria.* We then reach the following estimate for Victoria as a whole :—

District.	Number of Aboriginals.
Gippsland	1,000
Murray District	1,500
Western Port District	3,000
Portland Bay District	3,000
Wimmera District	3,000
	11,500

This estimate would still give a density of only one person to 7.65 square miles.

This figure of 11,500 is considerably in excess of Parker's figure of 7,500, and there is good reason for thinking that Parker's was by far the most carefully made of the early estimates. It would seem (1) that Parker was not making allowance for the tribes on the Murray River, who count for 2,000 in my estimate. (2) Parker made no allowance for the very heavy mortality from small-pox for which we have good evidence in Victoria (except Gippsland) in the decade before the white settlement. (3) It will be noticed that very regularly estimates for a large area give a smaller proportionate population than those for smaller areas. We should allow, I think, a very great weight for estimates made for limited areas by reliable informants such as Curr and Le Souef, who had far better opportunities of getting exact information than Parker had. I have therefore relied on such statements in making my general estimate. (4) The figure does not by any means seem excessive when we consider the great diversity of language and dialect in Victoria. If we allow only 500 persons for a tribe or language and only 100 to 120 for a dialect, the total estimate of 11,500 for the colony is not extreme, and would, indeed, seem to be too small. Taking all these things into consideration, my own impression is that 11,500 for the original population of Victoria before the small-pox is decidedly an under-rather than an over-estimate.

7. **Queensland.**—(i) *General.* As it is difficult to arrive at any estimate of the numbers in New South Wales, I propose to consider Queensland first. In dealing with this area it must be remembered that before the white settlement there had been already a mortality from small-pox which was probably very heavy and that there was, in many districts, in the first two or three years of settlement, an enormous mortality, chiefly, though not entirely, amongst men, as the result of massacres by settlers and police. There is abundant evidence that many thousands of aborigines were shot in order that the white man might enjoy undisturbed their tribal lands.

(ii) *Estimates for Various Areas.* The first area I propose to consider includes a small part of New South Wales. It extends from the Clarence River in the south to Broad Sound in the north, and is bounded by the watershed between the eastward and westward flowing rivers. This area included a number of tribes. From the Clarence River to the Burnett River the chief tribes were the Yukumbil, Yagara, Djandai, Waka, Kabi and Koreng. From Port Curtis to Broad Sound there seem to have been seven smaller tribes—Tarambara, Yetimarala, Kuinmurbara, Ningebal, Warabal, Tarumbal and Urambal. Each of these tribes was subdivided into sub-tribes, and for the greater part of the area the sub-tribes have names which are formed by means of the suffix *-bara.* A probably incomplete list from W. H. Flowers enumerates seven such sub-tribes for the Kuinmurbara, five for the Ningebal, four for the Tarumbal, and four for the Warabal. For the Kabi tribe we have two lists, one giving sixteen and the other 23, but even by combining the two it is not possible to make a complete list.

Each tribe had its own language, and each sub-tribe spoke its own dialect of the 694
tribal language. The sub-tribe was further divided into a number of hordes, each of which was a land-owning group. My own inquiries for this region have led me to conclude that each horde occupied, on the average, about 100 square miles or less, and may be taken as having on the average 30 members, men, women and children, or more. This will give us a density of population of three persons to 10 square miles. The part

of the region that lies in Queensland may be estimated roughly at 50,000 square miles, and the population would therefore be 15,000. This would mean that in the northern part of the region the small tribes would contain about 450 individuals in an area of about 1,500 square miles, divided into sub-tribes of perhaps 100 persons, each subdivided into a few small hordes. The larger tribes, such as the Kabi, would number 2,500 persons or more divided into sub-tribes of about 100, and these subdivided into small hordes.

That this estimate is very moderate is indicated by early statements. Thus, Howitt's informant (Flowers) states, with reference to the Kabi tribe, that "about the year 1859 these blacks might have been counted by thousands." In an account forwarded to Curr by the Chief Commissioner of Police, Brisbane, in 1879, with reference to Great Sandy or Fraser's Island, which is a small part of the Kabi territory, it is stated that in 1849 the population of that island, which was split into nineteen "tribes," amounted to about 2,000 souls, of whom 300 or 400 still survived in 1879. J. D. Lang, in 1861, wrote: "Frazer's Island is rather of indifferent character, in point of soil and general capabilities, in the estimation of Europeans; but it is an excellent fishing station, and abounds in the requisites of aboriginal life. It is consequently very populous—the number of aborigines in the island being estimated at not fewer than 2,000." This figure of 2,000 for the island seems excessive. It may well be that such a number might be found in the island at certain seasons when there were visitors from the mainland. We know that very large numbers of natives used to collect together in the Bunya Mountains from a wide radius to feast on the bunya nuts when they were in season. Still, Lang's statement indicates that the estimate I have made for the whole region is probably well below the true number.

The basin of the Burdekin River and its tributaries, and the coastal districts from Mackay to Cairns give an area of something over 65,000 square miles of well-watered country. According to G. F. Bridgeman, there were four "tribes" within a radius of 50 miles or so of Port Mackay. The country was occupied about 1860, and during the eight or ten years which followed, about one-half of the aboriginal population was either shot down by the police or perished from disease. Numbers were carried off in 1876 by measles. In 1880 one of the tribes numbered about 100. This would seem to give us a figure for the original population of not less than one to 6 square miles. James Cassady states that the Halifax Bay tribe occupied a tract of country fronting the shores of the bay for about 50 miles and extending 15 miles inland. It was divided into seven sub-tribes. The population in 1865 is estimated to have amounted to about 500 persons. The numbers in 1880 were approximately 200, the decrease being said to be due mostly to massacres by settlers and native police. Even if we allow an area of 1,000 square miles for the tribe, this gives us a density of one person to 2 square miles, each sub-tribe numbering about 70 in an area of under 150 square miles. Lumholtz, who visited the Herbert River in 1882, when the number of natives had already somewhat decreased, and who had good opportunity for making a reliable estimate, describes the natives as divided into what he calls "family tribes," i.e., apparently hordes, each containing about 20 to 25 individuals, often less. His estimate of the extent of a tribe is about 40 miles by 30, and its volume at 200 to 250 persons. This gives a density of only two persons to 10 square miles.

A comparison of the accounts given of the country around the Cape River indicates that the tribes here were divided into a few large sub-tribes, each with more than 400 persons and occupying about 1,600 square miles. This gives a density of not less than four persons to 10 square miles.

Allowing for differences of population in different parts of the area, greater on the sea-coast but less in such a forest or scrub region as the Herbert River, I think we are safe in allowing one person to 4 square miles for the whole area, or 16,250 in all.

695 The area occupied by the Dawson, Comet and Mackenzie Rivers and other tributaries of the Fitzroy may be estimated at something over 45,000 square miles. A tribe in this region at the head of the Comet River is estimated to have numbered 500 in 1860, was 300 in 1869, and 200 in April, 1879. A reasonable estimate for this region is 10,000.

If we compare the statements of Roth with those of Curr's informants it would seem that the Boulia district contained fifteen or more small tribes numbering from 100 to 300 persons. Roth estimates the area at 10,000 square miles, but that is, I think, a gross under-estimate. The area in question is probably 30,000 square miles, and we can perhaps allow for it a density of one person to 10 square miles.

(iii) *Total for Queensland.* It would take much space to discuss critically each part of Queensland. The conclusion I have reached after examining the available evidence, admittedly not, by any means, satisfactory, is that Queensland could not have contained less than 100,000 aborigines, and probably had more than this.*

8. New South Wales.—For New South Wales I will not examine in detail the scanty data available. In 1788, the first year of settlement of Port Jackson, Governor Phillip took the numbers of the aborigines of Port Jackson by causing inspectors to visit every cove or inlet at the same time. One hundred and thirty were counted, who had with them 67 boats or canoes, and many were known to be in the woods making these vessels. The Governor at that time estimated the population between Botany Bay and Broken Bay at 1,500. This population was practically extinct by 1845. A native of the tribe occupying the southern coast of Port Jackson stated that in his recollection, in the time of Governor Macquarie (1810–1821), there were about 400 individuals in the tribe. By 1845 he and three women were all that survived.

The coastal region of New South Wales probably was fairly densely populated, perhaps more in the north than in the south. My estimate is that that part of the State contained about 25,000 aborigines, speaking more than twenty different languages, and that the rest of the State had about 20,000. To be on the safe side we may put the total for the whole State at 40,000.

9. **The Northern Territory.**—Ethnological researches in the Northern Territory now in progress will ultimately, it is hoped, help us to obtain a more accurate knowledge of the original population. Existing data suggest that the whole country probably contained 35,000 persons divided into more than 60 tribes, each with its own language.

10. **Tasmania.**—For Tasmania the available evidence is unsatisfactory. Early estimates of the population are from 6,000 to 8,000 (G. A. Robinson), 5,000 (Captain Kelly), not much, if at all, over 2,000 (Dr. Milligan), and between 700 and 1,000 (Backhouse). There seem to have been four tribes with four distinct languages, divided into sub-tribes with different dialects, and then again divided into hordes which rarely contained more than 30 or 40 individuals. The best estimate that can be made is that the original population was probably not less than 2,000 nor more than 3,000.

11. **Total for Australia.**—It has been impossible to discuss all the data on which these estimates have been based. As remarked in reference to Victoria, it is noticeable that estimates for small areas always give a greater density of population than those for larger areas in the same part of Australia. I believe that in general the estimates for small areas are more reliable than those for larger areas. It has been necessary to consider the reliability of each statement by judging as well as possible what opportunities the person had for making careful observations. Statements by persons who lived for some years in close contact with the natives before depopulation had begun or had proceeded very far have been given the most weight. Allowance has been made for differences in the food supply in different regions. Finally the estimates have been throughout considered in relation to the languages and dialects (tribes and sub-tribes) and land-owning groups (hordes).

* I may quote two out of many scattered statements which go to show that Queensland had a large aboriginal population. Thomas Hall, of Warwick, records how 200 to 300 men would take part in a wallaby drive in the Darling Downs region. A. L. P. Cameron wrote in 1884; "In 1868 I saw gatherings of from 800 to 1,000 in Western Queensland, about 150 miles north of the New South Wales boundary line, and now I am told, on trustworthy authority, that the whole district could not produce a third of that number."

The following estimate, then, I regard as giving the *minimum* that we can reasonably estimate for each portion of Australia. 696

FORMER ABORIGINAL POPULATION OF AUSTRALIA.

District.	Estimated Number of Aborigines.	Area (square miles).	Density (number of square miles per aboriginal).
Western Australia	52,000	975,920	18·8
South Australia	10,000	380,070	38·0
Victoria	11,500	87,884	7·6
Queensland	100,000	670,500	6·7
New South Wales	40,000	*310,372	7·8
Northern Territory	35,000	523,620	15·0
Tasmania	2,500	26,215	10·5
Total	251,000	2,974,581	11·9

* Inclusive of Federal Capital Territory.

This estimate gives the density of population for the whole continent as being one person to 12 square miles. There is good evidence that in some parts the density was much greater than this, and in considerable areas was at least as high as three persons to 10 square miles, while even in fairly arid regions there was a density of one person to 10 square miles. Omitting, therefore, about one-third of the continent as being desert and having a very sparse population, we ought to be able to reckon that the remaining 2,000,000 square miles would have had a density of population of one person to every 6.5 or 7.5 square miles. At the former figure we should have a population of a little more than 300,000, and with the latter over 260,000.

It is not possible to give an exact count of the number of native languages, still less of the dialects into which they were subdivided. It seems fairly certain, however, that there were more than 500 distinct languages, so that our estimate would allow about 500 persons to a tribe or language on the average. What knowledge we have indicates that we cannot allow a smaller figure than this.

In conclusion, therefore, I would say that the available evidence points to the original population of Australia having been certainly over 250,000, and quite possibly, or even probably, over 300,000.

A. R. Radcliffe Brown, 'Former numbers and distribution of the Australian Aborigines', in Commonwealth Bureau of Census and Statistics, *Offical Year Book of the Commonwealth of Australia*, no. 23, Government Printer, Melbourne, 1930, pp. 687–96.

7.2

Henry F. Dobyns

AN APPRAISAL OF TECHNIQUES WITH A NEW HEMISPHERIC ESTIMATE

THE IDEA that social scientists hold of the size of the aboriginal population of the Americas directly affects their interpretations of New World civilizations and cultures. Vaillant (1944: 29), for example, wrote that "population pressure" was "often an indirect cause of war in the Old World," but that it "was virtually non-existent in Indian America." Certainly Germany, Italy and Japan went to war a generation ago claiming they were so overpopulated that they required additional territory (Organski and Organski 1961: 4). Vaillant also concluded, on the other hand (p. 112), that "sheer pressure of population was an important cause for the military exploits of Tenochtitlan." Cook (1946: 83), moreover, independently raised the question whether the religion "centered around human sacrifice" of captives, which was the "focal point of the entire Nahua civilization," was not evidence of a "social urge" toward checking population increase. He pointed out that mass immolation of captives seems to have become common just when the central Mexican population reached its maximum density and the margin of subsistence turned "somewhat precarious."

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Such relationship between cultural interpretation and human population in a given area is perceived because social scientists view changing population density as a social dynamic. The transition in social organization from homogeneous to segmented structure has been directly attributed to population increase. An increasing population related to increased food supply "is sufficient to account for the appearance," Oberg (1955: 483) claimed, of a social structure with moieties, sibs, and associations. "Numerical change and concentration appear" to Oberg (p. 484) "to be the principal factors affecting this change." A closely related point of view sees the formation of new tribal groups as a function of increasing population. "If the population increased to the neighborhood of 5,000, most tribes—even nationality tribes—seem to have broken apart from sheer weight of numbers" (Kroeber 1955: 304–05).

As a result of this view of population change as a cultural dynamic, social scientists (Naroll 1956: 688–93; Mason 1959: 87ff.; Ember 1963: 229–32) have taken population density to be one index of cultural development. There is, however, little novel, other than numerical expression, in such views. A Swiss artist who worked in trading posts on the Missouri River in the early 1850's remarked upon "a distinct difference in the development of Indian nations, according as more densely populated communities increased the necessities of existence" (Jarrell and Hewitt 1837: 289).

Discussions of the relative power of national states share this view of population as a fundamental independent variable. Population size is "a major determinant of national power" according to Davis (1958: 199) because, first, it is one of the main factors in the size of a nation's labor force; second, mass production and distribution are advantageous proportionately to the scale of the population they serve; and finally, military personnel for fighting or occupation duty must be drawn from total population ever since the invention of the citizen-army. Organski and Organski (1961: 31) predicted no "dramatic shifts" in population rank of nations because the days of mass migration have passed. Yet they clearly see population as a major component of national power (p. 3). They assert that a "great power" must have at least 45,000,000 inhabitants (p. 13) although they recognize that population is not the only determinant of relative power (p. 4).

This view of population finds its anthropological expression in a view of society which assumes political integration to be a constant. Murdock (1957: 674) has in effect considered gross size of population the only independent variable for distinguishing between types of social order. In a closely related albeit dynamic view, Spicer (1962: 39, 99) attributes differential results of Indian reaction to Hispanic culture to variations in aboriginal population density.

The whole school of recent thought that relates cultural development to the amount of harnessed energy available within a given society directs attention to the importance of the amount of manpower available in that society. Cottrell (1955: 6), for example, views man as one type of 'energy converter' in terms of which all other such converters can be evaluated. He identifies the relationship between food raisers and other members of a society as "the crucial area for analysis" because of the determination of minimum energy requirements by the size of population (p. 117).

The particular concept that social scientists share of aboriginal American population in turn assumes some direct practical importance in contemporary affairs. It becomes broadly significant because the social science concept diffuses to public policy makers (cf. International Labour Organization 1953: 30–31). It influences such men as attorneys involved in the decision-making process of governmental commissions and courts (cf. Campbell and Clark 1962: 23–24). It colors the conclusions of other scientists; e.g., in a medical history of the conquest of the Americas whose thesis is "that disease had profound and specific effects" which were both "decisive and determining," one glimpses this implicit conditioning of conclusions in a statement about the Indians: "In view of the immense territory they held, their total numbers, probably between 2,000,000 and 2,500,000, were astonishingly small" (Ashburn 1947: xvii).

It seems worthwhile, therefore, to examine the validity of the widely held concept of a low density of American population prior to European conquest and colonization. This paper[1] attempts to analyze some major methodological reasons why estimates of aboriginal American population have yielded a picture of small scale preconquest human population in the Western Hemisphere.

CONSERVATIVE HEMISPHERE WIDE ESTIMATES

Rosenblat (1935; 1945: 92; 1954: 102) arrived at a hemisphere-wide estimate of 13,385,000 Indians for 1492, taking that date to represent preconquest conditions. Rosenblat's work comprises the most important general summary of the subject published in Spanish to date and is cited approvingly by writers in Spanish (cf. Barón 1942: 143–45). Rosenblat implied an average aboriginal population density in the New World (including Greenland) of one person in just over three square kilometers (0.319 per km²). In the English analysis most widely referred to, A. L. Kroeber (1939: 166) estimated 8,400,000 Indians in the Americas in preconquest times, although he admitted that his figures were low. This estimate implied an average aboriginal population density in the Americas (including Greenland) of only one person per five square kilometers (0.2 per km²). Both Kroeber and Rosenblat proceeded implicitly on the general methodological assumption attributed by Wilcox (1931: 44) to James A. Garfield (who attributed it to Samuel Johnson): "To count is a modern practice; the ancient method was to guess; and when numbers are guessed they are always magnified."

The Rosenblat and Kroeber hemispheric population estimates represent the extreme reaction to those made by earlier commentators on New World population beginning with Bartolomé de Las Casas. In 1541 (MacNutt 1909: 314; Philips 1656: 1), Las Casas undertook to estimate, not the total native population of the Americas, but merely Indian mortality within the area then subjected to Spanish control. He calculated that "more than twelve million persons, men, and women, and children, have perished unjustly and through tyranny, by the infernal deeds and tyranny of the Christians" (MacNutt 1909: 317) or in an earlier translation (Philips 1656: 5) "there have innocently perished above Twelve millions of souls, women and children being numbers in this sad and fatall list."

Las Casas asserted, in other words, that more Indians than Kroeber believed existed at the time, and nearly as many as Rosenblat estimated to exist, perished during the 40 years after 1500. Actually Las Casas felt the true 40-year mortality had been 15,000,000 Indians, more than Rosenblat thought existed, and his jurisdiction-by-jurisdiction mortality estimates totaled over 24,000,000 Indians (Rosenblat 1954: 101 n. 11). In 1560, Las Casas raised his estimate of Indian mortality to that date to 40,000,000 (Las Casas 1951: 363).

Prior to the Rosenblat-Willcox-Kroeber impact on social-science images of preconquest Indian population size, several markedly higher estimates of aboriginal New World population had been made which to some extent at least placed Las Casas' estimated Indian mortality within the realm of possibility. Rivet (1924: 661) estimated from 40,000,000 to 45,000,000 and Sapper (1924: 100) from 40,000,000 to 50,000,000, a density of about one person per square kilometer or a bit more. Spinden (1928: 660) placed the aboriginal population apogee at about 1200 A.D. with from 50,000,000 to 75,000,000 individuals. His figures implied an aboriginal peak population density from 1:2 to 1.8 persons per square kilometer. This wide range of estimates moved Steward (1949: 655) to point out that such discrepancies made evident the methodological difficulties inherent in estimating aboriginal populations and proved that methods or data were faulty. This paper attempts to

[1] This analysis originated from research carried out for the Arizona State Museum with funds from the Arizona State Parks Board. Writing was supported by the Cornell Peru Project with funds from a grant to Cornell University by the Carnegie Corporation of New York. Manuscript preparation and revision was aided by the Comparative Studies of Cultural Change under Cornell University Contract AID/csd-296 with the Agency for International Development's Office of Technical Cooperation and Research. Conclusions are the author's and do not reflect policies of any supporting agency.

TABLE 1

INITIAL INDIAN MORTALITY IN MEXICO AND CENTRAL AMERICA, ACCORDING TO FATHER BARTOLOMÉ DE LAS CASAS

JURISDICTION	ESTIMATED CASUALTIES	PERIOD	CITATIONS
Guatemala	4,000,000–5,000,000	16 years	MacNutt 1909:356 Philips 1656:51
New Spain	4,000,000	12 years	MacNutt 1909:342 Philips 1656:34.
Honduras, etc.	2,000,000	11 years	MacNutt 1909:351 Philips 1656:44
Panama	800,000	8 years	MacNutt 1909:335
Nicaragua	500,000–600,000	14 years	MacNutt 1909:341
slaves*	500,000	10 years	MacNutt 1909:340
Total	11,800,000–12,900,000		

* Sold in Panama and Peru, where they died, according to Las Casas.

point out methodological faults after comparing population estimates made for areas within the hemisphere.

AREA POPULATION ESTIMAT'

North America. Mooney essayed a c .ental aboriginal population estimation for North America by building up totals from tribe-by-tribe estimates. His calculation of total preconquest Indian population north of Mexico including Greenland was 1,153,000 (1928: 33), and it has greatly influenced subsequent estimates. Kroeber (1939: 31) reduced Mooney's total to 1,026,000 by substituting his own calculation of 133,000 Indians in California (1925: 882–83) for Merriam's (1905: 598) figure of 260,000, which Mooney had accepted. Kroeber (1939: 134) believed that the aboriginal population of North America actually fluctuated around 900,000 inhabitants, Rosenblat (1954: 102) more or less accepted Kroeber's (1939: 143) reasoning in estimating 1,000,000 persons in North America around 1492.

In order better to compare the significance of these figures, one may convert them into densities. Mooney's estimate implied an aboriginal density averaging one person in 19 square kilometers (0.0535 per km²). Kroeber's lowest estimate implied one person per 24 square kilometers (0.042 per km²). Rosenblat's estimate implied 1 individual per 22 square kilometers (0.0465 per km²).

Mexico and Central America. Kroeber (1939: 166) estimated that 3,300,000 persons lived in the civilized area of Mexico and Central America (considering Costa Rica and Panama as culturally part of South America) in preconquest times. Rosenblat (1954: 102) estimated 5,300,000 individuals inhabited that area, thus topping Kroeber by 2,000,000 even though he was concerned with refuting the "Black Legend" of Spanish responsibility for depopulating Indian America (Rosenblat 1954: 11). The originator of the legend, Las Casas, certainly claimed that many more Indians perished in this area than either Kroeber or Rosenblat estimated existed (see Table 1). Sapper (1924: 100) calculated an ecological potential of 12,000,000 to 15,000,000 inhabitants for Mexico.

South America. Kroeber (1939: 166) postulated 4,000,000 aboriginal inhabitants in all South America including Panama and Costa Rica, placing 3,000,000 of these in the Inca Empire. Thus he viewed the Mexican and Andean civilizations as on a par in population. Rosenblat (1954: 102) estimated 6,785,000 for the continent south of Panama, with 3,300,000 in Peru, Bolivia, and Ecuador. This implied an average aboriginal population density of 0.38 person per km², whereas Kroeber's figure implied an average of only 0.22 person per km². Rosenblat (1945: 92) inferred a smaller population in the Andes (4,150,000 in Columbia, Ecuador, Peru, and Bolivia) than in Mexico (4,500,000).

By way of contrast, Las Casas (Philips 1656: 97–98; MacNutt 1909: 384) estimated 4,000,000–5,000,000 Indian casualties during the first 15 years of the Spanish-licensed, German-led occupation of Venezuela, 1526–41. He estimated that 2,000,000 people (Nuix 1782: 13), captured farther east on the coast of South America and shipped to the islands of Puerto Rico and Hispaniola, died in the mines and other works (MacNutt 1909: 380). Las Casas (MacNutt 1909: 401) also accused the Spaniards of killing over 4,000,000 Indians in the Inca Empire within the first decade of conquest there, relying heavily upon the eyewitness account of Friar Marcos de Niza. This amounted to an estimated mortality of 10,000,000 to 11,000,000.

One measure of the recent dominance of the Rosenblat-Willcox-Kroeber school of thought about the density of aboriginal American population is its wide imitation. Kubler (1946: 339) also favored an estimate for 1531 of only 3,000,000 persons in the Inca Empire outside the provinces of Quito, i.e., modern Ecuador (assuming a 50% population loss in 30 years), and thought the total could have been no more than 6,000,000 (assuming a 75% loss from 1531 to 1561). Rowe (1946: 185) arrived at an estimate of 6,000,000 persons in the preconquest Inca Empire extending from north of modern Ecuador to northern Chile and Argentina. Steward (1949: 663) proceeded from this base to guess a total of 4,700,000 population in the Inca Empire: 2,330,000 in highland Peru, 1,170,000 in highland Bolivia, 500,000 in Ecuador, and 700,000 in highland Colombia outside the Chibcha area. Steward (p. 665) assumed a preconquest population of 9,000,000 in all South America. Bennett (1946: 7) contented himself with stating "it cannot be directly inferred that the number of Indians at the time of the Conquest was the same or greater than the present census figures indicate," and concluded that the Andean population at the time of conquest was likely not under "4,500,000 and not over 7,500,000" (p. 8). Baudin (1961: 24) had, on the other hand, placed the population of the Inca empire "at the time of the conquest" at between 11,000,000 and 12,000,000. Polo (1913: 53) estimated 10,000,000 to 12,000,000 at the time of the 1533 conquest. The latter figures are consistent with Sapper's (1924: 100) estimated ecological potential of 12,000,000 to 15,000,000 inhabitants in the tropical Andes.

The reasoning behind low population estimations ignored contemporary comment by Spanish imperial officials on the historic process of Indian depopulation. In 1685, for example, the Marques de Varina addressed the Spanish king on the subject. In arguing against attributing depopulation solely to mining operations (a tendency among promulgators of the "Black Legend") he pointed to the complete extinction of the Caribbean Island natives and those of valleys of Peru and nearby areas "where there are no mines and more than eight million Indians have perished" (Fernandez 1949: 36). The contrast between the views of modern social scientists and of a colonial authority whose active career spanned a score of years about the time that American Indian population probably began to recover is very marked.

It can be said with justice that the Marques de Varina may have inflated his mortality figures to try to frighten the King of Spain into action. To make such an assumption does not entitle the analyst to discard those figures; it does require him to seek corroborative or negating evidence. Rosenblat, Kroeber and their imitators have not done so. Their characteristic methodology has included depreciation of all historical population figures. They deprecate the departure of historical witnesses from the "truth" for motives they intuitively impute, but which uniformly led said witnesses to overestimate, in their opinion, aboriginal populations. They ignore the fact that eyewitnesses, whatever their biases, at least observed population trends which the modern analyst can never witness. Thus Rosenblat (1945: 185) claimed, "The old estimates are always hyperbolical," without offering sound evidence to support such a claim. As Sauer (1935: 1) pointed out: "Modern students commonly have been inclined to discount early opinions of native numbers, but rarely specified their reasons for doing so." Although Kroeber (1939: 177) characterized Sauer's work as "another of his revolutionizing studies," he remained convinced by his own preconceptions. Although in 1936, under the impact of Sauer's and Meigs's (1935) analyses, he made explicit some of his previously implicit assumptions, Kroeber (1939: 180) remained antidocumentarian:

> The vast majority of figures by contemporaries are too large. This fact will be generally admitted. The problem is to know when the exaggeration is slight and when it is unreasonable. In general, documentarians tend to cling to the more moderate figures given in the records, ethnologists to distrust them generically. Where Sauer shaves sixteenth- and seventeenth-century statements, I am likely to reject most of them outright.

