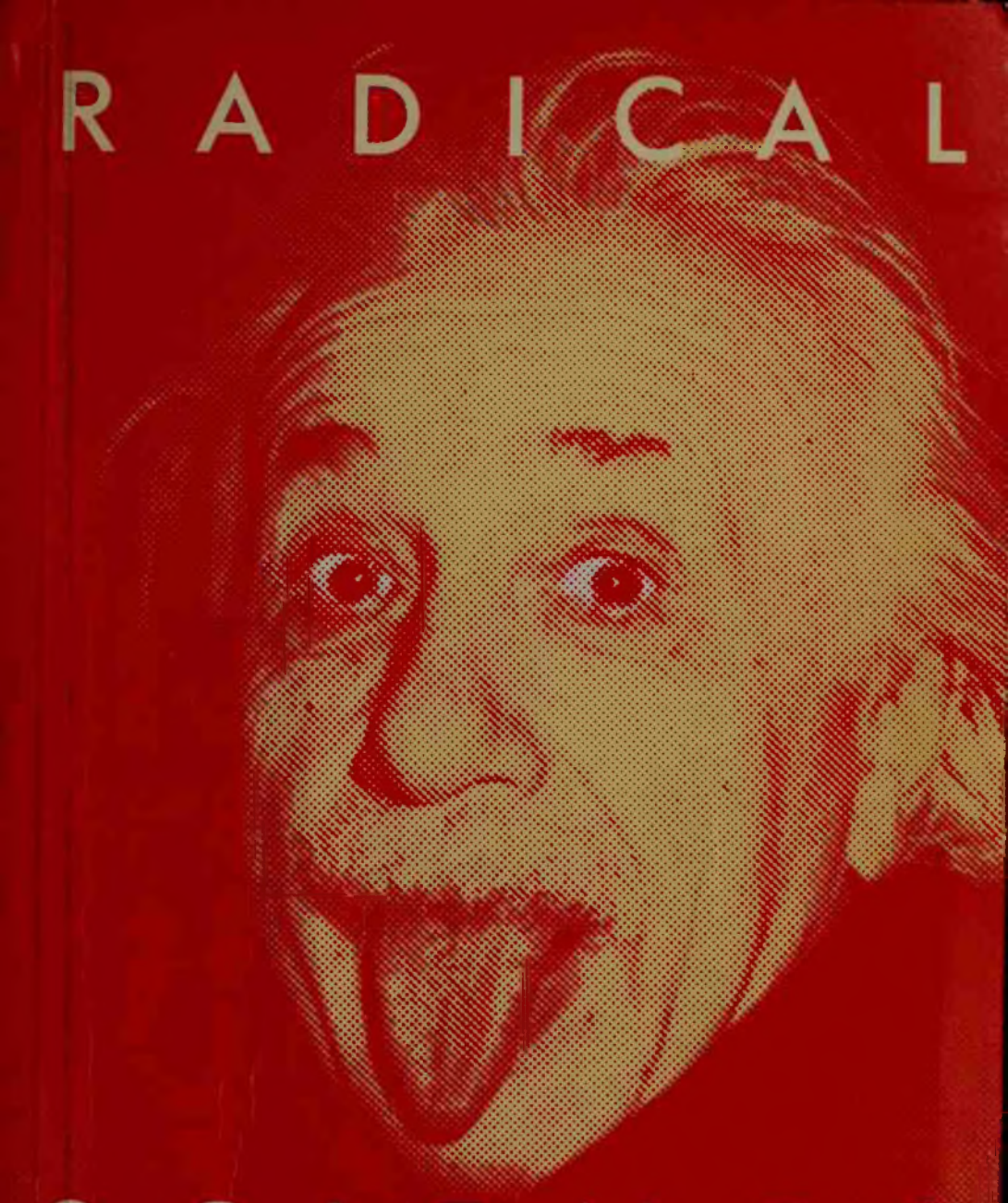
RADICAL SCIENCE



ESSAYS EDITED BY LES LEVIDOW

Radical Science Essays

Edited by Les Levidow

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David Dickson is a science journalist who currently works for the US journal *Science*, and was a founding member of the RSJ editorial collective. He is the author of *Alternative Technology* (1974) and, more recently, *The New Politics of Science* (New York, 1984).

Albert Einstein worked for a time in the Swiss Patent Office and then became a relativist. (This has been distressing for some, confusing for others.) However, he remained clear about one thing—his belief in socialism—which, for some strange reason, we hear little about.

Sheila Ernst did the research on which her essay is based as a research student in the Medical Psychology Unit at Cambridge. While doing this work, she became involved in the women's movement, which had a strong influence on the direction the work took. Her interest in putting her ideas into practice led her, via self-help therapy, towards her current work at the Women's Therapy Centre, London.

Luke Hodgkin is Reader in Mathematics at Kings College, London. He is currently writing a book on mathematics in the French Revolution.

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Simon Pickvance lives and works in Sheffield. He has helped to provide a health & safety information service for workers through the local Science for People group.

Jack Stauder did his undergraduate work in America and then a Ph.D. in Social Anthropology at Cambridge Univeristy where he was also active in the anti-war movement. He began teaching at Harvard, where he gave a course for which students evaluated their own work. This was very popular with the students but not with the authorities, who did not renew his contract. Then he taught anthropology at Northeastern University in Boston.

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Texts

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Introduction

BOB YOUNG

'Radical science' as an oppositional politics was born in the 1960s. It grew out of the same ferment that led to the movement against the Vietnam War and to May '68, at a time when anti-authoritarianism felt visionary and real. Among the authority roles it seemed appropriate to challenge was that of scientific and technological rationality in maintaining the existing power relations in society. More particularly, two organizations were reacting to the use of science in chemical and biological warfare in the Vietnam War. Their images were impeccably liberal – the British Society for Social Responsibility in Science (publishers of *Science for People*); and, in the USA, Scientists and Engineers for Social and Political Action (publishers of *Science for the People*).

Let me try to evoke the setting in which the essays in this book were first conceived. I well remember my own introduction to this world. I wrote a review in the *New Statesman* (reprinted at the end of this essay) about racism and scientific arrogance. It conveyed concerns typical of the time. In criticizing C.D. Darlington's *The Evolution of Man and Society*, I drew a firm line between science and illegitimate extrapolations, a firm line between use and abuse – thus protecting 'real science' from its abuse by right-wing ideologues.

In the two years after I wrote that review, new perspectives were brought to bear which deepened the critique from the use/abuse distinction to the legitimating role of science in society. Shev Lal, a concerned molecular biologist, had seen my review and got me invited to a conference on 'The Social Impact of Modern Biology'. My own experience was that the conference was the first and last time that a liberal and a radical critique could successfully occupy the same platform. Indeed, there were strong tensions even then. On the liberal side, David Edwards was still in the foothills of the research with Patrick Steptoe which would raise the spectre of 'test-tube babies'. After he described the research on which he and Steptoe were embarked, he replied to a questioner that he felt no need for public regulatory bodies outside science to assess the legitimate boundaries of *in vitro* fertilization and related technologies. Now we have the Warnock Committee and legislation in the British Parliament and American Congress. Jim

Watson, co-discoverer of the structure of DNA, could be righteous towards the commercial goals of drug companies. Now we have biotechnology reconstituting the pharmaceutical industry, along with many others. We were also addressed by Jacob Bronowski, arguing for 'the disestablishment of science', freeing human enquiry from government controls. There was a dramatic moment when a woman in the balcony got up and shouted that there would one day be 'host mothers'—perhaps in South Africa—carrying babies for other, richer women. Bronowski began his most affecting oratory by saying that his talk was for the little lady in the balcony. Now we have more than half of our research and development budgets determined by military funding and just under half of our researchers sponsored by the same patron, with a customer-contract basis for almost all of the rest; we also have commercial agencies for host mothers in the United States, with laws pending against them in Britain.

There were, as I recall, three radical talks at that conference. Since then we have all had to accommodate to the times. Jonathan Beckwith had just won a prize for isolating a human gene; he had given the money to the Black Panthers, a revolutionary organization in the USA, and had sworn off genetic engineering research in the contemporary context. Now he does that kind of work again in a medical school with huge commercial support for biotechnological research. Stephen Rose and Hilary Rose attacked the concept of the neutrality of science. He now appears on British television's 'Horizon', still arguing a radical case for scientific priority-setting, while she is a professor of social work. I tried to move the debate forward by relating natural science to the sociology of knowledge, specifically eschewing the very line-drawing I had so confidently done two years earlier. The conclusion of my talk reverted to Darlington's book and the reception of my review.

> This leads me to the current debate. Professor Bettelheim tells us that student radicals are suffering from neurosis (many, he assures us, can be cured by psychoanalytic psychotherapy). Professor Lorenz explains human aggression and student protest in ethological terms (less hope there), while Herbert Marcuse tells us that the positions of both the young radicals and the old reactionaries are biologically determined. The list of authors who have recently written ideologically prescriptive works in the guise of descriptive and generalized accounts based on genetics, ethology, archaeology and anthropology, and general biology is by now familiar to most of us: Morris, Ardrey, Comfort, Towers and Lewis, Koestler. It is growing daily. From the point of view of professional scientists, one can feel safely distanced from this pseudo-biology. We do not take it seriously when Lysenko cites pseudo-evidence against intraspecific competition, or when Robert Ardrey claims that masses of scientific data support the inevitability of such competition. We can even feel that it is little to do with us if Professor Darlington—with "FRS" prominently printed on the bookjacket—cites a mass of scientific and historical evidence punningly

interpreted in support of reactionary social doctrines, including *apartheid.* (We knew he thought such things, after all.) But just as the Lysenkoists argued that modern genetics gave support to Western bourgeois reactionaries, it is clear that Professor Darlington's pseudo-science gives comfort to the South Africans. For example, when a highly critical review appeared over my name in the *New Statesman,* I had a letter from a South African graduate patiently explaining that my reading of the book was a result of my ideological bias. My point is that of course he was absolutely right about me, *and* I am right about Professor Darlington's book.

More and more people are trying to base generalizations about man, society, culture and politics on the biological sciences. They have always done so and will continue to do so. Many of them may be relatively easy targets, but the essential point is that no one can confidently draw the line between fact, interpretation, hypothesis, and speculation (which may itself be fruitful). It seems to me that it is the social responsibility of science to enter wholeheartedly into this debate and directly answer such works in the non-specialist press. Paradoxically, we must relax the authority of science and see it in an ideological perspective in order to get nearer to the will-o'-the-wisp of objectivity. We have won a Pyrrhic victory in establishing the part-reality and part-myth of the autonomy and objectivity of science, and the existence of this Society and its conflicting aims reflects our unsteady position. In one sense science should feel strong enough to stop flailing horses which died in the nineteenth century in their attempts to protect the status and methods of science. But in another sense we need—for our own moral purposes—to think seriously about the metaphysics of science, about the philosophy of nature, of man and of society, and especially about the ideological assumptions which underlie, constrain and are fed by science. Since we have systematically weeded out this tradition among working scientists—one which flourished until the 1920s—we need help from other disciplines in gaining the necessary perspective, and we could well turn to the continuing traditions of enquiry in the social and political sciences which have gained impetus from the civil and international conflicts of advanced technological societies.

We can, if we are reluctant to consider evaluative concepts as integral to biology, retain the distinction between the scientific and the evaluative. Although I believe that the maintenance of this distinction is philosophically indefensible, the programme which can be recommended for the further study of social responsibility in science is operationally indistinguishable from the one which follows from the strong version of my thesis: study works on ideology and social science and apply their analyses to our own work in order to test the limits of pure science. I suspect that little will remain inviolable, but whether or not I am right about this, we will have cultivated a perspective which encourages the evaluative and political consideration of scientific concepts and will find ourselves in greater control of extrapolations from our work and much more wary of the specious aura of scientific objectivity in which they are cloaked. I am in no sense recommending an anti-rational, much less an irrational, activity. The aim is to open up more aspects of science and its context to public debate so that conflicting values can be discussed *as such.* Science is no substitute for morality or politics, nor is it independent at any level from

> them. We need to see that ideology is an inescapable level of discourse and need (in the first instance) to debate conflicting ideological positions and (in the last instance) to face and resolve the actual conflicts between the needs and goals of men in the appropriate way.

I was approached at the end of the conference by three lots of people, each seeing in my talk affiliation with their position. Jacques Monod linked it to his own individualist, existentialist position, the view which he developed in his touching book on man alone in the universe: *Chance and Necessity*. David Edge offered a hand of comradeship over treating science anthropologically and included an extended, exploratory version of my talk in his new journal, *Social Studies of Science*. David Dickson and Jonathan Rosenhead, who were BSSRS activists, approached me and said that the organization was getting into an establishment rut; together we wondered what to do. On the question of the politics of scientific research, we embarked upon the planning for a conference on 'Workers' Self-Management in Science'. There were also two BBC radio series on 'Science and Ideology' and on 'Are Hierarchies Necessary?'. That is, we set about establishing a radical agenda for scientists and for how science is perceived in society.

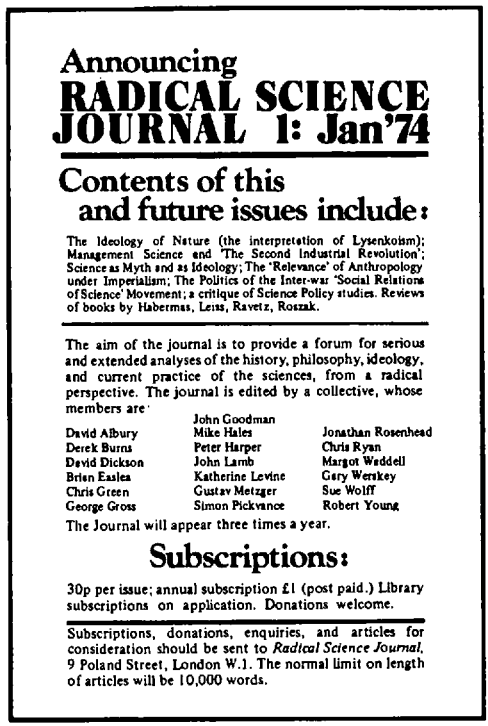
We also decided that the whole set of issues around science, technology, medicine and politics needed to be critically examined and re-theorized, just as other aspects of knowledge and culture were being scrutinized at that time. Indeed, there was lurking about the conference a person who had linked the critique of science with the wider political movement of the time. Theodore Roszak had recently written *The Making of a Counter Culture: Reflections on the Technocratic Society and its Youthful Opposition*. But Roszak was too put off by the number of Nobel Prize winners present and the air of self-congratulation. So he wouldn't give his talk and only heckled from the balcony.

Radical Science Journal

Out of all this came the *Radical Science Journal* (see vintage advert).

Its first editorial (reprinted at the end of this essay) declared the use/abuse model inadequate for challenging the science specific to capitalist society and for constructing a new form of knowledge needed for socialism. Inviting readers to contribute material for publication, the editorial also described the Collective's democratic process. Re-reading the editorial now, I can only ask: What has happened in the intervening years to all that hope and energy? And to that touching faith in the politics of process?

With the *Radical Science Journal*, the libertarians in the radical science movement moved on politically – from attacking chemical and biological warfare with a use/abuse perspective, to analysing the role of science in the capitalist mode of production. The essays reprinted in this collection



Announcing
RADICAL SCIENCE JOURNAL 1: Jan'74

Contents of this and future issues include:

The Ideology of Nature (the interpretation of Lysenkoism); Management Science and 'The Second Industrial Revolution'; Science as Myth and as Ideology; The 'Relevance' of Anthropology under Imperialism; The Politics of the Inter-war 'Social Relations of Science' Movement; a critique of Science Policy studies. Reviews of books by Habermas, Leiss, Ravetz, Roszak.

The aim of the journal is to provide a forum for serious and extended analyses of the history, philosophy, ideology, and current practice of the sciences, from a radical perspective. The journal is edited by a collective, whose members are:

	John Goodman	
David Albury	Mike Hales	Jonathan Rosenhead
Derek Burns	Peter Harper	Chris Ryan
David Dickson	John Lamb	Margot Weddell
Brian Easlea	Katherine Levine	Gary Werskey
Chris Green	Gustav Metzger	Sue Wolff
George Gross	Simon Pickvance	Robert Young

The Journal will appear three times a year.

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Subscriptions, donations, enquiries, and articles for consideration should be sent to *Radical Science Journal*, 9 Poland Street, London W.1. The normal limit on length of articles will be 10,000 words.

have been drawn from the first four issues and still seem fresh. Each essay is widely read and cited, and each of the authors has continued active work in the politics of scientific theory and practice. The issues with which they deal in their essays are still being illuminated by those pieces.

Meanwhile, what of the development of the perspective of the *Radical Science Journal* Collective? I see it in three phases. In the first, the role of values and politics in science was further explored – hammering away at the distinction between science and ideology, at the privileged position of science as an unequivocally progressive force or as a basis for socialism. While we were developing this critique, relativists were taking the sociology of knowledge to its limits, losing any radical political perspective on the way. Theoreticist marxists were clinging to an orthodox notion of science as the basis for socialist theory and practice, while vulgar marxists saw marxism as scientific in the narrow, positivist sense.

Our own point of view moved on from use/abuse and from science as ideology. We were greatly influenced by the labour-process perspective of Harry Braverman's *Labour and Monopoly Capital: On the Degradation of Work in the 20th Century*, which appeared in 1974, the same year as our first issue. Braverman was not particularly concerned to look at science,

technology, medicine and other forms of expertise *as* labour processes, but it was relatively easy for us to extrapolate his work in that direction. The attraction of doing so was to get away from debates which we had begun to find tedious. For example, when we said that science is ideological, it was too easy for uncomprehending or malicious critics to equate that with saying that science is untrue. Rather, we rejected the counterposition of 'ideology versus truth'. Our position was that scientific priority-setting, concepts and findings embody the values of the epoch, as well as of the patronage system which funds them, while still being true and efficacious. After all, the Manhattan Project was the expression of a political and governmental process; it was of its time and spared no expense. The results of all that scientific and technological labour also worked: at Los Alamos, Hiroshima, Nagasaki, as well as in the tests and developments since Bikini Atoll and beyond.

A labour-process perspective focuses one's attention on science as human labour, as a practice. The elements of the labour process—raw materials, means of production and purposive human activity, leading to the production of a use value—could be applied to any theory, therapy or thing. At the same time, attention directed to the connections or articulations between labour processes highlights where research and development priorities come from—how things get into the pipeline. Who pays, who decides, who benefits? That is, labour-process studies keep the critique of science *in* society.

There is no point in repeating here the theoretical and applied analyses which have appeared in the pages of the *Radical Science Journal* since the essays which have been revised and reprinted for this book, but certain of them do sum up our work (and are still available from Free Association Books): my essay 'Science *is* Social Relations' in *RSJ* 5; the entire issue of *RSJ* 6/7, particularly Les Levidow's essays on IQ; Karl Figlio's analysis of 'Sinister Medicine? A Critique of Left Approaches to Medicine' in *RSJ* 9. Finally, we attempted to sum up our position and reply to our critics in a long essay (commissioned but then rejected by the *Socialist Register*): 'Science, Technology, Medicine and the Socialist Movement' in *RSJ* 11. Subsequent issues have been thematic:

Medicalization (12)
Scientism and the Left (13)
No Clear Reason: Nuclear Power Politics (14)
Free Associations: Psychoanalysis, Groups, Politics, Culture (15)—which has since become an independent journal
Making Waves: The Politics of Communications (16)
Issues in Radical Science (17)
Compulsive Technology: Computers as Culture (18)
Gender and Expertise (19)

Expertise and Daily Life

In this period, the role of expert knowledge in people's lives has grown amazingly. Recent dramatic developments include the microprocessor revolution, computer-aided design and manufacturing, genetic engineering, 'test-tube babies', spare-part surgery, high-technology entertainment. And we are said to be on the threshold of a whole new generation of 'thinking computers'.

The media have begun to concern themselves with all this – the news, television documentaries, newspapers. But their analyses are on the whole at the point where the radical science movement began – use/abuse. Our aim in publishing this book is to invite readers to move on to a more evaluative and political analysis of the substance, along with the context, of expertise – seen as a totality of relations – and to make the origination, the conduct and application of scientific research more democratic and accountable. We are still arguing for a science of the people and a science for society, rather than forms of expertise which are designed primarily for profit, palliation, and control.

I want to close with some reflections on a half-century of science as it has touched our lives in one sphere. My aim is to try to evoke the pace of change and the rapidity with which new developments get taken into our lives and alter them. To convey this, I have only to look around my room as I write. I have nearby a choice of television, video, radio, reel-to-reel tape, a cassette deck, a Walkman, a dictating machine, and a compact disc player. These are in addition to a tape player and stereo in the car.

When I was a boy there were no tape recorders. I bought a wire recorder when I was at school. When my eldest child was born in 1958, there were no cassette recorders. When my second child was born in 1968, there were no car stereos; when my third child was born in 1971, there were no video cassette recorders or Dolby noise reduction systems. When my next child was born in 1979, there were no personal stereos ('Walkmans'). And when my fifth child was born in 1981, there were no compact disc players on the market.

Looking more broadly, all of the following have been discovered, developed and/or deployed in my lifetime (b.1935):

long-playing records	emulsion paints
cheap offset litho printing	antibiotics
xerographic photocopying	transistors
television	microprocessors
ballpoint pens	computers
satellites	space vehicles
satellite communication	ultrasound
radiocarbon dating	most polyesters
isotope tracers	microsurgery

transplant surgery	genetic engineering
polio vaccine	monoclonal antibodies
the jet engine	tranquillizers
radar	contraceptive pills
rocketry	insecticides
intercontinental ballistic missiles	herbicides
sonar	nuclear weapons
the digital watch	nuclear power
DNA	heart by-pass surgery

When I was a boy, ice was delivered daily to the home and milk came by horse-drawn cart. Clothes were washed in a black cast-iron pot over a wood fire. And I am only middle-aged. Of course, for most of the world, the innovations listed above – except for the transistor radio – are still not part of domestic consumption.

Most of these developments have become part of our daily lives without our having anything to say about them. Science and technology are fundamental mediators of our experience, our culture. Surely it is time that people other than researchers began to find it appropriate that they should hold views on such matters. The project of the Radical Science Collective continues to try to support that process of achieving greater political clarity, accountability and democratic origination of scientific change.

Note

1. Of the contents of *RSJ* 1-4, all the main articles are reprinted here, except one from *RSJ* 2/3, Gary Werskey's 'Making Socialists of Scientists: Whose Side is History On?'. This was of considerable interest but has been superseded by the author's book on the same topic, *The Visible College*, a collective biography of British scientists and socialists of the 1930s. Werskey's book—published by Allen Lane in 1978 but now out of print—is available through Free Association Books.

NEW STATESMAN 26 SEPTEMBER 1969

Understanding It All

ROBERT YOUNG

This book is a snare for the unwary.* It displays impressive erudition, is written in an authoritative manner, and has a seductive surface plausibility. However, when all is said and done (after nearly 700 pages, innumerable charts and tables, and an impressive bibliography) it is clear that Professor Darlington has written a profoundly reactionary book in the guise of legitimate scientific hypotheses and findings.

The Evolution of Man and Society has been given the full treatment. It was previewed in one of the Sunday glossies, reviews have had pride of place in the newspapers and weeklies, and a précis of the argument has appeared in a major historical journal. Why? Its topic bears on a general issue which embraces many of the trendiest writings in the last few years: the relationship between nature and culture. This theme was at the centre of the Enlightenment debate on the relationship between reason and nature. It formed the rationale for the concept of a social science in the mid-19th century when political economy, psychology, the study of the brain, and the developing disciplines of anthropology and sociology were all integrated by the theory of evolution. Evolutionism implied that man was a part of nature and that human nature was the product of a non-purposive, undesigned interaction between random biological change and the vicissitudes of the environment. Man *and all his works* could be seen as the product of processes which were basically biological. Nineteenth-century attempts to apply the theory of evolution directly to psychological, social and cultural phenomena were premature, and the various disciplines went their separate ways after the turn of the century. Man's place in nature remained an open question, although it was clear that the theory of evolution was basic to the issue.

In the last few years there has grown up a loosely-related family of inquiries which have readdressed themselves to the question which preoccupied Enlightenment and evolutionary thinkers. Some students of animal behaviour have made dubious attempts to extend their ethological concepts to account for human aggression, nationalism, and other aspects of social behaviour. Structural linguists have argued that there is an important innate component in the development and structure of human languages. Experimental psychologists have tried to spell out the physiological basis of sexual and emotional functions. These attempts to reduce complex human interactions to better-understood and presumably more basic categories have been complemented by efforts on the part of anthropologists and sociologists to maintain qualitative distinctions between animal behaviour and the actions of humans in a social context. Thus the kinship systems and intentions of men are central to the social sciences and are said to play very different

* **The Evolution of Man and Society** *by* C. D. DARLINGTON *Allen & Unwin 70s*

roles in biology. Similarly, the psychological and psychiatric theories which have been influenced by Continental phenomenology have set their faces against the use of traditional scientific categories in the explanation of human situations. Laing and his large following among young people want no part of the depersonalisation and reification of man. Indeed, some have gone much farther into mysticism, astrology and orgone boxes.

But no one denies that genetics has governed the biological evolution of man and that we are in many ways determined by our genetic endowment. Whatever one's reservations about the precise relationship between the categories of scientific explanation and those of human explanation, there is *a* relationship, and it is important to consider it carefully. In the meantime, the disciplines which could supply the relevant findings have undergone dramatic developments: archaeology, anthropology, demography and genetics. These have led Darlington to reassess the question in an audacious form. He believes that in the last 25 years it has become possible to relate biology and history much more closely and to attempt a single account of human and social evolution.

What form should this take? If we say that all of man's works can be included in a biological framework, then the temptation becomes almost overwhelming to attempt to write history as biology. Since the basic mechanism of evolutionary change is genetic and since the orthodoxy of scientific explanation implies that a more mechanistic account is preferable to – because more basic than – a less mechanistic one, the programme of Darlington's book seems almost inevitable: rewrite history as genetics. Genetics and demography have shown themselves able to claim and win a place in the crowded timetable of teaching in the social sciences which many feel outweighs their solid contributions to the understanding of society and history. They can do so, because social scientists feel, defensively, that these disciplines are somehow more scientific.

Having placed this book historically and conceptually, one can now consider it historiographically. Darlington sets out to cover 20 million years. He says in his preface that the attempt to integrate history and biology 'could have no point unless the same principles cou'd be shown at work all the way through man's evolution and history'. At the end of the book he says of H. G. Wells's *Outline of History* that

> his idea of one history of man is vindicated. It has become the only aim that we can justify today. And we are indebted to his successor, Carleton Coon, for maintaining that aim with the much greater resources at his command.

In fact, histories whose scope extends from Plato to Nato are the object of almost universal derision among historians (Darlington reaches Plato at p.232), and Coon's works on the history of man and especially on the origin of races have been the object of heated controversy in America. Darlington often quotes Coon with approval but gives no hint of the allegations of rank racialism which his work has engendered.

In short, *The Evolution of Man and Society* is a takeover bid. Nothing about man 'can be regarded as outside the scope of genetic enquiry'. This is an unexceptionable programme. What is worrying is that Darlington claims that he has reached a major conclusion on the basis of scientific evidence: 'The processes by which human societies evolve are thus in principle the same as those working at a pre-human stage of evolution.' The argument of the book, however, depends on the systematically ambiguous use of a number of key concepts. In particular, when cultural or historical terms would do as well, the author routinely substitutes biological terms. Thus there are about 90 occurrences of the term 'evolution', where 'history' or 'development' would do. Similarly, there are about 150 places where phenomena are explained in terms of genetics when there is little or no evidence to support the claim. His most dubious speculations characteristically appear parenthetically or are stated authoritatively as matters of fact. Finally, the style is portentous and very general claims and sweeping conclusions appear gratuitously with little apparent relation to the preceding narrative. It is often difficult to see how the historical narrative (interesting in itself) supports the biological thesis of the book, while the cumulative effect of the use of biological terms is very misleading. Surely one of the first duties of an innovator – and certainly of a popularising one – is to point out which of his claims are contentious and speculative and to present their evidential basis, lest the non-specialist reader be led astray.

There is hardly any human attribute or social change which is not at some point 'explained' in genetic terms. The author fails to distinguish customary or learned behaviour which may have an as yet unknown genetic component and equally unknown genetic consequences from stable customs and habits for which there is no known genetic basis. Here are a few examples of

phenomena for which large genetic claims are made: the incest taboo, belief systems, castes, skills, trades, moral codes, militarism, pastoralism, social classes, being a prophet, horsemanship, ability to govern, Christian tolerance, Jewish religious devotion, homosexuality and the master-slave relationship. The abilities of Newton, Bismarck, Marx and Lenin are largely attributed to their being class *hybrids*. The following quotations help to convey the tendency of the book: 'All advanced societies, as we have seen, arise from a stratification of social classes whose genetic differences and mutual dependence are the permanent foundation of their advance.' 'Indeed class differences ultimately all derive from genetic and, usually, racial differences.' 'The colonised and genetically stratified societies are, as always, advancing faster than the uncolonised or de-colonised and unstratified societies.' 'In short, racial discrimination has a genetic basis with a large instinctive and irrational component. Its action may be modified by education or by economic processes. But it cannot be suppressed by law.' On the next page science tells us that racial segregation and apartheid are not to be lightly dismissed. The principle of subordinating one racial group to another 'has governed the evolution of all advancing societies since soon after the beginning of agriculture. And it has been the means of their advancement.' By the way, 'All the great races of man differ in smell; they dislike one another's smell and are kept apart by it. But in the nostrils of all other races the pygmies positively stink. It is a property which has arisen from their genetic and ecological isolation.' The Irish, the working class and black men do not fare much better, though they have other genetic deficiencies.

On the positive side, Darlington has raised very interesting issues connected with unconscious selection, sexual selection, monasticism, primogeniture, hereditary immunities, and the dynastic and social consequences of inbreeding (and incest) *versus* outbreeding. The book also touches on eugenic questions which cannot be answered within the limits of science as now constituted. However, these matters are so inextricably interwoven with dubious assertions which are liable to reactionary and racialist interpretations that one would do better to start elsewhere. The relationships among natural selection, the transmission of cultural and social norms, and the processes of social change are far more complex than Darlington shows, and many potentially fruitful analogies between the social and biological sciences are by-passed in favour of his own conclusions.

Professional botanists and geneticists acknowledge that Darlington's cytological work of 30 years ago is internationally recognised as a lasting contribution to science. His ideas on the evolution of genetic systems, among others, have been very provocative. Similarly, specialists in the separate cultures and periods covered in parts of this book find his bibliography contains most of the basic works needed to prepare a high-level popularisation. He is Professor of Botany at Oxford, a Fellow of the Royal Society, and the author or editor of 15 previous books and innumerable scientific and occasional papers. In his own field his reputation is secure. However, all agree that a *very* firm line should be drawn between his solid cytological work and his stimulating ideas on the one hand and the social and political extrapolations which he has attempted on the other.

I have tried to convey the conclusion that in the opinion of one historian of biology this book is insidious, and its surface plausibility depends on ambiguities, premature conclusions and downright puns. At the end of the volume Darlington has listed 'a succession of pioneers'; in the proof copy these were called 'some precursors'. The list includes Paine, Malthus, Darwin, Marx, Galton, Bagehot, Tylor and Acton. A psychiatrist colleague of mine turned up the other day to discuss problems of explanation in that confused discipline. I spoke of my preoccupation with trying to convey the problems raised by this book. 'It is well-known,' he said. 'A specialised scientist stares down his microscope for 40 years and does very good work. Towards the end of his career he asks himself about the wider meaning of it all. He racks back the focus knob on the microscope, tilts the instrument back, and looks about him through its eyepieces. He stares hard for a time, a marvellous gleam comes into his eyes, and he exclaims, "I understand all!"'

RADICAL SCIENCE JOURNAL

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Editorial Statement

THE AIM OF THIS JOURNAL is to provide a forum for serious and extended analyses of the history, philosophy, ideology and current practice of science and technology, within a socialist perspective.

Science and technology permeate our society. Their consequences are seen by more and more people as disturbing or even frightening. Capital-intensive, technologically 'advanced' machinery is used to increase the productivity and discipline of the work-force. The hierarchical division of labour is increased, and so is the exploitation and alienation of the worker. He or she is 'deskilled' as traditional techniques are automated or computerised. Government and corporations, pursuing profits and production, pollute and destroy the world we live and work in. In the name of the consumer and the national interest they deplete our resources. Abroad and (in the last resort) at home, this society is maintained by force. The police and military use their technological arsenal (sophisticated surveillance techniques, CS gas, sensory deprivation, fragmentation bombs, the electronic battlefield) to defend it.

These are some of the more obvious effects. But science also underlies many of the assumptions and beliefs which people hold about the world. Scientism is rampant; increasingly, people understand the world by making analogies between scientific results and areas of human activity to which they are quite inapplicable. Deference to expertise is encouraged by the claims of science to neutral and objective knowledge. Powerful institutions claim the neutral authority of science for their self-interested policies by use of the management and policy 'sciences'.

Concern about the effects of science is not new, although it is now perhaps deeper and more widespread : e.g. the Marxist scientists of the 1930s, the Bulletin of the Atomic Scientists, and the Campaign for Nuclear Disarmament. The dominant perspective which we have inherited is the 'use-abuse' model of science. According to this model, scientific knowledge is regarded as a collection of objective statements about the natural and social world, a collection which is essentially neutral with respect to competing groups and classes. This neutrality enables competing classes to use the *same* scientific knowledge in pursuit of their conflicting goals. This perspective allows for no specifically socialist science, and therefore no distinctive socialist scientific theory and practice. The conclusion is that while in capitalist or other class societies science (and particularly technology) are inevitably used by the ruling class in exploiting and oppressing the working class (and the peasantry), under socialismthe *same* science can be used in the interests of all.

Most of us on the editorial collective started with something like this outlook. It is a reasonable description of the approach adopted by the British Society for Social Responsibility in Science on its foundation in 1969, and many of us have come to our present involvement in issues of science and society through BSSRS and its penumbra of groups and activities. However our positions have developed progressively over the past five years, as indeed has that of BSSRS. Other members of the rather diffuse editorial collective have come to these issues by way of the 'counter-culture', the women's movement, the organised revolutionary left, through their own experience of scientific work. While we are seeking to develop and unite our understanding of the issues, these differences of emphasis and style are still with us.

One point of general agreement is that understanding of the interaction of science and technology with other social activities and institutions is still primitive. It follows that our editorial policy leans towards inclusiveness rather than exclusiveness. The hallmark of our approach is tentativeness–we believe that a 'correct' line cannot be laid down at present, and that active debate must be the priority.

Apart from the production of this journal, the members of the editorial collective are separately engaged in more direct political activity. We would regard *Radical Science Journal* as a futile project if isolated from other political practice. We hope contributors will bear this in mind. and help to clarify the links between theory and practice. Our aim will be to avoid scientific and political jargon which makes analyses accessible only to an elite. Such contributions will not be published until they have been translated.

Where do *we* stand? We believe that our task is not to analyse *science*, but to understand *society* better through analysing the interaction of science and technology with other social activities. This understanding must provide, for ourselves and others, a guide to political action. We find the 'use-abuse' model inadequate for such understanding. For us the concept of scientific knowledge, and indeed the concept 'scientific' itself, is highly problematic. Within the physical and (even more) the biological and social sciences, it is possible to find broad theories which serve clear class interests, or which require underpinning assumptions about the role of man in nature. The meaning and status of scientific knowledge, theories or paradigms remain, to say the least, in need of critical analysis. Indeed it can be held that nature itself is a social rather than 'objective' category, so that scientific theories contain a social component at a very basic level. They reflect, reinforce, legitimate or undermine particular social practices through what they imply to be true or untrue, possible or impossible, scientific or unscientific. Scientific theories have an inescapably ideological function precisely because they lead us to understand and experience nature in socially conditioned ways. This does not mean that effective techniques cannot result from such work–scientific enquiry *does* capture an *aspect* of reality. The question is, what sort of aspect?

Within such a perspective, the concept of a specifically socialist science cannot be rejected. Scientific theories developed and interpreted in socialist societies within a socialist conception of humanity and nature will have quite different ideological functions, and qualitatively different social practices and social relations. Technology is more directly tied to the form of organisation and control of society. Our present technologies, developed to achieve the social ends of capitalism, have specific social practices and social relations associated with them. Socialist society therefore, with human liberation as its overall aim, can take over unquestioningly neither the language of established scientific theories nor their content nor the technologies developed and used in existing advanced societies.

The preceding paragraphs are not a manifesto, but rather an interim statement. They can only be an introduction to the range of problems which *Radical Science Journal* must handle. There are, of course, many other attitudes to science on the left which need to be critically analysed. One example is that of the counter-culture criticism of science, which rejects the possibility of a net benefit from the application of science, not just under capitalism, but under *any* organisation of society–a special but extreme case of our 'non-neutrality' model of science. The contention is that both the methodology and content of science have a logic of domination, and that the domination of 'things' leads inevitably to the domination of people. This clearly relates to our own concern about the tendency for scientific rationality to be taken as a model for all rationality. We do

not accept this condemnation of all rationality, nor do we accept that socialism is possible without the widespread application of science and technology in *some* form.

Nevertheless the extreme counter-culture case must be answered by socialists. So too the priority given by many scientists and non-scientists to the search for intermediate and alternative technologies designed to free the Third World from dependence on capital and technology from the developed world, and to free ourselves from a resource-hungry technology. Socialist discussion of the environmental movement has been largely limited to criticising its authoritarian and merely utopian elements. This criticism is justified, but in the absence of effective socialist analysis and propaganda, the utopians and authoritarians have achieved credibility and a large following among those who might otherwise share our political perspective.

One central aspect of any socialist approach to science and technology must be to consider in what ways they may be of use to rank-and-file and community organisations. Technology may have developed largely to meet the requirements of society's powerful hierarchical institutions, but scientists and technologists who commit themselves to working-class movements may be able to adapt these techniques for defence or counter-attack. Scientists and technologists themselves are being proletarianised, and the potential for political activity—such as scientific trade unionism—in this privileged sector of the working class needs to be explored.

To our knowledge, there has been until now no journal devoted to a discussion from a socialist/Marxist standpoint of such questions concerning the meaning and nature of scientific practice and knowledge. By providing such a forum we hope to fill what seems to us a major gap in current socialist theory and practice. The powerful connections of 'scientific' practices with economics, politics and ideology; the confusion over how socialists should see science and technology; and the fragmented, elitist, mystifying nature of scientific education, all make the creation of an effective, non-sectarian forum a matter of urgency. If this journal is to succeed in its objectives we need an abundant and continuing supply of contributions. We should add that while our overall intention is to provide a forum for socialist thinking on science, technology and society, we will also publish contributions from non-socialist authors which we think important.

The 'softer' or social sciences are clearly more accessible to analysis in this way, and *Radical Science Journal* starts off with a greater emphasis on these areas than we would like. This is an imbalance we hope to correct gradually as socialist critiques of the natural sciences are built up.

Readers and potential contributors should know how the collective takes editorial decisions. Normally an article is first vetted by a few collective members to see if it is potential *Journal* material. If their reaction is favourable the article goes for detailed review to three or four referees, some of whom may be from outside the collective. They report on the article's argument, coherence, style and accessibility. On the basis of those reports (and after all the collective members have had an opportunity to read the piece if the reports are favourable) the collective decides to publish or not, or to send back for possible revision. All of the articles in this issue have been through this process, some more than once. Members of the collective may disagree with the content of an article we publish, in which case they can submit a comment on it for publication. Naturally we hope that readers will also contribute to and initiate such debate.

This journal can succeed only if we receive a steady flow of radical critiques of the history, philosophy, ideology and current practice of science and technology. We therefore ask socialists and others sympathetic to our aims, particularly those working in the sciences, to contribute to the future of this enterprise by sending us articles, reviews, notes, letters and graphics for publication.

Although our short-term financial position seems moderately secure, we can only achieve medium-term viability if we can build up a broad base of subscriptions. You can help by taking out a subscription (we'll gladly accept donations too) and encouraging friends to do the same. Please recommend *Radical Science Journal* to libraries and get your local bookshops to stock and display it. Our intention is to make *Radical Science Journal* an effective part of socialist theory and practice.

Technology and the Construction of Social Reality

DAVID DICKSON

Technology has become an integral part of our social environment and an essential element in almost every field of everyday activity. One of the specific characteristics of contemporary society, for example, is that some form of machine is required to carry out most acts within the social domain. We use machines to travel, to communicate, to produce commodities, to provide services, and even to entertain each other. An understanding of the role of technology in society has become important, not just for its own sake but as part of our general understanding of society itself.

This paper has two purposes. The first is to attempt to outline the political nature and function of technology. This will be defined, not as a heterogeneous set of machines but as a social institution that embraces both the machines and the relations between them implied by their use. The second purpose is to indicate how the material role of technology in society relates to the socially-accepted interpretation placed on this role, in other words, to its legitimation.

My general thesis is that technological development is essentially a political process. In a material sense, technology sustains and promotes the political system within which it has been developed. At the same time, technology acts in a symbolic manner to support the legitimating ideology of this system—the interpretation that a society places on the world and on its position in it. Both these factors, I suggest, play an important part in determining the nature of technology itself. Thus, while arguing against those technological determinists who see social developments—including changes in social organisation and in the distribution of power—spontaneously emerging from developments in technology, I hope to indicate that it is equally possible to challenge those who support an economic determinism, in which technology is seen as no more than a tool of economic or political development. I would like to suggest that the social relations

of production—the relationships between the different social groups or classes involved in the production process—become incorporated in the means of production, and that technology and social patterns therefore reinforce each other in a somewhat dialectical fashion at both a material and an ideological level.

The implication of this thesis is that one can explain the nature of the technology developed by a capitalist society only by relating it to the patterns of production, consumption and indeed social activity in general that maintain the interests of a dominant section of that society. This is achieved through hierarchical patterns of social organisation, facilitating authoritarian forms of social control. These modes of organisation and control, I suggest, become incorporated into the technology that capitalism develops. Technology embodies the relations between men and thus becomes the apparent source of the exploitation and alienation to which the capitalist relations of production give rise. I would also suggest that many socialist countries, by appropriating and subsequently developing a mode of production initially formulated within a capitalist framework, have been able to introduce forms of social organisation and control that are essentially capitalist in nature in order to make effective use of this technology. Thus, although the analysis below relates primarily to the situation in industrialised capitalist societies, many of the arguments could equally be applied to industrialised nations of a nominally socialist nature.

The following essay is divided into four sections. The first section indicates how we might approach the political nature of technology as a social institution. There are two aspects of this. The first, from what might be called the synchronic perspective, attempts to look at a society's technology as it exists at any one point in time. Here I wish to suggest that technology can be viewed as a 'language' of social action from which we extract those individual elements—whether tools or machines—required to carry out a particular task. As a consequence of this, technology shares the same structure as that of the dominant modes of social action and interaction, themselves reflections of the distribution of power and the practice of social control. The second aspect is taken from a diachronic perspective and examines the historical development of technology. Here I suggest the crucial importance of understanding the political nature of the process of technological innovation. During the early stages of the development of capitalism, for example, innovation was used directly as one of the means by which the capitalist strengthened his control over the activities of the work-force. Innovation has subsequently become institutionalised into the category of research and development, an essential element in the economic machinery of the contemporary capitalist state. At the same time, it can still be shown that innovation is used in a directly political manner to maintain and reinforce the control of a dominant social class.

The second section of this essay deals with the way that society *interprets*

the nature of technology and of technological development. I wish to suggest that this interpretation exists in the form of a myth. A myth provides an apparent historical interpretation of a particular state of affairs. The Romulus and Remus myth, for example, provided an explanation of the origins of ancient Rome. In the case of technology, I suggest that this explanatory function is achieved by the myth whose message is that 'technology is politically neutral'. Furthermore, I suggest that this myth implies a particular interpretation of both the nature of technology and the process of technological development, one largely derived from a positivist interpretation of the practice of science and the extension of this into the world of everyday experience where it becomes expressed as the social philosophy of 'scientism'.

In the third section, I shall indicate how the existence of this myth determines the way we are led to interpret the nature of technology and some of its implications. In particular I suggest that technology functions symbolically within a number of what I call 'modes of significance': the various perspectives or frameworks (be they economic, political, artistic or whatever) that we use to interpret events or experiences and hence to construct our own social reality. Here again I suggest that technology is generally interpreted to function in a neutral manner, merely providing the symbols by which particular notions are expressed or 'signified'. As with the material function of technology, I shall argue that this interpretation disguises the essential symbolic role that technology plays within each of these modes of significance; furthermore, to divide experience into such modes is already to distort such experience, a distortion linked to the atomistic and reductionist tendencies of scientism.

Finally, I have added a very brief discussion of how it might be possible to break out of our present situation, to strip away the mythological disguise that surrounds technology and reveal the political reality that lies beneath. This, I argue, can only be achieved through activities that directly challenge the content of the myth of the political neutrality of technology, as well as the power structure which this myth maintains and on which it relies.

The Political Nature and Role of Technology

In the way that the term 'society' is used to indicate not only a collection of individuals but also the relations between them, so 'technology' will be defined as an abstract concept embracing both the tools and machines used by a society and the relations between them implied by this use. Therefore, in the following discussion no distinction is needed between machines and tools, both of which can be defined as objects selected or fabricated by humanity as a means of changing the state of our material environment. The above definition allows us to see technology, in common with the legal or the educational system, as a social institution. It can also be distinguished

from technique, which may be defined as the act of applying knowledge to a particular task, whether directly or with the aid of a tool or machine—that is, with the aid of an element of technology.

It is through the action of individual machines and tools that technology is directly experienced. Furthermore, each machine that we experience is used by an individual or a group as a means of carrying out a particular action in the social domain. If we see a car in the street, for example, we know that it is being used by its driver to make a particular journey in a particular manner; if we see a telephone in a house, we know that this is one way its owner communicates with other members of society; if we see a computer on a factory floor, we know this is one way management has decided to control a production process. We might say, therefore, that each machine that exists in a society objectifies a particular mode of social action, in other words, gives this action an objectified form. A car can be looked at as the objectification of private motor travel, a computer can be looked at as the objectification of the exercise of a certain form of control over a production process, and so on.

Beyond those activities which the individual can carry out without the aid of machines or tools—a sphere that has continuously diminished as technology has increased in importance—all other social actions require the manipulation of some form of machine or tool. We can therefore say that, at this level, the machines to which the individual has access coincide with the forms of possible social action that lie open to him. These actions include those which might appear relatively personal, such as cooking; not only does such an action rely on the activities of others—those who produce the food or the power used—but also the way that the activity is carried out, and hence the specific machines that are used in the process, tend to be held in common by all members of a society. Virtually our whole daily routine is achieved through machines. Shaving, making coffee, frying an egg, catching a bus or a train to work, speaking on a telephone, watching television—each of these actions is achieved through the use of an element of technology. And the succession of machines that we employ can, conversely, be interpreted as the 'objectifications' of our sequence of daily activities.

Technology, in fact, can be considered as a 'language' of social action. From this language we select those individual elements—whether tools or machines—which we then apply to carry out a particular task. The instrumental (means to an end) activity of the individual, carried out through the successive applications of a variety of machines, might be considered as a personalised form of 'speech', corresponding to the notion of a personalised technique as it has been defined above. And in the way that language is experienced through the spoken or written word, which therefore become the objectifications of social ideas and concepts, so, as we have seen above, technology represents in the individual machines of which it

is composed the objectifications of social action. Susanne Langer has written, in her book *Philosophy in a New Key*, that 'words are certainly our most important instruments of expression, our most characteristic universal and enviable tools in the conduct of life'. We can turn this analogy around and suggest that machines increasingly provide the means by which social actions are organised and objectified as a language of action from which elements are selected and utilised as required by an individual or a group in order to carry out a particular task or activity.

While machines, like words, coincide with possible social actions that the individual is able to carry out, they also, again like words, implicitly contain constraints on this action. Although we can carry out those activities in a social domain for which suitable machines are available, at the same time we are unable to carry out those activities for which machines are necessary but are not accessible. Indeed, some activities become excluded by the introduction of a new machine. We cannot, for example, travel by train to those destinations from which services have been withdrawn or, as I have discovered, be notified by a library with a computerised loan system immediately a book has become overdue as, although technically feasible, such possibilities are not incorporated into the design of the respective system or machine. Few of us are in the position to devise machines to perform tasks which we have selected or defined ourselves. For the most part we have to rely on those machines which society makes available to us. The institutionalisation of technology has meant that the choice of particular machines, or at least the control over this choice, remains in the hands of a dominant social class. And since technological innovation, as we shall suggest below, is only carried out to the extent that it coincides with and maintains the interests of this class, new machines will only be introduced within the constraints that are imposed on the activities of the individual members of society.

A particular case of the possibilities and constraints expressed by technology is that contained in the forms of communication that can take place between different members of society. The design of media of communication determines the way in which it is possible for the individual to communicate with other members of society at anything more than a face-to-face level. The telephone system, for example, makes long-distance conversation possible between two individuals. As our present telephone system is organised, however, it excludes the possibility of group discussion without elaborate prior arrangements being made with the Post Office. The technical aspects of implementing this are, I have been assured, relatively simple. Yet the system was designed primarily for two-way conversation—the dominant mode of communication within an individualistic society—rather than group discussion. Another example of the constraints on communication implied by the telephone system is the financial one. Those with sufficient means are now able to converse on a global basis. Those without these means

are either denied the opportunity to use a telephone at all (at least in their own homes) or, if they attempt to use the system without paying, become treated as criminals. (The absurdity of the situation is shown up by the fact that those convicted at a 'phone phreak' trial in London could only be accused on the charge of stealing electricity, a charge on which half of the accused were found not guilty.)

Other modes of communication, such as television or the railways, offer similar examples of how the articulation of a particular aspect of technology as a social institution already coincides with the way that it is intended the technology should be used. The railway system, for example, with its heavy emphasis on conveying businessmen between centres of production or commerce and its systematic pruning of 'non-economic lines', immediately shows us what are considered to be the 'important' routes on which fast and efficient transport is 'necessary'. (This point is underlined by recent attempts by the EEC Commission in Brussels to get the railway authorities of different Common Market countries to co-ordinate their development plans aimed primarily at speeding up rail travel between its commercial centres.) The railway system can thus be seen as an 'objectification' of one of the modes of communication required by a capitalist economy, one that requires the rapid transport of its business executives—and occasionally other classes, as in the case of migrant workers or those on holiday—yet is frequently able to ignore the interests of those less essential to the functioning of the economy, and indeed often to withdraw services to these in the name of 'rationalisation'.

Another area in which the freedom allowed the individual, and the constraints imposed on him, become directly expressed in technology is that of industrial production. Within capitalist society, production is organised on the basis that the activities of the worker are subordinated to the decisions of management, these decisions being an expression of the interests of capital. The relationship between worker and manager is therefore a hierarchical and authoritarian one, with production ideally organised in a way that enables decisions to be taken at the top and carried out unquestioned at the bottom. Industrial technology not only provides, in its individual machines, the physical means by which decisions about the nature of his work are imposed on the individual worker, but also a formalised system—such as a production line—the means by which a general pattern of social control is maintained. Stephen Marglin, for example, has emphasised how in the early stages of the Industrial Revolution the reasons for organising production in the weaving industry on the basis of factories was not, as often claimed, a move made necessary to exploit the capacity of new machines. It was a direct move by early merchants to gain control over the activities of weavers—a control that they had lacked when production had been organised on a 'putting-out' basis and the weavers had been able to work relatively independently. More recently, an advertisement for a system of

factory-based building construction methods claims that one of the problems of traditional site construction is that 'the labour force [is] difficult to supervise and often unskilled'. The introduction of such methods is a direct response, in other words, to recent labour unrest in the building industry. One advantage, it claims, of factory prefabrication is that 'We've no problems of scattered supervision. So quality's always up to scratch' (*Sunday Times*, 18 November 1973).

Within capitalist society the wealth produced by the productive activity of the worker is turned into capital, much of which is subsequently reinvested in technological equipment. This equipment is frequently used simultaneously to harness the worker to a particular type of work, to dictate the rate at which he works and to achieve the reproduction and increase of the capital that employs him. Industrial technology therefore embodies the social relations between men required by a capitalist economy. 'The development of the means of labour into machinery is not fortuitous for capital; it is the historical transformation of the traditional means of labour into means adequate for capitalism', writes Marx in his *Grundrisse*.

We can extend the analysis of the technology employed in industrial production to the whole of the social world. Capitalist society relies for its reproduction on the rapid turnover of commodities and hence on the exercise by the producer of close control over patterns of consumption. In many cases the machines purchased by the individual have been designed with these considerations in mind. We have, for example, the concept of 'built-in obsolescence' incorporated in a machine to make sure that it does not last for too long or stylistic innovations that are used to identify 'next year's model' that will (once and for all, we are told) put Mr Jones in his place. One of the major criticisms of the drug industry contained in the Sainsbury Report of 1965 was that far too much research was put into relatively minor adaptations to well-established drugs, while one US businessman replied to a survey on industrial research policies that the main problem was 'to obsolete our products before someone or something else does'. Thus even when looking at the machines possessed and used by the individual (and even a drug can, to a certain extent, be viewed as a medical 'tool'), we see that each machine serves to meet his apparent needs only to the extent that his 'consumption' or 'appropriation' habits conform with the behaviour that is required of him to maintain the effective operation of a capitalist society. Even the do-it-yourself movement, whether in house-building, car repairs or whatever, has been turned by commercial exploitation into an extension of those industries.

In general we can say that a society's technology, when viewed as a social institution rather than a heterogeneous collection of machines and tools, is structured in a way that coincides with its dominant modes of action and interaction. Although these modes of action and interaction are maintained and often reinforced by technological factors, they are primarily a historical

product of the power relationships within a given society and hence of the economic factors by which these relationships are expressed. Yet the modes of action and interaction embedded in technology themselves coincide, as we have seen for example in the case of the telephone system, with the pattern in which power is distributed in society. Technology does not just provide, in its individual machines, the physical means by which a social society supports and promotes its power structure; it also reflects, as an institution, this structure in its design. A society's technology can never be isolated from its power structure and technology can never be considered politically neutral.

So far we have looked at technology from a synchronic perspective that provides us with a sort of 'snap-shot' view at any particular point in time. Society, however, is in a continuous state of transformation. If we are to understand the role that technology plays in this process, we must look closely at the process of technological innovation as the way that particular machines are incorporated into the functioning of society. Innovation, as a socially-mediated process, should be distinguished from technological invention—the fortuitous and often accidental creation of a new machine. The former frequently follows the latter, but this sequence is by no means inevitable (although one of the implications of the growing economic importance of the activity known as research and development is that it becomes increasingly difficult to distinguish between the two).

Technology, as a social institution, can be seen as a cross-section of the process of technological innovation in the way that the photograph of a football match, for example, provides a static description of the situation at a particular instant in the match. And just as an understanding of football requires a simultaneous awareness of its 'diachronic' and its 'synchronic' aspects—for example how a left-wing attack is generated and how a goal is scored respectively—so it is equally important to study both aspects of technology to grasp its full political role.

Here I would again argue the essential nature of technological innovation as a political process. In an attempt to guarantee the maintenance of its hegemony as a prerequisite to its material prosperity and sometimes its very survival, the dominant class in any society—whether feudal, aristocratic or capitalist—will attempt to ensure that wherever possible technological innovation remains subordinate to these requirements. Although the increasing efficiency of the production process may be required by a capitalist society in order to ensure its *economic* survival, and this in itself be one determining factor in technological innovation, at the same time innovation has become an important means of attempting to ensure its political survival too. In the terms of our previous analysis, one of the purposes of innovation is to maintain and, where possible, to reinforce the dominant modes of action and interaction in any society, and hence the existing distribution of power. As long as technological innovation remains in the hands of a dominant social class, it becomes an important means of defending the power structure. And it

is only, I would maintain, when the hold on innovation is wrested from this class, as it was from the feudal landlords by an emerging merchant class in the early stages of capitalism, that technology's potential as providing the means of sustaining a new form of power structure or a new system of social relations of production is released.

