The Problem

The activity of modern natural science has transformed our knowledge and control of the world about us; but in the process it has also transformed itself; and it has created problems which natural science alone cannot solve. Modern society depends increasingly on industrial production based on the application of scientific results; but the production of these results has itself become a large and expensive industry; and the problems of managing that industry, and of controlling the effects of its products, are urgent and difficult. All this has happened so quickly within the past generation, that the new situation, and its implications, are only imperfectly understood. It opens up new possibilities for science and for human life, but it also presents new problems and dangers. For science itself, the analogies between the industrial production of material goods and that of scientific results have their uses, and also their hazards. As a product of a socially organized activity, scientific knowledge is very different from soap; and those who plan for science will neglect that difference at their peril. Also, the understanding and control of the effects of our science-based technology present problems for which neither the academic science of the past, nor the industrialized science of the present, possesses techniques or attitudes appropriate to their solution. The illusion that there is a natural science standing pure and separate from all involvement with society is disappearing rapidly; but it tends to be replaced by the vulgar reduction of science to a branch of commercial or military industry. Unless science itself is to be debased and corrupted, and its results used in a headlong rush to social and ecological catastrophe, there must be a renewed understanding of the very special sort of work, so delicate and so powerful, of scientific inquiry.

If we are to achieve the benefits of industrialized science, and avert its dangers, then both the common-sense understanding of science and the disciplined philosophy of science will need to be modified and enriched. As they exist now, both have come down from periods when the conditions of work in science, and the practical and ideological problems encountered by its proponents, were quite different from those of the present day. Science is no longer a marginal pursuit of little practical use carried on by a handful of enthusiasts; and it no longer needs to justify itself by a direct answer to the challenge of other fields of knowledge claiming exclusive access to truth. As the world of science has grown in size and in power, its deepest problems have changed from the epistemological to the social. Although the character of the knowledge embodied in a particular scientific result is largely independent of the social context of its first achievement, the increase and improvement of scientific knowledge is a very specialized and delicate social process, whose continued health and vitality under new conditions is by no means to be taken for granted. Moreover, science has grown to its present size and importance through its application to the solution of other sorts of problems, and these extensions react back on science and become part of it. For an understanding of this extended and enriched 'science', we must consider those sorts of disciplined inquiry whose goals include power as well as knowledge.

Extracted from J.R. Ravetz, Scientific Knowledge and its Social Problems, Oxford University Press, 1971.

When the perceptive person considers the state of science today, she or he may be forgiven for some bewilderment. The traditional dreams of scientific progress, for the improvement of the material and then the moral condition of mankind, seem to be in the process of realization as never before. But now they are mixed with nightmares, of widespread and increasing, perhaps irreversible degradation and destruction of the environment and humanity as well. When we consider particular issues and problems, such as pollution or economic development of the world's poor, it becomes increasingly difficult to disentangle the positive, beneficial aspects of science (including the technology in which it is practically realized) from the malignant. The task of framing a policy for science which is both benign for humanity and the environment, and also practical in political and technical terms, becomes ever more daunting.

If we are to avoid the sterile responses of rejecting either all of the achievements of science, or all of the criticisms, we must find some way to disentangle the good and evil (as we perceive them) in the phenomenon of science today. To this end I have produced an analysis into categories that are much more simple than I would normally employ. I consider the Good, the True, Society and Cosmology, in relation to the world's poor, and to the roots of our own world-view. It is unavoidably schematic, and many of my statements must be given without the qualifications that would normally lend softness and subtlety to them. However, if this essay is accepted as a sketch of a position to be articulated, and a challenge to complacency in our view of science, it can be worthwhile.

In my discussion, I shall first state the phenomena of the present paradoxical condition of science, then indicate the historic roots of our predicament, and conclude with a sketch of possible ways to a solution. Lest the analysis be too abstract, I also include a discussion of the phenomena from the perspective of that majority of the world's people who are poor.

I shall organize each phase of the discussion under four main headings. The first two are the perennial themes of the Good and the True. These express the basic human values in whose terms the worth of science is judged. The others are Society and Cosmology. These represent the context of scientific endeavour; they are broadly (though not exclusively) paired with the Good and the True for their main sorts of interaction with the world of science. I shall vary the order in which the four aspects are introduced in each section for the better presentation of the argument in each case.

I shall frequently observe how, in human affairs and even in the work of science, it is impossible to segregate off the 'negative' aspects of a phenomenon from the 'positive'. This characteristic may be called 'duality', 'complementarity', 'polarity' or 'dialectic'. I do not have a consistent theory or terminology for describing or analysing it. Gerald Holton (1974) has already discussed this patterning in connection with the progress and assessment of science; I stress its paradoxical and disturbing aspects rather more. It suggests the troubling conclusion that any of our actions are liable to be evil in part. How then are we to apply scientific rationality for human good? I have no answer to such a question; I can only say that at this point I must exhibit the polarity in all these phenomena, as a precondition for true understanding and good action.

The Positive Image

We may begin with a brief statement of the popular faith in science, as revealed by opinion polls. The main emphasis is on what can be apprehended: the improvement of the material conditions of life, through the reduction of drudgery and danger. Even the recognized hazards of pollution, and various possible global crises, do not yet balance the real benefits, in the comfort and security of life, consciously enjoyed by increasingly wide groups of people in recent decades. In our pragmatic world, such goods vouch for the truth of science as a system of factual knowledge. The traditional enemy of science, scriptural religion, is a great nuisance in some places (as in the less advanced parts of America) but no real threat. And although the ancient occult sciences had a frightening growth during the turbulent 1960s, they have by now at least stabilized, still out on the fringe. The hegemony of official, institutionalized natural science and its applications is unchallenged. And the exciting progress of science still gives hopes of new discoveries and powers. The response to the new challenges of environment pollution and resources scarcity, though troubled by an unaccustomed degree of political involvement and debate, shows that science can successfully modify its work and its style to make a continuing contribution to human welfare. Even the recent protest movements, populist in style with occasional Luddite or Arcadian themes, are only a cry for an improved application of the modern scientific principle of government, the achievement of a greater welfare for a greater number. And, at the very deepest level of culture, the cosmology in which our science operates, where material effects are commensurate with their impersonal

causes, may well rescue from the 'occult' those yet unexplained phenomena (such as dowsing and acupuncture) where reliable public repetition ensures their genuineness.

This is, then, an optimistic picture for the present state and immediate future of science. Anyone who wishes to remain reassured may stop there, confident that any impending problems are only local difficulties. Any other opinion is only a matter of judgement. But the picture would be incomplete without the other side.

The Problems

The Phenomena

There is an abundance of piecemeal criticisms of science, and reservations about some aspects of its state. I will introduce the discussion of the phenomena by quoting from one revealing commentary which was not (apparently) even intended as a criticism. Since it was made by Professor Harvey Brooks of Harvard University, a truly wise man of the world of science, it is all the more revealing. Although he is explicitly referring to technology, we can safely include 'science' in the judgement.

The central fact about modern technology is that its powers for both good and evil increase as it evolves. . . Living with technology is like climbing a mountain along a knife-edge which narrows as it nears the summit. With each step we mount higher, but the precipices on either side are steeper and the valley floor farther below. As long as we can keep our footing, we approach our goal, but the risks of a misstep constantly mount. Furthermore, we cannot simply back up, or even cease to move forward. We are irrevocably committed to the peak. (Brooks 1973, Zygon, Vol. 8, No. 1, 35)

One could not ask for a more frank and self-aware confession of the Faustian drive of our modern scientific enterprise. We can agree that our technologybased world, underpinned by modern science, is in a state fairly described as 'brilliant but precarious'. Perhaps there is no way to preserve its present benefits except by putting them increasingly at risk, through progress of the sort imagined by Professor Brooks. If this is so, that is a truly paradoxical situation. Let us explore the structure of that paradox.

Problems: The Good

To ourselves and our immediate ancestors, the vastly increased material prosperity and the consequent political and social amelioration are products of the applications of science. Those who, in one way or another, preach the virtues of austerity or poverty encounter the brute fact of our affluence and the apparently universal desire to maintain and increase it. It therefore comes as a disquieting surprise that so many present and impending ills seem to derive from that same science-based technology. Moreover, it is not at all certain that further application of the same approach will cure these troubles. Some, such as ionizing radiation, diffused asbestos fibres, or a disturbed atmosphere, may well be beyond our means of control.

Furthermore, some applications of science present increasing threats to the stability and survival of our civilization: with nuclear energy the question is not whether but when uncontrolled proliferation of weapons will occur; with microprocessors, the economic redundancy of humans on a mass scale conjures up visions of unprecedented social readjustment or maladjustment. We notice here that science, acting through a technology responding to military demands and market opportunities, *makes* the problem. Its solution will involve some techniques, but essentially lies in the sphere of politics and values. Thus the faith of the Enlightenment is stood on its head.

The Baconian vision of a philanthropic science now become clouded and compromised. Restoration of confidence will not be accomplished by exhortation or rhetoric. Nor will it do to try to keep the name of 'science' clean, as by the logic that gives science credit for penicillin and blames society for the Bomb. There is certainly no clear way through, from our inherited ideology of science as a source of the Good.