Certainly Kroeber's description of documentarians applied to many historians. Hammond (1927: 122 n. 463), for example, thought New Mexico colonizer Juan de Oñate "probably exaggerated" when he gave 3,000 as the population of Acoma Pueblo in 1598. Oñate and others "exaggerated the numbers in order to convince the king of the importance of the province," according to Hammond. Yet the Spaniards slew either 600 or 800 Acomas and captured 570 or 580 in a three-day battle (Hammond 1927: 120-21) that involved only part of an anti-Spanish faction—as Indians who had been out of the pueblo cultivating their fields during the conflicts later testified at the Spanish trial of Acoma captives held in Santo Domingo Pueblo (p. 122)—and two pacifist leaders had deserted the pueblo with their followers some time before (p. 113).

Steward (1949: 655–56) sought to state explicitly the methodological assumptions he and other anthropological analysts of like persuasion have employed. His brief summary of various estimation methods failed, however, to do more than describe them from his point of view, which falls within the range of methodology of the skeptical group. He did not evaluate the results of the several methods of estimation employed, nor spell out criteria for data evaluation except in applying some procedural propositions of Kroeber's formulation. It seems appropriate, therefore, to review some of the principal estimation methods that have been employed by various investigators and the relation of these methods to data and their accuracy.

PROJECTION METHODS

Some form of projection of population figures for small, more intensively studied areas to larger, less well-studied areas of the New World is the method by which a number of Americanists have estimated aboriginal population.

Hemispheric Projections. Taking one at least approximately known set of data, the contemporary Indian population, Rivet attempted to project back through time to an aboriginal population estimate using a factor derived from one well-studied area. Rivet's (1924: 601) basic population figure was about 15,000,000 living Indians in the Americas. His projection factor was derived from North America, with 403,000 living Indians as compared to an estimated 1,148,000 at discovery (p. 600). Assuming a constant ratio of depopulation in the hemisphere, Rivet estimated a maximum of 40,000,000 to 45,000,000 aboriginal inhabitants, a density of 1 person per square kilometer over the entire area (pp. 601–2). In 1931 Kroeber (1939: 160) asserted that we "may not infer from present-day large populations to native large ones." In support of this methodological assertion, Kroeber posited a straight-line increase of population throughout historic times by projecting backward in time the apparent rate of increase implied by Humboldt's figures for 1793-New Spain and the Mexican census of 1930. "If it could be assumed that the increase could be continuously projected backward, we should be starting with little more than 300,000 souls in 1500 A.D.—and that in all New Spain . . ." So although he cited Rivet's estimates, Kroeber (p. 165) had already rejected his method.

Under the impact of Sauer's analysis of documents, Kroeber (1939: 180) was willing by 1936 to grant some methodological validity to this projection procedure. "Modern population is some index of original native population, at least under Latin-American rural conditions. Sauer uses this principle." Kroeber displayed his continued aversion to historical evidence by immediately shifting the argument to a discussion of the concept of "subsistence potentialities." (As a matter of fact, Sauer had not "used" a principle of equivalence of modern rural and preconquest aboriginal populations; he merely commented [1935: 32] on their apparent coincidence.)

The fact that enumerated "Indian" population is currently increasing implies that it has done so for some time. Kroeber's straight-line projection of this population trend all the way back to preconquest times took cognizance of this late historic trend. It failed to take into account, however, unassailable early historic evidence that Indian population in fact fell from its prehistoric level to an absolute historic minimum, a nadir from which it is still recovering. Kroeber's choice of 1793 population figures for New Spain was undoubtedly dictated in large part by their availability, but that date unfortunately falls very close to Jenner's perfection of vaccination against smallpox. Vaccination has been, in all probability, the major single factor in New World native population recovery since 1800, so any backward projection of post-1800 population trends which assumes that pre-1800 and post-1800 disease environments were not seriously different falls into fatal error.

Kroeber's objections aside, the method employed by Rivet actually was subject to several limitations. The precision of enumeration of living Indians clearly affects the final estimate obtained by such extrapolation. Racial fusion to produce "a new type, the so-called mestizo," is a major population characteristic of Latin America (Cook 1946: 81) and the difference between the Indian population figures reported by the U.S. Bureau of the Census and by the Bureau of Indian Affairs reflects the difficulty of even defining "Indian" as a result of interbreeding. So the projection of historic "Indian" population figures is always subject to the error of reasoning from a racially mixed historic population to a pure Indian prehistoric population.

Of equal methodological importance, population

is not the static quantity such projection implicitly assumes. As censuses subsequent to 1924 have shown, enumerated "Indian" population continues to rise, so that the estimate of aboriginal populace obtained in this manner depends entirely upon which census returns are employed. This defect is well illustrated by the difference between Rivet's figure of 15,000,000 contemporary Indians and Spinden's (1928: 660) figure of 25,000,000, published only four years apart.

An additional defect in Rivet's projection procedure was his dependence upon an estimation of prehistoric North American population in order to derive the depopulation ratio he employed. The hemispheric population estimate obtained by this method could be no more exact than the estimate of aboriginal North American population which yielded the factor used to project the aboriginal population of the rest of the hemisphere. Rivet employed Kroeber's North American estimate, whose merits will be examined below. Still, obtaining a ratio from data however faulty represented an attempt at working out an empirical method not even essayed by Spinden, who simply assumed Indian population about 1200 A.D. to have been two to three times greater than contemporary Indian population.

United States Projections. On a less ambitious scale, MacLeod attempted to estimate the aboriginal population of the U.S. by projecting estimated aboriginal population densities of studied areas over a known geographic area. Thus as Willcox (1931: 49) remarked of a different analysis, "in starting with an assumed density of population he used a better method" than mere compilation. MacLeod assumed that 2,000,000 square miles of the U.S. were fit for aboriginal Indian habitation. He accepted Kroeber's figure of about 130,000 Indians in California in precontact times, which yielded a density of one person per square mile (MacLeod 1928 : 15), and reasoned that if the rest of the country were as densely settled as California, there were 2,000,000 Indians there in aboriginal times.

MacLeod checked his estimate by computing aboriginal population density in tidewater Virginia from early English descriptions. Multiplying warrior totals by a factor of three, which he considered conservative since it implied a population barely reproducing itself, MacLeod (pp. 545–46) found a density of two Indians per square mile in the area most familiar to English colonists. Willcox (1931: 53) accepted estimation of the population of India from army size, but surprisingly preferred (p. 55) Mooney's lower figure to MacLeod's higher one for Virginia, on grounds that Mooney presented "fuller evidence." Mooney (1928: 6) presented a whole page of tabular population figures for the South Atlantic states, to be sure, including all of six unsupported lines on Virginia Indians. MacLeod (1928: 545) meticulously marshalled his sources and explicitly stated his hypotheses, so his conclusions would seem much to be preferred on procedural grounds. His estimated tidewater Virginia population density suggested to MacLeod (1928: 16) that the national estimate based on Kroeber's California figure was low, taking into consideration the fertility of the St. Lawrence

River Valley, the Saskatchewan lakes and the north Pacific Coast fisheries. So he estimated "a pre-European Indian population of roughly three million" in the United States, not the 4,000,000 a literal interpretation of his own figures implied.

MacLeod implicitly assumed the Virginia Indian population observed by Englishmen to have been aboriginal in density, an assumption that may be questioned in view of the early prevalence of epidemic diseases. Then he assumed that the density of Indian population in Virginia was appreciably greater than the average over the U.S., which agreed with Kroeber's (1939: 166–71) theory of greater population density along salt-water shore lines.

Later investigation has indicated that MacLeod erred on the side of caution. Aschman (1959: 178, Table 7) recently estimated that aboriginal population density averaged 1.12 persons per square mile in the central desert of Lower California. This is one of the world's areas least hospitable to human habitaton (Aschman 1959: 5–27). Such a density of aboriginal populace in such an adverse environment certainly suggests that the Upper Californian density should have been considerably greater than Kroeber thought, if population pressure were equally adjusted to recources available in the two Californias. This implies that Kroeber's estimate of one Indian per square mile is low, and that MacLeod was overly conservative in not applying his tidewater Virginia population density of two Indians per square mile nationwide instead of compromising on 1.5 persons per square mile, a figure halfway between his and Kroeber's density estimations for the opposite shores of the continent.

Cook employed a similar projection method for estimating aboriginal population of Lower California. He (1937: 7) accepted Meigs's calculations of an average of 1.15 persons per square mile in the northern part of the peninsula, raising the figure slightly to 1.3 ± 0.1 to allow for slightly greater density. Projecting this average density over 29,100 square miles of the peninsula provided Cook (p. 8) with an estimate of upwards of 38,000 persons in aboriginal times. This implied a total peninsular aboriginal population on the order of 48,000 individuals.

In view of Aschman's more recent investigations, that figure should be regarded as a minimal estimate. If Aschman's conservative central desert figure, 1.12 people per square mile, is projected over the 42,200 square miles of all Lower California (Cook 1937: 7), one finds that the area held at least 47,264 aboriginals, even if the more favorable northern and southern environments were no more densely populated than the central desert. Since they undoubtedly were more heavily populated, the aboriginal peninsular population must have exceeded 50,000 by a wide margin.

California Projections. It is clear from the Rivet and MacLeod uses of the projection method that the aboriginal population of North America has become a key figure in hemispheric estimation, and that the aboriginal population of California occupies a key position within that larger total. The fact that Kroeber's estimate of California aboriginal population was obtained by revising downward Merriam's earlier figure, which yielded a density estimate equal to Aschman's for the central desert of Lower California, calls for a re-examination of Kroeber's and Merriam's methods.

Merriam anticipated MacLeod in projecting the population density of a small, documented area over a larger geographic region. His base line in time was 1834, when upward of 30,000 converted Indians were reported in California (Merriam 1905: 597). This datum Merriam projected outward through space and backward through time. He inferred that there was one unconverted Indian in the area the missions tapped for every three converts (p. 598), raising the 1834 population for the immediate missionized area to 40,000. He took depopulation into account by allowing for a loss of 10,000 due to Spanish contact, bringing his estimate of precontact population to 50,000. Then (p. 595) he calculated the area tapped by Spanish missions to have been 1/5 of the non-desert part of the state, inferred an equal aboriginal population density, and estimated 250,000 Indians. Adding 10,000 persons in the desert areas, he arrived at 260,000 Indians in aboriginal California (p. 598).

Kroeber (1925: 880) considered some of Merriam's assumptions quite conservative. He thought that Merriam's proportion of 1834 to 1769 population was too low "in view of the enormous mortality at some of the missions" (pp. 880–81). He questioned Merriam's base line population figure, preferring "the exact figure of 24,634 for 1830" (p. 881). He also defined the missionized area as nearly 1/3 of the state instead of 1/5.

Starting from an 1830 population rounded off at 25,000 mission Indians, and retaining the proportion of 1/3 this number of unconverted natives, Kroeber calculated a population of 33,000 for the mission area. He allowed 1/2 that figure for decrease since 1769, estimating 50,000 in the missionized area as of 1770. Projecting this figure across the state yielded him a total estimate of 150,000 individuals. Using a different method (1925: 882–84), he arrived at a total of only 133,000. Kroeber (1939: 143, Table 9) calculated a population density averaging 0.433 person per square kilometer during aboriginal times in California. This density varied from 0–5 persons per square kilometer in the desert areas to over 70 individuals per square kilometer in favored parts of the Central Valley and Pacific Coast north of San Francisco Bay (p. 154: Map 19).

Yucatan Projections. The population density estimates for late aboriginal times arrived at by Kroeber, MacLeod, Cook, Meigs, and Aschman for different parts of North America make appropriate a discussion now of a density estimate of a very different order for the peak population of the Old Maya period in Yucatan. Ricketson (1937: 16) arrived at an estimate of 270.8 persons per square mile by applying a series of explicit assumptions to hard-won archaeological survey data. Ricketson and his associates counted prehistoric house-mounds within a defined surface sample area at Uaxactun, then multiplied the number discovered by a factor of five persons per house-mound. They derived this factor from contemporary studies of household size in Yucatecan settlements. Ricketson arrived at his 270.8 persons per square mile estimate by explicitly assuming that only 1/4 of the house-mounds encountered were occupied simultaneously. Assuming that all recorded house-mounds had been occupied at the same time would have implied a peak population density of 1,083.3 persons per square mile of habitable land—a clearly illogical situation.

While the problem to which Ricketson addressed himself in projecting the estimate of population density at Uaxactun to the Yucatan Peninsula was the scale of population in Old Mayan times considerably prior to the epoch of peak hemispheric aboriginal population, his methodology merits mention. Having found only about ½ the area surveyed at Uaxactun to be habitable and to bear evidence of prehistoric habitation, and having found some corroboration for this local ratio in trail characteristics elsewhere, Ricketson explicitly assumed that only ½ of the surface area of Yucatan had been habitable. Employing his lower estimate of density, he arrived at an approximation of 13,300,000 individuals as the peak Old Mayan population.

In his synthesis of Mayan archaeohistory, Morley (1947: 316) followed Ricketson, repeating his "estimates ranging from a minimum of 13,300,000 to a maximum of 53,300,000." The latter figure assumed simultaneous habitation of all house-mounds and was clearly unrealistic. In revising Morley's text, Brainerd (1956: 262) explicitly reduced the proportion of house-mounds occupied at any one time to one in every eight recorded, thus arriving at an estimated density of 136 persons per square mile. He felt that this figure was still higher than reality, arguing that the "normal Maya homestead" includes two houses or more. Brainerd implied, then, a density of from 64 down to 45 individuals per square mile, or a total Yucatecan population on the order of 2,216,666–3,325,000. Elsewhere Brainerd (1956: 47) claimed that there is no evidence for a heavy lowland Mayan population at any time, and suggested the 30 persons per square mile modern Yucatecan density as "a likely maximum."

This guess by Brainerd clearly conflicts with the carefully worked out estimates based on actual archaeological survey data. Yet, whether the "true" figure was 30, 45, or 64 persons per square mile, the Mayan data from the lowland rain forest area of the Yucatan Peninsula indicate one of the peak densities of New World populations. These densities contrast starkly with Kroeber's (1939: 159) average density of only three persons per square kilometer in Mexico and Central America. They suggest that Kroeber was indeed correct when he wrote (p. 160) that: "The actual population in 1500 A.D. may have been more" than his 3,000,000 estimate.

"DEAD RECKONING" METHOD

In undertaking a systematic examination of the question of the size of aboriginal American population north of Mexico, Mooney (1928: 1) planned to divide the continent into a number of "natural sections," and then to "discuss the population of each in turn, first generally and then tribally." This plan produced tabular listings of tribal populations added together into regional and continental totals, in what Kroeber (1939: 131) labeled the "deadreckoning" method.

When the latter reconsidered the question of aboriginal Californian population, he (1925: 882) estimated each group separately by this method, using:

> early estimates of travelers and settlers; the conclusions of ethnologists familiar with the people at a later time; the number of known villages or village sites; the tribal count in the Federal census of 1910 . . . apparent rapidity of decrease in various areas; the availability of food supply in each habitat, and indications of the ratio of density of population in adjacent areas of differing surface environment.

Using this procedure, he arrived (p. 883) at an estimate of 133,000 Indians living in California in 1770.

Steward (1949: 656) has summarized one of Kroeber's principal working assumptions in these words: "most contemporary estimates, particularly by the early Spanish missionaries and administrators, were too high." He then went on to concur: "Kroeber's suspicion of such estimates is certainly justifiable in the case of soldiers, who obviously exaggerated the number of their enemies. Missionary guesses of independent tribes were also liable to exaggeration" (p. 657). Steward meant, presumably, that Spanish soldiers magnified the number of their enemies through fear, desire for reinforcements, or hopes for glory, and missionaries through desires for increased financial support for conversion activities.

Kroeber's second assumption was that "a competent ethnologist may correct such estimates for an area he knows well" (Steward 1949: 656). Admitting that the assumption might not be true, Kroeber (1939: 180) claimed "it is evidently made by American anthropologists who have concerned themselves with the subject." One of the inherent pitfalls in such an assumption is that the ethnologist attempting a regional or continental synthesis cannot possibly judge data on other groups as accurately as those for natives among whom he has personally carried out investigations. Nicholson (1962: 880) recently called attention to the failing of "viewing the high cultures of Mesoamerica through North American ethnography-tinted glasses" as contributing to "Kroeber's gross underestimate of the contact population of Mesoamerica."

The probable accuracy of corrections in an area he knows well by an ethnologist of Kroeber's unquestionable field experience may be determined by a recheck of his estimate of the number of California Indians in immediate pre-Spanish times, clearly the key case in current estimations of aboriginal American population. Such a recheck has been made and will be discussed below in considering its methodology.

There remain yet other considerations with regard to the "dead reckoning" method. One shortcoming in its previous application to population estimates is the matter of consistency of societal scale. Kroeber (1939: 131) claimed merit for his California population estimate because it followed Mooney's society-by-society reckoning technique. Navigation by dead reckoning involves, however, constant placement of one object in spatial relationship to others. So Kroeber's choice of methodological labels may be questioned, since both he and Mooney ignored available population landmarks; e.g., they apparently did not cross-check their estimates of the population of inimical tribes to discover whether or not they were logically consistent. The consequences of failing to compare population estimates for one group with those for its enemies may be pointed out briefly using the League of the Iroquois as an example.

A very interesting picture of the efficiency of Five Nations warriors can be built up by some real dead reckoning. Mooney (1928: 4) estimated and Kroeber (1939: 140) accepted a population of 5,500 for the Confederacy, although Kroeber (p. 133) did admit that "the Iroquois proper are put disproportionately low, perhaps under the influence of Hewitt, who seems to have been impressed by the humble beginnings of the great confederacy." Adding up their estimated populations of tribes at war with the Five Nations, one finds the Conestogas estimated at 5,000; Eries, 4,000; Neutral, 10,000; Huron and Tionontati, 18,000; Abnaki, 3,800; Mahican, 3,000; Delaware, 8,000; Western Shawnee, 2,000; Catawba and related tribes, 17,500; Algonkin and Ottawa, 7,300; Montagnais, including Naskapi and others, 5,000; and Cherokees, 22,000 (Kroeber 1939: 141)—a total of 106,100. If one then adds up the populations of tribes which one or another of the Five Nations had reduced to subject, tribute-paying status, one finds the Montauk estimated at 6,000; Massachusetts, 13,600; Pennacook, 2,000; Wappinger, 5,600; Nanticoke, 2,000; and Conoy, 2,700 (Kroeber 1939: 140)—a total of another 31,900 individuals. This adds up to 138,000 Indians the Five Nations had to control or fight. To be sure, the comparative efficiency of Five Nations warriors cannot be gainsaid. Father Jean Pierron said that a Mohawk victory over the Mohicans in 1669 "was more glorious than profitable because they [Mohawks] are very few in numbers, compared with their enemies, who can bring against them fifty men to their one" (Thwaites 1899: 155). Even so, either 50 or 25 to one seems rather unlikely odds in Indian geopolitics and throws some doubt upon Kroeber's claim that he and Mooney employed dead reckoning. The Mooney-Kroeber figure of 5,500 for the Five Nations is clearly a low estimate. Perhaps 10 times that number would be a more reasonable approximation. Since the tribal units formulated by Kroeber represent a considerable range of historic time, and do not necessarily reflect aboriginal times at all, this little exercise in dead reckoning is offered merely to indicate some consequences of neglecting to carry out this kind of cross-checking.

Another shortcoming of the "dead reckoning" procedure as hitherto employed has been lack of rigorous definition of "aboriginal conditions." This has resulted in presentation of historic Indian populations as "aboriginal" which were in fact surviving remnants long after intensive contact with Europeans had begun. Thus Mooney (1928: 4, 6) took the date 1600 as "aboriginal times" in the U.S. North and South Atlantic States, and in eastern Canada (p. 24). He took 1650 for the Gulf and Central states (pp. 8–9, 11), and 1670 for Central Canada (p. 26). Then he retreated to 1690 for the Southern (p. 13) but 1780 for the Northern Plains and Northwest Coast (pp. 15–18) including British Columbia (Mooney 1928: 27-30). To Mooney aboriginal times meant 1740 for Alaskan natives (pp. 31–32) but 1769 for Californians (p. 19). He accepted a date of 1845 for the central mountain region (p. 22), but 1680 for the tribes of New Mexico and Arizona. He dated aboriginal times among Greenland Eskimos at 1721 (p. 23).

It may be argued that differential dating of "aboriginal" population is in fact necessary because the biological and social environment of American natives shifted from conditions favoring population growth to deleterious ones at different times. Even so, the particular dates taken by Mooney to represent aboriginal population conditions are in many cases much too late. A case in point is that of the Northern Plains Indians. In his analysis of Indian population trends there, Wissler (1936: 36) found that: "The history of the areas studied reveals contact direct, and through intermediaries, as early as 1670;" Mooney's figures, from 1780, are 110 years later.

Another pertinent example is that of the Pueblo Indians of northern New Spain. Mooney (1928: 22) estimated their population as "aboriginal" in 1680. Yet that was the very year those Indians rose in revolt against their Spanish overlords and drove the latter south out of the present area of Arizona and New Mexico. The Pueblo Revolt of 1680 was quite clearly a native reaction against directed cultural change. It reacted against Spanish conquest and colonization of Pueblo lands that began in 1598 (Hammond 1927: 94–104, 112–23) and by 1601 had reduced the Indians, upon whose food production the Spaniards subsisted, to "eating branches of trees, earth, charcoal and ashes" so that many starved to death (p. 145). Spanish colonization of the Pueblo area created a regularly-traveled trade route to central Mexico along which disease agents undoubtedly moved with people and goods, so that aboriginal biological conditions must have ended among the northern Puebloan peoples soon after 1598 if not before.

A comparison of Mooney's 1680 population estimates with some others for the same population shows that his "aboriginal" Pueblo estimates were much lower than reality and much later than pre-European times. Nearly half a century before the Pueblo Revolt, a Franciscan administrator had estimated a Pueblo population of over 58,500 (Hodge, Hammond, and Rey 1945: 62, 64–65, 67–69, 71–73, 75) without counting two provinces. Earlier Spanish accounts generally suggest even larger population figures. An early U.S. trader in New Mexico, Josiah Gregg (1844: 270; Thwaites 1905: 56–57; Moorhead 1954: 188) formed an opinion of Pueblo population trends consistent with Spanish documents when he guessed that the Pueblo populace which he estimated at 9,000–10,000, had been 10 times as numerous at the time of conquest.

By way of contrast, Willcox (1931: 55) implicitly approved the aboriginal population cutting tendency by citing Kidder as having "lowered Mooney's estimate of the original pueblo population of Arizona and New Mexico more than two-fifths." As a matter of fact, Kidder (1924: 39) published his guess, unsupported by direct documentation, four years before Mooney's post-humous work. Kidder's "Spanish conquest" could mean 1540, 1598, or 1692, so his time referent was no more specific than Gregg's. He cut 5,000 off Bandelier's (1890: 121–36) estimation of Pueblo population at about 25,000 at the time of conquest. Spicer (1962: 14) recently concluded that the Puebloans numbered no more than 40,000 about 1600 A.D.

Other investigators have utilized documentary sources in order to estimate aboriginal population size in quite different ways. These investigators may readily be distinguished from the "skeptical" group by their empirical approach to documentation and their relatively imaginative procedures for determining the reliability of historic documents rather than assuming these to be systematically erroneous on the side of exaggeration of population.

These investigators have long since met one of the two conditions Kroeber (1939: 131) set forth for revising his figures. "I shall cheerfully admit a larger population for native America," he concluded, when there should be "convincing studies of specific districts in Latin America which, with maintenance of a reasonable balance within the whole of Latin America, compel a total there more than seven or eight times as great as Mooney's," Cook, Simpson, and Borah have demonstrated fairly conclusively from documentary analysis an aboriginal central Mexican population alone more than 10 times Mooney's estimate for North America. Rosenblat has displayed none of Kroeber's openness to proof. The rigidity of his thinking appears in the consistency of his hemispheric population estimate in two revisions of his original work despite new evidence and interpretations. Rosenblat's bibliography expands, but his estimate remains static.

Cross-Checking Source Consistency

A generally accepted method of checking the reliability if not the validity of cultural data collected by interviewing informants is cross-checking what one says about a phenomenon against what another says about it. This is perhaps the simplest empirical method of source evaluation, firmly embedded in Anglo-American court procedures and Civil Service Commission background checks as well as ethnological field investigation. It may be applied equally well to statements preserved in historical documents and contemporary respondents' statements.

Cook (1956) has provided a model of cross-checking method by directly comparing data collected from different informants by different ethnographers. Commenting upon one ethnographer's criticism of a local county history, Cook (1956: 111) implied criteria for evaluating the accuracy of sources: "If we are going to discredit the testimony of the chief concerning his own village thirty years previously, we had better throw out along with it the information secured from septuagenarians who have to recount at second hand what their forefathers told them."

Cook consistently estimated greater populations for north coast California Indians than had Kroeber. Cook (1956: 84) found that Kroeber's decision that there were two Yurok houses for every three housesites was in error, and by comparing village and house counts compiled by Kroeber (1925), Waterman (1920), and Merriam, (1905) estimated 3,100 Yurok where Kroeber set 2,500 as absolute maximum. Comparing Kroeber (1925: 116), Loud (1918) and Merriam, Cook (1956: 93–97) estimated 3,300 Wiyot in contrast to Kroeber's 1,000. Cook (pp. 99–100) doubled Kroeber's (1925: 130) estimate of aboriginal Hupa population by comparing his facts with those presented by Goddard (1903), Merriam, and an 1852 map. Cook (p. 101) more than quintupled Kroeber's estimated Tolowa population as of 1850, utilizing data from Waterman (1925) and Drucker (1937); he doubled (p. 120) Kroeber's (1925: 275) Coast Miwok population estimate by comparing his information with that furnished by Barrett (1908) and Catholic mission registers.

It is not necessary to summarize all Cook's re-estimates to point out that he ably and amply demonstrated by cross-checking what different informants remembered and told ethnologists about different houses and villages. The implications for demographic methodology are clear. Cook concluded (p. 81) that information obtained from native respondents possesses only limited demographic utility. He emphasized the psychology of human memory, which retains qualitative facts such as village names and locations better than quantitative facts such as the number of inhabitants in a settlement or the number of villages. "This failure to retain and transmit accurate knowledge of number or mensuration becomes intensified," he commented (pp. 81–82), "if the informant is required to reach across an intervening period of unrest and confusion, both physical and mental, to an era of stability long since vanished." Another factor making for variation between informants is that a given individual cannot be familiar with the entire content of his culture.

The methodological assumptions made by "skeptical" analysts excused them from engaging in such a laboriously scientific method as systematic cross-checking of sources. Kroeber's sweeping imputation of early estimates by missionaries, administrators, and soldiers, sanctioned by Steward, precluded even cross-checking statements of one type of European against those of another.

Estimates of aboriginal American population recorded by literate Europeans on the scene during historic times can in fact generally be cross-checked against other similar estimates, or estimates derived from other types of documents by the analyst.