During the early stages of the Industrial Revolution the first capitalists faced the problem of exerting discipline and control over the workers whose labour power they required. We have already referred to the way the factory system was initially developed as a means to this end. But the machines, too, that were subsequently introduced in such factories were frequently brought in with the conscious intention of providing a means by which this discipline and control could be maintained. A number of examples of how this was achieved are given by Andrew Ure in his book *The Philosophy of Manufactures*, first published in 1835. He quotes, for example, the decision of a number of manufacturers to extend the size of the spinning mule. By decreasing the overall number of mules required in a particular factory, adult spinners were displaced and the number of their assistants increased, thereby weakening the factory apprentice system and reducing the spinners' authority. Although such modifications were often costly, Ure, referring to early labour troubles in the spinning factories, writes that 'in doubling the size of his mule the owner is able to get rid of indifferent or restive spinners and to become once more master of his mill, which is no small advantage'. In both the development of the factory system and the subsequent technological innovation, we see how the new patterns of social control of industrial capitalism become incorporated in the modes of production.

'One can understand why the thoughts of employers turned to workshops where the men would be brought together to labour under watchful overseers, and to machines that would solve the shortage of manpower while curbing the insolence and dishonesty of the men', writes David Landes in *The Unbound Prometheus*. Discussing the same period, Marx remarked that 'no special sagacity is required in order to understand that, beginning with free labour or wage-labour for example, which arose after the dissolution of serfdom, machines can only develop in opposition to living labour, as a hostile power and alien property, i.e. they must, as capital, oppose the worker'.

Within contemporary society, technological invention and innovation are drawn together and institutionalised into the category labelled as 'research and development'. Although this process appears to be determined primarily by economic considerations, it is not difficult to see how the institutions of government, industry and universities become increasingly interlinked into a single system whose purpose is to ensure the economic growth and the political stability of industrial capitalism.

Beyond its merely economic function there are a number of ways in

which the political dimension of technological innovation becomes apparent within the industrial situation. In particular, there are four situations—at least—that we can identify. The first is where technological innovation is used directly to increase supervision and tighten control over the activities of the work force. The instance quoted above of the introduction of factory-based construction techniques in the building industry is one example. The second situation is where innovation has been used as a means of introducing stability into a work situation by displacing militant factions of the work force; here the introduction of containerisation into the ports provides a typical illustration. The third situation is one in which the purpose of innovation is to achieve an apparent improvement in the work situation and hence a removal of possible sources of conflict; the widescale introduction of working methods referred to as job enrichment, where work is carried out in apparently independent teams rather than in a linear production line system, is a typical example of this. By placing relatively insignificant decisions in the hands of the worker, such as the rate at which he decides to work to meet a predetermined target, it is hoped that pressure will be taken off demands on significant issues, such as rates of pay or the level of targets. Finally, technological innovation can be used as a threat to blackmail sections of the work force into particular tasks; a number of employers, for example, have claimed that if demands by women machine operators for equal rates of pay with men were granted then the employers would introduce machines to take over the women's work.

Thus in both direct and indirect ways we can see how technological innovation serves a political purpose by maintaining and reinforcing the position of a dominant social class, and how technological development can therefore be interpreted as a political process. Again this illustrates the central thesis that technology, as a social institution, plays a directly political role in society. It demonstrates the manner in which technology coincides with the dominant modes of action and interaction. It can be seen as one of the essential mechanisms by which a particular form of social control is maintained.

The Interpretation of the Role of Technology in Society

So far I have concentrated on a description of the objective functioning of technology in society. This analysis, however, would be sufficient only if all actions were purely instrumental in nature—in other words, perceived and carried out merely as a means to an end. It would also require that the consciousness of the individual was developed through a relatively simple feedback mechanism from an awareness of the social world as a mere object and from the effects that his instrumental actions have on this world.

We must now ask, however, how the individual perceives technology and his relation to it. We must deal, in other words, with the realm that

covers the *significance* attached to technology as well as its objective role. I hope to indicate how this significance serves to maintain the cohesion of the social world in which we operate, and the social reality that we inhabit. It does this, I suggest, by providing us with an interpretation of technology that disguises the exploitative and alienating role technology plays within industrialised capitalist societies and leads us to accept a particular mode of technological development as being a unique, inevitable and politically neutral process.

This interpretation, I maintain, is experienced through one of the major myths of contemporary society: the myth that technology is, in fact, politically neutral. One of the functions of any myth is to explain or interpret a certain situation or set of events by providing them with an apparent historical context. This is used either consciously or unconsciously as a substitute for their true history and hence removes such a situation or event from an awareness of its material genesis. Many myths, for example, such as the stories of the Old Testament, have formed an important element by which the social function of religion is articulated. Myths, in other words, are one important way in which any ideology is experienced by an individual.

The myth of the political neutrality of technology is frequently invoked both by the politician seeking to explain the effects that a particular technological development has on society, and the technologist seeking to disclaim responsibility for such effects. Inadequate policies or insufficiently sophisticated technologies will be blamed for problems such as industrial pollution, traffic congestion or the indiscriminate nature of military technologies. In each case the technology itself remains blameless. Scientists and technologists who have developed techniques that are subsequently used in military applications—such as defoliants in Vietnam or infra-red rifle-sights for night use in Northern Ireland—claim that the responsibility for the *development* of such techniques should be clearly distinguished from responsibility for their *use*.

The substance of the 'technology is politically neutral' myth is that the outcome of any particular act involving the application of an element of technology is determined entirely by the motives behind that act. The technology utilised is held to be entirely independent of such motives. It claims that we first select a particular task that we wish to carry out and then select the appropriate machine which will enable us to carry out this task. Although the choice of the task may, according to this notion, be consciously or unconsciously determined by political factors, the subsequent choice of machine or technique to achieve the task is claimed to remain a politically neutral, technical task.

There are, of course, other myths that have been associated with technology in the past. Industrial power, for example, was an important symbol of Britain's imperialist strength during the 19th century, while heavy industry took on an almost mystical significance for the Bolsheviks in the

early years of the Russian Revolution. There is also the contemporary myth that our social problems are in many ways *caused* by advanced technology: in other words, that advanced technology has become intrinsically anti-human. Although this myth ignores the political factors behind the oppressive nature of technology in advanced capitalist societies, it nevertheless comes close (in a number of respects) to the interpretation I have suggested in the previous section by refusing to separate the nature of technology from the uses to which it is put.

The myth of the neutrality of technology legitimates the contemporary social function of technology by attempting to take it out of the realm of political debate. We cease to question the nature of the institutionalised structure of technology and the way in which it has developed historically; we accept both as natural, or rather as inevitable. Thus we are told that the railways are run in a certain way because it is the most 'efficient' way to do so, or that the only way of getting over current environmental problems is by increased technological sophistication.

Furthermore, this myth is an essential element of a general mechanistic and functionalist ideology of contemporary society. This ideology postulates an equivalence relationship between the concepts of industrialisation, modernisation, and technological and social development. It might, therefore, be called the 'ideology of industrialisation'. It is this ideology, for example, that legitimates the continued pursuit of economic growth by placing primary emphasis on the generation rather than the distribution of wealth. It also seeks to explain the functioning of society purely in operational terms, denying the relevance of political considerations concerned with the distribution of power or patterns of social control. Within industrialised capitalist countries—and frequently in the underdeveloped countries too, when a nascent capitalist economy has been enticed into a world market—it is used to suppress the potentialities for individual and social emancipation offered by particular machines and legitimate their use towards socially exploitative ends.

The same myth provides an interpretation of technological innovation as the objective, technical response to particular circumstances. Here the legitimation can be expressed in terms of 'increased efficiency' (e.g. the introduction of assembly-line production techniques), as the 'technical' solutions to social needs (e.g. the development of a new transport system), as the economic 'rationalisation' of outdated techniques (e.g. the introduction of automatic telephone systems), or even as the need to maintain a competitive position in a world market (e.g. Concorde). Whatever the terms used, they attempt to imply an objective rationality to the process of technological innovation that presents any particular innovation as being the 'logical' response to a given objectively-defined situation.

Already it can be seen that many of the terms and concepts utilised by such an interpretation have been derived from the practice of science.

Indeed, it is possible to suggest that the legitimation of both the social function of technology and technological innovation is based largely on the ideology of 'scientism', which is itself legitimated by the positivist interpretation of the practice of science as the pursuit of an empirically discovered objective truth.

Positivism as a natural philosophy combines the traditions of empiricist and rationalist thought. This was originally done to strengthen the belief of scientists (and frequently of non-scientists too) in the exclusive validity of scientific truth. Scientism is the transformation of positivism into a social philosophy, the basis on which man explains and interprets the nature of society. As such, I maintain, it forms an essential element of what has been referred to as the ideology of industrialisation. In essence it conveys the conviction, as Jurgen Habermas expresses it, that 'we can no longer understand science as one form of knowledge, but must rather identify knowledge with science' (*Knowledge and Human Interests*). The basic message of scientism is that an apparently 'scientific' approach to any problem or situation is both necessary and sufficient to indicate how its objective, politically neutral resolution can be achieved.

Furthermore, it can be argued that the importance of scientism, as part of a contemporary ideology, is closely associated with the heavy dependence of advanced industrialised societies on discoveries made in the scientific laboratory. In traditional tribal societies technology was derived directly from human practice; this practice was implicitly related to the dominant values and social patterns of the prevailing culture. The very scale and complexity of modern technology, however, precludes the possibility of its development from direct practical experience. Technological development now relies heavily on the application of abstract modes of reasoning and on knowledge, frequently—although arguably—attributed to the results of scientific investigation.

Technological innovation does not spring directly from a laboratory bench. The application of abstract knowledge to practical tasks, as we have seen above, is already a politically determined process. Its legitimation is provided by the apparently neutral interpretative scheme of positivist science, rather than collective practical experience. It is the legitimation that science provides, as much as the potentially useful knowledge that it produces, that is important for an understanding of the social role of contemporary technology.

This process can be seen at work in the development of ideas of scientific management. Under the application of scientific management, the division of labour that emerges from the supposed scientific analysis of the practice of commodity production becomes embedded in the design of production technology. The way a factory production line is designed, for example, implicitly contains the idea that production line methods are the most appropriate for a particular mode of production. The social fragmentation of the

work force necessary for the efficient running of a hierarchical system becomes directly experienced in the physical space placed between each worker on the shop floor. Such innovations are justified, however, on the grounds of 'increased economic efficiency' or some such argument, implying that it has been possible to mathematicise the work situation in an objective, scientific way. Scientism thus legitimates a process by which the division of labour, initially emerging as an economic and social necessity, has subsequently become institutionalised in the very design of production technology while at the same time being fashioned to meet political ends. (For further argument see Gorz; Hales; Young.)

The same situation occurs outside industrial production. Edmund Leach, for example (in 'Has Science a History?'), has suggested how science is used as a legitimating factor in contemporary medicine. 'Medical knowledge has ordinarily been acquired and preserved as a technology: a practical way of dealing with a particular situation', he writes. 'It was only quite late in the present century that it came to be generally accepted by the medical profession that innovations in medical practice ought to be justified by scientific criteria.'

In social planning the use of supposedly objective models of human and social behaviour serves to legitimate the imposition of social policy. Development planning in the underdeveloped countries has similarly been predicated on a model of Western positivistic rationality which seeks to imply that the social problems of these countries can be solved by the rational application of Western science and technology and by fostering the development of Western-style institutions, attitudes and values.

The use of the 'scientific method' in this way makes it important to refer briefly to the origins and development of modern science. It was during the philosophical revolution which took place in Western Europe in the 16th and 17th centuries that the general validity of a secular mechanistic science as a natural philosophy began to be accepted, not just by a few isolated intellectuals but on a broader social scale. Breaking away from the essentially non-functional traditions of Greek science, the first modern scientists (including figures such as Copernicus, Galileo and Francis Bacon) began to realise the potential value of scientific knowledge to society as providing it with the means not only to interpret the world, but also to change it. The philosophical basis of this new science was provided by Descartes, whose strict division of reality—or rather of knowledge of reality—into the categories of subject and object has provided the basis of the practice of Western science for the past three hundred years.

Descartes' method was not the only one available. The philosopher Leibniz postulated an alternative interpretation of reality in his *Monadology*. His philosophy of monads was based on concepts of wholeness; it emphasised the relationships between phenomena and between events rather than the distinctions which separated them. Yet this precluded the possibility of a

scientific method that would allow men direct access to the forces of nature and was hence unable to ally itself to an instrumental material base. It was Cartesian logic that held the day; Leibniz's ideas were allowed to fall into obscurity. One consequence of this was the apparent social acceptance of a cleavage between values (the realm of the subject) and facts (the realm of the object). This cleavage in turn appeared to legitimate the separation of feeling from thought, a process that can be traced in almost all fields of cultural activity. After the time of the metaphysical poets, to take but one example, we can recognise a sharpening of the distinction between poetry and prose. Rousseau later claimed that 'Descartes' philosophy had slit the throat of poetry'.

This whole process was accompanied by the emergence of the notion of purity applied to various forms of cultural activity. Science, despite recognition of its instrumental importance, became 'pure' science, consciously divorced from its social applications and frequently justified in its own terms as the disinterested pursuit of knowledge. The idea of art for its own sake (*l'art pour l'art*) appeared at roughly the same time, first in the busy commercial climate of 15th-century Italy and subsequently in 17th-century France under the politically motivated encouragement of Louis XIV. During this and subsequent periods, the myth of the intrinsic value of the art object was used by an emerging merchant and capitalist class to legitimate the appropriation of wealth by objectivising it in non-commercial (i.e. cultural) commodities.

We even find the concept of pure work beginning to emerge at a roughly similar period with the beginning of the wage system of labour. Labour power was now something that had to be exchanged; this relation necessitated the dissociation of the worker from his social context, in other words his alienation. Originally subject to collective group relationships, themselves responsive to the natural rhythms of social intercourse, work became regarded as an economic commodity as it was gradually drawn into the organisation of factories where it was directly subject to principles of efficient organisation of production. This process was assisted by those religious movements such as Methodism which preached that hard work was *per se* a virtuous and soul-saving activity.

The idealistic concept of the intrinsic value of an individual's activity, whether mental or manual, divorced from its social role hides an important paradox. Advanced industrialised societies tend to be characterised by a strong moral and cultural emphasis on the value of abstract scientific knowledge—even with no immediate or apparent application in sight—and on the apparent need to push back relentlessly 'the frontiers of scientific discovery'. Yet the fruits of the application of this knowledge to practical tasks are an essential element in the efficient operation and expansion of such societies. Whereas in a cultural domain the abstract, almost metaphysical qualities of science squeeze out the more mundane instrumentality

of technology, within the sphere of social production the relative importance of the two is dramatically reversed. In other words, the stress placed on the cultural importance of abstract science legitimates the ideology of scientism. Yet it disguises not only the exploitative way in which science is put to practical use through technology but also the fact that the very existence of contemporary science—in terms of support for research and development—results directly from this practical use.

A further aspect of scientism is that it promotes a passive acceptance of the existing state of affairs. The function of scientists becomes to provide an explanation for why it is 'natural' that things should be—and thus remain—as they are. Scientism, by accepting such explanations, stimulates a general amenability to manipulation by the dominant political forces. Science may contain the potential of being transformed into a materially progressive force; but under the ideological disguise of scientism it seeks to obscure the possibility of political emancipation and is used to suppress such a possibility by preaching the 'natural order' of the existing system. It dismisses as irrational or unscientific any attempts to challenge our contemporary situation in terms of the class interests which it maintains. (A group of correspondents to the *Guardian* in April 1973 drew attention to a news story reporting psychological 'research' as indicating 'sexual attractiveness as a predictor of attitude and response to risqué humour' by looking at reactions to 'dirty postcards'. They write: 'This is surely the most blatant use of the label of "science" to legitimise what is actually prejudice, stereotyping and denigration of women.')

It is not difficult to find expressions of this ideology. A typical illustration is provided by Victor Ferkiss in his book *Technological Man*, when he writes that 'technological man, by definition, will be possessed of the world view of science and technology, *which will themselves provide a standard of value for future civilisation*' (my italics). Others such as the French biochemist Jacques Monod have claimed to demonstrate that science itself provides a rationality that has superseded, and hence can be considered superior to, previous forms of religious or political rationality. Scientific truth becomes the substitute for religious truth. Yet its ideological and dogmatic nature—by definition imperceptible to those who believe it—is no less dangerous than that of religion. For by attempting to set up a system of rationality that is apparently *above* humanity, it distracts attention from the way that such a rationality can be used to obscure the nature of the relationships *between* people.

The officially sanctioned view of the world becomes one seen through the 'objective' categories of science. We see, for example, the Roskill Commission on the siting of London's third airport taking into account only those social factors which can be 'scientifically' quantified; or we see behaviourist psychologists reducing the cultural and social differences between ethnological groups to measures of performance in an 'intelligence'

test. Economists tell us that if something exists then we can quantify it; sociologists and psychologists conversely tell us that if we can quantify a concept (such as intelligence) and show that it behaves in a quasi-scientific way then this is sufficient to indicate that the concept exists in an objective fashion.

Furthermore, by maintaining the Cartesian split between the abstract world of the subjective and the material world of the objective, the ideology of scientism legitimates the idea that conception of a particular line of action can and should be divorced from execution, and theory from practice. Whereas Marx suggested that human labour was a value-creating process that integrated the essential elements of social life, and in particular brought together the realm of the subjective and that of the objective, scientism seeks to legitimate the division between, for example, 'cultural' and instrumental activity and thus to maintain the confrontation between subject and object.

We see the false distinction between objective and subjective reality reflected in what is referred to as the 'two cultures' debate initiated by C.P. Snow in 1959. Although it may not have been Snow's original intention, this debate has now come to signify the confrontation—between the sciences on the one hand and traditional human interests on the other—that appears to manifest itself in many aspects of social life. What the debate generally fails to do, however, is to distinguish the essential nature of an activity from the way in which it has been institutionalised, and subsequently mythologised, as part of the dominant ideology. The reasons for the apparent contradictions between the sciences and the humanities are to be found in an ideology that first identifies these as autonomous activities independent of the material base of society, and having done so, gives them opposing roles in a way that virtually guarantees the emergence of conflict.

Technology as a Social Reality

In the above two sections I have indicated firstly how technology functions in a political fashion in society and, secondly, how its political role is disguised by the dominant ideology of the system. This is done through the existence of the myth of the neutrality of technology, and the interpretation this provides of both the social function of technology itself and the process of technological development. In this section I hope to indicate some of the consequences that this interpretation of the role of technology and its basis in the legitimating ideology of scientism has for the way that we interpret the nature of technology. I want to suggest that in a symbolic sense technology appears as an inevitable or given element of the social reality that we inhabit. This reality appears divided into separate ways of looking at events or phenomena, for example from a political, a historical, a cultural or an economic perspective. Technology, or rather the symbols that it provides, appear to play a neutral role within each perspective, as the mere expression of a reality that lies behind it.

It is this situation, I suggest, that attempts to persuade us to ignore the ideological nature of our interpretation of the function of technology and accept technology as a 'natural' element in our social reality. In other words, not only does technology function politically in a material sense to promote the interests of a dominant social class by maintaining a particular form of social organisation; it also, I suggest, functions politically in a symbolic way in that the particular interpretation placed on technology is one designed to support a dominant ideology. Because the individual is conditioned to accept a socially given interpretation of the world, and the place of technology in it as being natural and hence inevitable (through, for example, the reinforcement provided by the educational system or the mass media), the political nature of this interpretation is obscured. Furthermore it is obscured within each apparently autonomous perspective on reality. It also leads us to interpret the instrumental activity of the individual as the consciously designed acts of a socially autonomous, rationally motivated subject, rather than as itself a process which is subject to the societal constraints imposed on the individual on the one hand and the expression of his own physiological motivations on the other.

The importance of looking at the way technology functions within different perspectives, or what I shall call 'modes of significance', springs from the fact that an individual machine performs many roles in society. A car, for example, can function for its owner not only as a means of private transport—its direct material function—but also as a social status symbol, a weekend hobby, a means of self-expression, a desirable object, a business reward (in place of a salary increase) or even, if suitably antique, as an economic investment. A recent advertisement, for example, implied that the Italian gliding down the Autostrada del Sol at 200 kilometres an hour in his Rolls Royce 'is fulfilling some important aspects of his nature. His love of beautiful things. His appreciation of engineering technique. His passion for driving. And his pride.'

For the car manufacturer, the car functions simultaneously as a profitable commodity, as an expression of his apparent contribution to the development of society, or as a determining factor in the design of his production line or his sales strategy. For the non-owner, the car can become a pedestrian hazard, an environmental menace, a direct challenge to public transport, or a sublimation of unconscious drives. To those living in underdeveloped countries, it can function as a status symbol, a desired by-product of modernisation, a source of employment—either in factories or garages—a drain on foreign reserves, or a symbol of economic exploitation by neo-colonialist forces.

The same machine functions simultaneously in many different ways—or, one might say, expresses a number of different values. Many of its functions differ from those which the machine was initially designed, in an instrumental sense, to fill. The Model T Ford, for example, was in no sense 'designed'

for its antique value sixty years later. The total symbolic function of each machine is therefore a composite one. Although it may originally have been developed to perform a specific instrumental function, as the means to a particular end, its existence within a social environment endows it with a range of further functions. For its existence the individual goes far beyond the material properties of the objects of which it is made up, as it provides him with the framework for his whole interpretation of reality.

One can no longer say that *the* function of a machine is to perform a single, isolated task. Each performs a range of functions. And in many cases these functions are purely a product of social environment—cars as antiques, for example—and totally unrelated to any material role. Conversely, to condemn a machine merely on the basis of one of its roles—cars as a source of pollutants, for example, or computers as a source of clerical error—is shallow (even if deserved) criticism. It is misleading to separate one role of a machine from its others in this way, just as we have previously seen it to be misleading to separate the action of one machine from that of another in assessing the function of technology as a social institution.

The essential task is to discover how one particular role relates to and reflects the 'values' of the social milieu in which it has been defined, and of which the machine has come to form both an extension and an integral part. To do this it is necessary to re-establish the connections which have been severed between what I have called the different modes of significance in order to see the extent to which the articulation of 'meaning' within each is itself a socially determined process. We tend, for example, to separate the apparent economic value of an object from its apparent aesthetic value without realising the extent to which both are expressions of a particular way of looking at the world, itself the product of our social experience within a class-based society.

Taking the above illustration of the motor car, we might say that when interpreted as an economic investment, a business reward, a profitable commodity, a challenge to public transport or a drain on foreign reserves, we are referring to its economic function or significance. As a status symbol or a symbol of foreign oppression it is functioning in an immediate social or political way. Interpreted as a source of pollutants or as an object requiring an amount of non-renewable natural resources for its construction, we are looking at its environmental significance. Yet it is not difficult to see how the meanings associated with it at each of these levels—such as a drain on foreign reserves becoming a symbol of foreign oppression—are closely associated to one another. Reality, in other words, forms a 'seamless web' within which any fragmentation or categorisation is, it can be argued, a socially determined process.

Given the general acceptance, however, of a clear differentiation between each mode of significance, each mode is treated as being defined independently of any social determinants and subject merely to the

articulation of an internal logic (such as, for example, the so-called laws of classical economics or of aesthetics). Technology, as we have indicated, functions symbolically within each of these modes of significance. In other words, it provides a set of symbols by which the apparent significance or value of a social phenomenon can be articulated. As in the discussion of its material significance, however, technology tends to be treated as if it were a neutral element in this process, a symbolic tool by which significance is expressed rather than an essential element in the process of expression itself.

Take, for example, the relationship between technology and another conventionally accepted mode of significance, that of art. We have already referred to the ideological nature of the 'two cultures' debate. A particular aspect of this debate is that, rather than talking about technology and art lying within the same dimension of collective experience, technology is referred to as something isolated from—and even, to a certain extent, opposed to—man's 'artistic' activities. This is apparent in debates that take as their terms of reference the 'impact of technology on art' or the apparent confrontation between man the artist and man the technologist. We read discussion on technology and art, or technology in art, but seldom technology *as* art. It is considered legitimate for the artist to use technology in his work but only as a means of expressing his (artistic) ideas. Kinetic art and computer art provide two examples of this. In such instances the artistic and technical aspects of any work of art usually remain clearly defined. Not only does technology appear politically neutral, but it appears neutral within the artistic process as well.

A second, possibly more important, level of significance is that of political debate. We have already seen how the myth of the neutrality of technology obscures the political function of technology in society. A corollary of this is that attempts are made to neutralise any political opposition to the use of technology by translating it into the de-politicised language of the myth itself. This is the case with the so-called 'technological fix'. Here it is claimed that the social problems apparently created by technology can be solved by the application of yet more technology. We are informed, for example, that the use of sophisticated monitoring and control devices will itself be sufficient, given adequate government legislation to ensure their use, to combat the problems of environmental pollution, or that appropriately designed technologies will solve the social and economic problems of underdevelopment. My argument is not against the necessity or even the adequacy of the individual machines implied by such policies but against a scientific analysis that both ignores the political roots of the problems that it seeks to solve and subsequently pretends that solutions can be achieved merely through technological means.

The same is true, although in a slightly more subtle way, of the development of techniques of 'technology assessment'. The question here is not whether the impact of technology on society should be assessed but how

it should be assessed. As currently being developed within industrial capitalist societies, technology assessment seeks to evaluate this impact—and hence to recommend those measures by which the 'undesirable' aspects of technological innovation can be minimised—using the very same methodological categories (such as objectivity and political neutrality) as those by which the technology itself is claimed to have been developed. It obscures the fact that the nature of the categories selected as the basis for analysis already disguises the essential political elements of the problems under review.

There is also the converse ploy of neutralising criticism. Those who propose technological programmes, in the belief that such programmes are outside the realm of political debate, may be prepared to listen to criticism of a purely technical nature and frequently to modify plans in the light of such criticism . Nevertheless they are able to justify dismissing any *political* challenge as irrelevant to the issue under discussion. Thus an official inquiry into the siting of a motorway will concentrate on where such a motorway should be placed to create the 'least' environmental or social disturbance, not whether a motorway is really needed at all or whose interests the motorway will serve.

Related to the de-politicisation of technology, of course, is the interpretation of politics itself as a purely technical activity, and the evaluation of political activity in primarily instrumental terms. Political programmes tend to be assessed on what they have achieved in terms, for example, of economic performance. Political parties are compared on the effectiveness by which they have been able to handle a capitalist economy. The extent of aid to underdeveloped countries is evaluated in terms of its size as a percentage of gross national product. Thus we have the convergence theory of politics, the alleged death of ideology—since everything is now claimed to be done 'scientifically'—and the apparent development of a politically neutral technocratic society.

This process also takes place in education, where techniques such as the Keller-plan method of programmed learning, based heavily on Skinnerian behaviourist ideas, are again judged purely by their effectiveness at improving the speed and efficiency of what becomes known as the learning process. Frequently quoted by supporters of such techniques is the fact that students 'prefer' such methods of teaching since they are more effective in helping them to pass exams; this is itself an indictment of the nature of the educational system and the forms of assessment that it uses, rather than any indication of the intrinsic value of the techniques. It is these and similar facts that lead to the characterisation of contemporary ideological legitimations as 'mechanistic' and 'functionalist'.

To summarise, I have tried to indicate in this section how technology functions in society not merely in a material sense but also in a symbolic one. It does so within a number of what I have called modes of significance.

The conventional interpretation of this process, I maintain, is that the symbolic function of technology within each of these modes is a purely neutral one: the symbol is considered independent of what it is supposed to express. Furthermore, each mode of significance—whether economic, cultural, political or even, I would claim, subconscious—is held to be independent of the others and subject only to its own internally articulated logic. This whole interpretation of the social world in which we live, I suggest, disguises the implicit connections between the various modes of significance. It also hides the symbolic role that technology, in maintaining the myth of its neutrality, plays in reinforcing the apparent nature of each of these modes of significance and the extent to which they are politically determined. Technology functions symbolically in an ideological fashion to maintain and reinforce a particular form of social organisation and control, just as it functions materially towards the same ends.

Conclusion

In discussing the social function of technology, the purpose of this article has not been so much to provide an explanation for a particular state of affairs as to describe the legitimation that a dominant social class attempts—not always successfully, one should add—to place on that set of affairs. In no way therefore does it lead directly to a recipe for an adequate political practice that would alter such a situation. Yet it does provide some indications of various aspects required by such a practice. In particular, it points to the need to challenge technological innovation directly on political grounds, as part of an overall struggle for emancipation from the exploitative system which technology maintains in industrial capitalism. Furthermore it indicates that what is experienced as oppression *by* technology is in fact the articulation of political repression experienced *through* technology. The nature of technological innovation as a political process means that those who in our society control the process of innovation are able to use this control to strengthen their position—a situation in which scientists and technologists become direct accomplices. Once this has been realised, it will be seen that political struggle belongs as much to the laboratory bench as it does to the factory floor and indeed to all other areas of everyday activity.

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The 'Relevance' of Anthropology to Colonialism and Imperialism

JACK STAUDER

In the aftermath of a large student rebellion at Harvard in the spring of 1969, a graduate student in anthropology raised a criticism of our field which I have often heard:

> Social anthropology—traditionally a field concerned with explaining and understanding small-scale cultures and societies, especially in the non-Western world—is a field that could make relevant contributions to our understanding of major events and problems of the world: wars of liberation, the effects and causes of racism, economic exploitation, colonialism, imperialism... [However, Departments] ensure that anthropology will remain isolated from and irrelevant to social and political problems... (Anon., p.71).

Many students in anthropology want an anthropology that will provide them with an understanding of 'major events and problems of the world'. Therefore they demand 'relevance', meaning an anthropology relevant to their felt needs. I sympathise with this desire and share it. But the basic issues underlying the complaints in the quoted passage cannot be reached by posing the problem in terms of relevance. By and large, anthropology has always been 'relevant'. The question to ask is, relevant to whom and for what?

Early Development of British Anthropology

The institutional origins and early growth of British anthropology in the 19th century were closely linked to an interest in the possible practical value

of anthropology as an applied science. Members of the early anthropological societies in Britain were especially concerned with questions of race and slavery, great issues which agitated British society throughout the first two-thirds of the century. Anthropological journals of the time were filled with articles making recommendations on these questions. Although it was scarcely to be left to scholars to decide a question involving profits and power relations, many early anthropologists nevertheless hoped that their resources might be of some utility to the interests involved in the expansion of European power around the globe.

The slavery issue died down in the 1870s, as slaves in the Americas became poorly paid 'free' plantation labour, and as the slave trade was replaced by other, more 'legitimate' forms of exploiting Africa. But the last two decades of the 19th century witnessed the acceleration and culmination of British imperialist expansion, particularly in the 'scramble' for Africa in which Britain succeeded in asserting against other European powers her claims to rule or 'protect' diverse territories with large native populations. During the same period, anthropology was attaining academic respectability in Britain (in the form of university chairs), and some of its promoters hoped to ally the new science of man not with controversial popular causes, as had been the case in the pro- and anti-slavery debates earlier in the century, but with the science of good government, specifically the administration of colonial peoples.

A survey of the 27 Presidential Addresses to the Royal Anthropological Institute from 1893 to 1919 shows that in over half the President raised claims regarding the practical uses to which anthropology could be put in serving the Empire. Frequently the complaint was made that, despite anthropology's potential practical value, neither government nor business seemed interested, and little in the way of funds or official recognition seemed forthcoming. These complaints were largely unheeded in the following years: in 1910 British anthropologists were refused even a grant of £500 from the government to set up an 'Imperial Bureau of Anthropology' within the Royal Anthropological Institute (Ridgeway, p.10).

Thus, around the turn of the century in Britain, anthropology found itself in a peculiar position. The British Empire was at its zenith, rapidly extending its effective rule over millions of new subjects. The period was marked by Victorian confidence in the application of science to achieve progress and profits. British anthropologists were promoting their new science as a potentially valuable tool to be used in the Imperial mission, to aid government and commerce and the advance of 'civilisation'. Anthropologists were desperately courting government for private recognition and especially financial support for anthropology in its potential uses as they saw them. But government and wealthy benefactors were spurning these advances and seemed little interested.

This discrepancy between anthropologists' desire to serve British

imperialism and the lack of support for anthropology by the British ruling class is a phenomenon probably explained less by the short-sightedness of the ruling classes than by the shortcomings of anthropology at the time. For despite the assertions of anthropologists, the actual state of British anthropology at the turn of the century was such that in practice it was producing mostly work of little or no possible use to colonial administrators, missionaries and traders. In the years before World War I, anthropology in Britain was dominated by controversies between diffusionists and evolutionists,[1] who held in common, however, an historical and often speculative approach that was primarily concerned with reconstructing the past of mankind. Few professionally trained anthropologists had seen Africa or other colonised areas first-hand, and anthropologists at home were dependent for their data mainly on the reports of untrained observers engaged directly in the colonising process. It is little wonder then that the picture anthropology gave of Africa up to the First World War was very unreal.

It has been argued that it was the very distortions and lack of truth in 19th-century anthropology which made it useful during the early periods of imperial enterprise. But if 19th-century ethnology[2] was eminently suitable as an intellectual justification for colonial expansion, it was not well suited at all to the succeeding era of colonial consolidation and exploitation. Populations of Africans, once pacified, needed to be administered. Ideas about African culture and society which were grossly unreal or distorted were no help in this second stage of colonialism; in fact, lack of proper understanding of Africans might well stand in the way of achievement of British colonial goals: to rule with a minimum of trouble and cost and with a maximum of stability and profit.

A few astute colonial administrators around the turn of the century realised that they needed better and more systematic information about the people they were ruling. These administrators took the initiative in demanding that anthropology be taught to colonial officers and others working in parts of the Empire, and special anthropology courses were subsequently set up at Oxford and Cambridge for officers on leave and cadets training for the colonial service.

Sometimes administrators who showed an interest in anthropology were assigned to investigations involving the collection of ethnographic materials. Also in a very few cases colonial governments had obtained the services of professional anthropologists to provide the colonial government with knowledge of the social organisation and customs of particular peoples as a background for administration (Forde, pp.843-46). This handful of persons had begun to write a few relatively systematic and reliable accounts of African cultures by the 1920s.

On at least a descriptive level, some of these early ethnographies achieved their design of providing the kind of information about a subject people's culture and society that could be of some use to administration

and missionary work. But while often exhaustively descriptive, their books lacked explanatory power: when necessary they resorted to diffusionist or evolutionary hypotheses to explain particular customs; for although these ethnographers were primarily interested in the present life of the peoples they observed, they lacked a theoretical framework appropriate for their purpose.

By the 1910s and 1920s there was a growing felt need in British colonial Africa for a 'newer anthropology' that would be adaptable to the practical requirements of British imperialism. And, as I have indicated, British anthropologists had constantly aspired to serve the Empire and by so doing to serve what they saw as the interests of anthropologists by obtaining funds and recognition. I have suggested that the major obstacle to this mutually beneficial alliance between colonialism and anthropology lay in the current state of anthropological theory and practice, or lack of it. The 'older' anthropology, ethnology, diffusionism and evolutionism stood in the way of progress. The time was ripe for revolution.

The conscious element in the revolution has rightly been identified with the names of Malinowski and Radcliffe-Brown. But it was certainly Radcliffe-Brown who was the more conscious of what he was doing. In the same year (1922) that their pioneering functionalist[3] studies, *Argonauts of the Western Pacific* and *The Andaman Islanders*, were published, Radcliffe-Brown was teaching in South Africa in the new post of Professor of Social Anthropology at the University of Capetown. In vol. 1, no. 3 of the South African journal *Bantu Studies* of that year appeared an article by Radcliffe-Brown entitled 'Some Problems of Bantu Sociology'. In his opening paragraph the author indicated that to his mind the key problem for 'Bantu sociology' was in fact the problem of colonialism:

> In Africa, more perhaps than in any other part of the world, social anthropology is a subject not of merely scientific or academic interest, but of immense practical importance. The one great problem on which the future welfare of South Africa depends is that of finding some social and political system in which the natives and the whites may live together without conflict; and the successful solution of that problem would certainly seem to require thorough knowledge of the native civilisation between which and our own we need to establish some sort of harmonious relation.

Radcliffe-Brown adds that 'in the establishment of the department of social anthropology in the University of Capetown this practical importance of the subject has been kept constantly in view, and the teaching and research are being organised on this basis' (p.38).

In his article Radcliffe-Brown goes on to make several other points: the importance of the study of kinship for understanding African society; the importance of fieldwork; and the superiority of the functionalist approach over an historical or ethnological approach. He explains that

> In dealing with the facts of culture or civilisation amongst primitive peoples who have no historical records there are two methods of explanation that we may adopt. The first may be called the ethnological method; it attempts, by the co-ordinated study of physical characters, language and the various elements of culture, and with the help of such archaeological knowledge as is available, to reconstruct hypothetically the past history of a people in its main outlines. Such problems are very interesting ... But interesting as it is, and important as its results may sometimes be, this ethnological method does not often provide, and does not seem likely to provide, results that will be of any assistance to the administrator or the educator in the solution of the practical problems with which he is faced (p.39).

An alternative method of dealing with culture Radcliffe-Brown calls the 'sociological', i.e. the structural-functionalist:

> The aim of this method is not to reconstruct the history of a people but to interpret their institutions in the light of general laws of sociology and psychology. If, for example, we investigate by this method the custom of [lobola (bridewealth)], we seek to determine the function of that custom, what essential or important relations it has with other institutions, what part it really plays in the economic, moral and religious life of the tribe, and to what important needs of the social organism it is related. Such an investigation thoroughly carried out would enable the anthropologist to foretell with some degree of certainty what would be the general effects on the life of a tribe of an attempt to abolish the custom in question (pp.39-40).

In 'Some Problems of Bantu Sociology' Radcliffe-Brown came to the conclusion that 'the study of such problems, the sociological and psychological problems of native life, is certainly far more likely to lead to results of practical value to South Africa than the study of ethnological problems' (p.40). A year later, in 1923, Radcliffe-Brown returned to this argument in an address entitled 'The Methods of Ethnology and Social Anthropology', presented to the South African Association for the Advancement of Science (1923). In this address he opens a full-scale attack on all the different approaches to culture current at the time: the psychological approach, the evolutionist approach, the ethnological or diffusionist approach. To demolish the older kind of anthropology to make way for the newer, sociological and functionalist anthropology he proposes, he raises as a final argument the criterion of which kind of anthropology can best serve the interests of colonialism: 'Now while ethnology with its strictly historical method can only tell us that certain things have happened, or have probably or possibly happened, social anthropology with its inductive generalisations can tell us how and why things happen, i.e. according to what laws' (p.141). Radcliffe-Brown advances his faith that ultimately knowledge of such laws of social behaviour will give men control over social forces and 'enable us to attain to practical results of the very greatest importance' (p.30).

Stability and unity in society were of course the conditions which

corresponded to the fundamental theoretical concepts on which Radcliffe-Brownian sociology was to be based: namely the concepts of integration, equilibrium and solidarity. These were more than abstract concepts in the context of South African society, where a subject but increasingly rebellious African population outnumbered the white settlers trying to dominate and exploit them. Radcliffe-Brown continues in relation to the racial confrontation in South Africa:

> Now I think this is where social anthropology can be of immense and almost immediate service. The study of the beliefs and customs of the native peoples, with the aim not of merely reconstructing their history but of discovering their meaning, their function, that is, the place they occupy in the mental, moral and social life, can afford great help to the missionary or the public servant who is engaged in dealing with the practical problems of the adjustment of the native civilisation to the new conditions that have resulted from our occupation of the country... (pp.142-3).

In fact much if not most British social anthropology in Africa during the 1930s and 1940s was to be focussed on political and legal institutions. Such an emphasis must be seen in the light of the strategy adopted by British colonialism to implement and maintain social control over the millions of people under its government in Africa. This strategy, which came to be known as 'Indirect Rule', avoided where possible the use of direct administration and direct coercion—though, of course, military force was always held in reserve. Given a necessity to economise manpower and finances in Africa, British colonial governments preferred to retain and utilise traditional political institutions. These were integrated into the colonial administration, and traditional political authorities were maintained as paid agents of colonial rule. But if African political institutions were to be adapted and used by colonial government, it was imperative to understand what they were and how they worked; to this end, anthropology was often seen as critically necessary. As E.W. Smith noted, 'the extension of Indirect Rule has been preceded by, and based upon, anthropological research' (p.xxii).

In the context of Indirect Rule, functionalist social anthropology seemed obviously superior to the older, ethnological approach, for the 'newer' social anthropology not only concentrated on identifying and describing the key social and political institutions of a subject people, but it also attempted to analyse how these institutions worked. Those concerned with problems of administration and knowledgeable about anthropology came to accept the validity of Radcliffe-Brown's claims on behalf of the practical superiority of the functionalist method (Hailey, pp.42-45; Smith, pp.xvii-xxv).

Traditional African societies were usually seen by the functionalists, as well as by the colonial governments, as being 'healthy' societies, well-balanced, well-integrated and maintaining an enviable degree of social control over their members. The British strategy of Indirect Rule had aimed

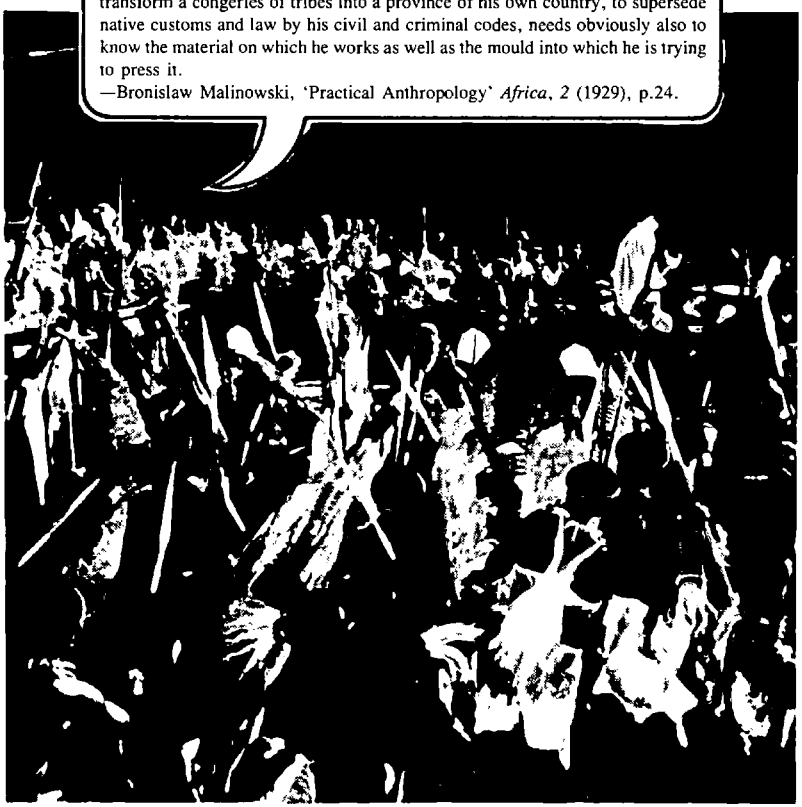
Anyone ever thought of trying indirect rule?
Our anthropological advisor says:

A scientific study of facts in this matter would reveal clearly that "direct rule" means in the last issue forced labour, ruthless taxation, a fixed routine in political matters, the application of a code of laws to an entirely incompatible background. And again as regards education, the formation of African baboos and in general the making of the African into a caricature of the European.

The political indirect rule which was the guiding principle of Lord Lugard's political and financial policy in Africa should be extended to all aspects of culture. Indirect cultural control is the only way of developing economic life, the administration of justice by Native to Natives, the raising of morals and education on indigenous lines, and the development of truly African art, culture, and religion.

But whether we adopt in our practical policy the principle of direct or indirect control, it is clear that a full knowledge of indigenous culture in the special subjects indicated is indispensable. Under indirect or dependent control the white man leaves most of the work to be done by the Natives themselves but still has to supervise it, and if he does not want to be a mere figurehead, or blunderingly to interfere in something which he does not understand, he must know the organization, the ideas and the customs of those under his control. The statesman, on the other hand, who believes in direct control and who wants rapidly to transform a congeries of tribes into a province of his own country, to supersede native customs and law by his civil and criminal codes, needs obviously also to know the material on which he works as well as the mould into which he is trying to press it.

—Bronislaw Malinowski, 'Practical Anthropology' *Africa*, 2 (1929), p.24.



to incorporate and preserve these integrated and stable units within an integrated and stable Empire. This intention, however, was progressively being undermined by two other contradictory features of British imperialism: by the racist assumption that European civilisation was culturally superior to African civilisation and destined to replace it; and, more importantly in the long run, by the European drive to exploit Africa economically and the radical socio-economic transformation this entailed in the lives of African people.

Astute colonialists, including foresighted anthropologists like Radcliffe-Brown, regarded the symptoms of social change with alarm. They saw the old tribal systems disintegrating and asked how they could arrest and control the forces which had been unleashed. Radcliffe-Brown definitely considered it the mission of anthropology to provide colonial agencies with the necessary knowledge about the processes of social integration and change. The same concern, both theoretical and practical, can be seen in the work of other British functionalist anthropologists in Africa in the 1920s through the 1950s. Such concern might reflect the hopes, on the part of some anthropologists, that by protecting the social fabric from disruption they could spare the peoples they studied from some of the worst consequences of colonialism. But such conservatism can also be seen as ultimately protective of colonial rule itself, for the social health of native society was linked in the colonial mind with the political health of the British Empire.

For example, Radcliffe-Brown offers a clue as to the ultimate source of anxiety underlying his recommendations: 'One can feel quite certain that more knowledge of the nature of Indian culture and proper grasp of the laws of social integration would have prevented our long experiment with India from reaching its present unsatisfactory position' ('Applied Anthropology', p.279).

In India at that time an organised movement for national independence had emerged as a definite threat to British rule. In Africa less organised rebellions against colonialism had also occurred and would soon develop into movements for national liberation. These anti-colonial struggles were an underlying source of worry to colonialists and anthropologists like Radcliffe-Brown who saw in them the final outcome of the tendencies they regarded as 'social disintegration'. The ultimate concern of these far-sighted

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Opposite:
Direct rule meant British administrators antagonizing the natives by clumsily suppressing their tribal customs and offending their traditional leaders. As a result of such arrogance, in 1879 a hundred-odd British troops found themselves defending a mission station against four thousand Zulu warriors. Until then it had been inconceivable that what was regarded as an uncivilized rabble of native savages could strike such a blow at Britain at the height of its military might.

men, it could be suggested, was to arrest the process of disintegration of traditional societies, not for the sake of the people living in them, but in order to arrest the incipient process of disintegration of the British Empire itself.

In retrospect we see of course that Radcliffe-Brown's ambitions for anthropology were unrealistic. Neither social science nor enlightened administration could contain the forces, set in motion by colonialism, which would eventually undermine and overthrow the British Empire. But these developments, and the rapidity with which they would take place, were not clearly foreseen in the 1920s and 1930s, and Radcliffe-Brown's optimism about the potential applicability of a revolutionised sociology and functionalist anthropology carried the opinions of those who counted. For many decades anthropologists had been claiming that they could be of practical service to government and business. But as I have pointed out, before the 1920s these claims were mainly ignored by the interests on whom they were pressed. During the late 1920s and into the 1930s, however, this situation changed fairly rapidly, and the alliance long anticipated by anthropologists was established. British anthropology was suddenly and increasingly successful in gaining recognition and substantial funding from private and governmental sources.

For 1925 the Council of the Royal Anthropological Institute reported a momentous breakthrough. For the first time in the history of the association it had received large donations from outside sources: £1,000 from the Carnegie Trust and $17,500 from the Rockefeller Trust. In 1931 the Rockefeller Foundation made a further grant of $10,500 to the Institute. These grants were small in terms of Rockefeller disbursements but they were unprecedentedly large sums in terms of the Institute's normal budget, which had always been less than $3,000 a year before 1925, the bulk of this income coming from memberships and subscriptions to the Journal (RAI, 1926, 1927, 1930).

But a more important development for the future of anthropology in Africa occurred in 1926 with the foundation in London of the International African Institute. The Institute's aims were to bring about 'a closer association of scientific knowledge and research with practical affairs' (Lugard, p.2). The Institute was established with contributions and support from all the colonial governments in Africa as well as from the British and French home governments, and from various missionary bodies (Hailey, pp.51-52). On the governing board of the Institute sat a mixture of former colonial administrators, heads of missionary associations, and eminent academics specialising in African studies. The first head of the Institute was Lord Lugard, famous not only as a soldier for his role in the 'pacification' of many parts of Africa but notable also as an administrator and as the architect of the theory of 'Indirect Rule' practised in British Africa.

The International African Institute was to play a leading role in social

science research in British Africa. From the 1930s to the present it has financed the publishing of many of the best ethnographies about African peoples, and its journal *Africa* has carried many of the most important anthropological articles about Africa. Many of these books and articles were closely related to colonial problems and 'practical affairs'.

That the Institute was able to succeed in playing such a vital role in stimulating and shaping anthropological research in Africa was due in no small part to the grant of $250,000 it received in 1931 from the Rockefeller Foundation. Thus the bulk of professional anthropological research in British Africa in the late 1920s and the 1930s was dependent on Rockefeller money in one way or another—money which of course partially derived from the profits of the growing Rockefeller financial and mineral holdings in Africa. Whether or not the trustees of the Rockefeller Foundation had ever heard of Radcliffe-Brown, the 'newer' anthropology he was championing, with its emphasis on scientific applicability, stood to thrive on the disbursement of Rockefeller grants.

'Indirect Rule' and the Functions of Functionalism

In any case, it was in the late 1920s and early 1930s that anthropology in the British Empire began receiving in substantial sums the money which allowed the profession to be greatly expanded and which allowed anthropologists to pursue foreign fieldwork as a matter of course. Apparently anthropology had finally convinced colonial governments and capitalist benefactors of its potential practical uses to them. The struggle for acceptance which British anthropology had waged for more than half a century was successful.

However, the kind of anthropology which had succeeded was functionalist social anthropology. Ethnology, the historical approach, inquiries into the origins of culture: this kind of anthropology did not succeed; in fact it hardly survived in Britain. Research and writing was increasingly, and finally overwhelmingly, social anthropological and functionalist in character. Other anthropological approaches were less and less funded, published or practised. A new orthodoxy had been established and was to dominate British anthropology until at least the end of the British Empire in the 1950s and 1960s.

Whatever the theoretical advantages of the new functionalist approach, and whatever the methodological abuses of the historical approaches at the time, one must still ask why functionalism triumphed so rapidly and completely in Britain and her colonies whereas it made slower and only partial inroads into the anthropological currents of other nations such as the United States, France, Belgium, the Netherlands, etc.; and no inroads into the anthropology of Germany and Eastern Europe. No doubt many factors can be adduced, but I would argue that this situation may be at least

partly explained in terms of the differing colonial commitments and colonial systems of the various powers—or the absence of colonies in the case of Germany (after World War I) and Eastern European countries. To verify this hypothesis would of course require an analysis of the anthropology in other countries, a task which cannot be attempted here. But a crude correlation apparently exists between the successful spread of functionalist social anthropology in several countries and the degree of colonial commitment of each of these countries. In this respect, the British Empire was exceptional. It was far larger than other colonial empires both in terms of the absolute population of non-whites being ruled as well as in terms of the ratio of non-white subjects to the population of the mother country.

In sub-Saharan Africa, British colonies included a larger population than the French, Belgian and Portuguese colonies combined. It was in these African colonies that manpower shortages and the costs involved in administration were especially serious. Lord Lugard and other British colonial officials of the time were explicit about this problem as well as about its importance in arguing for a system of 'Indirect Rule'. The British attempted a system of adapting and utilising indigenous institutions which made it especially important for administrators and others in the African colonies to understand native institutions—hence the supreme usefulness to them of a social science which attempted to describe and explain the systematic functioning of these institutions.

On the 'African Negro'

...a happy, thriftless, excitable person, lacking in self-control, discipline, and foresight, naturally courageous, and naturally courteous and polite, full of personal vanity, with little sense of veracity... His thoughts are concentrated on the events and feelings of the moment, and he suffers little from apprehension for the future, or grief for the past...

The African negro is not naturally cruel, though his own insensibility to pain, and his disregard for life—whether his own or another's—cause him to appear callous to suffering...

He lacks power of organisation, and is conspicuously deficient in the management and control alike of men or of business... He is very prone to imitate anything new in dress or custom...

In brief, the virtues and the defects of this race-type are those of attractive children...

It is extremely difficult... to find educated African youths who are by character and temperament suited to posts in which they may rise to positions of high administrative responsibility.

—Rt. Hon. Sir F.D. Lugard, *The Dual Mandate in British Tropical Africa*, William Blackwood & Sons, 1922.

What I am suggesting, tentatively, is that the critical factor in the British adoption of a thorough-going system of Indirect Rule in Africa is to be found in the magnitude of the British imperialist mission there and elsewhere. And I am also suggesting, again very tentatively, that the critical factor in the thorough adoption in Britain of the functionalist approach in anthropology was related to the same colonial situation: the need to preserve, understand and utilise native institutions in controlling subject populations in Africa. At the very least I think it is possible to demonstrate that the demands for a 'practical' anthropology—an anthropology useful to British imperialism—did play an important role in the determination of the theoretical direction British anthropology was to follow.

While recognising that more research is needed in these areas, I believe my interpretation is oriented in the right direction: towards an interpretation of the development of anthropology in terms of the material situation in which it was practised. Accounts by Evans-Pritchard and others of the development of anthropological thought in Britain have tended to be exclusively idealist in their approach: that is, they try to explain ideas in terms only of ideas. In these accounts the synchronic functionalist[4] approach triumphed simply because it was theoretically superior to other approaches. Such interpretations avoid the question: superior for what purpose? or, superior in what situation? In terms of the colonial situation an alternative explanation is possible to account for the success of the synchronic functionalist approach in the anthropology of the British Empire. In this explanation functionalist social anthropology can be seen as itself functionally adaptive to the requirements of British imperialism. Therefore this approach was recognised and supported by the dominant institutions participating in the imperialist enterprise and was thereby greatly aided in becoming the dominant theoretical ideology of British anthropology.

What I have also indicated is the necessity for reassessing the view of British functionalism current among many anthropologists and anthropology students. This view, simply put, seems to be that because a narrowly synchronic functionalist approach was inadequate to explain social change, therefore functionalism is and was irrelevant to social problems or problems of the 'real' world such as imperialism. In fact, I am suggesting, functionalism was very relevant to colonialism and the social problems of its day. Such relevance is seen in the attention almost all the noted British functionalists paid to questions of applied anthropology; almost all wrote more than one article on the subject (for references see Forde, pp.862-5; Mair). And of course the sources of support for British anthropologists, their choice of problems and their activities in the field and elsewhere in co-operation with colonial governments were all politically relevant at the time, whether or not the anthropologists wished to recognise this.

Let us take an example, the origins of a modern anthropological classic: E.E. Evans-Pritchard's research among the Nuer. This research, conducted

almost 40 years ago, resulted in *The Nuer*, a pioneering study employing structural-functional analysis and an equilibrium model to analyse the Nuer lineage system, their ecology and political behaviour. Students who read *The Nuer* usually find it an interesting or even a fascinating study but generally do not find that it contributes to their 'understanding of major events and problems of the world'. In this sense *The Nuer* appears to be irrelevant anthropology.

In fact, in its historical context, Evans-Pritchard's research did have a relevance and remains revealing today if we look at its genesis. In the first line of his preface to *The Nuer*, Evans-Pritchard states: 'My study of the Nuer was undertaken at the request of, and mainly financed by, the Government of the Anglo-Egyptian Sudan, which also contributed generously towards the publication of its result.' The Nuer were clearly not selected for study out of pure scientific curiosity. What was the urgency, the necessity, of a study of the Nuer?

Later in the book Evans-Pritchard supplies us with the information needed:

> The truculence and aloofness displayed by the Nuer is conformable to their culture, their social organisation and their character. The self-sufficiency and simplicity of their culture and the fixation of their interests on their herds explain why they neither wanted nor were willing to accept European innovations and why they rejected peace from which they had everything to lose ... Had more been known about them a different policy might have been instituted earlier and with less prejudice.
>
> In 1920 large-scale military operations, including bombing and machine-gunning of camps, were conducted against the Eastern Jikany and caused much loss of life and destruction of property. There were further patrols from time to time, but the Nuer remained unsubdued. In 1927 the Nuong tribe killed their District Commissioner while at the same time the Lou openly defied the Government and the Gaawar attacked Duk Faiyuil Police Post. From 1928 to 1930 prolonged operations were conducted against the whole of the disturbed area and marked the end of serious fighting between the Nuer and the Government (pp.134ff.).

Evans-Pritchard recounts these personal hardships in his introduction to apologise for his work being only a 'contribution to the ethnology of a particular area rather than a detailed sociological study' (p.15). Other than to explain his misfortune in his book, he is not interested in how the Nuer were reacting to colonial aggression, and to himself as a perceived agent of colonialism. And except for the incidental passage quoted above, he does not describe the British war against the Nuer, much less the nature of British colonial policy and administration in the Sudan. Studying the colonial government was not a government anthropologist's job.