Problems: The True

For centuries it has been the proud boast of spokesmen for science that by their way, and no other, could real knowledge be attained. This ideology has supported the established doctrinaire style of the teaching of science, where every problem has one and only one solution. So long as the development of science seemed a simple progress, either the discovery of new laws or the correction of (retrospectively) obvious errors, this view was plausible. But now perceptions of change within theoretical science, and a new awareness of the ways science is actually deployed in the solution of practical problems, have eroded the old confidence.

Within science, we cannot any longer believe that the application of scientific method will protect scientists from adopting or maintaining false beliefs, nor that these innovators subsequently proved correct were more rigorous or methodical than their critics. The progress of science, while still accepted as real on a large scale, becomes more problematical in its details.

When challenges are thrown up by discovery of malfunctions in our environment, the weakness of our science as a practical tool is exposed. We usually discover our ignorance concerning both the data and the mechanism of their production. And the conclusions of natural science, in these matters that affect our survival, are far from the ideal of Galileo: neither certainly true nor

necessary, and depending essentially on human judgements. The category of 'trans-science' problems, capable of a scientific statement but requiring a political solution, describes our predicament well. And the need to regulate technology and hazardous scientific research redresses the long-standing imbalance of intellectual prestige between technical expertise and human wisdom.

Problems: Society

The promise of science deriving from the Enlightenment was not merely for material benefits, but for the cultural progress attendant on the diffusion of the scientific spirit. Science was then seen as an affair of 'savants' or philosophers, whose primary loyalty was to the truth and to humanity. The faith in the civilizing powers of the ethos of science then survived nearly to this day. But once we depict science as largely composed of research-workers, who are 'puzzle-solving within paradigms' that are set by their leaders or by external agencies, the political and social significance of the endeavour is transformed. Somehow it has proved possible for scientific communities to make a concordat with repressive regimes, securing a limited area of autonomy, in return for co-operation and acquiescence where desired. It then ceases to be a puzzle that scientists can be recruited for military ρ r commercial ventures of dubious morality; or that the norms of behaviour of pure research are so easily modified in the interests of employers or governments.

In the name of 'science', large bureaucracies have developed, most notably in the military sphere. However isolated these may be from ordinary academic communities, they require the acquiescence of the apparently uninvolved academic teachers and researchers for their continued operation and influence. Nuclear weapons of mass annihilation, now endowed with a survival instinct of their own, provide a clear example here.

Also, the 'democracy' of scientific work and scientific knowledge is now revealed as somewhat abstract. There is unequal access to élite institutions; there is the political influencing of priorities and standards in all but exceedingly pure research; there is a heritage of job discrimination against minorities in science as elsewhere; and there are the strong survivals of prejudiced attitudes in many fields legitimated as scientific (consider the significance of the enforcement of the supine rather than the squatting position for childbirth).

The result of all this is that the lessons that science can give to society are muted; the traditional assumption of the moral superiority of scientists is no longer automatically tenable. Then, when the applications of science create such hazards for the whole world, any attempted scientific solution is easily discounted as part of the problem.

Problems: Cosmology

Since cosmological assumptions are unquestioned and unquestionable in the teaching and practice of any science, it is difficult to detect them, still less to probe their adequacy. But there are cases on the borders of the hitherto successful reductionist sciences of nature, where cosmology operates fairly explicitly, if not as a logical entailment then at least as a stylistic pressure. Thus the obsolete atomistic ideal of 'genuine' science still informs research in behavioural disciplines, promoting an 'objectivity' in methods that is believed to render insight unnecessary, and resulting in vacuity on a very large scale. The result is a pretence of knowledge and power where there is none, and confusion and impotence worse confounded.

The reductionist dogmas in fields dealing with humans in health and disease, as nutrition and medicine, still impede the development of broader conceptions of problems and solutions. To appreciate the state of medicine in this respect, one need only consider the neglect of physiotherapy, itself not an 'alternative' system but only one that sees the whole body belonging to a human being. Or there is the continued purveying of addictive mood-influencing drugs on physicians' prescription (which we might call 'ethical dope'), since the emotional roots of distress and illness are intractible to medical science. The refusal to recognize the role of social and psychological factors in health has its institutional and economic roots also; but these are given plausibility by the 'scientific' flavour of the dominant picture of the human body as a piece of plumbing.

The main article of faith in the present world-view of science is the denial of any intelligence or consciousness operating here in the world, other than that which can be localized to the brains of ourselves or of very closely related species. Hence the Darwinian theory of evolution, which while it explains everything predicts nothing and seems structurally incapable of falsification, is accepted as the only conceivable rational explanation of how the unimaginably rich and subtle order of nature has come to be. In this view, the defining criterion for quality of an organism, a type or (by natural extension) a culture, is material survival, achieved through successful propagation. Applied to the civilization of which it is an expression, it yields up a paradox: our survival, as particular societies, as a civilization, indeed as a species, has been problematic for nearly two generations now; and most increments of scientific knowledge make us, on balance, less likely rather than more likely to survive. The positive argument for dimensions of reality beyond those allowed by Descartes has been relegated to cranks and cleries; but this reductio ad extinctionem of our reductionist cosmology in its own terms may illustrate the depth of the new dilemma of the modern scientific world-view.

The Perspective of the Poor

Their Experience of Science

To speak of the 'poor' has an old-fashioned, condescending tone, unsuitable for this era of anti-colonialism and development. But the easy optimism of the post-war period about the transfer of our science and technology is gone; and it is now clear that because of social and political factors, the poor will be with us for a long time. In considering the human meaning of science, we must therefore consider their plight. In doing so we must either think dialectically or else perpetrate nonsense.

'The poor' is an overly general term, since there is a continuum of misery, from the homeless of the United States through those south of the border, down to the teeming millions of the flood-plains of Bangladesh. Some poor countries seem to pull themselves up, others remain in abysmal poverty. But for most countries it is recognized that poverty is not a simple, 'natural' imbalance between mouths to feed and land for growing food. Hardly any country is lacking in an affluent upper-class; few famines are caused by empty warehouses. The world's poor nations now function as the slums for the world's rich: the places where no one believes in them as home. Old-style tribalism or landlordism is replaced by agribusiness and runaway industries. Again in most cases, a rotten, oppressive *Gemeinschaft* gives way to a *Gesellschaft* that offers advancement for some at the price of total misery for many. No matter how poor a peasant is, it is always worth someone's while (be it landlord, merchant, bureaucrat or spouse), to exploit them (in the last case, her) economically, politically or however.

Hence we see the general stagnation (admittedly worsened by the ruinous interest payments after the decade of loan-pushing by the big banks) which requires such heroic or even gargantuan efforts to break. The various technical-fix fantasies inspired by European science over the decades, from enforced sterilization, through bureaucratized high-technology agriculture, or attempts to 'stamp out' disease germs regardless of their social milieu, can now be seen as the benevolent aspect of a process of scientific-cultural exploitation of the world's poor, whose commercial side is the exportation of dried cows' milk and cigarettes for the babies and mothers out there.

The Poor and the Scientifically True

The facts of science, which in their own culture seem individually so peculiarly irrelevant to human concerns, become implements of the worst sort of imperialism, that of consciousness. At every level, from the grading of prestige by the abstractness of one's field of research or study, through the shaping of a syllabus around scholastic rather than practical concerns, to the enforcement of a harsh, flat, alien style of thought in problem-solving, the imported Western science is quite other than 'universal' and 'democratic' in the lives of those it touches. And those who are incapable of assimilating it are then doubly victimized: as Illich has shown, they are then officially deemed inferior for having failed to better themselves through obtaining certificates of a skilled brain.

To be sure, the use of knowledge as an instrument of social differentiation and ideological control goes back long before modern science. Indeed, the ostensibly public character of our science has enabled it to function repeatedly as an instrument of liberation from the tyranny of official esoteric knowledge in 'traditional' societies. But if we are to be open to reality, rather than try to pat it into shape, we must accommodate this dialectical interpenetration of the nice and the nasty. This is nowhere more necessary than among the poor, who might even be defined as those for whom the contradictions of material life are not tucked away out of sight.

The Poor and Science-Based Society

In their social and political lives, the world's poor have been similarly exposed to the raw contradictions of our scientific way. The political benefits of modernization have been reserved mainly for an urban élite. The machinery of constitutional and even democratic or socialist government is displayed to the appropriate European mass media periodically, but the cynicism of the show is scarcely concealed. At its worst, chieftains using jet airplanes rather than spears, or gangsters with epaulettes and aid funds, run 'kleptocracies', peculiarly debased mixtures of tribal and capitalist societies, or bureaucratized tyrannies of one sort or another. It is not for us to be condescending, since their stagnation and underdevelopment is the obverse of our 'overdevelopment', whereby we must extract their cash crops and minerals to feed our wants and fashions.