Northwestern Mexico. Sauer (1935: 2) employed the latter type of cross-checking technique for arriving at estimations of aboriginal population in the northwestern part of modern Mexico. Since the documents available seldom recorded total populations, he converted "warriors, families, baptisms or other items" into approximations of total populations. In converting number of families into total population, Sauer used a factor of six, one person less than the average contemporary family size in the same Mexican region. In converting baptisms into total population in completely Christianized areas, Sauer explicitly assumed a birth rate of 40 per thousand, following contemporary Pueblo and past rural Scandinavian rates. Missionary records also frequently yield figures on baptisms performed on small children of the "age of innocence" when parents proffered their offspring for the rite. Sauer (1935: 3) found this age span to have been "four years of age and younger" and determined that contemporary age distribution placed 1/10 of the total population in that age range. He also assumed explicitly that children under doctrinal instruction were aged 5 to 12 and constituted about 1/5 of the total population. Inasmuch as historical data were available for "small and discontinuous areas" Sauer necessarily also indulged in many interpolations, aware of the hazards in such a projective procedure. As a result, he arrived at a series of population estimates "in apparent disagreement with the opinion of American anthropologists"—particularly Mooney and Kroeber.

Sauer's (1935: 5, Table 1) results are worth comparing directly with Kroeber's in order to indicate the order of difference obtained by their different methods. Sauer calculated a range in aboriginal population density from 10 persons per km² among the southern Totorame to 0.2 persons per km² on the Gulf of California coast of Sonora. Kroeber (1939: 136, Table 7; 158, Table 11) calculated a density of 0.28 persons per km² for the entire "Southwest" cultural area in Mexico. Thus, Kroeber's over-all estimated density was only slightly greater than the density that Sauer calculated for the most sparsely settled tribe in the area.

Such a difference in estimates of population density reflects, of course, very different estimates of the total population in the region. Sauer estimated a total of 540,000 individuals, at 2.6 persons per km² or nearly 10 times that computed by Kroeber from his estimated total of only 100,000.

Direct tribe-by-tribe comparisons between the Sauer and Kroeber estimates are possible only for the Northern Pimans. Sauer (1935: 5, Table I) estimated 30,000 individuals, an aboriginal density of 0.3 persons per km². Kroeber (1939: 136, Table 7) estimated 6,600 Papagos with a density of 0.0924 persons per km² and 4,000 Gila Pimas with 0.266 persons per km², a total of 10,600. The main difference between Kroeber's and Sauer's figures is one of comprehensiveness, Sauer having estimated the entire Northern Piman population and Kroeber only two parts of it. Sauer (1935: 30–32) calculated the following numbers for the Northern Piman subgroups: Sonoran River headwaters 1,000; Himeri 4,000; Altar River drainage 2,000; Desert south of the Altar 4,000; Tumacacori area 1,000; San Pedro River Valley 2,500; Middle Santa Cruz 4,500; Gila River 1,000—a total of 20,000 riverine Northern Pimans plus a total of perhaps 10,000 Papagos.

Surprisingly, Sauer estimated only 1,000 Gila River Pimas where Kroeber estimated 4,000. For this subgroup a later independent estimate is available. Analyzing documents from roughly the same period as Sauer (including the same sources used by Sauer but additional ones as well), Ezell (1961: 17) concluded that "even a figure of 3,000 would be too low an estimate for the Gila Pima population of pre-Columbian times." Ezell reasoned that the aboriginal population was greater than the 3,000 reported because Spanish travelers who recorded village populations failed to visit all extant settlements and also did not see possibly some or even most of the women and children in settlements they did reach, and because epidemic disease had taken some toll prior to 1700, when the documentary record began. Neither Sauer nor Ezell nor I could go back in time earlier than extant documentation in this area, which lagged behind the spread of Old World disease agents which almost certainly reduced the aboriginal populace even before literate explorers arrived on the scene.

Combining Sauer's "Middle Santa Cruz" and his "Tumacacori area" yields a total of 5,500 Indians. I have estimated 3,100 persons in the lower valley in 1700 (Dobyns 1962: 27) and 2,400 in the middle valley then (1963*a*: 181), recognizing that the 1700 population was no longer aboriginal nor full strength.

Thus such independent studies as have been carried out suggest that Sauer's estimate of Northern Piman population was conservative even for the period of initial contact with Spaniards (disregarding the isolated early explorations of Vazquez de Coronado and the friar Marcos), and that Northern Pimans had earlier been more numerous. As Kroeber (1939: 178) remarked of Sauer's study, "if he is right all our figures for the American Southwest must be far too low." Sauer's entire estimation procedure for northwestern Mexico may be taken to have yielded conservative figures. If that be the case, then Kroeber's (1939: 159) forecast that "it may in the end be proved that Mooney and I have throughout cut figures too low" has been borne out.

Lower California. Cook (1937: 7) subjected a colonial estimate by Baegert of 40,000 to 50,000 persons in Lower California in pre-Spanish times to the same sort of cross-checking. Baegert was a Catholic missionary who had worked on the peninsula half a century after settlement began (Cook 1937: 2), so his estimate was necessarily itself a backward projection of first-hand data obtained by direct observation under rapidly altering conditions. The Mexican historian Lemoine (1959: 251) followed Clavigero in accepting Baegert's estimate but apportioning to the Pericus 1/4 of the total. He asserted, however (p. 250), that one cannot state precisely the number of inhabitants of the peninsula in the years immediately prior to the arrival of the Spaniards.

In attempting to cross-check Baegert's estimate against other types of sources, Cook (1937: 14) utilized three other classes of data: (1) projection of population densities worked out for restricted areas from historic documents, (2) projection of smaller population components extrapolated from baptismal records, and (3) projection back through time of maximum enumerated population, using a constant factor.

Cook borrowed (p. 7) the population density figure of 1.15 persons per square mile that Meigs (1935) had worked out for northern Lower California, increasing it to 1.3 ± 0.1 persons per square mile to allow for a "slightly denser population in the extreme south" and a greater ratio of coast line to total land area south of the region Meigs analyzed. Substracting Meig's area from the peninsular total, Cook (p. 8) then projected his density figure over the remaining area to obtain an estimate of 33,000 persons. Making the same areal reduction in Baegert's estimate reduces it to 35,000.

In working out a procedure for deriving estimates of aboriginal Indian population "in fully Christianized missions," Cook (p. 6) used a constant factor to convert number of children baptized into a total population approximation. Sauer (1935: 3) had employed the factor 10 in northwestern Mexico, where missionaries baptized children aged four and under very soon after contacting a pagan population. He assumed, in other words, that children aged four and under constituted 10% of the total population. Cook (1937: 9) followed Sauer's reasoning, but because missionaries in Lower California tended to baptize children up to age 10, he explicitly assumed that children up to that age constituted 1/5 of the total native population.

Analyzing reports of numbers of children under 10 baptized at several missions whose area of native occupancy he calculated, Cook (1937: 8–12) then projected his estimated total population to the remaining area for which he had no baptismal records available. Thus he calculated (p. 12) 45,100 ± 9,000 native inhabitants in what later became Jesuit mission country in Lower California. This would have meant a population density of 1.56 ± 0.31 persons per square mile.

In order to obtain yet another cross-check on Baegert's historical estimate, Cook employed an additive or standard factor increase method originated by Meigs to derive an estimate of aboriginal population from still another set of historical data. This method employed the maximum reported enumerated population in Christian missions as the base datum, and explicitly assumed that the aboriginal population was in every case 2½ times as large as the maximum number of people at any time following conversion. This procedure provided Cook (p. 13) with an estimate of 48,000 aboriginal Lower Californians in the Jesuit mission area. This method obscured the actual scale of depopulation between aboriginal and Christian times by assuming that it was a constant and uniform ratio of population loss over the entire area.

Finally Cook (1937: 14) averaged the four estimates of aboriginal Lower California population derived from different sets of data, finding that the extreme values deviated by 15% from 41,500 individuals. Inasmuch as Cook relied entirely on documentary sources, his estimate must be taken as conservative, since his data could not escape the temporal limitations of conversion-period documentation, as in the Northern Piman case.

Central Mexico. The consequences of the "skeptical" and ethnohistorical methods may be directly compared in the case of estimates of the aboriginal population of the central Mexican civilization. Rosenblat (1954: 97 n. 1) followed Humboldt in deprecating Franciscan estimates of the number of Indians baptized immediately after the conquest on the grounds that these were inflated through a desire to aggrandize the evangelical labor of the order. "All parties were equally interested in exaggerating the flourishing state of the recently discovered nations." In other words, Humboldt imputed the reliability of priests' baptismal figures by charging them with ambition: "the Franciscan fathers vainglorified themselves for having baptized more than 6,000,000 Indians from 1524 to 1540."

Cook and Simpson (1948: 19), on the other hand, rejected the idea that priests could be uniformly charged with willful desire to magnify their exploits or with slipshod bookkeeping. From clerical sources, then, they (1948: 20–22) calculated a preconquest population of 9,030,000 in Central Mexico, by projecting the Franciscan figures back in time to 1519 and outward in space to the civilized area. Their projection only slightly exceeds the figure that one of the 12 pioneer Franciscan friars in New Spain, Toribio de Benavente, or Motolinia, opined had been baptized from 1521 to 1536—more than 9,000,000 Indians (Steck 1951: 183).

Addressing the Spanish King in 1609, Pedro Fernandez de Quiros (Torres 1866: 507) asserted the initial population of the New World was approximately 30,000,000. He used as his base figure a total of 16,000,000 Indian baptisms he said he saw recorded in a Franciscan convent at Xochimilco (p. 508). Quiros reasoned that the Indians baptized by priests of other orders, those not baptized, and the population of the Caribbean Islands would raise the total Indian population to 60,000,000. Although Rosenblat (1954: 97 n. 1) cited the Fernandez figure as inflated, he did not criticize Fernandez's methodology, which was in fact defective. Fernandez did not specify the time period during which the Franciscans baptized 16,000,000 Indians, so his base cipher is highly suspect.

There is, on the other hand, internal evidence in Motolinia's own account which suggests that he, and probably other priests, underestimated the number of baptisms actually performed during the first 15 years of mass conversion following Spanish conquest in New Spain. Motolina recounted that he and another priest on one occasion baptized 14,200 Indians "by actual count" in five days (Steck 1951: 190), a rate of 1,420 baptisms per priest per day. Another priest has been credited with baptizing 14,000 Indians during a single day, and on other occasions 10,000 and 8,000 (Rosenblat 1954: 97 n. 1), so Motolinia's account of his own activities rings true. Called to a town near Tlaxcala on one occasion, Motolinia baptized 1,500 individuals in one day besides preaching, confessing, burying, and marrying (Steck 1951: 203). Since Motolinia presumably wrote of memorable events, it is not likely that a typical day's baptisms approached 1,400 or 1,500 Indians. Assuming an average day's baptisms numbered 700 per priest during this period of mass conversion, and assuming that each priest baptized in this manner only once per week, then each would have baptized at the rate of 36,400 persons annually. Motolinia reported 60 Franciscans in New Spain in 1536, 20 of them not yet baptizing for lack of knowledge of local languages, more than balanced by 20 pioneers who had died after baptizing "more than one hundred thousand" in three instances, and by 20 who had returned to Spain (Steck 1951: 181–82). Sixty priests would have baptized 2,184,000 Indians annually, granted the assumptions previously stated. When it is remembered that Dominican, Augustinian, and secular priests augmented the Franciscans (Steck 1951: 182 n. 5), it may be seen that Motolinia himself very possibly failed to appreciate the total magnitude of postconquest mass conversion in New Spain, even though he credited himself with baptizing 400,000 Indians (Clavigero 1781: 282 n. t; Rosenblat 1954: 97 n. 1).

Cook and Simpson did not stop with their Franciscan baptism-based population estimate, nor with consulting only one class of documentary source. They went on to compare clerical estimates with other types. They considered the two primary chroniclers of military conquest, Hernan Cortez and Bernal Diaz, as "competent and responsible," expressing doubt that they had motives for exaggerating army size estimates, and pointing out that their estimates accorded "very closely in size" with those of native annalists describing preconquest aboriginal campaigns (Cook and Simpson 1948: 23). Willcox (1931: 53), who doubted aboriginal American population was very great, nonetheless accepted estimates of the early 17th-century population of India drawn from reported army size plus extent and type of agriculture. Rosenblat (1954: 97–98) accused Cortez of overstating the size of native armies he fought in order to emphasize the valor of his troops and his own merits as commander, but offered no evidence to support his accusation.

A method of estimating Indian population trends that is feasible when documents from two or more time periods report appropriate information (even when it cannot be directly compared to other types of sources as in central Mexico) is projection from army size to total population. A series of records of military expedition sizes is particularly useful for this purpose. MacLeod (1928: 44) employed this method to estimate the extent of decimation of the Massachusetts tribe during the epidemic of 1616–19 at about 90%. He simply extrapolated from the pre-epidemic estimate of 3,000 warriors to the post-epidemic estimate of 300.

In order to convert army size into total population estimates, Cook and Simpson (1948: 26) computed a factor by finding the proportion of adult males in the Mexican population enumerated in 1930, which was about 1/5. They applied this factor to army size estimates for Tlaxcala, Mexico-Tenochtitlan, Texcoco, Cholula, Huajotzingo, and Chalco plus Michoacan, and projected this to central Mexico, estimating 8,950,000 total population based upon military figures (p. 30). In both these estimation attempts, Cook and Simpson, in contrast to Rosenblat and Kroeber, relied upon eyewitness accounts, a generally preferred procedure among historians including Clavigero (1781: 281) who also relied heavily upon the "testimonie oculare" of Bernal Diaz and Cortez.

Finally, Cook and Simpson (1948: 3–9) compared their estimates derived from two contemporary types of sources with a third estimate obtained by projecting back in time their determination of the 1565 population of central Mexico, a determination made from administrative reports of a wide range of types. Kubler (1942: 615–16, Table 1) anticipated their procedure, but did not attempt to calculate aboriginal population. He felt "this method is not suitable for determining total populations" (p. 607) so failed to follow up his methodological advance. Cook and Simpson (1948: 30) obtained a projection factor by establishing "for a reasonable sample of towns or provinces the ratio of population in 1519 to that in 1565." The ratio they worked out indicated that the 1565 population had been about 40% of that in 1519. Applied to their 4,409,180 figure for the central Mexican populace in 1565, this factor yielded an estimate of 11,002,450 persons living in the preconquest year 1519 (Cook and Simpson 1948: 38). They accepted the method of projecting an established population figure as the most nearly accurate of the three they employed. Their 1565 population estimate, being derived from several types of settlements, provided a more reliable basis for projection than Kubler's (1942: 614) figures which were "almost entirely dependent upon encomienda lists."

In other words, Cook and Simpson found considerable consistency between the three types of documentary sources they interpreted and compared. It is significant that they showed that military and clerical population estimates tended to underestimate rather than to overestimate as Willcox, Kroeber, Rosenblat, and Steward assumed.

Whether by intent or by accident, analysts of the "skeptical" school of thought have emphasized the motives priests may have had for overreporting unconverted American population, and the motives soldiers may have had for overestimating enemy forces (Steward 1949: 657). Empirical evaluation of sources by comparison of documentary statements produced under different circumstances reveals impelling motives to underreport Indian populations. Kubler (1942: 614) explicitly assumed that clergymen being "usually pro-Indian, striving constantly to moderate the burden of tribute" tended to underreport Indian populations. He also assumed that *encomenderos* overreported population from opposite motives. Cook and Simpson (1948: 5) tested this latter assumption against actual data. They compared the populations of the Cortez estate in terms of the tribute values reported by its owners with those shown on a royal inventory made after it was confiscated. The owner's statement for 1560 was 37,314, but the crown agents found 72,139 persons seven years later. Cook and Simpson point out that *encomendero* reports of population or of tribute receipts tend generally to be depressed in this way out of motives of economic gain. It was a "plain case" of concealing assets from the tax collector (Cook and Simpson 1948: 4). Another partial check on aboriginal central Mexican population is found in a report by Alonso de Zorita (Pacheco and Cardenas 1864: 114–15) to the king of Spain. The Spaniards forced the Indians living within a 30–40 league radius of Mexico City to erect a great stone wall to contain their cattle. "They say that over two million people between masons and laborers were occupied . . . four months or a little less." Presumably the Spaniards or the Indian foremen kept books on the work crews so the figure talked about should have been fairly accurate. That such a work force could be raised after the decimation of the first smallpox epidemic certainly bespeaks a large central Mexican aboriginal population. An area with a radius of 100 miles (40 leagues of 2.5 miles) would clearly not include nearly all central Mexico. Yet to raise a work force of 2,000,000 in such an area would require a total population considerably larger to sustain the workers, who built the wall at their own expense. Assuming that all the workers were men, an equal number of women is implied. It is doubtful whether the Spaniards could muster all living males for such a project, or even all the able-bodied males. If the conquerors were able to turn out even as much as two men out of every three for their project, 1,000,000 more would have stayed at home, implying another 1,000,000 women, or 6,000,000 Indians altogether. Such a number would have meant a population density of about 190 persons per square mile or 73.4 per km² in the most densely populated part of central Mexico.

The comparison of populations at 2 points in time to calculate a ratio for use in further estimation is an obvious method used by several investigators already mentioned. Rivet obtained a depopulation ratio for the hemisphere by comparing contemporary with estimated aboriginal North American population. Cook and Simpson obtained another depopulation ratio for central Mexico by comparing 1565 counts with estimated preconquest population in several cities or areas. Rowe (1946: 184, Table 1) obtained a ratio of four to one for Peru by comparing Spanish reports of 1525 or conquest population and 1571 populations five provinces.

Tribute-payers. Cook and Borah have produced an analysis based upon the largest community sample yet obtained from New World population records for the first postconquest century, using the biochronic ratio determination method. Their comparison between 1550 and 1570 populations in 64 central Mexican tribute-paying communities on the coast and in the highlands yielded an average annual depopulation rate of 3.8% (Cook and Borah 1957: 465). This was depopulation from endemic disease, since no epidemics occurred in central Mexico during this 20 year period (p. 467). This computation effectively disposes of the Kroeber (1939: 160) claim that any shrinkage in Mexican Indian population occasioned by the conquest was transient and made good by increase resulting from the "new experience" of internal peace under Spanish colonial administration. It also contradicts the Kubler (1942: 623) contention that "Between 1546 and 1575, it will be seen that an extraordinary rise in population occurred." Apparently Kubler's sample of *encomienda* towns—the defects of which he himself pointed out—was very seriously different from other Mexican towns of this interepidemic period. It would appear that the *encomiendas* must have been attracting migrants, since their inhabitants are not likely to have been any more immune to disease than other Indians.

The Cook and Borah computations were based on tax records so the underreporting motive of tribute recipients presumably produced minimal population figures in these documents. All the more significant, therefore, is the Cook and Borah (1957: 466) estimate of about 25,300,000 central Mexicans living in immediate preconquest times. This was obtained by projecting back to 1519 the population estimate for 1565 derived from tax records by Cook and Simpson (1948: 50–165). This higher estimate of the size of aboriginal central Mexican population should be considered a minimal estimate inasmuch as it projects only the endemic disease depopulation rate Cook and Borah found to have obtained between 1550 and 1570, and does not take into account the tremendous depopulation produced during severe epidemics in 1545 (Ocaranza 1934: 84; Bancroft 1883: 756; Zinsser 1935: 256; Kubler 1942: 623, 631), in 1531 (Ocaranza 1934: 84; Steck 1951: 88), and in 1520–21 (Shattuck 1938: 40; Ocaranza 1934: 83; Sahagun 1955: 61–62; Diaz del Castillo 1956: 293, 328, 349, 336; Steck 1951: 87–88).

In a later study of central Mexican population over the shorter period between 1531 and 1610, Cook and Borah (1960: 49) present evidence that their previous estimate of preconquest population was lower than reality. Their new calculations show a population trend "fundamentally the same" as that Cook and Simpson originally described. The new study, however, "differs from that work in postulating a far higher initial aboriginal population and a far greater and more rapid decrease."

If the first three major epidemics reduced the central Mexican population by 4,700,000 individuals, then the 1519 population was on the order of 30,000,000. The 1st epidemic alone probably reduced it by more —early chroniclers thought half the Indian population died (Nuix 1782: 81), a reasonable statement of smallpox epidemic mortality in any non-immune population with no knowledge of proper care of patients infected simultaneously so that many perish from lack of care, including food. A population of this size would have had a density of 58.4 persons per km².

Borah (1962*a*: 175) has noted that the native population on Mexico's Gulf and Pacific coasts virtually disappeared in a single generation.

An additional check on preconquest central Mexican population derived from a different class of documents has been provided by Borah and Cook. The documents most likely to yield direct information on preconquest affairs would be preconquest documents—virtually all destroyed by zealous Spaniards. Borah and Cook (1963: 22–44) have employed the next best sort of sources now available—three very early colonial records of tribute payments to the preconquest Triple Alliance. Two of these are codices with pictographic representations, one annotated in Spanish, and the third a set of testimonies on preconquest tribute practices the Spaniards elicited in 1554 from aged informants.

In estimating population from tribute records, Borah and Cook (1963: 6–17) assembled evidence that "systems of community taxation and allocation did indeed exist throughout central Mexico." They took into account slaves and others not subject to tribute exactions (pp. 18–19), and the areas of central Mexico independent of the Triple Alliance (pp. 20–21). They had to make several arbitrary decisions as to the frequency of tribute payments and the relative values of commodities paid, which they reported quite explicitly (pp. 45–59). Besides calculating average family size, they had also to determine family tribute quotas in order to estimate population (pp. 60–71).

Assuming average family size to have been 4.5 persons, Borah and Cook (1963:88) estimated 25,200,000 central Mexicans on the eve of Spanish conquest, pointing out that an average family of 5 members implied a population of 28,000,000. These two estimates imply population densities of 49 and 54.5 persons per km² in central Mexico as defined by Borah and Cook (1963: 157). Certainly the density of all central Mexico would have been less than in the lacustrine area, where the wall-building figures cited above indicated 73.4 persons per km².

The Cook and Borah and the Borah and Cook calculations emphasize the Cook and Simpson finding that computations of total populations based upon baptismal figures or army size tend to produce underestimations. This conclusion accords with ethnographic experience that the investigator with limited opportunities for personal contact with peoples of a different culture finds it very difficult to form a realistic idea of how many individuals there actually are in a sizeable population.

A projection exercise may be employed, for what the comparison is worth, to evaluate the various estimates of central Mexican population. Vaillant's (1944: 127, 137) figure of 300,000 residents of Aztec Tenochtitlan just prior to Spanish conquest is reduced by Borah and Cook (1963: 78–79) to 235,000 by excluding 125,000 in Tlaltelolco from a combined city total of 360,000 (an average of 6 persons per house). This can serve as a basis for projection to the Empire and non-imperial peoples of central Mexico. The imperial capital contained 7.1% of the total population if that were 3,300,000, but only 4.4% if the total were 5,300,000, 2% if the total were 11,000,000, 0.93% if the total were 25,200,000, and .78% if the total were 30,000,000. Since Tenochtitlan derived its economic support primarily from the Aztec Empire, which had to maintain other urban centers, it is doubtful whether it could have comprised 7% of all central Mexican population. The larger estimates of total population appear progressively more reasonable by comparison to the city.

Additive Methods

In estimating the size and density of aboriginal populations in the central Lower California desert, Aschman (1959: 147) employed what he termed "the additive method," which appears to be quite similar to bichronic determination. Ascertaining the "maximum recorded population" in Roman Catholic mission records, Aschman then simply added numbers to take into account documented population events. He (pp. 152–53) added the number of deaths during disease epidemics the priests reported had occurred prior to the date of recording the maximum recorded population. In calculating the number of natives in a defined area, Aschman (p. 154) also took into account those Indians shifted administratively from one mission jurisdiction to another and inhabitants of settlements omitted from the maximum recorded population. Aschman figured, moreover, what proportion–20% of the total populace in one area—remained still unconverted to Christianity and not, therefore, reported at the time the maximum recorded population was noted.

Being based upon church records of actual Indian convert populations under direct control of missionary priests who were subject to periodic inspection by ecclesiastical and civil authority, Aschman's additive method neatly invalidates any assertion that the figures in question represent priestly overestimation of little-known aboriginal populations. His method shows very clearly that church baptismal records grossly underrecorded true aboriginal population for a number of reasons, among them Indian mortality prior to the first conversions, resistance to baptism by native religious leaders on grounds that it was a lethal ceremony, and sheer missionary ignorance of unconverted native populations with which little or no contact occurred. Indeed, the major criticism that must be directed at Aschman's method is that it seriously underestimates true aboriginal population because it is bound to contact-period records which reported maximum *recorded* populations long after initial contact and which all too often obscure the real scale of depopulation (Aschman 1959: 244).

Aboriginal Social Structure Reconstruction

Means' reconstruction of the sociopolitical organization and probable population of the Inca Empire was one of the most original estimations of the size of an aboriginal American population using an ethnohistoric method. Means analyzed Spanish chroniclers' descriptions of imperial Inca administration as related by Indians who survived the conquest. These accounts show reasonable consistency in describing the main features of imperial administration, and from them Means (1931: 292) worked out the hierarchy from emperor to smallest administrative unit. He had the advantage that Inca political administration was ordered, at least in theory, in terms of administrative units of similar population size at all levels, and the model numbers were well and consistently remembered by the chronicler's native informants. Multiplying out the ramifications of the administrative system and computing average family size as 5–10 persons, Means (1931: 296) arrived at an estimate of from 16,000,000–32,000,000 persons in the preconquest Inca Empire at its largest extent. The higher figure is consistent with the estimate of 25,200,000–30,000,000 for aboriginal central Mexican population suggested above, in the sense that previous students of aboriginal numbers have accorded approximate parity to these two populations. Thus Sapper (1924: 100) placed the ecological potential of both Mexico and the tropical Andes at some 12,000,000–15,000,000 inhabitants. Kroeber (1939: 165) assumed 3,000,000 for each area. Rosenblat (1945: 92) calculated 4,500,000 in Mexico and 4,150,000 in Colombia, Ecuador, Peru, and Bolivia.

This method has the innate disadvantage of limitation to a hierarchically organized state whose administration is based upon a principle of equally populated administrative units. It is not clear, for example, whether the Aztec Empire was governed by such a system. Cervantes (1914: 295) copied by Herrera (1936: 192) stated that Moctezuma had 30 lords with up to 100,000 vassals, and 3,000 "lords of places." *If* the 3,000 local chiefs were under the 30 lords, and over a base population of about 3,000,000, the Aztec would have been organized rather like, although more simply than, the Inca Empire. The scant historic references to such neatly dovetailing figures foster the suspicion that the 3,000 local chiefs were not underlings of the 30 lords, the latter being simply heads of larger units conquered by the Aztecs. The system of requiring all to reside part of the year in Tenochtitlan and leave their relatives as hostages in order to depart (Cervantes 1914: 296; Herrera 1936: 192) greatly resembled the Inca device for controlling conquered rulers. The Aztec tax-collectors (Cervantes 1914: 298) appear to have been independent of the chiefs, and a direct arm of imperial authority, but Spanish destruction of nearly all of their "painted books, where there were so many accounts and such reason as to be a marvel" prevents a direct estimation of population based upon their table of organization. In indirectly estimating population from fragmentary pictographic records, Borah and Cook (1963: 156) found the populations of various central Mexican provinces to have been quite unequal, judging from the tribute levied on each.