The Nuer is therefore mostly irrelevant to our understanding of colonialism, or of how a people might resist it or how they fail and are conquered. But

was Evans-Pritchard's work irrelevant to colonialism? That the colonial government should support his work and direct his enquiries indicates that they thought it was not irrelevant for their purposes. Exactly how the intelligence Evans-Pritchard gathered was applied by administrators we do not know since this was not a fit subject for study; but the understanding which had been acquired of the Nuer political system, their values, their ecology, etc, would have a definite usefulness. Bombs, machine-guns and mercenaries were wasted trouble and expense if the Nuer could be dominated and administered 'peacefully' (with force in reserve). 'Peaceful' domination must rely on manipulation; and manipulation rests on knowledge. Evans-Pritchard's work was therefore *not* 'isolated from and irrelevant to social and political problems'—in fact it originated in the need of British imperialism for information to deal with its problems.

Of course most anthropologists in the colonial era were not directly employed by governments, nor did most arrive to do research in the immediate wake of war or rebellion. Nevertheless the position of most anthropologists in colonial territories was essentially the same. They were expected to gather information and provide insights which would aid colonial governments in administering colonial peoples; most ethnographers of the period prefaced their books with the wish that their work might prove helpful to the administrator.

The funding of anthropology in the 1920s and 1930s has already been described. After World War II funding was increasingly channelled through the various colonial governments and the various regional social science institutes in Africa. That Africa's first social science institute should be established in Northern Rhodesia, in the general vicinity of the largest Western investments in Africa (South Africa, the Rhodesias, Katanga), is probably not coincidental. A full account of the development of the Rhodes-Livingstone Institute, and of the extent to which the intentions of its founders and patrons influenced its research, is a study that remains to be done. But recently some of the most notable theoretical and practical work that has come out of the Institute has come under the sharp attack of an African anthropologist for its pro-colonialist, pro-employer bias (Magubane).

Many colonial anthropologists probably thought of themselves doing work that was primarily 'scholarly' rather than 'practical', but the distinction was not cut-and-dried in terms of 'relevance' to colonial administrators. As Lucy Mair says,

> We should not put an account of the social structure of an African people in a separate category as a "theoretical" study because it was not focussed on some problem of special concern to the government of the day, although of course we should agree that many of the generalisations which such accounts make possible are of more interest to the theorist of society than to the practitioner of government. We recognise, too, that some studies bear more closely on problems of policy than others, and priority lists were drawn up

> with these in mind. Studies concerned with the changes currently taking place in African society have a prominent place among these (p.98).

Anthropology in British colonial Africa—and, I would submit, in other areas under colonial domination as well—was basically very 'relevant' to the colonial governments and to the imperialist systems of which they were part. Anthropologists helped gather the intelligence about native people which allowed these people to be efficiently ruled and exploited in the colonial interest—that is, in the interests of the white settlers, the colonial



Professor Schapera talking to Kwena informants
at Lentsweletau village, Botswana, 1938

administrators, the government in the metropolitan country, and the Western capitalist enterprises in Africa. Most anthropologists accepted as given this imperialist framework and its concomitant myths. Then, as always, the reality of violent domination and exploitation was cloaked by rhetoric and myth assuming a harmony of interest between ruler and ruled, exploiter and exploited.

Naturally many anthropologists developed sympathies for the people they lived with and studied. Some of them, as Kathleen Gough Aberle has noted, took the role of 'white liberal reformers' and tried to intercede to protect the people they studied 'against the worst forms of the imperialist exploitation' (p.13). Such attempts, however, did not basically challenge the imperialist system. As this system was predicated on domination and exploitation, it was basically impossible for anthropology and anthropologists to serve the interests of the people they studied by suggesting reforms. What anthropologists may have come to think and say about colonial policies and practices was of little moment compared to the services they rendered colonial interests by providing them with intelligence relevant to these interests' continuing domination and exploitation of the peoples studied. In short, anthropologists were part of the colonial system; few if any of them really opposed this system with the perspective of overthrowing it. In fact, probably most anthropologists, consciously or unconsciously, accepted unquestioned their roles and concomitant privileges in the colonial system, and their work was generally confined and directed it (see Maquet; also Harris, pp.514-67).

The Relevance of Contemporary Anthropology: the American Experience

But what of anthropology today? What of its relevance to 'major events and problems of the world'? Direct colonial administration of most tropical countries has been abandoned after several wars and numerous rebellions and threats of mass unrest: the colonial powers were forced to accept a policy of strategic retreat. Did this mean the end of imperialism and of anthropologists' service to it? I would argue the contrary, that imperialism as a force in the world is stronger than ever, that neo-colonialism has replaced the older forms of colonialism, and that the 'relevance' of anthropology remains essentially the same as I have described it under the old colonialism. The main change has been that the United States has now become the leading imperialist power in the world.

What relevance does anthropology have to US imperialism—specifically to US government and business operations abroad and at home? It is no secret that private and government agencies employ, either regularly or on a contract basis, a not inconsiderable number of anthropologists to do work directly related to furthering the interests of US government and business.

The Agency for International Development (AID), the Central Intelligence Agency (CIA), the Defence Department's Advance Research Projects Agency (ARPA), the Centre for Research in Social Systems (CRESS)—formerly the Special Operations Research Organisation (SORO)—the Human Relations Research Office (HumRRO) and their various projects all maintain stables of social scientists including anthropologists. So do other government agencies and numerous private research corporations like the Atlantic Research Corporation, A.D. Little, Human Science Research, Simulmatics Inc., etc., which specialise in classified contract work for the government. Large industrial corporations also occasionally employ anthropologists.

What work do these anthropologists do? We may assume that they are employed to do work 'relevant' to the needs of their employers. Much of this work is kept classified: only portions which do not compromise the interests of the sponsor may be allowed to be published for scholarly audiences. What we learn about this work is usually accidental or indirect. For example, titles of classified projects sometimes appear in the *Congressional Record*, e.g. for ARPA; 'Rural Value Systems, Republic of Vietnam', by Human Sciences Research; 'Thai-Malay Village Study (Project Agile)', by M.L. Thomas, Northern Illinois University; etc. Or, more vaguely, we note a great increase of research interest in poverty-stricken and minority group areas of Thailand since the beginning of guerrilla activity there.

However, here I am not interested in the issue of classified research *per se*. Rather, I am trying to make the point that anthropology clearly does have a relevance to 'major events and problems of the world' we live in. For I assume that the US government and US corporations are paying for, and sequestering, knowledge that is relevant to their interests.

This situation was probably brought home for the first time to most anthropologists by the revelations concerning Project Camelot (see I.L. Horowitz). For this project many social scientists including anthropologists were approached to undertake studies to make

> it possible to predict and influence politically significant aspects of social change in the developing nations of the world ... The US Army has an important mission in the positive and constructive aspects of nation-building in less developed countries as well as a responsibility to assist friendly governments dealing with active insurgency problems (from the recruiting letter for Project Camelot, in I.L. Horowitz, pp.47-9).

In the words of two commentators:

> The Camelot project is but one of the more blatant large scale counterinsurgency research efforts that the behavioural sciences have shown little hesitancy to undertake, especially if the money is plentiful and if 'basic research' can somehow be integrated into the design.
> Camelot was still-born, but counterinsurgency is alive and healthy in the social sciences. (Kaufman and Park, pp.12-13).

*Select Behavioral Science Contracts Related to Foreign Areas and Foreign Populations**

Title	*Location*	*Description*
1963 American Mount Everest Expedition.	Berkeley Institute of Psychological Research	Psychological aspects of stress behavior.
Changing values in Japanese, Americans, and Japanese-Americans	Institute of Advanced Projects, University of Hawaii	Analysis of how Japanese change their values as they come in contact with American culture.
International conflict (Israel and Egypt)	Stanford University	Analysis of relationship of opinions and writings of decision makers and the actual decisions rendered.
Foreign research symposia	Social Sciences Research Council	Meetings of American and foreign scholars in Europe on social psychology.
Persuasive communications in the international field	University of Wisconsin	How foreign nationalities react to various kinds of American communications.
Sociopolitical precursors to insurgency	Pennsylvania State College	Study of insurgency and causes related to it to determine role Navy plays.
Nationalism and the perception of international crises	University of Texas	Perceptions that people have of international crises and relating this to the psychology of the persons involved.
Group factors influencing creativity	University of Illinois	Discovering how a heterogenous group can establish a common communication system in order to be effective; some of these groups composed of individuals of different languages and cultural backgrounds.
Group equilibrium	Rutgers University	Studies made in U.S.A. on small group effectiveness have been replicated in Japan.
Role theory	University of Missouri	Theory of role structure. Work being done with collaborators in Australia and England.
Cross-cultural investigation of some factors in persuasion and attitude change	University of Maryland	Structure and mechanics of attitude change methods; research replicated with Japanese subjects to determine generality of findings.

* Source of data: *Behavioral Sciences and the National Security*, Report No. 4 (Washington, D. C.: U.S. Government Printing Office, 1965).

The American Anthropological Association reported to its members that 'The Fellows should recognise that although Camelot is dead under that name, in a sense it has only gone underground. Similar types of projects have been conducted and are being planned under different names and through other kinds of agencies' (AAA, p.9).

Most anthropologists, however, have no direct connections with the Department of Defence or the other agencies I have mentioned. Most anthropologists receive their funds for research from non-Defence agencies like the National Institute for Mental Health (NIMH), the National Science Foundation (NSF), the Smithsonian Institute and the Ford Foundation. They apparently choose their own subjects of research, and their research is not classified. What is the relevance of this kind of research to imperialist interests?

Most anthropologists have regarded the NIMH and the NSF as operating in an autonomous manner, as a sort of left hand of the government, oblivious to the requirements of the right hand, the Departments of Defence and State and the CIA. Credence is given this appearance by the fact that anthropologists rather than government officials have been sitting on the panels deciding grants and fellowships. The government, particularly under the Kennedy administration, has not failed to recognise that 'basic research' in the social sciences is not at all irrelevant to government requirements, especially in the long run. Thus work not immediately relevant to 'Defence' purposes has been funded, and anthropologists have been allowed to decide who among them and their students should receive support. Of course such independence is only allowed by a government confident that the profession will police itself: that it will not approve projects likely to conflict with the 'national' interests and that it will approve projects that on the whole are supportive of the government's long-range requirements. In general this means useful knowledge about the peoples and problems the US government and US corporations may have to deal with.

The lead in shaping social science research to the needs of the US government and large corporations has been taken less by government agencies than by the major private foundations: Ford, Carnegie and Rockefeller. The top officers and trustees of these foundations come of course from the ruling circles of business and government. And the wealth of these foundations is of course invested in capitalist enterprises, many of them with foreign holdings of the sort that US foreign policy is designed above all to protect and advance. Since their inception, these private foundations have been instrumental in serving the interests they represent by shaping US education in general, and the social sciences in particular. (For an extensive discussion of these activities, see David Horowitz in *Ramparts*.)

An example of the foundations' influence on anthropology is the leading role played by the Ford Foundation in establishing and shaping 'area studies' programmes. Ford has sponsored massive research programmes in every

strategically important area of the world, and Ford has funded most US centres of foreign affairs research. In African studies alone, Ford has exercised a dominant influence. It has provided hundreds of fellowships for research in Africa and it has helped set up and fund most of the major African studies institutes. What has been Ford's purpose? In 1958 Ford set up a special study committee to report on the needs which African studies had to meet. 'Without any reluctance', they wrote,

> the Committee has been able to agree on the unwisdom of academic aloofness from the needs of national policy, or of Africa itself. It seems to us most appropriate and desirable to encourage Africanists individually and in their associations, to provide assistance in professional consultation, in training for specific needs, in designing research so as to be useful to government and business (Cowan *et al.*, p.12).

Anthropology and US Imperialism

It is my contention, then, that the two major sources of funds for American anthropological research—the US government and the large foundations—use their money and influence in ways both direct and indirect to ensure that research is done 'relevant' to (i.e. serving) their interests in managing 'major events and problems of the world' today. Of course some research is more immediately relevant than other research: the more relevant research is a more likely candidate for support, but 'basic' research has not been ignored. The most prosaic study of the social structure or religion or psychology of an obscure ethnic group may prove useful for controlling this group. If not critical information at the moment, it may be tomorrow in an unstable world.

What I have been arguing is simply that anthropology, as an institutional activity, is not autonomous and not unrelated to other institutions of our social system. As an activity dependent for its support on the dominant institutions of our society—the federal government, the large corporations and the large private foundations—anthropology is expected to serve the interests that control these institutions and the universities. As these interests extend to foreign countries, anthropological research on foreign peoples can provide 'relevant' knowledge to aid the US government and US corporations in achieving their goals. Urban and rural anthropological research within America can aid the same interests to achieve similar goals.

The system of political and economic domination of hundreds of millions of people—US imperialism—is a system which, far from offering hope to the world's people, blocks their aspirations and oppresses them. Neither does US imperialism benefit most people in the United States: in fact it helps the same small class of corporate capitalists and their politicians maintain a system of domination and exploitation at home. (Kaufman and Park, pp.9-11, 19-23, summarise this argument.) Yet the people anthropologists

study are not members of this ruling class or allied elites abroad. Anthropologists study the people who are oppressed by imperialism. We live among them, make friends with them, and most of us come to sympathise with their aspirations. We would like to think that we are responsible to them, that we would serve their interests if we could, that we would side with them against oppression. Do we?

This is the real contradiction in anthropology today—not the question of 'relevance'. For as I have tried to show, anthropology is in fact relevant, but its relevance is one which presently and potentially serves interests which are contrary to the interests of the people we study. Our science *is* useable, and is often used in the service of imperialism, in the interest of domination and exploitation.

The problem I have posed is not primarily one of individual morality, of doing 'dirty' work or 'clean' work. Rather, the problem posed is an institutional one: it is that, together with other social scientific research, anthropological research *in general* is systematically shaped and utilised by the dominant interests in our society for ends which many, hopefully most, anthropologists would oppose. In a manner not unprecedented in human society, we as individuals are usually not fully aware of all of the institutional pressures that constrain and shape our work, or of the potential uses that our work may have in the hands of others. We do not see selective funding in operation, nor do we usually investigate why particular programmes are set up rather than others, why particular individuals have access to, or power over, funds which others do not. We accept jobs and grants as they are available, angling for them in ways we think might be successful. We are genuinely interested in phenomena such as social change, social structure, values and motivations, etc., which are also phenomena in which others elsewhere are interested—including agencies that have the power to use such information for purposes we would oppose. Working in a narrow academic context, we tend to see our work in terms of particular theoretical problems and of academic advancement. We are often ignorant of the possible 'relevance' our work may have in others' hands—and in any case, after we publish our findings we have no control over their use.

But we delude ourselves if we imagine that most anthropological research has no use and will never be used. Basic run-of-the-mill anthropological research is regarded as 'relevant' by those who have the desire and resources and need to render it so. We can only hope that our particular work might not be relevant, or comfort ourselves with notions about the unreliability and uselessness of social science in its present state. Meanwhile millions of dollars in computer technology and in hired minds will be working continuously to make our data relevant, reliable and useful to imperialism.

But how could our data *not* be relevant to imperialism? Anthropological

research is research on the people of the world subject to imperialism, but these are people who have always fought back in their ways, off and on, now passively and now fiercely, in one area at one time, in another area at another time, rebelling against forces which oppress them. Such rebellions, mostly futile in the 19th century, are becoming more and more successful in this century as people learn how to fight their oppressors. Imperialism, to survive, must counterattack with ever more sophisticated weapons, hard and 'soft'. It must understand the people it dominates, so as to understand how to prevent them from overthrowing that domination. Social science is necessary to that end; and anthropologists, as social scientists who work most directly in contact with the people oppressed by imperialism, are meant to be the scouts of the imperialist forces in what will be a bitter and protracted war during this century against the forces of resistance, liberation and socialist revolution rising among the world's peoples.

But many anthropologists and anthropology students would not choose this role of serving imperialism. They would want to serve the people they study. To the degree that they come to see this contradiction, the question arises of how they can best resolve it.

It is my opinion that, in the absence of revolutionary changes in the wider society, anthropology as a whole and as an institutional activity cannot be radically changed or reformed so as not to serve imperialism. As long as we live within an imperialist system the same forces which now shape and utilise anthropology will continue to operate and will continue their institutional domination over the practice of anthropology. Are we to try to serve ourselves by serving imperialism? Like Evans-Pritchard, we can go as an agent of the enemy into the camp of the people we study. But we can also choose another road, which I believe holds the future: we can ally with the people abroad and at home—in their struggles to create a new world where science can truly serve the people and not be a tool for their oppression.

Notes

This is a shortened version, by Kath Levine, of two articles by Jack Stauder. 'The Relevance of Anthropology under Imperialism' was written for the Wenner-Gren Symposium on 'Relevance' in Anthropology, held in March 1970. 'The Functions of Functionalism' was presented at a meeting of the American Anthropological Association in New York, November 1971.

1. *Evolutionism and Diffusionism*: Rival theories, developed in the late 19th century, to describe and explain the development of cultures. Influenced by evolutionist theories of nature, evolutionist anthropologists, led by the 'father of anthropology' E.B. Tylor, claimed a linear and separate development of societies from 'savagery' through 'barbarism' to 'civilisation'. Evolutionism implies that Western societies are much further 'advanced' than 'primitive' cultures and hence may have a 'civilising' duty to perform. In the early 1920s

the evolutionists were briefly contested by the diffusionists who attempted to trace widely dissimilar societies back to shared cultural origins and connections.

2. *Ethnology*: The 'natural' history of the cultural development of non-literate societies, largely from hypothetical reconstruction. For an account of the difference between ethnology and functionalism, see Radcliffe-Brown's discussion, quoted below.
3. *Functionalism*: In this sense the method of analysing a culture or society as an organisation of means designed to achieve certain ends. Closely related to *structural-functionalism*, an approach which treats a society as an isolable structure or organism composed of elements, none of which can change without effecting change in the other elements. These approaches are characteristically *synchronic*, i.e. ahistorical.
4. See note 3.

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Management Science and 'The Second Industrial Revolution'

MIKE HALES

Management science is an unknown territory for many people, scientists or not. This state of affairs arises partly from the screen of expertise which management science is able to draw around itself, and partly from the mystique of Management which operates generally in society. Before I embark on the examination of management science, therefore, I should explain why the exercise is worth conducting. Our increasingly managed social and economic world generates a lot of more or less fundamental criticism. I want to demonstrate that it is worth directing some fundamental criticism at the activities which put themselves forward hopefully as the science of managerial practice in such a world.

On one hand we would expect that, to the degree that these activities are scientific, they would generate techniques and theoretical insights which might be useful and interesting outside the specific social context in which they are created. In addition, if management science is more than a mere uncritical projection of the ideas and practice of managerial elites, then we should be able to see in it and through it some of the tendencies of social and economic development that, as practical critics of society, we need to understand and use.

On the other hand we would expect that a science of management in the context of advanced, 'organised' capitalism would have a profoundly ideological cast. The existence of revolutionary, socialist and quasi-socialist movements in the world at large, and movements for workers' control and self-management within the capitalist world, constitute a threat to the continued existence of capitalism—in its present form, at least. As a result there is a continuing need for myths and images which will serve to legitimate the social order of advanced capitalist society. The myth of progressiveness and neutrality that attaches to 'science' has proved very useful in this respect, as has the notion that some areas of social activity of great importance are legitimately reserved for 'experts' (and their employers) only.

Critique of management science as ideological practice has a special relevance for radical scientists. Without being grossly unfair, it seems that the ideas and activities of last generation's radical scientists are relevant to radicals today mainly as a source of lessons in how *not* to think and act. Seeing science as somehow both 'neutral' and 'progressive', and eager to see science applied towards rationalising social and economic practice, many pre-war British scientific radicals supported or became involved in wartime work which gave rise to what is now known as Operational Research. Management science, as the corpus of theory and practice of which operational research now forms a central part, is heir to the rationalistic, progressivist legacy of the scientific radical tradition.[1] One of the forms in which this tradition is currently articulated is the set of ideas and practices associated with the 'second industrial revolution' outlook in management science. This outlook sees contemporary social and economic development in terms of *technological* development and comes close in many ways to the 'vulgar marxism' of the pre-war radicals. As Trent Schroyer remarks, the societal system is seen as 'determined by the immanent logic of scientific and technological progress'. And he adds: 'In a way, all capitalists have now become vulgar Marxists!' (Schroyer, p.319). In the course of my argument I shall show briefly some of the ways in which this kind of crude materialistic theory of historical development serves ideological functions in the world of advanced capitalism.

A great deal of management science, however, carries with it no *explicit* view of society and its historical development. Operational research, for example, is commonly regarded as a practice consisting merely of the application of more or less sophisticated mathematical and statistical techniques. Although such an image captures less than the full truth, much of the operational research work carried out in industry, commerce, government, and 'defence' fits it closely enough. Yet operational research defines its general project in a much more ambitious way: it professes to be 'the attack of modern science on complex problems arising in the direction and management of large systems of men, machines, materials and money in industry, business, government and defence' (Beer, p.92). In the course of its development, operational research has constituted itself as an extension of the approach of the natural sciences to problems of management; thus the claim to comprehend 'systems of machines and materials' might be taken, on the face of it, to be well-founded. But as far as 'systems of men and money' are concerned (that is, *social* systems, albeit of a specific kind), the claim to scientific adequacy clearly requires some investigation.

The natural-scientific approach of operational research is the subject of discussion in the first part of this article. I shall look at an example of the kind of operational research which relies heavily on quantitative techniques—'technical OR', as I shall call it. I shall try to show the way in which a science of management modelled on the natural sciences reflects

a particular understanding of the nature of social relations in capitalist society. Then I shall look at an approach in management science which has strong methodological links with operational research but which draws upon a more sophisticated and explicit understanding of social relations than is implicit in most technical OR work. In this case (which is dealt with in the second part of the article) I want to show that this relatively sophisticated 'social-scientific' approach has no better grasp of some basic aspects of society than technical OR has, and this for the same basic reasons. During the second part of my discussion I shall also outline the 'second industrial revolution' view. Then, in the final part, I shall outline an alternative way of viewing social and economic development as it is reflected and mediated in the structure of scientific activity, and in management science in particular.

Technical OR and People

Uncertainty concerning the ability of operational research to cope with problems involving people is found not only outside the profession but within it too. Aware of the imperfect nature of their new science, OR men have made what they like to call 'the human factor' and its scientific treatment a recurrent topic of discussion over the years. Nevertheless, despite attempts to build bridges between operational research and the so-called 'behavioural sciences', the majority of operational research work remains amateurish and unsystematic in its efforts to grasp the specifically human (cultural and social) dimensions of its objects of research.

In some cases these dimensions are simply ignored. For example, during the 1960s a major Anglo-European OR consultancy was engaged to work on a feasibility study for a projected oil-refinery complex to be sited on Teesside. Working in collaboration with a prominent planning expert, they were to produce a single report on economic and environmental aspects of alternative coastal and inland sites. In fact, a strict division of labour was achieved: the operational research report dealt only with narrowly financial cost considerations, avoiding all the problems of conceptualising and evaluating social costs, which were left to the other 'expert' party. In cases where a study employing operational research techniques has at least tried to model human and social aspects of the problem, what typically happens is that an attempt is made to create a uniform system of measurement. The metrical system which offers itself is, of course, a monetary one. The notorious Roskill London Third Airport study springs to mind as a case in point. In this study, people were evaluated as inputs to the money economy: a man is not a man but a discounted cash flow. The manifest absurdity of placing a negative value on certain people's lives (old-age pensions, for example) was avoided only by algebraic sleight-of-hand. The same logic of the market results in historic buildings being worth only what they are insured for.

This fetishism of measurement in cash terms could be illustrated by many examples, but only one further example of this general tendency of reduction need be given here. At an international conference in 1960, Sir Charles Goodeve, F.R.S., the father-figure of British operational research and first Silver Medallist of the Operational Research Society, suggested a way of measuring satisfaction of social needs. He noted that certain social groups attract people to them and, in the classic operational research style, he took a model from the natural sciences (from thermodynamics in this case) and suggested that it could be used by analogy to conceptualise and quantify the social system. He supposed that the strength of attraction reflected the degree of satisfaction of needs within the group; this attractive force was to be measured—by finding out how much money people would have to be paid to stay out of the attractive group! This approach is rather more imaginative than that of many actual operational research studies, but in its logic—the reduction of society to a matter of money and markets—it is typical.

Of course, things which are intangible or which differ qualitatively *are* compared in the course of everyday social activity. But the social processes by which things come to be valued relative to one another differ radically from the formal attempt of operational research to commensurate *quantitatively* everything from chalk to cheerfulness. To decide that this man's time is worth more than that man's comfort, for example, demands that the decision be made in the face of the full social complexity, through a consensual process of evaluation and re-evaluation in a community of which both men are equally part; this condition is *a priori* unsatisfied in a class-divided society. Indeed technical OR's way of creating a basis for decision is precisely the opposite: it abstracts from the manifold dimensions of the social existence of its objects, reducing them until they fit within the narrow compass of its formal calculus. This reductionist tendency is in part *technically* determined, because of the formal limitations of the techniques concerned. But equally, the reduction is enforced and endorsed by the socially sanctioned reduction of value to that most abstracted of categories, money. Thus technical and social conditions conspire to make a 'scientific' result ideological: 'ideological' because the saying 'money and markets are what make society cohere' is true not universally but only in the ideal free market of classical capitalism. I am not arguing against any and all uses of quantitative techniques in decision-making, but the nature of the consensus on which an 'objective' result is founded must itself be subjected to close analysis and criticism.

Thus technical OR's approach to 'systems of men and money' can be seen to be problematic. This is true also of its approach to 'systems of machines and materials', despite its natural-science pedigree. For 'machines' and 'materials', as operational research approaches them, are not *natural* categories but social, specifically *economic*, categories. Operational research

assumes that men are the same as machines, materials, and money in this crucial respect: all of them are kinds of things, so-called 'resources', at the disposal of managers of organisations. Men, when they are things of this kind, are called 'personnel' or 'manpower'. Living labour, however, has certain properties that make it more difficult to manage than machines, raw materials and financial capital, which are dead forms of past labour. Industrial social science has developed precisely as a response to the recognition of this fact. The 'scientific management' of Frederick Taylor, an early precursor of management science, treated men as things. The modern industrial social sciences, springing up in the wake of scientific management, attempt to palliate the grossly mechanistic—and greatly resented—practice of Tayloristic science. Later in this article I shall look at the way in which scientific management and management science are related. Before doing this, I want to discuss the way in which technical OR's treatment of people compares with that of the social or 'behavioural' sciences.

After the Second World War, when operational research started to develop in Great Britain outside the military sphere, one of the first places in which it took root was the newly nationalised coal industry. The first jobs carried out by the Coal Board's operational research group were closely associated with technological aspects of the production process. As the group gained experience and began to win managers' confidence, other kinds of problems began to come its way. The Coal Board was faced with the task of rationalising the system of coal production taken over from private enterprise. 'Rationalisation' is difficult to define in any circumstances. The meaning of the term becomes especially problematic when the industry concerned serves others that are not socially owned and produces products for a competitive market in the absence of anything that could be called a rational national fuel policy. The further complication of a geographically scattered labour force with a strong tradition of militant action in defence of its interests made the problem of closing and expanding pits, and running-down, transferring and recruiting labour forces, very complex. It was in this context that the operational research group was asked, in the late 1950s, to study the factors affecting labour turnover in the pits.

Statistical analysis showed that the wastage rate for recently recruited workers was significantly different from that of men employed for a longer time, that this was true for different kinds of work and that the statistical relationship between wastage and length of employment had a standard form. Taking the statistical analysis further, it was shown that wastage also depended on the age of employees, the availability of other jobs and the distance from home to work, among other things. On the basis of such findings it became possible to put into policy form decisions on whether it was in the Coal Board's interest to build houses for transferred employees in order to stop them leaving the industry, whether it was cheaper to transfer or recruit labour for an expanding pit, and so on. Using the same techniques,

the cost of providing a bus service for employees was balanced against the savings to be expected in recruitment costs. The general mentality of this approach should be fairly clear. It was recognised that (to quote the official history of operational research in the Coal Board) 'for reasons of efficiency and humanity it seemed best to transfer the experienced miners from the closing pits'. Yet what in fact happened was that 'the study was treated as a cost-reduction exercise, i.e. how to obtain the necessary labour at a minimum cost. The investigating team ... determined the wastage rates for the different classes of labour, converted these into an expected length of stay at the new pit, and related this to the costs of training and transport' (Tomlinson, pp.181-93).

I want to make two points about this. The first concerns the way in which this approach differs from the one that sociologists at the time would probably have adopted, the 'Human Relations' approach. The central concept of Human Relations sociology is 'morale'; concepts such as 'labour wastage' are secondary, being derived from variations in morale. The operational research approach not only differs from this approach but tends to come into conflict with it. Once operational research work had produced a correlation of wastage with length of employment, there was then available to management a measure of 'normal' wastage in various conditions. Thus in many cases managers could afford to pay *less* attention to the question of morale as such. The official account describes this kind of approach, quite correctly, as 'mechanistic' and different from studies 'concerned with people'. The reason why people will or will not move a hundred or two hundred miles to work are not even intelligible questions in the terms of such a line of research—likewise the question of whether people are satisfied with their work and, if not, then how they might be. These questions are at least kept open by a 'morale' approach, even if no adequate answers are forthcoming, while they are circumvented and made irrelevant by an approach which sees people as things whose behaviour can be controlled within limits by controlling their physical environment, for example by providing houses or bus services. In this respect the physicalistic approach of technical OR *is* similar to the dominant approach in one of the other behavioural sciences, namely to behaviourism in psychology, in which the 'internal states' of an organism are deemed irrelevant to the scientific understanding and technical control of behaviour.

This takes me to my second point. A science studying people that never sets out to comprehend them in their full dimensions as people is bad science; it is not adequate to its object. To the degree that it is used as a basis for the treatment of these people in practice, it is also morally bad. When we look at the Coal Board study in these terms, however, an interesting distinction appears. While being, on this latter criterion, morally bad, the study was not necessarily bad *science*. If it seems that I am contradicting myself and rejecting my criterion of good science, let me explain. The people who

were the object of the Coal Board study were studied not *as people* but as something else: they were studied *as objects* with certain kinds of properties, required for certain purposes by the Coal Board, and responding to the manipulation of their environment in certain ways. Their opinions and needs were not the objects of research for this study; the essential social quality—the ability of people to act with others in generating, sustaining and reconstructing their own social practice—was left out of account.

That quality was not simply left out, in fact, but denied. For one of the ends implicit in this research was the satisfaction of the Coal Board's economic objectives while *reducing* the possibility of the concerted social action of the objects of study—in the form of strikes for example. To find a reason why these people were studied as objects, and treated as such in the decision-making process, we must go further back than the study itself, to the interest of the Coal Board in the matter. The coal industry produces a commodity, coal, employing wage-earners to do this. For this purpose the people who are employed are needed only as 'hands': 'manpower', as they are called these days. The purpose of manpower planning is simply to control manpower supply. This was the objective of the Coal Board study, subject to certain financial constraints. As far as the researchers were concerned, the objects of their research existed not as *people* but as objects in this class of 'manpower'. In adopting this epistemological stance, the researchers were merely acknowledging the common understanding of the community of which they were part—a necessary requirement if their results were to be valid in that discipline.

As for the quality of their science, 'science' means many things. One of the things it means is 'the discovery of laws from which techniques may be developed for controlling things'. In terms of this conception of the logic of science, which I shall refer to as the *technical* or *instrumental* logic, there is no reason to say that the Coal Board operational research work is bad science. In another sense, though, it *was* bad science; it was not true to the nature of the subject matter that it studied. There are kinds of science, and this work may have been good of its kind. But it denied the social being of the mass of people studied by turning them from social *subjects* into economic *objects* both in its theory and in the associated managerial practice. It denied the *practical* logic of social reality, the ability of people to see and develop themselves through their shared practice.[2] This transformation from the practical to the instrumental domain is not simply to be laid at the door of the manager of the research team for failing to include any social scientists in the team. As I shall argue below, social-scientific sophistication by no means guarantees the faithful reflection of social practice in a science of management. The logical end of instrumental control over economic objects was already presupposed before the study was even commissioned, in the nature of the Coal Board as an organisation within a competitive, commodity economy, in the wage-bargain in which both

miners and managers are bound, and in the professional scientists' ethic of 'service' to management and the firm.

To the degree that they adopt an instrumental approach to 'systems of men, machines, materials and money', all operational research studies can be subjected to the kind of critique that I have described against the Coal Board work. It is still very common for operational research teams to have no social scientists in them, as was the case with the Coal Board group. The kind of statistical-mathematical-quantitative approach used in the Coal Board study is dominant in technical OR, though the forms it assumes can be very much more complex. Nevertheless, there is more to operational research than its techniques. In particular, some operational research work employs a fairly sophisticated practical understanding, embodied in the methodology of 'the systems approach'. This approach is brought to one of its highest degrees of contemporary articulation in the theory and practice of the 'socio-technical system theory' school in management science. Thus I shall go on in the next section to discuss how people fare under the regime of management as seen by socio-technical system theory.

Systems, Shared Values and 'The Second Industrial Revolution'

Many industrial and administrative systems are organised on a model similar to that put forward by F.W. Taylor, the 'father of scientific management'. The essence of Taylor's job-design philosophy is that the whole work process should be constructed in 'the one best way' by building it up from elements, each of which has been scientifically studied and reconstructed. As a matter of simple efficiency, a man performing a given task under the Taylor regime will be performing at the limit of his capacity. Thus the more difficult task of scientifically studying his work may not be performed by him and has to be allocated to another man of superior skill and intelligence. Ideally, a rigid division of mental and manual labour is thus achieved; this, Taylor hoped, would bring an end to shop-floor militancy or 'systematic soldiering' as he called it. For if 'the one best way' has been scientifically discovered, there can be no room for argument on the workers' part; provided that they are paid higher rates to submit to this regime of strict discipline, they can have nothing to grumble about. So went Taylor's argument.

Taylor's mechanistic logic, realised *par excellence* in Henry Ford's production line, was put into practical form in the early decades of this century. There arose in its wake waves of vigorous and violent reaction as men experienced the deprivation of freedom and blows to their pride that resulted when management tried to treat them like machines or idiots, soulless income-maximisers. Not that such treatment was new, but Taylor attempted to bring exploitation to the state of a systematic science at a time when fierce competition made 'rationalisation' imperative for the survival



Time-and-motion study with Gilbreth clock and cine camera

of firms. There arose another school of thought concerning the way men should be treated in industrial organisation, again based on a claim to scientific veracity—the Human Relations movement. This school rediscovered a truth denied by scientific management, namely that people like to talk and be talked to, to consult and be consulted, to live and act as members of a social group. In short, they like to be treated as people and to recognise others in the same way. And so, although its inception may have been coincidental, it was no accident that the Human Relations movement followed in the wake of scientific management, mopping up the human residue left behind by the mechanistic organisation of the work process.

These two approaches are complementary. The scientific management approach, as embodied in industrial engineering ('production engineering' in Great Britain), aimed at the most technically efficient organisation of the material process of production itself. The Human Relations movement aimed to produce the most satisfactory social organisation surrounding the work process. Now, to someone versed in systems thinking, this duality of scientific practice might seem to be a blatant case of 'sub-optimisation'. And in fact this is one of the criticisms that underlie the claims to fame

of the protagonists of socio-technical system theory. Socio-technical system theory has been developed since the Second World War—notably by F.E. Emery and E.L. Trist at the Tavistock Institute of Human Relations, London—as a method for conceptualising, investigating and changing social and technical systems *as a whole*, thus overcoming (so it is claimed) the shortcomings of earlier approaches. In its emphasis on the 'system' concept and on social and technological change, socio-technical system theory is typical of an important tendency in industrial social-science thinking in recent years, and it has close sociological and methodological connections with operational research.

The socio-technical approach, its supporters claim, is more than a methodological advance; it is a necessary response to a historical change that our societies in the West are undergoing—'the second industrial revolution'. I don't have the space to examine the historical validity of this concept in the detail it deserves, nor can I deal with other related themes such as 'post-industrial society', 'the end of ideology', 'the convergence of advanced industrial societies', and so on. For present purposes, we need only to know about the particular claims that are made about contemporary history. These are two-fold: claims about technological development and claims about cultural development.

Taking the former claim first: as automated production technology has developed, so the argument goes, the nature of production work changes. Manual work is absorbed by the technology, that is, by the machine-system of production; the worker is left with mental functions only. Furthermore, as mental operations are automated—through the programming of routine decision-procedures and calculations, and through direct automatic control—only those mental functions are left which cannot be automated: trouble-shooting, handling emergencies, discovering and orchestrating the interactions between parts of the system, creating and responding to changes in the organisation of production, and so on. Examples of this kind of work are usually taken from the process industries, especially chemicals and petroleum production. The responsibility, initiative, autonomy, and good labour relations of work in process production are contrasted with the characteristics of work and workers on the automobile assembly line. The upshot of this second industrial revolution, based on 'information-processing technology', is that the 'ox'-like worker of Taylor's model is becoming obsolete—if, indeed, he ever existed. A new, more intelligent, more responsible, self-regulating worker is required. 'The advance of technology itself has reversed the world of Frederick Taylor' (Trist, p.113).

Before going any further, we may note one of the paradoxes of this viewpoint. Although developed in reaction to the mechanistic nature of Taylorism, the socio-technical approach contains its own form of mechanism, rather broader in conception and more subtle, but still mechanistic. The operator of an effectively designed automated system fits

into the machinery of production in as calculated a way as does the assembly-line worker. The difference is that in the former case the calculations are done by the cybernetician in addition to the mechanical engineer and the ergonomist. Management science which leans on system theory tends to have a strong stream of cybernetics in it, and both system theorists and cyberneticians view organisms and organisations as organic machines in which the parts are significant only with respect to the functions they perform in the adaptation of the whole to its environment. Thus an operator is not the operator *of* a system; he is an operator, a mere transfer function, *in* a system. The cybernetician has the conceptual tools of 'information', 'variety', 'homeostasis' and so on. With these he can go beyond the simple mechanistic calculus of time and motion, as found in scientific management. In theory, at least, the social system of production can be analysed in a systems-cybernetic mode, as an extension of the machine system.

This mechanistic tendency appears in the socio-technical approach. Men are often described in the socio-technical literature as if they were important merely because they are the cheapest sufficiently sophisticated control devices on the market. Socio-technical job-design methodology has the basic structure of cybernetic-system design. First the 'key process variances' are identified; these are the disturbances that upset the physical and social smooth running of the system. Then physical and social systems are designed with sufficient intrinsic flexibility to control these. In a Norwegian paper-pulp plant, for example, undesirable variations in pulp quality were brought under control by installing, not a mechanical or an electronic control system but, despite the reluctance of the workers, a social and informational control system. Provisions were made to enable the process workers to obtain more information on the ongoing quality of the product, and intra-group communication channels were enriched. At the same time a bonus scheme was devised which tied the bonus of the workers *as a group* to the quality of the product. Thus the workers were given an incentive and a means for learning more about the practical skills of quality control and for involving themselves more in the articulation and achievement of managerial objectives at the plant level.

Whereas Taylor measured men by the standard of the mechanical technology of his time in order that both might be integrated for greater efficiency, the socio-technical systems approach measures men by the standard of the 'information technology' of 'the second industrial revolution'. If this were all that there is to the socio-technical approach, there would be little to distinguish it from the crude 'natural-scientific' approach of Taylorism or technical OR. However, we still have to deal with the social changes noted by socio-technical theorists. These theorists identify an emerging new culture: a new attitude to work on the part of workers. Workers are no longer so easily fobbed off with boring, fragmented, monotonous jobs with no future, the theorists tell us; workers want more meaning in

their work and more control over it. I don't wish to disagree with this as an observation. I believe that in the capitalist societies of the West there is an alternative—workers' control and self-management—and that, as workers come to recognise that it is not necessary that they should be alienated in their work, this alternative will gain strength. Rather, I want to show how socio-technical theory stands in relation to this alternative and to discuss briefly the meaning that it attaches to the term 'alienation'. This is important because the discussion will show that, despite their differences, there is a basic similarity between this relatively sophisticated social science approach and the technical OR approach we looked at above.

Socio-technical theory's interest in social change has two aspects of significance for us here. One concerns the content of the theory itself; the other concerns arguments about the historical necessity of the theory. Taking the second point first, socio-technical theorists see a happy convergence between social and technological development at the present moment in history. Technology, it seems, is driving history along, producing far-reaching demands on the quality of the labour force. But, thankfully, the very qualities demanded are emerging as the values of 'a new work culture'. There is a strong dose of wishful thinking involved here. More than that, there is a strong ideological rationale implicit in the use of terms which have different meanings for people with different social positions, thus giving a false impression of agreement. Louis E. Davis, an American exponent of the socio-technical view, speaks of the new values of 'participation and control, personal freedom and initiative' (Davis and Taylor, p.421; et passim). For management, and therefore for socio-technical theory, this means *participation* in those areas of responsibility devolved by higher management under economic pressures; *control* only of the technical process of production (as far as limited expertise allows), not of the wider economic and social processes; *personal freedom* to select one of the 'careers' designed by manpower planners; and *initiative* in acting in the employer's economic interest. Whether this is or will be the workers' own understanding of their own developing demands is much too open to debate—and to practical action—to justify Davis' claim that industrial needs and cultural development are convergent.

Thus socio-technical theory's understanding of history has a clearly ideological thrust. It presents contemporary social and economic development as convergent with a sectional interest, the economic and social interest in retaining the existing power relations through which the employee relates to his work and its products. This ideological stance is reflected *within* socio-technical theory, in its structure of concepts. In their theory of the way in which society coheres, one of the central concepts used by socio-technical system-theorists is that of 'shared values'. This is matched at different levels in their theory of implementing social change by the concepts of 'institutionalisation' and 'problem-solving'.

Shared values are what determine the systematic unity of individual actions of members of social groups; 'shared values' exist when there is a commonly accepted understanding which serves to orientate and restrain social behaviour. Thus the logic of the concept of 'shared values' is the logic of *tradition.* This in itself is paradoxical in a theory which lays so much emphasis on innovation and change. But the essence of tradition lies in its unchangeability, its inaccessibility to those who are subject to it, and in recognising this the paradox dissolves. For shared values are seen as things to be 'installed' (as Emery has put it), and the installation process is monitored and its limits are controlled by the managers of the process, assisted by their researcher-consultants. The account of the socio-technical programme at Shell Refining UK, for example, makes clear the attention paid to careful top-downwards control of the process of debate and agreement (Hill). Irrespective of their formal status in the organisation, most of the people involved in the process are not managers but *managed.*

Culture is a two-sided concept. It can imply the restraint of social behaviour by established understanding, or it can refer to the domain of social experience in which people work out their understanding of the world. These two aspects—external determination, and self-formation—are dialectically opposed. The theory of culture implicit in social-technical theory emphasises the former, conservative aspect. Such an emphasis is not in conflict with a top-downwards control process but, on the contrary, supports it. This theme of keeping the members of a social group in line with what is desired of them by their managers is followed through in the notion of institutionalisation. In a period when technology, economy and culture are changing rapidly, there is a risk that managers will not be able to exercise effective control over the organisations they are supposed to manage. Recognising this, Emery and Trist propose that a strategy of 'institutionalisation' should be adopted. New values must be encouraged which will allow governmental, trade-union, and business organisations to function harmoniously.

As an example of what they mean, they suggest that British farmers will have to relinquish a great deal of control over what they produce, how it is produced, to what standards, and how most of it is to be marketed in order that the National Farmers' Union can function effectively. The new values that farmers must come to share will 'relate them to the wider society' (Emery and Trist, pp.249-251). Specifically, these values represent the capitulation of farmers to the imperatives of an international economy dominated by large, multi-national industrial groupings and financial interests. This logic appears at the opposite end of the organisational scale in the concept of 'problem-solving'. Problem-solving presupposes fundamental agreement on the most wide-reaching issues, which are therefore removed from the arena of 'practical' discourse. Socio-technical theorists tend to advocate problem-solving as the desired mode of interaction between

managers and managed in preference to bargaining, which implies that conflict of interests exists.

The notions of shared values, institutionalisation and problem-solving lead us into the problem of alienation. Another example will be helpful here. Socio-technical researchers were called in to study the morale of busmen in Dublin following a period of severe labour unrest (van Beinum). The researchers' conclusions were hopeful: there existed, so they claimed, 'a commonly-shared set of values' between workers and management. However, this seems to be another case of socio-technical Newspeak. It is true that the busmen expressed a basic commitment to their jobs, which they felt were potentially satisfying and provided a valued public service. But to say this is not necessarily to express a commitment to management's objectives as such. Indeed, some of the workers suspected that the objectives of the top management, which was responsible also for concerns other than the bus company, were in conflict with their own. That is, it seemed to them that the predominantly financial interests of top management were in conflict with the busmen's immediate objective of providing a useful and valued social service. Economic decisions at higher levels determine how easy it is for the men to fulfil their immediate objectives. For example, the level of fares, the level of vehicle maintenance, and the general level of service provided to the community are all basic questions of 'resource allocation' which strongly affect the physical, mental and social aspects of the work of a bus crew. Yet the men were ignorant of and lacked control over these higher-level decision processes. This provided an objective basis for their suspicion and mistrust.

Such suspicions of management were rightly regarded as signs of alienation. The socio-technical solution was to sharpen up formal definitions of responsibility at the lower levels of management and within the crews and to propose improved communication between these managers and men at the level of day-to-day operational matters. Problem-solving was advocated. This solution reasserts the very hierarchical division of decision-making and operating labour that provides the basis of the men's alienation from the decisions affecting their work. Instead of genuinely shared values, evolved and manifested through a shared social practice of decision-making and discussion at all levels, what we see in this case is an example of the 'operationalisation' of top management's objectives. The systems approach operates by taking the objectives of the highest hierarchical level of the system and translating them into narrower and more specific objectives at each lower level. The busmen's set objectives can then be said to be an 'operationalisation' of top management's objectives in that the employers' success in achieving their objectives depends on the busmen's success in achieving theirs. This *might* be described as sharing in the running of the organisation. But both contributions and rewards depend on the level within the hierarchical system, and no single set of standards or responsibilities

is shared throughout the whole system. The 'autonomy' of the various different levels of jobs within the organisation, about which socio-technical theory makes a great deal of fuss, is another name for their alienation, their cosy encapsulation within a hierarchical structure of decision-making.

The example of the farmers and their union provides further illustration of what is absent from the socio-technical concept of alienation. The socio-technical solution to the farmers' problems requires that they relinquish control over the most basic economic decisions affecting their work. The only excuse for failing to recognise this solution itself as a prescription for alienation is that alienation of this kind has been accepted as part of most people's jobs for so long that it is usually regarded as normal.

It seems, then, that the socio-technical assault on alienation is not intended to be thorough-going. For socio-technical theory, work organised according to the principles of scientific management—fragmented, boring, simplified work in which the worker is required to play no creative or responsible part—is taken to exemplify alienated work. Thus *un*alienated work comes to mean complex, interesting work in which the worker's social skills are called upon in order that he may fulfil the responsibilities and problem-solving tasks which are now made part of his job. Given a large conception of alienation, however, socio-technical job design can be seen to create jobs that are still alienated in an important sense; that is, they presuppose strictly limited self-regulation *within* levels or functional subgroups of an organisation. Socio-technical theory argues that such self-regulation and 'autonomy' are necessary at all levels of the organisation if modern organisations are to continue to achieve their objectives. Thus, if socio-technical job design is successful, the behaviour of individuals is brought more into line with the needs of organisations as those needs are perceived by the organisational managers, but without any corresponding increase in the power of individuals to exercise control over the large-scale organisational conditions of their work and life from the bottom upwards.

It is apparent that in at least one fundamental respect the sophisticated socio-technical approach does not differ from the elementary, natural-scientific approach of technical OR. In the Coal Board study, people were regarded as objects whose social qualities, other than the abstract, objective quality of their being wage-labour, did not need to be considered in order to arrive at a useful guide for the practice of managerial control. In the socio-technical approach we find acknowledgement of the fact that still more effective control can stem from the recognition and managed 'revival' of these qualities. We find also a confidence that social products such as shared values can be successfully manipulated in the interests of the manager of social and economic institutions. What both approaches have in common is this: the free development of society as a conscious expression and articulation of the social being of its members is denied in the theory of management science and through the associated practice of managerial

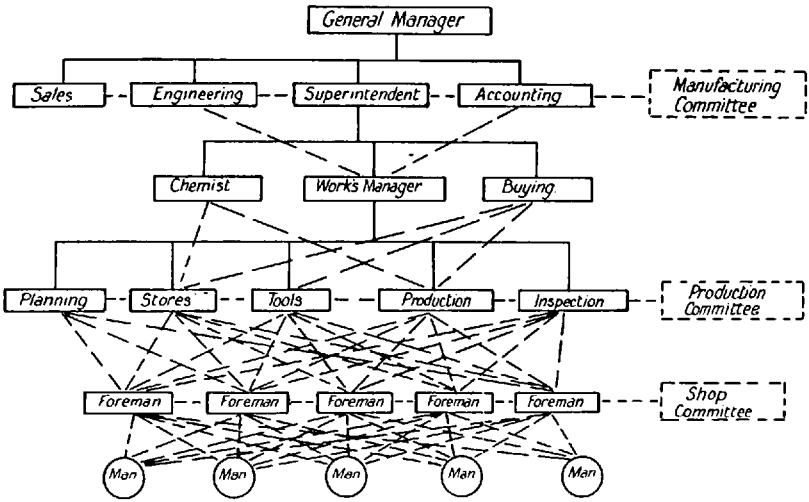
control. Thus in neither the behaviourist-instrumental mode (technical OR), nor the quasi-practical mode (socio-technical system theory), is management science able fully to recognise and develop people's social character as anything other than properties of a system.

In the remaining part of this article I want to go beyond these two specific moments in the development and current practice of management science, technical OR and socio-technical system theory. I shall try briefly to supply a way of looking more generally at the historical development of the forms of scientific aid to managerial control. In doing this I shall try to show that the inability of management science to recognise and develop the full social character of the system it studies is not fundamentally a theoretical or technical shortcoming but rather a consequence of the social, political and economic interest with which management science identifies, and which is reflected in the methodological and conceptual structures of the science.

Management Science and Scientific Management

Many writers on management science feel it necessary to make a strong distinction between management science and scientific management. Some of the earliest public statements about operational research, for example, carefully point out that the 'naive and offensive elements' of scientific management were not present in operation research. This dual moral and methodological rejection of the practice and logic of scientific management remains to the present time in some views of management science. Nevertheless, even in rejecting the scientific-management approach, some management scientists point out an underlying similarity between scientific management and management science in terms of their historical functions. Thinkers of this persuasion argue that management science bears the same relation to 'the second industrial revolution' (based on an 'information technology') as scientific management did to the first ('energy technology'-based) industrial revolution. Such attempts to develop an historical perspective are comparatively rare, however; a great many management scientists, and others working with the technology of the second industrial revolution, adopt an attitude not easily distinguished from the 'one best way' philosophy of Taylorism.

The American behavioural scientist, Herbert Simon, claims that 'No meaningful line can be drawn any more to demarcate operations research from scientific management or scientific management from management science.' I believe that this statement is misleading. The similarity of scientific management and management science in their historical roles, and the mechanistic approach of many practitioners of technical OR, might be taken as support for Simon's claim. It is also true that industrial engineering since Taylor's time has absorbed some of the insights of the behavioural sciences.



Nevertheless I want to argue in this final section that, although there is an important historical continuity between scientific management and management science, there are also important differences. I should point out that in thus disagreeing with Simon I am not therefore agreeing with the apologists of 'the second industrial revolution'. The interpretation that I am suggesting focuses primarily not on the development of technology as such but rather on the nature and development of capitalism in the 20th century.

Capitalism has undergone a very considerable growth and development in the present century. This fact is recognised not only in managerial folklore and ideology (as in the myth of the 'soulful corporation' described by Nichols) but also, occasionally, in management theory. Something of this development is captured, for example, in Emery and Trist's notion of 'the causal texture of the environment'. In their attempt to characterise the environments of organisations, they present four ideal types of increasing complexity. These range from the perfect competition of the laissez faire economy through imperfect competition to oligopoly and, finally, to what they call the *turbulent field*. This last type corresponds to the environment of the modern multi-national firm, confronted by other multi-national giants, powerful trade unions, rapid technological change and ever-expanding state intervention in national and international economic affairs. In such

conditions, capitalist planning institutions must be created to service the movements of these leviathans. The need for new social structures adapted to the demands of managing modern capitalism is recognised in Emery and Trist's concept of 'institutionalisation', as I noted in my earlier discussion.

A relevant aspect of this development from the entrepreneurial to the multi-national firm is the internalisation of markets within oligopolistic and multi-national firms. That is, such firms develop internal systems for achieving the co-ordination which was once left to the external mechanism of the various markets—the labour market, the raw materials market, the finished goods market, the technical innovation market, and so on. The increased rates of technological and economic change which are now typical, and the increased geographical scope of the operations of multi-national firms, produce a demand for better methods of controlling the operations and development of firms and of the state-controlled activities which service them. These requirements, together with the new control needs stemming from the internalisation by firms of market functions, are reflected in the historical development of management science.

What of the role of science and scientists in this development? Expressions like 'scientific and technological revolution' ('Prague Spring', 1968) and 'the white heat of the technological revolution' (Labour Party, Great Britain, 1964) have become clichés. Yet what they are supposed to mean is usually far from clear. In this discussion I want to take the emphasis away from technology, after acknowledging the extent to which sophisticated technology determines the products, the processes, and the cost structure of modern capitalist production. I want to place the emphasis instead on the functions of scientific work in relation to social development in capitalism. Some commentators (the French marxist Christian Palloix, for example) regard the employment of large numbers of technical specialists in the modern firm as an expression of a novel development, the appropriation by the firm of knowledge-generating activity. The particular form of this phenomenon, involving the employment of a large proportion of highly trained non-manual workers, is certainly a recent historical development. But if we look beneath its modern form, a state of affairs of much longer standing is revealed. When Frederick Taylor spent half his life learning 'the art of cutting metals', thus appropriating the knowledge that was hitherto possessed only by the workers whom he managed, he was only making a leap forward in a well-established tradition of the de-skilling of labour which continues today. A large proportion of the scientists employed in modern firms are the heirs to this tradition. As skilled mental workers, it is their job to ensure the evolution of technical knowledge and its embodiment in material form as technology, this responsibility having been removed from the unreliable 'hands' of the traditional workforce and entrusted to the controlled hand of scientific research. Scientific work of this kind aims

at the technical control of material things, of 'external nature'; it embodies an instrumental interest.

The tradition also has a second aspect. Taylor and his scientific and entrepreneurial predecessors were not only engaged in the appropriation of *technical* skills which would later be opposed to the workforce in the objective form of technology. They were also capitalising on the social nature of the production process. The technological aspect of the industrial revolution, the introduction of power-driven machinery, was anticipated by and confounded with a *social* innovation, the massing together of workers in manufactories. In the development which took place at the hands of capitalist entrepreneurs, the social nature of production was both recognised in principle and denied in practice. That is, the productiveness of the division of labour *was* recognised. Recognition was implicit in the principle of 'rationalisation' which guided Taylor and many others before and since. These people concern themselves with the objectified social relations of production, with 'the division of labour'. This aspect of the social nature of production was recognised in scientific form in Taylor's scientific management. Through developments in this domain of scientific work, the division of labour was taken to greater and greater lengths. But the *subjective* aspect of production, as a response to and determinant of social needs and as a process controlled through the social interaction of the producers, was obscured and denied in practice. Response to social needs is disguised in the individualistic garb of the 'free market'. Control by the producers is denied in practice, as the 'hands' are subjected to the external control of overseers and managers and, more radically, of the clock and the machine. The scientific appropriation of the subjective relations of production, following the appropriation of instrumental skills and of the objective social relations of production, has had to wait until more recent times; scientific work which aims at this constitutes part of industrial sociology and management science.