The failure of the post-war attempts to 'develop' the Third World economically and socially in our image reflects the loss of impetus of the total system of European imperialism, after a half-millennium of expansion. 'We' cannot keep 'them' in our economic, social and cultural system; 'we' cannot even prevent 'them' from lurching into barbarism. The 'civilizing mission' of Europe, a source of much of the earlier idealism and drive of our cultural imperialism, is open to question. We must, then, lose the complacent confidence in the simple universality and superiority of our scientific world-view. Its own traditional criterion of truth, which is power, now tells against it in this all-important respect.

The Poor and the Goods of Science

We need not believe that the pre-conquest 'other' world was a pastoral idyll, in order to accept that the European impact was peculiarly traumatic. We

destroyed their gods, either explicitly and deliberately, or by the imposition of a material and intellectual culture that emptied them of experiential meaning. To use an ecological analogy, European scientific civilization has been (for the others) rather like a weed: encountering an order where life, death, good and evil passed through regular cycles of alteration, our ways intruded (with people, guns, doctrines, diseases and drugs), and flourished wherever there was an instability, soon choking out the culture that belonged to that special place. Now it is settled there, ineradicably so, but producing strange and frequently inharmonious hybrids of life-styles and cosmologies. Therein lies a principal cause of the continuing poverty, stagnation and corruption of colonial life, in the social, economic and cultural dimensions. In such a context, apparently impractical and unrealistic criteria for economic life, such as those derived by E.F. Schumacher from the teachings of the Buddha, now become rather less fantastic than the perspectives of Americanizing the life-styles of all the world's billions.

The World's Poor and the European Scientific Cosmology

So long as we insist that religion is an affair of the intellect (believing the impossible, called 'faith'), somewhat aided by sentiment, and we rigorously exclude the possibility that the transcendent dimensions can be experienced palpably and ordinarily, we will remain baffled by other cultures. And, whether by accident or by the actions of our ancestors, the demarcation between the world's rich and poor runs close to that of cultures. To justify our own cosmology, we have denigrated the others, and those who live in them. This spiritual destruction will eventually be blamed by the victims on 'science', since the metaphysics of our science still dares not enrich itself beyond Descartes' dualism of a totally dead material world complemented by disembodied, desiccated rational souls. But we should not be swayed by culture-guilt into believing that every 'traditional' survival is superior to the more 'Western' rival; that would be to react, ignoring the polarity of such phenomena. Can we but say that the cosmological question is among the most urgent, and as yet the least attended, of all those on our agenda?

The Roots of Our Predicament

The Dialectic of History

Radical thinkers, intoxicated with their concepts, believe that all can be transformed on the turn of an idea. Conservative actors, settled in the constraints of inherited custom, sense that nothing can really change except of itself. The tension between the shallowness of the one and the complacency of the other, has motivated social philosophy in Europe for centuries. Only if we give up attachment to some simplistic Good as realized either in a fantasy future or in a fictional past, can we comprehend what history can tell us of ourselves.

The roots of our predicament will not be so simply enumerated; they extend back through centuries of contradictory phenomena of the sort we have just exhibited for the present moment of time. Each contradiction has its own cycle of development, during which the balance of apparent costs and benefits shifts drastically (what Illich calls 'the watershed') and perhaps suddenly. This sort of history can help us to understand the abrupt reversal of evaluation that has occurred in connection with Science in recent years.

The European Cosmology

Let us take the deepest root of all that defines European culture and consciousness: and let us not be dismayed if the glory of our civilization may also contain the seeds of its eventual decay. Identifying the essential common element in classical Greek philosophy and mature Hebrew religion can be a game without end (or even without rules). But if we cast around for themes, we see a significant cluster. There is a quite strongly marked drive for the explicit, as in the philosophical probings or the moral code respectively; with it is a need for an absolute foundation of knowledge of the True or of the Good that is independent of the will of ourselves or even that of a personal deity; and finally there is a personal identification with any position as the *exclusive* solution of its problem. These stylistic features of the two sorts of thought go with a general reduction of the degree of higher psychic functions that are believed to be enjoyed by the various beings of the external world, both visible and invisible. This is particularly striking when compared with neighbouring cultures of the time. To us, 'animism' and its relatives in 'superstition' and 'irrationality' have the full force of the uncleanliness associated with a taboo. In such a world as ours, the human person is then alienated from his total environment, and is self-conscious and necessarily self-justifying. Such a person is capable of the heights of individual achievement, but also of depths of immersion in loneliness and the meaninglessness of existence.

All possible variations in the degree of expression of these traits have occurred in the evolution of Europe; but we need only compare our sensibility to that of other familiar great cultures, be they Muslim, Indian, Chinese, even Japanese, to say nothing of non-literate societies, to appreciate their plausibility as a composite identifying portrait. We can see that a particularly exaggerated version of this cosmology took hold in Europe in the seventeenth century. The 'scientific revolution' was a sudden hardening of the sensibility of the educated classes, which fostered the rapid development of particular sorts of sciences involving a disenchanted mathematics applied to a dehumanized world of nature. This desperately flat metaphysical basis defined the style and content of the European natural science that soon moved beyond the level of its

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predecessors at an accelerating pace, eventually yielding the triumphs of knowledge and power that we all know.

The cosmological root of modern European science, its tendency to the reduced, explicit and exclusive style for objects and arguments, lies very deep in our culture. Opposed sensibilities lie equally deep: Pythagorean, alchemical and romantic themes recur through time, and they have been more important than is recognized, for the origins of our ideals of technical mastery as well as for our poetry. The scientists of the Renaissance were generally as much imbued with alchemy and magic as the poets. This tradition in natural philosophy lasted into the seventeenth century and produced some of the early works of great science; the scientific revolutionaries shared more with that tradition than they cared to admit. But these themes have, up to now, generally been maintained as a suppressed 'darker' side of our modern educated European sensibility. The sorts of enhanced experience of reality, external as well as internal, that are commonplace in other cultures, seem to have occurred, or to have been acknowledged, only fitfully in Europe. Thus, feng shui survives well in China except where it is self-consciously modernizing, while here 'ley lines' are still recognized only by the eccentric few. Hence the 'counter-culture' of the 1960s, together with its descendants, can justly be considered to be an importation of an Eastern technology, in this case of the non-tangible realm.

Our Roots: The True

The European idea of the True, which defines our sciences, is thus one where reality consists of objects with hard edges. Our ordinary logic admits only 'yes or no', not 'yes and no', nor yet 'yes nor no'. With such components we have built amazing structures of rigorous and rigid thought, and yet more amazing material realizations of them. Such a logic is now known to be inappropriate for comprehending the outermost world that it has discovered, the physics of the very large and the very small. There, the other logics, similar to those of the pantheist or the mystic, seem necessary. But that discovery is still scarcely appreciated. For coping with the vast problems of our uncontrollable total environment, we still use the logical tools suited for straight-lines and abstract mass-points.

This conception of truth is singularly ill-suited to variety or change in knowledge: our basic categories force us to oscillate between dogmatism and scepticism. Our philosophy of knowledge (in science and outside) cannot cope with error, but either ignores it, or explains it away as an accident that could in principle be eliminated. Describing the sorts of relatively successful knowledge that we can achieve (in science as in technology), and its roots in the interplay of personal, social and natural and metaphysical elements, is a philosophical task that is barely begun or even conceived. I am now beginning to see a way through, not using the idea of *error* as much as that of *ignorance*. It is possible to sketch out a dialectical interpenetration of knowledge with ignorance, each implying the other in a way that is not merely good dialectics, but also very important for understanding the uncertainties that affect all our statements about the world.

Our Roots: The Good

Our idea of the Good is correspondingly harsh. Particularly since the Reformation and the Scientific Revolution, the awareness of the benefits of inward contemplation has wilted before the rewards of external action. Consequently, in the interpersonal realm, the quiet, intangible reaching-out describable as 'compassion' is too soft to be distinguished from passivity or indifference. Instead our benevolence appears as a compulsion to reform the object of our concern, preferable by actions of universal applicability and under the guidance of a scientifically established theoretical system.

The reality of the human suffering, material and spiritual, that evoked this response, is not in question. Any nostalgic looking backward towards the days before the impersonal Welfare principle needs to ignore the overwhelming mass of misery and degradation that resulted from leaving charity to the charitable. Nor it is sensible to lay responsibility on individuals or cultures, for those vast changes in sensibility and inward experience which incline us to one or another conception of the Good or the True. But we now have had sufficient experience of the frustrations of the Good conceived exclusively in the material and social planes, that we should see the limitations in our world-view and ourselves in their light. Again, seeing ourselves as the exaggerated 'masculine' pole of a dialectic can provide the right sort of humility and insight.

Our Roots: Society

A look at European society in the terms of the present crisis well illustrates the need for dialectical thinking. The alienation of our inner and personal selves, so obvious to those outside our culture, is a drastic impoverishment. But it is complemented by an enrichment: a widespread awareness of a social and ethical self, as possessing the right to independence and dignity. This, even more than crass material goods and evils, has been the corrosive solvent of other cultures, previously organized around customary acceptance of inequality based on accidents of tribe, family or gender.