Mason (1957: 175) raised some common-sense objections to accepting the ideal Inca administrative system as having been rigidly applied in practice, claiming that it would have been "unworkable." Births and deaths of members of ayllus would have required "continual" adjustments in order to maintain any unit's population at the ideal level. The conquered provinces, moreover, varied considerably in population (Mason 1957: 174). Baudin (1961: 137; 1928: 121–22) had earlier made the same observation that mathematically accurate groups would not remain so for very long. Baudin (1961: 24) estimated the population of the Inca Empire "was probably between eleven and twelve million" at the conquest. Means (1931: 295) did not refute Baudin's stricture, but preferred to think that adjustments of actual units to the pattern by creating new units or reallocating households in old ones were in fact continual and effective. The Baudin-Mason criticisms are undoubtedly valid insofar as one might attempt an exact reconstruction of peak Inca Empire population, yet they do not seriously impute the utility of Means' method for obtaining an approximation of the actual population. Baudin (1961: 138), like Means, concluded that the Incas did adjust actual populations to the administrative model, contenting themselves with an approximation of the ideal.

The really serious limitation on the use of the method of estimating population upon the base of an ideal socio-political structure is simply the extreme scarcity of hierarchically organized states whose administrative structure is based upon a theory of uniformly populated governmental entities. An ancillary limitation on the use of this method is the rarity of state boundaries which coincide with those of cultural or geographic areas whose population an investigator wishes to discover.

Resource Potential Estimation

Another indirect method of estimating aboriginal American population, derived from the intellectual current of environmental determinism, consists of computing potential human carrying capacity of the various geographical regions of the hemisphere, and then assuming that actual aboriginal populations reached their ecological maximum. By this method Sapper (1924: 100) estimated a hemispheric aboriginal population of 40,000,000–50,000,000, an estimate quite consistent with Rivet's.

A closely related procedure is projection of a population density figure based on an ideal or model societal type over a known geographic area. Guerra (1952: 228) estimated preconquest Cuban population at from 22,000 to 88,000 individuals by classifying those Indians as hunter-harvesters and projecting Ratzel's figure of 0.5–2 persons per square mile density for such societies across Cuba's 44,000 square miles. As an alternative, Guerra estimated that Cuba's preconquest population may have reached a maximum of 220,000 if the Indians relied heavily on fishing.

When the Spaniards went into Cuba in 1511, wrote Las Casas, they found there "many fair Provinces, inhabited with an infinite number of people" (Philips 1656: 21) or "great and populous provinces" (MacNutt 1909: 329). Guerra (1952: 227) estimated the preconquest population of the island at possibly 1,000,000 even though Cuban historians and demographers may prefer much lower estimates. Willcox (1931: 56) for example, inferred that Humboldt would have "put the pre-Columbian population of Cuba at less than 200,000." Yet all he had to go on methodologically was Humboldt's analogy between the impressions of populousness which might have been gained by Columbus and by the Englishmen who landed on Cuba in 1762. Spinden (1928: 643), too, thought most of Cuba to have been "scantily" peopled, but on archaeological evidence postulated and aboriginal native population of at least 100,000 in Puerto Rico, with more than 1,000,000 in the "Taino nation" on Puerto Rico, Santo Domingo and Cuba. Las Casas claimed that "above 600,000" Indians had perished on Puerto Rico and Jamaica by 1541, leaving "scarce 200" (Philips 1656: 20).

Las Casas (MacNutt 1909: 321) described Hispaniola as aboriginally divided into five kingdoms. The population was so large that in one kingdom the "lords" were "every one of them" (Philips 1656: 11) or "one alone of them" (MacNutt 1909: 321) able to muster 16,000 warriors in the king's service. Perhaps the native leaders whom Las Casas said he knew sought to impress him. In any event, a large native population is implied even if the Indians were boasting. Another Spaniard attempting to impress upon the Spanish crown the extent of Indian depopulation to 1685 claimed that the island of Hispaniola had a population of over 3,000,000 Indians when discovered (Fernandez 1949: 29).

Direct Observation

Ordinarily the social scientist prefers first-hand observations and interviews with native respondents to the type of procedures thus far discussed for collecting data on population trends. In the geographically isolated Amazon Basin of South America, a few native populations have survived in areas so inimical to European exploitation that their first effective contacts with non-Indian society seem to have occurred recently enough for ethnologists to collect information on precontact population characteristics by interviewing informants. Vellard had this experience in 1938 among the Sabané and Tagnani divisions of the Nambikwara. The former began to approach Brazilian camps about 1926, and in 1929 their first influenza epidemic almost annihilated them (Vellard 1956: 80). Three hundred in one band died of pulmonary oedema in 38 hours (Lévi-Strauss 1961*b*: 286). In 1931 a group of 300 Sabané visited a Brazilian post, where they waited two months for a wagon train carrying them gifts and rations. Shortly after the wagons arrived, epidemic bronchial pneumonia virtually wiped out this group. The few survivors who fled to their settlements spread the epidemic.

Only 21 Sabané survived in 1938, by which time they had had to join the Tagnani. From an unknown pre-epidemic population, this group was reduced to less than 100 by 1938, including the Sabané refugees (Vellard 1956: 81). Holmberg (1950:9) reported that a Siriono school population halved during the years 1940–45. Métraux (1962: 107) reported a Kukraimoro group whose members were reduced by half during one year for lack of resistance to disease agents of the whites.

So few native American groups have survived unaffected by Old World disease agents to the present that the direct interview and observation methods can be but little used in studying the questions of precontact hemispheric population and historic population trends outside the Amazon Basin. Ribeiro (1956) has summarized numerous other samples of directly observed decimation of modern tribal peoples in Brazil. Métraux (1962: 108) summarized the contemporary situation of the Indians under the Indian Protection Service succinctly: "Pacification is synonymous with disappearance." The surviving unaffected groups are, moreover, by their very nature among the least important populations for determining the extent of preconquest human proliferation in the hemisphere. The civilized Indians subject to the native American empires were, by far the most numerous of all New World groups, and estimations of their pre-Columbian magnitude must rest upon methods adapted to documentary analysis.

SCALE OF DEPOPULATION BY DISEASE

If American Indian depopulation still occurs within the present century on the scale Vellard encountered among previously isolated Amazon Basin tribes, the central question in attempting to estimate how many Indians inhabited the Western Hemisphere prior to European discovery would seem to resolve itself into determining whether such severe depopulation was the rule or the exception.

The key case of the California Indians merits reexamination in these terms. Kroeber's comparison of the number of Indians enumerated in California during the 1910 census with his own estimate of aboriginal population convinced him (1925: 886) that only one Indian remained where eight had lived prior to the coming of the white man. He declared, however, that: "The causes of this decline of nearly 90 per cent within a period ranging, according to locality, from only 6 to 14 decades, are obscure. New diseases and alteration of diet, clothing, and dwellings have undoubtedly contributed largely" (p. 887). Because other peoples subjected to such influences have thrived, Kroeber went on to posit a cultural explanation for the decrease he recognized. He pointed to a direct correlation between intensity of contact with Europeans and depopulation (p. 888).

Kroeber (p. 891) ended on a final note of skepticism: "The decrease is saddening, however cautiously we may assume the absolute numbers. But excessive exaggerations need also be guarded against, such as the statement sometimes cited that 70,000 California Indians died of epidemic diseases in a few years following 1830."

Cook (1943: 161) asserted that "it is to be doubted if Kroeber's conclusions can ever be subject to fundamental or drastic revision," but he nonetheless pointed out (p. 187) that "Kroeber, as wel as other investigators, have not realized the tremendous death rate at the California missions." In an ealier ethnohistorical study, Cook (1940: 7) found that the mission Indian population of California was only 18,100 in 1830 (compared to Kroeber's figure of 24,634) and fell to 14,900 in 1834 (compared to Merriam's figure of nearly 30,000). Such a revision of terminal mission-period population figures might, at first glance, seem to require a revision downward of the Kroeber and Merriam estimates of aboriginal population. Although he did not attempt to estimate the aboriginal population of the missionized area of California at the time, Cook (1940: 24) showed that the mission Indian death rate varied between 70 and 85 per thousand population, compared to a birth rate which declined from an initial 45 to 50 per thousand to about 30 per thousand (p. 16).

This discrepancy between death and replacement meant an absolute biological loss of hundreds of mission Indians annually; e.g., Cook (1940: 23 n. 4) found records of absolute losses of 608 human beings in 1796, 773 in 1811, and 616 in 1814. The drop in mission population from 1830 to 1834 when missions began disintergrating averaged 800 annually, probably signifying a net biological loss of around 600 per year. If the annual loss were in fact 600 on the average each year between 1770 and 1830, the total Indian loss within the missionized region approximated 36,000 individuals. This figure is double the number of 1830 survivors, not half the number of survivors, as Kroeber calculated. It indicates a premission population on the order of 54,000 Indians in the missionized area.

In a venture into "dead reckoning" estimation of aboriginal Indian population, Cook (1943: 194) succeeded in raising Kroeber's figures for the California Indians (excluding the Modoc, Paiute, Washo, Eastern Mono, and Colorado River tribes) by only 7%; even using many documentary sources, he arrived at 133,550 for the same groups Kroeber assigned 125,000.

Cook (1939: 177) had already published mortality figures from seven California missions during the smallpox epidemic of 1828, showing a death rate of 51.5 per thousand population from that cause alone. Cook (p. 185) also noted a severe smallpox epidemic in 1837–39 in California with contemporary estimates of Indian mortality expressed in tens of thousands.

Precisely the matter of epidemic mortality in 1830–33, which Kroeber had expressly discounted, provided Cook with the evidence necessary to revise fundamentally and drastically his own and Kroeber's conclusions. Utilizing mainly traveler's accounts of pre-epidemic populations, the spread of the contagion, and its impact on the Indians, Cook found that Indian mortality was approximately the same in the Willamette Valley and the central San Joaquin- Sacramento Valley of California—about 75% of the pre-epidemic population. This meant that ¾ of the lowland riverine dwelling Indians who had already survived some six decades of Spanish and Mexican rule or contact with Indian Christian converts, were wiped out during a single summer (Cook 1955a: 322).

In other words, the aboriginal population of the San Joaquin-Sacramento River Valley exceeded Kroeber's estimates by a factor of two or three. The Wintun pre-epidemic population was on the order of 24,000–36,000 using Kroeber's (1925: 883) 12,000 ethnographic estimate as a base, or 28,500–42,750 using Cook's (1943: 179–81) revised ethnographic estimate as a base. Wintun population may well have been even larger in 1770. The Yokuts pre-epidemic population was on the order of 36,000–54,000 prior to the 1833 epidemic, rather than 18,000 as estimated by Kroeber and previously accepted by Cook (1943: 192–93). Even that total was certainly lower than it had been in 1770, since the Yokuts in the San Joaquin Valley were near enough to missionized populations to have contracted earlier epidemic diseases. In a seperate analysis of San Joaquin Valley aboriginal population based on Spanish sources, Cook (1955*b*: 70) estimated a total more than four times that of survivors in 1850. He reiterated that "warfare, massacre, forced conversion, starvation and exposure" plus "sweeping epidemics" together "destroyed in the aggregate fully seventy-five per cent of the aboriginal population."

Since Kroeber approximately halved Merriam's estimated aboriginal Californian population, Cook's factor of two indicates that Merriam's estimate of 260,000 aboriginal Californians was more or less accurate as an estimate of pre-epidemic population. Cook's factor of three yields a figure of approximately 400,000 aboriginal population. This sum needs to be reduced only sufficiently to omit the mountain and coastal tribes, which were not affected, from the calculation of epidemic depopulation. An aboriginal Upper Californian population on this order would have had a density averaging perhaps two persons per square kilometer. In view of the greater ecological potential of Upper as compared to Lower California, such a density is much more logically consistent with Aschman's minimal one person per square mile in the central Lower California desert than Kroeber's estimated density of less than half as much for the more generous ecological region.

Cook's (1955a) analysis of this California epidemic demonstrated the operation of two very important processes in the human ecology of aboriginal American populations. First, he showed the magnitude of mortality which a single epidemic can cause in a non-resistant population. Second, he called attention to the biological fact that epidemic infection is not limited to tribal populations in immediate face-to-face contact with Europeans. The decimation of native Californians was not limited to missionized Indians, but extended outward as far as disease agent and vector could spread infection from intrusive (white) carriers to aboriginal populations. It is necessary to maintain constant awareness of these two processes or fundamental trends among natives of the New World. Any interpretation of reported native populations during the early years of contact with Europeans which ignores the tremendous mortality caused by epidemics inevitably underestimates the size of the aboriginal populace.

Cook's study showed quite clearly that one fatal defect in both Kroeber's data and method was ignoring disease, and particularly epidemic disease, mortality. This defect led to projecting post-epidemic ethnographic populations backward in time on the naive assumption that such simple projection could yield even approximations of truly "aboriginal" populations in the biological sense. Here reference should be made to Steward's (1949: 656) summary of the remainder of Kroeber's (1939: 180) methodological assumptions.

> (4) a rich ecology usually means a greater native density, but such factors as iron tools and friable soils must be taken into account in comparing modern and native densities; and (5) a rich culture is usually an index of a high density.

Nowhere are Old World disease agents mentioned as a factor changing native populations before the ethnographer's arrival among them, although as Vaillant (1944: 288) later pointed out: "European diseases, like smallpox, measles, and tuberculosis, wiped out great sections of the population." Kroeber (1939: 160) discussed apparent fluctuations in the Indian population of the central Mexican area without any mention of the impact of the introduction of European diseases to the civilized Mexican Indians, and the toll of life these took in both endemic and epidemic form. Kubler, in contrast, concluded that: "Widespread and repeated epidemics was of course the major determinant and the most obvious one" of population decline in Mexico. Kroeber (1939: 160) did take into account warfare as a possible cause of depopulation, but omitted his own earlier recognition of diseases as one cause of native decline in his analysis of California Indian population (Kroeber 1925: 887).

The significance of epidemic depopulation among North American Indians had been pointed out by many writers before Kroeber and Rosenblat. Three anthropologists, upon two of whom Kroeber drew for material, did so in 1928. After concluding that there "are fewer Indians today than at the coming of Europeans," Spinden (1928: 660) asserted that: "The greatest factor in depopulation has been disease in epidemic form." Mooney (1928: 5, 7, 12–14, 21, 24–25, 27) repeatedly mentioned epidemic depopulation of specific North American Indian groups. MacLeod (1928: 40–52) also pointed out the ravages of epidemics among North American Indians. Wissler (1936: 36) explicitly recognized epidemic depopulation as an independent variable in Northern Plains Indian settlement. He concluded that because they largely escaped the smallpox epidemic which decimated other tribes in the Saskatchewan area about 1780, the Assiniboin were able to multiply rapidly and spread over most of the area. The previously marginal Cree, in turn, "expanded in numebr over the same territory" after ⅔ or more of the Assiniboin perished during a smallpox epidemic in the 1830's.

Later on, Steward (1949: 668) at least recognized that European diseases were one factor in native population decline, calling attention to their differential effect in neighboring tribes, some being wiped out and others surviving in strength to the present. Hoebel (1949: 401) noted pestilence and warfare as the major causes of Indian depopulation.

Such scientific awareness of epidemic impact on native American population rests, of course, upon a very firm historical record. Las Casas was merely the earliest and best-known commentator upon the biological plight of the Indian. The controversy he initiated has probably waxed so hot as long as it has because he attributed Indian depopulation to Spanish cruelties—which, being expressions of human will, were presumably controllable—rather than to diseases whose transmission to virgin populations became inevitable once contact was established between Indians and Europeans. Transmission could not be avoided in any event, given the state of medical knowledge in both Europe and Indian America in the 16th century.

Defenders of European colonialism in the Americas have pointed out this defect in Las Casas' argument. Nuix (1782: 81) counted smallpox as one of four major causes of native depopulation. He blamed Spanish mining, (p. 46) the nature of Indian agriculture (pp. 76–77), and the lack of communication and trade with Spain, the last of which he attributed to the actions of other nations (p. 85). Lopez de Velasco (1894: 26) who mustered information on the entire Spanish colonial empire in the Americas, commented in 1574, "In every place discovered, the natives were in the beginning much more numerous than those who have existed later, because in many provinces where there used to be a great multitude of them, they have become almost entirely extinct." He attributed the depopulation he recorded to warfare and abandonment of fields to avoid subjugation, "from which there followed during the first years general mortality and illnesses never seen in those parts, such as smallpox which the Spaniards transmitted." Many local historians in the Spanish colonies recognized the prime role disease played in Indian depopulation. A 1579 observation on the decrease of Indian numbers in the Province of Tabasco was: "It has declined to this point because of great illnesses and pestilences which have occurred, both those characteristic of this province and those of general character throughout the Indies . . ." (Scholes and Roys 1948: 166). Clavigero (1781: 282) recognized that an infinite number of Indians perished in the epidemics of 1520, 1545, and 1576 in New Spain.

A defect in Kroeber's hemispheric estimation method was, as in Rosenblat's, omission of information on disease mortality patterns in populations estimated. The defect in method might be stated in the form of another implicit working assumption which Steward did *not* state: (6) A disease agent introduced into aboriginal American populations from the Old World has no significant effect on them.

Such an assumption is, of course, demonstrably unjustified, as Cook's analysis of the 1830–33 California epidemic, my (Dobyns 1963*b*: 496–515) analysis of epidemics among Andean Indians between 1525 and 1720, and many medical studies of epidemiology have shown. Any method of aboriginal population estimation which seeks to project back through time census or ethnographers' enumerations of recent Indian populations must assume that disease agents have affected the population trend of the group under investigation and seek data as to the nature and extent of their effect.

DEPOPULATION RATIOS

Having indicated dissatisfaction with several previous estimates of the scale of population of aboriginal America, I find it incumbent upon me to indicate a workable method. I suggest another projection method employing a depopulation ratio established by comparison of relative numbers of a given group at two points in time. One such time should be that when the population analyzed fell to its lowest numerical strength.

Clearly, it would be preferable methodologically to establish depopulation ratios for each societal unit from pertinent documents, but that is impossible for lack of records. Even to establish regional depopulation ratios for each significant cultural area lies beyond the scope of this paper. The present attempt aims only at outlining a reasonable method for obtaining depopulation ratios.

In order to determine a "standard" hemispheric depopulation ratio for demonstration purposes, a ratio will be established between the known or confidently estimated preconquest population in 1 area and a known or closely estimated nadir population in that same area. The ratio thus obtained will then be tested for consistency against comparable ratios for other areas in order to arrive at what appears to be a generally applicable ratio.

Central Mexicans. Turning again to the Cook and Simpson (1948: 47) analysis, one finds that they estimated the nadir of central Mexican population as about 1,500,000 Indians. Comparing this with their estimate of 11,000,000 central Mexicans living in 1519 (p. 38) yields a depopulation ratio of 7.33 to one for central Mexico. In other words, where one Indian survived in 1650, 7⅓ had lived prior to conquest.

The Cook and Borah calculations of a 1519 central Mexican population on the order of 25,300,000 compared to the same nadir population provides a depopulation ratio of 16.9 to 1 for central Mexico.

Since those calculations ignored the tremendous depopulation caused by the first of three historic epidemics in central Mexico, a higher aboriginal population on the order of at least 30,000,000 is suggested. Such a figure means a depopulation ratio of at least 20 to 1 over the 130 years from aboriginal times to population nadir. In one small area, as a matter of fact, Cook (1958: 20–22) estimated an aboriginal population of 10,000 for Ixcatlán which stabilized at about 500 survivors in the 17th century—a depopulation ratio of exactly 20 to 1 parallel to a reduction from eight settlements to one.

Coastal Mexicans. A local historian in 1579, only 58 years after the Spanish conquest, asserted that the Province of Tabasco then contained less than 3,000 Indians. When conquered, it had held 30,000 inhabitants (Scholes and Roys 1948: 166). This meant a 10 to 1 depopulation ratio in less than 60 years.

Andeans. The best test for the central Mexican depopulation ratio would be comparison to such ratios for the native populace of the Andes, where civilization and urbanization also flourished prior to conquest. Unfortunately, early historic population trends in the area of the prehistoric Inca Empire have not yet been so systematically studied as those in central

Mexico, so the data required for a direct comparison are simply not available. The best that can be done is to consider some sample areas.

The Indian population of the central coast of Peru almost completely disappeared during colonial times. There the population of Rimac (the modern Lima area) declined from 16 to 1 and that in Chincha from 25 to 1 in less than half a century after conquest (Rowe 1946: 184, Table 1). Rowe, like Means, estimated the preconquest population on the basis of socio-political structure, allotting five persons to each of the families in the three "jurisdictions of 10,000 families each" in the valley (Cobo 1956: 301). Since Rowe based his family of five on 1571 conditions, after conquest and infection altered family composition, the actual aboriginal population (and, therefore, the depopulation ratio) was almost certainly greater.

Another useful sample is the work force compelled to dig ore from the Potosi mines and extract metals from it. Viceroy Toledo designated 16 Andean highland provinces to provide this labor in 1575, a generation after conquest, assigning 95,000 Indians. By 1663 the number had fallen to 40,115, and by 1689 it was down to 10,633 (Vellard 1956: 85). While other factors, such as out-migration from these provinces to avoid compulsory mine labor, helped to lower their population, the toll of disease is certainly reflected in the indicated depopulation ratio on the order of 9 to 1 over 115 years.

In contrast, the Marques de Varinas in 1685 wrote a description of the road from Lima to Paita very similar to travelers' accounts of California Indian settlements after the 1830–33 epidemic. "One recognizes at very short intervals mounds of skulls and bones of these miserable beings which horrify those travelling the road" (Fernandez 1949: 30). He estimated that scarcely 20,000 Indians remained of over 2,000,000 who had once inhabited this region (Fernandez 1949: 29), thus suggesting a depopulation ratio of 100 to 1.

Unsatisfactory as these figures are, they permit some comparisons with central Mexican ratios. The 9 to 1 ratio for the Potosi mine tributary area is nearest the central Mexican ratio derived from the Cook and Simpson low estimate of aboriginal population, or the Tabasco Province 10 to 1 ratio over only the first six postconquest decades. Yet the Peruvian ratio comes from a period beginning over 40 years postconquest, so cannot possibly take into account early population losses.

The figures for the Rimac Valley may have been skewed by immigration of mountain Indians to the colonial capital of Lima, so Rowe's 16 to 1 ratio is perhaps too low. It agrees most closely with the Cook and Borah estimate of aboriginal central Mexican population which fails to take into account early epidemic depopulation. The remaining Chincha area ratio of 25 to 1 is somewhat higher than the 20 to 1 ratio derived from a suggested 30,000,000 aboriginal population in central Mexico.

Californians. Lacking really adequate comparative data from the Andean region, one turns again to the California Indians, who have played an important role in forming anthropological conceptions of the size of pre-contact American populations. The Cook and Simpson central Mexican depopulation ratio of 7.33 to 1 and the Peruvian mine tributary ratio of 9 to 1 are most nearly consistent with Kroeber's (1925: 886) 8 to 1 California Indian ratio. Actually, that ratio should be stated as 8.3 to 1, even if the 1910 census figure of 15,850 is rounded off at 16,000.

Raising Kroeber's estimate by Cook's suggested factor of 2 to take into account epidemic mortality during the 1830–33 epidemic yields a depopulation ratio of 16.6 to 1. This is very close to Rowe's Rimac River Valley ratio in coastal Peru and the ratio for central Mexico based on the Borah and Cook estimate of preconquest numbers.

Multiplying Kroeber's low California estimate by Cook's suggested factor of three provides a depopulation ratio of 25 to 1. This is consistent with Rowe's Chincha ratio from Peru, and higher than the central Mexican ratio based on the 30,000,000 estimate. Since Spanish missionization in Upper California began in the 1770's, the period of population decline to the 1910 nadir was 130–140 years. That is comparable to the time from conquest to nadir in central Mexico, but considerably longer than the time span of the two coastal Peruvian ratios cited.

Tierra del Fuegians. The natives of Tierra del Fuego appear to be on the road to biological extinction. Cooper (1946*a*: 108) reported Ona survivors to number 50. Bridges (1948: 521) reported less than 150 full-blood Yahgan, Ona, Aush, and Alakaluf survived in 1947, plus as many half-breeds. Lipshutz and Monstny reported only 40 Ona in 1950, with 63 Yamana and 80–100 Alakaluf (Vellard 1956: 82–83). The Ona alone were estimated at 2,000 toward the end of the last century, and Bridges (1948: 521) estimated the total population at from 7,000–9,000 Indians in 1871. A single measles epidemic played a key role in upsetting the biological balance, killing half the Yamana in one area in 1884 (Bridges 1948: 125–26). Thus the Ona had a depopulation ratio of 50 to 1 from 1870 to 1950, a period of only 80 years.

Amazonians. In the process of depopulation of the Sabané Indians already alluded to, this division of the Ñambikwara dropped from about 1,000 in 1926 to 21 persons in 1938; only one Sabané remained where 47.6 had lived only 12 years earlier. The Sabané appear to be approaching extinction even faster than the Ona.

Surviving Sabané have joined forces with survivors of other Ñambikwara groups, so an amalgam social unit may eventually survive. When Rondon first penetrated Ñambikwara country in 1910, he encountered a very numerous population estimated at 20,000 in 1916 (Vellard 1956: 80). Not only the Sabané and Tagnani, but also other divisions were virtually if not entirely extinct by 1938 and the total population reduced to under 1,000 individuals (Vellard 1956: 81). Thus the depopulation ratio for the entire Ñambikwara group had been on the order of 20 to 1 over a period of 22 years.

Even more extreme was the case of the Cayapo, living on the banks of the Araguaya River. Dominican priests from France established outposts on the stream in 1903 to convert these warlike natives, who frequently attacked rubber tappers and prospectors. A few years later the Dominicans estimated the tribal population at 6,000–8,000 individuals. Intergroup contact was limited almost entirely to the priest, who kept alcohol, tuberculosis, and venereal diseases from reaching the Cayapo. Despite their efforts, the Cayapo were reduced to 500 by 1918 and to scarcely 27 individuals by 1929, and two or three survivors by mid-century (Vellard 1956: 78–79). By 1918 the depopulation ratio was already 12 to 1, and by 1929 it was 222 to 1, indicating virtual extinction for the group.

The Caingangs dropped from 1,200 in 1912 to about 200 in 1916, and 80 in the 1950's, a ratio of 15 to 1. The Mundurucus decreased from 20,000 to scarcely 1,200 between 1915 and 1950, a ratio of 16.6 to 1. The Timbira, who numbered 1,000 in 1900, mustered barely 40 by 1950, a 25 to 1 depopulation ratio (Lévi-Strauss 1961*a*: 14–15).

Northern Pimans. American Indian groups can survive great depopulation through continual social and political amalgamation of survivors if the aboriginal population base be sufficiently populous (and, perhaps, geographically dispersed). This truism is, of course, rather obvious in the cases of the heavily populated native empires. The total aboriginal population necessary to insure eventual survival of an amalgamated populace is apparently not so large as the extreme depopulation ratios encountered among peoples with simple technologies in adverse environments in South America might suggest. Several thousand Northern Piman-speaking Indians in the United States today represent a recovering population that evidently reached its nadir sometime during the last century. Yet mortality was historically extremely high among the missionized riverine divisions of the Northern Pimans. One riverine group on the lower Santa Cruz River suffered a depopulation ratio greater than 51 to 1 between 1700 (earlier records are not available) and 1801, yet survived through continued interbreeding with immigrants from the same language group (Dobyns 1962: 28).