We have, then, three categories of scientific work in relation to social development in capitalism. There is work concerned with technical and technological development. This embodies the interest of *instrumental* control over external nature. There is work concerned with objective social relations in the firm and its environment. This embodies the interest of *instrumental* control over externalised, reified social relations, that is, social relations turned into mere structures of *things*. And there is work concerned with the subjective structures of social relations within the firm and its environment. Such work aims for an understanding of the way people relate to each other and learn to work together; it embodies a *practical* interest. The important difference between management science and scientific management may now be put in this way: management science contains work in the second and third categories; scientific management falls wholly within the second.[3]

The achievement of Taylorism, as a scientific development in the second

of these categories, lies in the structural socialisation of labour, by which I mean that it provided a method by which the productive activities of individuals could be orchestrated as moments in the movement of a single whole.[4] This problematic also embraced the work of others (including Max Weber in Germany, Henri Fayol in France, Brech and Urwick in Great Britain, Chester Barnard in America) who were less immediately concerned with the production process and rather more concerned with administration. All of these developments in management theory were concerned, to a greater or lesser degree, with means of specifying formally and regulating the *details* of productive and administrative work. All, to a greater or lesser extent, made use of a deterministic machine-model of organisation. Operational research represents a change within this line of development in its use of *in*deterministic (probabilistic) models in place of deterministic ones. Instead of being primarily concerned with the detailed content of individual activities, the methodological stance and the techniques of operational research impart to it an immanent interest in the form of systems of activities as they appear at a higher organisational level.

To put it another way, operational research studies the activities of individuals or groups as they might appear to someone with an interest in the co-ordination of activities rather than in their detailed supervision. Scientific management attempted to split up work into fragments, externally regulated. Operational research, by virtue of its systems methodology and statistical techniques, can attempt the integration of systems of activities which have their own internal structure of regulation. The result, ideally, is a whole which is either largely self-regulating or so structured as to make deviations from desired performance easily seen by those managing the process. (If we typify Taylorism by the production-line concept, then this kind of approach might be typified by critical-path planning, for example.) Although technical OR can thus be seen as following the same historical project as scientific management, the project of the structural socialisation of labour, it tends to balance the fragmenting tendency of Taylorism.

Putting this in terms of centralisation and decentralisation, the logical end of scientific management lay in the complete centralisation of decision-making and control. Operational research, however, is technically able to reflect on and operate in structures in which control is centralised and decentralised to various degrees. This technical capability is, under the given historical conditions, used for the purposes of retaining control of the significant features of activity from above while relegating control of the details to a subordinate level of organisation. However, the interest in instrumental control over the objective structures of social activity that is implicit in technical OR is not the same as an interest in *hierarchical* control. Scientific management cannot be used to order productive activity in any way other than an hierarchical way, separating decision-making from operation. But with operational research a choice is implied: hierarchy or not?

Committed to servicing a capitalist world economy, operational research scientists have, whether they know it or not, made a choice.

The logical shift implicit in technical OR has a positive historical significance. The structural socialisation of labour has now advanced much further than it had in Taylor's time, when the drive for rationalisation and the tendency towards oligopoly was only beginning to make itself felt. As multi-national firms have grown, the geographical scope of the division of labour within the firm has greatly increased through penetration of foreign markets and 'backward integration' to assert control over raw-material supplies. Problems have thus emerged of exercising control at the periphery from the centre. The depth of the division of labour has increased too. There now exist not only manual workers directly engaged in production but also many grades of mental workers engaged in productive, clerical, administrative, managerial and research activities, including, of course, management scientists. Many of the decision-making and information-generating functions traditionally performed by the entrepreneur and co-ordinating functions once performed by the market have been devolved to these mental workers. Problems of control in such conditions are clearly very different from those in the entrepreneurial firm, and they have been aggravated by the increasing pressures of international competition. With its integrative approach and formal techniques, technical OR can be seen as the scientific arm of a strategical attack aimed at effective central control coupled with a considerable degree of autonomy within the various areas of activity and at the progressive 'rationalisation' of this complex division of labour.

The development of management science beyond the limits of scientific management is not simply confined to advances within the domain of the structural socialisation of labour. As I have said, management science acts as an agent of the process by which modern capitalism 'rationalises' itself. Rationalisation requires that objectives be clarified, and one of the aims of operational research that was not implicit in scientific management lies precisely in this domain of clarifying objectives and priorities. The construction of operational research models is not simply a matter of measurement and the application of standard techniques for the 'optimisation' of the performance of the system. Operational research also has a repertoire of largely informal procedures through which it clarifies what counts as 'optimisation' or 'improvement'. In developing these procedures, management science enters the domain of praxis, the domain in which mutual understandings, norms and values are established and made clear. In attempting to become a scientific practice in this domain, management science goes quite beyond a concern with objective social relations alone and begins to constitute itself as an area of scientific work in my third category. The 'one best way' logic of scientific management gives way to a 'learning' problematic, generating a praxis through which 'managers' and 'researchers'

mutually refine their understanding of the system of which they are part.[5]

As an abstract logic, this approach has no hierarchical implications. 'Management' may refer to a fundamental function of all rational social praxis or to a specific function of a technocratic elite. To see the form in which this practical interest is embodied in management science's actual practice, we may look a little closer at the 'autonomy' of sub-systems as they appear to management science. I have said that management science's approach to management problems can result in giving a degree of autonomy to hierarchical levels or to functional and geographical divisions of an organisation. Doing this in practice brings with it its own problems of making sure that the part functions in the interest of the whole. Success in achieving this becomes more vital as rationalisation becomes necessary following increasing competition and squeezes on profits; employees of the firm at all levels have to be made to adapt to and, if possible, contribute to change. Scientific management failed to recognise that the well-springs of creativity and change lie in free personal and social development. Sophisticated management science attempts to establish a managed form of development which approaches this *in appearance* in the hope that the benefits, in terms of creativity and co-operation, will be reaped by the organisation.

'Autonomy' is a central theoretical and ideological focus in socio-technical system theory, as I noted in the second part of this article. In that context the concept appears as the dialectical inverse of 'operationalisation'. That is, according to the systems model of an organisation, 'those managing' clarify the objectives of the whole system. These objectives are then 'operationalised' in terms of the activities of 'those doing' at each hierarchical level of the organisation. Management scientists play a part in both the practical activity of clarifying objectives and the instrumental activity of operationalising them. What results is a skeletal form, structured overall by an instrumental logic emanating from 'the Top', which must be filled out by the 'autonomous' detailed activity of subordinates. This hierarchical structure of 'practical' within instrumental action is found throughout the organisation. The praxis generated by the most sophisticated management science is thus false praxis. That is, instead of being the self-development of self-conscious social beings, it is a development contained within an external, instrumental logic. This logic, attributed to a mythical entity—The Organisation—is ultimately the logic of Capital.

Once again, then, we find the basic unity of management science which treats people as things and management science which appears to treat them as people. The Coal Board study and the Norwegian pulp-mill study, for example, are merely two different modes of operationalising the objectives of the organisations concerned. In the case of the Coal Board the praxis involved was limited to the interaction of management scientists and managers. Workers, as is traditional, were involved only as objects of manipulation, not as subjects in the decision-process. In the Norwegian pulp-

mill case, praxis was extended to the workforce, whose co-operation and creative participation were intrinsic to the success of the project. In both cases what we see is the attempt to restructure the social system—objectively, in one case, objectively *and* subjectively in the other—in order that 'the objectives of the organisation' might be fulfilled. In both cases people were a means to the external end of economic survival of an organisation in a capitalist environment. In both cases we see the fundamentally instrumental rationality of management science, and management, as a capitalist practice.

Thus, potentially, social praxis in the world of production becomes an object of scientific study. When production is seen as the production of *capital*, however, praxis must become an object of manipulation. It need hardly be said that this aspect of management science—the appropriation of subjective social relations—is far less fully developed than the appropriation of objective relations as practised by scientific management and technical OR. Nevertheless, fragmentary efforts are made. Operational research within the 'learning' problematic tackles this task more or less systematically in a micro context under the rubric of 'problems of implementation'. The philosophers of 'the systems approach' have made significant inroads into problems in this area. And at a macro level, as in the case of socio-technical system-theorists' involvement in the so-called Norwegian Industrial Democracy Project (Emery and Thorsrud), some systematic attempts have been made.

The degree to which such projects have so far been successful, on a micro or macro scale, is less important than their formal significance. As capitalism has developed, the social division of labour has expanded upon the basis of the progressive appropriation of material nature and of reified social relations. For the most part, however, the social nature of production remained tacit. Society is classically represented as an aggregate of individuals who find their social reality only in the market. As the power of firms grows, deforming the mechanism of the market, this power is expressed in the expansion of the division of labour within the firm. This developing objective socialisation of labour finds itself reflected in and advanced through, first, scientific management and, later, management science. Just as society was objectively co-ordinated through the market at an earlier stage of capitalist development, so it was subjectively rooted and mediated in tradition. As we have moved from 'traditional' to 'modern' society, traditional understandings and modes of social action and cultural formation have begun to prove inadequate, and the problem of controlling the *subjective* co-ordination and development of society presents itself to the manager of industry and society. Where management science goes beyond scientific management it reflects this need to subject social practice to conscious reflection and direction under the specific imperatives of modern capitalism.

To conclude this discussion of scientific management and management science: I have presented briefly a way of looking at them which shows the roots of their similarities and also of their differences. In order to do this I have not had to fall back on technology as the main thread of my explanation. Technological development does, of course, play an important part in this kind of explanation, mainly as an assumed dimension of the process through which the main development proceeds and as an agency through which competition largely finds its expression in the world of monopoly capitalism. But the main basis of the explanation lies elsewhere, in the growth and development of a particular mode of economic organisation. Thus my explanation is not primarily technological.

Nor is it determinist. The successful appropriation of social interaction and the processes of cultural formation by a science of social management is in no sense an accomplished fact. It is simply necessary if capitalism is to continue its development. The development of the objective socialisation of production leads capitalism into deeper crises of stabilisation; and the science which services this development carries in it the tacit promise of other modes of rational organisation of society. Similarly the development of the subjective socialisation of practice in the domain of production contains the potential of dissolution and transcendence of capitalism. Management science carries unconsciously the promise that 'those managing' can be the same as 'those doing'. In a class-divided, hierarchically structured capitalist society this promise must remain suppressed. Management science, as it advances beyond scientific management, brings with it new forms of the old contradictions.

It is here, fundamentally, that I part company with those who talk of 'the second industrial revolution'. For them social development and capitalist development are convergent. Certainly there is revolution somewhere in the air, but it is not *technological*. Setting their discussion in terms of technology, and not in terms of society or power, gives away the root of their ideological position. For if it is the 'inexorable' march of technology that drives the process of change (and not the historically circumscribed growth and development of a system of social and economic power), then the claim that the boss-worker relationship is on its way out, that 'the advance of technology itself has reversed the world of Frederick Taylor', might be made to sound more plausible. If this ideological position can be sustained then the managed development of a false praxis under the imperatives of capitalist development can be presented as natural and necessary.

I argue that this development is not 'natural' and necessary. It is one possible mode of social development. But an externally managed, and therefore false, praxis can only develop actively by reference to images of its opposite, self-management, and by trying to pass itself off as its opposite. This contradiction, together with its technical capability and practical understanding of managing and directing, make management

science a vivid testimony to the 'nature' of current social development. The fundamental unity of management science and scientific management lies in their role of working out in scientific terms the logic of domination of capital over work and praxis in particular historical conditions. The project of management science is too immature and its contradictions too complex for confident predictions to be made. But I will make an assertion. The promise of management science—the promise of self-management—will not realise *itself*. Organised, self-conscious agents are needed to make this ideal real. Will any of these agents be management scientists? That's another story; and history has yet to write it.

Notes

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1. Another bearer of the seed of the 1930s scientific radicalism is the burgeoning institutional activity of science policy studies. Although I am not dealing specifically with science policy research in this article, it is worth noting that it tends to be very close in many logical and methodological respects to management science. My critique might therefore be easily redirected into the critique of science policy research.
2. The sense in which I use the word 'practical' may cause some difficulty. It corresponds to another word I shall use, 'praxis', which means 'self-conscious, self-developing social activity'. Thus 'practical' as I use it means the *opposite* of 'unreflective, rule-of-thumb, undynamic'—its common connotation. 'Praxis' and 'practical' have three major overtones of meaning which, individually or together, are foreign to bourgeois thought in general and the Anglo-American philosophical tradition in particular. One of these is collectivity; praxis is shared, collective social practice. The second is communication; common language is the medium in and through which shared meanings are established, and in which resides the possibility of co-operative, directed social action. The third is critical self-reflection, through which the limits on social development become intelligible and new social relations become thinkable.
3. This system of categories is developed from Habermas' theory of cognitive interests (see Bibliography), about which nothing more can be directly said here. The derivation is treated more fully in my D. Phil. thesis.
4. This term is taken from Sohn-Rethel (see Bibliography). My analysis is based on an extension and critique of Sohn-Rethel's important analysis of scientific management.
5. This concern with critical self-reflection, and with the way in which systematic theorising is related to practice, grows from management science's philosophical

roots in the pragmatist tradition. But importantly, of the three major overtones of 'praxis' noted above, collectivity is neglected by these thinkers.

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The Politics of Abortion as 'Family Planning'

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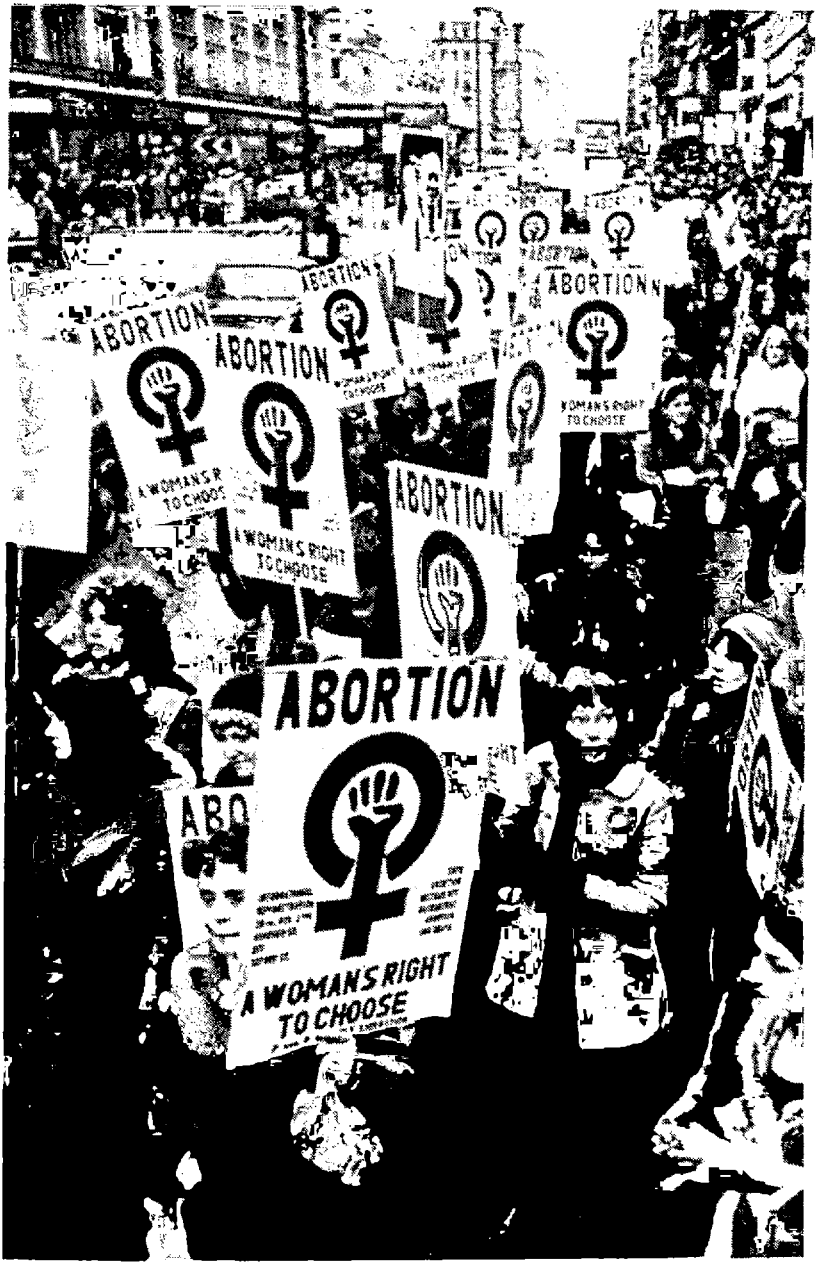
In a general practitioner's morning session, most of the people waiting are women. The same is true of the people in mental hospitals, ante-natal clinics, welfare clinics and family planning clinics. Women are the main recipients of medical treatment; they also take the main responsibility for the family's health.

The medical care system is more than just a service. Medical expertise plays its part in reinforcing women's subordinate role in society. In the case of women's requests for abortions, doctors exercise medical judgements which they see as necessarily involving both technical and moral criteria. Based on my interviews with several general practitioners about their attitudes towards authorising abortions, I will analyse here how doctors make their medical judgements.

Nuclear Family Politics

Women have to ask doctors for abortions. It might be argued that abortion is like any other operation, where the doctor as 'medical expert' has to have the last word because he is the possessor of specialised knowledge. However, the case of abortion shows clearly that medicine, like other areas of pure and applied scientific knowledge, is not politically or ideologically neutral. As my study shows, doctors' decisions as to whether to grant abortions are often affected by considerations other than the medical risks involved.

In particular, 'medical' decisions about whether or not a woman should have an abortion are based on the assumption that children should be born into and brought up in the conventional 'nuclear family'. The term 'nuclear family' commonly refers to a unit consisting of husband, wife and dependent offspring (Gordon, p. 1). When an abortion is granted because a woman is unmarried, because she cannot cope with looking after more children unaided, or because her financial situation or housing are inadequate, the abortion is performing an ideological role in reinforcing existing social arrangements. This may not be—but often is—the *intention* of the doctor


ABORTION
A WOMAN'S RIGHT TO CHOOSE

and/or the woman; this effective reinforcement is one of the functions of the decision. The requirements of production and the housing arrangements in our type of industrial society militate against alternatives to the nuclear family. The traditional 'extended family' was a residential unit composed of husband, wife, dependent offspring, and married sons and their spouses and offspring. There was more family to help out with child care and expenses. Radical alternatives to nuclear and extended families include various forms of co-operative and communal arrangements, especially ones involving departure from the primary burden of children's care falling on their biological mother; alternatives involve sharing this role with other men and women, often including the children's biological father, whether or not he is their mother's husband.[1]

How does the nuclear family model enter into medical judgements on abortion? Hospital consultant gynaecologists possess a privileged power to grant or deny abortions. Their power is based upon their expertise in the area of women's biological functioning, not upon an appreciation of women's social experience, nor upon any formal study of the moral issues involved.[2] Yet in effect they exercise their power in a way that passes judgements upon a particular woman's relation to the nuclear family and on her attitude towards the medical profession itself.

As part of their presumed responsibility, doctors feel entitled or even required to treat as deviant and punish those women who fail to display appropriately deferential attitudes. Such moral judgements by doctors are all the more transparent in the case of women who request abortions early enough to qualify for the relatively new, simple and safe 'D and C'. (Dilation and curettage is the very straightforward surgical procedure of dilating the neck of the womb and gently scraping the inner lining with a small curette to dislodge the embryo, thus inducing menstruation.)

Moreover, we have to be aware of the reluctance with which the profession as a whole takes up new and simpler abortion methods which would reduce their role—and power—as technical experts.[3] An extreme example of this reluctance was one doctor I interviewed who insisted on using hysterotomy (major surgery involving cutting directly into the womb through the abdominal wall) in every woman whom he approved for abortion, even if this meant waiting two months until pregnancy had reached a stage where the operation (as opposed to D and C) could be justified on surgical grounds. This case is extreme in the degree of self-awareness shown, but it illustrates a norm whereby doctors, under the guise of their role as medical experts, reinforce the *status quo* in the basic social structures into which children are usually born: the conventional nuclear family.

The family acts as a milieu for conditioning children to the capitalist society in which we live by fostering respect for authority, possessiveness, individualism, consumerism, competitiveness.[4] The unwaged work which women do in the family—caring for children, housework, shopping and

cooking—is as necessary to capitalism, at present, as the work done in factories. In fact, since 40% of married women also have jobs, they are making their 'contribution' there as well. Yet women are supposed to do all their work in the home for 'love' (Dalla Costa and James). Thus, once again, when doctors prescribe tranquillisers to women who are finding their roles as wives and mothers unbearable, or recommend abortions as a way of preserving the family structure, they are making political decisions under the guise of ideologically neutral medical expertise.[5]

If we look at the history of the system in which doctors work in Britain, their role becomes even clearer. The National Health Service was set up as an integral, though autonomous, part of the welfare state in 1948 (Eckstein). These institutions were never seen as 'people's services'. Rather, they grew out of a recognition by the state of a need for social investment, in the same way that nationalisation grew out of the state's recognition of the need to invest in industry. Both were forced on the state by working-class demands but used by the state to serve its own interests. An injection of public money and a reorganisation of existing health facilities were required to improve efficiency and to produce a healthy workforce at all levels of mental and manual labour. Even the doctors saw this, and the conservative British Medical Association issued a report in the 1930s advocating some form of health service.

The welfare state had particular implications for women (Eckstein, pp.133-36). These were clearly stated by Sir William Beveridge, whose 'Report on Social Insurance and Allied Services' was a significant policy document for both the welfare state and the National Health Service: 'In any measure of social policy in which regard is had to the facts, the great majority of married women must be regarded as occupied on work which is vital though unpaid without which their husbands could not continue. In accord with facts the Plan for Social Security treats married women as a special insurance class of occupied persons and treats man and wife as a team' (p.49). Thus the welfare state, the National Health Service, the capitalist industrial order and the nuclear family are all parts of an integrated ideological and material structure. Abortion policy can be seen as one of the current means of its maintenance.

Lest it be thought that anyone wants to eliminate welfare benefits, the National Health Service or easily obtainable abortions, it may need to be said that the point is to show that all these relative reforms cannot be separated from their role in maintaining the capitalist industrial order. Reactionaries who would gladly eliminate them do exist, and against these we must defend them. Yet they would not have been granted if 'enlightened' owners and managers had not seen the need for them.

The present crisis in the National Health Service must be understood in the light of its history and ideological role. In Britain's current economic situation, the state has to find ways of cutting back non-productive sectors

of the economy and where possible making the working class pay more for its own services. The government is trying to maintain Britain as a viable capitalist state; if this means choosing between expenditure on arms or on health and education they will choose the former, unless the working class makes so much trouble that they are forced to try to pacify it again. The state is not now prepared to maintain health services at their present level, let alone improve them in the ways that patients and health workers would like.[6] This is part of the broader context within which women are challenging doctors' control over them on issues of abortion. What form does this challenge take?

Many women have organised themselves into health groups to learn about their own bodies: reading, carrying out self-examination, going to doctors and clinics together for solidarity, questioning treatment, learning about alternative medicine.[7] From these groups women have started to go out to give talks in schools, run Workers' Education Association classes, talk to medical students, and write about health in local community newspapers.

There are now new and simpler methods of abortion—particularly menstrual extraction, which can be used early on in pregnancy and for which women's health groups are being trained. The suction method is somewhat more complex but could easily be administered by trained technicians. But the removal of these technologies from the exclusive use of doctors would only change one aspect of the patients' relationship to medicine: the pregnant woman's 'doctor-deference relationship'. At the same time, ideas about what truly socialist medicine could be like are beginning to be raised in some of the action committees set up in hospitals during the recent British nurses' and technicians' strikes. This process is still at a very early stage, but it is through the development and linking of work done in women's health groups (and men's too?) and health workers' struggles, that we can move towards a real people's health service.[8] In these very practical struggles, there is a valuable role for demystifying the authoritarian aspects of Britain's apparently liberal abortion law.

1967 Abortion Act

Legalised abortion and increasing provision of free contraceptive advice have been seen as significant advances made in very recent years in the condition of women in British society. I shall argue that Britain's 1967 Abortion Act and its implementation by the medical profession is a form of repressive desublimation. That is, it gives to the woman the subjective feeling of greater control over her sexuality, whilst—when seeking an abortion—she is still in fact in the role of a supplicant before a powerful expert. She can make a request in an acceptable tone of voice but, and this is the fundamental point, she has *no rights* in the matter.

A woman who wants an abortion can ask a doctor for help, which he is legally entitled to give her if he (and a colleague) consider that her case falls within the interpretation of the Act. The woman herself has no legal right to demand an abortion. The decision lies with the doctor, even where the medical implications of abortion would be straightforward. According to the 1967 Act, legal abortion can be carried out on three main grounds: (1) physical or mental risk to the health of the woman, (2) physical or mental risk to the health of existing children, (3) the likelihood of the child being born abnormal. Madelaine Simms, one of the mainstays of the British Abortion Law Reform Association at the time of the parliamentary reform, described the workings of the Abortion Act after three years: '...the whole concept of "grounds" for legal abortion is a fiction, but a necessary fiction in the face of reform of the law'. She reminds us that

> ...at the time the Abortion Act was passing through Parliament much time was devoted to considering the precise limits to be imposed. This was probably a necessary exercise at the time... but now that the Act has come about the "grounds" are seen to be an artificial construction. They tell you more about the attitudes of the doctor than about the condition of the patient... Doctors have more freedom than women. They can interpret the law more or less as they wish. The reform has helped women by freeing the medical profession to act according to its own best judgement.

The doctor may be, in some cases, a *benevolent despot*, but despot he/she is. It is the doctor who wields the power. The law provides a woman with an illusory freedom: she can probably get an abortion, thus being able to avoid having a child she does not want, but this freedom is available to her only at the discretion of a doctor.

I shall now try to show how the fact that the power is in the doctor's hands affects abortion first on a national and then on a local level. In each case the medical profession's ideological biases and status affect the working of the Act. In particular, while the effects of a crude anti-abortion line are obvious, the more subtle liberal approach can ultimately be seen to be equally oppressive.

The most blatant manifestation of the doctor's power is shown in the regional variation in the numbers of abortions performed. In the first half of 1970 the NHS abortion rate varied from 7.9 per 100 live births in London to 2.4 in Birmingham; i.e. the London rate was nearly three and a half times that in the Birmingham area. This is at least in part determined by the attitudes of the consultant gynaecologists. (Other factors, such as the differential distribution of gynaecological beds, also play a part.) General practitioners in an area like Birmingham are helpless, even if they themselves are sympathetic to abortions, because the hierarchical organisation of doctors means that the consultant determines his hospital's abortion policy by being able to refuse to carry out abortions. In fact one professor sits on

all the appointment committees for consultants; he has been heard to say he would not sit next to a doctor on a bus if the latter 'performed abortions on any but the most narrow grounds'. Thus if a woman lives in the Birmingham catchment area and wants an abortion, she is more than likely to be forced to resort to the private sector—if she can afford it or is fortunate enough to be recipient of a loan scheme or charity. This situation hardly needs further discussion; the hierarchy of power relations is clear-cut. It remains only to emphasise that, as is routinely the case, the poor suffer more than the rich.

Another aspect of the way in which some doctors appear to be imposing their own ethical viewpoint is reflected in the national statistics of the distribution of NHS abortions, as opposed to private ones, between married and single women. In 1968, of 13,000 unmarried women having abortions, 7,000 (over half) went to private clinics, while of the 10,000 married women, 8,000 (four fifths) were aborted on the NHS; i.e. only one fifth went privately. This would suggest that doctors on the whole are more sympathetic to married women as 'entitled' to a sexual relationship, while they feel that the unmarried should, in some sense, be asked to pay for their sins. This is supported by my own field work, which I shall discuss later. Public morality is still far from accepting sex outside marriage. Sex is seen as tied to reproduction, children should be born within wedlock and, once one's sexuality is placed within the control of wedlock, decisions about bearing children are granted to the wife and mother. While such attitudes are current, it would not be surprising to suggest one possible interpretation of the above figures: that doctors express their prejudices by putting the single woman in a position where she has to get a private abortion or none at all.

The two examples discussed above can be seen as straightforward limitations of women's freedom by conservative doctors. The more interesting and pernicious aspect of the power structure of the abortion system is to be seen in its allegedly *permissive* and *liberal* applications. My own study examines the working of the abortion process in a liberal area. It includes interviews with single and married women getting abortions, general practitioners and various members of the hospital staff who work with women getting abortions, such as doctors, nurses and clerical workers.

Let us now look more closely at the 'micro' level, bearing in mind Marcuse's account of the contradictions of progressive movements in a capitalist society:

> According to a dialectical proposition it is the whole which determines the truth—not in the sense that the whole is prior or superior to its parts, but in the sense that its structure and function determine every particular condition and relation. Thus, within a repressive society, even progressive movements threaten to turn into their opposites to the degree to which they accept the rules of the game (*Repressive Tolerance*, p.97).

In what ways has the practice of the 1967 Abortion Act accepted or even enforced the rules of the game? I want to look, first of all, at some attitudes of those who perform the important role of gatekeeper at the door of the abortion system. In this article I use the GPs as the illustration for my argument. An analysis of other professionals involved would reveal similarities, always allowing for the effects of their different roles in the process. I shall argue that the GPs act in the role of 'medical experts', giving their professional sanction to women who want abortions. This means that instead of women coming to their own decisions or even going to an experienced outsider (a *potential* description of a GP's role) for advice, she is in fact being relieved of the burden of responsibility for her own life. The decision about terminating her pregnancy is equated with *medical* decisions which she would not feel competent to make, such as about when or whether to remove an appendix or a tumour.

The criteria which the doctor uses in making this decision lie anywhere along a continuum from the purely moral to the allegedly purely scientific, as it is the doctor who decides what criteria to apply. The patient colludes with the doctor's power by deferring to him/her as a secular priest. In some ways, she usually *wants* to collude. Yet in reality the decision to have an abortion is far closer to, for example, the decision to get married; the help of a doctor *qua* doctor should be necessary only when she needs (a) to know whether she is pregnant, (b) to know whether or not the operation is medically feasible and (c) to undergo the operation. Yet the doctor not only takes over the woman's decision; he also puts that decision within a framework of conventional morality, inextricably intermingled with science and with his professional status. This morality takes as its base-line the nuclear family and sees deviations from this model as situations which it is his province to correct. He does not see it as part of his role critically to examine or to correct the model.

Before we go any further, I think it important to emphasise that I am *not* trying to show that the (often liberal) GPs I am describing are intentionally repressive or bad. Their role is a structural one. They have been given a particular training to perform a function in society. They usually enact the role for which they have been socialised with the best of intentions and genuine concern for their patients. The oppression that they perpetuate is a structural consequence of this. They do not perceive their behaviour as being ideologically determined because, as so often happens, the ideology is so deeply internalised that they are not conscious of it; it seems 'natural'. It is only the critics of the *status quo* who are seen as 'political' or 'ideologues'. An analogy may make this point more clearly. I have often found that, in discussing Women's Liberation with men, they point out to me that they are not oppressive to their wives or female friends: they are kind, considerate and thoughtful, do their share of the household work, etc. However, the oppression of women in our society is not a matter

of the individual male's good or bad intentions; it is a structural oppression based upon the social and economic organisation of the society in which we live.

Moral Guardians

Let us now look at the results of the interviews which I carried out with eighteen GPs. Only one of them could be described as not being 'liberal' in his attitudes towards abortion. I shall describe his viewpoint in some detail and then go on to describe how in a less blatant way the other doctors propound similar views in spite of the fact that they see themselves as being on the opposite side of the fence. Dr W describes himself as surprised to learn how close his own attitudes are to those of McClaren, the Birmingham gynaecologist who opposed the 1967 abortion law reform. Dr W sees himself as 'ombudsman between the mother and the foetus'. He believes that the woman who wants an abortion is in such a state of stress and fear that she cannot really know what she wants. It is up to him to judge how much of this stress is a temporary state of mind that she will overcome. He interprets the 'mental health' clause of the Act to mean that there must be some sign of 'derangement' in the woman before he can recommend an abortion. At the same time he feels that the single girl who has been 'let down' by her boyfriend, or a married woman who is pregnant by a man other than her husband, should be granted abortions.

He describes a case that makes him angry—a girl who has had one abortion and then went on the pill. She took herself off the pill and got pregnant again. Now she wants another abortion. She simply sees her pregnancy as a nuisance. Dr W sees himself as being in a better position than the girl to make an objective assessment of her situation and thus to make more realistic projections into the future for her. He is *not* saying that it is his job to try to elucidate the situation for the woman so that she can make up her own mind; he takes seriously the job that he believes the law to have delegated to him, namely to be an impartial judge.

But how impartial is he? What beliefs underlie his judgement? Let us return to the three cases described above. A single girl who has been 'let down' can be seen in a sympathetic light: she is a pathetic figure who has been wrongly treated by a man. The expression 'let down' implies certain views of a man's responsibility to a woman and does not include the possibility of the girl's taking an active role in becoming pregnant or in the breakdown of the relationship. As a pathetic figure she does not present a brazen threat to accepted social norms. As a passive figure rather than an agent she cannot be held responsible for her situation, nor can she be expected to 'put it right' by marrying, since the man concerned is gone. The man can be seen as a bad object, leaving the girl deserving of the doctor's pity and help.

In the case of the girl coming along to ask for her second abortion, the new element may not be obvious. The girls would face equivalent situations if they had to go through with their pregnancies, but from the doctor's point of view the girl who calmly gets herself pregnant for a second time is defiantly behaving badly and does not appear to perceive the seriousness of her situation. He almost seems to want to 'teach her a lesson' by insisting on her going through with the pregnancy, thereby depriving her of NHS facilities and forcing her into the private sector.

The married woman pregnant by another man may, as Dr W pointed out, cause a great deal of misery and havoc if she has to have her baby. She can be seen as potentially destructive of the family. Can her husband be expected to have another man's baby? Can she have two husbands? To solve the problem, the baby must be removed.

However, it seems a moot point as to whether any of these three women can be described as deranged unless we openly acknowledge 'deranged' to be the equivalent of 'deviant', in which case the girl looking for her second abortion is certainly as deviant as the other two and therefore equally deserving of an abortion. Dr W would seem to be saying in effect that the decision to grant an abortion is based on some compromise between the possible destruction and threat to the norms of family life and the need to punish a woman for her deviation from those norms. Thus if a woman deviates but is sorry for doing so or is obviously suffering as a consequence, she deserves help because she does not want to threaten the *status quo*. If she appears to be determined to continue to deviate, as in the case of needing a second abortion, then she must suffer the consequences in the hope that this will teach her or others a lesson. My research shows that a great deal depends on the woman's ability to be, or to appear to be, pathetic. It turns out to be expensive to approach the doctor in terms of Women's Liberation, demanding abortion as a right.

As I have said, Dr W is the only doctor in my sample who can be described as conservative about abortion. We will now look at the attitudes of the liberal doctors and see how they exhibit similar forms of paternalism, protecting the 'hegemony' of the family as the correct and healthy living unit. Dr V summarises the position well. He said,

> I'm a liberal about abortion. I don't think I should let my morality interfere with medicine... I think young people lack a moral code. They have no standards set. They don't play sport any more. All they are interested in is drugs and sex. The values in life have altered and marriage is no longer so important. Sex should be played down—there are other things in life. I know it's not up to me to do anything about it. My job is to bear the consequences.

Dr V's attitude to sexuality was shared by all but one of the doctors, who said explicitly that she believes it better for young people to have premarital intercourse than to suffer the sorts of frustrations she and her generation

underwent. (But note that even she talks of *pre-marital* intercourse, thus re-affirming the norm of marriage.) What is the meaning then of the doctor's having to 'bear the consequences'? There are two aspects to this particular role of the doctor. One is the taking of responsibility, rather than the woman; the second is the way in which he/she tries to ensure that the consequences of current sexual practice are not destructive to the norms of family life.

Dr Y gave clear expression to her views on the potential destruction of the family as the possible outcome of current sexual behaviour. It is interesting that she is also one of the most efficient doctors at arranging abortions quickly and advocates abortion on demand for women who are less than three months pregnant. Dr Y said,

> I can't understand what these girls want [referring to women students]. I tell them I think they have behaved stupidly. It's not the way our society is run. Sex which is not part of love and the family is dangerous. If people want to destroy the family then they must think hard and seriously about alternatives to put in its place. The family is basic to the stability of our society.

Abortion is here seen as an attempt to shore up the banks against the oncoming flood.

Let us now look at the situations in which doctors say they would be most sympathetic to a woman saying she wanted an abortion and see how they reflect the attitudes described above. Nine situations were described above:
(1) young single girl who had no possibility of getting married; (2) older married woman; (3) married woman not able to cope with another child because she had so many already, or, (4) because her children were very close in age; (5) married woman pregnant by a man other than her husband; (6) mentally retarded woman; (7) married woman with older children who does want to start coping again with babies; (8) woman who seems to go on having illegitimate children; (9) any woman who doesn't want to have a baby.

Reasons (4), (8) and (9) are controversial. One doctor felt that, once a woman was married and had children, she would be able to cope with more. He seemed to think that the fact of natural parenthood gave some surety of a good outcome, which he specifically stated could not be relied on in an adoption situation. Some doctors seem to put the baby's future at top priority, while others are also concerned with punishing the woman for her bad behaviour so that she cannot feel that she has 'got away with it'.

The women towards whom several of the doctors did not feel sympathetic are married women who do not want a baby because it would be inconvenient; engaged couples who could marry, especially if they are in a financial position to do so; single girls who are seen as being careless, particularly when they have already had one abortion; and married women

who say they can't cope. Only one doctor felt sympathetic towards single girls felt to have been careless despite a previous abortion.

Thus, in general, doctors feel that an abortion should be granted to girls who are in no position to get married and to women whose ability to cope with their families would be impaired by having another baby. So those who could marry should do so; those who could have a child within marriage, without undue economic or physical distress or work overload, should have the child. Those who are not married should not have to have children but should appear contrite and not make the same mistake twice. Deference, contrition and pathos are essential.

As we have already noted, the doctors feel responsible for helping the woman to come to a decision whether or not to have an abortion, even though some doctors recognised that most women they saw had already made up their minds to have the pregnancy terminated. The most liberal of them recognised that the present situation in a high abortion rate area (such as the one studied) is virtually 'abortion on demand'. But they did not want the law changed, as it meant that they would no longer be able to have a full discussion with the woman before she had an abortion. It is interesting that they do not consider the possibility of the discussion being more fruitful where the woman, rather than the doctor, has the ultimate right to decide upon the outcome.

Doctors' views on the extent of their responsibility vary from the extreme of seeing themselves as ombudsmen, as described earlier, to the belief that it is their job to give their patients information and impartial advice, but not to 'twist the patient's arm'. Dr M compares a single pregnant girl directly with a man who does not want an operation. He would explain the risks and difficulties involved, but finally it is up to the patient. What he does not see is that the very terms in which he would formulate those risks and problems are ideologically loaded. Other doctors involve themselves in different degrees of responsibility and manipulation while often realising, as one doctor put it, that 'the public wants the doctor to play a certain role' and that 'in effect the doctor is acting as a rubber stamp'.

Sending a patient wanting an abortion to a psychiatrist seems to be a favourite form of exercising medical responsibility. The local gynaecologist will accept the recommendation of two GPs for an abortion and in fact prefer it because it is quicker than waiting for a psychiatrist's appointment. Bypassing the psychiatrist means that the abortion can be performed earlier. Some GPs seem to be unaware of this fact. Others send a woman to a psychiatrist to punish her if she appears to be demanding rather than asking for an abortion, if she doesn't seem sufficiently worried about the situation, if the doctor feels unsure about whether the woman should have the abortion, or if he feels nervous about gaining a reputation as an 'abortion boy'. Here we see the doctor deferring to a higher order of secular priest. Some doctors use the option of sending the woman for a private abortion

as a means of exercising control. One doctor says explicitly that she thinks it is too big a decision for a woman on her own, while another openly admits to his sense of male superiority. 'Women shouldn't have to make this decision. Women should be protected.' Another says that he wouldn't like a practice in the centre of town (i.e. university-dominated) because 'they are all too bloody clever there. They are always asking questions. I have to tell my patients what to do and they must do it or go away.'

A strange sort of game is being played out. On the whole the doctor will not refuse an abortion, provided the patient asks for it rather than demands it. The doctor has to see the patient as contrite—as desirous of maintaining the present forms of social organisation rather than of disrupting them. Thus deviant behaviour is subsumed within present structures. The stray sheep are returned to the fold.

Postscript

Re-reading this essay a decade later, I have been alternately impressed by my political foresight (on the subsequent development of the welfare state and the NHS) and appalled by my naiveté in describing the relationship between the nuclear family and the availability of abortion. It seems to me now nearer the mark to describe the 1967 Abortion Act as giving women the subjective illusion of greater control over their sexuality. While abortion now is far more public and acceptable, women's sense of being the subjects of their own sexuality—rather than the objects of others'—is still far from being realised (see Coward; Cartledge and Ryan). Nor has the relationship between women's sexuality and motherhood become psychologically or socially simpler now that it has become practically easier to separate the two.

The context of the early 1970s was very different. Now the struggle over the NHS has become far more serious, with growing fears that a two-tier system of 'panel' and private medicine may be foisted upon us. At the same time there has been far more significant critical work done on the nature of medical practice, the effects of its hierarchical structure, its domination by men and male technology. An obvious example of this is the research done on childbirth and the growth of a far more militant, albeit rather middle-class, natural childbirth movement (Chalmers and Richards; Oakley).

The 1967 Abortion Act has been sustained despite great opposition from reactionary organisations like SPUC and LIFE. Inevitably the struggle has sharpened; anti-abortion forces have become more visible and coherent, but this has been more than matched by a favourable change in the general climate of opinion. When I wrote this essay in the early 1970s, it was very daring to flypost a few posters with slogans like 'Abortion on Demand'. Now advertisements for abortions are displayed side-by-side with the 'LIFE'

advertisements on the underground trains. Many family planning clinics advertise the availability of the 'morning-after' pill and offer abortions easily. A few hospitals have established model abortion clinics where the staff want to be involved in the work, the bureaucracy is made as simple and non-punitive as possible, the least damaging medical techniques are used and women are offered good supportive counselling.

A liberal legal reform is thus only one part, though a very necessary part, of an ongoing struggle between the oppressed sectors in society and the established power. There is one point in the essay where I refer to Marcuse's concept of 'repressive desublimation' and suggest that abortion law reform could be seen in this light. I think that this conveys a misguided view of the nature of political struggle, as if once we had found the possible reactionary elements in a reform we should jettison it; in reality the struggle over abortion has been a far more active and changing process than my original description suggested. Since women have become more confident about the availability of legal abortion, they have been able to explore the significance and meaning of the abortion experience for themselves. In the pamphlet *Mixed Feelings*, produced by a pro-abortion feminist group, women wrote about the kinds of conflicts they experienced in having abortions (see also Dana). Similarly, in the early days of the National Abortion Campaign (NAC), the issues raised in this essay were seen as diversionary. Recently, the split in NAC and the establishment of the Women's Reproductive Rights Information Campaign (WRRIC) has shown a clear political recognition that abortion cannot be seen only in terms of a one-demand campaign. I see these projects as the link between a more sophisticated analysis of the relationship between abortion and women's oppression and my own rather crude comments on the nuclear family. Of course, our starting point must be the availability of abortion on demand and the recognition of a woman's right to choose if and when she has children. Yet such demands do not deal with the kinds of conflicts which many women experience in a patriarchal society, in which they are imbued, consciously and unconsciously, with the sense that through motherhood they may gain a sense of identity and a pseudo-resolution of their unsatisfied emotional needs; at the same time this society offers them other possibilities and shows them what a low value is in fact placed on a mother's work, both economically and socially. Each woman has to cope with these mixed messages as best she can, within a context of semi-effective contraception and a culture in which she is still a sexual object. Of course she must have abortion available to her but this does not alter the fact that when a doctor 'grants' an abortion he is providing a kind of false resolution to her conflicts. Politically the implications are two-fold; abortion has to be seen as part of a much wider socialist politics and an understanding of unconscious experience has to become a part of that politics too.

Notes

1. Alternatives to the nuclear family have been described in a more sophisticated way in recent feminist writings, such as those by Michèle Barrett, Mary McIntosh, Lynne Segal and others. In addition, psychoanalytic concepts have informed the relevant writings by Dorothy Dinnerstein, Nancy Chodorow, Louise Eichenbaum and Susie Orbach; these writers all argue the psychological and political importance for men playing a far greater role in child-rearing.
2. Since this essay was written, the peculiar relation between technical skills and value-judgements has arisen even more starkly over artificial insemination and test tube babies.
3. In their studies of midwifery and the medical profession, Ehrenreich and English show how the development of health care as a male-dominated profession has led to an increasingly technological approach to medicine.
4. The ideological role of the nuclear family has been analysed by Wilhelm Reich, Hans P. Dreitzel, Joan Busfield, Eli Zaretsky and David Cooper, among others.
5. This paragraph seems strangely dated in a decade of ever-increasing unemployment and austerity. Yet the main points still hold true: women are guided 'back home' as jobs become scarcer, while cutbacks in health and social services all mean more unpaid work for women, as those responsible for running the home. Meanwhile the apologists for these attacks extol the virtues of family life, especially authoritarian child-rearing.
6. Of course in the 1980s this has become a gross understatement, as we see massive cuts in NHS services, alongside the growth of BUPA and private clinics. We may be seeing the end of the NHS as we know it in Britain, and the return of a two-tier system: the 'panel' or 'public' hospital for the poor, and private medicine for anyone who can afford to pay the insurance premiums.
7. The growth of women's self-help groups was catalysed partly by *Our Bodies, Ourselves*, produced by a Boston-based collective and then adapted for Britain (by Phillips and Rakusen).
8. Since the 1970s the radical 'voluntary sector' has grown enormously, especially in cities which have had left Labour-run local councils: this sector has built upon alternatives described here. We now have more established small organisations, ranging from radical GP practices to the Women's Health Information Collective. The natural childbirth movement has grown as well. And in many areas the Community Health Councils have played a more radical role than had been anticipated, both by pushing for better services and by trying to incorporate many of the 'alternative' methods into existing NHS structures.

Organisations

ALRA, Abortion Law Reform Association, 88a Islington High Street, London N1.
Boston Women's Health Book, 465 Mount Auburn Street, Watertown, MA, USA.
NAC, National Abortion Campaign, 47 Waldram Park Road, London SE23.
WRRIC, Women's Reproductive Rights Information Campaign, 52 Featherstone Street, London EC1.

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Science as Alienated Consciousness

ALFRED SOHN-RETHEL

EDITORS' INTRODUCTION (Radical Science Collective)

The development of a radical critique of science and technology should have much to gain from students of the history, philosophy and social relations of science. However, most work in these academic fields reproduces—rather than critically analyses—the existing approaches to understanding the nature of scientific thought and practice. The history of science is dominated by studies which take no account of social and political influences on scientific ideas. The philosophy of science is preoccupied with formal systems and logical issues; the sociology of science with models drawn from the very sciences upon which it is supposed to be shedding new light. Studies in the social relations of science—both current and past—characteristically employ a narrative rather than analytical historical style and a use/abuse model of evaluation. The result is that the academic study of science is almost wholly lacking in a radical, critical dimension.

Marxist studies might be expected to illuminate these questions. However, the attention of marxist scholars has been largely devoted to the historical and human (or social) sciences. Marxist studies in the natural sciences are relatively undeveloped. Entering the literatures of existing approaches is far from easy, and their main examples are seldom tied to particular historical case-studies.

Various marxist tendencies have gone some way to developing critiques of the natural sciences. Lenin, Trotsky and Mao, following Marx, have looked at science's roots in the productive process and tried to analyse the relationship between science and technology and their (future) place in a socialist society. All three were faced with these issues as practical problems: Lenin as a leading and founder member of the Russian Bolshevik Party; Trotsky as Chairman of the Scientific Technical Board of Industry in Russia in the 1920s; and Mao as Chairman of the Chinese Communist Party throughout the period of China's enormous technological advance. Other, more academic, writers leading on from (or away from?) Marx have

seen science as a superstructural reflection of the economic base of a given society (e.g. the Russian historian of science Boris Hessen); or have interpreted science in a wider totality of relations, laying greater stress on symbolic and other theoretical elements (Frankfurt School – Max Horkheimer, Herbert Marcuse, Jürgen Habermas, Alfred Schmidt, Trent Schroyer); or have proposed a new interpretation of Engels' ideas of objectivity which is based on a parallel between classical science and Marx's later 'scientific' work, and which demarcates scientific knowledge sharply from ideology (the French School centred round Louis Althusser).

Within the bourgeois academic tradition there are three bodies of literature which have attempted to draw on the insight that science and society must be seen as importantly related, but without wishing to draw conclusions which would undermine the basic structures of the existing social and political order. Work in the 'sociology of knowledge', which began with Karl Mannheim's softening of views of Max Weber and Georg Lukacs, is coming into greater vogue as a result of the alliance which Berger and Luckmann have forged with phenomenology. Academic functionalist 'sociology of science' has developed primarily in America in the wake of Robert Merton's application of models drawn from biology to the study of science as part of a stable society (organism). There have been significant cross-currents of ideas between the sociology of knowledge, sociology and anthropology. In anthropology itself, an approach has recently emerged under the loose heading of the 'anthropology of knowledge' in which Mary Douglas and Robin Horton have suggested that science should be looked at as a belief system of a particular cosmology, just as other societies' belief systems are examined and interpreted.

All of these individuals and groups have something to say on the relationship between science, technology and society, and a radical critique of the relationships will involve an understanding of their positions. We hope in future issues to provide annotated reading lists on these approaches to science, its history and its social, political and ideological relations, including critical commentaries. This is itself a formidable task, even if the aims are limited to critical exposition.

That the rise of modern science is connected with the rise of capitalism is a thesis that every marxist would wish to support. But the relationship between the *content* of scientific ideas and socio-economic variables is the one which is least clear. The relationship between the rise of capitalism and the rise of science as an *activity* and as a *world view* is vexed enough. It was the subject of a heated debate in the historical periodical *Past & Present* from 1964 onwards, and historians remain vague about it. For example, the marxist historian E.P. Thompson wrote in 1965, 'The exact nature of the relationship between the bourgeois and the scientific revolutions in England is undecided. But they were clearly a good deal more than just good friends' (*Socialist Register*, 1965, p.334). A decade later, no one

seems at all certain about any of the three main levels of the issue: the socio-economic relations of science as an *activity*, as a *world view* and as a growing body of *theories* and *findings*. These are problems for historians and for scientists and technologists.

The consequent questions are many and important. How is 'modern' science to be distinguished from 'non-modern' science, scientific enquiry from other forms of enquiry? What are the relations between science and ideology? Are they sharply distinct? Is science progressively eliminating the domain of ideology? Or is science an expression of ideology? Is there a special form of revolutionary activity for scientists and technologists? Some would argue that science has legitimate claims to objectivity but that it has an ideological role and set of relations as well: that the two aspects are inseparable, just as fact and value are.

Another important and vexed set of questions involves the appropriateness of the concepts of 'base' and 'superstructure' to the analysis of science. Is science a productive force and therefore part of the economic 'base' or does it (also?) in some way *reflect* the economic base and belong to the ideological 'superstructure'? Will socialist scientific knowledge differ qualitatively from capitalist scientific knowledge? Hitherto, science has been carried on as the work of a 'scientific mind' divided from the workers' bodies. What will happen to the scientific head when, as a result of a socialist revolution, the workers' control of production places that head on their own shoulders? Will they be able to do so, and will the head remain the same or will it change as a result? What will science be like when it is carried on as the productive force of the producers, compared with science acting at present as a productive force in the service of capitalist power over the producers?

Looking at these same issues conceptually, is the base-superstructure model, however complexly elaborated in terms of 'mediations' and interactions between base and superstructure, an acceptable theoretical framework for an understanding of modern science and its development? If not, can the model be sufficiently enriched, or must it be totally replaced by some conception of a totality of relationships or something which has not yet occurred to socialists living in a context which so actively discourages seeing the structures which will shape a human world? The most restrictive view of the relationship of science to the wider society is a narrowly determinist one in which all ideas—including scientific ones—are mere ideological reflexes of economic and technical constraints and requirements. If we oppose this with a more comprehensive framework, what degree of autonomy has the superstructure—including scientific thought—from the base? The work of Alfred Sohn-Rethel cannot be readily assigned to any one of the groupings outlined above. Perhaps it could be argued that his early development in Germany was closest to the Frankfurt School, but his academic career was long-delayed by Nazism, since he had to flee to

Britain when his underground work was discovered before the Second World War. He continued to develop his ideas on the relations between mental and manual labour from a marxist perspective, while earning his living as a school teacher. In 1970 he published a book in this area which was immediately pirated by interested student readers who objected to the price of the official edition. He has since been appointed to a professorship at Bremen. George Thomson's comments on his book give some idea of its scope:

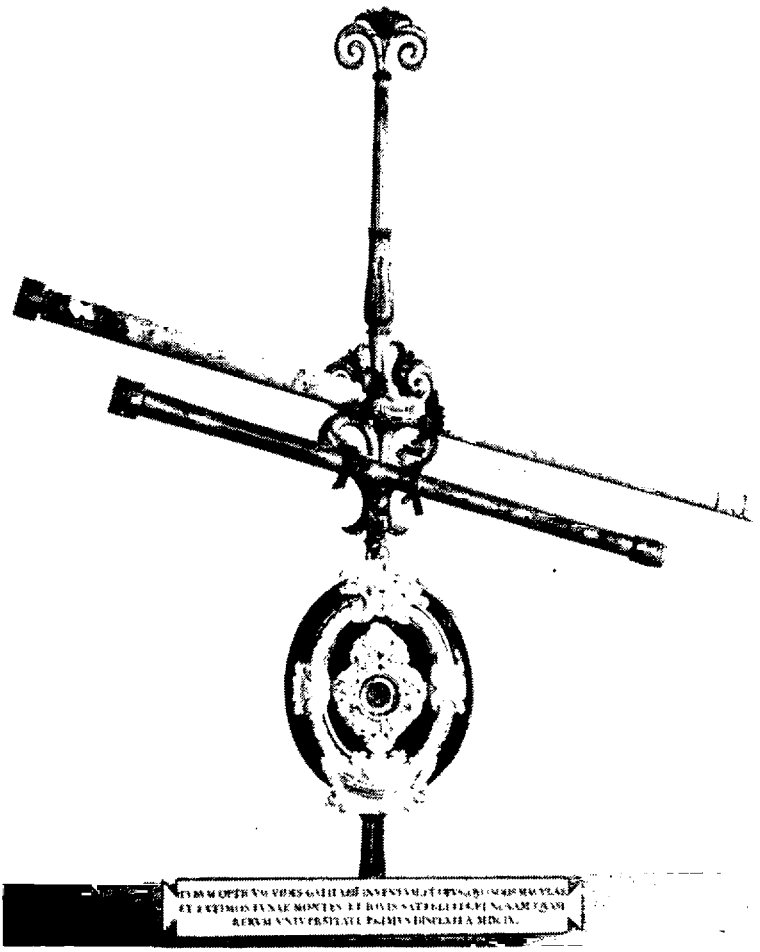
> When studying the economic basis of tragedy... I realised that my conclusions must apply equally to other ideological products of ancient democracy. Accordingly, in the present volume, I have examined the part played by commodity production and the circulation of money in the growth of Greek philosophy. In this I am greatly indebted to Dr Alfred Sohn-Rethel, whose study of Kant has led him independently to similar conclusions, to be published in his *Intellectual and Manual Labour*. Not only has he permitted me to read his book in manuscript, but in discussing my own he has helped me to appreciate the profound philosophical importance of the opening chapters of *Capital* (*The First Philosophers*, 2nd edn., 1961, p.7).

Sohn-Rethel was thus extending his analysis from traditional domains of study of relations between the economic and the cultural to include new domains. In a later study he touched on scientific management and on science itself: 'Mental and Manual Labour in Marxism' (in Walton & Hall, eds., *Situating Marx*, 1972). He was invited to focus on science in a seminar he gave that same year to the Wellcome Unit for the History of Medicine at Cambridge, and he developed it further in a talk given in the series on 'Alienation' at the Institute of Contemporary Arts in 1974. The present paper is drawn from that talk, which the RSJ collective asked him to make available in a publishable form. Members of the collective were very interested in the article but had two sorts of reservations. The first was that its language was difficult for two reasons: first, because the author's native language is German, causing problems of expression which do not arise in conversation; second, because his long familiarity with marxist debates makes inevitable the use of terms which may be unfamiliar to many of our readers. With Sohn-Rethel's permission, members of the collective completely rewrote the article, adding annotations, definitions and further reading. The draft was then sent to him, and his extensive comments, alterations and additional passages have been incorporated into the text. The second reason for our reservations lay in the relationship between his analysis and existing marxist debates on science. We are therefore publishing the article as a first step in a debate which we hope will be catalysed by his strong claims, and we are grateful to him for allowing us to draw him out beyond his own surer ground in order to make a contribution to that debate.