This new secular self-consciousness is variously realized under titles such as 'democracy' or 'socialism'. With its enhancement of individual effort and acquisitiveness, it is deeply related to the social order we have inherited, and also provides a psychological and social context for our style of scientific enquiry. Its negative side is increasingly realized in communities with permanent social tensions and unrealizable popular expectations. Should our

A Critical Awareness of Science

technology fail to continue to buy acquiescence abroad and at home, individualism and anomie could degenerate into anarchy and chaos.

The Power of European Imperialist Science

All the foregoing generalizations could be a subject for the academic history of ideas, were it not for the recent transformation of the scattered learned sciences and empirical arts into an engine of unprecedented and scarcely imaginable power, both for the production of new knowledge and for its translation into tools. The consciousness of this power, which has made modern Europe so qualitatively different in its material culture from any other civilization ever, has produced the expectations and mass demands of the material good life, which rulers must now either respect or else attempt to crush with barbaric brutality. The very sudden transition to a future other than that of increasing material plenty has not yet been grasped by those who manage our nominally Welfare States. Its implications for politics and the political role of science are just some more items on the agenda of those who must try to steer us past the shoals that we and our ancestors have made.

In the long view, the power of our modern science has much of its human significance in the cycle of domination by European society over the rest of the world, that got under way in the fourteenth century and reached its summit in 1914. It is a myth of recent origin that science and scientists were not involved in our imperialist conquest. The mathematical sciences in the sixteenth century served exploration and war; similarly the 'field' sciences in subsequent centuries. There was an intimate connection, personal and ideological, between the English philosophical movement that eventually produced the Royal Society, and the rape and plunder of Ireland in the Civil War period. Thus our science has, in its global context, the same contradictory history as the rest of our culture: destruction and development, oppression and liberation, all joined in one discordant ensemble.

The reaction to European science by other peoples will be similarly inconstant and inconsistent: generally the tools are desired, but their cultural entanglements are viewed with a mixture of admiration, envy, hate and fear. The naivety of the good intentions of those of us who would export or impose our own scientific ways have been sufficiently exposed. But the problem of unscrambling the mess of our imperialistic scientific heritage has barely been considered.

Ways to a Solution

The European Crisis

The problem of European science is thus a problem of the civilization of which it is an integral and defining component. For Europe as a whole, this has been a century in which much has gone wrong. The self-confidence of the middleclass European male as he surveyed the world in, say, 1900, is now lost for ever. One war of butchery led by incompetents; another of genocide started by a maniac; various empires breaking up in big and little chunks with no prospect of repair; the constant and permanent threat of total catastrophe should there be a misreading of a warning about nuclear weapons; a natural environment now reacting in unpredictable and non-linear ways; and most recently a realization that the material prosperity (and hence social stability) of the midcentury years depended on local and foreign circumstances that cannot be maintained. Although scientific discoveries continue to be made and applied, the sense of triumph in that progress is muted or altogether lost. The natural sciences now raise social and ethical problems at every turn, and the social sciences are sadly recognized as being as far as ever from being mature and effective foundations for enlightened social engineering.

To view this array of worries as 'a problem' and to seek a solution as if it involved only the construction of lines and circles, is itself a very strong commitment, not at all justified by the phenomena under review. Let us instead consider the various styles of response that present themselves.

The Academic Reactionary: The Absolute Value of Truth

For many scientists, the immediate world of routine teaching and research has not yet changed all that much; and from that perspective it is not easy to see why it should change at all. The academic scientist's fictional past is a compound of the innocence of science in the days before it yielded material power, and the affluence of science once its products could be put to use in the world. To protect his world, he invokes an idol of Objective Truth, making it sovereign over all other considerations (except perhaps the proven immediate hazards of research). Proponents of this view are unhappy victims of a narrow professional ideology of 'pure research', designed to resolve the contradiction that science requires constant external support and yet is unable to give a detailed accounting of the benefits rendered in return. Because they identify with a past that never was, I call them 'reactionaries', even though their general politics might sometimes be quite liberal or even radical in appearance.

Philosophers of science are increasingly admitting the complexity of the process whereby scientific knowledge is achieved, and are recognizing unsolved and insoluble problems of clarifying the structure and content of generally accepted scientific theories. Those who still believe that significant scientific facts can be collected like pebbles on a beach are adhering to a very fundamentalist faith. Further, the ever closer interpenetration of science and industry makes the claim of 'purity' hollow; and once there are external influences on the choice of problems (and hence of results) the 'objectivity' of science loses an important dimension. The distinction between the free

A Critical Awareness of Science

acquisition of knowledge and its subsequent socially constrained application, is little more than the last refuge of a precarious minority vested interest.

The intellectual collapse of an ideology may take several decades or even generations to work its way through to common-sense understanding, even among the eminent. The delay will be particularly marked when there are special interests to protect, and no satisfactory alternative presents itself for that function. Hence we may expect many reiterations of the old faith, superficially modified by lip-service to 'social responsibility'. One salient point at which the unreality of such rhetoric becomes obvious, is the claim that scien. tific knowledge is essentially 'public'. There are sectors where this holds, but when we consider research and development (R & D) managed in either military or commercial institutions, or in many civil service settings, we see that 'repeatable' is by no means the same as 'public'. Proclaiming 'public knowledge' in order to conceal the importance of 'corporate know-how' serves to confuse debates on technology policies. There, the styles of politics, journalism and even detective work are frequently more appropriate for achievement of public scientific information, than those of traditional refereed academic research.

But even here there are dialectical contradictions. The autonomy obtained by the 'pure' scientist in his research or academic institution does fend off the worst sorts of blundering or malevolent interference in the process of enquiry. To have science serve 'the people' is a noble ideal, but in practice a bureaucracy stands between the two parties, and defines those tasks by which the service is to be done. Then all sorts of irrelevant and damaging pressures may be applied, for the perennial power-game of society does not depend on the 'ownership' relation for its motives and rewards. Hence political radicals may need to find shelter in universities behind scientific reactionaries, for the pursuit of their own work. If they are embarrassed thereby, that is a sign of the room for further maturing in their understanding.

The Policy-Science Conservative and the Achievement of the Good

A sort of science which is not peculiarly devoted to acquiring knowledge is now becoming recognized in principle by sociologists of science, although not yet by philosophers of science. Yet the importance of such science, which may be called 'applied', 'mission-orientated', or 'regulatory' or 'public-interest' (emphasizing one or other aspect) is great and steadily growing. The directors of this work will be found in government, in industrial labs, and (as individuals with a personal and exceptional outside role) in universities in Europe. In America, the 'service' tradition in state universities, the vast research businesses run by the great universities, and a general spirit of entrepreneurship have long ago blurred the distinction between 'basic' and 'mission-orientated' research.

In this sort of science, resources of manpower and equipment are deployed,

sometimes on a massive scale, in response to problems set by some external goal, be it for military, commercial, political or social benefit. Those who promote and manage such enterprises must be sensitive to the desires of their clientele, lest they find themselves abandoned as obsolete and irrelevant. Of course they need not adhere slavishly to the client's conception of any given problem, no more than any professional abandons his expertise and selfinterest when undertaking a brief. However, a reasonable facsimile of 'relevance' must be maintained by policy-science. Its task may be defined as the setting and solving of those technical problems which promise to resolve the practical problems brought by corporate clients.

In comparison with those academic scientists, eminent and humble, who find the outside world an unwelcome intrusion, the policy-scientists may appear as very aware and even radical. They maintain familiarity and perhaps a dialogue with critics of existing policies, and show no doubt that the various social responsibilities of scientists must be fully and publicly discharged. When I refer to them as 'conservative' I do not use the term in a pejorative sense, but only to call attention to an attitude or commitment. Generally, they do not welcome change either within the social system of science nor in its technical and political environment. But, like enlightened conservatives in any sphere, they attempt to comprehend it, to respect its cause and thereby to channel it away from dangerous courses.

However, such a conservative starts with the conviction that every problem can be managed, and in particular without cost to his professional status or ideology. Nor can he easily accept that an unpleasant problem is deeply rooted in a social system with which he strongly identifies. I have personally seen how difficult it is for individuals, however well motivated, to be simultaneously part of the problem and of the solution. Hence the powers of analysis, and even of perception, of such a person have their limits; paradoxically these tend to be more severe on matters closer to that person's professional competence and commitment.

The policy-scientists suffer from another disability; while they command (or influence) research funds that dwarf those of their academic colleagues, they do not control curricula for science teaching at any level. Hence their influence on the consciousness of younger scientists is indirect, and the false ideology of 'pure' science is still passed on with remarkably little direct challenge from inside the world of science.

The conclusion is that this group of scientists might indeed manage well, so long as the problems are manageable. But should we have a convergence of crises in and around science, then we should not be surprised to find an inadequacy of vision and resolution, even among those policy-scientists who had been aware of some separate components of the crisis. Also, should our society encounter the sorts of contradictions of basic values and cosmologies that already afflict the world's poor, then our particular dominant conception of the Good, material and social, and the policy-scientists who operate in its framework, might even become hindrances to their resolution.