The fact that this group barely survived suggests that a ratio on the order of 50 to 1 over the period of a single century approaches very closely the maximum possible depopulation that will permit survival and recovery by a given population. A ratio any higher almost certainly means extinction for the group. Yet it must be remembered that aboriginal disease conditions ended among the Northern Piman Indians long before 1700, so survival of even greater population losses seems possible.

General Ratio. From the wide range of depopulation ratios encountered for various native American populations in widely scattered portions of the hemisphere, it appears that a ratio of 50 to 1 over a century marks the approximate outer limits of human survival and population recovery. Criteria for identifying a minimal depopulation ratio for all native American populations are less readily established. The data analyzed in this paper do indicate that ratios clustering around 7 or 8 to 1 over a period of 130–140 years between contact and population nadir are to be regarded as much too low to reflect the reality of American Indian depopulation. These data also indicate that depopulation ratios on the order of 16 or 17 to 1 are approaching historical reality, but are also very likely low.

The data that best withstand testing are those indicating a depopulation ratio of 20 to 1 for central Mexico from conquest to the beginning of population recovery and a ratio of 25 to 1 for the California Indians and the coastal Peruvian Indians of the Chincha region. It is, therefore, interesting that a Spaniard writing in 1685 should have opined that the Spanish government "has now left of twenty parts one" (Fernandez 1949: 29) on the Caribbean coast between Vera Cruz and the Orinoco and in southeastern South America. The depopulation ratio of 20 to 1 appears to be a sound, if perhaps conservative, tool to employ as a hemispheric minumum. It agrees with Borah's (1962*b*: 179) estimate of 95% reduction in native population over a century, although a longer period for such loss is accepted in this paper.

ESTIMATED ABORIGINAL POPULATION

Assuming a "standard" hemispheric depopulation ratio of 20 to 1 between initial contact and the beginning of population recovery should permit estimating aboriginal American population if sound figures on the size of the various Indian groups when they reached their respective nadirs can be found. If a population did not recover and the group became extinct, a depopulation ratio has little utility for calculating its original pre-contact scale. It may be possible to use a ratio to approximate the aboriginal magnitude of such extinct groups based on the number of survivors about 130 years after initial contact. There is some evidence that this was frequently the time native American populations required to reach nadir and begin to recover—at least it was so among the central Mexicans and California Indians.

As an antidote to previous overly low estimates of aboriginal American population, and as a working hypothesis to stimulate research into this question, a summary of the calculations performed in this paper is offered in Table 2.

Borah (1962*b*: 179) recently advanced the figure of 100,000,000 as very possibly the population of the New World at the end of the 15th century. The calculations in the present discussion support his approximation, for his estimated population would indicate a hemispheric average depopulation ratio between 20 and 25 to 1. My figure implies a hemispheric average population density of 2.1 persons per km². Borah's implies 2.4 per km².

Abstract

Social scientists often consider population size as an independent variable of major importance. The author analyzes, therefore, methodological reasons why most prior estimates of aboriginal American population imply small scale preconquest societies and concludes that the population was far larger than has been thought. The range of previous estimation is so great as to indicate that some methods or data must have been faulty. Skeptical anthropologists and historians have regarded historic population figures reported by contemporary observers as larger than reality. Ethnohistorical estimations based on careful cross-checking of direct and indirect sources of popu-

TABLE 2

Estimated Aboriginal Population

Area	Nadir Population	Date of Nadir	Projections × 20	Projections × 25
North America	490,000[a]	1930+	9,800,000	12,250,000
Mexican Civilization	1,500,000[b]	1650	30,000,000	37,500,000
Central America	540,000[c]	1650	10,800,000	13,500,000
Caribbean Islands	22,150[d]	1570	443,000	553,750
Andean Civilization	1,500,000[e]	1650+	30,000,000	37,500,000
Marginal South America	450,000[f]	?	9,000,000	11,250,000
Western Hemisphere			90,043,000	112,553,750

lation data demonstrate that contemporary observers underreported the true magnitude of native American populations.

Estimates obtained by projecting backward through time population data from modern Indian census counts are seriously defective, as are past applications of the the dead reckoning mode of estimation. Ethnohistorical methods are less deficient. Cross-checking sources serves to increase the accuracy of estimation, and informant knowledge is evaluated in terms of opportunities for accurate observation and especially for accurate memory, since it is more difficult to retain quantitative than qualitative data.

Additive reconstruction of Indian population from historical records is handicapped because the geographic spread of literate Europeans lagged behind the diffusion of new disease agents which decimated aboriginal populations. Direct observation of Indian population trends by anthropologists suffers the same limitation, even though several have recorded extremely rapid depopulation of Amazon basin peoples within the century.

Calculation of aboriginal population from preconquest social structure is possible only in 1 area governed by an imperial ideal of administrative units consistent in population.

a Rosenblat (1945:22) gives figures for Canada, 127,374; the United States, 332,397; and Alaska, 29,983; a total of 489,754.

b Cook and Simpson (1948).

c Rosenblat (1954:59).

d Rosenblat (1945:81). The 1650 population of the Caribbean Islands was only 10,000 Indians, according to Rosenblat (1954:59). By 1825 the natives had been exterminated (pp. 36-37). Since contact began earlier in the islands than on the mainland and insularity probably hastened the depopulation process, the 1650 population was already well under comparability with the nadirs of populations which recovered. Using the 1650 figure, for lack of a better, yields an estimate of 200,000 persons almost certainly less than the actual aboriginal population.

Using a shorter time period than 130 years for the Caribbean Islands may compensate for the apparently faster rate of population decrease there than on the mainland. The Indians of Jamaica were already extinct by 1570 (Rosenblat 1945:81), but the other islands marshaled some 22,150 surviving aborigines. Taking this figure as equivalent to the nadir of population that recovered implies a pre-contact populace of about 443,000 individuals, compared to Willcox's (1931:56) "less than six hundred thousand" obtained by assuming that 1/3 of the modern Caribbean population lived on that island, and Rosenblat's (1954:102) 300,000.

e The figures Rosenblat (1945:36, 57) assembled indicate that the Indian population of South America may have reached its nadir later than the population of central Mexico, because his estimates for Indian survivors at the epoch of national independence are appreciably lower than those for 1650. Taking his 1825 approximations as representative of the Indian nadir, and using the factor of 20, one would postulate an aboriginal population in all South America on the order of 72,500,000 persons.

Such a high figure is not consistent with previous images of the relative population density of Andean and Mexican civilizations. These have generally been assumed to have encompassed populations of about the same magnitude.

It would appear, therefore, that Rosenblat's approximations for 1825 represent in fact a recovering South American Indian population already past its nadir, despite the fact that his 1825 estimates of Indian survivorship are lower than those of 1650 and in spite of Kubler's (1946:336, Fig. 32) graph of colonial population decline in Peru until after 1781.

f Sapper (1924:100) allotted tropical eastern South America a population 17-20% of the size of his estimate of that in the Andes. Rosenblat (1954:102) allowed Brazil, Venezuela, the Guianas, and Paraguay somewhat over 40% of his Andean figure. Steward (1949:666, Table 3) allowed these national areas plus lowland Colombia 42% of his Andean estimate. These proportions of 20–40% imply from 6,000,000 to 12,000,000 aboriginal Americans in this area, based on an Andean total of 30,000,000. A Spanish decree of 1639 claimed that Portuguese slave hunters had captured over 300,000 Guaro Indians from 1 province in the Uruguay-Paraguay area during a 20-year period. Hernandez (1912:123) supposed that 2 Indians escaped for every 1 enslaved, and estimated a 1,000,000 preconquest population, with possibly 500,000 more in Paraguay proper. 6,000,000 Indians living in tropical South America prior to contact seems a resaonable supposition in view of historical population movements of this magnitude.

For temperate South America, Sapper (1924:100) estimated an aboriginal population 8-13% as large as his estimate of Andean civilized populace. Rosenblat allotted Uruguay, Argentina, and Chile a population about 22% the size of his Andean figure (Rosenblat 1954:102). Steward (1949:666, Table 3) allocated these areas 28% of his Andean population size. An aboriginal population 10-20% as large as the Andean total implies another 3,000,00-6,000,000 Indians in this section of the continent.

The aboriginal Araucanian population alone has been estimated at from 500,000 to 1,500,000 (Cooper 1946*b*:694). If the nadir in this group's population occurred around 1900 with approximately 100,000 survivors (Cooper 1946*b*:695), then the higher estimate for aboriginal numbers appears conservative, inasmuch as it would imply a relatively mild depopulation ratio of only 15 to 1 for an amalgam native group constantly at war, with the Spaniards for centuries, as well as exposed to contagion.

Approximately accurate estimates of aboriginal American population may be achieved by comparing the population of a given area at two or more times in order to establish population trends expressed as ratios of the size of the population at 1 time to its size at another. Early historic depopulation was great in the Americas: a well-documented instance of recovery following depopulation from 50 to 1 over one century indicates limits to such a trend. Greater population loss probably results in extinction. A hemisphere-wide historic depopulation ratio of 20 to 1 is postulated. Applying it to more or less well-established historic Indian nadir populations suggests that the New World was inhabited by approximately 90,000,000 persons immediately prior to discovery.

H. F. Dobyns, 'Estimating Aboriginal American population: An appraisal of techniques with a new hemispheric estimate', *Current Anthropology*, vol. 7, no. 4, 1966, pp. 395–416.

Charles Wilson

SMALLPOX AND ABORIGINAL GENOCIDE

IT IS SCARCELY NECESSARY to describe the achievements and reputation of Noel Butlin: helpfully they are set out in the foreword to his latest book* by Professor H.V. Coombs. Nevertheless, it may be useful to refer briefly to Noel Butlin's standing amongst economic historians. The bonding of suitable elements of economic theory and economic statistics into the fabric of what for centuries was essentially descriptive historical narrative has transformed the nature of this branch of history — at first slowly, in the last few decades with increased speed and momentum. In this new branch of study Noel Butlin is a commanding figure.

In a preliminary canter through his latest book, the reader will be relieved to find the source of its striking title. Since the full quotation from which the title is taken is not spelt-out anywhere in the book itself and is not referred to in the disappointingly inadequate index, it may get overlooked by those readers who do not trouble themselves to peer at the small print on the unnumbered page preceding the title.

It is taken from the dispatch sent by Lord John Russell to Sir George Gipps on 21st December 1838. It ran as follows:

> You cannot overrate the solicitude of H.M. Government on the subject of the Aborigines of New Holland. It is impossible to contemplate the condition or the prospects of that unfortunate race without the deepest commisseration (*sic*).

Here His Lordship inserted a sentence, omitted by Professor Butlin. Since its effect is to shed more creditable light on British intentions towards Aborigines than indicated elsewhere in his study, it is worthwhile quoting it. It ran:

> I am well aware of the many difficulties which oppose themselves to the effectual protection of these people, and especially of those which must originate from the exasperation of the Settlers on account of aggressions on their property, which are not the less irritating because they are nothing else than the natural results of the pernicious examples held out to the Aborigines, and of the many wrongs of which they have been the victims.

The subject of the whole correspondence, it should be said in fairness to all those concerned, was a searching inquiry by H.M.G. into an incident when a contingent of NSW Police, pursuing some Aboriginal misdoers, fired on others who happened to be innocent of any crime. His Lordship continued:

> Still, it is impossible that the Government should forget that the original aggression was ours.

He concluded by assuring Gipps of every support from the Crown in his efforts to secure justice for the Aborigines and "every social advantage" which it was the Colonists' duty to impart to them. It may be interjected that it cost little to adopt such a moral stance in Whitehall. Nevertheless, where so many whites are to be impugned, Russell's sense of responsibility should not leave any occasion for doubt.

In his Preface, Butlin speaks with deceptive modesty about the objectives and character of his study. It is "frankly and explicitly speculative and hypothetical. It deals not with history but with what might plausibly have been if reality was broadly consistent with a set of theoretical assumptions". He believes that he has "endeavoured to eschew moral or political judgements". He concedes that "one is likely to have opinions about the morality of genocide". But he believes that it is "more important that the matter be brought out starkly into the open than that one be diverted into moral judgement". He concludes, confusingly as it seems to one who has read the book, "if at times the book seems cold-blooded, this is intentional in order to avoid obfuscation". We shall return to this point later.

Meanwhile let us observe that his declared purpose is with *population* — to reform the picture which scholars as well as ordinary readers have of the black population of Australia in 1788 when the First Fleet landed, and the subsequent decline of that population down to the middle of the 19th Century. Australians, he believes, have lived

* Noel Butlin: *Our Original Aggression. Aboriginal Populations of Southeastern Australia 1788-1850* (Sydney, 1983).

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with a very "comfortable perception" of the fate of the blacks. He is much concerned with the possibilities of error which result from too easy reliance on statements by whites. Again we shall need to consider later how objectively Butlin balances the possibilities of error in *both* white and black statements or reported statements and recollections.

What are the figures he challenges? Down to 1930, two figures were quoted by the handful of scholars, public servants and the like who happened to be interested. The first Governor, Arthur Phillip, had ventured a guess (and never claimed that it was more than a guess) that there were a million blacks in 1788. The official Government figure proposed through the 19th Century was 150,000. In 1930 this "received figure" was doubled by Professor Radcliffe-Brown, to 300,000. The basic task of Butlin's book is to challenge the Radcliffe-Brown figure.

For his (1788) total he substitutes a figure of the order of five or more times, for Victoria and New South Wales alone. He feels compelled to dismiss Radcliffe-Brown's total as completely unacceptable; although he does not commit himself to a figure of blacks for the whole of Australia in 1788, it nevertheless begins to appear that if he *had* done so, the total would begin to look more like Arthur Phillip's casual guess than like Professor Radcliffe-Brown's 300,000.

"No one knows how many blacks lived in Australia from 1788." This frank, and, to the reviewer incontestable declaration of demographic agnosticism, supported by suitable props. leads Butlin on page 5 to write: "we may seem to be facing a hopeless task in efforts to reconstruct possible pre-white population. Well, let us see". If the search for "likely" evidence to support Butlin's revisions of traditional figures and assumptions were carried out with the same respect for impartiality, fragility and probability of that evidence, there would be little cause to take issue with him.

This, however, does not seem to be the case; what emerges is a series of conclusions which purport to be no more than propositions — "inferences" — hedged about by confessions that they are not categorical assertions. Only negative criticisms of Radcliffe-Brown and his calculations are in this latter category (p.170).

On the other hand it is plain that Butlin's propositions are not merely "counter-factual" historical stuff such as the dreams of American historians of railways are made of e.g. an imaginary world chock-full of 20th Century benefits that *might* have been achieved without wasting money, land, labour, iron and steel on railway systems. He believes that the black population in 1788 *was* a great deal larger than his predecessors in such expertise had supposed it to be. Equally plainly, and naturally, he expects or hopes that others will find themselves agreeing with him. He believes that this means moving away from that perception which he has described as "comfortable" but which, if this book is worth writing and reading, is untrue and must therefore be unacceptable.

Let us clear our minds: this is an exercise in persuasion as well as in economic modelling. The next step is to see whether the evidence adduced by Butlin for an important re-reading of history is acceptable. To be acceptable — and here Butlin is surely correct in the earlier statement quoted above — our approach must be objective, impartial, dispassionate and free of moral or political bias, particularly that imparted by hindsight. In a controversy fraught with all the risks of topical emotion (and bitterness) the evidence must be solid and irrefutable. History must be more than hearsay; in so far as hearsay may be involved, it must be treated as open to question, and all witnesses, whatever their views, whether they are black or white, must be treated as equal. If we are to demand of history that it should be something more than the opinion of one man, one party or one school of thought, more than propaganda or guesswork, the evidence upon which it rests must be subject to laws of evidence at least as strict as those applied in a court of law and acceptable only after scrutiny as severe as that employed in the most rigorous legal cross examination. Are these conditions fulfilled here?

Butlin came to the problem of estimating the size of the black population of Australia by accident. His article jocularly entitled "Yo Ho Ho and How Many Bottles of Rum?" in 1983 led him to ask questions about Aboriginal consumption of alcohol. This led him back to Radcliffe-Brown, and to the search for what he calls "plausible" replacement of Radcliffe-Brown's figures. Although the study of population in this context was new ground, Butlin explores the technical possibilities and problems of his subject with all the confidence of the veteran statistician (Chapters III–VIII inclusive).

The problems are, nevertheless — even for him — fearsome, and the fact shows through in the character of these chapters. This is a speculative study throughout, but nowhere more so than in this section where the author's context of institutions is woefully deficient in hard fact. It is many years ago since M.M. Postan remarked in his inaugural lecture as Professor of Economic History at Cambridge that "advances" in subjects of a socially-scientific kind are usually "small and uncertain". He was thinking of situations for which relatively abundant information is available. But his observation was correct. French, and now British, historical demographers, deploying new and refined techniques on immense quantities of detailed evidence, are still juggling inconclusively with birth rates, death rates, age of marriage, male and female, relative importance of men and women in demographic progress and so on. (In passing, it is worth noting that the traditional picture of continuous demographic advance of European population since the recovery from the decline of the Roman Empire has gone. Europe (like Australia) was subject to *cyclical* rises and declines.)

In Australian history at its inception, solid evidence is so rare that the only "conclusions" that can be reached must be either inferential, or interpreted, as fairly as may be, from such personal observations as exist. The most obvious source of information on what Butlin describes as the first and second epidemics of smallpox (which he sees as the strongest support for his belief in a "large" 1788 population) come from (a) statements by members of the First Fleet or later settlers or (b) inferences from a combination of personal statements *and* economic and statistical models.

Incontestable evidence of the epidemic of 1789 in New South Wales is to be found in the diaries of Watkin Tench and other officers of the First Fleet. Tench, in particular, speculated on its origins. Did the French under La Perouse bring it? Had earlier European voyagers brought it? or the First Fleet? The surgeons certainly carried with them in bottles materials for inoculating "suspected persons"; but to infer that the epidemic "was produced from this cause were a supposition so wild as to be unworthy of consideration".* The Tench diary is mentioned in Butlin's bibliography but given no credit in the text.

Tench's editor, Professor Fitzhardinge, in his immaculate commentary, did not question Tench in his exoneration of his fellow officers from any suspicion of spreading the disease. He also agreed with him that Captain Phillip had stated that no case of smallpox had occurred on the voyage. He recognised only the possibility that it might have been brought in by the First Fleet "in a latent or weak form, unrecognised as such . . ." (p.305-306). This may or may not have been correct. Professor Butlin, however, is unable to resist the attractions of a melodramatic *deus ex machina*. While he concedes that Phillip could hardly have been so wicked as knowingly to permit or arrange deliberate infection himself, he believes that "*it cannot be beyond reasonable bounds that Phillip was pressured into action*".

Having no evidence whatever that this inference was correct, he can only support it by reference to North America where there is fragmentary evidence from 1763 that deliberate infection with smallpox was used to try and get rid of unwanted Red Indians. By the time this idea has been milled about in the author's mind, and no alternative explanation has been found, it becomes the final general inference No. 6 amongst the concluding summary of the study — that it is "*likely that the infection of the Aborigines was a deliberate exterminating act*". This must have been the joint responsibility of the Commander and his Officers: clean contrary, it should be noted, to the solemn and sacrosanct Commission of duty to the Aborigines by which they were bound by the Crown, a Commission worded by the very authorities who (Professor Butlin assumes) now ordered them to commit genocide. Who else?

The resulting epidemic of 1789 is then credited with a mortality of an order comparable with the disastrous ravages of smallpox amongst the Red Indians.

Shock tactics indeed! They make strange comparison with the methodology of the author in his other mood — the logical, statistical searcher after knowledge of the Australian economy, cautiously feeling his way forward through a jungle of probabilities, amassing his evidence for his decisions with scientific prudence.

The first smallpox epidemic is perhaps the most critical single factor in the Butlin study, for the author singles out smallpox as the undoubted prime factor in the reduction of his large 1788 population of Aborigines from a quarter of a million in 1788 to 10 or 15,000 by the mid-19th Century. In this, it is worth remarking, he parts company with many historical demographers of Europe who are ready to admit that they are unable to calculate how bubonic and pneumonic plague, and smallpox, compare in their *long term* effects with a large concourse of other killer diseases and infections which appear to have been an almost continuous cause of mortality in Europe down to the 19th Century, especially that amongst mothers and infants. In Europe the mortality of children under the age of 5 was commonly regarded as a permanent fact in society from the 16th Century onwards. Four deaths out of five was often said to be the average. This was still the case well into the 19th Century in many areas.

This is not to suggest any exact parallel in Australia amongst the Aborigines; but to place (as Butlin does) smallpox a long way first, venereal disease second and other diseases nowhere, is a bold move where no evidence exists in any unquestionable form, and where direct observation is largely ignored. For I do not believe that any reasonably objective reader of the despatches, diaries and letters that emerge from the top *échelon* of officers of the First Fleet can come to any other conclusion than that the accusation of *deliberate* infection of the native population with smallpox is anything but grotesque fantasy.

" 'No one knows how many blacks lived in Australia from 1788'."

It is at this point that one begins to recall the author's early remarks (p.3 Chapter 1) where he gives the first evidence that his claim to be free of moral or political prejudice is less clear than it might be. The description of whites fighting, killing, introducing prostitution, alcohol, using even missions and protectorates as "death inducing resettlement devices" switching funds which should have gone to blacks to "perks" for whites, and — most important — deliberately transmitting Western diseases, is untrammelled by any modification or qualification. It is paralleled in other parts of this study by similar imbalances. There is a recurrent tendency to accept black evidence as evidence; white evidence is often dismissed as propaganda, exaggeration, or the unreliable mumblings of the irretrievably unintelligent.

It is difficult at certain moments in this study not to ask whether the author's judgements in non-statistical matters are as exigent as his invariably immaculate calculations where numbers, proportions and mathematical models form the stuff of his argument. The explanation of the "1789 epidemic" in New South Wales (where it undeniably existed in a virulent form) is extremely important. For it leads us, under the author's guidance, to another epidemic, this time in what was to be Victoria and perhaps South Australia. Butlin writes about this as if (at certain points) it was exactly contemporary in its incidence and length to the outbreak in New South Wales; elsewhere, it is merged vaguely into a second epidemic which broke out in 1829 but with which it had

* Watkin Tench, *Sydney's First Four Years* Ed. L.F. Fitzhardinge, 1961 (p.146).

vague and unspecific contacts. Diagram I (p.23) refers to this as the *First Small Pox Epidemic 1789.* What is the evidence? In Victoria it is said to be "pock marking" observed decades later. Much farther West and North there is said to be further pock marking evidence, mass graves and black memories of disease. When this alleged epidemic is discussed in Part C (Chapters 11–13) it is merged with references to disused cooking mounds, unused ovens, suggesting massive depopulation. Disregarding the adage that old men not only forget but invent, the author calls to mind, for sympathetic consideration, the elderly Aborigine who could remember the days when his people were as thick on the ground as cockatoos. Ingeniously, Butlin stirs together a potion composed of black memories of a Golden Age, a few archeological relics which have made themselves evident to amateur archeologists; blending the reality with the process known to demographers and statisticians under the useful if infelicitous title of "backward extrapolation", he produces, as the proverbial rabbit from the hat, another vast area of Australia where the Aborigines could thank the white man for introducing them to virulent mass killer diseases.

Two comments on the epidemic in Victoria "in 1789": First, nothing can confidently be dated — pock marks, disused ovens, elderly memories — any of these could well refer to a period *after*, even long after, 1789. If it is difficult in all this mass of speculation, to give a positively affirmative answer to any major question, it is equally dangerous to assert negatives and deny that any particular phenomenon could have happened at all. All the same, one is bound to ask — who carried the "1789 epidemic" from New South Wales to Victoria and beyond?

It was to be almost another quarter of a century before the white "aggressors" peered over the Blue Mountains. Is it likely that infected blacks could have staggered all the way from New South Wales over the mountains to Victoria or beyond? Could the disease have been carried by the French explorers on their way to Tasmania? Or sealers and whalers from America? Not by 1789, would seem to be the answer. Given, especially, that smallpox infection demands an unbroken human chain. This particular epidemic "of 1789" in Victoria and beyond seems to be one extrapolation too many.

As a statistical exercise, Butlin's study is a virtuoso performance. Its management, not to say manipulation, of material compels admiration; but the admiration — in the case of this reader at least — is reluctant. The major defects in the Butlin argument are, first, the willingness to create a *prima facie* case for or against parties to the conflicts inherent in the Aboriginal demographic story without plausible, let alone adequate evidence, sometimes without any evidence whatever.

The argument that a large population of Aborigines in 1788 was reduced to a fraction of its size some 60 years later by deliberate genocide is the most serious case of a grotesque twist in the Butlin logic. Obstinately rejecting all suggestions that the major disease responsible could have been introduced from other sources, Butlin plunges in to accuse Arthur Phillip and his principal officers — in face of all the other evidence that in fact they carried out their Commission to protect Aborigines and live in amity with them with unbelievable patience and at great risk to their own lives. (Phillip himself very nearly lost his own life in a murderous attack.)

Second, the account of the so-called smallpox epidemic of 1789 in "Victoria" is fundamentally muddled and misleading. It is more probable that the precarious evidence of a smallpox epidemic in that place relates to outbreaks at considerably later dates.

It is possible that such arguments may still allow that the Aboriginal population was larger than the Radcliffe-Brown figures suggest. It cannot be consistent with the *genocide* theory which refers specifically to *1789.*

Third, Butlin's insistence on the primary role of smallpox in reducing the Aboriginal population seems to be associated with the insidious attraction of that particular disease for those who wish to place the responsibility for Aboriginal decline on the white aggressors. Venereal disease (which comes a poorish second in the process of deliberate destruction) was already present in Australia and throughout the South Pacific. A considerable number of other diseases may also have been present. Butlin's argument for dismissing them as relatively unimportant is unsatisfactory. Conversely, his persistent contention that the Aboriginal exposure to smallpox must necessarily have been comparable in its general results with that of the North American Red Indians half a world away is a long shot, and a debatable one, to say the least.

To reject such arguments as doubtful inferences from ingenious but fragile pseudo-statistical models does not mean that one dogmatically rejects all suggestions that the white settlement of Australia can be totally isolated from the subsequent reduction in Aboriginal numbers and well being. This is an unhappy fact of Australian history. It is only to reject the contention that the men of 1788 must carry the whole responsibility. If *British* settlement had not taken place, it is almost certain that some other, not necessarily better, form of settlement would, following Cook's discovery. The European theatre of war and diplomacy was dominated at this time by the desperate struggle between England and France. The British settlement by convicts was not a completely voluntary act. It took place under the pressure of Anglo-French rivalry and was shaped by the contemporary belief that the attachment of a defenceless, therefore for practical purposes "vacant" colony could be justified to the outside world only if the occupation was "effective". There is no question; the bitterness of the dogged, long-drawn-out debate in the years before 1788 is adequate proof that convicts would not have been sent if another more voluntary and satisfactory form of settlement could have been devised. It proved impossible to devise anything better, and it is still difficult to think of any other solution. The French themselves came round to the view that a convict settlement in the area of the Great Southland was an excellent idea. But by this time it was too late for them to put it into practice in Australia. They proposed, therefore, New Zealand and the proposal was only nipped in the bud in the nick of time.