Our own conceptual reservations are of two sorts. At crucial points

in the argument, he suggests that a longer account could be given which traces the relations between economic structures and scientific laws. That is, having pointed out what we consider to be important formal analogies, homologies and parallelisms between commodity formations and scientific conceptions, he implies that a detailed genetic, causal account could also be given. We feel that the demonstration of formal congruences would itself be a significant contribution but that, at the present stage of the debate, the relationship between this and precise explanations of the socio-economic *causes* of a given scientific theory are problematic. Put crudely, we do not yet know where we are between an account which says, 'It was no coincidence that this happened in science while that was happening in capitalism' (Darwin's natural selection theory was developed in the high period of competitive capitalism, for example), and one which says, 'The development of hydraulics was determined by the needs of the mining industry.' The first is illuminating but too broad, while the second is both implausible in this simple form and too narrow. We think it prudent of the author to have refrained from giving *detailed* causal accounts; we are not at all sure that they will have a significant place in a marxist analysis. The point of analysis is to provide a basis for further practice in the field concerned. Thus one has to be able to identify the *major* determinants of a situation so as to be able to intervene effectively.

Our second conceptual reservation comes from the internal history of the change from Aristotelian to Newtonian physics. Sohn-Rethel claims that the Galilean concept of inertial motion is *the* distinguishing conceptual feature of modern science. He argues that this concept evolved as a consequence of the nature of the capitalist mode of production, and argues further that Galilean science has produced objectively valid knowledge of nature that can, however, be correctly understood only in a socialist society. But look carefully at the intellectual history: Galileo, as both a committed Copernican and committed believer in the mechanical philosophy, was compelled by the logic of his position to claim that the earth's circular motion around the sun is natural, i.e. inertial. For to have upheld the Aristotelian belief that the natural place of earthly matter is to be at rest in the centre of the cosmos would have left the Copernican Galileo with the impossible task of accounting for the fact (as he maintained) that the planet earth annually orbits the sun yet with no forces acting on the earth. For Galileo, the mechanical philosopher, could not and did not believe in the existence of occult forces, i.e. forces acting at a distance through the power of 'sympathy' or 'antipathy'. He roundly rebuked Kepler for maintaining such beliefs. Thus, for Galileo, the earth's circular motion had to be inertial: there seemed no other rational choice. (Descartes, however, was to provide an alternative explanation in terms of vortex motion.) Given this situation in the internal logic of Galileo's position, it could be argued that the internal aspect of this episode in the history of ideas about Galileo's reasons



Galilean telescopes

for introducing his concept of inertial motion does not call for an account such as Sohn-Rethel's. In that sense, the internalist would consider his analysis 'unnecessary'. The RSJ collective would go on to say, however, that an account which *appears* not to call for further explanatory factors may *demand* them for marxist reasons, as a condition of replacing a bourgeois academic level of understanding by a political level.

In his comments on our draft of this editorial introduction, Sohn-Rethel takes strong exception to the argument that the internalist account could even *appear* to be complete. He writes,

> Galileo's notion of inertial movement (or Descartes' first explicit statement of it) was not gleaned from nature, not even from astronomy, Copernican or Keplerian, as the movement of the celestial bodies is not rectilinear. In fact, inertial movement is not an object of observation, as Koyré argues forcefully. Thus without "Sohn-Rethel's analysis" the Galilean concept of inertia and Newton's "first law of motion" remain totally inexplicable as to their source and legitimacy. Galileo's inertial motion was in a straight line, conforming with the axioms of Euclidean geometry. It cannot, therefore, be empirically related to, or theoretically derived from, the circular or elliptical inertia of the classical bodies as interpreted by Copernicus or by Kepler. The genetical and logical source of the concept of inertia at the root of classical mechanics still demands an explanation, and I cannot agree that my analysis is or could appear to be unnecessary. Ernst Cassirer's study of "Einstein's Theory of Relativity" (*Substance and Function* and *Einstein's Theory of Relativity*, Dover) contains a cogent argumentation to the effect that the advance from Newton's to Einstein's theory could not have proceeded if the Galilean inertia were based on any kind of physical reality. It is precisely the lack of such reality to which it owes its fundamental methodological importance as the key concept of classical mathematical physics. But even though Cassirer is correct in the way he poses the epistemological question regarding the Galilean concept of inertia, he has no satisfactory answer to it....

as Sohn-Rethel claims to have.

It is obvious from this sharp disagreement on the history of ideas within physics that a more complex discussion is required than the one presented in the following article, if only because our remarks elicited this important futher analysis of the issues as seen from inside the natural philosophy of the period. However, we would like to stress that issues at another level cry out unmistakably for marxist analysis. How are we to explain (1) the commitment to and articulation of the Copernican world system and the several mechanical cosmologies of the time and (2) the subversion of those mechanical philosophies by the rise of a cosmology in which the finite cosmos would become an infinite universe? This much more sweeping change made rectilinear inertial motion reasonable, whereas it had been 'inherently absurd' in a finite cosmos. The concomitant introduction and acceptability of the existence of 'occult forces' acting at a distance, i.e.

gravity, was necessary if inertial motion could no longer be thought of as circular. Such an analysis would, we feel, not only entail consideration of the rise of commodity production – and the 'technical needs' of the emerging bourgeoisie – but also the explicitly stated goal of the 'domination of Nature', the importance of the Hermetic and associated traditions in the Renaissance, the impact of the Reformation, and—last but not least—a consideration of the 'internal logic' of the Copernican/Newtonian revolution.

Other ways of approaching aspects of these crucial developments in the formation of the modern scientific attitude also call for ideological and historical analysis. Surely 'it was no coincidence' that a political theory of possessive individualism, with laws defended as a way of avoiding random collisions of individual pursuers of pleasure and avoiders of pain, developed in a period of interacting atoms guided by mathematical laws in science. Surely it is worth looking at the relationship between the doctrine of primary and secondary qualities in science, closely allied with the banishment of purposive explanations, and the hegemony of commodity relations in the development of capitalism. Similar parallels in the eighteenth, nineteenth and twentieth centuries cry out for analysis of formal congruences of the basic structures of the economic and intellectual formations. Sohn-Rethel has launched what we see as a new, critical attempt to bring such relationships into the domain of marxist analyses of science. Now we must conduct those analyses.

Returning to a point raised above, what about science in a socialist society? What, if anything, would change? In a successful, thorough socialist revolution it is obvious that the mode and social relations of production would change. The priorities of resource allocation would be changed. The hierarchical and elitist and racist and sexist and ageist character of capitalist science would be changed. But the theoretical and technical achievements of existing science would not disappear. Newton's laws and 'Schroedinger's equation' are theories which correctly describe the behaviour of matter in limited domains of applicability and to a certain degree of accuracy. What would, however, certainly undergo change in a life-enhancing society is the overall world view—the cosmology in which Newton's laws, quantum mechanics and other scientific theories find their human interpretation. What perhaps at present appears to be the only possible way of interpreting the significance of these theories would, we feel, come to be seen as a world view that reflects and partly makes 'natural' an alienated, basically joyless mode of being in the world.

However, the main task for revolutionary scientists and technologists is not so much to try to construct a detailed research programme for socialist science—in any case this is not possible until they are firmly socialised in a socialist society—but to engage in activity which will lead to the building of such a society. This activity will take many forms—trade union struggle, fights for democracy in labs, etc.—but an important *part* of it must be a

critical analysis of the determinants and functions of science and technology in our present society. Without such an analysis, our activity may well be mindless and ineffective.

AUTHOR'S INTRODUCTION

Mathematics and the natural sciences are usually considered to be independent of political and ideological determinants. Both have come to be seen as models for rational thought, and the method of science is offered as the only path to relatively objective and trustworthy knowledge. So goes the orthodox view, and indeed some of the most striking examples of the abuse of power in modern history—such as the 'Lysenko affair'—have been concerned with attempts to suppress, delay and distort the findings and theories of science.[1] Yet a central element of marxist thought is that all theoretical constructions are, in the last analysis, determined by social existence and that in every epoch the ideas of the ruling class are the ruling ideas.[2] Marx characteristically applied this analysis to social and political/economic ideas, such as the category of 'labour'.[3] Can the same type of analysis be applied to the ideas of mathematics and natural science?

I believe that the possibility of socialism presupposes the primacy of society over science and that in order to bring about the necessary reorientation of priorities, it is necessary to unmask or 'demystify' the a-historical, timeless and universal claims of mathematics and science. I would go further and say that the special claims of these intellectual formations play an important role in justifying, and hence maintaining, the distinction between mental and manual labour, a crucial distinction in maintaining an authoritarian and hierarchical social order. It therefore becomes an important task for marxists to demonstrate that the *logic* of scientific thinking *originates* in social *history*.

My strategy in this paper is to attempt to do this with arguments drawn from the period in which abstract, intellectual reasoning first appeared in our civilisation—the sixth century BC—and to apply the general position further to a scientific theory which was central to the development of modern science: Galileo's law of inertial motion.[4] In this way I hope to attack the norm of timeless-universal logic at its inception, and in a crucial modern case. In so doing I hope to take all justification out of the beliefs in an intrinsic necessity for the separation of intellectual from manual labour.

My central thesis is that the forms of the social relations of production are the determinants of the forms of thought; in particular that there is a determinate relationship between the mode of scientific reasoning and the commodity exchange abstraction. Only by tracing this historical determination is it at all possible to understand the logic of conceptual and scientific thinking. I should stress at the outset (as I have done elsewhere) that I am not proposing simple parallels between science and the economy, much less

a one-to-one correspondence between the requirements of technology and the findings of science.[5] I am concerned with derivations requiring a much more penetrating and incisive kind of analysis than a mere search for simplistic parallels or for 'explanations' of science as a reflex response to this or that technique and practice of production.

Before beginning my argument, I would like to make two very important disclaimers. It is necessary to make them at an early stage because I find that sympathetic and comradely readers have made interpretations of my work which are at variance with my true theory. In particular, this is so of the members of the *Radical Science Journal* collective who (with my enthusiastic approval) have attempted to 'translate' my lecture and to make it more accessible to English-speaking readers. As a consequence of what I consider to be various misunderstandings and 'mistranslations' of my ideas, I have submitted alternative passages in a number of places and have trusted the RSJ collective to interpolate them into their text. This has produced, in some cases, a less clear presentation, but it is a more faithful one. My remaining disclaimers point in two directions and involve matters of deep debate in marxist critiques of science. In one sense my interpreters would wish to make much more of my theory than I would, and in another they wish to grant much less than I claim. That is, unlike some other socialist analysts of science, I do *not* wish to weaken the distinction between science and ideology and to interpret science *as* ideology. On the other hand, in their editorial headpiece to this article, the RSJ collective compliment me for pointing out what they consider to be 'important formal analogies, homologies and parallelisms between commodity formations and scientific conception' and (in an earlier draft of my paper) refer to 'congruences' between economic abstractions and scientific ones. My reaction to these compliments is to reject them, since I claim that my theory does much more, and extends beyond this level of interpretation.

I will try to illustrate the theoretical issues involved by reproducing a passage from the RSJ draft, followed by my reply. This is not a mere textual quibble: it raises fundamental issues concerning the understanding of the relationship between science and capitalism in the present, and science and socialism in the future. My 'translators' wrote as follows about my argument:

> If the analysis succeeds—or helps others to do so—it will bring the timeless, ahistorical and ideal categories of science into the domain of ideological analysis. The mental and ideological phenomena which Marx discusses in, for example, *The German Ideology*, in terms of alienation, reification, fetishism, false consciousness, etc., and which he shows to be expressions of economic exploitation, private appropriation and class division, could be extended to include the categories of mathematics and natural science.

My reply to the RSJ collective shows our sharp disagreement: 'I do not

place science and the conceptual powers of cognition on a level with ideology. Science has objective validity; it accomplishes tasks of social necessity, although it does this with a false consciousness. The traditional and academic philosophies of science are ideology, not science itself. Mathematics is not ideological, even though it is a phenomenon of consciousness. Marx draws a deep line of division between the ideological forms of consciousness and its capacity to achieve objective knowledge (which I may term its cognitive faculties). Marx never lists science among the ideologies which, in the famous Preface of 1859, he enumerates as "the legal, political, religious, aesthetic or philosophic—in short, ideological forms" (Marx & Engels, *Selected Works* in one vol., Lawrence & Wishart,1970, p.182) or among "superstructural phenomena". This division between science and ideology must not be blurred or put in doubt.'

Turning now to my second disclaimer, I wrote as follows to the RSJ collective about their editorial introduction and certain passages in their 'translation': 'That which I object to more than anything else and what I regard as a definite misrepresentation of the real meaning of my theory is your expressing it in terms of "analogy, congruence, parallelism, etc." which I am said to show to exist in "social being" on the one side and in "consciousness" on the other (to use the marxian expressions here for brevity's sake). If we are agreed—as I think we are—that there cannot be any other expression or representation of pure form abstractions than pure concepts, then it is sufficient to prove the existence of such form abstractions to account for such concepts. There is no "gap" left between the two spheres or ranges of being and thinking, no mere analogy or similarity or parallelism. The crux of the matter lies in proving the independent existence of form abstractions prior to the existence of appropriate concepts. And this is the brunt of my theory: that it proves the existence of pure form abstraction being involved in commodity exchange, and one that is caused by human action, not by human thought, in a way hidden to the people when and where and while they cause it. This is the vital point which, therefore, must be made as cogently and compellingly as possible. Once this crucial point has been established, all that requires further explanation are the circumstances which have caused the hidden form abstractions to have struck people's minds at one date and locality in history rather than another. But this is a matter for historical argument, no longer one of theory and epistemology. Or to put it more accurately, I claim my theory and analysis to be a true example of marxist historical materialism in that the logical and the historical strands of the argument become inseparably linked or—to call it by its technical term— "dialectical".

'If you find my theory mainly "important analogies" and add, "he implies also a causal account", only to go on to say that you consider such causal attempts premature, then, please, allow me to explain to you the cardinal content of my theory. It is to say that I have penetrated the secrets

of conceptual abstraction to the point of tracing its original formation from the realm of thought, where it was hitherto regarded as specifically rooted to the social process. Now, conceptual abstraction can be likened to the workshop of all scientific formations, ancient, modern and contemporary. By showing that it derives (!) from the social process and that there exists a real abstraction operated by human social action (e.g. commodity exchange) which can be proved to be genetically and logically prior to the ideal abstraction operated by thought, one changes epistemology to an extension of historical materialism. This is the basic thing to be needed for laying the ground upon which a 'radical science' and a radical critique of alienated science can be placed. This does not come at the end or in the middle of radical science, but it is the entrance to the whole undertaking. Everything else done in the name of radical science or of its critique is premature, if this foundation of the undertaking is not established in the first place. (If this principle is not clearly brought out in the text of my article, then the publication of it in your Journal misses its purpose.)

'It is on this basis and on this basis only that you can make the structure of society the measure of the form and function of science, that you can follow through the changes in science as a historical process conditioned by definite fundamental changes in the formation of man's social existence in Nature, i.e. that you can obtain an idea of what science amounts to for giving mankind the right or the wrong productive forces in its existential relation to Nature.

'It is also the clue for establishing the social class issue as it applies to science, by looking at the relation of mental to manual labour. This was furthest apart at the beginning of commodity production—which only arose because social production, formerly carried on in common and in the context of what I term a "society of production", broke up into a multiplicity of private producers working independently of one another and thus having to rely on the exchange of their increasingly specialised products as commodities—in order to give to the division of labour the social coherence required to make it possible, thereby starting the whole historical evolution of "societies of appropriation" culminating in capitalism.

'Now, the relation of mental to manual labour also remains a closed book unless you realise that the mental labour divided from manual labour is couched in terms of the social synthesis and of the real abstraction operating in it, in other words, unless you realise that conceptual and scientific thinking is *social* thinking or the *socialised* form of the human mind which becomes indispensable when manual production loses its original social capacity. Thus the changes in the relations of head and hand depend on the changing degree of the individualisation of manual labour. This individualisation was extreme so long as production was carried out as peasant and artisan production—that is to say, until the end of the Middle Ages. Then, from the fifteenth century onwards, production assumed an increasingly

social magnitude and content, which caused its growing dependence on capital and ended up with placing it under capitalist control. While this happened in the economic field, there arose in the mental and political sphere the fusion of the previously divided spheres of the literate and the illiterate classes and between the Church/State and the dependent population, and, closely linked with this, the division of the world into the sublunary and the translunary spheres. While production was single small-scale, the social cast or providers for the social tasks had to stand outside production. But as production became social, it absorbed that separate sphere, and we witness the fusion of them—economically, politically, ideologically— into, among others, the Copernican infinite universe governed by the same laws of physics and mechanics.

'All these changes, which you make a reason for reservations to my theory, this theory really serves to explain. Of course my theory leaves an enormous lot still to be done, but I maintain that it supplies the methodological foundation on which all this work can be done. So long as the theory of science remains the preserve of a separate epistemology, all this work must remain stunted, superficial and groping about in trivial details, without really breaking into the sanctuary of bourgeois intellectual prerogatives. Only the critical liquidation of this entire traditional epistemological reasoning, which I claim to have achieved, creates the possibility of what you are setting out to do in your *Radical Science Journal* and in the books which it is going to start publishing before long, providing it proceeds on a solid theoretical foundation.'

The RSJ collective and I are agreed that the above quotations from my reaction to their 'translation' of this article provide a clear expression of some of the issues which divide us and which are at the centre of attempts to provide a marxist account of science. The RSJ collective also feel that my strong reaction to their attempt to clarify my ideas is itself the clearest expression in English of those ideas. Now, to resume the argument with a text which they have prepared and I have altered in places.

SCIENCE AS ALIENATED CONSCIOUSNESS

As I argued in an earlier paper, if our faculty of mathematical reasoning—or if certain axiomatic principles of modern science—could be accounted for by conclusive formal derivation from elements of the social base of the historical epoch concerned, then idealism would have its foundations cut from under it.[6] If the social bases underwent certain deep-seated changes affecting the relation of labour to the social nexus, the forms and axiomatic principles of science would show significant changes. Indeed, such changes are apparent in twentieth-century developments of relativity theory and quantum mechanics, as compared with Galilean-Newtonian physics. Changes of this kind would also affect that actual or potential relationship between mental and manual work. Thus, a plausible case for the association of

mathematics and natural science with alienated consciousness would be a contribution toward the building of socialism through the demystification of science and by making us comprehend the real historical connection between our social relations of production and our understanding of nature.

Of course, it is not unusual to express doubts about science and scientists as the bearers of, and guides to, a bright future for mankind. My aim, however, is to go deeper than the waning confidence (and even distrust) of science and its abuses. My aim is to consider the nature of science, and to present it as cognition paired with alienation. This approach flies in the face of the traditional interpretation of the relationship between science and social values. The positivist attitude of orthodox science characteristically treats individual facts and theories as independent of the network of evaluative relationships in which they are embedded.[7] That same attitude treats science as a whole as independent of social values. Even the positivist scientist, however, would not deny the significance of the historical settings in which classical Greek philosophy and mathematics on the one hand, and the scientific revolution of the sixteenth and seventeenth centuries, on the other, arose. These two great bursts of intellectual activity were, he would grant, certainly fostered by their respective historical epochs. If he knew a bit about the history of ideas, he would also grant the central influence of Greek mathematical ideas on a crucial feature of the rise of modern science—Galileo Galilei's philosophy of nature. But he would not wish to join the marxists in attaching importance to the relationship between the economic systems of these epochs and the particular form of abstract conceptual reasoning by which they are characterised.

For a marxist, the two historical periods in question are particularly important. During the first, commodity production became fully grown, as indicated by the invention and rapid spread of coinage in the seventh and sixth centuries BC.[8] In the second, of course, the rise of modern science was embedded in the rise of modern capitalism. Both of these stages of social development were of the greatest significance for the history of mankind. If it were possible to establish a direct link between the basic conceptual formations of Greek science and Galilean science on the one hand, and their respective historical epochs and distinctive modes of production on the other, then science would be seen to have a social nature, despite the denials of its positivist exponents and historians.

The objectivity of science demands its neutrality with respect to social issues, and this acceptance of social neutrality is part of the training that every scientist undergoes. Scientific truths are held to be valid regardless of the time and conditions of their genesis and their application. In his professional life the scientist blinkers himself from all of the rest of existence. But is this neutrality really intrinsic to science and conditional to its objectivity? Is it not perhaps a more profound blinkering—to the role played by the scientist and science in the interests of capital? In that case, the very



Frontispicce to Galileo's *Dialogue* of 1632: Aristotle, Ptolemy and Copernicus debate the structure of the universe.

objectivity of science would be an expression of its alienation,[9] denying the scientist a self-awareness of the significance of separating intellectual from manual labour. The critique of science as an alienated form of consciousness would require an examination of the relationship of science to manual labour throughout the stages of the development of science. Put another way, the critique of the separation of science from society parallels the critique of the separation of mental from manual labour, just as their integration is the standard by which to measure the socialist advance towards a classless society.

Whether or not science is alienated consciousness in the sense indicated can only be answered by looking closely at the historical origins of science or, more precisely, by attempting to explain science as a product of the social history of commodity production. By this I mean deriving the conceptual foundation or *the logic* of scientific thought from the social relations of commodity production. In other words, the aim is to do something which the positivist scientist considers absolutely impossible—a point which I want to stress. It is a common tendency of idealistic thinking—for example, in positivism and in traditional thinking (except Hegel)—to treat the logical and the historical aspects of mental forms entirely independently.[10] This tendency characterises a failure to consider the mutual and interpenetrating aspects of the terms of a relationship, i.e. it is the hallmark of undialectical thinking.[11] In positivist and traditional thought, substances and issues interact. In dialectical thought, they do not merely interact but interpenetrate, and the meanings of the respective substances or issues only exist by virtue of the relationships. Meaning is relational. The consequence of undialectical thinking in this instance is the mutual support between separating logic and history on the one hand and mental and manual labour on the other: the historical analysis carried on separately from the logical helps us to continue to consider a class society to be natural and inevitable.

Thus the issue as to whether science is essentially unhistorical, or whether it is an outcome of social history in an epoch of commodity production, is both an important and an open one. I want now to examine one particular concept, one which has played a cardinal role in the foundation of Galilean science: the concept of inertial motion. This is a non-empirical, ideal concept of motion, completely amenable to mathematical treatment, and serving as the cornerstone of the mathematical and experimental method in modern science. On the foundation of this method, science promised to be able to work out the hypothetical laws of nature in accordance with the belief that mathematics was the language of nature. Science also promised to achieve an exact knowledge of nature from sources other than manual labour—in fact, from mental resources totally alien to manual labour but compliant to the needs of capital. In the conceptual foundation of this science, all ties with historical realities are finally severed. Our task is to show the historical foundations of the concept of inertial motion and to demonstrate

that the appeal of the concept—its attractiveness as an abstract, mathematical *kind* of knowledge—lay in those historical foundations.

In order to be quite sure not to load the dice in favour of a marxist critique, I begin with an astute idealist witness for putting the case for inertial motion, Alexandre Koyré, who is one of the most distinguished exponents of the history of science as the internal history of ideas, thus excluding its social relations. I quote from his essay on 'Galileo and the Scientific Revolution of the Seventeenth Century', which is a good summary of his extensive Galilean investigations:

> Modern physics, which ... is born with and in the works of Galileo Galilei, looks upon the law of inertial motion as its basic and fundamental law ... The principle of inertial motion is very simple. It states that a body, left to itself, remains in a state of rest or of motion as long as it is not interfered with by some external force. In other words, a body at rest will remain eternally at rest unless it is "put in motion". And a body in motion will continue to move, and to persist in its rectilinear motion and given speed, as long as nothing prevents it from doing so.
>
> The principle of inertial motion appears to us perfectly clear, plausible, and even "practically" self-evident ... The Galilean concept of motion (as well as that of space) seems to us so "natural" that we even believe we have derived it from experience and observation, though, obviously, nobody has ever encountered an inertial motion for the simple reason that such a motion is utterly and absolutely impossible (given the fact that such motion would at least presuppose motion in a vacuum, through absolutely empty space). We are equally well accustomed to the mathematical approach to nature, so well that we are not aware of the boldness of Galileo's statement that "the book of nature is written in geometrical characters", any more than we are conscious of the paradoxical daring of his decision to treat mechanics as mathematics—that is, to substitute for the real, experienced world a world of geometry made real, and to explain the real by the impossible.
>
> In modern science motion is considered as purely geometrical translation from one point to another. Motion, therefore, in no way affects the body which is endowed with it; to be in motion or to be at rest does not make any difference to, or produce a change in, the body in motion or at rest. The body, as such, is utterly indifferent to both. Consequently, we are unable to ascribe motion to a determined body considered in itself. A body is only in motion in its relation to some other body, which we assume to be at rest. We can, therefore, ascribe it to the one or to the other of the two bodies, ad lib. All motion is relative. Just as it does not affect the body which is endowed with it, the motion of a body in no way interferes with other movements that it may execute at the same time. Thus a body may be endowed with any number of motions, which combine to produce a result according to purely geometrical rules; and, vice versa, every given motion can be decomposed, according to the same rules, into any number of component ones....
>
> Thus, in order to appear evident, the principle of inertial motion presupposes (a) the possibility of isolating a given body from all its physical environment,

(b) the conception of space which identifies it with the homogeneous, infinite space of Euclidean geometry, and (c) a conception of movement—and of rest—which considers them as states and places on the same ontological level (meaning that rest is, as it were, only a particular modality of movement). Commonsense, indeed, is—and it always was—mediaeval and Aristotelian.[12]

Thus, according to Koyré, Galileo's notion of inertial motion is not gleaned from experience. Consequently, it is on a totally different plane from the ideas of his predecessors, who could not tear themselves away from Aristotle and from the basic assumptions and analogies of the Aristotelian tradition which were based on artisan production first carried on by slave-labour and in the Middle Ages by serf or free labour, but in both cases considered in terms of use-value. The idea of inertial motion with which Galileo broke new ground was a non-empirical concept which offered the singular advantage of having the element of motion in common with innumerable phenomena of nature, and of being at the same time a mathematical concept which could be treated like a piece of Euclidean geometry. In fact, it opened the door through which mathematics could establish itself as an instrument of the analysis of given phenomena of movement and yield a mathematical hypothesis which could then be tested experimentally. In what follows I shall attempt to explain the concept of inertial motion from outside scientific thought, from something which is not yet science and is not even thought.

Societies of Production and Societies of Appropriation

My explanation is marxist in that the forms of thought are not taken into separate consideration as a province all their own. The forms of consciousness are regarded as an integral part of social existence and as determined by the material environment and economic necessities of their epoch. The forms of scientific consciousness should not be treated as an exception to this approach. We can clear our historical field by making a broad distinction which will help us to gauge the place which science occupies in history. The distinction is between societies of *production* and societies of *appropriation*.[13] It distinguishes societies according to the functional synthesis by which they hold together and form a viable whole. I refer to this overall form of social synthesis as a society's *nexus*. In societies of production the nexus is rooted in the productive activities of the people, in the communal way in which they carry on production. Whether the people work as a collective or in separate groups or singly, they know what the others are doing. They act in concert according to custom, a council of elders (who may, of course, control the power structure), or some such set of institutions found routinely in comparatively small social units. What we find on the basis of such 'communal modes of production' (as Marx calls them) is that their practice is rational, i.e. makes sense to outsiders, but their theory is irrational

(looks bizarre to others). They have a firm control over their social life process and usually regulate it to the smallest detail, but in their judgement of the events of nature they bring to bear the standards of their own rules of conduct.

If we make a big jump forward in social development from the Stone or Bronze Ages to an advanced stage of the Iron Age, however, we find the producers equipped with metal implements: spades, sickles, hoes and ploughshares on the land, and hammers, saws, pincers, files, etc. in the artisan workshops. The producers in possession of such productive forces can take the best advantage of them by working independently of each other. They thereby become private producers, evolving an increasing amount of specialisation of trades. Of course the greater the development of the division of labour, the greater the mutual interdependence.[14] But it is a very different sort of interdependence from the collective or communal one discussed above. Social relations come to be determined *by* the relations between the products, whereas before they were determinant of production. The division of labour and the commodity relationship take over from and dominate the relations between people. The producers and owners of individual products enter into a network of private exchanges. In this interplay of exchanges, the means of production and the products become private possessions which are traded as commodities. It makes little difference to the formal rules of the exchange relationship—taken singly—whether the exchange agents exchange products of their own or have obtained them in other ways. In the process of exchange, the products of labour count as values differing in quantity only. The values must be agreed by their owners as being equal in order for the change of possessions to count as 'exchange'. The property of both owners—counted in terms of value—must come out of the deal unimpaired.

So instead of societies of production, we now have societies of appropriation, in which production is for the market and in which the nexus of society results from activities of exchange, i.e. mutual relationships of private appropriation. *In this situation we find the previous relation of theory and practice reversed.* The social practice is irrational in that people have lost control over the system in which they live. As commodity production and the division of labour continue to develop, the irrationality of practice increases further, while the mode of thinking of the people assumes the conceptual form which we normally associate with rationality. In Greek antiquity, for the first time in history, commodity production reached the stage of a monetary economy, which Engels classes as 'commodity production fully grown' and as the start of 'the age of civilisation'. In the conceptual mode of thinking then developing, nature is conceived as an object world independent of man for the first time.

The Origins of Abstract Thought and Science: The Commodity Exchange Abstraction

This combination of irrational social practice and rational theory of nature is peculiar to commodity production. It has persisted—with important political and social modifications—throughout various developmental stages and is still with us. Does it leave us any hope of combining rational practice with rational theory? If so, what would become of science as we know it today? The term 'science' represents the tradition of rationally controlled thought and knowledge of the object world from Greek antiquity onwards. It also represents a clear-cut division of intellectual labour from manual labour. This division was not characteristic of the communal modes of production preceding civilisation; and if we could bring into being a society which combined rational socio-economic practice and rational theory of nature, the fundamental separation of head and hand could not persist. But let us look at the structural features and historical origins of the vile mess we are now in, even as we glimpse at a better future. The emergence of reasoning in conceptual terms around 500 BC has sometimes been called 'the Greek miracle'. And it is to this historical and logical phenomenon that we must turn, seeking an explanation for it on the basis of the anarchic social process of commodity production. But can we show that it is an alienated form of consciousness? It certainly possesses the idea of 'the truth', but has the idea of 'truth' itself emerged in history in possession of *false* consciousness, and is that consciousness *necessarily* false?

We are now approaching the heart of the argument, and I find myself falling naturally into technical marxist language. To help with definitions and to provide a summary of the argument to come—a sketch map—I offer the following summary of my position, succinctly put by George Thomson, on whose own arguments I shall draw below. The concept of 'pure reason' was formulated in classical Greece as a reflection of the relations of a monetary economy.

> In this way, man, the subject, learnt to abstract himself from the external world, the object, and see it for the first time as a natural process determined by its own laws, independent of his will; yet by the same act of abstraction he nursed in himself the illusion that his new categories of thought were endowed with an immanent validity independent of the social and historical conditions which had created them. This is the "socially-necessary false consciousness" which on the one hand, has provided the epistemological foundation of modern science right down to our own day, and, on the other, has prevented philosophers from recognising the limitations which are inherent in their "autonomy of reason" in virtue of its origin as the ideological reflex of commodity production.

Thomson continues on the topic of 'the illusions of the epoch':

> It is characteristic of the ruling class of each epoch of class society to regard the established social order as a product, not of history, but of nature. This

> is what Marx and Engels called "the illusion of the epoch". It corresponds to the concept of "socially-necessary false consciousness", and it follows from the marxist principle that "it is not the consciousness of men that determines their being, but, on the contrary, it is their social being that determines their consciousness"... These "illusions" are inevitably reflected in the philosophical and scientific theories of the ruling class. The world of nature and of man is interpreted on the basis of certain assumptions which are accepted without question as absolute truths, although in fact they are historically determined by the position of the given class in the given epoch.[15]

Turning now from the summary to the argument itself, what is the origin of the peculiar abstraction which lies at the basis of thinking in terms of timeless universals of the sort we find in Greek philosophy and in Galilean and Newtonian science, in particular that of inertial motion? My answer to this question is a marxist one: I do not look at the mind and the nature of thinking as the primary source of this abstraction but at the social process of commodity production. Long before it becomes a theoretical concept, this abstraction is operative in the exchange process of society. It originates in human action, not thought. It may be unusual to speak of 'abstraction' as a spatio-temporal reality, but this is not loose talk or a mere metaphor, any more than 'abstract art' is. Marx uses the expressions 'commodity abstraction' and 'value abstraction' quite deliberately and with great care in his analysis of the commodity in the opening chapters of *Capital*. The process of exchange is the source of the abstraction which permeates commodity-producing societies, and its influence extends to the formation of intellectual categories. The following sentence from Marx is a model for the process I wish to explore: 'The social exchange process gives to the commodity which it transforms to money, not its value but its specific value form.'

Use vs. Exchange

This distinction—between the value of a commodity, i.e. the magnitude of its value, and the 'specific value form'—was first made by Marx and is of cardinal importance to my argument. The magnitude of value and its determination concerns economics, while the form of value relates to the abstraction operating in exchange and hence to the abstract mode of thinking arising from it. I argue that, when exchange relations in society reach a stage where the creation of coined money is called for— which first happened early in the seventh century BC in Ionia—the abstractness of exchange begins to impose itself upon people's mode of thinking in a way initiated by Greek philosophy. This, put in the briefest way, is my historical and logical explanation of the birth of philosophy in the Greek society based on slave-labour, and of the birth of modern science in European society based on wage-labour. To substantiate this view, three points have to be

established: (a) that commodity exchange is an original source of abstraction; (b) that this abstraction contains the formal elements essential for the cognitive faculty of conceptual thinking; (c) that the real abstraction operating in exchange engenders the ideal abstraction basic to Greek philosophy and to modern science.

On the first point, commodity exchange is abstract because it excludes use. During the time that a commodity is subject to a transaction of exchange, it must remain exempt from use. But while exchange banishes use from the actions of people, it does not banish it from their minds. The minds of the exchanging agents must be occupied with the purposes which prompt them to perform their deal of exchange. Therefore, while it is necessary that their action of exchange should be abstract from use, there is also a necessity that their minds should not be. The action alone is abstract. The abstractness of their action of exchange will, as a consequence, escape the minds of the people performing it. In exchange, the action is social, the minds are private. Thus, the action and the thinking of the people part company in exchange and go different ways. In pursuing point (b) of our theses we shall take the way of the action of exchange, while for point (c) we shall turn to the thinking of the commodity owners and of their philosophical spokesmen.

The abstract character of the exchange process is due to the fact that while it is an action carried out with useful things, it is itself concerned with the non-use of them. The non-use has a positive reality. If it did not, commodity exchange could not occur: there would be no commodities, only useful things. The action of exchange occupies a definite and measurable duration of time, the time required by the exchange transaction. The exchange can only take place thanks to this absence of use lasting through time. It can be short or long, but we know with certainty that the period of the exchange was a period of non-use. It is equally certain that commodities were thought to be objects of use, or nobody would have bothered to exchange them (and confidence tricksters would be out of business). The banishment of use during the exchange is entirely independent of what the specific use may be and can be kept in the private minds of the exchanging agents. (Buyers and sellers of sodium chlorate might have gardening in mind or bomb-making.)

This somewhat tricky and tedious analysis is simply an attempt to penetrate the construction of the economic category of 'value', a category which always relates to use as a precondition but blots it out for the purpose of the act of exchange. Students of natural science are likely to be unfamiliar with the formal features of the concept of value and to suspect that I am making heavy weather of the obvious. I would make two points in reply. First, it is worth recalling that long and strange analyses are familiar in, say, topology, atomic physics, cosmology or endocrine physiology. Second, I am building rather carefully toward the crucial conclusion of my argument.

To return to the concept of 'value': it is a purely practical abstraction, really nothing more than an escape out of the contradictions between exchange and use—the transformation of quality for use into quantity for exchange. I am not at all concerned with questions in the academic discipline of economics—with how this quantity is determined for the exchange relations of existing commodities. I am trying to lay bare the features of the formal quality of 'value' as the category we require in the practice of exchange. My concern is to make absolutely certain that the abstraction it involves is original and primary, and is spatio-temporal at the same time that its antecedents are not already in the minds of people engaged in exchange. It can then be treated as a 'real abstraction' of primary social and historical origin, and I shall soon show its relationship to scientific abstractions.

The reason for exchange and use being separate activities is that they belong to totally different reference frameworks. Exchange is a transaction of purely social significance which affects its objects only in their *social status* as property of private owners changing possessions. In order that this social change can obey its appropriate rules, the assumption must be made that the *physical status* of the commodities remains unchanged while the social action is in progress. You can easily convince yourself of this when looking in a shop window at a garment or other object of use marked with a definite price. The object stands or lies there in the fictional state of remaining in the same immutable physical state during the whole time that the price does not alter. Not only is it not to be touched by an unauthorised person but even nature itself is supposed to be at a standstill in the body of that commodity and, as it were, holding her breath for the sake of this social business of man. (It is to the fact that nature does not hold her breath that we owe many of the bargains of 'shop-worn' articles and foods whose 'shelf life' is coming to an end.) This experience is apt to draw our attention to the important fact that the contradiction between exchange and use extends even further than into man's physical activities and includes spontaneous events of physical reality. The abstractness of exchange —or, more precisely, of the action of exchange—is an example of what we class in conceptual language as 'non-empirical'. It is a feature of this—as with other non-empirical categories—that the abstraction of exchange can bear no trace of any distinguishing mark by which one historical moment and locality can differ from another. The exchange abstraction is characterised by complete indifference to time and place, although it has itself the reality of an occurrence in historical time and place.

Bourgeois Science

And now to come to the heart of my argument. While the act of exchange casts this spell upon the physical status of the commodities and any material change in them, it is itself, nevertheless, a physical act dealing with the

commodities as material things, e.g. by moving them physically through time and space from one owner to another. (Sometimes the owner moves, as into a newly bought home.) This context contains the complete structure of the exchange abstraction. We have only to put it under a magnifying glass and analyse it in all its details to obtain the epistemological foundation of science in the ages of commodity production.

The real abstraction of exchange comprises the complete set of non-empirical form elements which, when translated into correct concepts, or rather, when correctly identified, provides the theoretical foundation of scientific thinking, both for ancient Greek philosophy and for modern science on its emergence in the sixteenth and seventeenth centuries. Our main concern is with the latter, which developed in close historical association with modern capitalism and should by rights—as well as on logical grounds, as will be shown presently—be classed as bourgeois science.

First, however, let me stress the fact that the purely formal nature of exchange is the same for all acts of exchange throughout the epochs of commodity production. This is also stated by Marx:

> No matter how long may be the series of periodical reproductions and antecedent accumulations through which the capital now functioning may have passed, it always retains its primal virginity. As long as the laws of exchange are upheld in every act of exchange, individually considered, the method of appropriation [of the surplus] may be completely revolutionised without in the least affecting the property right applying to the production of commodities. This same right remains in force no matter whether things be as they were at the beginning when the product belonged to the producer, and when the latter, exchanging equivalent for equivalent, could enrich himself in no other way than by his own labour; or whether things be as they are in the capitalist period when to an ever increasing extent social wealth becomes the property of those who are in a position to appropriate, again and again, the unpaid labour of others.[16]

This invariable form of exchange provides the chainwork of the social nexus. It also acts as the epistemological foundation of science as rational theory. I say 'acts', for this epistemological foundation is not itself as something existing in an ahistorical world of ideas where the theory of knowledge dwells; it is a collection of historical events, each occurring in space and time, as real as the actions of use which they rule out. It is only by virtue of their comparable reality that use and exchange exclude one another in time and space and that the whole abstraction occurs. It is for this reason, the banishment of use and the replacement of it by a new reality, that in the German language the system of exchange is sometimes spoken of as a 'second nature' (*eine zweite Natur*)—a nature of purely social reality and significance and of exclusively human understanding.

From the point of view of commodity production our entire social network is made of this 'second nature'. I soon become aware of the dividing line between the 'first' and the 'second' nature if I take my dog with me

to the butcher's. How does he analyse the proceedings? He understands a great deal and seems to have very definite ideas of property, judging from the way he snarls at people whom he suspects to have designs on the meat he is allowed to hold in his mouth, ready to run home. But when I have to shout at him: 'Wait! I haven't paid for it yet!' his analysis and mine part company. He knows these bits of paper and metal: he has seen them before. He knows they are not for eating and that they carry my scent or the butcher's, but their meaning as money is not part of his world of nature. Of what does this meaning consist? First of all, its meaning has no existence anywhere but in our human minds, although it is not of a mental origin. Its nearest empirical embodiment is money—coins and notes. But is the paper or the metal an integral part of it? Which metal? Silver, copper, this alloy or that or the legendary gold? Neither these metals nor the paper of the promissory notes possesses the absolute physical immutability which the exchange abstraction demands. The issuing bank has to make up for the imperfections of the 'first nature' on the standards of the 'second' by pledging gratuitous replacement of coins and notes that have suffered wear and tear from their circulation. Money must be made of matter; mere thought money would not be accepted for payment. But it should not consist of any empirical stuff; it must be real but non-descript. Because such a substance does not exist, we have to make out with aids from the first nature as second-best. (The word 'substance', incidentally, was not chosen at random. I want to return to it later.)[17]

The whole of this physical act of property transfer through exchange calls for close scrutiny. Each of its features has a constitutive role to play in the formation and historical genesis of the conceptual mode of thinking which we call the 'scientific' view of nature. For example, in this transfer of commodities separated from use, *time* and *space* take on a different aspect: they become *continuous and homogeneous extensions.* In a relatively short essay I can only assert this and the other formal features of the commodity exchange abstraction. The detailed analysis is published in my book on *'Geistige und körperliche Arbeit'* (*Intellectual and Manual Labour*). The immutable and non-descript nature of the commodity requires us to probe into its materiality. This aspect is its *substance,* while the properties of its use-value adhere to it as its varying *accidents.* There would also be the question of the *form of exchangeability* of the commodities between owners. In their relationships of exchange they assume a solipsist attitude: only their own private thoughts on use-value have meaning and reality to them. The relationship of ownership is also mutually exclusive in each of their minds. There is also *a principle of pure mathematical reasoning* involved in commodity exchange. The equivalence of values in the exchange calls for changes in the *mode of quantity* when goods of different dimensional measurements are involved. Their mode of *absolute* quantity seems definable only by saying that one commodity is greater or smaller than, or equal to,

another. The abstract quantity emanating from the postulate of equivalence is no longer an entity which one could measure but an *abstractum* allowing for nothing but pure thought. There are also features of *atomicity* and of strict *functional causality* involved. To make my argument completely convincing, it would be necessary to spell out the analysis of each of these formal features of the commodity exchange abstraction and their relationship with scientific reasoning.

Application to Law of Inertial Motion

I have chosen the concept of inertial motion in the Galilean method of modern science as a test case for the historical account of a specific theoretical instrument of science. For this purpose, I shall take the following description of the commodity movement as it forms part of the exchange abstraction, and indeed sums it up: *The commodities describe a pure linear movement through abstract—that is, empty, continuous and homogeneous—space and time as abstract substances which thereby suffer no material change and which are capable of none but quantitative differentiation.* The affinity of this description to the Galilean concept of inertial motion is so close that it is impossible to brush it aside and ignore its consequences. The most weighty of these is the non-empirical character of the pattern as a whole.

However, there appear to be discrepancies as well. Most important among these is the lack of a reason why the commodities should, in their capacity as abstract substances which are not subject to material change, continue to move indefinitely. The crucial analogy to inertial motion seems not to keep moving for long, and if it comes to a halt, so does my argument. Let us take money as an example and see if we can keep it in motion. At the butcher's my money moved no further than to his till. It is perfectly true that, in all cases of what Marx calls 'simple commodity exchange', money does not move in a continuous and endless way. Indeed, if it is used for hoarding it might end its course at rest in a stocking or in Silas Marner's iron pot. But in focussing on these particular cases, we miss the general case, just as a physicist would fail to understand inertial motion if he saw only objects which were subject to friction or gravity. If we think of money as endowed with the function of capital, the movement of my coins will not end in the butcher's or any other salesman's till. In a capitalist economy money must move continuously or it will not retain the substance of its nondescript value. In capitalism money will not cease to move until the final end of money itself. Recalling our general historical framework, we should remember that the rise of modern science coincided with the rise of modern capitalism, and it is surely appropriate to associate Galilean dynamics—which was central to the specific features of seventeenth-century mechanics—with the triumph of capitalism. The power of movement emanating from this new science permeated that epoch—even into painting,

architecture and music.

But why should the circulation of capital be associated with the observance of movement in a straight line and at even speed? Surely this *is* an interpolation of Galilean invention but one that is fully justified since irregularities of the movement could only spring from the world of use which is banished from exchange. The assumption that the movement be in a straight line and of even speed has important methodological implications in yielding a concept of inertial motion fully amenable to mathematisation. By purifying the concept of all heterogeneous factors, inertial motion assumes the importance of being the absolute minimum of what constitutes a natural event—a movement which takes place while all of physical nature stands spellbound and still. And this minimal event has the quality of a pure conceptual thought, since it is not truly an event in real nature but a mathematical abstraction. It was in this capacity of elementary or minimal form of movement that Galileo used his concept of inertial motion as the basic means for constructing mathematical composites in regard to observable phenomena of movement—for example, the ballistics of cannon balls, which he proved to be nearest to parabolas.

I very much hope that the terms in which the concept of inertial motion have been presented compellingly recall those which characterised the suspension of the rest of nature during the commodity exchange process. But even with all this similarity and even structural identity 'in the bag', there remains the important difference that while the commodity exchange abstraction refers to a social reality in time and space, concerned with action and expressing the form of the economics of commodity production, Galileo's inertial motion is a purely intellectual abstraction concerned with a logical ideal and aimed at the understanding of nature. How is it possible to bridge this gap between being and thinking and between such discrepancies of reference?

The link through which the commodity exchange abstraction is conveyed to the thinking mind is coinage. I want to relate the social being of monetary economy to what I said earlier about 'substance' when I was discussing the abstract or non-descript yet real matter from which money ought to be made. It was perfectly clear then that the insistence on the correct description of 'money-matter' would have led to a pure concept—that concept of 'substance'—as the only adequate designation of 'money-matter'. The first to arrive at a near identification of this abstract matter was the Greek philosopher Parmenides (about 520 BC) in his concept of 'the One' and 'that which is', that which alone has reality and constitutes the real substratum to all changing sense appearances. In later philosophy the concept of 'substance' was adopted instead and described by Kant, for instance, as the remaining element (*das Beharrliche*) in all appearances, one which neither increases nor diminishes in quantity. As to economists, commodities are composites of exchange-value and use-value; thus to philosophers all

objects of cognition are composites of substance and accidents (sense-qualities).

None of the philosophers using this or other of the abstractions due to exchange claims to have himself formed these concepts. Parmenides tells us how he had his concept revealed to him by the goddess Dike; Plato relates the ideas to a pre-existence of ours before birth; Kant to a transcendental synthesis *a priori*. These conceptual identifications of the non-empirical exchange abstraction and its elements are invading our consciousness ready-made. They are abstract before we know of them, products of our actions but hidden from our minds. Marx makes the same point when he says that the abstract constitution of value, into which (as he puts it) 'not an atom of nature enters', can never find an identical representation. This is because, within the network of exchange, it only expresses itself by the irrational equation with the use-value of another commodity. The tie-up of money with one or other use-value on the one hand, and one or other metal as a substrate for its function on the other, is thus inescapable but only within the market.[18]

Apart from the market, however, there is another realm in which the value or exchange abstraction finds an identical expression: the domain of the conceptual reasoning of the pure intellect. The real abstraction of exchange or value is one of pure form or of a whole interconnected bundle of pure form abstractions. And the only existing representation of the realm of pure form is that of pure concept. Thus, as to the bridging of the gap between being and thinking, our explanation points to the occurrence of a non-empirical abstraction in social being giving rise to abstract thought. But it was no easy task to work out the conceptual representations of the form abstractions contained in exchange. It took the Greek philosophers from Thales to Aristotle three hundred years of astounding intellectual effort to become fully conscious of these abstractions, to formulate them, to probe into their uses and implications, to recognise their antithetic contrast to the world of the senses and yet their intrinsic reference to it, etc. What spurred these thinkers on was not solely 'philosophy', the love of wisdom, but the urgency of the class struggles brought about by the spread of monetary economies. But let us now return to the alienated consciousness involved in Galilean science.

Science as Alienated Consciousness

In considering science throughout the ages of commodity production, we have argued that the rational theory with regard to nature which science represents has its roots in an irrational social practice. That is, it grew from the same soil as did commodity exchange. Conceptual reasoning—including scientific reasoning—is an offshoot of 'second nature'. The logic of conceptual thought, being based on commodity exchange, is a logic of

appropriation, not a logic of production. A logic of production can only be the logic of the producers themselves—either individual producers like peasants or artisans, or collective producers of a great variety of possible groupings. A real and human 'logic of production' would also have to be founded on a reintegration of head and hand, a reunion of mental and manual labour. The sciences of commodity-producing societies are, on the contrary, rooted in intellectual activities which are divorced from manual labour: class societies have class-bound knowledge and theory of knowledge (epistemology). The resulting knowledge is framed in terms of basic concepts which are ones of nature in the abstract. By this I mean those abstract forms of time and space, of matter, of movement and of quantity involved in the physical activities of exchange. Thus, what causes this action to be abstract is its purely social character, but that which is abstracted in it are form characteristics of Nature fundamental to all natural phenomena of motion.

I described earlier the abstract pattern of the motion carried out by the act of exchange as the minimum of what constitutes a natural event. It also constitutes that which amounts to the inescapable necessity to which any empirical event of motion is subjected. Therefore, as the set of non-empirical abstractions contained in that pattern conveys itself to conscious attention and assumes the shape of concepts, the human mind is endowed with the capacity of probing into that which is necessary in the events of Nature and into judging what is possible or impossible in Nature. And this capacity does not accrue to the mind by empirical observation and sense perception or productive practical experience of labour. It accrues to it by virtue of the genetic origin of the cast of its conceptual form, made of *a priori*, ready-made, non-empirical elements. These result, ultimately, from man's natural history having taken the course of a social mode of existence due to his producing his means of livelihood by his own labour as the prime difference of man from animal.

Now, if anything approaches the miraculous in the history of the human race, it is this reasoning capacity on what is possible or impossible in Nature by the aid of mathematical logic. It also constitutes the absolutely enigmatic feature in man's cognitive capacity which has forever puzzled the philosophers and has forever eluded their attempts at explanation. For although the activities of commodity exchange are completely confined to social interaction and are nowhere related to material processes of use and to our material interchanges with nature, they are nevertheless the formal elements of nature, even if it is nature in the abstract. Thus by thinking in these terms, the scientist is in possession of concepts which, by representting nature in the abstract, are applicable to the concrete observable phenomena of nature, provided a suitable method is found of achieving this application.

Galilean science did establish such a method and was able to produce 'objectively valid' knowledge of nature. It accomplished its socially

necessary function, but it did so with a false consciousness, or rather, in ignorance of its real identity. The real nature of its abstract intellect (or, as Locke termed it, its 'faculty of the pure understanding'[19]) is that by virtue of its historical formation it represents a mode of thinking of an *a priori* social capacity. The form of thinking which is common to Galilean science and commodity exchange plays an important role in maintaining a given set of social and economic relations. It has an ideological or 'socially synthetic' function. The form of social synthesis prevailing in any epoch depends on the dominant structure of production or, more precisely, on the form of the labour process of production, which, in turn, depends on the stage of development of the productive forces. Thinking in terms of the exchange abstraction is thinking in terms of the social synthesis. In other words, it is social thinking—thinking for and on behalf of society. Moreover, it is thinking which is directly or indirectly trained upon nature and which dervies from it a socially necessary character—necessary, that is, for social survival.

From this observation I derive the general proposition in the theory of knowledge that the socially necessary forms of thinking in any epoch are those corresponding to the socially synthetic functions operative in that epoch. This is, of course, only a generalisation based on the particular historical instance of modern classical Galilean/Newtonian physics and of capitalism in its classical condition of unimpeded competition. The proposition can be no more than a hypothesis, albeit a well-founded one, applicable to epochs of different social formations, including our present one of monopoly capitalism.[20]

Thus the abstract conceptual reasoning characteristic of the epochs of commodity production presupposes the social synthesis peculiar to what was earlier termed 'societies of appropriation'. These arise from an atomisation or individualisation of manual labour. 'Articles of utility', writes Marx, 'become commodities only because they are products of the labour of private individuals working independently of each other.' And again, '...labour—productive value—always remains the labour of the individual, only expressed in general terms. Hence productive labour—as labour productive of value—in relation to capital, always presents itself as labour of a separate worker, whatever social combinations these workers may enter into in the process of production.'[21] The 'general terms' which the labour of the individual worker assumes by producing commodity values is the formal character of the 'second nature' of which the conceptual mode of thinking—including scientific thinking—is an offshoot. Of course, the individual is as unaware of the social character of this form of thinking as he is of the genesis and root-cause of his 'pure intellect'. The social character of these supposedly universal conceptions is only apparent in a veiled way—in the very 'universality' or 'universal validity' of this mode of thinking. That is, the 'correct' thought of the individual is not only thought

for himself but, *ipso facto*, for 'everyone'.

Modern Science and Modern Capitalism

However, it is precisely this *a priori* (in principle, not derived from experience) social character of the abstract mode of thought which helps us to understand the historical coincidence of the rise of modern science and the rise of capitalism. Throughout classical antiquity and the Middle Ages the predominant form of production was the individual and small-scale peasant and artisan. It was based on a personal unity of idea and execution, of plan and execution, of head and hand on the part of the producer. It did not call for or offer scope for the application of science within production. But a change in this situation was gradually brought about during the commercial revolution which began in the late Middle Ages and widened and deepened throughout the following three or four centuries (ca. 1200–1600 AD).

This is a fascinating period of history, one which also saw the invention of gunpowder and firearms and a complete revolution in the techniques of warfare. Over a vast range of activities these developments engendered tasks which the limited economic, technical and theoretical resources of the craftsman became less and less capable of solving. Economically, the artisan producers came to rely increasingly on (and thereby came increasingly under the control of) merchant capitalists. In the technical field we find a minority of craftsmen developing into 'experimenting masters', artists and architects. This occurred in large-scale tasks such as the building of town halls, fortifications, public monuments and cathedrals; the regulation of rivers and planning of water supplies; the casting of heavier and heavier guns; the growing demands on mining and on transportation. These 'craftsmen of scale' drew for advice on the knowledge of scholars—especially mathematicians—and brought about a completely novel symbiosis between practical tasks and mathematical analysis and theory. Artisans like Brunelleschi consulted with scholars and mathematicians like Toscanelli. Toscanelli had never linked his mathematics with any manual tasks, Brunelleschi could not link his manual tasks with an appropriate mathematics. We learn from Toscanelli's letters how both thrilled at the result of the collaboration. Other names tell the same story—those of Ghiberti, Donatello and Alberti, of Giorgio Martini, of Leonardo and of Dürer, followed by a string of such consulting mathematicians as Tartaglia and Benedetti or Cardano and Ferrari, Stevin and finally Galileo himself.

The evolution of modern science can be metaphorically described as the bringing into the scope of a single mind the mathematical and artisan aspects. But just as the mediaeval gap between scholar and producer is being bridged, an even wider one is opening up between the scientist who devises the productive forces for a social mass of proletarian labour in capitalist

employment.[22] In the mathematical and experimental method the role of the bodily organs—hands and senses—is reduced to the reading of measuring instruments that translate the evidence of the individual producer into the mathematics of the socialised mind and its timeless-universal logic. The scientist may work with his hands to make a piece of apparatus, although technicians are as likely to do so, while the fabricators of the scientists' instruments are very unlikely to have much idea about their design or function. (Indeed, they are unlikely to have the requisite education, and in 'advanced' countries in the Common Market they are likely to be foreign workers who speak little of their 'host' country's language.)

Returning to the Renaissance, the predecessors of Galileo were prevented from finding a successful technique by their inability to wrench themselves free from empirical conceptions of movement through space. Galileo's purely mathematical concept of inertial motion placed the analysis of natural phenomena of movement on the same level as abstract social thought, not dependent on experience. Corresponding exactly to this revolutionary turn of Galilean thought was the step which the capitalists took by taking over the workshop and the means of production of the artisans. By this seizure the capitalist became the 'producer' and (by an even more ironic misnomer) 'manufacturer', while the real producers now 'worked' for him as wage-labourers. With this momentous change of the relations of production, responsibility for the process of production shifted from the artisan-craftsman to the capitalist. The latter, being usually unskilled, could only fulfil this responsibility by means of his money. He used it as capital to purchase in the market the complete array of materials and services which, brought together under his command and according to his directives, made up the totality of the production process.