The Isolated Social-Political Reformers of Science

The 1960s saw a dramatic increase in awareness that the social institution of science in any class-divided society must, in many important respects, be tailored to fit the requirements of its context. Young scientists who had been misled by the apparent objectivity of scientific knowledge, and perhaps by the personally liberal sentiments of some teachers, were shocked to discover the degree to which science, and themselves, were recruited to the service of a repressive social system. A vocal minority, in America and elsewhere, then dedicated themselves to the reform of science, under the slogan 'science for the people'. A similar discovery was made in China, and contributed to the campaigns of the Cultural Revolution, now described as the rule of the Gang of Four.

Since that turbulent decade, such social reformers have had a thin time. Their organizations in the West have dwindled, and China has swung away from the traditional left in social and political style. What still flourish in the West are special-issue campaigns, usually centred on some particular objectionable technology. Though many organizers and participants privately have radical opinions about government and bureaucracies in general, there has been little concerted attempt to concentrate criticism on the scientifictechnical establishment as such. Even the movements for 'appropriate' or 'alternative' technologies, varying between populism and meditation for their bases, do not focus their public criticism on the social institutions which foster the 'high technology' that they wish to replace.

So a radical critique, in social-political terms, of the world of 'science' is left to a very few authors and editors. I personally believe that the enfeeblement of the critical spirit reflects the much reduced plausibility of any solution to the problem of 'science for the people', which is cast in traditional political terms. Since analyses of this sort need to provide solutions working at the social level, not just for mere individual people, the political reformers of science are now the victims of their own criteria of success, which are severe in themselves and perhaps also inappropriate. Once one loses faith in the unique and dominating influence of the abstraction called 'ownership', the path of radical reform leads away from its traditional European ideologies, such as socialism; but whither is not yet determined.

Radicalism for Science-from Red to Green

As the force of traditional socialism wanes, there emerge new forms of protest and their associated theories. As yet they are scattered, ranging from middleclass NIMBY campaigns (Not In My Back Yard), through the counter-science groups which collect and publicize environmental health statistics that contradict the official denials of danger, to the ecological activists (such as Greenpeace) who use commando-Gandhi tactics to publicize global scandals; and over to the alternative health movements, be they feminist, oriental or spiritually inspired.

So far there is little direct confrontation with Science as such; perhaps for such people that symbol is as outworn and irrelevant as Religion. At the political level the engagement is with various sections of the politicaltechnical establishment; in that context the lone scientist making his exciting discoveries is as obsolete as the knight-errant on horseback. At the theoretical level, the enemy is partly that establishment, partly our industrial society, and partly ourselves.

As with every confrontation, there is a dialogue. Politicians of all sorts experience partial conversions to Green thinking; and as campaigners gain power they experience the burdens of responsibility. In some fields, such as health, the sharing of insights seems genuine; younger doctors have less of the traditional scientistic phobia of any treatment that cannot be explained in atomistic terms. All these movements bring us fairly directly to the existential questions of what is the real quality, and hence the real meaning of life; and as the twentieth century comes to its close, it is increasingly clear that neither the knowledge nor the power of science could provide fully satisfactory answers.

Can a Cosmological Critique be Relevant?

To discuss alternative world-views seriously is nearly enough in itself to have one classed as an 'anti-science element'. To believe that paranormal phenomena exist, or that 'altered state of consciousness' has any significance, is to place onself well out on the eccentric fringe where, for instance, the *jus docendi* would not extend. Yet to insist that rationality, the values of civilization, the success of science, or some other good is essentially tied to the Cartesian reductionist cosmology is really to be somewhat parochial. It cannot be denied that in the other great civilizations where science has flourished, such as China, India or Islam, the socially constructed reality of educated common sense has been different from and richer than ours. Nor can we deny the chronology of our own great scientific revolution: advances in technology, and even the master-works of early modern science such as those of Gilbert, Kepler and Harvey, belonged to an older world-picture, and were achieved *before* the spread of the new metaphysics proclaimed by Descartes, Galileo and Gassendi.

Perhaps the particular contributions of modern Europe, its science/ technology and its political democracy, have been related to its disenchanted cosmology. But perhaps its negative side, its aggressive and destructive actions towards other people and other species, are equally so related. Again, we do not try to determine whether 'good' or 'bad' is in some absolute excess; rather we study the contradictory tendencies as they have developed and nurtured, and then try to sense in which way a resolution may flow.

At this concluding point of my analysis, a rather elementary statement of

the credo of the European scientific world-view might be more plausible, and useful, than at the beginning. Let us consider: 'The true facts and the good things are provided by the Science of a purely tangible world, and they are each simple and separate'. Attempts to control nature and to reform society by applications of this faith have become increasingly problematic and throw up phenomena that are paradoxical, contradictory, and increasingly difficult to control, at every step. This is the substance of the cosmological criticism of science today.

What Sorts of Problems, What Sorts of Solutions?

It is certainly difficult to avoid a schizoid feeling when one writes (or reads) about radical criticisms of science while located physically in a comfortable room in some affluent and secure European city. Perhaps meetings about the great problems of science could have an obligatory session in some thought-provoking location, such as Beirut, Belfast or Phnom Penh, not to mention Hiroshima. We could then be reminded that the summer of 1939 was an exceptionally fine one.

My own views on the various elements of the criticism of science as outlined here, are of little importance. If I did not think them worthy of reporting, I would have described something else; but if I believed the whole range of criticisms literally and one-sidedly, I would not have bothered to write at all. I hope that I have at least exhibited a range of problems, and indicated some elements of possible solutions. If there is any point which I feel prepared to defend now, it is the approach that appreciates and strives to encompass the apparent duality and contradiction in all these phenomena. In this I implicitly criticize and depart from the scientific style as laid down by Galileo and Descartes; how much of a change in metaphysics is entailed thereby has yet to be seen.

Adapted from a lecture by J.R. Ravetz entitled 'Critiques of science today', presented at a Nobel Symposium on Ethics for Science Policy', held at Stockholm in August 1978. A much abridged version was published in the proceedings of the symposium: *Ethics for Science Policy* (ed. T. Segerstedt), Pergamon, 1979, pp. 49-56.

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Risks and Their Regulation

This essay is adapted from several short pieces that I wrote over the space of nearly ten years. These were stimulated, in various ways, by the work I did on a report for the Council for Science and Society, *The Acceptability of Risks*, between 1973 and 1976. I produced drafts for the chapters; these were then reviewed and modified by a working party; and the whole text was finally edited before approval and publication by the Council. The report is too long to reproduce here in its entirety; and I feel that none of its separate sections would read well if taken out of its context in the tightly structured argument of the report. Although I do regret not seeing it republished, it is better to settle for the work which is all of my own composition.

The first section is an introduction to the subject, adapted from an article originally written for the journal *Physics Education*. In it I discuss the uncertainties inherent in the scientific study of risks; and I draw out the policy consequences of this aspect of risks research. This work on risks has led to my most recent researches, conducted in collaboration with S.O. Funtowicz, on uncertainties in quantitive information. Our joint book, *Uncertainty and Quality in Science for Policy*, is due to be published by Kluwer Academic in 1989.

The second section is based on a lecture presented at a conference on 'Technological risk: its perception and handling in the European community', held in Berlin in April 1979. This was sponsored by DG XII of the European Community, and it was held in the reconstructed Reichstag building, next to the Berlin Wall. It was set up as an attempt at a dialogue between scholarly critics of civil nuclear power and some influential persons in the research section of that industry. An unforeseen event gave added point to the conference: the accident at Three Mile Island. Some of the nuclear power people showed obvious concern, and also a sense that their credibility would henceforth be seriously affected; but others carried on with superb contempt for the assorted critics and radicals, those present no less than those absent.

The third section is extremely brief, but even it has an interesting story. The

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essay on which it is based was sent to the editor of the journal *Minerva*, who was a good friend of mine, as a sketch for a fuller piece. It was a development of ideas on quality control, that I had worked out in connection with science in my book *Scientific Knowledge and Its Social Problems* (1971), and which were still not popular. The editor offered to print the sketch as it stood immediately, as a comment on a long article that was just then ready for publication. I accepted the offer, not knowing anything about the article. This turned out to be a programmatic manifesto by a very important person, Wolf Häfele, the leader of the energy study at the International Institute for Applied Systems Analysis and also a well-known proponent of nuclear power in general and the fast-breeder reactor in particular. His paper very boldly stated the difficulties affecting any analysis of the risks of nuclear reactors, and proclaimed that there *would* be an answer. My little piece, arguing qualitatively about regulation rather than quantitatively about risks, gave another impression.