Professor Butlin does not seem to me to have disposed of the possibility, indeed probability, that Aboriginal decline was at least hastened by weaknesses or propensities inherent in Aboriginal society itself, as well as by the erosions of smallpox originating with infected islanders visiting the North Coast with the monsoon. (A comprehensive revision of the purely medical character of this confluence of infections by Judy Campbell coincided with the publication of the Butlin thesis in 1983. It adds an important new dimension to the controversy.)

The continuing validity or otherwise of an 'internal' explanation of Aboriginal decline brings us to Butlin's attack on Professor Geoffrey Blainey which forms most of Chapter VI of the Butlin study. The Blainey view is that the blacks were highly warlike, not only towards whites but towards each other. Warfare was, therefore, a major factor in restraining the population growth and presumably reducing the Aboriginal population from its 1788 figure. Butlin does not see strong substantiation for this view. Using standards less exigent than those he applies to the white aggressors, Butlin here insists — and he is right — on the need for more evidence.

Watkin Tench was probably the most civilised, most educated of Arthur Phillip's officers. He was a man of the world, equable in temperament, sound in judgement, a man who pondered his words, of keen observation of people and things, articulate, possibly the best observer in the First Fleet. Tench knew he was writing history. He is therefore entitled to our respectful attention — just as Professor Butlin is entitled to enjoin upon us if he wishes that Tench represents only one man's opinion.

Tench was certainly not hostile to the natives. They were, (he wrote) "a people for whom I cannot but feel some share of affection . . .". By the time he wrote his description of them, his experience of negotiating with them had left him a trifle disillusioned with that concept of the noble savage which he had learnt from the fathers of the Enlightenment. Tench was never condescending. That the Aborigines had some elements of duty and conscience was a belief he held on to, but he had no illusions about Banneelon, Colbee, or most of the others. They were lazy. They were, when it suited them, untruthful. This he accepted. But equally, they were not stupid. "All savages hate toil, and place happiness in inaction . . . Hence they resist knowledge and the adoption of manners and customs differing from their own"; and so on. Tench (in short) was fair and reasonable. When he comes, therefore, to their less engaging characteristics, he is also to be listened to.

The Aborigines were, for example, intrepid, quarrelsome and warlike; but they were not implacable in seeking revenge. They were superstitious but they were not debauched. The barbarous ferocity which one tribe showed to another (and occasionally to white intruders) was unfortunately turned loose on their women. And the women were

> in all respects treated with savage barbarity; condemned not only to carry the children but all other burthens, they meet in return for submission only with blows, kicks, and every other mark of brutality. When an Indian is provoked by a woman, he either spears her, or knocks her down on the spot: on this occasion he always strikes her on the head, using indiscriminately a hatchet, a club, or any other weapon which may chance to be in his hands.

(At least a century later, Steve Fairbairn — like Tench an admirer of much in Aboriginal character — could give a very similar account of their sexual relationships in his *Fairbairn of Jesus* (Ch.10) 1937.)

Such treatment of their own women went on alongside a good deal of rape of women belonging to the enemy in war. Tench particularised his general remarks about the native attitude to women by describing the heartrending treatment which the favoured Aboriginal "ambassadors", Banneelon and Colbee, meted out to their wives. Banneelon, a particularly intelligent, shrewd and resourceful fellow, pursued his wife for days at a time intent on murdering her. He was thwarted only by the Governor and his officers. Tench quotes several other examples. The land which Butlin calls "pre-contact Australia" was no Utopia.

Take the view of women and their place in Aboriginal society for what it is worth, the view of one man, but a very fairminded and dispassionate observer. Add to it the observed effects of smallpox and the various forms of venereal disease deriving from whatever origins — and they are all uncertain. There is general agreement, including, as I understand him, Professor Butlin, that all these diseases affected persons of different sex and age differently. But women — the pregnant, the childbearers, the potential mothers — were victims common to them all.

Venereal disease alone could result in their being either incapable of sexual intercourse or incapable of bearing a live child, or being infertile. The other principal victims of smallpox, especially, and the specifically childhood-afflicting diseases, were infants up to the age of five.

Put these horrifying impediments facing women and children together. Have we not here some part of a possible answer to the question — why did the Aboriginal population of 1788 (whatever it was) decline (to whatever it became) by 1850? And let us note in passing that Tench was writing (although he did not publish until a few years later), in the period between the landing of the First Fleet in January 1788, and the outbreak of the smallpox epidemic in the Sydney area in April–May 1789. He was, therefore, one of the few people who saw and accurately described a small part of the Aboriginal world and society on the eve of its submersion by outside influences. "For all practical purposes," Professor Butlin writes (p.5), "economic historians have been silent on the subject of Australian race history." Somewhat confusingly, he goes on later on the same page:

> Traditional wisdom among the general historians has been that a small black population, making little impact on the natural environment, largely 'faded away' before the white man. In particular, blacks offered little in the way of resistance to whites.

Here, as in a number of other places, his statement of broad historical situations is less helpful than the rigorous logic with which he accumulates evidence for conclusions derived from his models. Evidence of black resistance to whites is freely scattered about, all over different kinds of Australian history. If "economic" historians have indeed been silent, it is largely because they have been particularly conscious of that lack of statistics which has propelled Professor Butlin to write the kind of book he has written: what he calls (p.7) "hypothetical history", based on "plausible" assumptions. Accordingly he is compelled to explain that his "conclusions" (presented in inverted commas) are intended to suggest "not a description of what actually happened but of what . . . might plausibly have been". Thus he has to warn readers (p.8-9) that only his Chapter II, discussing disease background, deals with what he calls "real" history.

The trouble with this presumed division of history into real and hypothetical, based in turn on assumptions and the models which follow from assumptions (assumed to be "plausible" assumptions) is to know where the one begins and leaves off and the other starts and takes over. It is not difficult for a hypothetical historian to state his intentions and assumptions in a preface. It is possible for him to state his "inferences" in a final assessment (Chapter XV). It is easy to say that of the fourteen inferences, only one — the total destruction of the late Professor Radcliffe-Brown's population figures — is to be treated as a categorical assertion (by which he presumably means "real" history). Yet the rest "need to be taken seriously". It is claimed that the book, lying between preface and epilogue, is based on principles of "broad consistency" which cover all the other aspects of Australian history where the coming of whites "de-stabilised" (this is the claim) the total structure, and therefore size of the black population. Yet what is a "plausible assumption" to the cook may be highly indigestible to the guest. This reviewer is unable to stomach the assumptions about the perpetration of genocide by the first "aggressors" for which there exists no evidence. This particular "inference", which the author uses to justify an accusation in itself, and from all other evidence, quite unjustified, is made in spite of the fact that he cannot know, as he admits, whether smallpox and venereal disease came from sources other than British.

To Professor Butlin this may be the right way to write hypothetical history (the history of what might have been): to this reviewer it is the way to write a history of "what might not have been" — or, at the very least, it is not the way to write a history "of what might have been" which is in the least convincing to the student of such real history as can also be based on existing real evidence, limited as this is.

The danger is that for all Professor Butlin's efforts to explain that he is writing imaginary history, there will be many readers who will, in their growing confusion as to the distinction between "real" and other history set out between Chapters III and XV, mistake hypothesis for reality, imaginary history for real history.

If, as is possible in an age of general decline in primary and secondary education, imaginary statistics are to go hand in hand with careless reading, how long will it be before we learn from "history" books for use in schools in Australia and overseas, compost of cartoons and scissors-and-paste, something like this?:

> The First Governor was a man of good intentions: but it seems almost certain that under pressure he weakly gave in to the powerful pressure of others and gave orders to spread infective material among the Aboriginal population, then numerous, prosperous and happy. This spread smallpox (while white sailors spread syphilis and gonorrhea) all over southeastern Australia, leaving only a small fraction of diseased and poverty-stricken Aboriginals alive by 1850.

This unwitting establishment of a new myth might be avoided if on his last page the author had repeated his earlier *dictum* that "no one knows how many blacks lived in Australia in 1788", and added, "and no one knows with any certainty how or why their numbers changed by 1850".

POSTSCRIPT

Since the above comments on Professor Butlin's *Original Aggression* were written, a number of events now call for discussion. Judy Campbell has contributed an important article, "Smallpox in Aboriginal Australia 1829-31" (*Historical Studies* Vol 20. October 1983). On this Professor Butlin has commented in his "Macassans and Aboriginal Smallpox" in *Working Papers in Economic History* No. 22, A.N.U., May 1984. Richard Broome has contributed an interesting note to *The Age* (10 Nov., 1984) "Swift End to 50,000 years of Good Health".

Briefly, Judy Campbell concludes that the disease which afflicted the Aborigines most severely *was* unquestionably smallpox. It was transmitted to them not by European colonists but by Macassans.

Mr Broome still prefers the alternative proposal of Professor Butlin that it spread from the First Fleet in Sydney. He thereby seems to come perilously close to turning Butlin's imaginary history into real history. Thus. . . . "It is a terrible thought, but Governor Phillip was under extreme pressure in 1789 . . . etc." Just as I feared. (See above). Too late; Noel Butlin has written in his *Working Paper*: "I have not, in my book, proposed that it (the infection) was deliberate." Then — somewhat contradictorily? — "if it occurred deliberately, transmission might have been the work of some embittered or deranged individual".

Professor Butlin seems to me to do nothing to strengthen such possibilities as may exist of a 1789 epidemic in (pre) Victoria. On the other hand Judy Campbell is (understandably) able to envisage an outbreak, given that it must derive, like all smallpox in Australia, not from British sources but from the impact of Macassan visitors, conveyed by a direct chain of Aboriginals.

The plot thickens. A first, southern (pre-Victorian) smallpox epidemic might conceivably be of Australian provenance, non-white (i.e. Macassan-Aboriginal) or white (possibly American(?)) sealer or whaler origin. If it was the latter, however, it cannot possibly be dated earlier than (say) 1802.

The likelihood that the "1789" epidemics in Sydney and "Victoria" were linked through white responsibility deriving from the Governor or someone under his authority has now (*pace* Mr Broome) been shuffled out. In spite of Watkin Tench's emphatic dismissal of infection from any First Fleet medical source, however, Professor Butlin insists on keeping his anonymous madman as his last card. But his major preoccupation, he still insists, is population size. The origin of the epidemics is purely "a side-issue''. Nevertheless, the impact of smallpox on population size remains crucial. The timing and character of the ''Victorian'' outbreak must affect *population size*, to say nothing of his criticisms of Radcliffe-Brown's calculations.

Here we are back with fundamental confusion; for this is the one point in Professor Butlin's book which (one gathers) is not mere hypothesis.

Yet the size of population depended on mortality, above all smallpox mortality. One out of two of the major outbreaks remains — at least as to area, timing, origins and size — totally hypothetical. We are back with the dilemma that we are asked to accept as part of ''real'' Australian history, irremovable conclusions derived from speculations which are entirely imaginary or inferential.

C. Wilson, 'History, hypothesis and fiction: smallpox and Aboriginal genocide', *Quadrant*, vol. 29, no. 3, 1985, pp. 26–32.

Noel Butlin

7.4

ABORIGINAL DEPOPULATION

I HAVE ALWAYS ABSTAINED from response to reviewers and even from correction of misapprehensions in subsequent writings. However, there comes a point at which, if only in the public interest, reviews become so distorting that a response may be appropriate. This seems to me to apply to Professor Charles Wilson's review article on my book *Our Original Aggression: Aboriginal Populations of Southeastern Australia 1788-1850* (Allen and Unwin 1983) in your March 1985 issue. It is also relevant to comment on some public notices, not so much of my book as of claims made about it, in the Australian press, most notably that by Hugh Morgan in the *Age* (26 and 29 January 1985).

Wilson's review and Morgan's comments, as published, are so grotesquely misleading as to produce mischief. Morgan accuses me of being part of a campaign to denigrate our "ancestors" and specifically to accuse Governor Arthur Phillip of deliberately practising genocide against Australian Aborigines. Wilson asserts that "Butlin plunges in to accuse Arthur Phillip and his principal officers" of the same deed. Wilson, in referring to a later *preliminary* working paper by me (a reference that should never have been allowed in a reputable journal without my permission), claims that I have retracted this alleged charge against Phillip. He says that I am now relying, to sustain *his* alleged charge of deliberate infection, on some "anonymous madman".

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Wilson predicts that schoolbooks will make some confused rendering of my alleged statements. On the contrary, it is he and Morgan who have made the allegations and they who will bear the responsibility. I have made no such allegations. To avoid any further confusion on their part, I also ask them to note that I am also not withdrawing anything (because I have nothing to retract).

I sympathise with Hugh Morgan. He is, after all, a very active public speaker and there may well be times when even he could be desperate for some means of attracting attention. In this particular case, as a senior mining executive, he is also preoccupied with Aboriginal land claims. Were he to read my book carefully and think about its implications, *on the assumptions that they can stand up to testing*, it might begin to dawn even on him that the inferences that I drew as probabilities are far from supporting the land claims of modern Aboriginals.

I sympathise also with Charles Wilson. Apart from the particular British attitude that he carries (so innocently that British imperial endeavours go far with him to justify Aboriginal depopulation), he has other limitations. As a life-long business historian, accustomed to the security blankets of masses of documentary evidence, he has been promoted in his role of reviewer of my book far beyond the level of his understanding. He displays a profound ignorance, as well as the ignorant prejudice of many whites. He concludes for example that, *because it took the early settlers a quarter century to cross the Blue Mountains*, blacks would have had enormous difficulty in "staggering", when infected with smallpox, all the way from Port Jackson over the mountains to Victoria and beyond (p29). Dear me. Here indeed is a mess of pottage. He knows nothing of Australian terrain, nothing of Aboriginal communication systems, nothing of the mode of transmitting smallpox, nothing of the duration of the illness. But he does know all about my "grotesque twist of logic", the nature of my "allegations" and, apparently, some deep underlying political "purpose" in writing my book.

In considering our history, it is time that Australians (and British) grew up. That time is greatly overdue for Morgan and probably past for Wilson. I have no guilt about Australian Aborigines of old and — though some may think that I should — none about modern Aborigines. I was not present in 1789; and I have no role in Aboriginal affairs today. It happens to be my basic belief that modern Aborigines will get what they work for not what anyone — Morgan or our Government — hands to them. As a nation, we need to try to view our history dispassionately and come to terms with it. There is far too much emotive nonsense in a great deal of historical writing and reviewing and far too much concern with catch-words and the invention of "atmosphere" as an art exercise rather than with scientific evaluation. This does not help us to grow up. The absurdities of publicised misrepresentations by people such as Morgan and Wilson equally stand in the way of maturity.

Wilson views the progress of human thought only through narrowly focussed piecemeal research on documents, myopically building up an edifice bit by bit, gradually correcting the architecture and eventually yielding the perfect, precise, final structure. This tunnel vision would indeed have stood in the way of a vast mass of human progress over the past centuries. There are several other and often more powerful techniques available to us. One that is widely used is to propose hypotheses and to test their implications. My book is an example of this procedure. Typically, one cannot refute a hypothesis directly but only test the results of using it. There are many areas of science in which no conclusive answer can be given as a result of testing or at any rate no conclusive answer with available testing methods and we must rely on probabilities. Where does Wilson think that most of the physical sciences or such areas closely related to my book as archaeology would be without this type of approach? In making and testing such hypotheses, these areas of thought have made prodigious advances and, in the process, cast away a great deal of earlier thinking — in other words have demolished whole edifices, not patched them up.

In using this latter approach, *as was necessary in Australian circumstances,* I do not think that my book has demolished much in the way of an edifice. Nevertheless, Australian historians and anthropologists have generally accepted the estimates of Aboriginal populations before white contact as about 300,000 for Australia as a whole, about 11,500 for the present Victoria and about 55,000 for the present NSW. These represent very thinly spread numbers indeed. The conception of this extremely low concentration is vital in anthropologists' interpretations of the entire fabric of Aboriginal society. If that low concentration is exposed to serious doubt so too may be many of the interpretations of other aspects of Aboriginal culture.

American anthropologists and archaeologists for long accepted a similarly low concentration in the American hemisphere (not merely the United States) before white settlement. This acceptance has recently been under severe challenge and it would appear that there is now wide acceptance of a quite radical reassessment. In place of the older guess of a 700,000 pre-contact population north of Mexico, the more venturesome prefer 12-15 million in the present USA and Canada and the more conservative accept 5 millions. It is recognised that any of these revisions imply drastic reconsideration of many features of American Indian culture.

Professor Charles Wilson displays profound ignorance of this debate given his quaint comments on my "plausible assumptions" which are based on world epidemiological and demographic experience. Nor does he show any comprehension that this world experience is vital *evidence* for Australia. (This is not surprising in a man whose career has been concerned with European business history.) There is no known group of humans who were ever able, on first encounter, to resist smallpox. The mass death rate is replicated over and over again in relation to encounters with smallpox. If Wilson rejects the power of this evidence he must clutch to some peculiar but unspecified characteristic of the biology of Australian aborigines.

This would, indeed, be fiction. My *hypothesis,* in the respect, was: let us suppose that, when infected with smallpox, Aborigines were affected similarly to other primitive societies in early contact with whites throughout the "new worlds" of the American hemisphere during and after the 16th century. Hardly a daring hypothesis! To Wilson, this is fiction. So much for business historians.

If Americans can debate such issues seriously, why do writers and reviewers in Australia have to sink to paranoia and accusation when *a more conservative hypothesis and implied outcome* is suggested for Australia than for the United States? I have made only one firm statement in my book: I am confident that the traditional Australian estimates for Southeastern Australia are radically understated. This conclusion does not depend only on the modelling in my book. The general modelling was designed to deal with the manifest weaknesses in the process of achieving the received estimates and because the author of the estimates (Radcliffe-Brown) recognised the problems but did not know how to tackle them. All he did was to declare *that he wanted to get a figure that was so low that no one could possibly accuse him of exaggeration.* I cannot be confident that either my assumptions or the resulting orders of magnitude of populations are correct. What I have done is at least to make a first try at something that should have been attempted decades ago. In doing so, I realise that I am at the very limits of experiment, trying procedures that no-one else has tried. The problem and the methods are different from those of European demographers, despite Wilson's confusion on this particular issue.

What were the problems of estimation? Radcliffe-Brown, who made the accepted calculations, was a young English anthropologist who was briefly in Australia in the 1920s before going to an academic position in the United States. He confronted the following. The NSW Legislative Council sought, in the early 1840s, the estimated numbers of Aborigines currently in various areas of Police and Land Commissioners. The returns, with some supplements, included

(a) current approximate enumerations for most of the present Victoria aggregating *to range of 12-14,800*;

(b) information that, *over the preceding 5 years,* the population in the areas *had fallen by about a quarter to a third*;

(c) advice that, in some areas, only adults were dying *because there were no children*;

(d) the statements that there was a radical sex imbalance, with many fewer women than men.

Radcliffe-Brown also acknowledged that there had been some prior killing and land occupation by whites after the first settlement in Victoria in 1835; that venereal disease was common amongst the blacks and that there had, as he accepted it, been two epidemics of smallpox across most of Victoria about 1789 and 1830. With this before him, he incorporated into his Australia-wide estimate of *pre-1788* blacks, a Victorian figure of 11,500 — *below the lower limit* of the Legislative Council returns in the 1840s. Implicity, but not explicitly, he was saying either that the Legislative Council figures were exaggerated or that the black population had actually increased since white arrival.

It would require a gross exaggeration in the Council's returns on population size to accommodate even to the other information in them, quite apart from white occupation of land, white killing, smallpox and venereal disease. My book took up the challenge of this array of problems and tried to develop a technique of handling them. I proposed that an appropriate order of magnitude for pre-contact blacks in NSW and Victoria *that should be a target figure for testing further* was about 250,000. I do not regret it if the results disturb existing thinking. I can only refer people to the much more mature American debates on precisely the same problem. I do not regret it either if my "answers" are wrong. It would be surprising indeed if most things happened to come right in a first effort that should have been made long ago. The ill-founded conditions of Radcliffe-Brown's estimates have been there for 55 years for anyone to read — and do something about! This, not the causes of two smallpox epidemics, is what my book is all about. It is not a subject in any degree appropriate to a business historian whose research experience has focussed on individual European enterprises.

Wilson accuses me, in making these assumptions in my models, of imagining an early Black Utopia and of preferring information from blacks rather than whites. This is a figment of Wilson's imagination. If there is a Utopia anywhere, it is in the minds of anthropologists but only insofar as they believe that pre-contact Aborigines had (allowing perhaps for long swings) achieved a stable stationary relationship with their environment, given their technology. If this is wrong, a new starting point for my book is needed. So, too, is it needed for much of current anthropology. This is, in fact, one of the main explicit inferences in and challenges of my book.

The reference to my preferring "black evidence" in Wilson's review is more than quaint. Wilson's five thousand word "review" of the contents of my book is a superb example of untrustworthy evidence of white men in an unfamiliar environment. His confused interpretation of black communication difficulties inferred from white problems in crossing the Eastern Australian hills is symptomatic not only of his innocent prejudices and manifest ignorance but of a vast array of similar innocent prejudices and ignorance by early whites (some were not innocent). Henry Reynolds *(The Other Side Of The Frontier)* has amply illustrated the radical reassessments that come when one tries to use black evidence.

But there is also a crucial technical point relating to smallpox that Wilson clearly does not comprehend. This was one of the few human diseases that left an imprint on its surviving victims — facial pitting that could be recognised reasonably readily *by those familiar with smallpox.* There are several problems in this, none of them prominent at Port Jackson in 1789 when white medical residents could identify the disease. After that, there is a little white medical evidence and a great many problems with white commentary.

By the 1830s, a large proportion of settlers in NSW and Victoria had no familiarity with smallpox. Their sighting of pock-marked faces some years after an epidemic therefore leaves a large element of doubt about the location and spread of smallpox. It is only by introducing black evidence that one can bring a significantly greater element of confidence (never certainty) into any assessment, and most obviously when the blacks associated observed pock-marking with a prior disease and population loss.

Wilson tries to propose an alternative explanation of black population decline — wife-bashing by black males and venereal disease. "Put these . . . together," he says. "Have we not here *some part* (my italics) of a possible answer to the question" of the black population decline by 1850? Some part, indeed! What part? What poppycock! Wilson wants to have an alternative "partial" explanation based on, according to him, one man's (Tench's) picture of the blacks in the first settlement. Tench, Wilson says, was not "hostile" or "condescending" to the blacks, was "probably the most civilised, most educated". And "Tench knew he was writing history. He is *therefore* (my italics) entitled to our respectful attention" (p30). At last, Wilson has some papers and some writing to clutch. This is sheer nonsense. Tench's book is undoubtedly charming and quite well-written. Tench's claims to education could not match that of Dawes nor his picture of the blacks begin to parallel that of Collins. Tench, in my view, was an attractive young man, with some impetuous and humane feelings. He did not, as Dawes did, when the two were ordered by Phillip to cut off "ten black heads" in exchange for one convict body, make his protest so clear as virtually to mutiny. Dawes was sent packing by Phillip and so ended Dawes' and the British Government's plans for the development of astronomy in NSW along with a rational design for the town of Sydney. It is precisely because he "knew he was writing history" that we have to treat Tench with some circumspection. (Incidentally, Wilson accuses me of not giving a specific reference to Tench. I suggest that he reads page 20 line 22.) As to wife-bashing and venereal disease, both were clearly common in Britain during the late 18th and early 19th centuries. No one has noticed that the population declined there in consequence.

One could go on with the ineptitudes, ignorance and prejudice ad nauseam in Wilson's review. Let us turn to my alleged "grotesque logic", my "plunging" in with an "accusation" against Arthur Phillip or, in Hugh Morgan's case, with my "vilification" of our ancestors.

Hugh Morgan warrants scant notice. I am cynically amused that he rises to the defence of a bureaucracy. My impression was that he felt (The *Age*, January 29, p11) that the only good bureaucrat was a dead one. Perhaps he has got himself confused and thinks that dead bureaucrats had actually been good when they were alive. If so, he must anticipate that history will judge his current opinions poorly.

I happen to think that Phillip was probably a good choice for the job in 1788. So what did I say in my book that led Morgan and Wilson to make their remarkable interpretations? What I said in any substantive way was, first, that in my judgement the 1789 epidemic was most unlikely to have been due to other than the bottles of "variolous matter" brought (quite properly) to the settlement. The basis of this can be evaluated from my forthcoming article in *Historical Studies*. I cannot detail it here. Summarily, there were extremely large obstacles to the transmission of smallpox over thousands of kilometres from Macassan fishermen on the Northern Australian coastline to Southeastern Australia. The obstacles lay in the nature of the virus, the relevant climatic conditions and the communication system of Aborigines *in the northen monsoonal belt*. By contrast, in the two known outbreaks in Eastern Australia in 1789 and about 1829, there were potential white triggers at or near the spot where *and when* smallpox was sighted by whites. There were no other extensive outbreaks in Queensland, in particular, after the 1830s, even though the Macassan fishermen carrying the virus to the coast from the north continued to come frequently until the first world war. Nevertheless, large black populations remained in Queensland and apparently available for infection throughout the whole period.

We face, then, in 1789 a basic problem: How could bottles of virus, under the control of white officials, pass into black hands? So I wrote (p21) "Excluding, for the moment, the extreme outside coincidence of a northern source . . ." then

> Logically the possibilities are:
> (a) transmission by some authority
> (b) transmission deliberately by convicts
> (c) transmission unintentionally by convicts
> (d) transmission unwittingly by blacks (p21)

I commented on *each* of these very briefly, in turn. Neither I nor anyone else can possibly rule any of these out (including Phillip, despite my expressed judgement in his favour) in explaining how bottles of variolous matter could have been transferred from white control to blacks. Certainly Morgan and, it would appear, also Wilson decided that I should not be allowed to list the first possibility. And when I say that I did not propose that it *was* deliberate *in actual fact*, I am declared to have retracted and, in Wilson's case, to be pursuing some "anonymous madman".

What does "transmission by some authority" mean? I believe that my judgement of Phillip is clear on pp.20-21 of my book. I find it strange that persons repulsed by the possibilities of "deliberate" action ignore possibility (b) above even though I indicated a little of the background of that issue. But their homing in on Phillip merely displays ignorance of what "some authority" meant in early Australia. There were over 200 officials, in military and civil roles, appointed by the British Government; and in addition a considerable number of convicts who were, of necessity, appointed to official positions by Phillip — chiefly because the C.O. of the Marines and the Lieutenant Govenor refused to co-operate with Phillip. These included convicts appointed to assist the medical officers. To imagine that this array of officialdom represented the best of British society borders on the ridiculous. To quote, as Wilson does, Phillip's official instructions is simply inane — they were the same instructions given Empire-wide to senior British administrators.