Since the capitalist has no productive function to contribute to the process with his own hands, the process must—from the standpoint of the capitalists' ideology—be a self-acting affair. The production relations of capitalism are represented by the capitalist and presented to the worker as inevitable, inescapable, automatic and natural (law-like) processes. This appearance of determinism and automaticity is not a consequence of the technological nature of the machinery as such; rather, it is part of the ideology of capitalism, which represents particular social relations (and their histories frozen in the form of machines) as natural laws.[23] The theory of classical mechanics is in perfect keeping with this appearance of automaticity and inevitability of the process of production and is often appealed to as an analogy for capitalist relationships. Once again, this is not because it is in the nature of mechanical processes but because of the apparently spontaneous and automatic nature of the inertial motion used in constructing them.

Despite this close link between the control of production by capital and the epistemological basis of classical mechanics, capitalist production was not ready to make full use of science until the second half of the nineteenth

century. Before responsibility for production could lie wholly in the control of capital, all aspects of artisan production had to be eliminated. Only then did this responsibility come to rely fully on science as a means for developing the technology and organisation of the production process. By then capitalism was nearing the end of its free-market regime and approaching the turn to monopolism, the governing characteristic of which is the need to manipulate the market regulators of commodity production. This must, in turn, lead to changes in the social synthesis. Concomitantly, it has evolved new forms of physics.

We have now reached a point in our story where a whole new chapter requires to be written. Its elements parallel those in Greek antiquity, the transition to modern science and capitalism, and the formal identity of Galilean inertial motion with classical capitalism. But the new conceptions are monopoly capitalism, the mathematical treatment of the 'time' and 'motion' of the labour process and the materials of production. A detailed analysis (which I have embarked upon elsewhere and which is closely allied with Mike Hales' argument in *RSJ* 1)[24] would stress the interconnections between 'scientific management', conveyor belt factories and the new management sciences and the formal features of quantum mechanics and molecular biology. But I have ventured far enough into domains of science which are the province of historical and philosophical specialists.

It is said that scholars turn to the past with a zeal that implies that the present is not available for analysis. My aim has been to offer a mode of analysis which has centred on a period which is sufficiently distant in time to allow useful perspective, but sufficiently allied to our own situation to suggest fruitful ways of attacking the present mode of alienated thought and being which allies science with the continuing commodity form. By their continued subjection to the standards of capital, these scientific conceptions are adulterated so as to perpetuate the division of head and hand. Correctly understood in ways which transcend the current synthesis of science and the commodity relations of capitalism, they could contribute perspectives which would aid the social unity of mental and manual labour, reuniting men with one another and with themselves.

Notes

These notes were added by the editors to Sohn-Rethel's *RSJ* 2/3 (1975) essay as a guide to further reading. See also his *Intellectual and Manual Labour: A Critique of Epistemology*, Macmillan, 1978.

1. Giorgio de Santillana, *The Crime of Galileo*, Univ. of Chicago, 1955. Zhores A. Medvedev, *The Rise and Fall of T.D. Lysenko*, Columbia, 1969, Anchor paperback.
2. Karl Marx and Frederick Engels, *The German Ideology* (1845-6), Lawrence & Wishart, 1964, pp.61-2; Karl Marx, *A Contribution to the Critique of Political*

Economy (1859), Lawrence & Wishart, 1971, pp.20-21.

3. 'This example of labour shows strikingly how even the most abstract categories—despite their validity, precisely because of their abstractness—for all epochs, are nevertheless, in the specific character of this abstraction, themselves likewise a product of historical relations, and possess their full validity only for and within those relations.' Karl Marx, *Grundrisse: Foundations of the Critique of Political Economy (Rough Draft)* (1857-8), Pelican, 1973, p.105. For a comprehensive introduction to Marx's thought. see Bertell Ollman, *Alienation: Marx's Conception of Man in Capitalist Society*, Cambridge, 1971.
4. For historical and philosophical assessments of Galileo's role in the development of modern science, see Stephen F. Mason, *A History of the Sciences*, revised edn., Collier, 1962, Ch.14; Edwin A. Burtt, *The Metaphysical Foundations of Modern Physical Science*, 2nd edn., Routledge, 1932; also Anchor paperback, especially Ch.3; Alfred N. Whitehead, *Science and the Modern World*, Cambridge, 1925; also Fontana and Mentor paperback, especially Ch.3.
5. Alfred Sohn-Rethel, 'Intellectual and Manual Labour: An Attempt at a Materialistic Theory', *Radical Philosophy* 6 (Winter, 1973), 30-37, at pp.36-7, notes 6, 7; also 'Mental and Manual Labour in Marxism', in Paul Walton and Stuart Hall, eds, *Situating Marx*, Human Context Books, 1972, pp.44-71. In rewriting the present essay, the RSJ collective have drawn on and adapted passages from these two earlier articles in order to increase the accessibility of the argument.
6. 'Mental and Manual Labour in Marxism', pp.46, 50-52.
7. For a history and analysis of positivism, see Leszek Kolakowski, *Positivist Philosophy: from Hume to the Vienna Circle*, 1966, Pelican, 1972, especially Chs.1,8. (USA edition, *The Alienation of Reason.* Anchor, 1968). For a discussion of the contrast between the British analytic tradition and marxism, see Charles Taylor, 'Marxism and Empiricism', in Bernard Williams and A. Montifiore, eds., *British Analytical Philosophy*, Routledge, 1966, Ch.10.
8. See Frederick Engels, *The Origin of the Family, Private Property and the State* (1884), Lawrence & Wishart, 1972. George Thomson, *The First Philosophers*, Lawrence & Wishart, 1978.
9. For a brief discussion of this concept, see Ernest Mandel and George Novack, 'The Marxist Theory of Alienation', Pathfinder Press pamphlet, 1970.
10. For a fuller discussion of this point, see 'Mental and Manual Labour in Marxism', pp.47-8 (note 5).
11. G.W.F. Hegel may evoke such a misleading impression by making dialectics into a system of logic, which he applies as a fixed pattern to various provinces of reality and of thought, such as history, law, aesthetics, religion and even nature. Treating dialectics this way is—even in the case of Hegel's probings into the depths of it—nevertheless the most absolute of all possible versions of idealism, Indeed, Hegel himself conceived of it *as* 'absolute idealism'. Only materialistic dialectics is in keeping with itself. It must be gained by countless individual quests of critique of non-dialectical thought. This is a matter of the class struggle over every issue and against the bourgeois thinking of the declared defenders (as well as the unwitting upholders) of class-divided society (author's note). The issues involved in Hegel, dialectics, their relations with marxism

and the issue of applying dialectics to nature are complex and very controversial. A shortlist: A.V. Miller, *Hegel's Philosophy of Nature*, Oxford, 1970; Herbert Marcuse, *Reason and Revolution: Hegel and the Rise of Social Theory*, 2nd edn., Routledge, 1955; Peter Sedgwick, 'Natural Science and Human Theory: a Critique of Herbert Marcuse', *Socialist Register 1966*, pp.163-92; Alfred Schmidt, *The Concept of Nature in Marx*, New Left Books, 1971; Jeff Coulter, 'Marxism and the Engels Paradox', *Socialist Register 1971*, pp.129-56.

12. *Philosophical Review* 52 (1943), p.337. See also Koyré, *Newtonian Studies*, Univ. of Chicago 1965; and 'Galileo and Plato', in Philip P. Wiener and A. Noland, eds., *Roots of Scientific Thought*, Basic Books, 1957, pp.147-75.
13. See also 'Mental and Manual Labour in Marxism', pp.56-7. For extended discussions of the issues raised in this and the following sections, the classical sources are *Grundrisse* and *Capital*. For a discussion of the notion of rationality in economic anthropology, see Maurice Godelier, *Rationality and Irrationality in Economics*, New Left Books, 1972. The relations between kinship structures in primitive societies and class structures in class societies are discussed in Emmanuel Terray, *Marxism and 'Primitive' Societies*, Monthly Review, 1972.
14. On the relations between the division of labour and interdependence, see Emile Durkheim, *The Division of Labour in Society* (1893), Free Press, 1964. Durkheim's theory is that of classical bourgeois sociology. On its relationship with Marx's approach, see the lucid exposition by Steven Lukes, 'Alienation and Anomie', in Peter Laslett and W.G. Runciman, eds., *Philosophy, Politics and Society, Series 3*, Blackwell, 1969, pp.134-56.
15. Thomson, *The First Philosophers*, pp.341-2; see also rest of Ch.16.
16. Marx, *Capital*, vol.1, 3rd edn., Lawrence and Wishart, p.550.
17. See below and Marx, *Grundrisse*, 'Chapter on Money', for fuller discussions.
18. For a discussion of the importance of realising the material nature of the function performed by money, see Jonathan Friedman, 'The Place of Fetishism and the Problem of Materialist Interpretations', *Critique of Anthropology* 1 (Spring 1974), 26-62.
19. John Locke, *An Essay Concerning Human Understanding* (1690), Everyman, 1961. Locke's work is the primary philosophical source of the British empiricist tradition in philosophy and philosophy of science. See also Richard I. Aaron, *John Locke*, 2nd edn., Oxford, 1963; and Gerd Buchdahl, *Metaphysics and the Philosophy of Science: The Classical Origins Descartes to Kant*, Blackwell, 1969, especially Ch.IV: 'Locke: Narrowing the Limits of Scientific Knowledge'.
20. See Paul A. Baran and Paul M. Sweezy, *Monopoly Capital*, Monthly Review Press, 1966.
21. Karl Marx, *Theories of Surplus Value*, edited by G.A. Bonner and E. Burns, London, 1951, p.179.
22. This passage enhanced from 'Mental and Manual Labour in Marxism', p.59. A discussion of the social background to Renaissance ideas and the appearance of intellectual rationality in various cultural forms is provided by Peter Burke in *Tradition and Innovation in Renaissance Italy*, Fontana, 1972. See also Alfred von Martin, *Sociology of the Renaissance* (1932) also Harper Torchbook.
23. See David Dickson, *Alternative Technology and the Politics of Technical Change*, Fontana, 1974, especially pp.44 ff. and 'Technology and the

Construction of Social Reality', *Radical Science Journal* 1 (1974), 29-50. For further views on marxism, science and the history of science, see Ernst Bloch, 'The Marxist Concept of Science', in his *On Karl Marx,* Herder & Herder, 1971, pp.141-52; Paul Piccone, 'Towards a Socio-Historical Interpretation of the Scientific Revolution', *Telos* 1 (Spring 1968), pp.16-26.

24. Mike Hales, 'Management Science and the Second Industrial Revolution', *Radical Science Journal* 1 (1974), 5-28; Alfred Sohn-Rethel, 'The Dual Economics of Transition', *Bulletin of the Conference of Socialist Economists* 2 (April 1972), 36-65, reprinted in *The Labour Process and Class Strategies,* Stage 1, 1976, pp.26-45.

'Life' in a Biology Lab

SIMON PICKVANCE

In capitalism there's a lie put across about individual success. One of the functions it serves is to break the collective strength of workers; another is to increase competitiveness. Both are intended to serve the purposes of production. This lie about the possibility of individual achievement is put across in science. Science is about creating individuals who know things, normally pretty meaningless things in themselves—hence the rewards for single 'innovators' in each individual field. Because we're all scientists here, and we're accustomed to competition for individual enlightenment, you expect that kind of approach from me. Now when people got in touch with me to ask me to give a talk, I think they needed someone to 'do the biological sciences'. I think probably many of you could 'do the biological sciences' as well as I can, so I'm hoping that we may be able to encourage long contributions from the floor rather than try to cut them. What I think they represent, those long contributions which have been made at previous sessions at this conference, is people's resistance to the position of somebody on the platform.

I'm going to make four points which I hope will provoke some discussion. Firstly, I'm going to argue that existing critiques of biology are fragmentary ones. Secondly, I'm going to look at the relations of production in biology labs. In a third section, I'm going to treat biological knowledge as a commodity in capitalism. In the last section I hope to move towards the idea that we cannot create a socialist biology without socialising the means of scientific production. In connection with this last point, I had hoped that Ian Gibson (a former union executive member of the

Editor's Note:
Simon Pickvance's essay (as well as Luke Hodgkin's) comes from a talk given at a conference organised by the British Society for Social Responsibility in Science (BSSRS) in January 1975 on the theme, 'Is There a Socialist Science?'. Pickvance's talk developed out of his experiences working at the Laboratory of Molecular Biology (LMB) in Cambridge. The Postscript comes from a letter he wrote to Edward Yoxen, then a fellow post-graduate student in Cambridge, who has since written extensively on molecular biology and biotechnology (see updated Bibliography).

Association of Scientific, Technical and Managerial Staff) would have spoken by this time. I can't cover much of his ground but I feel that scientific trades-unionism is an issue relevant to the question of the ideology of capitalist science.

Piecemeal Critiques of Biology

Both from the scientific establishment and from Marxists we have critiques of science as it is now practised. In coffee time discussion in the biology lab where I was working last year, from scientists who were 'unpolitical', there was criticism of Jensen and Eysenck as 'bad scientists'; Konrad Lorenz, Desmond Morris, and Robert Ardrey were also 'bad scientists' because of methodological errors they made. Barry Commoner would be dismissed as a careerist, and Linus Pauling's most recent work on Vitamin C and health would be due to his senility. Velikovsky would be called a pseudoscientist. Lab scientists protect themselves from the social implications of science and technology by making every instance an aberration. Just as for BSSRS a few years ago, science is 'abused'. Science is too liberal an institution, according to lab scientists, to be free of 'bad scientists'; the bulk of scientific practice, however, remains good.

There is a similar piecemeal critique from the Left. Radical scientists still conceive of science as consisting of some 'political' islands in a sea of relatively harmless research. So they often take part in union activities but not in critiques of their labour. For example I mentioned to several biologists what I wanted to talk about here today. One asked me if I would 'do' race, culture and intelligence. Another told me that he discriminated between socially active and socially neutral areas of science. He accepted that all science is interpenetrated with or by ideology, but he saw only certain areas actually used by capitalism for their content. When a neurobiologist (someone who works on the functioning of the nervous system) heard that a friend and I thought that neurobiology as a whole should now be recast in a socialist image, he said that he could see the ideology of Patrick Wall's analysis in the BBC series 'Are Hierarchies Necessary?' (In this, Patrick Wall describes the use of the concept of hierarchical organisation in neurobiology and suggests that it owes its popularity to the hierarchical organisation of society.) But this neurobiologist could see no general critique beyond that. Even to Bernal, embryology had been funded (and had had particular emphasis laid on particular areas) because of the needs of a developing capitalist society, but to him the everyday practice of the embryologist is free of such implications, at least from my reading of *Science in History*.

I want to show that piecemeal analysis is not enough. Under capitalism all science is largely determined by the relations of capitalist production; there is no safe place to hide.

Here are some of the fragments of a critique of biology which we must try to draw together. I'm listing them partly because a year ago I was in the situation of not knowing of the existence of critiques of biology, and I guess some people are in the same position now. Also others may be able to add to this list. Arthur Galston, a botanist, has written on the ideology of a piece of plant hormone research. In the field of pharmacology there is the excellent work of 'Marxists in Medicine' on the development of drug companies and pharmacology in capitalism, 'Take a Pill'. From genetics, one would want to include Beckwith and Green's work on the alleged inheritability of criminal behaviour in the XYY syndrome, and there is the whole radical input into the race, culture and intelligence debate. In molecular biology there is the Michaelson article on Sickle Cell Anaemia, and a *Science or Society?* article on the political background to Watson and Crick's discovery (the 'double helix' model of an important biological molecule found in all cells). And in ecology there is the work of people like the New Alchemy Institute, the Soil Research Association and Chinese socialists in new agricultural and horticultural methods *(Peking Review)*. These are the kinds of approaches which I think should be unified. We should be looking for something which fills in or gets rid of the spaces between these islands of ideological critique. To achieve this I want to look at the practice of science.

Laboratory Production Relations

When I started working in the Laboratory of Molecular Biology in Cambridge, I had a number of ideas about what it was going to be like, the ideas we are all socialised into and hence all share when we leave university. We expected to do our own experiments. We would be working with animals and plants rather than writing pointless essays and doing meaningless experiments and working at a distance from them as we had done as undergraduates. We would be among sympathetic colleagues, doing a job which we expected to be fulfilling at last. (There had been that continual distancing of the time when we would be doing what we wanted.)

When we are postgraduates we learn what science is like. All too easily we forget or rationalise the departures from these prior images. What actually happens is that the scientist's work is determined for him or her, if not by an overzealous supervisor, then by a current of thought. ('The next experiment in this area will be such and such' is the kind of thing one often hears.) This is becoming increasingly the case as research groups get larger and as the determination of goals from either outside science or high up within scientific management—as that determination of goals occurs within the state—gets stronger. The scientist works long hours, often determined by the life cycle of the organism he works on or the time available on a computer. He is separated from his home life and from the overall fulfilment

which the postgraduate had looked forward to. (I'm sorry, I'm using 'his' where I mean 'his' or 'her'—it's a reflection of the strong sex bias both in science in general and among the salaried scientists in our lab in particular.)

In the Laboratory of Molecular Biology there were students working on sequences of sub-units in proteins involved in a metabolic pathway in an organism the choice of which meant very little to anybody. Or students might be working on a cancer cure which was meaningful only in terms of the intrigue of grant-winning. In other words, the student is a long way from the direct relation to his experimental organism, and to a problem, which he or she might have expected.

And of course the scientist is in battle with his or her colleagues. In our lab this was most notable among post-doctoral fellows, on whom the economic pressure was greatest. They wouldn't be able to sit in the same coffee room with one another; there were 'collaborations' which never took place; they couldn't talk to one another; there were shadows who reappeared in the lab at times when the men who had sacked them weren't there. It strikes me that the life of a graduate student, and of the no-longer-so-aware post-doctoral scientist, is very similar to that of any other industrial worker; they experience the contradictions of comradeship and competition in the lab, the sacrifice of home life to work, the alienation from the process of production and from the final product.

This is no mere formal resemblance; nor is it restricted to individual consciousness. In collective awareness the similarity exists too. In a series of struggles in which scientists in Cambridge, Edinburgh, Swansea and Southampton and elsewhere have been involved—over technical wage scales, job evaluation, safety, and postgraduate teaching—the division of labour in labs has been clear. There are research managers and research workers.

There is now, for example, a category of tenure for post-doctoral scientist, agreed between the AUT (Association of University Teachers) and the Medical Research Council, which specifically bars the recipient from control over the research he or she carries out. Mike Cooley's and Andre Gorz's descriptions of speed-up in the labs are relevant now. For example Jonathan Cooke was describing today how, in the Mill Hill Medical Research Council labs, he had been asked to fill in a form saying how long he expected certain tasks of research life to take; like a production line worker, his work was being quantified so that it could be controlled. And the health and safety struggles now being waged in labs are the first of many, as research managers force greater and greater sacrifices from their workers on limited budgets.

Post-doctoral students, graduate students and technicians share interests that they do not share with their bosses, who exact surplus value from them. Of course there are intermediate positions in research management, occupied probably by many of the people in the audience now, in which material

interests are not always clear, and in which conflicts between political attitudes and material position can be tolerated. We are all in more or less contradictory positions right down to the lab sub-proletariat, the cleaners of lab floors. The class analysis of science—like that of industry—is complex, not simply binary, and is constantly changing. For example, research management is becoming highly centralised. This increasingly unites the interests of local research management and workers as economic pressure on science is intensified.

Biological Knowledge as Commodity

A biologist is a worker; his product is a commodity for use and exchange, like a piece of plastic or a carburettor. By seeing molecular biology in this way, much of its current practice in labs and industrial conflicts in these labs can be integrated. In molecular biology amino-acid and nucleic acid sequences (these are the orders of sub-units of the large molecules that compose living cells) have a known exchange value for money, for degrees or for reputation. Most people are probably working on them mainly for one or more of these reasons, though the only reason a friend could find when asked why he had worked on his particular sequence was 'because it was there'.

Choosing a particular method of analysis is a social choice, in which we select a method whose success can be assessed for rewards; that is, it has exchange value. It must also serve a purpose, either ideological or directly, in production. We should ask—and it's a question which seems as yet to be unanswered—why, in particular, questions of information in molecular biology and questions of synthesis and structure are the ones which determine the structure of the subject. Molecular biology is not exempt from ideological analysis as some, including Steven Rose earlier in the conference, would have us believe.

But the problem is, in the complex mediations of political ideology by science, how to work out what these chosen areas of interest really represent. It's not sufficient to say, as for example is accepted by many molecular biologists, that information is such a large element in molecular biology's way of explaining things because information scientists came into molecular biology after the Second World War. This is not a sufficient account. We must look for example at the connections between the use of information in explanation in molecular biology and the role of inherited capacities in the 'Race, Culture and Intelligence' debate. That's the kind of gap we have to bridge, the complexity of the mediations when we analyse ideological use.

Biology also has a direct productive use, as increasingly the tools of research are determined by industry and the state. Increasingly *directly*, I should say, as they have always been so determined. Ageing and cancer have become key concerns of biology, for example, but to whom are these

studies most important? To the rich nations and the rich of those nations. And analytical biochemistry as a whole has use for drug industries, dominated by the profit motive.

So in our critique of capitalist biology we should understand that all science in which capitalist production relations prevail is an embodiment of those production relations. The division of labour, the alienation of workers, and the domination of humans by one another and over other living things: all are common elements in the development of Western science. We haven't rooted out these faults from our critiques; nor do we see them as general characteristics of existing science. We have the notion of separable critiques, for example, a critique of neuropharmacology and a critique of neurophysiology. (Neuropharmacology is about the chemicals of the nervous system and their manipulation by drugs; neurophysiology is about the organisation of systems above the level of chemicals—cells and tissues.)

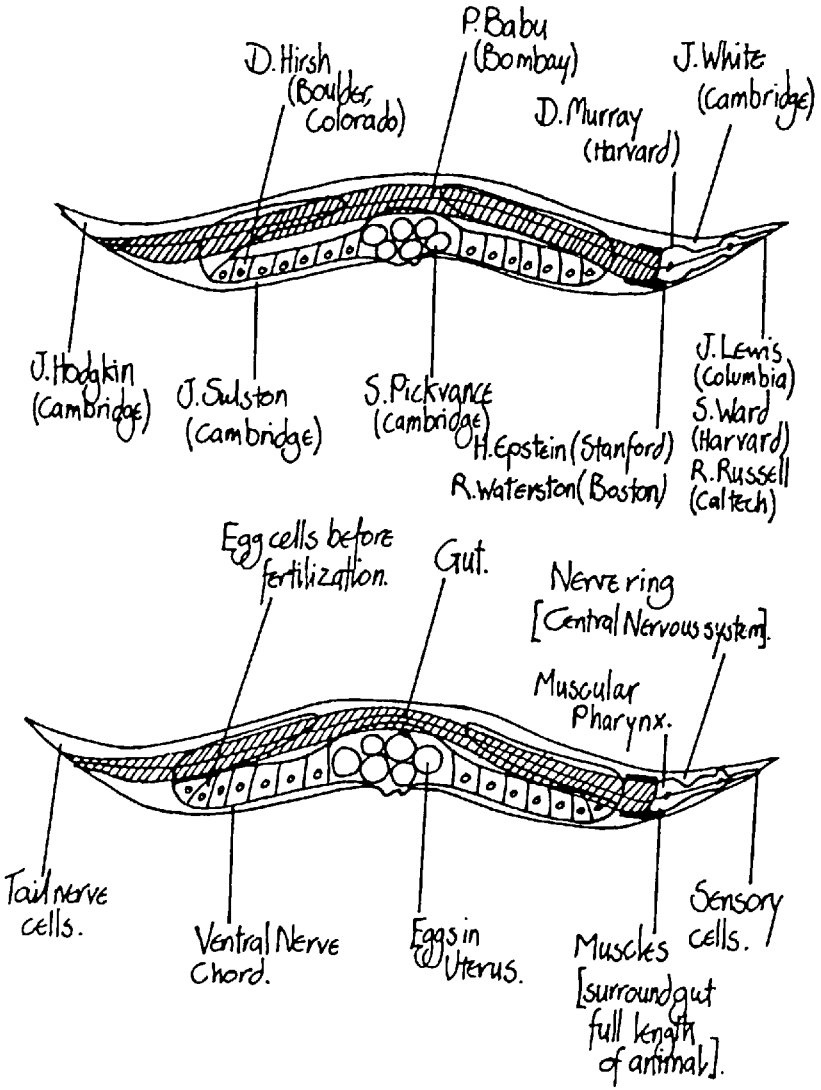
We have these separate critiques because, in the drawing of lines between subjects, we have the division of labour at work. We have people, as Mike Cooley has accurately described, who are identified by the machine they tend or by the fragment of perception they take to themselves. When we mount ideological critiques, we are still not critical enough of the embodiment by science of the division of labour.

In the critiques of reductive biochemistry and ethology from a Marxist perspective, we have a critique of the alienation of man from his sensuous nature. Molecular biology says that man is nothing but the behaviour of his molecules. Certain molecular biologists are in print as saying just that, and ethology says that man is just an animal. (See Young, 1969.) The system of production relations in capitalism is actually used to explain the economy of the cell (Rose and Rose), the equilibrium of the ecological or physiological system, the mechanism of evolution (Young, 1971).

At another level we should look in the critique of domination for the mediations of sexual domination in society. What the Women and Science Collective said about the 'machismo' element in science was very clearly demonstrated in the lab in which I worked: a connection between male sexual prowess and success as a research worker in the lab. Certain approaches, for example what have been described as the nurturative as against the penetrative approaches to science, are mediations again of domination of women by men or humans by other humans.

Division of Labour

Now I'll give an example from my own work, where I think I may be able to make the relations a bit clearer. I was working on an animal which had been chosen as research subject at a time when career pressure in molecular biology was getting strong and molecular biologists were moving into new areas. In brain sciences, both in artificial intelligence (which is a field in


P. Babu (Bombay)
D. Hirsh (Boulder, Colorado)
J. White (Cambridge)
D. Murray (Harvard)
J. Hodgkin (Cambridge)
J. Sulston (Cambridge)
S. Pickvance (Cambridge)
H. Epstein (Stanford)
R. Waterston (Boston)
J. Lewis (Columbia)
S. Ward (Harvard)
R. Russell (Caltech)
Egg cells before fertilization.
Gut.
Nerve ring [Central Nervous system].
Muscular Pharynx.
Tail nerve cells.
Ventral Nerve Chord.
Eggs in Uterus.
Muscles [surround gut full length of animal].
Sensory cells.

which computer analogies to mental processes are worked out) and in biology there was an optimism that the repetition of certain simple processes would 'explain' brain function.

Two molecular biologists in my lab chose a small nematode worm (nematodes are a group of mainly parasitic worms, like the roundworm of pig and human guts). This animal had a number of advantages for this kind of study. It took little labour to maintain; that is obviously economical in technical effort. It interfaced well with machines, particularly with the computer and microscope. Its development was of a kind which lent itself to digital analysis—to a form of analysis which owes its roots to computer modelling. I'm very pleased that the question of simulation came up in Luke Hodgkin's talk (reprinted here), as it's a very powerful method of analysis in biology.

In the lab the worm was subdivided. For example, the gut was given to one worker to work on, the head to another, the tail to another, the muscles to another. By the time I arrived, there were a few portions left. I was given the eggs to work on. And over the whole structure there presided one man who knew about the connections between these different parts. And that's what is called team research. In my fragment of the animal I was expected to take the conceptual apparatus of molecular biology to look at the development of the cells of this animal and in particular to how they were derived.

Now what this means is to abstract from the total development of this animal in an environment, and in an environment affected by humans, and to abstract from that system, one particular element, one particular way of explaining that development. I was expected to look at its cell-lineage. But the very notion of explanation—accounting for a phenomenon in terms of a few variables—is totally irrelevant to a socialist biology. What we need to know is how in our total social interactions with that animal we can resolve our contradictions with it—a course of action closer to co-operation than domination. That would be my image of socialist biology.

I was expected to look at its lineage, because lineage is the mode of explanation of development which serves best as use and exchange value in molecular biology. I was expected not only to abstract the lineage as explanation, that is, the derivation of all cells (like a family tree, for example, as explanation), but also to abstract particular events and particular parts of the animal as a whole. The process of development is a continuum and I was expected to look at particular parts of it. There are processes with long names, like blastulation, gastrulation and so on which normally form chapter headings in books, because they normally involve different sets of workers.

Now I was expected to concentrate my attention on one part. That process is the division of labour at work when one looks at one organism. This process of fragmentation reflects the division of labour in our research group and in embryology as a system of production. It also reflects the reductionist

viewpoint with its ideological and productive uses. A certain system of rewards has been laid down for particular approaches to biological problems. The research commodity embodies these approaches which embody and are mediated by the relations of production in capitalism.

A Socialist Biology?

Now I want to go back to individual and collective consciousness in the lab. We must ask, 'Can socialist science be created except with socialised relations of production?' Our struggle to develop a socialist biology must go hand in hand with our struggle for the means of production in labs. If you're in your scientific trade union, begin to think about the papers you write. Because you have interests at stake in not thinking about them, your lab life will become whole only through struggle. The individual and collective consciousness described earlier forms and arises from individual and collective struggle. For example, I got my ideas from the feeling of alienation I experienced in the lab. That sense of alienation goes back into a critique of the work that I've been involved in and into struggle with the trade union in the lab—critiques like those of Jon Beckwith, Steven Rose, Ethan Signer and Arthur Galston, all these people working within labs. And the alternative technology ecologists working in utopian settlements: their science will only be as socialised as are the means of production in those labs and settlements. They will suffer from internal and external contradictions to the extent that, as individual scientists or as collectives of scientists, they cannot change their consciousness, and to the extent that their lab or settlement is in contradiction with the system of production relations of the rest of society.

From my own experience there is a long way to go. Obviously scientific trade union struggles and struggles for critical consciousness in labs are at their very beginning, developing our consciousness of and struggle against, for example, the division of labour, or the hierarchical organisation of the lab, or developing our awareness of alienation from ourselves and from other people in society. It's only from these roots—which nourish all attacks on capitalist science—that we can develop these critiques and a socialist biology.

Postscript

Dear Edward,

This is intended as a reply to some points you made a while back about omissions in my 'thesis' and is really half-way between a letter and something I have been thinking of including between the two parts of it. Sorry it's impersonal, but I'm writing at the typewriter.

You, Anne Cooke and Bob Young have all commented that my critique lacked insights into what it felt like to do this work. Edward, you made

two parallel criticisms of lack of insights into life as a scientist, and, seen from the complementary viewpoint, failure to describe science as lived experience. I feel these criticisms strongly, as I thought I had succeeded in achieving both things you have noted as failures. Now, when I re-read the pages I understand what you mean. For example, I wanted to describe how it felt to come to a lab and to enter into the way of thinking of the people there, and then to struggle to re-emerge to refind one's subjective view and validate it. I thought that just to describe in abstract terms was to describe all that mattered, so that even bewilderment became an event outside myself. Of course I knew those feelings were important to my work, but I protected myself from responsibility for them by being abstract. I'll try again.

My first enjoyment of the LMB (Laboratory of Molecular Biology, Cambridge) wasn't surprising, considering my background. From school onwards I wanted to do research, once my first inclination to do English had been broken. Before 'A' levels I decided to go to Cambridge, with LMB already in my mind, and that decision was no more than a development of my parents' enthusiasm for achievement, particularly in science. And proximately in everyday life, people would ask in awe about the lab, encouraging and forming a particular view of it in my mind—in which Francis Crick and Sidney Brenner were important, for example. I was proud to be there.

I also felt put down by quite a lot of scientists without feeling particularly that they were 'better scientists' than I might be—hence some discomfort. More important was the effect of going to China. I didn't mention it in the first part of my thesis because it had little effect on my first year's work. My separation of science from the political viewpoints that going to China had formed in me was almost total. I felt tense towards the lab because it might encroach on this important area of my life—and rejected completely one gentle scientist and his wife who invited me home to talk about the experience of going to China. Even during the year there was a growing tension—but I was content with it. When there was a student sit-in at Easter I combined it (I conceived of it as 'it', that is, as separate) with long hours of work.

Popularly, scientists in the lab would say that it was when I began to find I wasn't succeeding that I began to opt out of science: the discomfort overcoming the enjoyment. Their argument is circular; external effects were involved. I was so fucked-up by my relationship with my girlfriend that all my inconsistencies were worrying me, particularly by the end of the summer. At the same time in September my girlfriend and I had parted, I got glandular fever, and I discovered that my eyes had become strained by doing so much microscopy. (I had actually been annoyed by other people in the lab taking summer holidays so that the working environment was less stimulating!) I suddenly felt, at the same time, all the sacrifices I had

made for work.

The contradictions that first year defined my feelings subsequently. I got more together after working out my feelings towards my girlfriend—schizoid feelings towards her and towards science resolved one another together. I began to be able to cope with scientists outside the lab, and to express myself more actively in politics in the lab, first through the union and then in my last year through the substance of my work. I could admit what I was doing to the different circles of people I knew inside and outside the lab. The conflict over leaving science which afflicted me the second Christmas worked itself out. It seemed too much the image of a lefty to leave science like that, not reflecting the importance that science still held for me (like an economic determinant), to dash out as though I had no identity invested in the changing present.

When I write like this, it is as though clear water ices over day by day, paragraph by paragraph, as abstraction takes over self-reflection. I feel you still want to know what I felt towards Sidney Brenner (answer: pity, hatred and liking in that order of frequency), towards other scientists, trade unionists and my work. What I've written here is only the backdrop to a reply to your request for science as lived experience.

You want to know what I was feeling when I did an experiment or thought of an idea. You don't want the evasiveness of being told my emotions. Of course I was happy when I saw something I had been searching for, and of course I was annoyed when eggs died, but so much underlies those bare emotions. To start with, I was trying to establish something. The animal is new and has to be seized. During the first few months, I was striving to seize it and know something nobody else knew. Hence the technical problems encountered were a nuisance which would involve depending on other people. Because it was so difficult to find out from other people in the group what had been done, many of my discoveries were rediscoveries and it was annoying to be told solutions to problems after rather than before I began to tackle them. When I first saw the egg developing I was elated. It was beautiful, and it meant I could accumulate results. Its beauty to watch was in tension with finding out that new thing, which meant watching the same process again and again and revising the beautiful explanation the egg had created in my head. The talks I gave at Christmas and after a year marked my knowledge of the egg. The task changes. It is concerned with extending observations and testing what I have said in public against new events. Finally it becomes achieving the lineage—being the first person to do so—and the work becomes more routine, grinding it out.

So there is the motivation which mediates between happiness and annoyance: to achieve status through knowledge. I'd think about a problem like working out the pattern of speeds of cell division in different lineages, all the time, at home and in the lab. I would be anxious that it had not been resolved by anyone else, particularly when reading the literature, though

I knew that I could not have perceived the problem had the conceptual net not already been available, and if to me, why not to someone else? Even at a forefront lab the fortuitousness of my own studies did not assure my uniqueness. I'd also be anxious about the attitude of other people in the lab because notoriously colleagues take over one another's problems. And as I would get very high when I was resolving or identifying a problem and very low afterwards, I found it difficult to distinguish between stupidity and utility in my suggestions. This made my relations with other scientists all the more difficult. There were very few people whom I did not feel threatened by but still respected: fellow graduate students until *we* became competitive with one another, and a few others. David Baillie was remarkable in being able to tolerate stupid ideas in the hope of hearing a good one; but he was not made welcome in LMB. Such was the weight of status on analysis.

From that it's obvious that I am an individualist and would have been even if I had not had my own subject to work on. During my first year I worked in a large lab with others working in it. While performing a routine job we would be talking to one another. When my work began to be appreciated I was given a room of my own. It was windowless and I was on my own with the microscopes. So my individualism became reflected in my work situation. I was really uneasy in it and would break off from microscopy or analysis of pictures just to walk round a bit and talk to people.

I mentioned aesthetic feeling before. But beauty was not simply pleasurable or the recompense for efforts made to keep the egg alive. I always wanted other people to see the egg when it was alive, but its beauty had a shocking public aspect. I wanted to communicate it to friends who knew nothing of the technical background to my work. But I could only tell them that it was beautiful—so that the experience became locked in, part of my alienation from others.

The simplest interpretation of lab events' significance was suggested by the variability of the amount of work I did. Perhaps the fluctuations in everyone else's work had similar causes. When I first met my girlfriend of that time, in May 1972, for example—I was so happy that any problem seemed soluble—the amount of work I got done was in retrospect extremely variable, being related to these feelings. At other times I would come in, sit down, and be reduced to fury by failure of a technique. The simple interpretation is to associate penetrating minds with penetrating penises—for me, to associate 'not being able to cope' with 'not being able to "make it" '. It is an analysis played with by many men. It is facile because status, aesthetic rewards and the need to have an image like that of a Nobel Prize winner are all separable and important. And I've always been nurturative, allegedly a female quality, as well as penetrative; with penetrative I ally 'manipulative'. I was keen to develop Nomarski optics as a technique but at the same time keen not to interfere with the development of the egg if

possible. My interest in embryology, though reluctant, seemed to be a substitute for an active sex life, but I felt this mainly as projection because I thought others would think it. It is already a common jibe at female biologists that their specialities are sublimated sexuality. (Francis Crick's paper on panspermia would be a good case study here.) So my maleness and femaleness—if that's what penetration and nurture really concern—were fairly directly expressed through subject of study and method of study as well as through my mood when working. For most scientists in the lab, the connection is probably at least as close as for me. In naming their newly discovered nematode mutants, they use acronyms with sexual connotations: the nematode screws, masturbates and is basically male in research conception, even if female and secondarily hermaphrodite in biological terms. Women scientists don't argue; there are, apparently by edict, almost none in our division, though there are many women working as technical assistants there.

Potentially the most far-reaching aspect of sexuality mediated by lab work is its relation to domination and submission. Did I try to dominate the nematode to the extent that my relationship with it is dominating, like the male sex role in capitalism? This is to me the cutting edge of the 'critique of domination' in the everyday life of scientists. Every day, whether trying to dominate or not, I struggled with that embryo. It is that which most clearly separates scientists from others: their individual struggle with something nobody else understands (because the scientist and the division of labour would not allow them to) and with one another over that thing. Coming up the steps into the air-conditioned lab, up the stairs and along the corridor at a run, the scientist returns to that. The egg controls me: hence my reluctance to give up research, hence my return to the hated coffee room, to the eye-straining microscope, to the library late at night. So possibly the struggle of the scientists with the 'material' mediates sexual struggle. The competition for status and money would be sufficiently individualistic to account for struggle with the meaningless, but to work collectively together we would also have to loose ourselves from the sexual meaning of that competition.

Now I want to leave off because I can't go through the whole 'thesis' adding these insights. I want to say, 'sense this in everything else'. To get deeper we would need a scientific consciousness-raising group—it's too easy for me to hide.

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Death in the Plastics Industry

CHARLIE CLUTTERBUCK

On 22 January 1974, B.F. Goodrich of Kentucky announced that three of their men who worked with vinyl chloride had died of a very rare liver cancer—angiosarcoma.

This news shocked the plastics industry across the world and highlighted a series of deficiencies in our present system. The vinyl chloride (VCM) tragedy is not characterised by the kind of blatant irresponsibility inherent in the bladder cancer cases, when ICI and Dunlop were fined £21,000 and £2,000 respectively. They were found culpable of negligence for exposing workers to a foreseeable risk of injury from NONOX-S fumes (b-naphthylamine), after the risks became known in 1946 (Cassidy). Instead, 'vinyl chloride' highlighted the limitations of a moral responsibility and the severe limitations of technologies designed for one purpose—maximum production at the least cost. Once built, the *technology* determined the course of action. How could a multi-million pound, multi-national industry operate for over 30 years, use a chemical in vast quantities, without knowing about the dangers to the health of workers? This question challenged not only company doctors but the whole scientific-industrial complex, the surrounding legislative apparatus and the very *thinking* of many chemists, engineers and designers.

It is not a new challenge. Throughout the development of capitalist technology there have been a string of 'unpredictable' health hazards that 'defied' control. Capitalist production technologies have produced an endless stream of health disorders attributable to chemicals. Pottery workers were exposed to dangerous lead levels for over a hundred years *after* substitutes had been found (Meiklejohn). Mercury was responsible for the mad hatters, and dry sandstone was responsible for the life expectancy of 29 years among dry grinders in Sheffield. Composite grindstones were not introduced to save the lives of wet grinders until 30 years after their invention; legislation in 1927 ensured that this control measure was enforced. Such is the nature of capitalist production, pouncing on all new inventions! Since the turn of the century, with science more directly involved in production, there have been oil mists, asbestos, solvents, epoxy resins, etc., all defying control. Fifty occupational lung disorders have been uncovered since

1961. It is in the nature and ideology of the technology, which blatantly puts profits before people, that hazards will continually be created, then go undiscovered, then, when found, be rationalised away—away from concern.

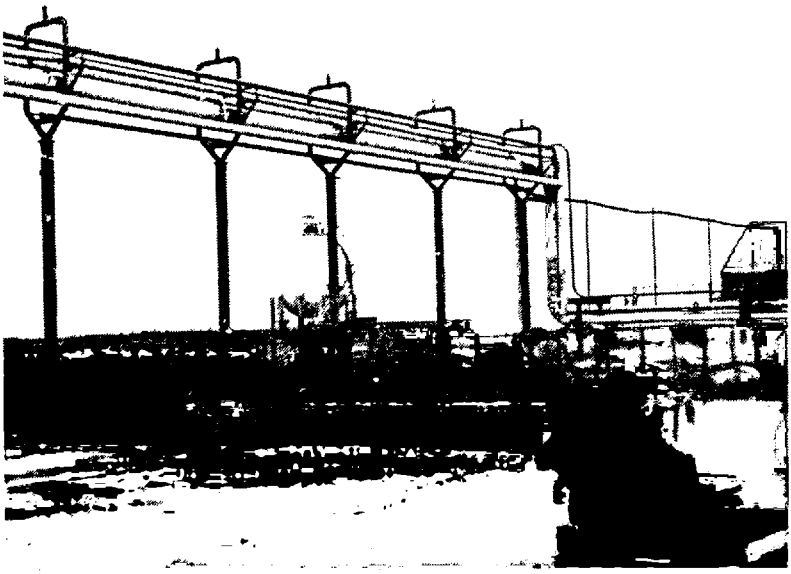
History of Hazards

PVC (polyvinyl chloride) is all-pervasive. Since the war, despite public reluctance, it has permeated every household. At the time of writing overall world growth of plastics is running at about 15% per year. It is likely to slow to a modest 10% in future. In the UK, PVC production is now 400,000 tonnes a year, worth over £100 million. It finds its way into home and factory by 30,000 miles of pipes, over 40,000 tonnes of electric cables, and 30,000 tonnes of flooring. 140 million records, 400 million bottles and 70 million shoes are made of it. Its most beneficial use is in 1000 miles of colliery conveyor belting, having replaced rubber, which had a higher fire risk.

Brian Ayres is the shop steward of the PVC production unit at the £120 million BP Baglan Bay Plant in South Wales. His job is to go into the large pressure vessels (autoclaves), after the vinyl chloride monomer (VCM) has been 'polymerised' to polyvinyl chloride (PVC), and scrape the PVC off the walls. He does that eight hours a day: two days on afternoons, two on nights and then two on mornings. The technology required that somebody do it. The plant has been operating about four years, and it is the newest of the six in the UK.

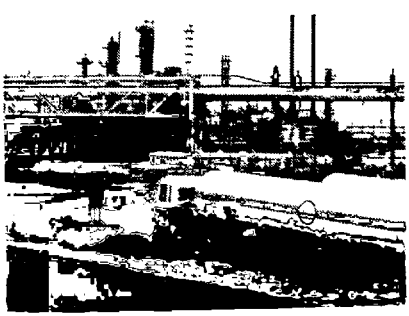
He says that before 1974, when first employed, 'We'd been told that VCM, having been used as an anaesthetic, that the only hazard appertaining to it was the explosive hazard and the fire risk. The only thing was that it could knock you unconscious, if you were in it for too long. Although we were wearing covering overalls, when you were working in the polymer, releasing VCM, you'd get this cold freezing feeling, especially round the ankles and crutch areas. That would make you get out of the vessel pretty smartish. Those were the bad old days, the really bad old days, considering what we've come to now.' On another occasion, a man unloading vinyl chloride from a tanker was overcome and taken to hospital. There he spent three days vomiting, with his legs 'frozen and his torso burning'. The company doctor asked him 'What did you have for breakfast?'; the man replied 'I think you've got the wrong case. I was unloading vinyl chloride when it happened.' Unperturbed, still in search of another truth, the doctor continued, 'Are you sure you didn't have any fish for breakfast this morning?'

There were other events during the 1960s which could have served as a warning that vinyl chloride was not 'harmless'. In 1965, Earl Parkes gained $5000 compensation from Goodrich for a liver condition that his physician testified was due to chemicals and named vinyl chloride. In 1969, ICI paid £8000 compensation to two men for a bone disease—acreosteolysis—that



Above: storage vessel for PVC
Lower left: PVC fabrication plant with ventilation units
Lower right: ICI Mond at Fleetwood, Lancashire





has also been shown to be related to VCM. In 1961, Dow Chemicals Limited considered, on the basis of animal experimentation, that vinyl chloride was more toxic to the liver than generally recognised (Torkelson *et al.*). They reduced their levels to one tenth of the generally accepted standards. The Threshold Limit Value (TLV) in the US and UK was 500 parts per million (ppm), and 50 ppm at Dow. In the USSR, the maximum acceptable concentration was 12 ppm. This low level is related to a 1949 Russian study (Tribuch) which showed a high incidence of liver disorders among men handling VCM, and to the very different system used in the USSR for establishing standards.

Crucial to proving the causal relationship between vinyl chloride and angiosarcoma was the work of Dr Cesare Maltoni. A year after a preliminary report on the carcinogenicity of vinyl chloride at high concentrations, the Manufacturing Chemists Association (MCA) in the US initiated its own research—to be carried out by Maltoni. The MCA entered into an agreement with its European sponsors (including ICI and Montedison) to share information, *but not reveal it without their consent* (Edsall). In August 1972 Maltoni found angiosarcomas and other tumours in experimental animals exposed to VCM concentrations as low as 250 ppm. In January 1973, US scientists visited Dr Maltoni and learned of his findings. However, they did not notify NIOSH (National Institute of Occupational Safety and Health in the US), nor did they make any attempt to warn the public or exposed workers. ICI contend that these were only preliminary findings and that, *at the time*, they did not appear to offer a serious threat to health (Barnes). Doubt persisted as to whether evidence could be extrapolated from the animal studies to industrial situations, and angiosarcoma was considered so rare that an increase in its incidence was not important. In late January 1973, NIOSH requested information on possible hazards associated with occupational exposure to 23 chemicals, including vinyl chloride. The MCA responded by recommending a precautionary label that made no mention of toxic effects on animals or people. In other words they appear to have deliberately deceived NIOSH. Apparently the MCA has claimed that the withholding of data was due to their agreement with the European manufacturers to keep the data confidential until an arrangement for their release could be worked out.

While there are plausible grounds for debate as to whether it was correct to withhold the data at this stage, no such doubt exists regarding the second stage of Maltoni's work. In November 1973 his more extensive studies confirmed the 1972 results. The MCA revealed the Maltoni data to NIOSH only on the same day that Goodrich made their announcement. In May 1974 Maltoni revealed his latest findings, which showed that angiosarcomas were found at 50 ppm VCM. He is now undertaking a programme of 12 tests with vinyl chloride at different levels, lengths, and routes of exposure. The work established the causal link of VCM and angiosarcoma. It also established that rat experiments can be used for extrapolation to humans. Previously,

it was argued that animal experiments were no substitute for the epidemiological evidence (counting the bodies). Maltoni showed that if the experiments are undertaken with due regard for normal working conditions, they can be made relevant. For example, PVC pneumoconiosis has been established by exposing rats inside the bagging unit of a PVC plant. Meanwhile industry continues to undertake short-term (often 3-week) testing, which is useless at isolating carcinogens. To make sure the long-term hazards are not isolated, few long-term exposure studies are undertaken. The excuse is that they are too expensive, and that there wouldn't be enough rats to go around.

In the absence of these studies, the means of detection of long-term hazards leaves much to be desired. As Professor S. Epstein (President of the Society of Occupational and Environmental Health Officers) has said, 'In the absence of pre-testing, the worker is unwittingly used as an involuntary test subject. Human experimentation of this kind is unnecessary and amoral, and our tacit participation in it is a matter of shame.' Thus, workers are often the first to notice the ill-effects of new chemicals. At Columbus Coating Fabrics in the US, it wasn't until the workers, a high proportion of whom were suffering from peripheral nervous disorders, went on strike for four weeks that attention was focussed on methyl butyl ketone as a hazardous chemical (Scott).

Similarly, the method of *isolation* of the hazards of VCM requires scrutiny. At the recent 18th International Conference of Occupational Health, three claims were made regarding its isolation in one afternoon. An epidemiologist (somebody who calculates the incidence of disease in selected groups of people) claimed it was the epidemiological work which isolated the hazard. A biochemist claimed that it was the animal studies. Someone else then pointed out that these proved the causal relationship but did not *spot* the disease. The same person then went on to assert that it was the fastidiousness of the Goodrich Company doctor, Dr Creech, who was responsible for the discovery. There is a lot of rewriting of history going on. Nobody mentioned the efforts of Earl Parkes, a worker at Goodrich. He twice took Goodrich to court, twice gaining compensation—once in 1965 and once in 1970—*despite* the testimony of Dr Creech (OSHA). Creech claimed that Parkes' liver disorder could not have been caused by vinyl chloride, as the levels of exposure were too low. If Parkes had not created such a fuss, the MCA work of Maltoni might never have been set up, and the records at Goodrich would not have been screened. When Parkes died of angiosarcoma, it was found that two other workers had also died of the rare disease. Then the public announcement had to be made. Only then were Maltoni's results released. The indictment of the medical-industrial complex is obvious. The need for a worker-orientated science of industrial health is equally obvious.

Response of Industry and Government

The extraordinary control measures that have now been taken must be seen in context. After the 1974 Goodrich announcement, the most expensive and comprehensive measures ever undertaken to deal with an industrial health hazard were mounted. Virtually all of the US National Institute of Occupational Safety and Health (NIOSH) field teams were mobilised to undertake epidemiological (group health) studies of various PVC plants (Tabershaw). Similarly, the Employment Medical Advisory Service (EMAS) in the UK, with the help of the Factory Inspectorate and industry, initiated many studies. The PVC manufacturing units were the subject of closest examination. VCM production plants and PVC fabrication processes were also investigated; so too were warehouses storing PVC, transportation services, meat packers using PVC wrappers, aerosols using vinyl chloride as a propellant (used only in the US), drinks contained in PVC bottles, and emissions into the atmosphere. The implications were enormous. Throughout the world, 17 men handling VCM were found to have died with angiosarcoma by June 1974 (McGinty). By November the total had reached 24 (Levinson). A man from the BP Barry plant in South Wales died at Christmas. Some months later the estimated toll had reached 38 dead. During the epidemiological studies it was found that the number of other cancers among PVC production workers was above the 'expected' incidence. In particular, the numbers of lung, brain and kidney tumours were unexpectedly high. It was also realised that, because cancers take 20 years to develop, these figures may be only the tip of the iceberg.

Other rapid developments that occurred were improvements in monitoring techniques and control engineering. Initially, monitoring was carried out irregularly, and then accurate to only about 200 ppm. More accurate techniques of gas chromatography were soon introduced and could measure to about 10 ppm. Eventually the accuracy was improved to 1 ppm, then to one part per 1000 million. It is now carried out continuously, in such a way that the levels are known to workers immediately. It is intended that 'alarm' systems will soon be introduced. Control engineering concentrated on improved ventilation systems, reducing leakages at valves and finding ways of cleaning the autoclaves by high pressure water systems. These developments are continuing slowly because of delays and shortages of the necessary pieces of equipment.

Some £9 million have been spent on organising and improving plant design in the UK. A further £4 million are estimated to have been lost in production (Barnes). This is something of a lie: because of the so-called energy crisis, prices of raw materials rose, so that the ratio of capital costs to running costs altered dramatically. It means that profitability is maintained at 65% throughput instead of 80% (*Sunday Times*, 13 July 1975, Business Section). While much of this cost would have been unnecessary if control

measures had been taken at the design stage, it must be granted that the industry did undertake expensive control procedures. The question must then be raised—*why have these extensive measures been undertaken?* Many other industrial hazards would benefit from similar considerations. For instance, the reduction of dust in mines would have saved the health of at least 40,000 coal miners with pneumoconiosis, and asbestos control would have saved the lives of many workers and their families living around factories and docks. There are 30,000 industrial widows to testify to industries' inadequacies.

There are perhaps two main answers to our question. The first relates to the nature of the associated disease. Angiosarcoma is a cancer, a lethal and rare cancer. As Brian Ayres, who for 18 years had worked down the mines facing the risk of pneumoconiosis, says, 'Once people became so much *aware* of this disease, that this is *death*, they were concerned.' The moral obligation was clear, especially considering the industry's failure to spot it previously. The rarity of the disease also meant that the causal connection with VCM was undisputed. What if vinyl chloride had caused lung cancer? The association would probably not have been noticed, and if it had, it would have been disputed. For instance, the increased numbers of lung cancers and bladder cancers in men working in coking ovens has never received the same amount of publicity and concern (Lloyd). The only similar disease, directly related to occupational exposure, is mesothelioma. This is the cancer of the lining of the lung; it is caused by exposure to low levels of asbestos. The incidence of this disease is being systematically 'shrunk in the government laundry of statistics' (*Guardian* 11 July 1975; Kinnersley, 1973).

This introduces the importance of the nature of the industry, especially the plastics industry. Firstly, the distribution of the usage of the chemical enabled the disease to be isolated. If the chemical had been widely distributed, the incidence of disease would have gone unnoticed. Similarly, control procedures were relatively easy to organise in the six main plants in the UK. The costs of control, although relatively high compared to other hazard control, are relatively low compared to the highly capital-intensive petrochemical industry. But most important, it was obvious that the plastics industry is not going to cease production, despite all the claims by industry that it would have to cease if proper health demands were met. It is not only a vital sector of the economy, it is *the* expanding sector of the future. There has been a slowing down of the expansion, due to the high capital investment needed. However, due to the paradox—that because of the massive increase in raw material prices, plants can now operate at lower production levels and still make a profit—there will soon be more developments, expansion and investment, and this sector will fare better than others in the coming economic crisis. Simply, the plastics industry has made a lot of money in the past, it is in a strong position for the future, and so it can afford to implement control procedures.

'The size of the North Sea oil reserves gives cause for confidence, with the possibility that the UK chemical industry could become the strongest and most dynamic in Europe by the 1980s' (Savage). The product of intense commercial pressures forcing processes to the 'frontiers of science' was adequately demonstrated by the Flixborough explosion (Grayson) and the subsequent extensive efforts of the Factory Inspectorate to deny even the possibility of a 'two-bang' catastrophe; the 'two-bang' theory had implications for the whole of the petrochemical industry (Kinnersley, 1975).

In order to legitimate its position, industry rationalises the contradiction between 'people' and 'production' by looking to its system of 'acceptable' standards. In the US and the UK, health standards are based on the Threshold Limit Value (TLV). The TLVs are drawn up by the American Conference of Government Industrial Hygienists (ACGIH). Says Professor Epstein, 'They are a quasi-official body which often represented industrialists' views more faithfully than those of the public' (Scott, p.85). Although they have now been officially by-passed by the USA OSHA Act of 1970, they and their standards are still influential. Run mainly by Dr Stockinger, they intend to be an 'opposing voice to what NIOSH has put out' (Scott, p.191). The TLV, upon which most of our standards are based, is the 'condition to which nearly all workers can be repeatedly exposed, day after day, without adverse effect. Because of the wide variation in individual susceptibility, a small percentage of workers may experience discomfort from some substances at concentrations at or below the threshold limit.' It thus accepts that a proportion of workers will suffer. This is different from the USSR standards, which seek to protect all workers.