Finally, the last section is based on an article, 'The political economy of risk', which was written so that I could publish some ideas on risks that could not be expressed so explicitly in the report for the Council for Science and Society. It appeared in *New Scientist*, and was later republished in a booklet, *The Risk Equation*. Were I to write it again, I would be more explicit about the great variety of risks, so that the 'risks triangle' is significant only in some cases. On the other hand, I would emphasize more strongly that the imposition of a risk is a form of oppression. Indeed, in the economies known as 'socialist' or 'centrally planned', one may say that the imposition of risks on workers, neighbours and the natural environment has partly replaced profit-taking as the leading form of oppression. The imposition of risks, and the weaknesses of their regulation, have certainly become very sensitive, politically and ideologically, in recent years. Also, the question of why there should be such wanton imposition of risks in the absence of a profit motive could become an important focus for deeper studies of political economy in general.

RISK ASSESSMENT—A SCIENCE OF UNCERTAINTIES

Risks are very much in the news now; and the study of risks has assumed great importance for policy purposes. It is also a very useful example for the philosophical understanding of science. Until very recently, it was assumed on all sides that when 'science' was brought to bear on a problem of public concern, it would provide assured, objective facts from which the correct policy conclusions could be derived. This faith was shared even by those who worked in the fields from which the facts came, in spite of their personal experience of the extreme difficulties of producing information of adequate quality. It was in this spirit that the first quantitative studies of risk acceptability were begun in the later 1960s in connection with civil nuclear power. After only a decade of research and argument, the proponents of risks research had become much wiser and perhaps also sadder. For in spite of the apparently 'natural' character of the phenomena, there remain irreducible and important uncertainties in any assessment of a risk of a technological or industrial system. Moreover, the social and personal elements of a risks situation cannot be separated off from the scientific and engineering elements. Risks are in their way a total phenomenon; and must be studied as such.

Here, I will use examples primarily from nuclear power; this is appropriate, since this was the issue around which the science of risk assessment was created. I will try to show that while there is an objective 'scientific' core to any decision on risks, this is conditioned strongly in its interpretation by inexactness, uncertainty and value commitments. Simply citing strings of numbers without qualification is, in this area, *not* the scientific approach but rather a variety of pseudoscience.

The study of risks is very much an 'applied' or 'mission-orientated' activity. There is no organized body of knowledge achieved for its own sake, nor an elaborated structure of theoretical concepts. The 'scientific' part of the study of a particular risk is therefore always related to a practical task of management; it necessarily aims at results that are immediately useful for policy, rather than being elegant or theoretically deep. The work is all very empirical, indeed exploratory. Ideas and methods are still subject to modification and debate. There is no exclusive expertise which requires years of specialist training for its possession.

For our present purposes we may consider the task of risk management as divided into four phases: assessment, evaluation, decision and execution. The first one looks most like 'science'. The 'intensity' of a risk is measured by its 'probability' and 'harm' jointly (most simply, but not necessarily, by their product). In the next phase 'values' enter; to determine its 'acceptability' the given risk is compared to analogous known or existing risks and also to possible risks of alternative decisions in the case of major policy choices. All this information is provided by experts and is then fed into a process of decision conducted by people in a political role. It is recognized that in many cases of risks (particularly those involving choices of new technologies), the 'scientific' input is insufficient to determine the correct answer to immediate policy questions and so 'politics' quite legitimately comes in. Finally there is the phase of 'execution' which will involve the establishment and operation of monitoring systems, both hardware and human. Since an unenforceable decision is the worst of all, the practicalities of monitoring are a valid consideration in all the previous phases of the process, even including the framing of the problems for 'assessment'.

I should say that this model is capable of refinement in many ways, depending on the purpose of the discussion. For example, even before assessment must come recognition of a risk. This is not a trivial affair; and first signs of a lowlevel risk are frequently based on uncontrolled, anecdotal information, which may well relate to other physical causes or even just generalized fears and suspicions. Public authorities must steer a course between chasing after every

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complaint (incidentally producing public concern and disturbance in the process) and being too cautious and then being accused of complacency or collusion with those interests that create risks. Thus even before 'assessment' can begin, there may be a quite contentious policy decision on whether there is anything there to assess!

Risk Assessment—the Elements

The elements of a risk assessment are, in form, quite simple: measures of the probability of an occurrence, and of the harm in the case of its happening. One can even imagine O-level questions: given such components for several comparable risks, determine which is the most, or least, 'acceptable'. There are many cases, indeed the vast majority of those where risk assessments are made, in which the techniques can be applied in a straightforward way. These will be in the various fields of 'safety' — at work, on roads, with respect to fire, medicines or consumer products. So long as the following conditions are satisfied, a straightforward process of risk assessment can be entirely adequate, functioning as an analytical tool supplementary to 'common sense'.

- 1. The risk situation is simple with a few clearly separable causes.
- 2. It occurs so often that there is a strong base in 'historic data' for the various empirical probabilities.
- 3. The sorts of harm are limited in variety and are capable of meaningful measurement on a common standard (or a few such).
- 4. Calculated measures of 'intensity' are used for *comparison* between related similar risks as an aid to design or management.

When we consider the risks that come under public scrutiny, we encounter a dilemma that is quite common in 'science in society' problems. It seems often (though fortunately not always!) that the more important the risk for public policy as a question of human or environmental welfare, the more it diverges from the four conditions defining the possibility of its effective assessment in a scientific way. Indeed, one distinguished American nuclear scientist, impressed by the intractable problems thrown at him for decision, coined the term 'trans-science' (Weinberg 1972). This describes those urgent problems, typically occurring in risk assessments, whose *form* is quite scientific but whose *content* puts them outside the limits of technical feasibility. His example was low-level ionizing radiation from nuclear power stations, the statistical test of whose possible carcinogenic properties would (by techniques available ten years ago) require some eight billion mice!

For the moment, let us defer the difficult or insoluble problems and stay with the easier ones. How is an assessment carried out? It is not enough to see how often there are some bodies to pick up and cart away, after a particular type of accident. Each undesirable event must be analysed both for its *causes* and for its *consequences*. Accidents are relatively rare events that occur through 'commission' and 'omission'. First, there is a chain of situations, events or actions, themselves each harmless or of only mild harm, but which when combined produce the occurrence in question. But also, for the accident to occur, the normal routines and procedures whereby the above 'incidents' are monitored, corrected or otherwise prevented from concatenation must be absent or ineffective. One simple case of these could be a 'blind corner' on a road, which an occasional driver (most likely one unfamiliar with the road) takes too quickly, and where a warning sign is obscured for a period in early summer by an overhanging tree (before the tree-pruning team get around to it). All it needs is for an exceptionally careless driver or some distracting presence to send a car around dangerously *and* a stationary car or pedestrian to be in the way. Then that very rare event (relative to many thousands of safe passages through the year), an accident, occurs.

When experts analyse such accidents they fill in the places in a matrix, or 'tree', with all the various predisposing causes and incidents as inferred from this and related accidents. They can then see which are most significant, and thereby advise on the most economic and effective policies for improvements.

Similar to such a retrospective analysis for the management of existing risks is the prospective analysis of the risks of installations being designed. There the 'fault tree' is constructed by analogy with known similar units; the probabilities of failures or incidents, inserted at the 'nodes' of the structure, are derived from historic data. The overall probabilities of accidents of various sorts are calculated by the compounding of those of their antecedents. Those which are too high, by some standard of evaluation, are then redesigned.

The 'human' element, the causes by 'omission', does not enter explicitly in such models since it is known that the 'incidents' may themselves be the result of inattentions, malevolence or some other human cause. However, where the monitoring functions become very sophisticated or elaborate, this aspect of the risk becomes exceedingly difficult to model or quantify.

Risk Quantification in Real Problems

Up to now I have sketched the elements of simple assessment problems with an indication of where complexities occur in the causal analysis. Now we must consider the real and very challenging problems that arise from the significant degree of inexactness in all the quantitative measures and estimates.

First, and perhaps most obviously, there is the estimation of harm. Where this is mainly material damage, as in many fires, then some sort of cost of replacement may be a convenient and reasonable measure. But where people are at risk, calculations can easily go wild. How much is a life worth, in coin of the realm? There are a variety of measures, ranging from compensation awards through to average investment in safety per year of life saved. They have little in common in basis, calculation or result. Do we count lost earnings or suffering of self and family? Or do we also discount the costs to society of

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maintaining non-productive persons? There is no 'objective' cost of a life, nor any of non-lethal harm. Hence we may say as a principle calculations of personal risk that invoke a monetary measure of harm are liable to be highly arbitrary or biased.

Policy decisions are best based on an explicit measure of human injury. Traditionally this has been done, for the case of nuclear power, by numerical conversion of radiation dosage into cancer cases, but even this cannot encompass genetic damage to future generations. By some standards such harm is ethically impermissible even at very low incidence because of its irreversible effects on totally innocent persons.

Coming now to the measurement of probabilities, we are on ground that should be familiar to every scientist, for real quantitative science begins with a recognition that perfect precision is impossible; the real skill in using mathematics lies in the management of inexactness. Students know that every set of experimental readings has a 'spread', sometimes inappropriately called 'random error'. Indeed, one way that teachers can detect concocted data is when it fits *too* closely to the known theoretical curve! And the description of a quantitative result is at best faulty, and at worst meaningless, unless it is accompanied by an indication of its inexactness. This may be done by a very simple convention such as 'significant digits' or by some more elaborate notation. But in its absence we may find an implied precision which is totally false and misleading, especially in the case of estimate-statements like '2 $\times 10^{-6^\circ}$.