To believe, as Wilson at least implies, that the military officers were unaware of deliberate policy practices in respect of American Indians and the efforts to exterminate them with smallpox disregards the career record of the senior NSW military command. Wilson should look it up before he embarks on his style of commentary. He does not seem to comprehend the enormous risks that faced the early settlement in 1789. Moreover, to believe that the mass of officials or convicts were unaware of how inoculation proceeded is to be in direct conflict with the widespread and well-recognised formal and informal practices of inoculation in 18th century Britain. To argue that the whites and blacks were disengaged for a long time before the 1789 epidemic requires re-writing the official records to omit the references to physical conflict in the months before and immediately up to the outbreak of the epidemic. And there is, unfortunately, a problem of a major turnaround, some time between 1788 and 1790, in Phillip's own attitudes to the blacks. One cannot simply rule out the various possibilities of deliberate action at some level.

To demand of me, as Morgan and Wilson do, to remain silent about a particular logical possibility, to conceal some considerations, to sweep some possibilities under the carpet and out of sight is an affront to intelligence and reason. I am not, repeat *not,* accusing anyone. I think that all four possibilities are, unfortunately, there and are extremely likely to remain part of our history. I indicated in my book that I would put Phillip very low on my list of conceivable suspects (if transmission was not, in fact, accidental). His senior military and (some) medical personnel are not nearly so easily exempt. And it is, in any event, "easy" to envisage an accident. A question of responsibility for adequate control of the virus would remain (as a matter of logic, not of guilt) if transmission *was* accidental. I accept all four as likely in the absence of adequate evidence. In any event, I believe that the matter of responsibility to keep the virus secure is a serious issue that is not at all easy to escape.

In saying this, I see no reason why any living Australian or Britisher should feel any sense of guilt. Anyone is, of course, entitled to judgement in the absence of evidence. But until evidence is actually produced to prove that no act by officials (at any level) was involved, I am not either able or willing to shorten my list of possible explanations merely because the ignorant or the prejudiced regard one of them, without any evidence, as distasteful. All four, not one or some, remain until proven one way or the other beyond reasonable doubt.

N. Butlin, 'Reply to Charles Wilson and Hugh Morgan: Aboriginal depopulation', *Quadrant*, vol. 29, no. 6, 1985, pp. 30–3.

Charles Wilson

A REJOINDER

PROFESSOR BUTLIN has spoken (*Quadrant*, June 1985): loud, perhaps, rather than clear, but from the clouds of epithets some very useful points emerge. First, he is concerned, not primarily with the causes of two smallpox epidemics but with the revision of estimates of the early aboriginal population of New South Wales and Victoria. These have been widely accepted and are too low.

This is welcome news. The priorities are in better order and I wish him well with his continuing enquiries. It remains only to comment on some aspects of his argument which I cannot help questioning on grounds of fact.

First, my qualifications to write on the subjects handled in his book, *Our Original Aggression*. He states on four separate occasions that I am a "business historian". He makes it pretty clear that accordingly I am unsuitable to deal with the problems with which he is concerned. His judgement of me may be correct but if it is, it is for the wrong reason. I have spent my life teaching and writing history of many kinds. A Bibliography (1983) of my published works prepared by my colleagues in Oxford and Cambridge lists 16 books, 19 chapters in 'collective' works (e.g. *Cambridge New Modern History*, etc.), 24 research articles, 6 editorial prefaces: out of these 65 separate studies, I can identify *one* book on business and — marginally — 3 contributions to collective works. Butlin's statements are totally inaccurate and misleading.

Second, and to me more important: the role of Arthur Phillip in the Butlin scenario, and the wider problems of the role of hypothesis in historical research. Of this latter (as of most other considerations) Butlin thinks I am ignorant. On the contrary, I have long been aware of it and readily accept its utility: with reservations I have accumulated over a long period of examination and discussion in Britain, Italy and the United States. I hoped I made this clear in my article. If I didn't, he can read, for example my exchanges with Donald McFloskey in the *Journal of European Economic History* and my comments on the arguments of the old original counter-factualist, Professor Vogel and his disciples in a number of reviews and discussions. Briefly, hypothesis can in my view be extremely valuable: it can also be extremely dangerous in the hands of the careless. Hypothesis in historical research is usually a last resort where evidence either does not exist or leads to obstinate disagreement. Basically it can offer a mode of comparison and, like all comparisons, has the merit of acting as a stimulating form of suggestion. The danger arises because, in historical situations — more than in the sciences proper — it is difficult, often impossible, to 'rest' with any degree of scientific vigour.

In my personal opinion, Butlin is — for a statistician — apt to conjure up conclusions from comparison and hypothesis which strike me as trigger-happy. White soldiers in America deliberately infected (so we are told) Red Indians with smallpox: why not in Australia too? Governors and commanding officers were regularly given commissions similar to the orders to which Phillip worked: ergo there is a *prima facie* case for regarding all such commissions as nothing more than matters of form. Wife-bashing and venereal diseases were common in 18th century Britain where the population *grew*: why should they cause the aboriginal population of Australia to decline?

I suggest that this kind of "logic" is no logic. I might similarly argue that smallpox, which for Butlin explains depopulation in Australia, *must* have caused depopulation in 18th century Britain. It is a major difficulty of historical hypotheses that, unless they are very careful, they will assume that they can alter, remove or invent one variable in the fabric of history while assuming all the rest to remain the same.

Third, the largest variable remains the human variable. May I repeat Butlin's words: "*. . . it cannot be beyond reasonable bounds that Phillip was pressured into action.*" Note, not "*may*" or "*might have been*" but "*was*". This is not nit-picking: the implication of words, tenses particularly, are important. *Vide* No.6 of his conclusions: it is "*likely that the infection of the Aborigines was a deliberate exterminating act.*" When he lists his "probabilities" in his reply to me, (c) and (d) — convicts or blacks — must be eliminated, being unintentional. That leaves (a) transmission by some authority (the word "deliberate" is omitted) and (b) transmission deliberately by convicts. Why, Butlin asks do I "home in on Phillip"? Is it, he asks, because I don't like "distasteful" evidence? In a sense, yes —

distasteful because my reading of Phillip's whole career, personality and attitude to the duties with which he was entrusted strongly suggest that Butlin is chasing hares. And may I add, in all modesty, that I believe I have read almost everything in print on Phillip (and the hardships faced by him and everybody under his authority which I am accused of ignoring) and read it *at least* as carefully as he has. In this connection I turn to his counter comments on Tench, Dawes and Phillip.

About Tench Butlin is suspicious, not to say patronising: less well educated than Dawes, less observant that Collins? I disagree; but no matter, what is (for Butlin) significant is that Tench didn't protest so clearly "as virtually to mutiny", like Dawes, who did, and was therefore "sent packing" by Phillip. This, at least, is the only way I can read his version of the "punitive expedition" episode. It connects, unquestionably, with his added reference to the "unfortunate ... major turnaround, some time between 1788 and 1790, in Phillip's (sic) own attitudes to the blacks." Ergo one cannot simply rule out "the various possibilities of deliberate action at some level."

Here, once more, we come back to a systematic build-up of the probable (hypothetical) responsibility of Phillip, either as Phillip or as the ultimate "authority" in New South Wales. It has more than once been suggested that "'the ten black heads' in exchange for one convict body" signifies a fundamental change in Arthur Phillip's attitude to the aboriginals: sudden rage and a hardening of the spirit that paved the way for the harassing, the massacres, and so on that our latter-day reformers-in-retrospect regard as "typical" of the "white" *ethos*.

To this interpretation I would suggest as a fair response: read Chapter XII, pages 205-216 in Fitzhardinge's meticulous edition of Tench's *Journals*. This records the murder of Phillip's gamekeper M'Entire, a convict. Phillip ordered out a search party to find the victim. His order included a further paragraph:

> ... the governor strictly forbids, under penalty of severest punishment, any soldier ... ever to fire on any native except in his own defence ... the natives will be made severe examples of whenever any man is wounded by them: but this will be done in a manner which will satisfy them, that it is punishment inflicted on them for their own bad conduct, and of which they cannot be made sensible, if they are not treated with kindness, while they continue peaceable and quiet.

The narrative pursues in detail Phillip's reasoning and strategy, a mixture of his philosophy of deterrence and amity on which he based his own restraint after his attempted assassination and his (apparently) "new strategy of violence". He was convinced (p.208) that hitherto the natives had acted from "having received injury, or from misapprehension". M'Entire's murder was different. He had examined all the witnesses and was convinced it was "unprovoked ... the barbarity of their conduct admits of no extenuation ...".

Phillip nevertheless — reasonable as ever — discussed with Tench his doubts and proposals. Tench suggested that six, not ten, should be captured. Phillip at once agreed:

> if as many as this were necessary he would hang two and send the rest to Norfolk Island for a certain period, *which will cause their countrymen to believe that we have dispatched them secretly*. (my italics).

Two expeditions followed. Both were fruitless. The culprits vanished without a trace. The mutinous Dawes, promoted to moral superiority by Professor Butlin for his criticism of the Governor's policy in fact went on these expeditions alongside Tench. He was not "sent packing" until a year later. That his conscience troubled him (causing him to consult the Chaplain) is true: why shouldn't he? but his doubts were stilled. To treat Dawes as a hero and Phillip as a man whose charity and principles had both deserted him is outrageous. Fitzhardinge's comment cannot be bettered (p319 Ch.XII note 7):

> The object of the expedition was not punitive but deterrent. Phillip hoped by a carefully calculated show of severity to improve future relations ... when Tench suggested a more humane method of securing the same effect Phillip at once agreed.

There was no turnaround of attitudes in Phillip. Perhaps it was the German in him that held fast to the philosophy of punishment as a deterrent, preached by penological reformers from Baccaria to Bentham; perhaps it was the commonsense Englishman in him that bound him to respect the instructions of his Royal Commission to preserve the friendship and welfare of the aboriginals. The flaw in the arguments sometimes advanced by his critics (including apparently Butlin) is to assume that the Governor's responsibility for native relations and welfare can be divorced from his responsibility for preserving law and order. It indicated no change of mind or heart that, sometimes, he had to attend more to one than the other. Crime was crime. It mattered not whether the criminal was black or white. Professor Butlin is perfectly right: "As a nation, we need to try to view history dispassionately ..." Since the First Fleet was at least as much British as Australian, perhaps I may be allowed to suggest we all begin now.

With respect, I beg to differ from Professor Butlin in his retrospective estimations of Tench and Dawes. There was far more to Tench that romanticism or charm. Fitzhardinge's comment seems to me good sense: behind his "style" ... "his real interest was in man and what man would make of the country". (*Introduction to the Tench Journals* pxxiii). He was fascinated by the land and people of Australia as he found them. His whole career (in which his NSW service is only a short episode) was one of alert, bold and intelligent response to challenge and opportunity. Promotions (and trials) were his reward. He ended a very sound career as Commandant of the Plymouth Barracks. Dawes was potentially of similar calibre but for some reason never made it. Probably his weakness was of physique and nerve. He is last heard of in a testimonial to the Colonial Office by Tench (1827), written to support his old comrade's plea for compensation for service in NSW. Its wording is loyal but discreet; it suggests that the differences between Dawes and Phillip were more complex than Butlin suggests — and inevitable in a small community plagued by shortages of everything, including new faces. The original NSW settlement was a decent-sized village, with more than a normal village's normal share of gossip, scandal, jealousy and feuding. (I cannot reconcile Butlin's estimate that the "village" included 2oo "officials". This seems possible only by including all the marines, including ranks. Since most of them were probably barely literate, "official" seems hardly an apt description).

The basic reasons why I find it impossible to contemplate "authority" in NSW being "pressured" as Butlin suggests are two. First, from my reading, I do not believe Phillip's character would have allowed him to condone the infection of the aborigines with smallpox. Second (and more important) to Phillip's *intellect*, such an act would have been pointless, and worse. Quite apart from the possible risks to the white community (which modern medical research would certainly not rule out), the mortality would have been totally indiscriminate. Nothing in Marjorie Barnard Eldershaw's penetrating biography (still the best available) is more compelling than her analysis of the way Phillip always based his decisions over law enforcement firmly on his philosophy of deterrence. This meant that both the criminal (or his tribe) and the crime, were consistently evaluated and the penalty "worked out" in the light of the local circumstances prevailing. The decision and the reasoning were then made public and advertised to the maximum extent possible, both to the officers, convicts and aborigines. Indiscriminate slaughter could have served no purpose whatever in Phillip's scheme of things. Indeed the result could well have been in practical terms counter-productive as well as pointless.

Moreover, in a small claustrophobic society, collusion in such a stratagem would almost certainly have become common knowledge. Phillip never courted popularity. There were those even on his own staff who might not have hesitated to publicise any evidence of the Governor or his confidants' implication in such a scandal. The possibility that variolous matter might have been responsible obviously did not go unnoticed. Equally clearly it gained no credence.

One last point: "he (Wilson) knows nothing of (. . . many things including) the mode of transmitting smallpox, nothing of the duration of the illness". Nothing? Let us say "only what a general (even 'business') historian may learn from the experts". I have read the standard works on smallpox and other socially disastrous diseases as they manifest themselves in Europe. For their characteristics in Australia I have to rely on the experts of the World Health Organisation, the Curtin School of Medical Research in Canberra and the numerous observers, scholars (including especially Miss Judith Campbell) who have studied the spread and ravagings of smallpox from the early days of the colony down to 1980.

I am under no illusions: I fully realise my knowledge is fragile. But then, my priorities are different from those of Professor Butlin — indeed in reverse order from his. My major interest is to deepen our understanding of the original settlement, the objectives, philosophy and human values of the first Governor and his staff, and the deepest problems they faced. Butlin's is the size and movement of the native population. I see signs that in spite of — perhaps because of — our current controversies, a degree of understanding and common sense could develop between our different and sometimes conflicting interpretations of Australia's only history.

C. Wilson, 'Aboriginal depopulation: a rejoinder to Noel Butlin', *Quadrant*, vol. 29, no. 7, 1985, pp. 17-19.

ANU Reporter

MORE EVIDENCE OF ABORIGINAL 'SETTLEMENTS'

Research showing that there was significant stress and disease in pre-contact Aboriginal society in the Central Murray has lent much support to the theory that sedentary lifestyles began to develop in some parts of Australia in the last two to three thousand years.

There is a growing body of evidence, mainly from inland south-eastern Australia, that permanent settlements were taking on in areas that were suitable.

The latest research is presented in a doctoral thesis by Mr Steve Webb, who is lecturing in the Department of Anthropology and Prehistory. He is a biological anthropologist interested in palaeopathology, the study of disease in the past.

The level of stress, or disease, in a population is one way of determining its size and density. As more individuals congregate, hygiene tends to drop and infections and other forms of stress occur more readily.

Mr Webb has studied more than 11,000 bone samples, including 2500 crania, from all around Australia and representing groups from all different environments.

The object was to look for skeletal stress indicators, which can show up anaemia and other conditions, including periods which groups may have gone through 'feast' or 'famine'. Non-specific infections of the bone, arthritis and the presence of parasites can be detected using the technique.

The teeth are also excellent indicators of stress as some develop at different ages. By looking at different teeth, stress can be seen as it occurs at certain life stages of an individual.

Examining breaks in bones, made deliberately or otherwise, and seeing how they have been mended, provides an idea of nursing practices. According to Mr Webb there was a high degree of knowledge concerning the setting and care of broken limbs among many of the groups he studied.

Much of the skeletal material he looked at came from collections in Melbourne and Canberra. The Central Murray area is particularly well represented, leading to a large part of the study being focused on that area.

Mr Webb found that in most parts of Australia stress was lower than in the Central Murray, which at first may seem contradictory given that the area's environment, in fairly recent prehistory, was one of abundance.

'My interpretation is that these people were working harder than lot of others, even in the face of abundance,' he told the *ANU Reporter*. 'This means that a lot of people were sharing this abundance and consequently the population density was much higher than has been thought.

'In fact, I conclude that sedentary living and big populations were the main factors leading to this higher incidence of disease.'

Mr Webb said there were also high levels of anaemia along the NSW coast, suggesting high population numbers.

Mr Webb's findings strongly reinforce discoveries in recent years of sites where permanent occupations were established, as indicated by the remains of village sites and the foundations of small huts. Along the Murray have been found mounds which people used, perhaps to escape flooding. They were probably also used as huge ovens in many cases, particularly in the Central Murray.

This mound-building phenomena, which seems to have occurred only in the past few thousand years, is being investigated closely by a PhD scholar at La Trobe University, Ms Annette Berrymen.

The so-named 'Theory of Intensification' was first proposed by Dr Harry Lourandos, of the University of New England, in the mid-70s, as a result of his own research carried out in Western Victoria.

There has been much work done in North America, using palaeopathological techniques, to show that as Indians shifted from living in hunter-gatherer communities to sedentary lifestyles, which including farming, stress increased and this was concomitant with an increase in infectious disease.

Australian palaeopathology was very much a virgin field till Mr Webb came along. He was formerly a computer technician, and had been working at the Tidbinbilla tracking station. He decided to come to the ANU as an undergraduate and concentrate on Prehistory and Anthropology, particularly biological anthropology. He spent a year at Sydney

University, in the School of Medicine, while still an undergraduate, doing a range of anatomical subjects and work on the pathology of disease.

He returned to ANU to do his fourth-year honours and did his thesis on Palaeopathology and Australian Prehistory, which outlined the disciplines, its methodology, what work had been done in Australia and how it could tell more about Australian prehistory. He also did some field work to give a practical example of the techniques involved.

From this he went directly to his PhD, which he did in the Research School of Pacific Studies, in the Prehistory Department under Professor Jack Golson, and also under the supervision of Dr Alan Thorne.

Mr Webb's work has some other interesting aspects. For instance, he believes that due to the higher population densities in some areas, the overall population in Australia could have been easily in the vicinity of double that which was given as the early estimate of 300,000.

This coincides with the conclusions reached by Professor Noel Butlin, Professor of Economic History in the Research School of Social Sciences, who in his book *Our Original Aggression*, revised the original estimate of the population in pre-contact Australia and suggested a figure of 250,000 for New South Wales alone — not far below the earlier estimates for the whole of Australia.

Mr Webb's work may also contribute to the discussion on the extent to which introduced disease contributed to the reduction in Aboriginal numbers. There has been some suggestion in recent years that Aboriginal numbers were decreasing due to infectious disease even before white settlement began, though little evidence to support the argument has been produced.

Mr Webb's conclusions totally refute the argument. He has been able to show that there was variation in the state of Aboriginal health, with, for instance, such people as those living along the Central Murray being worse off than, say, the desert groups, who were very healthy. Overall, however, he says they were still free from the 'crowd infections' which were introduced by the early settlers, and in fact their population would have been growing.

'The most unhealthy Aboriginal groups were still healthier than the European migrants that came to Australia in those early days,' he added.

Mr Steve Webb pointing to fine horizontal lines on a leg bone — one of the methods of determining levels of stress and disease amongst pre-contact Aboriginal groups.

'More evidence of Aboriginal "settlements"', *ANU Reporter*, vol. 16, no. 6, 1985, p. 1.

Martin Flanagan

OUR PAST BEING FALSIFIED, MINING CHIEF CLAIMS

The executive director of Western Mining Corporation, Mr Hugh Morgan, yesterday attacked "the Orwellian reconstruction of our history", which he said was doing much to strip European settlement of Australia of its legitimacy.

The view was put in an address written by Mr Morgan, but delivered by a Melbourne barrister, Mr Richard Tracey, to the Australia Day Council of Victoria luncheon at Melbourne's Southern Cross Hotel. Mr Morgan could not attend because of a viral infection he contracted last week in Europe.

Yesterday's address follows others on related themes delivered by Mr Morgan over the past 12 months which have provoked heated reaction and resulted in him being touted as the voice of the 'New Right' in Australia.

Yesterday, after stating his belief that "Australia's national sovereignty and the legitimacy of the settlement that began formally on 7 February 1788 is under political threat", Mr Morgan turned his guns on those he claims are responsible for the "almost uncontested falsification of our past".

Mr Morgan referred to a statement by France's President Mitterrand in December that Australia had no indigenous population because it had been killed off.

"Let me make two observations," Mr Morgan said. "First, President Mitterrand would not say something unless he believed it to be absolutely correct; and second, he could only come to this belief if he had been given grounds for it on good authority. There are, I regret to say, many Australians whom the French President could quote as eminent and proper authorities to support his claim. Indeed, from my situation within the mining industry, there seems to be a regiment of public servants and politicians whose activities and statements do much to de-authorise and de-legitimise the European settlement in Australia."

Governor Phillip

Mr Morgan took as his model Governor Arthur Phillip who , he said, deserved the title of Australia's founding father for his "exemplary conduct in the years 1788 to 1792". He related an incident in September 1790 when Phillip was speared by an Aboriginal on Manly beach. Mr Morgan said that despite the attack, Phillip did not send out a punitive party because of an instruction from the British Government ordering him "to conciliate the affections" of the Aborigines.

Mr Morgan then turned to two recent historical references to Governor Phillip. The first was 'Our Original Aggression', a book by the Professor of Economic History at ANU, Noel Butlin, which, Mr Morgan claims, infers that Phillip was involved in deliberately infecting Aborigines with smallpox in an attempt to exterminate them. This, said Mr Morgan, was a gross libel without parallel in the history of Australian letters.

"We have moved from mere neglect of our pioneers to unjustified vilification," said Mr Morgan. "One cannot condemn President Mitterrand's statement given the largely unrebutted state of Professor Butlin's arguments."

Professor Butlin, asked to comment last night, said: "If that's what he thinks I said, good luck to him — he's welcome to his views."

Mr Morgan's other example was Sue Fabian's 'The Changing Australians', a book he said was widely used in Victorian schools. Mr Morgan alleged the book contained an illustration purporting to be Phillip after he had been speared, together with a 1791 newspaper account from the 'New Holland Morning Post', claiming that Phillip sent out soldiers to punish the Aborigines.

Mr Morgan claimed that no such newspaper existed — and that no printing press existed in New South Wales until 1795. A question beside the clipping invited the students to analyse and comment on the imputed personal bias of the purported author of the purported newspaper article, Mr Morgan said.

"The bias, of course, is meant to be seen by the students as sentiment of a racist, anti-Aboriginal kind."

Mr Morgan concluded his address by asking those present to join him in "resolving to recuperate our history as a source of inspiration."

"There are those amongst us, some regrettably in high official places, who seek not a celebration of this forthcoming bicentenary, but who are attempting to turn it into an occasion for mourning and recrimination."

Last May, Mr Morgan claimed that Australian mining companies had Christianity on their side in land rights disputes with Aborigines and appeared to denigrate Aboriginal spiritual beliefs. In September, he delivered a lengthy thesis in which he likened the conservationist movement to the Kelly gang and branded both as enemies of society. Later in the same month, he accused Australia's cultural, intellectual and religious communities of being motivated by an animus to free enterprise.

M. Flanagan, 'Our past being falsified, mining chief claims', *The Age*, 26 January 1985, pp. 1,3.

Hugh Morgan

A DAY TO REMEMBER REALITIES OF HISTORY

This is an edited text of a speech written by Mr HUGH MORGAN, executive director of Western Mining Corporation, for an Australia Day lunch last Friday.

ON this occasion, especially, should we take time off from the pursuit of our day to day activities, to remember our famous men and women, and to praise their courage, their endurance, their faith. Today, I wish to single out one such man, and to consider the way we have treated him.

Arthur Phillip was a quiet and lonely man who faced what seemed to be overwhelming problems. Although he had a very successful career in the Royal Navy, retiring ultimately with the rank of rear-admiral, he was not what we imagine to be the typical British naval officer.

His father was German, probably Jewish, who made a living in London as a teacher of languages.

London was then the richest and the most cosmopolitan city in the world and Arthur Phillip's career in the Royal Navy gives compelling evidence that 18th century British society was a society where talent was at least as important as social standing.

Arthur Phillip's determination, endurance, sense of duty, his exemplary conduct in the years 1788 to 1792, have surely earned for him the title of Australia's founding father.

It seems to me that 7 February, not 26 January, should be our national day, as it was from that date that Phillip's authority was legally constituted.

Now that our national sovereignty, and the legitimacy of the settlement that began formally on 7 February 1788 is under political threat, it may well be appropriate for us to break with the tradition of commemorating 26 January, a tradition which began in 1791, and use 7 February as a commemoration of the fact that the first European settlement in Australia was properly, lawfully, and peacefully constituted, not only in accordance with British law, but also in accordance with international law.

Prior to embarkation in May 1787, much of Phillip's energy and time had been spent in negotiating his commission, and the associated instructions, with Government officials and service chiefs.

There is one instruction especially which, I think, now deserves particular emphasis. It is probable that Phillip had the major part in drafting it and it has been made relevant recently by no less a person than President Mitterrand of France. Let me quote:—

"You are to endeavor by every possible means to open an intercourse with the natives, and to conciliate their affections, enjoining all our subjects to live in amity and kindness with them.

"And if any of our subjects shall wantonly destroy them or give them any unnecessary interruption in the exercise of their several occupations it is our will and pleasure that you do cause such offenders to be brought to punishment according to the degree of the offence."

That Arthur Phillip took these instructions very seriously indeed is shown by the following incident.

On 7 September 1790, word came that a whale had been stranded at Manly beach, and that a party of Aborigines, or Indians as

> ‘Arthur Phillip's determination, endurance, sense of duty, his exemplary conduct in the years 1788 to 1792, have surely earned for him the title of Australia's founding father.’

Collins calls them, was busy carving it up.

One of the Aborigines was Benelong, who had escaped from the settlement some months previously, and who now sent Phillip both his greetings and a stinking hunk of whale blubber.

Phillip immediately set off for Manly in a boat with Captain Collins, Lieutenant Waterhouse and four soldiers.

When they arrived at Manly, Phillip and the rest chatted with Benelong and Colbee for half an hour or so when one of the group, not one of Benelong's friends, speared Phillip through the shoulder, just above the collarbone.

Back in the boat the wound was bleeding profusely. The crew rowed back to Sydney in less than two hours. Phillip suspected he only had a few hours left to live, but Balmain, the surgeon, managed to remove the spear and Phillip was able to resume his normal duties within ten days.

After this near tragedy, not only was no punitive expedition organised, but relations between the Aborigines and the colonists improved. Phillip bore himself with considerable bravery. He never lost his temper. He did not give way to despair. He concentrated on what had to be done, formally, if he were to die within the next few hours.

I have gone into these matters in some detail. I do so because on 16 December last, President Mitterrand of France, a most important person in Western Europe, indeed in the world, claimed on French television that, and I quote: "There is no longer any indigenous population in Australia because it has been killed."

Let me make two observations.

First, President Mitterrand would not say something unless he believed it to be absolutely correct; and second, he could only have come to this belief if he had been given grounds for it on good authority.

We now have to contend with, what can easily be taken to be as malevolence in high places, seeking to recreate our history in such a way that we could, if we believed what we were told, no longer regard our pioneers, our forebears, with any sense of pride nor look to them with any feeling of gratitude.

There are, I regret to say, many Australians whom the French President could quote as eminent and proper authorities to support his claim.

A particular and eminent authority, one which may well have been used to brief the President, and which demands particular attention on this occasion, is Professor Butlin of the Australian National University.

In 1983, Professor Butlin published his book 'Our Original Aggression'; a book in which he discusses various theories and estimates of the Aboriginal population in Australia in 1788 and subsequently.

Professor Butlin is an economic historian with an international reputation and his arguments, his theories and conclusions automatically carry considerable weight, particularly overseas.