There are many other reasons for distrusting TLVs. They are often based on very little research. They ignore the possibility of two chemicals reacting to produce another hazard. (For example hydrochloric acid and formalin, two very common chemicals, react to produce 'BCME, a potent carcinogen'.) And they are not enforced; they have only once been quoted in court—when the levels exceeded the TLVs by a 100X. A lot of scientists place a lot of confidence in them, as they are a way of rationalising the scientists' interface with the shopfloor. They also epitomise the function of scientists—to legitimate and make acceptable obviously deleterious situations. They are a scientistic way of asking 'How much maiming can we afford?'

The reassessment of the TLV for VCM showed up some of the values implicit with TLVs; thus the VCM limit needs to be examined. By January 1974, the level in the UK was 200 ppm. Here and in the US, an emergency standard of 25 ppm time-weighted average with 50 ppm maximum exposure was set by June 1974. The 'permanent' levels were then decided in the US via the Occupational Safety and Health Association (OSHA) court hearings at the Department of Labor, and in the UK by a tripartite committee consisting of various representatives of the TUC, CBI and the relevant government

institutions. There was a marked contrast in style. The OSHA hearings were in public, where the confrontation that developed between the unions (mainly the Union of Rubber Workers and the Oil, Chemical and Atomic Workers) and industry could be easily examined. The unions demanded 'no detectable level' of exposure rather than 'zero', as it is impossible 'scientifically' to measure zero. The 'no detectable level' also allows for improvements in accuracy of monitoring, which would automatically improve the standard.

The unions' demands were supported by Professor Irving Selikoff, Director of the Environmental Science Laboratory at Mount Sinai School of Medicine, New York. Their evidence was based almost totally on medical submissions relying both on Maltoni's work and on the NIOSH submission that all proven carcinogens should be reduced to 'no detectable level'. The management of the industries claimed that such a level was 'impracticable', and if enforced would 'drive the industry out of business'. They relied, in part, on the evidence of the Arthur D. Little Institute to show the impact of closing the industry down. They also showed the technical difficulties in operating such a level. Dow Chemicals Ltd, who had been operating at lower levels than the rest of the industry, also considered that a 'no detectable level' was unfeasible.

The court ruled that industry had not proved its case that it was impossible to reach the standard. They thus decreed that from January 1976 maximum levels should be at 1 ppm, with a maximum of 15 minutes excursion to 5 ppm. Although seemingly very stringent, there were two main loopholes. First, the 1 ppm does not allow for improvements in monitoring techniques to improve the standard. Secondly, there were provisions for the wearing of respirators wherever higher levels were produced. Brian Ayres again: 'By committing themselves to these much lower figures, they are also committing themselves to wearing respirators. Now I won't wear a respirator; it makes the job that much more difficult. And then there's the problem of wanting money for it, whereas I want the emphasis to remain on getting rid of the hazard.'

In the UK the tripartite committee met, discussed and produced a Code of Practice. There is no way of knowing how the decision was reached to establish a permanent standard of 25 ppm, although one union member of the committee is known to have demanded a 'no detectable level'. A Code of Practice is not a regulation that is enforced, but is more like a highway code—to be used for advice. Industry views it as a requirement 'to keep moving' (Barnes). Brian: 'All I can say is, that if there is any factory in this country at this moment relying on it, I feel sorry for them. We're way ahead of the Code; we're down to about 12 ppm average.' This makes one wonder why the level was set so high in the UK. The Code of Practice on VCM merely legitimises the past inadequacies of the industry.(A new proposed Code of Practice was introduced in January 1976, with a time-weighted average of 10 ppm.) Similarly the Code of Practice on Noise was

set at a level which ensured a certain proportion will suffer hearing loss (Fletcher). If the US decided that 1 ppm was the only level acceptable on medical evidence, and that it was technically feasible, it is difficult to see how this present standard is based on anything other than a 'we've-got-to-keep-this-technology-going' principle—a technology based on gross past inadequacies.

Response of Workers

Action taken by workers in response to the hazard has been varied. At ICI Runcorn, spasmodic strike action was taken over a period of nine months for 'danger money'—an across-the-board payment of £10. At Vinatex, Chesterfield, concern was centred mainly on the risk of acreosteolysis (over 20 men showed various symptoms of the disease) and compensation. Elsewhere, concern was less and relied on the national decisions. This could be due to 'apathy', but, as the developments at Baglan indicated, the situation is more complex than can be explained away by such a term.

I was involved with the local environmental action group at Baglan, who were concerned with the nuisance from noise, dust, the risk of explosion and the completely dominating presence of the £120 million petrochemical plant. While visiting the plant, the management told me 'Actually, we're quite pleased about vinyl chloride; it's the only issue that we're in agreement on with the unions.' Bearing in mind what was happening in the US at the time, I was surprised. I could only presume that the unions here were getting different information regarding the hazard. The suspicion was confirmed by comments from the Medical Adviser, Dr Robert Murray. 'What you've got to say to the workers is that the risk is small: that apart from the risks involved when he drives his car, and eats too much, and drinks too much, here is another risk which he has got to live with' (*Nature*, 15 February 1974). Brian: 'When you talk about the men, our so-called leaders whom we elected to ivory towers in London, there was no information forthcoming from them. None whatsoever.'

I spent some time trying to establish contact with the shop stewards at the plant. It was not easy; the plant is well integrated, and any outside, independent, not officially recognised people are treated with certain suspicions. Such are the problems involved with undertaking the very straightforward task of making available the information which should be theirs by right. It shows how much more is required than simply publishing material in an academic journal, and presuming that the rest is taken care of. I made contact with Vernon Bowen, a well-respected member of the union. I wanted to find out the levels of vinyl chloride in the plant—then we could have the data to establish more than mere impressions. He took the proposal to the joint shop stewards committee. He was demolished. Here was a very competent union man who had a lot of experience, and he was

shattered by the response. The shop stewards considered that the responsible management was doing all it possibly could. That was in June 1974, long after the original news, and long enough for the management to have informed the workers thoroughly. Luckily, I was able to offer to Vernon some concrete proposals. Firstly, it was essential to distribute a few of the key articles (from *New Scientist* and from America) to the members. Secondly, given the coincidence of BSSRS undertaking a community TV programme, we could have a joint public discussion of the issues. I could put my case, and if they considered that I had a worthwhile contribution, they could say so. If they considered that I was talking rubbish, they could say so. When the time came, they reckoned that I could not possibly add to what they already knew. They agreed that they were proved wrong, and they were shocked. The evidence is on film.

Feedback from the encounter came from two sources. Firstly, I was contacted from the ICI Runcorn plant, where I was told that everything that had occurred at Baglan could equally have occurred at Runcorn. All were equally ignorant. Secondly, the joint shop stewards at Baglan unanimously invited me and another BSSRS member to talk at the plant to all of them. They also stated to the management (from whom they had to get permission) that, if we were not allowed on to the plant, there might be a 24-hour strike. A 24-hour strike on a petrochemical plant means a 3½-week closure. Despite this, the management refused to allow us on to the plant on the grounds that I had 'distorted the truth'. Surely that was up to the workers to decide? The strength of the shop stewards' demands could not be matched at the branch level, however, and the meeting had to be held outside the plant. At the very least, there is a lesson here about the right to withhold information.

Since that time, as Brian Ayres says, the management have come forward with a lot more information—they could not afford to be embarrassed again, and the men have become so much more aware. 'But for the fact that people like you come along, and shall I say, adding sticks to the fire that may be kindling all the time, it would have just been history repeating itself. Like when ICI were making that resin for the rubber industry.' Since that time Brian has been able to enter into a proper state of 'collective bargaining' on the exposure standards, competent to argue with the company doctor. He knows what he's after and he knows what is really feasible.

Resources were needed over and above those in the traditional economic and compensation areas. Sources of information—other than from management—were scarce. None of the three main UK unions involved (GMWU, TGWU, ASTMS) had their own researchers/advisers. The GMWU made the most effort to inform, using the information from ICF. In the UK, no independent member of the established scientific-medical community (e.g. TUC Centenary Institute), nor any member of the Factory Inspectorate, made any public statement. The press, TV (World in Action,

Open Door), and 'rogue' organisations (mainly BSSRS) took the initiative to inform. All had to rely on the developments in the US and international conferences (e.g. ILO and ICF). At best, it was a rearguard action. Despite the apparent power of the unions, they are unable to obtain totally safe working conditions, to change the concrete reality of the technology. The NUM have been trying for years. Brian knows the contradictions too. 'A proposition was made in the branch meeting to have a whole-scale shut-down of the plant—"Unless they make the modifications, the plant isn't going to restart". So I said, "Wait a bit now. You can't get the stuff off the shelf; it needs to be re-designed. It's going to take time. What you're doing is asking this plant to be closed almost indefinitely. You're volunteering to go out of the gate yourselves".'

Lessons

> Occupational carcinogenesis is a difficult field to approach, because the care of the workers' health is often in conflict with the productive interests of the factories. Industry has done a lot for experimental carcinogenesis, related to occupational cancer; often however the result is merely to prove the carcinogenic effect in animals of agents which have already proved to be carcinogenic for man: this is equivalent to saying that many people die of cancer but some make their living from cancer.
> —C. Maltoni, 'Occupational Carcinogenesis'

Until there is a technology which is not based on the control and degradation of labour, what can be done? What can be learned from the vinyl chloride tragedy?

For anybody working in such places, the case for workers' control is obvious. How can you trust anybody who balances capital against safety and health? In the film *Catch 22*, Milo Minderbinder bombed his own airfield in exchange for a promise from the Germans of the cost of the operation plus six per cent. When challenged by Yossarian, whose friend had just been killed, he said, 'Well, what do you expect? That is the law of the supply and demand of the market.' And, 'Catch 22 says they have the right to do anything we can't stop them from doing' (Heller, p.430).

The famous Oil, Chemical and Atomic Workers' (OCAW) strike, where 5000 people struck for five months to gain health and safety contracts, was fought on that issue. The management of Shell and Chevron considered that safety and health were their responsibility. The union considered otherwise. Risk management, or risk control (as it is also known), is the latest development in removing power from workers. 'It is more than just trying to prevent losses and buying insurance. The name signifies a different type of approach to the handling of risks involving a systematic study of the problem within the whole organisation ... It is part of the revolution which has changed management practice in the past 50 years, called Scientific

Management' (*Financial Times*, 9 July 1975).

To challenge this new development, and from the experiences of vinyl chloride, the following rights need to be established: the right to information, to regular health checks (*with* the results), to regular monitoring, to pre-testing of chemicals/plant before introduction, to independent advisers, to regular inspection, to walk off or black, to have model health contracts, to organise levies to gain money for courses, films and talks, and to re-engineer the plant before re-engineering the people.

The need for radical scientists to get involved in such struggles is obvious. Finding out what is really going on in the local factories, and the needs of the workforce, will soon make those scientists realise the problems that need to be overcome. The issues will not always be as clear-cut as with vinyl chloride. Specific chemicals, unusual cancers and media concern will not always be there to add strength to the case.

But, progressively remove the 'scientific' elements (the cancer and the chemicals) and you have a continuum which gradually peels back to expose the skeleton in the cupboard. Take out the rare carcinogenic chemicals, and there are still thousands exposed to coal, silica and metal dusts. Take away the chemicals and that still leaves two million being exposed to deafening levels of noise for eight hours a day. Remove the less glamorous physical hazards and it may start to reduce the prevalence of nervous disorders, such as ulcers and other stress-related disorders. It still leaves all the monotony, division of labour and the illimitable, unmeasurable alienation which prevents self-realisation and mutual aid.

If you're in doubt about the continuum, men at Baglan are not. It is only when comparing the different jobs on the plant that Brian gets heated. 'We, the autoclave cleaners, don't just look at a bloody clock, you know, where you're on your own just watching all the pressures, or how full the tank is. We can actually see the PVC powder on our plant, being bagged, and going out to the lorries. There's some sense of achievement. You're actually *in* that environment. It makes the job that much more interesting than the rest of the complex. That's why you get a lot of people wanting to work here, although they're shit-scared of angiosarcoma.' The high value those workers place on a job, because, however dangerous, it offers some relationship with the product of their labours and their fellow workers, speaks more than all the volumes of management science on risk-benefit analysis.

There has never been more need for a worker-orientated science of industrial health. The recent Health and Safety at Work Act of 1974 (HASAWA) 'should provide a boost' to Risk Management (*Financial Times*, 9 July 1975). Whether it fulfils the more important task of significantly reducing the toll of accidents and ill-health is anybody's guess. It certainly removes the conflict from the shopfloor to the courtrooms where it is more easily managed—especially with the help of expensive scientific expertise.

One thing learned from the vinyl chloride issue was that management didn't like emotion. The Chemical Industry Association geared their campaign to reducing concern (Barnes). The industry continually played down the other diseases associated with vinyl chloride. They even had the pictures of acreosteolysis sufferers cut from the 'Tomorrow's World' item on vinyl chloride monitors, because they were too emotional. They don't *own* emotion. *They own the science.* Dupont owned 25% and Dow owned 18% of the chemical patents taken out in the US between 1969-73. So it's best to keep the issue 'in the court of the rational man'.

Similarly, industrial health is continually directed towards the 'threshold limit values' and the 'dose-response curves'—the so-called scientific issues. Barnes, of Public Relations in ICI Plastics Division, considered that 'the *real* debate should have concerned itself solely with the questions of whether any residual risk remained once exposures to VCM had been reduced to 1-10 ppm range, and whether the use of respirators introduced a greater risk of death from fire or explosion.' In other words, let's talk about dose-response curves, and the risk feasibility of re-engineering people. The *real* debate? Who decided that? And why? Nothing to do with the fact that the only people with the time and experience are all paid by the plastics industry???

What would be the main differences between the management-dominated industrial health and a worker-orientated science of industrial health? Obviously, we won't know until workers' needs have been properly realised, and until scientists have been responding to those demands for some considerable period. Only then will we be able to perceive all the ideological components of the science of industrial health. However, some of the components are more obvious than others and can be fought against now.

Of the three main areas of industrial health and hygiene—control, pre-testing, and epidemiological work—it is easy to say that more work should be undertaken on each. We should know what the incidence of disease is from particular contaminants (epidemiology). Similarly, if we are to introduce chemicals, the least that could be done is to test the chemicals before they are introduced; there is no scheme similar to that for pre-testing of drugs and pesticides. Obviously, it is not desirable to use people as test subjects, so it is important that extensive pre-testing be developed. However, neither of these 'sciences' in foolproof, and neither need be developed if the hazardous materials were controlled at source. This, then, is the science requiring most attention.

Production, pressures and ideology are more thoroughgoing than merely a matter of requiring more research in particular areas. It is more a matter of the questions chosen within the subject areas. For example, within epidemiology, management scientists may consider the most important thing to look at is the distribution and effects of alcohol and various other drugs.

Other scientists may consider that the hazard and distribution of noise is a much more important problem.

Blame for accidents has always been heaped on the individual. The 'accident-prone' theory is now being updated, after being found to be disreputable years ago. Biorhythms causing 'triple critical days' (*Guardian*, 6 March 1975) are now held responsible. The same theory can only be used with difficulty to explain why 40,000 miners were individually responsible and susceptible to pneumoconiosis. But the trend to find a biologically determinist answer is with us (Heuper). With vinyl chloride, and many other carcinogens, it is argued that the sufferers were *previously* susceptible, that either the liver was already susceptible or the people were inherently susceptible. 'Those persons not protected by the TLV are the persons who, on account of altered genetic patterns of metabolism, are hypersusceptible to poisoning by industrial chemicals' (Stockinger). 'On this basis, the problem becomes that of finding the altered enzyme or the associated factor or products related to the hypersensitivity ... for the detection of the hyperreactor in industry' (Cuthbert). So that's the problem! Ah well, if that's all that is needed... a few more scientists should solve that. This approach would be relevant if there were an extensive Occupational Health Service to back up the work, to screen, etc. But that is not the case, nor is it likely to be. Instead these developments look more like the philosophy of the XYY chromosome fiasco—the ultimate in ignoring the lousy system, and instead developing a screening technique to weed out the 'susceptibles'.

A worker-orientated science would look at the system that produces the ills, rather than trying to blame a few individuals for apathy, accident-proneness, genetic susceptibility, smoking habits, etc. It would look to cleaning up, rather than covering up: *to re-engineering the plant rather than the people*. The 1883 Factories (Prevention of Lead Poisoning) Act was the first legislation to insist that people employed adopt measures such as wearing of respirators, overalls, etc. Section 7 of the new HASAW Act enshrines the principle, such that muzzling of workers with respirators and earmuffs (etc.) is now big business. The ideal for industry is obviously to find a machine to replace the hands. The DeVilbiss-Tralfa Robot is advertised as being unaffected by noxious spray materials—and capable of working 168 hours a week. Rather than waiting for the goggles, muffs, boots, muzzles and parachutes (etc.) to appear, involvement by workers in the design process is now needed. The Lucas Shop Stewards Committee is showing the way. The best place to build safety and health is in the design stage. It is also the best place to question the relevance and the desirability of the product and other aspects of production. Workers are in a better position than designers and planners to know what that means.

In conclusion, there can be no solution until scientists work *with* other workers, and until workers have the ownership of the means of production. The analysis of the ideology of the sciences, while essential, should

be for the purpose of establishing strategy and tactics. Such analysis can never be complete until the above preconditions are realised.

Postscript

Ten years after the world found out about vinyl chloride dangers, we can see that the petrochemical industry was successful in removing the grosser hazards while keeping the issue confined to the risk of a rare cancer in the fabrication plants. The arguments were not taken up about related chemicals (like trichloroethylene), nor did the industry accept that other cancers were causally connected. The arguments also confined fears to the fabrication units, preventing fears from spreading to the people handling the finished product PVC, whether they be other workers or consumers.

Yet there have been many incidents to show that the hazards are not so confined: from the incident in the USA when over 200 people had to be taken to hospital following exposure to fumes from a PVC extruding machine (Froneberg), to claims in Kidderminster, Worcester, that workers handling PVC dust may be getting cancer (*Worcester Source*). Probably the most substantative evidence comes from a survey, commissioned by ICI management, that concludes with 'the clearest evidence yet obtained that inhalation of PVC dust is associated with respiratory dysfunction and radiographic changes... Measures to reduce individual dust exposure would be prudent' (Inst. of Occup. Med.). Added to this is the recent 'Pansac' case of the worker, exposed only to PVC extrusion, who died of angiosarcoma. All these results fit in with what critics would have predicted, which the companies have tried to play down.

The 1976 UK Code of Practice is being replaced by a new 'Approved Code' that has semi-legal status; three times the length of the original, it is written in obfuscatory language. It has yet to be finalised through the cumbersome Health and Safety Executive (HSE) procedures, but it will be one of the first Codes under the COSH (Control of Substances Hazardous to Health) Regulations. It sets standards not so much for VC, but for other chemical codes that may follow. It is supposed to conform to EEC directives that lay down a maximum average exposure of 3 ppm over a 12-month period for VC. Trade unions and management disagree over how to translate such an annual maximum into a daily maximum. Some unions argue that it means that controlling action should be taken whenever the 3 ppm level is exceeded. Whatever the result of that argument, it will be much worse than what the unions are demanding—a 1 ppm maximum level.

In the last ten years there have been drastic improvements in the trade unions' ability to handle such issues. Most of the major unions—excepting the AUEW (Amalgamated Union of Engineering Workers) and TGWU (Transport and General Workers Union)—now have national safety officers. Some also have research advisers (e.g. ASTMS) and local officers

(GMWU). Most workplaces have safety representatives, most of whom will have been on TUC safety courses and be better informed than many in management. The unions are well represented on the tripartite HSE structures, though more could be done to guarantee their accountability to their memberships. Education and representation may have improved, but bargaining power certainly has not; and what has happened on a national scale has also happened with VC. PVC production has been contracted from six to four plants. Many shop stewards and safety reps have moved, been promoted or been made redundant (voluntarily, of course).

The PVC units at ICI Hillhouse, Fleetwood, are typical. Between 1978 and 1980 the joint shop stewards committee was able to hold a series of mass meetings (to take account of all the shifts) to discuss the dangers of VC and the management's response. Increasing concern and distrust were being expressed; the management wheeled in their top national doctor to allay fears. Workers began to see that there were differences of opinion among the medical profession, between the company's Dr Stafford and Dr Epstein in the USA; it appeared that ICI wasn't all-knowing. Granada TV started doing a programme. However, just as opposition was mounting—5 years after the 1974 revelations—two of the PVC units on the site were closed down. Recession had hit. This meant redundancy for those that wanted it, and removal for the rest—and that included the safety reps. They went to another plant, had to learn another job, no longer be a rep... Two of the leading union reps (one a convenor) have since taken up contracting jobs for the company, and it is doubtful whether the joint committee even meets now. So much time had to be taken up with the problems of shutdown and amalgamation that there was no time left for much else. (This is not by way of criticism; it happens wherever you work.) The decline of the PVC economy, like the rest, has created changes and re-organisation which prevents longer-term issues being dealt with. It is a bit like the crises of capitalism being used to clear out the debris and anything else the capitalists don't want.

But this isn't the only reason why little is heard of VC these days. A few major, rich companies were able to improve conditions dramatically; if the companies' figures are to be believed, then £5 million was spent per worker. Nobody denies that improvements have been made; there may be disagreement about how bad it was before and about whether it is good enough now, but things have improved. As my essay showed, because a spatially concentrated capital was running a high risk, it could afford to improve. Elsewhere, in many other industries and with many other hazards, the same economic conditions do not apply. Asbestos may be more dangerous, it may kill a million people in the UK, it may be found in schools and hospitals everywhere, but because the replacement costs are relatively high, the process of improvement will be slow, spasmodic and patchy. Even in the face of widespread protest, improvements for hazards like asbestos,

radiation, noise, and dust will depend on the power of labour, not on the logic of capital.

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Mathematics as Ideology and Politics*

LUKE HODGKIN

> *Discover the truth through practice, and again through practice verify and develop the truth. Start from perceptual knowledge and actively develop it into rational knowledge; then start from rational knowledge and actively guide revolutionary practice to change both the subjective and the objective world.*
> *—Mao Tse-Tung, 'On Practice' (Selected Works,* Vol. I, p.308)

Our aim is to develop a politics of science; and the physical sciences, particularly mathematics, have always been the most intractable part. In this article I want to approach the problem. There is first an old and recognised difficulty. Mathematics is the oldest science to develop a rigorous—even rigid—idea of its own practice, so that the scientificity of mathematics has come to seem not so much historical as natural after 2,500 years. The idea that the history of mathematics should include social determinants is seen as absurd even by Marxists.

We are talking about politics, though, and so about science in today's society; and the difficulty I've described would not matter if we could apply to the work of mathematics an analysis similar to the one Simon Pickvance has made for biology. (I'm saying 'mathematics' in general, where often I could include theoretical physics, sometimes other sciences—read yours in if you find it fits.) What I mean is an examination of the labour process which led to an understanding of 'the relations of production of scientific knowledge' analogous to the Marxist picture of industrial production. Such an analysis can clarify the way in which the limits of division of labour, hierarchies and work discipline all control what the scientist produces. But here again it's more difficult in mathematics and specifically in that part of mathematics where I have direct experience—the university, teaching and research. Teaching and research are individual activities and require a low level of personal interaction. Where there is work in which social relations ('organisation'), obviously enter, it's seen as non-scientific. The whole nature of my life and work, and of the work of my friends with whom I argue, leads to a picture in which science and the social relations which

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determine our existence are seen as independent, even by trade unionists who are conscious of the increasing constraints under which good teaching or research gets done, and of their market value.

Where do we start then? There is beginning to grow up an increasingly critical reflective viewpoint on the practice of mathematics itself. And the people who work in the field are the first to see that this viewpoint makes sense.[1] They see that mathematics is less a hard crystalline mountain of truth painstakingly built up by successive generalisations than a field for error, for the conflict of ideas, for vagueness, for continual approximation and conjecture as the means of progress. So it's recognised among mathematicians, if not outside, that we do mathematics one way, but we might well do it another. For example, we were allowed to talk of 'infinitesimal quantities' from the time of Leibniz (1680) to the late 19th century. Then it became ridiculous, contradictory, inadmissible. Now since 1965 or so the work of Abraham Robinson[2] has made it scientifically 'respectable' to do just what Leibniz and his successors were doing. And this is only one example among several. But even among those who grant this, the idea that the changes and conflicts which exist in mathematics should have social determinants may seem far-fetched. How, given the nature of the work as I've described it, could the social formation possibly intervene?

Marxists are the only people who have thought about such questions while at the same time being concerned with problems of political strategy. (And only a few of them.) Because I think it's essential to combine these two perspectives all the way through, I've drawn heavily on Marxist writers, as well as my own understanding of scientific practice, to undermine the abstraction of 'the scientificity of mathematics' and to place it in a modern political context. I was also led to look at Marxist debates—for example on what 'ideology' *means* and how it is produced—by criticisms of my first draft, where I'd made several dogmatic statements of the type: 'For Marxists, ideology is ...' Such statements played an important part in my argument. I was reminded that there are good Marxists who think quite differently. And so, in trying to justify my own views, I discovered the politics which underlies the debates involving Gramsci, Bukharin and others in the 1920s, Althusser and his followers and opponents today. I have tried to introduce this politics into the article, because I've found that the arguments of the past can shed light on our views of today's practice, while we in turn can use the criterion of practice here and now to judge and criticise the views of past Marxists.

One contribution seems particularly important for scientists: the London conference of 1931, where the Soviet papers by Bukharin and others were published as *Science at the Cross Roads*. The paper by the mathematician Colman is a particularly good example of the strengths and weaknesses of Marxist analyses of the sciences. It provided the focus both for the talk I gave at the BSSRS conference 'Is there a Socialist Science?' on which

this article was based, and for the first draft of the article. Colman analysed the weaknesses of Western mathematics at the time, in particular the absence of effective procedures for computing the answers to mathematical problems. He predicted that these defects would act as a brake on the productive forces, thus accelerating the stagnation of Western economies. And he claimed that a new mathematics of computation would arise ... 'under Socialism', hence, in the Soviet Union.

In World War Two, however, capitalist production expanded enormously, and at the same time a new mathematics—associated with the computer—arose. Was it an accident? Was the form of the new science independent of the economic circumstances of its birth? The answers seem to be obviously 'no', but what can we say that is more concrete? And what does this tell us about the place of mathematics in particular, and science in general, in capitalism today?

This article spends quite a long time on what is basically an attempt to clear the air on these issues. But the fog is thick—from my experience of discussing them so far. And given the importance of science as a component in modern capitalism, I don't feel we can afford to leave the fog there. I should add further, in answer to other criticisms which have been made or anticipated, that for me this fog-clearing—the disentangling of the nature of 'capitalist science' and its politics—cannot be something separate from the developing shopfloor struggles of scientific workers or the organisation of campaigns against science in its present servant/master relationship with capitalism, imperialism and the State. The two are part of the same movement, drawing on the same sources, and I hope they'll be seen as such.

Autonomy of Science?

The problem of the 'autonomy of science' has increasingly become a major one for the left in general. Some writers (Simon Pickvance, Brian Easlea, Bob Young) attack the idea that a neutral, non-social scientific knowledge can exist, or attempt to demonstrate how scientific theories are determined by the economic 'base' (Sohn-Rethel). Others oppose them with the claim that there *is* a 'science'—or rather there are sciences—which remain essentially unaffected by the social formation and which are deformed only by the incursion of something non-scientific—*ideology* (Althusser and his followers) or *scientism* (many BSSRS spokesmen). Althusser has stated this position at its most brutal: 'Science can no more be ranged within the category "superstructure" than language, which as Stalin showed escapes it' (*Reading Capital*, p.133). And later: 'All the lines of demarcation which philosophy draws can be reduced to forms of one fundamental line: between the scientific and the ideological' (*Philosophie et Philosophie Spontanée des Savants*, p.50). These statements—with the surrounding discussions, which make clearer what Althusser means by 'science' and 'ideology' (I'll return to this

later)—could well be taken as the extreme counter-position to those of Young and Easlea. They implicitly deny the possibility of using the terms 'bourgeois science' and 'socialist science'; in fact Althusser explicitly attacks the use of those terms in the introduction to *For Marx*. (The bourgeois/socialist science distinction also proved very controversial at the BSSRS conference where I gave the first version of this paper.)

Since I wish to defend the position that science is closely linked to the social formation, that today's science is, in some sense which I hope to make clear, *bourgeois*, and that a socialist science can exist which will be different, I should point out at the outset that these ideas have a long Marxist heritage themselves; the debate has continued intermittently in some form at least since Engels' *Anti-Dühring* (1878). Althusser, for example, in the first quotation I gave, was attacking Gramsci for treating science as a superstructure, for instance, when he says: 'According to Marxism it is evident that it is not atomic theory that explains human history but the other way around: in other words, that atomic theory and all scientific hypotheses and opinions are superstructures' (*Prison Notebooks*, p.468).

Bukharin, in his contribution to *Science at the Cross Roads*, wrote in terms which could well be used by (say) Bob Young:

> The idea of the self-sufficient character of science (''science for science's sake'') is naive: it confuses the *subjective passions* of the professional scientist, working in a system of profound division of labour, in conditions of a disjointed society in which individual social functions are crystallised in a diversity of types, psychologies, passions (as Schiller says: ''Science is a goddess, not a milch cow''), with the objective *social role* of this kind of activity, as an activity of vast *practical* importance. The fetishising of science, as of other phenomena of social life, and the deification of the corresponding categories is a perverted ideological reflex of a society in which the division of labour has destroyed the visible connections between social functions, separating them out in the consciousness of their agents as absolute and sovereign values (*Science at the Cross Roads*, p.20).

In other words, science is a practical activity among others in society, a social function whose agents ('bearers' in another version of Marx's language) are scientists. As with the economic, legal and other functions, the assignment of an absolute value to its products turns the practical human activity into an extra-human object, i.e. fetishises it. Bukharin not only denies that science is autonomous, but he also provides an explanation of why its autonomy is defended. And he does this in terms of the social division of labour and its effect on human practice, by drawing on Marx's analysis of productive labour and commodity exchange.

It's appropriate to go back to the discussions of the 1930s to situate the present one. Although at that time too there were different points of view among Marxists about the autonomy of science—in fact Stalin seems to have held both the points of view I've referred to—the problem was far

from occupying the central position it is in danger of holding today. I say 'in danger' because I think the question of autonomy is not a good political starting point for science as we confront it here and now. This is particularly true when the question is asked in an abstract form and the science considered is the 'classic' science of the Greeks, Galileo, Newton and Einstein. Periods of what Thomas Kuhn has called 'scientific revolution' are fascinating, but not typical; still less is the work of the 'great' scientists typical of scientific work. And at the same time when socialist historians have finally dethroned the great men of history as subjects, and turned increasingly from Danton and Robespierre, Pitt and Fox to study the crowd, the *sansculottes* and Luddites,[3] it seems odd that so many socialist scientists still think that science can be understood through its great men and their achievements. At present an enormous number of scientists are doing, for the most part, 'unrevolutionary' work under conditions of a close day-to-day relationship with one another and with their employer—a capitalist firm, the State or one of its organs. This is the basis of 'Science' as it exists now if we want to think politically rather than in terms of the history of ideas; and (to jump precipitately to the end of the argument), science in this sense in our society can only be bourgeois science.

But this is very much the sketch of the argument. In what follows I want to fill in the sketch first by looking in more detail at the categories 'science', 'ideology', 'production' as they apply to the present, in particular to the various scientific practices which now exist based on mathematics. I shall say how I think the categories as I have described them lead us to a different understanding of the questions of 'autonomy' and of 'bourgeois science'; and I'll draw some conclusions about the role of science (again mathematics in particular) in modern capitalism. I shall try to avoid making or attacking statements like 'Galileo's theories expressed the ideology of the rising bourgeoisie' until I've attached a definite meaning to them; and even then I shall prefer the present to the past and Joe Bloggs Ph.D. to Galileo. This viewpoint is already present in Bukharin's contribution to *Science at the Cross Roads*; as Gary Werskey says in his introduction to the book, we have a great deal to learn from the writers of that period.

Science

To understand 'science' and to be able to use the idea politically, at least some clarification is needed. Do we mean the work that scientists do, or do we mean the result of that work? (Compare the distinction writing/literature, for example.) Althusser—again summing up an older tradition in Marxist thought—refers to scientific (or sometimes 'theoretical') *practice* and scientific *knowledge* ('On the Materialist Dialectic', *For Marx* pp.167 ff.). Scientific practice is seen as analogous to productive labour; the analogy is carried through in terms of raw materials, tools and products;

and the end-product is scientific knowledge—which can in its turn be used as 'raw materials' or 'tools' for further scientific practice.

I think that this distinction is a valuable one, even if we suspend for the moment the question of precisely *what* we regard as scientific. It gives a criterion for seeing that some statements are too vague and general to form part of an analysis of science as we face it today. For example: 'the use or abuse of science' (old-style BSSRS); 'science is becoming a direct force of production' (Azcarate); 'science has become in itself technological' (Marcuse in *One-Dimensional Man*, p.128). All these need more analysis if they are to be assigned a sense and then accepted or rejected. (It's well known that a writer's use of broad terms like 'industry', 'nation' or 'society' often shows that the argument advanced is sociological and that political questions are being avoided. The same test could be applied to 'science'.)

We now have *two* problems—to examine the social nature of both scientific practice and scientific knowledge. Taking scientific practice first, however much we might want to argue about the past, it's clear that today the social context plays an overwhelming role in structuring scientific work. The subsistence of the mathematician Bloggs while he works on his thesis, his 'choice' of a problem and of how to approach it, his decision that he has done enough work and can write it down as a thesis and submit it—these are not part of an isolated struggle between himself and 'nature' or 'the literature'. Rather, they form part of a social process: his construction, subject to constraints, of a product called 'Bloggs' Thesis' in which his supervisor, his fellow graduates, contacts in the field met at seminars or conferences (patrons, advisers, rivals); and at a different level the Science Research Council, IBM, NATO and other potential sources of money may all intervene. (I myself found that two 'supervisors' were giving me directly conflicting advice on how to write my thesis—a confusing socialisation into the differing value systems of working mathematicians.)

The social character of scientific practice illustrated in such an example becomes even clearer when we move away from the field of 'pure' research and consider the much larger proportion of scientists who are working in industry or in education. We may disagree about how much of their practice is truly scientific (running a simulation of the British economy over the next ten years for ICI would be an extreme example); but if we postpone this question, the important point, which seems to be accepted by Althusser when he refers to scientific practice as a 'level' of social practice, is that *all* scientific practice is social. Hence, scientists are social beings not merely because they have relationships, jobs, insurance payments, political opinions (or none), and buy food and clothes, eat, drink, watch TV. They are social beings also when they are involved in their scientific practice; in fact, this work in some sense determines all the rest.

This viewpoint will seem overstated. It's a rather toned down version of how Marx and Engels saw work as determining life in general:

> As soon as the distribution of labour comes into being, each man has a particular sphere of activity which is forced upon him and from which he cannot escape. He is a hunter, a fisherman, a shepherd, or a critical critic [or a scientist] and must remain so if he does not want to lose his livelihood ... This consolidation of what we ourselves produce into an objective power above us, growing out of our control, thwarting our expectations, bringing to nought our calculations, is one of the chief factors in historical development up till now (*German Ideology*. pp.44-5).

Scientific practice (if we apply Marx and Engels' ideas) is related to scientific *knowledge* as the labour of any producer is related to its product. The scientist constructs a theorem, an elementary particle, a routine for solving equations (usually, by now, in collaboration with several others); but when the work is done, it's 'available' to others, sometimes freely, sometimes (if classified or patented) restricted or sold at a high price. The users 'consume' the work, working on it themselves to learn it, understand it, develop it, sometimes to apply it industrially or in another area of science. And so scientific knowledge grows and is consolidated into 'an objective power above' the individual scientists who produce it.

Bloggs' thesis may, when/if published, become 'Bloggs' Theorem', exchangeable (for some years) for jobs, recognition, invitations. It may be recognised as a special case of someone else's much more important work. It may turn out to have important applications in cosmology or soil chemistry, and Bloggs may suddenly find himself with a completely different group of colleagues, different interests, different work prospects. It may be proven wrong or go out of fashion. All these are effects of the interaction of Bloggs' work with the totality of scientific knowledge. They are unpredictable and external in the same way as (much more obviously) the changing state of the job market for scientists due to slumps, wars, cycles of capitalist expansion and contraction in general and 'events' like the space race. As with other production processes, it is the product, scientific knowledge, which 'society' needs; the particular producers are dispensable in the long run.

But although the scientific knowledge which we construct becomes a 'power above ourselves', it is *not* a power above society any more than capital (which is produced in an analogous way) is. I think this is a crucial mistake by those who have tried to separate 'science', in the sense of scientific knowledge, from the society in which it is produced and consumed. From a materialist point of view the knowledge produced by scientists has no existence except insofar as it is learnt, understood, applied, transformed in the practice of other scientists or, more generally, by people who need, for whatever reason, to use them. For we know that, in general, scientific truths such as the laws of physics are relative in the sense that they are always liable to be superseded by new ones or incorporated in them. It is tempting to extend the analogy with capital further; for science, like capital,

must circulate to grow and, one might almost say, must grow to exist.[4]

Finally, the very process by which scientific knowledge is accepted as true is a social one. I think that this idea, whatever its parentage in the bourgeois sociology of knowledge may be, must be accepted by Marxists as a condition for a political understanding of the work scientists do and the knowledge they produce. Our ideas of scientific truth, of the tests and evidences for it, are acquired and transmitted in institutions, usually teaching institutions; and these institutions vary greatly from one society to another. Even the two most elementary verifications that we have of our rightness in a piece of scientific work (themselves very different)—'It works' and 'I got full marks for it'—are socially defined in those institutions where we learn to say them. A 'solution' of a differential equation will mean one thing to a mathematician, another to an engineering student who is trying to relate what the mathematician is telling her to other things she's learning, and another again to a working engineer. Because of differences in teaching approach, it may mean something different to an engineering student in France and in England. Admittedly students are only taught partial results (who isn't?) but they use them, and their use is part of the social practice of mathematics.

Hence we have important ways in which scientific knowledge changes character, even locally, with changing culture. And this determining character of social institutions on the knowledge acquired is well known among scientists: X was at Princeton, at Grenoble, at Warwick, and so he/she sees a differential equation in a particular way, even has a particular idea of 'proof'. But this knowledge is not spread outside the restricted community of scientists, so that outsiders still believe in a universal agreement on an abstract 'rightness', which is something they have themselves been *taught* to believe in—at school, and in mathematics in particular. As Wittgenstein said: 'Our children are not only given practice in calculation but are trained to adopt a particular attitude towards a mistake in calculating' (*Remarks on the Foundations of Mathematics*, V,40). We must learn to live with this kind of insight and to build it into our view of scientific knowledge: learning is socialisation.

Ideology

I've tried so far to concentrate on one aspect of thinking about science: science as social practice. This brought with it recognition of the process/product distinction; and I then argued that not only the process of production of science, but the product itself, are social. Does this have to be said? The quotes in the introduction show that even comrades can think the opposite. And this point is really where I think *this* argument ought to stop. Once we recognise the social nature of scientific practice and scientific knowledge, we know that they are linked to the social formation as a whole and so finally

determined by its class nature—whatever 'finally' means. So it becomes possible to intervene politically in science; and if the classic examples (the Soviet Union in the '30s and '40s) are examples of a bad intervention, that's no reason *a priori* why an intervention couldn't and shouldn't be made.

It would be best to go on from here to discuss the politics of science as it exists in society today—assuming, as I do, that our aim is a political discussion (understanding from the point of view of changing), not simply an academic one (how the machine works). But the word 'ideology' has been mentioned so often that it needs to be disposed of first. Is science ideological? Is it only partly ideological? Does it matter? 130 years after the *German Ideology*, Marxists are still arguing about the meaning of the term; and one interim answer could well be to leave it that science, now, is political and then to get on with politics. This, I think, is too simple. (I assume we want to distinguish between the school, the factory, and the police even in recognising their common features.) When we describe *how* science is political, 'ideology' will be a name for one particular way.

In *The German Ideology*, 'ideology' seems to mean ideas which are false, distorted by class position. Gramsci was probably the first to see clearly the defects in this view, in particular its incompatibility with the reference to ideology in Marx's *Preface to the Critique of Political Economy*: 'the legal, political, religious, aesthetic or philosophic, in short, ideological forms in which men become conscious of this conflict [social revolution] and fight it out'. Gramsci seems to counterpose the view that ideology is the *only* social framework—'truth' or 'falsity' being irrelevant—within which people can think collectively of their situation in the world:

> [Many people] forget that the thesis which asserts that men become conscious of fundamental conflicts on the level of ideology is not psychological or moralistic in character, but structural and epistemological; and they form the habit of considering politics, and hence history, as ... a competition in conjuring and sleight of hand (*Prison Notebooks*, p.164).
>
> "Ideology" itself must be analysed historically, in the terms of Marxism, as a superstructure. It seems to me that there is a potential element of error in assessing the value of ideologies due to the fact (by no means accidental) that the name ideology is given both to the necessary superstructure of a particular structure and to the arbitrary imaginings of particular individuals. The bad sense of the word has become widespread, with the effect that the theoretical analysis of the concept of ideology has been modified and denatured (p.376, translation modified).

In fact, if one wants to preserve the word 'ideology' for *distorted* social systems of ideas, then another word is needed for those which are supposedly undistorted—and then moral and political condemnation are difficult to keep out. This is in spite of the well-known role of ideologies as the basis for revolutionary movements—Puritanism in England, rationalism in France, mechanical materialism/scientism for the late 19th century, social democratic

working class—and in spite of the dependence of any socialist movement on ideological 'weapons' (slogans, pamphlets, resolutions, speeches).

At this point we need once again to deal with Althusser's viewpoint, which is highly developed and, I think, deeply contradictory. (For a full account and critique, see Geras' article.) Briefly, Althusser shares the Gramscian view that ideology is *universal*, but at the same time he sees its effect as distortion:

> An ideology is a system (with its own logic and rigour) of representations (images, myths, ideas or concepts, depending on the case) endowed with a historical existence and role within a given society ... we can say that ideology, as a system of representations, is distinguished from science in that in it the practico-social function is more important than the theoretical function (function as knowledge) (*For Marx*, p.231).
>
> So ideology is not an aberration or a contingent excrescence of History; it is a structure essential to the historical life of societies (p.232).
>
> We know that a "pure" science exists only on condition that it continually frees itself from the ideology which occupies it, haunts it, or lies in wait for it. The inevitable price of this purification and liberation is a continuous struggle against ideology itself, that is, against idealism ... (p.170).
>
> An ideological proposition is one which, while it is the symptom of a reality different from its object, is false insofar as it relates to its object (*Philosophie et Philosophie Spontanée des Savants*, p.20).

I have juxtaposed these quotations without commentary because I think they show the amazing range of Althusser's attitudes towards ideology—a range not explained by developments in his thought alone. They represent a last-ditch attempt, carried to its logical conclusions, to separate out science as an activity whose truth is independent of society.

We're told therefore that ideology is a necessary part of any society, including a socialist society (quote 2); that it's a social practice similar to scientific practice, since it produces a system of representations (quote 1); and that the only difference is the extent to which the social function enters (quote 1 again). All of these seem to me reasonable and consistent conclusions to which I'd assent; they're an extension of the Gramsci position as described earlier. But the very weakness of the distinction drawn in the first quotation ('more important'—along what scale?) leads to the conclusion that ideology and science are in the same family of social practices. So, in order to preserve science (and quote 3 is quite explicit about this), Althusser has to choose a point along the line which joins them and defend it. The very reasonableness of his theory of ideology-within-society leads to an extreme unreasonableness when he deals with ideology-against-science. And this is, quite simply, because he is trying to draw a line which can't be drawn, a line where the 'practico-social' function stops. I've indicated my arguments against this line in the previous section; the only point which I want to stress here is that it's a *necessary consequence* of simultaneously adopting the

broad view of ideology which I've described, and at the same time trying to defend the independence of science from the social formation.

I have by now said, if not in so many words, that in my view science is ideological. But it should be obvious from the Gramsci quotations that by this I don't mean science is wrong, distorted, or serves ruling-class interests automatically. This is the sense in which the statement has been interpreted in the past, so that Feynman, Monod, Eysenck and Bloggs have been lumped together as people whose work 'is ideological' (and, in extreme versions, might as well be writing for the *Sun*). I'd rather say that scientific work, like any other work, takes place within the framework of an 'ideology of practice'[5]—an image of what that work is, what its significance is, which questions are interesting and which approaches to them will give results—in short, a complex of representations of scientific activity. This ideology—shared to a greater or lesser extent among a number of scientists—is socially determined and in its turn determines the choices and orientations for a scientist's practice, and so for the *kind* of knowledge he produces. In the next section I'll come back to a more specific account of how this happens.

A scientific 'ideology of practice' is, in today's conditions at least, a bourgeois ideology. By this I mean that one of its functions is to depict the activity of doing-science-within-capitalism as an acceptable choice for the individual. But capitalism seems to survive best by having a number of competing ideologies at its disposal—in politics, in philosophy, in art; and the same is true of ideologies of scientific practice. (Again, in traditional Marxist theories one tries to correlate these ideologies with 'fractions' of the ruling class. I won't discuss this question at all, since it has no immediate relevance.) I'll try to give an instance of this diversity. If you're an undergraduate mathematics student now (or at any time in the last 150 years), one of the subjects of major importance which you learn is going to be advanced calculus, or 'analysis'. This is the point where, if at all, difficult techniques are taught which make it possible:

1. to show the *existence* of solutions to problems in the calculus (for example, differential equations, integration problems, minimum problems) and
2. to *find* the solutions in a number of cases, or to estimate or approximate them.

By and large, part 2 is what used to be taught. It consisted of clever and difficult approximation techniques, plus learning certain cookbook formulae. It was often understandably disliked because it was not unified, and because it was usually difficult and boring. In the 1940s and '50s it began to be seen that (by virtue of recent theoretical developments) part 1 *could* be taught and learnt elegantly and simply as a unified course. There was a swing in this direction, of which perhaps the best expression is Cartan's *Differential Calculus* (1976). A student who has 'learnt' Cartan has a very good understanding, in terms of an abstract theory, of why certain problems

have a solution, and how one proves that they have one.

Inevitably, a reaction set in. The students in question couldn't solve quite simple practical problems required in probability theory, in engineering, in physics. A counter-ideology which stresses the concrete (part 2) has been expressed in a number of later textbooks, perhaps best in Dieudonne's *Infinitesimal Calculus* (1968); the title is deliberately 'reactionary'. In his introduction, Dieudonne explicitly attacks texts such as Cartan's:

> When one has seen a second or third year student toil over a change of variable or an integration by parts, one can scarcely be other than alarmed, particularly (as is sometimes the case) when the same student seasons his ignorance and clumsiness with a pretentious and useless jargon which he has also failed to understand. To pretend to neglect [classical mathematics] in order to plunge immediately into the most recent functional analysis is to build on sand and can produce nothing but sterility and verbiage.

I have left the technical terms in place to make clear that this highly ideological argument concerns the practice of teaching mathematics and hence the ideology of mathematical practice with which the student leaves the university. Since Dieudonne and Cartan are both writing textbooks for the French university system, in which courses are highly standardised by the Ministry concerned, it should be clear that neither ideology is anything but bourgeois. Still, they compete; and this competition, this non-uniformity, is an important feature of all sciences today.

In spite of all the differences I've described between the statement 'science is ideological' as I understand it, and the simpler possible understandings of the statement, I've found that a common response among scientists is still: 'but surely after the revolution, two and two will still be four? 23 will still be a prime number? the chromosome number of a rabbit will still be (whatever it is)?' Notice the way in which the most boring parts of science, those which can be incorporated in multiple-choice tests, are standing for the whole. More imaginatively, a socialist physicist might have asked in 1900: but surely, after the revolution, we'll still have causality?

Of course, a certain 'rightness' in particular results isn't easily perturbed by changes in social formation, language, culture, and so on. But what is that 'rightness' precisely? It's undeniable that Archimedes found the area of the surface of a sphere, that 16th century mathematicians and engineers rediscovered his works, and that the area hadn't changed in the meantime. But something interesting had changed, in fact several things. Archimedes—although less than other Greeks—was working in a framework in which truth was *proof from axioms*, and the rules for proof were strict. He also was not allowed to measure either the radius of a sphere or its area as a *number* because a 'number' meant one of 1, 2, 3,..., and a length, area or volume was a 'quantity'; proofs used no approximations, no decimals, and so on. So if one looks now either at the statement or at the proof of

Archimedes' results, they look very bizarre in comparison with the formula $4\pi r^2$ which one learns in school. They prove with great ingenuity and complication that the area of a sphere is equal to *the area of a related figure* since 'we can only compare like quantities with like'.

But they both looked as bizarre to the engineers of the 16th century, who were used to manipulating numbers like π, $\sqrt{2}$ and so on in much the same way that we do today, and who set more store by results than by proofs. In that period, when there was an immense interest in such problems for practical reasons, Stevin wrote a generalisation of Archimedes' work to 'barrel-shaped containers'. In setting it out he explained that he *could* write out the proofs following the methods of 'the Ancients', but that it would be both tedious and unnecessary since proofs (for simpler solids) by those methods—the famous 'method of exhaustion'—already existed. He therefore gives proofs which today seem inadequate but readable, and formulae which are much closer to our own. The practice of mathematics had shifted so radically that, although the surface of a sphere could be said to remain 'the same', the expression of the surface and the reasons for believing it had become completely different. Of course, mathematics as a science is no more reducible to a list of formulae than chemistry to the list of chemical formulae for all compounds. So there is a real sense in which 'the expression for the surface of a sphere' had changed from the Greeks to the Renaissance; ideology of mathematical practice is, among other things, a name for the change.

Institutions

From now on my aim is to move away from ideology as the focus of political thought about science. It's a question which shouldn't be avoided or dismissed; otherwise the 'neutrality of science' problem is waiting round every corner. But it's not the only, even the most important, question about science in society today. As a first stage, I want to shift from the vague 'in the head' view of ideology which might well be read into what I've said so far, to the social forms and institutions in which it's embedded. After all, in confronting the Catholic Church Galileo was not merely facing an organised group of people with different views from his own; he was up against a powerful institution which could deprive him of his job, ban his publications and torture him. As a necessary part of society, ideology—including the ideologies of Galileo's successors—tends to assume such material forms, though they may not be as all-encompassing.

And here Althusser has made an important, if controversial, contribution to the theory by identifying certain institutions as 'ideological State apparatuses' (ISA). (The contrast is with the usually recognised repressive State apparatus—police, prisons, army, State bureaucracy.) The institutions he proposes are given first as a list: 'the educational ISA (the system

of the different public and private "Schools"); the family ISA; the legal ISA; the political ISA (the political system, including the different Parties); ...' (*Lenin and Philosophy*, pp.136-7). Here Althusser is taking up an important point originally made, as he notes, by Gramsci: the power of the ruling class could not continue if its only expression were the repressive one.

> The school as a positive educative function, and the courts as a repressive and negative educative function, are the most important State activities in this sense: but, in reality, a multitude of other so-called private initiatives and activities tend to the same end—initiatives and activities which form the apparatus of the political and cultural hegemony of the ruling classes (*Prison Notebooks*, p.258).

So we learn to become citizens of capitalist society, understanding and adapting to our niche in the division of labour, our sexual roles, our possible choices of 'politics' and of general world-outlook, through these institutions—family, education, and others. In particular, most of what I've called scientists' ideology of practice arises in institutions and is transmitted through them. It's in schools and in the family that the choice to become a scientist is usually determined—and no one can pretend, for example, that these institutions give equal encouragement to women when they present science as one of a list of available futures. Having taught us to be scientists, the school, college, university, and later the institutions in which we work, force us to change and adapt our ideas of what scientific work is, changing our ideology—and our idea of what constitutes scientific knowledge.

Again, this is something I've experienced myself at several stages, and in different ways. At school I learnt that mathematics was difficult: for example, that the basic thing you had to know was how to do 'problems', that this could be enjoyable, but as the exercise of ingenuity (like solving crosswords) rather than anything broader. At university, thanks to changes I've already referred to which were beginning to take place in the fifties, I was encouraged to think that *real* mathematics was easy and a quite different pursuit. It consisted of proving things, and in contrast this was attractive and broadminded. (A key textbook in this context, and I don't think this is at all untypical, was Birkhoff and MacLane's *Survey of Modern Algebra*, which appeared in 1952.) I came to despise, and to try to avoid, the kind of mathematics which followed on naturally from what I'd done before—problems about heavy chains falling off tables, or the gravitational field of slightly flattened planets. Then the university made available a new ideology. A proportion of its students 'chose' it; I was one.

Similarly, institutions determine what, for the individual, is 'knowledge'. Does one learn of certain objects (e.g. a charge concentrated at a point) as though they existed or were merely a convenient fiction? Does

one learn that statistical mechanics (e.g. in the laws of thermodynamics) sees time as having a definite direction, while classical mechanics doesn't? And if so, does one learn a way of rationalising the contradiction? At any time, any two scientists even in the same field will have wide unspoken divergences on scientific 'facts' which are the product of their education and later history. So not only my ideology of scientific practice, but also my ideas of what constitutes scientific knowledge, are shaped less by myself than by these social forces outside me which transmit 'their' ideas.

In fact, it's well known that educational institutions in particular play a leading role in determining not only what an individual scientific worker thinks of as 'science', but what is 'science' to society in general. Teachers produce textbooks, textbooks influence widely accepted courses with particular inclusions and omissions and presentations of a particular science. The whole process is coordinated—often in an anarchic 'struggle for survival' way—by schools, colleges, larger education policy institutions up to Ministry level and of course publishing houses. When a book does survive it can become the canon of knowledge in the subject; its version of the science becomes, in many respects, the dominant version. A revolution can be set in motion by these same institutions (like the introduction of 'new mathematics' teaching in schools in the USA and Britain) which changes the science *materially* in that it changes the way in which a generation learn it.

This theory is very little developed. The quotations I gave at the beginning of this section only provide a start, and maybe in some ways a false start, towards understanding how these various institutions (and I haven't mentioned academies, professional associations, and many others) operate to form and transmit ideologies in society and so to structure science in particular. But I think that they are one of the significant connecting links or mediations between 'base' and 'superstructure'. It is through their influence that science retains and reproduces its character as *bourgeois* science—a knowledge adapted to a bourgeois society, whose forms of learning, of work, of organisation are rooted in the division of labour and so (as Simon Pickvance has pointed out) imitate the corresponding forms in society as a whole and are correspondingly oppressive.

It's true that this oppression isn't necessarily perceived; it's that of being 'forced to think' certain thoughts and repress others. So, as with the ideologies one learns in the family or at school which make it possible to live in this society, it is seen as natural. Different institutions, on completely changed models in a socialist society, would provide different images of science as practice, and of the practice in any science, so that, over time, science would change; and that would be a socialist science. I don't think we can construct futures of this kind here *because* of the society we work in, which makes them hard to see. But that's no argument against their possibility.

Production

And so, finally, we can get to the 'base'. It seems to be the key problem for the politics of science in general that setting side by side the statements
'Society is organised around, and determined by, production' and
'Science takes place in society'
has hardly ever led to anything that scientists could regard as a meaningful statement about their work, because the relationship is so complex and indirect. And this was certainly true in the 1930s, when there was most interest in the subject. Today certain things are simpler. The great names of science in the 1970s are not those of Nobel Prize winners. It should be conceded to Roche, Fisons, IBM and the US Defense Department that each of them has made a more significant contribution to science than any individual, however brilliant. And it's much clearer than it is with individuals why they've made this contribution 'in the last instance'. There is in fact an enormous and very visible business of science with clear links to the exploitation of the Third World, women, the working class, and often (as in the case of drug companies) the mass of people indiscriminately.

All this shows clearly that a large sector of scientific work is now organised directly around and for capital. It isn't new to say this, and for a large number of scientists it may not be their sector. So we still have the question: how much is the nature of science controlled by production? How far does the sector I've just described influence the development of the whole of science? And what are the forms of influence or control? I think that some of the keys to answering these questions are contained in the understanding of ideology I've set out in the last two sections. To proceed further I want to return to an illustration which I referred to in the introduction.