Such considerations apply even more strongly to statements of probability. I may say that the empirical probability of a particular coin coming up heads is 100%, but if that is the result of a single toss, my statement is really misleading. If I toss it, say, 10 times and come up with, say, 60% heads, what does *that* tell me about its possible bias? In fact, to have any rigorous meaning at all, probability statements should be the consequence of a *test*, organized around a particular *hypothesis*, to a *preassigned* confidence limit. Otherwise they cannot be distinguished from the results of a search for interesting strings of digits in a telephone directory.

The full theory of statistical testing and inference would take us too far afield from our present concerns. Let it suffice to establish two principles. The first is that every test, involving confidence limits, depends for its design on values: most simply the relative undesirability of the two sorts of error: 'pessimism' (the 'cry wolf' syndrome) and 'optimism' (or complacency). In a classic example, we apply different standards when sampling for rotten apples in barrels and for hypersensitive landmines in their cases. The second principle important for the assessment of statements about risk, is every quantitative statement involving probabilities (or other estimates of uncertainty) must have some indication of its degree of inexactness.

Finally, I must mention the more severe problems that inevitably arise in the assessment of the most important technological risks. The measurement and expression of *uncertainty* is a challenging but practicable task. But what to do

about ignorance? Sometimes this relates to phenomena that are too subtle to be studied effectively, as in the case of low-level radiation. But ignorance may equally well apply to quite important contingencies that are not at all unlikely and where a formal risk assessment requires some sort of number in a box. One obvious case in point is terrorism, sabotage or other forms of 'malevolence'. There have already been a number of incidents of this character, directed at civil nuclear installations. How seriously should they be taken? Some policy decisions are easily settled by a purely qualitative analysis of the problem. Thus there are no known plans to site nuclear reactors in Northern Ireland, in spite of the absence of indigenous energy supplies and the consequent high price of energy there. Whatever quantities we might assign to the attentions of the Provisional IRA, they are too high under our circumstances. However, in the Third World, governments cannot be so fastidious, and with the approval of the International Atomic Energy Authority nuclear installations are set up in countries where there is no certainty of firm and stable civil rule throughout the planned lifetime of a reactor.

Less dramatic but equally important is the inevitable ignorance about the behaviour of technologies that are new or complex or both. For example, the 'fault trees' used in analysis of nuclear reactors have needed to be 'pruned' since the catalogue of *all* possible pathways and their interconnections would yield a completely intractable problem. Hence, in the US system, accidents resulting from several things going wrong at once have been excluded and situations of human error in response to crises cannot be modelled at all. Yet at Three Mile Island it was just such a combination that created a classic accident.

Even the probabilities that are inserted at the nodes of fault trees become highly inexact or even speculative under such circumstances. Without a long history of use, even the failure rates for pumps and valves are guesswork unless there is a special and expensive testing programme for each such component. It takes only a few such 'guesstimates' to yield a very different impression; four probabilities in a chain, each 'optimistic' by a factor of ten (well within a reasonable range of inexactness) will then produce a risk apparently ten thousand times less severe, which may well bring it up from 'unacceptable' to 'acceptable' on the scale. And in the absence of good historic data, such probabilities are derived either from theoretical models of a physical process or from experts' estimates — frequently reducing to guesses in both cases.

Judgements about Quantities in Risk Management

We can now see how the assessments of real risks for policy purposes may become a much less tidy affair than the simple calculation 'intensity = probability \times harm' would indicate. This should be no great surprise to those who understand physical science. Indeed, the surprising thing is that some very abstract and simplified models of physical systems have, over the past few

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centuries, had such extraordinary and unprecedented power for human understanding and control of the physical world. But the particular interest of risks is that the complications to be coped with are not merely of the natural systems under study, but also of human beings, as we react to events and also as we try to describe paradoxical phenomena.

As a first example we may consider how qualitative judgements become very important, even crucial, in the expression of quantitative descriptions. Suppose we have an element of a fault tree, whose probability is known to within a factor of ten. If we write it by some suggestive convention, as (in computer notation) $p = E(-6\pm 1)$, then we may convey a misleading impression of precision in the ± 1 . But we cannot write 1.5—that would be absurdly over-precise! How to indicate the inexactness of the inexactness? That problem must be left for another occasion.

Further, the choice of a 'representative' numerical value within the *band* of reasonable values of an estimate will itself have policy consequences. A string of most-pessimistic values would probably (!) involve overstating risks; and optimistic ones, understatement; yet what is sacred about those in the middle? Sometimes, if the calculations are not too complex, a statement of whether 'optimistic' or 'pessimistic' values are chosen (with a quantitative indication, necessarily inexact!) can be helpful for clarifying subsequent discussion. But a display of some values or other, without explanation or qualification, can convey information that is inadequate or positively misleading. Thus we have the paradox that, particularly in the case of risks, quantitative assertions require qualitative judgements of their inexactness if they are to have genuine, useful content.

Another case to consider is where the paucity of the data requires the use of quite complex judgements on probabilities, as inputs, to a decision. Here as an example we have the problem of cracks in the pressure vessels of the PWR reactor. There is a chance that such cracks, once established, will propagate rapidly and catastrophically when the steel of the vessel is subjected to great stresses, typically in the case of some incident involving rapid changes of temperature and pressure. This is more likely to happen with large cracks than with small ones. Also, remote methods for sensing cracks are relatively more successful for large cracks than for small ones. The risk calculation then involves the probability that some crack which escaped detection by being 'small' nonetheless would propagate rapidly by being 'large'. Numbers can be assigned to each contingency but they must have their inexactness, with respect to statistical confidence limits, and also be accompanied by technical discussion of both the testing and the consequence of failure. After all that, the question of the 'acceptability' of the risk of pressure vessel failure by crack propagation becomes hedged in a labyrinth of qualifications and explanations (Cottrell 1982). The final answer may then be 'obvious' to the experts concerned, particularly if all the indicators point in the direction of an 'optimistic' conclusion. But the inescapable presence of judgements about quantities describing uncertainties and ignorance cannot be denied.

The above example displays another characteristic feature of nuclear technology that distinguishes it sharply from other technologies and the runof-the-mill risks that are managed with reasonable success. We can call this a 'zero-infinity' risk; or rather 'incalculable-immeasurable'. That is, the risks may well have been brought down below the point where numerical analysis is meaningful (say, one in a million). Yet the consequences of the most serious sort of accident, involving release of much radioactive material into the air and groundwater, are not to be contemplated as 'acceptable' in any way. This paradox cannot be resolved; in no way can nuclear materials be made benign. In such cases, where the numbers, however forcefully stated, do not carry

In such cases, where the humbers, newtreat terms of the experts are invoked. Rather as in a courtroom proceeding, the experts are assessed along with (or even instead of) their arguments. It may be very upsetting to scientists to be treated like the tame psychiatrists who are trotted out for and against 'diminished responsibility' pleas in murder cases; they deal in objective physical facts, not subjective opinions on mental states. But we have seen that while quantitative facts are still at the core of risk assessments, they are inevitably swathed in a variety of qualitative and value-laden judgements. For experts to pretend that this is not so, only decreases their credibility further. And this 'forensic' approach to the problem is not entirely inappropriate. For what is at stake is not the confirmation of purported knowledge, to be made at leisure by a community of scholars, but a decision on policy, to be made urgently by an agency representing society.

As the credibility of experts is eroded, it becomes ever more difficult to restore or even maintain it. The present severe plight of the American nuclear power industry is partly due to the ineffectiveness of the experts from the industry and from the Nuclear Regulatory Commission before, during and after the Three Mile Island accident.

More recently, observers of risks management have needed to reflect on the strange fact that it was university *students* in America who discovered a serious problem that had eluded all the experts for decades: the intense and long-lasting burden of radioactivity in the metals used as alloys in the PWR pressure vessel shell. These transform—for the worse—all the estimates of the costs and feasibility of decommissioning nuclear reactors (Norman 1982). Previous assurances by experts that 'there is no evidence of risk' then tell more about their methods than about the risk; and future assurances, however well founded, are inevitably discounted.

The science of risk assessment as we know it was created largely in response to the problems of the nuclear power industry. It was shaped partly for internal design methods and quality control and partly for public reassurance. Whatever else happens to civil nuclear power, this byproduct will survive as an indispensable intellectual tool for our coping with an increasingly complex and hazardous natural and man-made environment.

Risks and Their Regulation

Those who promoted civil nuclear power had the sense of being revolutionaries—they had a vision of a boon for mankind of nearly magical proportions. They were doubtless men of intelligence, integrity and commitment. If there was a failing, it was of simplicity. Just as nuclear reactions are essentially straightforward as physical science, so (they thought) should the assessment of their risks be essentially straightforward as a review exercise. But as engineering systems involving ageing, thermal stresses, corrosion and cracks, nuclear stations have proved increasingly troublesome. Similarly, the associated risks displayed novel and paradoxical properties: simple quantitative assertions would not suffice, and unpredictable human errors proved more important in practice than randomly occurring mechanical failures. The injection of politics, of the new 'environmental pressure group' sort, only further confused issues for the scientifically trained experts.