Therefore his accusation against Governor Phillip and his officers, and I quote, "it is likely that the infection of the Aborigines (with smallpox) was a deliberate exterminating act", (p 175), is an attack, particularly on Arthur Phillip as Governor, which, I think, has no parallel in the history of Australian letters.

That it should come from such an eminent source makes it a very serious matter indeed. One cannot condemn President Mitterrand's statement given the largely unrebutted state of Professor Butlin's arguments.

Let us be clear about this matter. We have reached the situation where the founding father of this nation, a man whose career shows him to have been an exemplary British naval officer, a man who with an Aboriginal spear through his shoulder behaved with courage, forbearance, and totally in accord with his official instructions, has been implicitly accused, albeit in an indirect and offhand fashion, of attempting to exterminate the Aborigines of Australia by deliberately infecting them with smallpox.

This indictment has been advanced by an historian with an international reputation, and a foreign politician would be perfectly entitled to quote Professor Butlin, at any international forum, as a most reputable authority for alleged Australian genocide against the Aborigines.

We have moved from mere neglect of our pioneers to unjustified vilification.

The genocide through smallpox accusation is not an isolated incident of Orwellian reconstruction of our history. As Roger Scruton, the British philosopher who visited Australia last year has argued the recuperation of our history has become a most important and significant duty for us.

One more example of such reconstruction will demonstrate the ubiquity and dangers of what has been happening.

It is a book widely used in Victorian schools, entitled 'The Changing Australians', and has been reprinted six times. Its author, Sue Fabian, sees the history of Australia, and I now quote Dame Leonie Kramer's description, "as a story of cruelty, exploitation, disadvantage, discrimination and injustice, its victims not only Aborigines, convicts and the poor, but also most women and children".

Miss Fabian describes the incident we have alluded to in which Governor Phillip was speared through the shoulder in September 1790. She reproduces an illustration purporting to be an extract from the 'New Holland Morning Post' of 18 October 1791.

In fact, there was no such paper — indeed there was no newspaper of any kind — for the very good reason there was no printing press in New South Wales until 1795.

I do not think we can over-estimate the dangers which this almost uncontested view of our past has for us. George Orwell tells us, through O'Brien, the secret police interrogator in '1984': "He who controls the past controls the present; he who controls the present controls the future."

Orwell was writing about Stalin's campaign to turn Trotsky into someone who had never existed.

We are confronted with attempts to turn the founders of this nation into monsters. If those attempts should succeed we will have real cause to fear for our future.

In three year's time we will be celebrating, and I use that word consciously, deliberately, emphatically; celebrating 200 years of British and European settlement in this island continent.

Our text for that bicentenary celebration could well be the same text that the Reverend Richard Johnson chose for the first sermon preached in Australia, on 3rd February, 1788. That text was from Psalm 116. "What shall I render unto the Lord for all his benefits toward me". The benefits we enjoy are benefits that have been made possible not just from divine providence but also because of the daring, the heroism, the courage, the endurance, the will, of the pioneers who travelled twelve thousand miles to settle this land.

There are those among us, some regrettably in high official places, who seek not a celebration of this forthcoming bicentenary but who are attempting to turn it into an occasion for mourning and recrimination.

'A day to remember realities of history' (an edited text of a speech written by Hugh Morgan), *The Age*, 28 January 1985, p. 11.

Hugh Morgan

HUGH MORGAN'S REPLY

Sir,

Professor Noel Butlin's response to Charles Wilson's review of Butlin's book *Our Original Aggression* (*Quadrant* March 1985), and to my Australia Day speech (Melbourne *Age* 29 January 1985), is remarkable for its condescension and for its spite. It has the hallmarks of the fury of a man who has been caught out.

Butlin's defence is, essentially, that he is the dispassionate historian, concerned only with the analysis of an historical problem, namely, the origins of the smallpox epidemic that caused such suffering amongst the Australian Aborigines in the late eighteenth century and early to mid-nineteenth century. He discounts any relevance of his analysis to contemporary affairs, and deprecates the notion that he has any responsibility for any consequences arising from his work.

"As a nation" he claims,

> we need to try to view our history dispassionately and come to terms with it.
>
> I see no reason why any living Australian or Britisher should feel any sense of guilt.

In my view these disclaimers, when put alongside his extraordinary methodology, are highly disingenuous. To apply his techniques to a much more recent historical tragedy, the murder of the Jews of Eastern and Central Europe in the early and mid-1940s, would mean postulating a number of wide-ranging hypotheses and regarding them as all of equal value. Further, if one were inclined to favour a particular contemporary geo-political cause, one would dismiss evidence pointing to Nazi complicity in their disappearance. If one did all of this, and at the same time argued that what had been done was not of passionate and profound contemporary significance, would be straining credulity.

Butlin makes much of the concepts of "hypothesis" or of "a particular logical possibility". Theoretically, I suppose, there are very many, perhaps an infinite number of logical possibilities which could be put forward as explanations for this or that historical event.

But in this question of the Aboriginal smallpox, there is one very obvious logical possibility which Butlin has scrupulously refrained from considering. That is the possibility that the "variolous matter" brought out with the First Fleet had lost all its potency during the eight months' voyage, much of which was spent in the tropics. This possibility could readily be tested by asking today's medical experts for their views. However, Butlin has not thought of asking, and we are entitled to speculate why.

An obvious and all too likely consequence of Butlin's hypothesising is to implant into Australian folk-lore the "logical possibility" of intentional, premeditated attempts to kill all the Aborigines around Port Jackson, with smallpox.

Contrast this "logical possibility" with Phillip's official instructions. Contrast this "hypothesis" with Phillip's behaviour in ordering Aborigines suffering from smallpox to be brought into the settlement for medical attention, despite the risks of infection. What is extraordinary, given these contrasts, is Professor Butlin's refusal to resile from defending his "hypotheses" and "logical possibilities".

Let me now turn to contemporary affairs. That guilt is a major weapon in the debates that have been raging over Aboriginal Land Rights is obvious to anyone with the most cursory knowledge of these matters. The words "genocide" and "holocaust" have been used with increasing frequency. Gary Foley was quoted in *The Bulletin* as stating that "asking Aborigines to celebrate Australia Day was like asking Jews to celebrate the advent of the Third Reich".

The Federal Minister, Mr Clyde Holding, used the word "holocaust" in a Ministerial Statement to the House of Representatives in December 1983, a statement that was widely distributed. These words are not used without forethought.

As evidence of the spread of such ideas, the statement by President Mitterand on 16th December 1984 that "there is no longer any indigenous population in Australia because it has been killed", is most significant. These words, and the ideas behind them, have profound contemporary political consequences, and if Professor Butlin does not understand that, he cannot have any claims, in my view, to authority as an historian.

All citizens, according to the common law, have a duty of care. Historians, particularly academic historians, have a professional responsibility over and above that of the ordinary citizen.

Professor Butlin cannot be surprised if his attempts to disclaim responsibility for the consequences of his work meet with hostility and strong criticism. As an observer of Australian universities, I think it is very disappointing that only one Australian academic historian, to my knowledge, has sought to challenge Professor Butlin's mischievous nonsense.

HUGH M. MORGAN,
Melbourne, Vic.

H. Morgan, 'Hugh Morgan's reply', *Quadrant*, vol. 29, no. 7, 1985, pp. 19–20.

Paul Sheehan

THE RIGHT STRIKES BACK

BEHIND the elm trees at the business end of Collins St., at No. 360, visitors walk through a tall foyer with marble walls and a marble floor and lighting that bathes the space of perpetual dusk. The centrepiece of the foyer is a large, trite sculpture called *The Universe*.

Visitors proceed to the lifts, which travel up the spine of the tower, and the spine of the Melbourne business establishment. On the lower floors, the lift passes the southern regional headquarters of Westpac. It moves on up, past the Australian offices of Merrill Lynch, the world's largest brokerage house, past De Beers Industrial Diamonds, Schroeder Darling merchant bank, past a cluster of Baillieu companies. It moves up to the headquarters of the Western Mining Corporation, and then to the 35th and top floor, the headquarters of North Broken Hill.

But the top floor is not the most important floor. The most important floor is the 29th, because that is where Hugh Morgan works.

Hugh Morgan sits in the severity of his office with its bare glass table and naked white walls and modest swivel chair. He sits in his blue suit, blue shirt and blue tie. He has polished black shoes and polished black hair. He caricatures his Geelong Grammar background.

HE has been running Western Mining Corporation Limited, one of the largest mining companies in the world, for nine years, since he was 35. But Hugh Morgan is much more than that.

"If I have to be a sacrificial lamb in the interests of debate, I don't care."

Hugh Morgan's eyes gleam even brighter than his hair. He is charming and unafraid. He has become, in some ways, the most important conservative figure in Australia. He is not merely an outspoken captain of industry, he is at the centre of a large and growing network of activists who are seeking to reshape the political agenda in this country.

They have decided to change public opinion. They are bypassing the universities. They are even hoping to begin Australia's first private university. They are not short of money. And they have decided the issues are too important to be left to politicians.

"You won't get change through politicians," Hugh Morgan says. "I can't think of a major political leader in the last 20 years who hasn't been crucified. Politicians can only accept what is accepted in the public opinion polls. So you have to change public opinion!"

They may be winning.

"I think an encouragin sign is that over the last four years, five years, I see a tremendous change in the language of conversation. The word competition is a discovery of the last five years.

"It is no longer a dirty word. The solution to a manufacturer's problem today in Australia isn't to take the biggest risk in his life and buy an aeroplane ticket to Canberra to get fixed with a new tariff arrangement.

"Today there is an enthusiastic groundswell of material that is coming forward without fear of retribution. That retribution is attempted to be heaped on some of our best academics in the most appalling manner (he mentions Professor Geoffrey Blainey and Dame Leonie Kramer). But there are now so many they are starting to feel comfortable.

"We have seen the rampant increase in influence in the Centre for Independent Studies in Sydney, the Institute of Public Affairs here in Melbourne (Hugh Morgan is a patron and financier of both). In Sydney the enthusiasm of the group who have founded Centre 2000, the preparedness of someone like (Professor Richard) Blandey at Flinders University to take on anybody and all-comers. Here is Michael Porter at Monash running the Centre of Policy Studies — his is the only section of the university that is expanding so rapidly, all on external funding. The marketplace is operating."

THE most significant of these groups is the Centre for Independent Studies, based at St. Leonards in Sydney. The executive director, Mr Greg Lindsay, a former school-teacher, says the think tank has an annual budget of $350,000, most of which is supplied by corporations and individuals. Hugh Morgan is a member of both the executive and the board of trustees. The centre's advisory board includes 15 professors. "We are trying to change public opinion by producing well-researched material," Mr Lindsay says. "We keep right out of politics."

Hugh Morgan keeps a record of all the press clippings generated by the centre and says they exceed that of any university in the country. "There's a marketplace for ideas," he says. "How far does your standard of living have to fall before the community at large does understand there is something wrong?

"Most of the Left have experienced a very closeted life. They have been the product, overall, of a State education system, to a State university where they have enjoyed State-provided living allowances, to then enter a State-run schooling system with a State-determined curriculum. And if you had to ask 'what actually qualified you for the great experiences of life' you would hardly come up with an ideal experiences path.

"Most of our armchair jockeys aren't fireable, but are happy to denigrate those who have a community obligation and work hard and create wealth to support them. A lot of these armchair academics are not responsible for what they say. I'm happy to have an academic get up and say what he thinks should be done, if he's prepared to put his superannuation dollars on his judgment." He and his fellow proselytisers call themselves classic Liberals. They do not regard themselves as conservatives, for they are futurists, ready to adapt to changing circumstances.

THEY are also social Darwinists, a philosophy which was publically untouchable in Australia until a few years ago. It is not dissimilar to the conservationism that has been triumphant in America since the election of Ronald Reagan. And in Australia it has been given considerable impetus by Hugh Morgan's willingness to say the unsayable.

Whenever he speaks now, he draws large crowds. He has been most widely quoted for his opposition to Aboriginal land rights and his attack on the mythology of Aboriginal innocence. "These fellows," he said in a recent interview "are portrayed as Rousseau-like, in touch with nature. But to raise these things as an issue invites denegration as a racist.

"If you allocate Aboriginal land on the basis of spirituality potential to that land, how can you deny the subsequent inevitable follow-on that you cannot separate Aboriginal law?"

He proffers a recent article from the Adelaide Advertiser about two young Aboriginal boys who went into hiding because they did not want to go through an agonising tribal circumcision ritual. He also recounts an anecdote about a recent case in the Northern Territory where a young Aboriginal arrived in hospital with a spear through his leg, the result, according to the doctor on duty, of a fairly common example of tribal justice. But these things, he says, are not reported in the east.

"You've no idea of the amount of suppressed material along those lines. I just can't . . . the volume of it is immense."

DID he get much personal feedback from his famous address last year when he was reported as saying Aboriginal land rights threatened to wipe out the Australian mining industry and represented a step backwards into a "world of paganism, superstition, fear and darkness?"

"It's amazing," he replied. "The address on Aboriginal affairs is the one that really got things wheeling. I had over 300 letters personally. Only four were against what I said. Of the four, I sought to speak with each of them. Two I sent a copy of the speech and asked them to come and

"You won't get change through politicians. I can't think of a major political leader in the last 20 years who hasn't been crucified. Politicians can only accept what is accepted in the public opinion polls. So you have to change public opinion!"

have a talk. No response. The other two, who are both teachers, read the speech, came and saw me, and completely changed their mind and were appalled at their understanding of what they had gained from the press reports they had read."

He is willing to say the unsayable, and touch the untouchable. He challenges the Church establishment in the name of free enterprise. He is concerned by many clerics' preoccupation with wealth distribution at the expense of wealth creation.

"I HAVE here a tome by Ian Hore-Lacy of CRA called *Work Creation and Distribution: Aspects of Public Theology* which is coming out soon. It's addressed at the Church. He's very middle ground. He's no rabid right-winger."

(Ian Hore-Lacy is CRA's principal consultant, corporate relations services. Asked about his work, he said: "wealth creation is a thoroughly acceptable activity for Christians. You can't redistribute wealth before you create it." He said he drew on some of the material produced by the American Enterprise Institute).

Hugh Morgan and his allies have four broad targets: the education system, the growth of the public sector, the power of trade unions and the arbitration system. It was little wonder, then, that the Hawke Government fairly quickly replaced him on the board of the Reserve Bank by a businessman with a more cosy relationship with Labor — Peter Abeles, of TNT/Ansett.

What frightens him most is that debate — "the portfolio of ideas" he calls it — is being crushed by the sheer weight of the public sector.

"The more you get this insidious expansion of the Public Service the less is the capacity of people to respect the truth. You find a fear of retribution. If you've got 44 per cent of the GDP in the hands of government you've got something like 50 per cent of Australian businesses which derive their livelihood from government expenditure."

Australia's greatest single problem, he believes, "comes from the heritage in Australia of the arbitration system — that you have to be the squeaky wheel, the bigger the squeaky wheel the better is the expectation that the solution will be halfway in between what is today and what is claimed. We have now 80 years-plus of an arbitration system working on just that principle.

"Ask yourself: if you were a shopfloor steward, how would you restrain from asking for more because the history has been that those who ask for more get it. How could you preserve your job? So it drives you to be unreasonable.

"IF you were to look at the demise of the old medical ethic, it was then the TV repairman, when the washing-machine repairman called at the doctor's house and was charging twice what a doctor did. The doctor had to do it in the middle of the night and had an ethic that said 'drop everything, come and treat my patient.' On the other hand he's got some fellow in overalls who'd probably had little training knock on his door and say 'My call is $40 and if I fix it have you got $50 cash?'

"In a sense it sort of acted as a cancer. Those sorts of cancers in terms of old-term professional ethics turn people into militant claimants because the system seems to have worked for the other fellow. It's not a question of responding to the market place. We've abolished the marketplace in our wage-setting structure.

"Australians have become very conditioned to the role of the Arbitration Commission. It is true that 30 per cent of the workforce is employed by government. Those two factors remove from many people's minds an understanding of what the marketplace is. It conditions them. They become Orwellian products.

"Merciless and bountiful. Hence the need to engrave in people's minds that in relative terms we were the richest country in the world at the turn of the century. Yet (look at) the last figures I've got on investment confidence (from the EMF foundation in Switzerland).

"What is staggering is that Australia was 17th, now ranks 21. Those above us include Mexico, the Philippines, Indonesia, India, Turkey, Malaysia, Korea, Ireland!"

His eyes widen, his eyebrows arch, his voice quavers theatrically, he smiles, looking heavenward.

"The trouble is we have blessed one sector with enormous powers. These powers are not subject to limitation. I refer to the trade union structure. I find it extraordinary that it looks like we are going to have an accommodation between government and the public sector unions who have just gone to the umpire, been refused their claim and embarked on a policy of, in effect, withholding supply . . . but the Government fails to see the analogy.

"THE only laws that control unions today are under the Trade Practices Act relating to secondary boycotts and some sections of the common law. And it is in the present party platform to abolish both limitations.

"The first step is to have a community understanding of the role of the trade union movement. What are their rights? What are their required obligations and limitations? This is today receiving very little or no attention whatsover."

He sees a danger in moving too quickly towards deregulating the wages system, as Andrew Peacock has pledged to do. Move too quickly, he says, and there would be industrial chaos. This would thereby damn the very argument in favor of the marketplace.

Is it because most of the present generations of Australians have never known real adversity?

"The last major event that affected this country was World War 2. However, of those who actually saw service and hardship, you'd probably be hard pressed to find 200,000 people. There might have been a million and a half that signed up but some were manning guns at Portsea. The number of people who saw real hardship and for whom we should express eternal thanks hasn't been sufficient to discipline the whole community.

"Quite clearly, rather than accept the challenges of growth, we would rather spend a hell of a lot more time at the beach. You have to question whether .05 per cent of the world's population can go to the beach and be custodians of 5 per cent of the world's land mass.

"How long will that be tolerated, particularly if the rest of the world has no respect for what we are doing?

Certainly in terms of our export industrial relationships we do not earn respect with our appalling reputation for strikes while supplying essential raw materials.

"Without putting our guts behind it and actually creating a fortress we have this fortress image which is no better than a paper bag. (Laughs). In which case you want to have friends, which raises another issue."

THE people most responsible for this mentality, he believes, are the Left-wing teachers who permeate the school and tertiary education system.

"The people who frame our opinions are teachers. There are a lot of good teachers, but in terms of imbuing a national spirit, of a preservation of the work ethic and the value and enjoyment that comes from it, these sort of values are absolutely abused by some in the teaching profession."

He believes the political agenda in Australia has been in the grip of the Left-wing for many years because it had the long-term vision to inculcate its ideas over several decades.

"You can be wonderfully evenhanded, but wonderfully biased. If you went to the Left-wing and asked them what were the important issues of the day you'd get nuclear energy, Aboriginals, wealth distribution, you'd get a whole agenda.

"If you asked the other side: wealth creation, defence, national price. If you then have most of the programs on the Left's agenda, dealing with each one wonderfully even-handedly that in itself is hopelessly biased because the agenda is the thing!

"You can get excellent programs on the ABC about agenda items which are basically the agenda of the Left."

He believes this agenda is driving Australian companies overseas. "We're going offshore because we're not competitive. In terms of the marketplace of the world, companies have an obligation to invest their funds where they see it best.

"Some of them will make mistakes, some of them will be very successful. It enhances our relationship in the international community. That's a plus. It's a minus in that it tells us we are not doing things right here. But perhaps it's the only way. When we've had this huge capital inflow it has masked poor performance.

"Just as we wish to invest offshore — we have so many Australian multinationals it doesn't matter — it is not one-way, which is to our benefit. Fortress Australia in terms of equity and control and capital markets is a nonsense.

> *'Many people do not understand that Australia, irrespective of its internal politics, is in a competitive environment in the great marketplace of the world from which there is no escape, and which is merciless.'*

"In farming or mining, it's assumed you're a simple dead-head. But if you look at those industries in Australia ... we are high-tech. And if we weren't, with our wage structures we'd have been out of business long ago."

THE greatest problem the mining industry faces is Aboriginal land rights. "All I can say from an industry point of view is that the only things which have gone forward since the Land Rights Act are the things that have already been discovered.

"Now, these areas are no less Aboriginal backyards than our backyards. We are reluctantly thrown in the forefront of the debate. John Citizen may one day be concerned that there are large areas of Australia that he is not allowed to go to.

"Now, that looks Aboriginal-bashing. I've got no problems with Aboriginals, goddamnit! They're nice fellas. If they're disadvantaged, they have a legitimate claim on the wealth of the community, like anybody else.

"But to classify, not on the basis of need but purely on the basis of caste? To put into the control of organisations that are totally foreign to the tribal structure of Aboriginals, Western-type council organisations frequently dominated by white activist lawyers and part-Aboriginals who historically have had no part in the traditions of Aboriginal life? We are making no lesser mistakes than all the people who have tried to tackle these problems in the past 200 years."

— Sydney Morning Herald

P. Sheehan, 'The right strikes back', *Weekend Herald*, 16–17 March 1985, p. 13.

Martin Flanagan

7.11

HISTORIANS DISAGREE WITH MORGAN

Mining chief Hugh Morgan's claim that Australian history was being falsified to undermine the legitimacy of European settlement was challenged yesterday by leading historians.

The exception was the professor of history at the University of Melbourne, Geoffrey Blainey, who said that Mr Morgan, the executive director of Western Mining, had made "a thoughtful and legitimate comment".

One of Australia's leading historians on Aboriginal-white relations, Professor Henry Reynolds of James Cook University in Queensland, said Mr Morgan wanted a version of Australian history "that was comfortable for capital".

Professor Reynolds said Mr Morgan's speech was "a typical, rather sloppy, Hugh Morgan polemic, which bears all the hallmarks of someone seeking to attract the attention of the media".

He said Mr Morgan's pronouncements ultimately came back to the political issue of land rights. "I understand that Mr Morgan is saying privately that Western Mining will cease to invest in Australia if land rights legislation under the present Government goes any further. Perhaps he would care to confirm or deny this publicly."

Professor Reynolds said he would welcome the opportunity to debate the issue publicly with Mr Morgan.

The professor of history at Monash University, Graeme Davison, said Mr Morgan had misrepresented the aims and achievements of recent historians of Aboriginal-white relations.

"The aim has not been to delegitimise European settlement but to relegitimise the place of Aborigines in Australian history," Professor Davison said.

In a speech read on his behalf at an Australia Day Council of Victoria lunch last Friday, Mr Morgan said Australia's history was undergoing "an almost uncontested falsification".

Mr Morgan took as his model Governor Arthur Phillip who, he said, had suffered a libel without parallel in Australian letters in the book 'Our Original Aggression' by the professor of economic history at the Australian National University, Noel Butlin.

Mr Morgan said Professor Butlin had claimed Governor Phillip was involved in an attempt to exterminate the local Aboriginal population by deliberately infecting them with smallpox. On Friday, Professor Butlin declined to enter the debate.

Yesterday Dr Richard Broome, formerly of La Trobe University and the author of 'Aboriginal Australians', said that smallpox scabs had been brought to the colony in glass bottles for the purposes of innoculation and Professor Butlin considered these to have been the most likely source of the outbreak.

"Butlin says the first possibility is that these could have been stolen by the Aborigines who valued glass," said Dr Broome.

"His second possibility is that the bottles were traded by convicts. The third possibility he raises is that the Aborigines were deliberately infected by Phillip and others.

"It is a distortion of Butlin's book to say that he arrives at the conclusion that Phillip deliberately infected the Aborigines. What he does do is decline to discount the possibility."

Dr Broome said Mr Morgan's view of Governor Phillip was idealistic and naive.

"Phillip was a man under a lot of pressure. I'd like him to be seen as a complex person in an extraordinarily difficult situation — neither a villain nor a hero," Dr Broome said.

"Phillip had his own problems feeding the settlement, and when his gamekeeper was killed by Aborigines he ordered a punitive expedition and stipulated that he wanted 10 Aboriginal heads brought to him with the intention that this would teach the Aborigines a lesson."

After persuasion from some of his staff, who felt the measure was too severe, Governor Phillip rescinded the order, Dr Broome said. Instead, a party was sent out to take a number of Aborigines into custody but failed because it could not find any.

"From that position, he lapsed into indifference and didn't even attempt to communicate with the Aborigines."

Dr Broome said Mr Morgan's views reflected a 19th century concept of history. "The 19th century view was to concentrate historical concerns around great men. Contemporary historians seek to understand rather than to judge," Dr Broome said.

Professor Reynold referred to Mr Morgan's assertion that France's President, Mr Mitterrand, might have been influenced by the views of authoritative people in this country when he said that Australia had no problem with its indigenous population because it had killed it off.

"This suggests a quite remarkable awareness of Australian historical writings within the French presidential palace," Professor Reynolds said.

"In fact, the best historical writing of recent years shows that the Aborigines have survived, not been eliminated, and that their political movements are directly involved with events in the past. An example is Lyndall Ryan's history of the Tasmanian Aborigines."

M. Flanagan, 'Historians disagree with Morgan', *The Age*, 29 January 1985, p. 3.

Bain Attwood

PHILLIP'S CONDUCT TOWARDS THE BLACKS WAS *NOT* EXEMPLARY

from B. Attwood

The executive director of Western Mining Corporation, Mr Hugh Morgan, has attacked what he sees as an "almost uncontested falsification of our past" ('The Age', 26/1). Mr Morgan himself, however, is guilty of an untenable reinterpretation of a crucial phase in Australian history — the early years of contact between Aborigines and Europeans at Botany Bay and the governorship of Arthur Phillip.

Mr Morgan has argued that Phillip deserves the title of Australia's founding father for his "exemplary conduct in the years 1788 to 1792"; to illustrate this, he has related an incident in September 1790 when Phillip was speared by an Aboriginal man at Manly Cove, noting that he did not retaliate.

The renowned Australian anthropologist, the late W. E. H. Stanner, in a meticulous study of Aboriginal-European relations in this period, has pointed out that there is little doubt that the fault for this incident was mainly Phillip's. (He also notes that Phillip had been pursuing a policy which gave rise to several major grievances among Aborigines, and that they had good reason for "a public remonstrance against Phillip".)

Professor Stanner also tells us that while Phillip "allowed no retaliation, and harbored no resentment" after the attack, within months another incident between the Aborigines and whites at Botany Bay saw him give orders for the capture and summary execution of several Aborigines.

Stanner concluded that in respect of his dealings with Aborigines during his governorship, Phillip emerges badly; he lacked common sense, his vision of them was warped, and he misunderstood them almost as badly as he did the two-sided racial situation. Moreover, by the end of his period as Governor, "there were already present both the elements, and the conditions for the persistence, of two realities which continued without material change, except for the worse, over the next 150 years": a racial pattern of dominance and subjugation, and a "structure of equities — the Europeans maximal, the Aborigines minimal".

Professor Stanner was led to believe that "one cannot make full human sense of the development of European life in Australia" without reference to these two realities; "in short, without an analysis of the Australian conscience".

In Hugh Morgan's "Orwellian reconstruction" of our history (and in his strategy for Australia's economic future) there is of course no place for such an examination.

BAIN ATTWOOD,
North Fitzroy.

B. Attwood, 'Phillip's conduct towards the blacks was not *exemplary*', *The Age*, 29 January 1985, p. 12.

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