We conveniently call 'mathematics' one subject today (or we subdivide it into any number of subfields). In fact, though, I'd suggest that we have *two* sciences which go by the name of mathematics, characterised by fundamentally different practices. The distinction is not between pure and applied; I'd rather describe it as between 'old' and 'new' mathematics, where the old includes pure *and* applied as they existed up to the 1930s. This was a subject characterised mainly by the features of analysis as I described it before—coherent methods for showing that solutions to problems existed, and more or less *ad hoc* methods for finding the solutions. The problems might be pure or applied, but the methods available were in the same family, characterised by a 'semi-informal' presentation (you explain what you 'need' to explain, but assume that your reader can fill in some steps), a carelessness about the smallest details. 'Solve the following partial differential equations by treating them as though they were ordinary differential equations', I was told by an engineering textbook the other day. This is a typical statement in the language of 'old mathematics', since it can be given to a student to

see how much she's learned, but it certainly can't be given to a machine.

What this mathematics lacked in the 1930s, and still lacks today, is a *coherent* procedure for solving problems. (This, I must add, is in spite of the fact—which has been pointed out to me by Sigurd Zienau and others—that a number of mathematicians and physicists were interested in precisely this question, and got some way towards providing partial answers.) It was in this context that E. Colman made his contribution to *Science at the Cross Roads*: 'The Present Crisis in the Mathematical Sciences and General Outline for their Reconstruction'. Here he described this particular shortcoming of mathematics as the 'gulf between theory and practice':

> While mathematical theory is extremely ramified and develops very rapidly and luxuriantly, mathematical practice, that is, the methods of calculation, scarcely moves forward at all; indeed it has practically remained stationary at the position it occupied in the 17th and 18th centuries ... A large number of mathematical methods, powerful in theory, are proven in practice to be useless because the forms of their application have not been worked out (*Science at the Cross Roads*, pp.223-4).

Colman made two other interesting points: first, that the so-called 'crisis' in the foundations of mathematics (the absence of secure logical foundations for most important proofs) was of secondary importance in comparison with problems like the above, from a materialist viewpoint. Second, that there was only one way out for mathematics: 'conscious planned reconstruction on the basis of materialist dialectics', and that this 'can only be made in a country where the national economy and science are planned' (pp.225-6).

This pronouncement, from the dynamic expanding USSR to the slump-ridden West, is a good example of the possible acuteness and blindness of an optimistic Marxist analysis. Ironically, the new mathematics which he had predicted came into being under forms and in circumstances which he did not predict (any more than anyone else in the 1930s). Broadly, the coherent practice of a mathematics oriented towards finding the solution of problems was born in the USA during the Second World War and after, when, precisely, industry and science collaborated closely with the military under conditions of much greater planning than had ever existed in advanced capitalist countries. It was then that the first computers (analogue and digital) were built by the collective efforts of MIT, Bell Telephone, IBM, and military and naval agencies. The resulting 'new' mathematics of computation and its associated fields have developed since then to such a point that they form, as I stated earlier, an autonomous science of comparable weight to the old, the emphasis being precisely on those effective methods of computation whose absence Colman had observed.

I must emphasise the distinctness of these two sciences, which is at a different level from the separation between different branches of 'old'

mathematics, for example. It is possible to learn one—in the sense of receiving a full training—and know nothing at all of the other. (Computer firms often stress that all you need as a basis for their training is 'a clear logical mind'.) Much more could be said to show that the two sciences—the old and the new—have different 'objects' and produce different kinds of knowledge.[6] A typical example, often quoted, is the method of solving systems of linear equations. The standard method taught in schools and universities requires something like 70,000,000 operations to solve 100 linear equations. But it looks elegant, and it's assumed that one doesn't want to solve that many in practice; exam questions involve three or four equations at most. In 'new maths' (numerical analysis or computing) textbooks this method, 'Cramer's Rule', is referred to only as something to be avoided. Instead they teach a family of methods derived from a technique called 'pivotal condensation' (actually invented in 1853 but very much neglected from then till the 1940s). For these 100 equations, solving by pivotal condensation requires only 700,000 operations and is less likely to introduce errors. If one sets these two methods side by side, it's obvious that they are looking at the same problem from two distinct points of view, and have different ideas of even such an elementary concept as 'the solution of a problem'.

But of course that is not all the difference between the two sciences, and in particular it's not what keeps them apart. The new mathematics arose in a specific relation to production. Although computer science and numerical analysis are taught in universities, offer Ph.D.'s and so on, this continuing relation to production, to the demands of institutions—which precisely require this 'practical' mathematics and can buy the machines (or the time) and the people to do it—is an essential component of the new. Hence we have a demarcation, determined by the relation to the productive process, which is self-perpetuating (as is usual where the demands of the specialisation and the division of labour are strong). A firm wants a computer scientist or a traditional pure/applied mathematician, very rarely a combination of the two.

Parallel to this division at the level of production, we have divisions in the ideology of practice, and in the institutions which transmit it. With regard to the institutions, I've already mentioned that there are generally separate departments for old and new mathematics at universities; similarly there is a parallel separation at the level of learned or professional societies, journals, etc. If a single firm employs both 'old' and 'new' style mathematicians, they will often work as separate groups, sometimes even competing to solve the same problem. Finally, outside universities we have the case of institutions (the Control Data Institute for example) which train computing mathematicians and have no parallel for traditional mathematicians. What ideology of practice, what image of their goals and of the nature of their work, distinguishes new mathematicians from old? I've already given

the example of a change in the idea of what it means to solve a system of equations.

As another, we could consider one of a number of terms which are specific to new mathematics' approach—for example the term 'subroutine', a standard sequence of calculating steps which can be incorporated in many longer computations as a block, such as a subroutine for finding a square root, etc. This is a scientific term in new mathematics but not in old. It cannot, strictly, be imagined in the context of old mathematics, since it is the answer to a collection of problems that old mathematics does not see as part of its object. (These include problems such as: How do we save ourselves effort in writing instructions for a machine to do a computation given that we know that certain sequences of instructions are going to crop up over and over again?) The term 'subroutine', though, has an ideological content also. This is not only because of the meaning of 'routine' in ordinary language (e.g. 'I could see he was going to go through that whole routine of his again'); it is also particularly because it relates to an image of the scientist's work which is peculiar to new mathematics: subroutines are constructed out of individual (atomic) steps and then become blocks out of which complete programs for computation can be built. The whole sequence, as a picture of the computer scientist's activity, is specific to that science and its near relatives, distinguishing them from other sciences. It is a component of what I've called the ideology of practice of the new mathematics.[7]

One important contribution to a Marxist history of science, it seems to me, would be to trace the origins of this ideology and its relations with other, equally practical, strands in the ideology of contemporary capitalism, such as the ideas of the assembly line and of scientific management. As far as the internal development of mathematics goes, the origins go back, at least in part, to the period of the 'crisis' of foundations which Colman regarded as relatively unimportant (the years 1900-1920). It was then that there arose the idea of reducing a mathematical deduction to a finite sequence of independent *atomic steps*—for example in Bertrand Russell's programme for reducing mathematics to logic. The idea was that only such sequences could be guaranteed 'safe', a word which at that time had the theoretical meaning 'free from contradictions' but which we can now see implies, more practically, 'capable of being carried out by a machine'. The link was pointed out in A.M. Turing's work in the 1930s—again, its full recognition had to wait till the actual machines existed (Hodges).

This bourgeois ideology of the early 20th century, as well as the demands of production in the 1940s, underlay the particular form of the practice of new mathematics as it arose. One of the founders, John von Neumann, recognised the break with the past when he wrote of a computing establishment in 1946: 'The inner economy will be very different from what we were uniformly used to since the days of Gauss ... New criteria of "practicality" and of "elegance" will have to be developed.' It's not

accidental that von Neumann picked on the notion of 'elegance'—one of the most straightforwardly ideological in the old style of mathematical vocabulary—as due for overhaul. I should stress that this is an important mathematical notion (read Hardy's *Mathematician's Apology* for a straight traditional point of view on this); 'elegance' is ideological, socially determined in the same way as notions with similar names in the arts are, as John Berger points out in *Ways of Seeing*.

I've gone into this example—the rise of computer science, numerical analysis and their relatives—in some detail, because it provides some idea of the complexity of the relation between production and knowledge, 'base' and 'superstructure'. Looking at the history, it seems impossible to deny a connection. In fact, not only was computer science developed in response to the needs of capitalism in the USA at the time, but the form it took was partly determined by the kinds of machine available: most recently, revolutions in 'hardware', e.g. transistors, printed circuits. But we have to keep in mind the presence and importance of other determinants—the problems that Colman drew attention to, the existence of ideas in formal logic that potentially changed the whole idea of 'solution' as a process, and so on. These make it impossible to say simply that the new science was a 'reflection' of the economic base. (And if I've chosen an example from 'applied' mathematics, something with an obvious link, it wasn't so much to make the connections with the 'base' more obvious as to make clear that they weren't the whole answer.) Production has determined the existence of the new mathematics and maintains it in separate existence, but its forms, its guarantees of knowledge—in short, its scientific practice—are related to production in more indirect and complex ways (mediations), through the structure of the science's institutions, its accumulated history, its ideology of practice.

Let me move to a more dynamic view of what has happened and is still happening. We know that scientific production is becoming integrated into production as a whole. Figures are supplemented by arguments (Bob Young) and analyses of the lived experience of scientific workers (Simon Pickvance). The goals of 'socialisation and mechanisation of mental labour', however often they've been turned into slogans for some particular transitional programme, are real and living as concerns science today. Capitalism (as I said at the beginning of this section) has by now radically altered the nature and organisation of scientific practice, so that even the pretence that this has something to do with the Cambridge Cavendish Laboratory in the 1930s, or any similar schoolteacher's model of scientific work, is wearing out. But such a change in the practice of science must sooner or later bring about a change in science (scientific knowledge) itself, since, as I argued before, scientific knowledge exists only in relation to its practice. This happened very early in mathematics, and the particular form which it took was the production of a completely new science, inside which socialisation and mechanisation were automatic, because guaranteed by a machine.

In the course of this change, a very important polarity for the bourgeois ideology of science has disappeared—the polarity of 'knowledge' and 'control' which underlies the separation between the roles of pure theoretician and engineer/designer (to take obvious extreme cases). In the 19th century, and in 20th-century 'old' mathematics, the pure mathematician *knows* a part of Nature; his knowledge is then available to others who may wish to *control* it. Hence—except in unusual circumstances—we have the ideology of free publication and diffusion of results, which distinguishes the first process from the second.

In the structure of new mathematics—the setting of a precise goal, the drawing up of one or more programmes to achieve it, their evaluation in terms of efficiency, and so on—the knowledge/control polarity seems to be absent. Precisely, the mathematics is centred around the achievement of *finite programmes of work*. And this says something about the kind of science and scientific ideology which, I think, is best adapted to the present stage of capitalism. The old-style Galilean ideology—the fearlessly independent investigator who cares only for scientific truth—could be absorbed via the knowledge/control model into a system of science and technology which benefited capitalism enormously in its early stages and is still doing so. Yet by the 1930s, as Colman saw, the inefficiency of the 'pure mathematics' component was acting as a brake on the productive forces. And under the pressure of the great expansion of those forces associated with the Second World War, not only a new mathematics but a new ideology of mathematical practice came into being. I feel sure that similar ideologies have arisen or will arise in other sciences as increasing financial stringency demands greater efficiency in problem-solving. But of course the form which the break—if there is one—will take in any particular science or in the founding of new sciences is much more unpredictable, and still more the 'feedback' which the existence of new sciences (new discoveries) will have on the future organisation of capitalist production and ideology.

Science as Politics

I've tried to give meaning to the statement 'science is ideological' and to suggest one for the statement 'science is determined by the needs of production'. Outside an academic context, one doesn't go around attaching meanings to statements just for fun. Even assuming that you agree that science under capitalism is not neutral, that the dominant function of scientific knowledge today is to be part of capitalism's resources in its continuing struggle for survival, politics doesn't stop there. In fact it begins, because this is where comrades start to disagree. How does the machine work? How best can we destroy it? To answer that, we need more than the general agreement we've had so far. Science is non-neutral in particular ways; science is ideological in a particular sense; certain powerful institutions, whether

or not one thinks of them as part of the bourgeois State, have a key function in keeping scientific practice bourgeois, in reproducing it, controlling it, continuing it. By thinking about these questions we can focus our attention on the political choices which the next step will demand and clarify our argument when we come to disagree about them. This is my interpretation of 'moving on in theory' as Bob Young calls it. It must exist and inform our moving on in practice. If we do believe science to be a central part of capitalism, then understanding it and changing it go hand in hand.

Notes

1. Here the early work of Lakatos has been fundamental, because it's readable, scientifically respectable (in that he clearly knew what he was describing) and convincing. See *Proofs and Refutations*.
2. Robinson proved that the free use of infinitesimal quantities like dx does not lead to contradictions, as it did in classical mechanics.
3. I'm thinking of works like Eric Hobsbawm and George Rudé's *Captain Swing*, Rudé's *Crowd in the French Revolution*, E.P. Thompson's *Making of the English Working Class* and many others, all produced in the last two decades.
4. There are counterexamples of 'stagnant' science (circulation without accumulation)—science taught by rote and transmitted from teacher to student over a long period. The analogy breaks down here, and more investigation is needed. But it certainly isn't applicable to science since 1800.
5. This corresponds quite closely to what Althusser calls 'spontaneous philosophy of scientists' in *Philosophie et Philosophie Spontanée des Savants* (p.99).
6. I am using the idea of the 'object' of a science as distinguishing the science, used in much of Althusser's work (particularly *Reading Capital*, p.157). For criticisms see the article by Geras already mentioned.
7. When I gave the original paper at BSSRS, it was suggested that the idea of 'simulation' was as good an indicator as 'subroutine' of the ideology of new mathematics, if not better. I'd agree in part, but the idea of simulation is less strictly mathematical—which is part of its great interest for studying current ideology (for example, its use by the Club of Rome in *Limits to Growth*).

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IQ as Ideological Reality

LES LEVIDOW

In the Race-IQ debate which has resurfaced in Britain and the USA since 1969, socialist critiques of IQ science have centred upon some notion or other of 'ideology'. That term has been invoked largely as an insult, intended to mean the opposite of 'truth'—as if for revolutionaries the questions were whether the claims made by IQ science are true or false, whether the claims are founded on reason or on mere rationalisation. If those were the questions, then our task would be to show that IQ science—by virtue of its technical defects, social class bias, political origins, etc.—violates the supposed rules and norms of scientific measurement. If so, then we could brand it 'ideological' and therefore 'unscientific', concocted solely to justify the existing society.

I want to argue, on the contrary, that revolutionaries need to ask instead *what kind of society IQ science helped to create.* This means asking what kinds of new relations between people IQ science helped to mediate, and therefore what kind of science it is and what kind of truth it is. And, as for its ideology, this means asking what kind of 'ability' it defines and reproduces, what kind of social relations it represents as somehow rooted in the nature of things and therefore as natural and eternal.

My approach intends ultimately to suggest that IQ testing, seen in historical perspective, was less a matter of justifying an existing capitalist society than of constructing a new stage of capitalism. It is with a similar approach that we will need to confront testing in Britain today, where—in the Great Debate over schooling—particular notions of 'basic skills' are gaining importance. Such rhetoric is directed less at justifying the present allocation (streaming) of pupils—be it 'meritocratic' or otherwise—than at representing so-called 'basic skills' as mere techniques for getting something done, as neutral facilities appropriate to any social circumstances. Accordingly, these 'thingified' techniques, as 'needs of industry', must presumably be more widely acquired by the future labour force in their (and society's) own best interests. So these 'basic skills', embodying the common or 'national' interest, are seen to be necessarily class-neutral because they are possessed apart from any particular social relations.

I want to argue, on the contrary, that we need to understand any

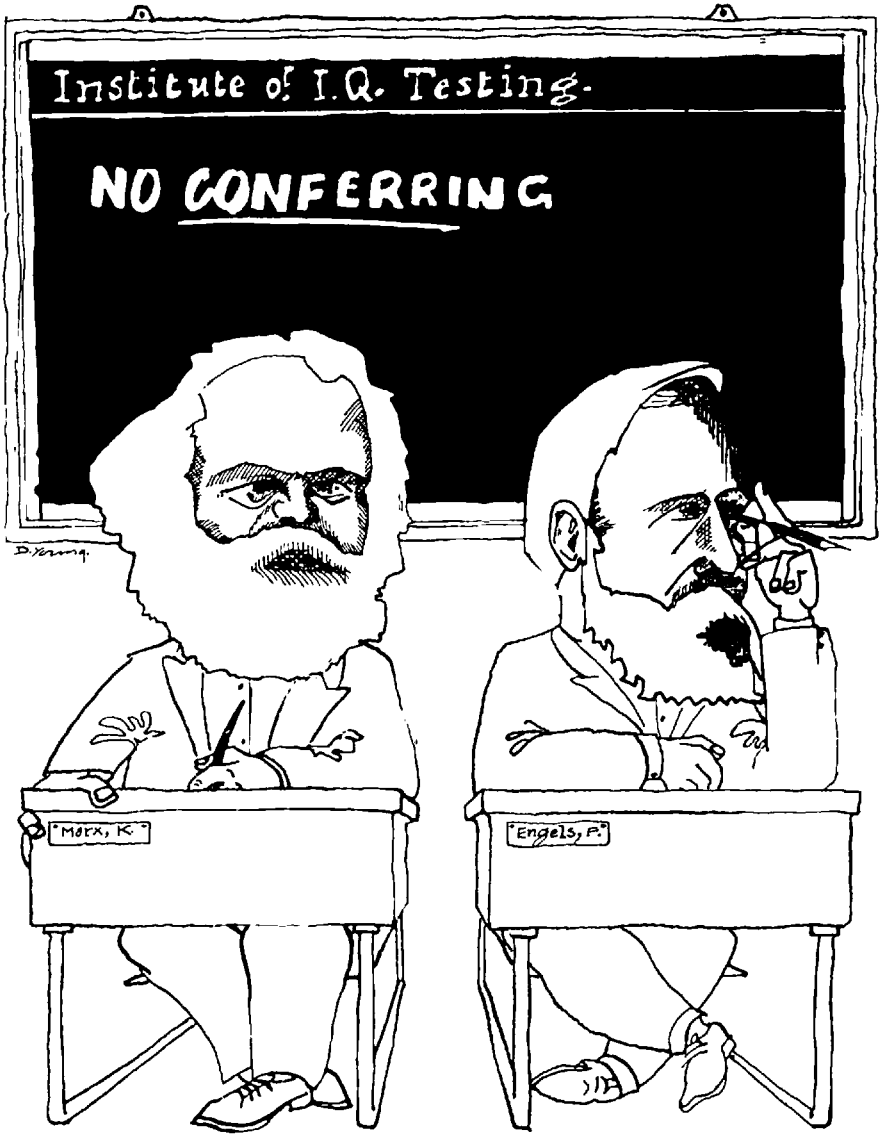
particular form of 'ability' not only as socially constituted—rather than simply justifying 'society' from outside of it—but also as contradictory, just as any social relations entail struggle. We need such an understanding so that we can recognise and build upon existing revolts against the social relations of capital's practices. It is precisely such revolts that provide occasions for seeing the way that the social relations (such as 'ability' itself) are normally mystified through science in the form of things—their possession, their properties, and their relations. So, through the example of the IQ debate, I will want to suggest that the belaboured distinction between scientific and ideological practices is at most a problem for capital, not for revolutionaries, because the project of overthrowing capitalism cannot itself be 'scientific'.

Marxism vs. Science

I will continue by explaining how I came to treat the IQ debate in such an unusual way. My interest in a critique of IQ, and of science as a whole, dates from the early 1970s, a time of difficult changes for the (American) New Left milieu of which I had been a part for several years. The days were over when the institutions, universities in particular, would go out of their way to accommodate people of our ilk. No longer students with a 1960s-era licence of 'repressive tolerance', we were moving up in academia, or out of it entirely. So we were faced with new sorts of settings in which to challenge our given roles—while at the same time needing to preserve them.

In other words, we were coming to terms with the problem of reordering our lives in a way that could extend our past radicalism rather than submerge it. For many of us, this meant quite ambivalent attempts to plan careers, or at least to give them a try. Despite our political commitment, we found ourselves falling into a normalisation of our lives, at least compared to the former fluidity. We were up against what seemed to be an 'objective necessity' imposed by the nature of jobs themselves, with the world threatening the bonds of comradeship on which we'd constructed our political identities.

At the same time, the Marxism that had been developed and rediscovered by the New Left's struggles was now in danger of becoming the exclusive property of the left sects, on the one hand, or of academic careerists, on the other. Whether this Marxism was an exhortation to party discipline or just another academic discipline, it claimed to determine objective truths about the material world out there, so that our own practices could be seen to be constrained by the very nature of things. In this historical shift, 'truth' was seen as a superior knowledge discovered by specialists according to a scientific method in which they were especially trained and qualified. Even where Marxism was connected to a sense of struggle, as


Institute of I.Q. Testing.
NO CONFERRING
"Marx, K."
"Engels, F."

in the left sects, its content was defined by ways of working together which reproduced much of the society that we'd set out to overthrow. In this degeneration, the rhetoric of 'scientific socialism' played a key role in disciplining ourselves not so much to real historical possibilities for collective struggle, but to 'objective laws of history', supposedly imposing their inescapable necessity upon us. The Marxism that we'd looked to for getting a political hold upon our lives was now helping to resign us to the narrow confines of the existing institutions.

My 'Short March through the Institutions'

From 1972 to 1974 I was one of many teaching assistants for an introductory biology course, as part of my work as a biology graduate student. In a largely unconscious way, there was a constant struggle over how I would relate to my students, as both teacher and evaluator, and how I would present the material. On one occasion I objected to the lecturer's use of particularly blatant capitalist metaphors (e.g. 'investment'), in the sense of challenging whether that's the way nature really is. Her reply was, for example, that certain economic concepts were simply the best pedagogical method for teaching the Krebs sugar oxidation cycle, especially for such urban students who had 'so little contact with nature', as she put it. 'Can you come up with a better method to teach the material?', she asked. Something was seriously lacking in my Marxism if my challenge could be effectively silenced by such a reply.

Only much later did I realise that she was actually 'right', in the sense that Sir Hans Krebs discovered the oxidation cycle in the 1930s through conceptual categories drawn from John Maynard Keynes, just as Darwin had drawn upon Malthus. So the problem is not how we interpret or reinterpret the facts of nature, but rather how a society constructs nature historically. But that's jumping ahead in our story.

My next institutional setting was a 'special education' school designed for secondary school students judged as 'emotionally disturbed' and/or 'socially maladjusted', but at the same time as cognitively 'normal'. It was therefore deemed a worthwhile task for society to invest in skilling and disciplining them into responsible and productive citizens.

Given the school's well-defined niche, the state-related testing procedures not only bore upon administrative judgements about new admissions, students' progress and transfers, but also held a pervasive presence in the school's everyday affairs. That is, the testing was merely a more formalised version of how we tended to treat the students as repositories of individual responsibility and skills to be developed for their individual self-interest as future employees and consumers. It was our job constantly to counterpose that 'interest' to their mischievous impulses—whose irreverence often attracted me but usually left me unable to respond other than by defending my own authority role.

While holding no naively grand illusions of being a 'socialist' teacher in a capitalist society, I certainly had hoped to teach science as something other than a collection of facts. I expected to teach science differently, through a process of developing experiments to answer questions about the world. Little did I realise, though—and so learned the hard way—that the testing paradigm actually defined what counted as knowledge. As it turned out, the students felt threatened by anything but the familiar 'learning' by rote, and they didn't hesitate to show their hostility to my attempts at teaching differently.

At the same time, with my difficulty at coming across to them personably—a difficulty compounded by my anxieties over the need to maintain control—the students felt bored by almost anything conventional, often to the point of simply ignoring me. They also took full advantage of the ambivalence with which I demanded their attention and handed them moralistic imperatives (much to my own surprise!).

As a modus vivendi, I somehow managed eventually to devise a formal teaching method where my 'personal' relation to them centred upon my commending them, in effect, for submitting to a particular work discipline. By having them do simple readings together as a group, I was able to give all of them the chance to prove themselves 'good students' by diligently finding the right answers to fairly mechanical questions. So, at least on the surface, they were 'learning'—even doing 'real science', as they put it—and I was managing to 'get results' without having to shout at them so much.

But any interest they displayed in the work—or in me personally—was bound up with their desire (however ambivalent) to be judged as worthwhile people by me, as the official dispenser of commendation. So the personal rapport was itself a lie, albeit a 'necessary' one for me, as a welcomed relief from the previous chaos and (literal) nightmares. But with my nascent 'professional success', I came to be taken over by that lie as a deadening, alienating routine which I came to resent at least as much as the students did.

The Radical Science Movement

It was from such confines that I began to turn to the radical science movement, whose activists were attempting to confront their own roles as scientific workers. The great stumbling block that I came up against from the very start was the movement's tendency to assume that there is an essence of science exempt from capitalism, or at least potentially so. They saw capitalism as abusing science or misrepresenting reality through science. The radical project, then, was to defend science from penetration by capitalist ideology and from abuse by capitalist applications. In other words, there was a pure core or method of true science which we needed to distinguish and salvage from all else.

So they attacked the more blatantly objectionable aspects of science

by invoking a narrow rhetoric of deviations from science, with insults such as 'pseudo-science', 'abuse of science', or even 'bad science'. Despite the epithets, some imaginative insights were developed, but as a whole this approach proved most unhelpful for getting a hold upon our intimidation by science, seen as a body of ideas or methods. With such an approach, we could claim only that capitalism was misinterpreting the facts, which were of course neutral, value-free products of scientific experiments. Missing here was any critique of how the facts themselves were produced by *practices* which are capitalist.

In particular, the IQ debate intrigued me as a symptom of how we remained entrapped by capitalist categories of practice and knowledge. IQ presented an enigma to any model that saw facts as simply having values and applications added onto them, because with such a model we could go no further than challenging the scientific rigour of experimental facts and their interpretation. We were stuck with an endless dispute over statistical methods and results.

To get out of that morass, we would have to claim that IQ scores were themselves ideology. But such a claim tended to be put in terms of IQ testing as a con, in that it's not at all about 'measuring' abilities (or anything at all) but merely a ruse to allocate individuals in the capitalist hierarchy—for example, by 'testing' for compliability or respectability. This means that IQ would be distorting or ignoring the true material reality of abilities, in favour of justifying an entirely different material reality.

But if we deny, in effect, that IQ is real, then how could we explain the successful transmission of the IQ ideology? To say that capitalism needs some way of justifying its hierarchy is to reduce IQ testing to a more or less arbitrary ruse. It is also to explain the credibility of IQ in an idealist way, in terms of the power of ideology—as mere ideas—brought in from the outside of a situation. Clearly we needed a better analysis to solve the enigma.

> For experiments upon young and untrained subjects such as ours, there is a further advantage in using none but the simplest apparatus. To boys, strange apparatus is distracting. Clock-work mechanism arouses irrelevant interests. Electric wires and keys inspire needless apprehensions. Consequently, in dispensing with elaborate instruments, the sacrifice of the mechanical regulation of objective conditions is often more than compensated by the exclusion of subjective irregularities and unstable attitudes of mind.
>
> In the actual course of the experiment, the personality of the operator was relegated to the background by making our procedure as mechanical as could be, using a written form of instructions to be recited to each boy at the beginning of every new test...
>
> —Cyril Burt, 'Experimental Tests of General Intelligence', *British Journal of Psychology, 3* (1909)

IQ as Social Relations

Back then my intuition for a way out of this quandary—an intuition which I've since developed through collective work—was to see IQ as a social relation which is mystified by the way it is represented, so that it can be both real and ideological. But to work out this problem, we had to grasp science as practices which define entire forms of knowledge, by way of constructing nature in particular ways. By this method, we could see that facts contain both values and applications, since they presume certain social relations of production.

As I was in the process of studying the history of IQ, one of my breakthroughs came from an incident that occurred during my one year of science teaching at a secondary school whose pupils were all black or Puerto Rican. I happened to be present while the school psychologist was administering the Wechsler test to one of the black pupils. To the question, 'What is the thing to do if you find someone else's purse?', the pupil grinned and replied, 'Do you want the whitey answer or the nigger answer?' I couldn't help but burst out laughing, and the pupil did as well. But the psychologist remained unmoved. So the pupil broke the awkward silence by capitulating; he asked the psychologist to repeat the question, so as to re-establish the formal test situation. The psychologist complied, so that the 'measurement of intelligence' resumed according to the rules.

There are many such examples of recalcitrance by testees, such as native American ('Indian') children who refuse to answer a test question to which other children present may not know the answer, so as not to shame them; or children refusing to indicate that they've found the answer until all the other children present are ready to do so. In the incident I've described, the pupil was isolated, apart from my laughter, and so had to draw upon his cultural identity in a more explicitly verbal way to defend himself from a question that probably struck him as an assault upon his identity.

Now, these examples of recalcitrance tend to be seen at most as evidence of test bias, in the sense that such test questions cannot accurately measure the intelligence of any but white middle-class children. Some critics have gone a bit further by locating the problem in the test situation itself as one alien to other cultures. However, even the most subtle critiques tend to reduce cultural considerations to 'social factors' that impinge upon the test from the outside and thereby distort the test score from its otherwise true, fair value.

It is rarely suggested that the test situation—indeed, the power relation between tester and testee—is integral to the alleged 'measurement of intelligence'. I want to argue precisely that: i.e., that IQ testing is no matter of a subject determining facts about an object, but is rather a particular social form of mental labour. In the incident I described, the pupil subverted his role as producer of answers to alien hypothetical questions, and tried instead

to make human contact with the tester, the intended recipient of answers. But the tester, as a personification of capital, couldn't take the joke, except at the price of surrendering control over the test situation. My telling of the incident is intended to suggest that 'intelligence' is a social relation, and that it's precisely the revolt against that relation which helps us to reveal the mystifying processes underlying the normal test situation, which we might otherwise take for granted.

To understand IQ testing as ideological, in any meaningful sense of the word, I found it necessary to draw upon the critical concepts that Marx developed for understanding how commodity exchange could be both ideological and 'objectively' real. As it turns out, the act of commodity exchange abstracts not from the material to the ideal but *within* material reality, so that material reality itself could be ideological, by virtue of the way it is socially reproduced. So it is within the act of commodity exchange itself that Marx could locate the material basis for both political economy and his critique of it as ideological. Let us briefly sketch his critical method.

Commodities can be exchanged only on the basis of their having different use values, and therefore containing different types of labour. However, the act of commodity exchange entails abstracting from those different qualities their common quality of embodying human labour in the abstract. This 'exchange abstraction' is not done intentionally or consciously, but simply by virtue of exchanging commodities in definite proportions according to the respective amount of average socially necessary labour they contain.

In this way, the equivalent 'exchange values', derived from equal quantities of dead labour, can take the mystified form of a physical property of the commodities themselves. Exchange value itself appears to derive from the physical nature of things, because the social relations between commodity producers take the form of a relation between things. In this way, historically specific social relations take a social form—exchange value—which represents those relations as natural.

> Equality in the full sense between different kinds of labour can be arrived at only if we abstract from their real inequality, if we reduce them to the characteristic they have in common, that of being the expenditure of human labour in the abstract.
> —Karl Marx, *Capital*, Vol. I, p.166

Through generalised commodity production, and then wage-labour, labour power itself becomes a commodity. Unlike other commodities, this labour power (capacity to work) must be exploited by the capitalist by setting it to work. Nevertheless, like other commodities, labour power is exchanged

at its value, determined by the average socially necessary labour time required to reproduce it.

However, wage-labour (and therefore political economy) mystifies this exchange by representing wages as the 'value of labour'. This is an ideological term because it assigns an inherent value to a particular sort of labour, the magnitude of its value arising from its physiological properties. The 'value of labour' mystifies the selling of one's labour power, as if it were fundamentally similar to an individual commodity producer selling the finished product of his/her labour.

The material basis for this mystification lies in capitalism's historical tendency to treat labour power as indifferent to any particular sort of labour. In other words, capitalist wage-labour abstracts from the particular qualities of labour its universal quality as labour power in the abstract. As with commodity exchange, people do not consciously set out from different types of labour power (or use values) and then ideally abstract out equivalent exchange values; on the contrary, by selling their labour for a wage, people daily engage in an 'exchange abstraction' without necessarily realising it. This is because capitalism regulates labour not directly, according to the particular 'use values' produced, but, rather, indirectly, according to the comparative exchange values produced by different labours.

> ... a social relation, a definite relation between individuals, here appears as a metal, a stone, as a purely physical, external thing.
> —*Grundrisse*, p.239

So wage-labour, the sale of and setting to work of labour power, is a relation of class struggle which takes the form of a thing, the 'value of labour'. This is as if what's being exchanged were a fixed quantity of labour, assigned a value by virtue of its physiological properties—hence, the 'rate for the job' and 'a fair day's work'. So here, as with commodity exchange, a social relation takes the form of a thing or a natural property of a thing. In this example, an exchange abstraction within material reality permits us to locate ideology *within* that reality.

The IQ Debate Proper

If we now return to IQ testing, then we can employ a similar method to analyse 'things' in terms of the social relations which both produce them and mystify them—in this case, to analyse 'intelligence' as mental labour in the abstract. When I speak here of a 'similar method', I do not at all intend to equate IQ scores with commodities, as if the reproduction of labour power for capital—in schooling, family, etc.—had to be modelled exactly

upon commodity exchange itself. But neither do I intend a merely formal analogy between IQ testing and commodity exchange (or between IQ scores and exchange value), since my purpose certainly is to grasp IQ—and the entire IQ debate—as a mediation of capital.

To do that, I will examine three of the issues which have arisen in the IQ debate concerning the measurement of IQ, its causes, and its effects. These issues actually tend to arise in a much cruder form; I have tried here to formulate them in the most sophisticated way possible so as to lend my critique the broadest application.

(1) 'How well does the IQ test measure intelligence?'
This question assumes that 'intelligence' is a pre-existing property already residing in the testee, and then asks how well a 'test'—as a thing—can determine the amount of that property. Furthermore, in this question the power relation between tester and testee takes the form of the power of the 'test' to determine something about the testee.

This mystification is not merely a distortion of material reality, but has its material basis in the testing process itself. Namely, the social context of the test situation does not call up test answers for any immanent concrete purpose, nor certainly for any immanent interest of the testee. (Nor does the wage-labourer's sale of labour power require the seller or buyer to have any interest in the concrete use-values produced.) Rather, the sole purpose of the test answers is for them to be judged against a pre-defined standard for comparison with other individuals' answers. By this process, the mental labour by which answers are socially constructed abstracts from all particular qualities the universal quality of mental labour in the abstract. It is by comparing testees' scores that their qualitatively different mental labours are equated as abstract mental labour.

This is not simply an ideal abstraction from material reality, but an abstraction within material reality. In other words, mental labour of different individuals is not first compared directly and then subjected to an ideal abstracting out of a universal quality. Rather, the mental labour is compared indirectly, by relating different individuals according to their respective quantity of abstract mental labour.

In this way, an ordinal scale of intelligence can assign positions to individuals according to the respective amounts of 'intelligence' that each possesses. (Likewise the 'value of labour' mystifies the exchange value of labour power as the physiological property of a thing.) In IQ testing the social relations of the production of answers, or the expenditure of mental labour power, takes the form of a quantifiable property of the testee, a quantity of a thing to be 'measured' by the test. So to ask how well the IQ test 'measures' intelligence is to accept the ideology of IQ testing, which represents 'intelligence' as a natural, quantifiable quality of individuals; it thereby represents mental labour as naturally a separate thing from manual

labour on the one hand and from social relations on the other. We might just as well ask how well wages 'measure' the 'value of labour'.

Next, let us move on to a question as to the possible causes of IQ, taken as a particular quantity of intelligence.

> What does a solely quantitative difference between things presuppose? The identity of their qualities. Hence, the quantitative measure of labours presupposes the equivalence, the identity of their quality.
> —*Grundrisse*, p.173

(2) 'How much of individual variation in IQ is due to heredity and how much to environment?'

As in the previous question about the measurement of intelligence, this question also takes intelligence as a natural quality of individuals differing in the quantity thereof. The question then proceeds to ask what determines those differing quantities, as products of two causal factors. With the category of 'environment', the social relations through which individuals construct their future selves take the form of a thing impinging upon them from the outside, beyond their immediate control. With heredity, genes are taken as naturally empowered to contribute quantitatively to the quality of 'intelligence'. Then, quantities of these two causal factors are seen to add together—or interact—to produce a quantity of yet another thing, IQ.

In this way, heredity and environment are separated out into the 'natural' and the 'social', as two separate things which then meet in the individual's development. The quality of intelligence, in reality a social construct, is projected onto nature—not as the social construction of nature by a particular society, but as an inherent property of genes, as a purely a-social thing, which then interacts with social things to produce a technical thing, a quantity of intelligence. Just as it is ideological for the political economists to ask what parts are played by nature (and society, respectively) in the determination of exchange value, so it is ideological to ask how much of the variation in IQs is due to heredity, *even if* the empirically 'discovered' answer is zero.

> The degree to which some economists are misled by the fetishism attached to the world of commodities, or by the objective appearance of the social characteristics of labour, is shown, among other things, by the dull and tedious dispute over the part played by nature in the formation of exchange-value. Since exchange-value is a definite social manner of expressing the labour bestowed upon a thing, it can have no more natural content than has, for example, the rate of exchange.
> —*Capital*, I, p.176

Next, let us move on to a question about the possible effects of IQ.

(3) 'How much of the inter-generational transmission of economic success is due to the transmission of cognitive ability?'

This question warrants some elaboration before demolishing it. Given that socio-economic status is somehow 'transmitted' from parents to their children, how much of that transmission across generations is due to a transmission of cognitive abilities? To its credit, this question intentionally begs the question as to how much those abilities are 'acquired' by heredity or environment. Because, if it could be shown that the inter-generational transmission of economic success is independent of cognitive ability, then raising IQs would not necessarily raise economic success. So the entire nature/nurture controversy over IQ would be rendered utterly irrelevant to any question of social policy aimed at economic equality.

However, the problem is that this question is concerned with the allocation of individuals to positions in a socio-economic hierarchy. It asks, in effect, whether such allocation is done according to cognitive abilities or instead according to personal ('social') traits. In so asking, the question separates out cognitive abilities from 'social' ones, as if technical abilities were not social as well. By defining such abilities as a property of individuals, the question must satisfy its own empirical requirements by taking IQ scores as a quantitative 'measure' of cognitive abilities.

As for economic success, also defined as a property of individuals (literally), this must likewise have its empirical requirements met by quantifying it as 'socio-economic status'. Instead of looking at the social relations by which labour power is reproduced, the question accepts the way that those relations take the form of 'status' (or distribution), as a quantifiable thing. In this question the only possible significance of cognitive ability is as a particular quantity of individual property, which may or may not be the cause of each individual's having a particular quantity of yet another property, status. The question doesn't ask whether cognitive abilities and socio-economic status might be related by virtue of their both being relations of the reproduction of labour power, social relations which take a similarly mystified form. Instead, the question simply takes for granted their social form, and asks to what extent the quantity of one thing (IQ) causes a quantity of another thing (socio-economic status).

> The human essence is no abstraction inherent in each individual. In its reality it is the ensemble of social relations.
> —Karl Marx, 6th Thesis on Feuerbach

The Historical 'Truth' of IQ

I have cited these three examples from the IQ debate to suggest that, in class society, material reality (including nature) tends to *be* ideological by virtue of the way that the reality is socially constructed and reproduced. That reality gives meaning to particular forms of knowledge, rendering them 'true', since they 'work', yet at the same time historically limited, since their truth depends upon and reinforces the particular power relations that constitute them.

In capitalism in particular, the forces of production (humans' relation to nature) take the form of merely technical things—technology, labour power, even 'ability'. The relations of production (humans' relation to each other) are seen to follow inexorably from the inherent properties of those 'thingified' forces. For example, commodity exchange follows on from the exchange value of the products of labour; wage-labour follows from the 'value of labour'; and divisions of mental/manual labour follow from the hierarchical ranking of IQ scores, the ordinal scale of 'intelligence'.

With the new science of IQ testing, historically new divisions of mental/ manual labour could be attributed to the very nature of mental labour. Of course, this 'nature' was not being simply 'discovered' but actively constructed, as a more interchangeable, technically defined abstract labour power. In more subtle ways, the rise of IQ testing could be connected to Edwardian liberalism in Britain and Progressivism in the USA, especially the way that each recast the individual as citizen/producer contributing efficiently to the national good, and as consumer needing to be serviced scientifically. Indeed, in that same historical period there was crystallised a new middle class whose model for practice was the professional-client relationship taking the form of an exchange of technically defined consumption needs (including information itself), in principle independent of the particular people involved. In this way, IQ testing could provide a material/ideological basis for the 20th-century meritocracy, the occupational hierarchy of abstract 'mental ability' possessed by individuals.

To the extent that the left protests at the 'inequalities' of that meritocracy—or its 'unscientific' basis—such protest unwittingly claims the legacy of IQ testing as its own. By demanding the full and free development of all individuals to their fullest potential—even if in the name of socialism— the left attempts somehow to extract the 'virtues' of bourgeois society from its nasty distorting defects. And more broadly, by speaking of objective conditions and forces, historical imperatives, and technical requirements—all abstractly 'thingified' apart from our own social existence in the capitalist order—the left reproduces capital's science, the knowledge that informs the extension of capitalist social relations.

Forefathers of IQ Testing

Wherever you can, measure or count.
—Francis Galton, *The Art of Travel*, 1855.

Dr. Eliot Slater describes both Darwin and Galton as showing a "compulsive curiosity" and the Galton family as exhibiting an "obsessional desire to count or classify".

Dr. Slater very justly noted the fact that, despite his "feminine streak" and "acute sensibilities", Galton seems to have been almost wholly devoid of aesthetic interests: "art, whether in colour or form, was no essential need for him"; he was totally unmusical; "and during his travels quite unimpressed by the magnificent scenery".

In Damaraland he met a typical Hottentot Venus whose buttocks were so immense that he felt they should be accurately measured. Yet any such approach was found to be misinterpreted. He resolved his dilemma by the device employed by Gulliver's Laputian tailor: "with the aid of navigational instruments" he measured the lady's dimensions "from a distance".
—Sir Cyril Burt, 'Francis Galton and his Contribution to Psychology', *British Journal of Statistical Psychology*, *15* (1962), pp.9, 10.

...the influence of a good environment appears most conspicuous where the influence of good heredity is also most conspicuous. There is an obvious practical corollary: it is far more urgent to provide brighter children with an education appropriate to the ability of each than to do so for the dull, the backward or the defective.
—Sir Cyril Burt, 'Ability and Income', *British Journal of Educational Psychology*, *12* (1943), pp.83-98.

The general fact is that our limited intellects can only hope to deal with the infinite complexity of Nature after analyzing it down into its bare unaesthetic elements.
—Charles Spearman, 'General Intelligence, Objectively Determined and Measured', *American Journal of Psychology*, *15* (1904), pp.201-92.

> This example of labour shows strikingly how even the most abstract categories—despite their validity for all epochs (precisely because of their abstractness)—are nevertheless, in the specific character of this abstraction, themselves likewise a product of historic relations, and possess their full validity only for and within these relations.
> —Karl Marx, *Grundrisse*, pp.104-5

For revolutionaries the task is to create practices which attempt to make our own constituent power relations historically self-conscious and transparent. Such a project develops methods of collective work which avoid reproducing professional or scientific 'expertise' in the social form of competitive private property. If we do reproduce it, then our allegedly revolutionary theory becomes just another academic discipline or a 'correct line' about the objective world 'out there', divorced from any struggle against our own material relation to capital. Revolutionary theory cannot imitate the virtues of capital's science but must inform our struggle against the power relations that make that science 'true'.

Bibliography

This essay is reprinted from *Radical Philosophy*, as cited below, and is based on a talk given at the magazine's January 1978 conference entitled 'Philosophy and the Critique of Ideology'. That talk was, in turn, based upon the more detailed argument made in *RSJ*, which also contains a more extended bibliography; in particular the *RSJ* article systematically criticises the texts by Steven Rose and by Bowles and Gintis, cited below; partly by paraphrasing Marx's critique of commodity fetishism.

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Why Socialism?

ALBERT EINSTEIN

Is it advisable for one who is not an expert on economic and social issues to express views on the subject of socialism? I believe for a number of reasons that it is.

Let us first consider the question from the point of view of scientific knowledge. It might appear that there are no essential methodological differences between astronomy and economics: scientists in both fields attempt to discover laws of general acceptability for a circumscribed group of phenomena in order to make the interconnection of these phenomena as clearly understandable as possible. But in reality such methodological differences do exist. The discovery of general laws in the field of economics is made difficult by the circumstance that observed economic phenomena are often affected by many factors which are very hard to evaluate separately. In addition, the experience which has accumulated since the beginning of the so-called civilised period of human history has—as is well known—been largely influenced and limited by causes which are by no means exclusively economic in nature. For example, most of the major states of history owned their existence to conquest. The conquering peoples established themselves, legally and economically, as the privileged class of the conquered country. They seized for themselves a monopoly of the land ownership and appointed a priesthood from among their own ranks. The priests, in control of education, made the class division of society into a permanent institution and created a system of values by which the people were thenceforth, to a large extent unconsciously, guided in their social behaviour.

But historic tradition is, so to speak, of yesterday; nowhere have we really overcome what Thorstein Veblen called 'the predatory phase' of human development. The observable economic facts belong to that phase and even such laws as we can derive from them are not applicable to other phases. Since the real purpose of socialism is precisely to overcome and advance beyond the predatory phase of human development, economic science in its present state can throw little light on the socialist society of the future.

Second, socialism is directed toward a social-ethical end. Science, however, cannot create ends and, even less, instill them in human beings;

science, at most, can supply the means by which to attain certain ends. But the ends themselves are conceived by personalities with lofty ethical ideals and—if these ends are not stillborn, but vital and vigorous—are adopted and carried forward by those many human beings who, half-unconsciously, determine the slow evolution of society.

For these reasons, we should be on our guard not to overestimate science and scientific methods when it is a question of human problems; and we should not assume that experts are the only ones who have a right to express themselves on questions affecting the organisation of society.

Innumerable voices have been asserting for some time now that human society is passing through a crisis, that its stability has been gravely shattered. It is characteristic of such a situation that individuals feel indifferent or even hostile toward the group, small or large, to which they belong. In order to illustrate my meaning, let me record here a personal experience. I recently discussed with an intelligent and well-disposed man the threat of another war, which in my opinion would seriously endanger the existence of mankind, and I remarked that only a supranational organisation would offer protection from that danger. Thereupon my visitor, very calmly and coolly, said to me: 'Why are you so deeply opposed to the disappearance of the human race?'

I am sure that as little as a century ago no one would have so lightly made a statement of this kind. It is the statement of a man who has striven in vain to attain an equilibrium within himself and has more or less lost hope of succeeding. It is the expression of a painful solitude and isolation from which so many people are suffering in these days. What is the cause? Is there a way out?

It is easy to raise such questions, but difficult to answer them with any degree of assurance. I must try, however, as best I can, although I am very conscious of the fact that our feelings and strivings are often contradictory and obscure and that they cannot be expressed in easy and simple formulas.

Man is, at one and the same time, a solitary being and a social being. As a solitary being, he attempts to protect his own existence and that of those who are closest to him, to satisfy his personal desires and to develop his innate abilities. As a social being, he seeks to gain the recognition and affection of his fellow human beings, to share in their pleasures, to comfort them in their sorrows, and to improve their conditions of life. Only the existence of these varied, frequently conflicting strivings accounts for the special character of a man, and their specific combination determines the extent to which an individual can achieve an inner equilibrium and can contribute to the well-being of society. It is quite possible that the relative strength of these two drives is, in the main, fixed by inheritance. But the personality that finally emerges is largely formed by the environment in which a man happens to find himself during his development, by the structure of the society in which he grows up, by the tradition of that society, and

by its appraisal of particular types of behaviour. The abstract concept 'society' means to the individual human being the sum total of his direct and indirect relations to his contemporaries and to all the people of earlier generations. The individual is able to think, feel, strive and work by himself; but he depends so much upon society—in his physical, intellectual, and emotional existence—that it is impossible to think of him, or to understand him, outside the framework of society. It is 'society' which provides man with food, clothing, a home, the tools of work, language, the forms of thought and most of the content of thought; his life is made possible through the labour and the accomplishments of the many millions past and present who are all hidden behind the small word 'society'.

It is evident, therefore, that the dependence of the individual upon society is a fact of nature which cannot be abolished—just as in the case of ants and bees. However, while the whole life process of ants and bees is fixed down to the smallest detail by rigid, hereditary instincts, the social pattern and interrelationships of human beings are very variable and susceptible to change. Memory, the capacity to make new combinations, the gift of oral communication have made possible developments among human beings which are not dictated by biological necessities. Such developments manifest themselves in traditions, institutions, and organisations; in literature; in scientific and engineering accomplishments; in works of art. This explains how it happens that, in a certain sense, man can influence his life through his own conduct, and that in this process conscious thinking and wanting can play a part.

Man acquires at birth, through heredity, a biological constitution which we must consider fixed and unalterable, including the natural urges which are characteristic of the human species. In addition, during his lifetime, he acquires a cultural constitution which he adopts from society through communication and through many other types of influences. It is this cultural constitution which, with the passage of time, is subject to change and which determines to a very large extent the relationship between the individual and society. Modern anthropology has taught us, through comparative investigation of so-called primitive cultures, that the social behaviour of human beings may differ greatly, depending upon prevailing cultural patterns and the types of organisation which predominate in society. It is on this that those who are striving to improve the lot of man may ground their hopes: human beings are *not* condemned, because of their biological constitution, to annihilate each other or to be at the mercy of a cruel, self-inflicted fate.

If we ask ourselves how the structure of society and the cultural attitude of man should be changed in order to make human life as satisfying as possible, we should constantly be conscious of the fact that there are certain conditions which we are unable to modify. As mentioned before, the biological nature of man is, for all practical purposes, not subject to change. Furthermore, technological and demographic developments of the last few

centuries have created conditions which are here to stay. In relatively densely settled populations with the goods which are indispensable to their continued existence, an extreme division of labour and a highly centralised productive apparatus are absolutely necessary. The time—which, looking back, seems so idyllic—is gone forever when individuals or relatively small groups could be completely self-sufficient. It is only a slight exaggeration to say that mankind constitutes even now a planetary community of production and consumption.

I have now reached the point where I may indicate briefly what to me constitutes the essence of the crisis of our time. It concerns the relationship of the individual to society. The individual has become more conscious than ever of his dependence upon society. But he does not experience this dependence as a positive asset, as an organic tie, as a protective force, but rather as a threat to his natural rights, or even to his economic existence. Moreover, his position in society is such that the egotistical drives of his make-up are constantly being accentuated, while his social drives, which are by nature weaker, progressively deteriorate. All human beings, whatever their position in society, are suffering from this process of deterioration. Unknowingly prisoners of their own egotism, they feel insecure, lonely, and deprived of the naïve, simple, and unsophisticated enjoyment of life. Man can find meaning in life, short and perilous as it is, only through devoting himself to society.

The economic anarchy of capitalist society as it exists today is, in my opinion, the real source of the evil. We see before us a huge community of producers the members of which are unceasingly striving to deprive each other of the fruits of their collective labour—not by force, but on the whole in faithful compliance with legally established rules. In this respect, it is important to realise that the means of production—that is to say, the entire productive capacity that is needed for producing consumer goods as well as additional capital goods—may legally be, and for the most part are, the private property of individuals.

For the sake of simplicity, in the discussion that follows I shall call 'workers' all those who do not share in the ownership of the means of production—although this does not quite correspond to the customary use of the term. The owner of the means of production is in a position to purchase the labour power of the worker. By using the means of production, the worker produces new goods which become the property of the capitalist. The essential point about this process is the relation between what the worker produces and what he is paid, both measured in terms of real value. In so far as the labour contract is 'free', what the worker receives is determined not by the real value of the goods he produces, but by his minimum needs and by the capitalists' requirements for labour power in relation to the number of workers competing for jobs. It is important to understand that even in theory the payment of the worker is not determined by the value

of his product.

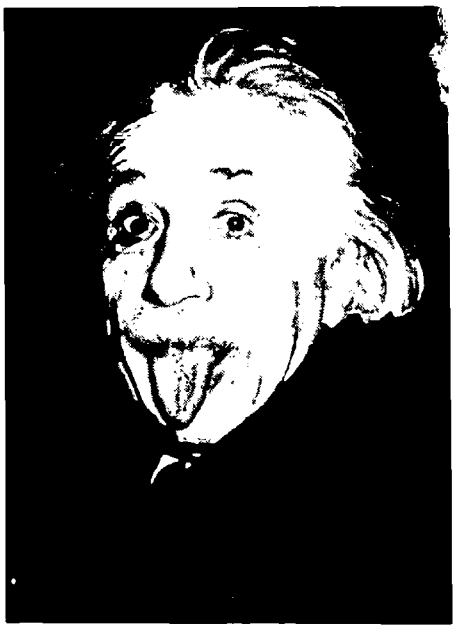
Private capital tends to become concentrated in few hands, partly because of competition among the capitalists, and partly because technological development and the increasing division of labour encourage the formation of larger units of production at the expense of the smaller ones. The result of these developments is an oligarchy of private capital the enormous power of which cannot be effectively checked even by a democratically organised political society. This is true since the members of legislative bodies are selected by political parties, largely financed or otherwise influenced by private capitalists who, for all practical purposes, separate the electorate from the legislature. The consequence is that the representatives of the people do not in fact sufficiently protect the interests of the underprivileged sections of the population. Moreover, under existing conditions, private capitalists inevitably control directly or indirectly the main sources of information (press, radio, education). It is thus extremely difficult, and indeed in most cases quite impossible, for the individual citizen to come to the objective conclusions and to make intelligent use of his political rights.

The situation prevailing in an economy based on the private ownership of capital is thus characterised by two main principles: first, means of production (capital) are privately owned and the owners dispose of them as they see fit, second, the labour contract is free. Of course, there is no such thing as a *pure* capitalist society in this sense. In particular, it should be noted that the workers, through long and bitter political struggles, have succeeded in securing a somewhat improved form of the 'free labour contract' for certain categories of workers. But taken as a whole, the present-day economy does not differ much from 'pure' capitalism.

Production is carried on for profit, not for use. There is no provision that all those able and willing to work will always be in a position to find employment; an 'army of unemployed' almost always exists. The worker is constantly in fear of losing his job. Since unemployed and poorly paid workers do not provide a profitable market, the production of consumers' goods is restricted, and great hardship is the consequence. Technological progress frequently results in more unemployment rather than in an easing of the burden of work for all. The profit motive, in conjunction with competition among capitalists, is responsible for an instability in the accumulation and utilisation of capital which leads to increasingly severe depressions. Unlimited competition leads to a huge waste of labour, and to that crippling of the social consciousness of individuals which I mentioned before.

This crippling of individuals I consider the worst evil of capitalism. Our whole educational system suffers from this evil. An exaggerated competitive attitude is inculcated into the student, who is trained to worship acquisitive success as a preparation for his future career.

I am convinced there is only *one* way to eliminate these grave evils,



On his 72nd birthday, photographers asked Einstein to 'smile for his birthday'.

namely through the establishment of a socialist economy, accompanied by an educational system which would be oriented toward social goals. In such an economy, the means of production are owned by society itself and are utilised in a planned fashion. A planned economy, which adjusts production to the needs of the community, would distribute the work to be done among all those able to work and would guarantee a livelihood to every man, woman and child. The education of the individual, in addition to promoting his own innate abilities, would attempt to develop in him a sense of responsibility for his fellow-men in place of the glorification of power and success in our present society.

Nevertheless, it is necessary to remember that a planned economy is not yet socialism. A planned economy as such may be accompanied by the complete enslavement of the individual. The achievement of socialism requires the solution of some extremely difficult socio-political problems: how is it possible, in view of the far-reaching centralisation of political and economic power, to prevent bureaucracy from becoming all-powerful and overweening? How can the rights of the individual be protected and therewith a democratic counterweight to the power of bureaucracy be assured?

Albert Einstein
On Being a Scientist

You have asked me what I thought about your articles concerning the situation of scientists in America. Instead of trying to analyze the problem, I should like to express my feeling in a short remark: If I were a young man again and had to decide how to make a living, I would not try to become a scientist or scholar or teacher. I would rather choose to be a plumber or a peddler, in the hope of finding that modest degree of independence still available under present circumstances.
—18 November 1954, from a statement in *The Reporter*

The hired hacks of an accommodating press have tried to tone down the impact of this statement, either by making it appear as if I regretted having been engaged in scientific endeavour, or by trying to give the impression that I attached little value to the practical occupations I mentioned.

What I wanted to say was this: In the present circumstances, the only profession I would choose would be one where earning a living had nothing to do with the search for knowledge.
—17 January 1955, from a letter to Max Born

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