But all that is behind us now; the present question is whether the European nuclear power industry can escape the fate of its American parent. New industries, such as biotechnology, are providing challenges to risk assessment that are still more demanding, technically and practically as well. It is all a long way from the security of examination question exercises, but for any scientist who has wondered how to convert some messy experimental data by some reasonably honest process into an acceptable result, it should not be totally unfamiliar.

PUBLIC PERCEPTIONS OF ACCEPTABLE RISKS

When one considers the various public responses to the risks that it recognizes, and considers their great variety, confusion, and even inconsistency, one is struck by the magnitude of the problem of achieving public acceptance of the various risks that seem inherent in our modern technological order. I wish to stress right away the apparent inconsistency in the public's classification of risks as either acceptable or unacceptable; for even though I feel very strongly that the public has something important to say to the experts about the risks which are imposed on it, still it would be false (and also it would be incorrect and harmful to my argument) if I were to pretend that there is some instant wisdom available to the general public in its evaluation of risks.

There are several possible responses to this phenomenon of the public's fickleness, if I may put it so strongly. One is the simplest, technocratic interpretation, which is to use this as evidence of the incompetence and irrationality of the general public when faced with problems of a sophisticated, high-technology society. This type of public response to risk is then strong evidence against the extension of democracy in public affairs and for the restriction of genuine decision-making power to a technocratic élite. Given this response, there will doubtless be various techniques applied to lull the public into

accepting what the experts know is best for it; but this should not be confused with a genuine attempt to meet the public's anxieties. A second approach is to take the public seriously, at least as a political force, and to try to see just what rational structure there might possibly be in its choices of risks to accept or reject. There might then be developed a taxonomy of risks, with some genera being found less acceptable to the public (regardless of their intrinsic degree of hazard). Then our policies which involve the creation of risks could be tailored to fit more comfortably with the public views or prejudices. This is still in the realm of a technical fix to the problem, in that it attempts to shift from a problem involving values to one where scientific methods, in this case at the administrative rather than the physical level, can suffice. Let me make it clear instantly that I do not oppose such taxonomies, but I have reservations about their effectiveness in solving a problem of this magnitude.

Finally, we may use this particular phenomenon, and the genuine crisis in government which it now threatens to create, to reconsider the whole field of risks, their production, their understanding, and their management. I am convinced that without such a deeper perspective, any attempts at management of the problem will be seen, and sometimes correctly so, as attempts at manipulation. And given the degree to which the problem of risks is political, then such attempts are liable to be rejected quite independently of their genuine motives and merits. My own thesis is that the peculiar structure of risks, both cognitive and technical, which is revealed by the variety of public perceptions of acceptance takes it out of the realm of scientific control on the technical side, and out of bureaucratic or manipulative control on the political side. For the management of risks in our present high-technology society, we must recognize that we can no longer apply the politics of 'economism', where quantitative differences can be negotiated between opposing sides; rather we must accept some features of the politics of 'ethnicity', where deep differences of values must be confronted and then used for the mutual education of both sides.

Cognitive Aspects

The first thing that we must appreciate about risks is their totality in human experience. Every human action (or even a decision for inaction) involves some risk, and the risk from any given action has ramifications for all other areas of human experience. Therefore, the hope that one can give a taxonomy, an evaluation, and finally a technical fix to the problems of risks is as ambitious as trying to put all of human experience and values on to a scale of measurement for mathematical or political manipulation. The variety in the public perceptions of acceptable risk partly reflects the variety of life itself. Our understanding of risks encounters certain quite severe difficulties which, I believe, are unique among problems of an ostensibly experimental or scientific character.

There are three basic contradictions in our cognition of risks. In many important senses risks are incalculable, unimaginable and uncontrollable.

For the first, we may observe that unless a hazard is so very common that the probability of occurrence is right up into the range of percentage points, then it lies below the limit of where anyone can normally make consistent calculations with quantitative estimates of probability. When we consider major hazards, of the sort where damage of an occurrence is very great, but the probability is very small, perhaps down to one in millions or even billions, then the phenomenon is like losing on a gigantic lottery. It is then in the realm of 'luck' rather than of a calculated gamble. The impossibility of calculating the risk in a gambler's way leads to a deeper sort of contradiction, namely that it is extremely difficult to imagine the hazard as affecting oneself. The attitude 'it can't happen to me' may indeed be the appropriate one for a sane and healthy person. Avoiding or preventing the occurrence of the hazard cannot then depend on a lively imagination of the outcome, but on some other considerations of costs and benefits, deriving perhaps from the system of control of the hazard. Finally, I would say that risks are conceptually uncontrollable, in the sense that one can never know, either in advance or even in retrospect, whether one did enough to prevent a hazard from being realized. Even in retrospect one is still left with two questions: how much more action would have been necessary to prevent it? and would such action have been within the bounds of reasonable behaviour? Thus all three contradictions are facets of the same phenomenon.

Now I am not advocating that all risks are totally incapable of being subjected to the processes of ordinary reasoning. Certainly, we do calculate probabilities, we do act with caution and prudence, and we do try to prevent the occurrence of accidents. However, my point is that the application of logical and mathematical reasoning, personal imagination and prudential measures does not have such a close relation to experience, either good or bad, that we can claim to be in control of the phenomena, either perceptually, personally, or technically. The reasoning involved in talking about risks is probabilistic rather than simply causal—i.e. we do not have simple chains of inference, inductive or deductive, and simple lines of confirmation or refutation, as we do in other areas. The inadequacy of causal reasoning, combined with these basic contradictions in thinking about risks, leaves us without adequate tools for their comprehension and management.

Probability Calculus

Some might argue that the application of a probability calculus to the analysis of risks solves these problems. I would strongly disagree. 'Probability' (in this sense of quantified likelihood) is an extremely new idea in our culture; only a few centuries have elapsed since its very first conception. The meaning of probability is multiple and, in each sense, obscure and confused. Also, the application of a probability calculus (even under such reasonable rules as we possess) to any real situation is (if properly done) as far more complex exercise than is realized by all except a very small minority among expert practitioners. Strictly speaking, any probability has meaning only when fiducial limits are attached to it; and furthermore, to be quite strict, and probability assertion has meaning only as a result of the testing of a hypothesis about an experimental situation. Certainly, we can quite effectively use probabilities as loosely and unrigorously as we use most other scientific concepts and tools; but we must face the fact that this is, as yet, a new and very rough tool, quite imperfectly understood. To blame the public for inconsistent reasoning with probabilities, when so much nonsense is purveyed by certified experts, is hardly fair or useful.

The burden of my argument is that reasoning in the scientific mode, either causal or probabilistic, does not suffice for getting us through the arguments involved in the management of risks. In other areas of experience where this is the case, we can replace or supplement scientific reasoning by a sort of 'personal' knowledge. In this, belief can be based partly on authority, and values and the decisions they constrain are based partly on personal affiliations. Indeed, that is the sort of reasoning which actually holds society together; the great insight of conservative political philosophy, as well as the force of conservative politics, is that explicit reason has only a minor and marginal role to play compared to unquestioned custom and tradition in society. In the areas of technological risks whose acceptability is now in question, this 'conservative' support is not available in full strength. First, the problems themselves have a technical component which calls for a large element of scientific discussion; this is in principle antithetical to the personal, 'conservative' approach. Moreover, the very novelty of the problems takes them out of the realm of the customary; here they lack that mellowness of age, which enables the conservative type of authority to be developed.

The political problem of risks is in these ways analogous to pre-revolutionary situations in societies under stress. In these the conservative style of resolution of tensions and conflicts becomes ineffective quite simply because it is no longer accepted. Societal cohesion must be maintained, if at all, by other means. Thus the problem of technological risks has structural features which put it in a distinct class, being capable of effective control neither by a purely scientific analysis (resting on an accepted expertise) nor on a 'personal', conservative, type of authority (on which our society normally depends for its smooth running). In consequence, when there is a polarized debate on risks, appeals from the authority of one side, whether based on scientific expertise or on political charisma, are very vulnerable to refutation or rejection. And because of the cognitive structure of the problems, there are in them no adequate internal barriers against a debate slipping totally out of control, with the two sides losing that element of dialogue which is necessary for a genuine resolution.

Risks and Their Regulation

Technical Aspects

Any discussion of technological risks must begin with a reminder of the great reduction in the hazards to life which has taken place, at least in the rich countries of the world, over the last generations. Life is now safer than it used to be; not merely are we better protected against micro-organisms that use us as hosts, but also in all aspects of daily living and travelling, there is no doubt that we are safer as well as cleaner than ever before. In the poor countries the situation is far more mixed. There have been some very dramatic reductions in the hazards of life, particularly in the cases where Western medicine and sanitation are appropriate. But many traditional hazards survive, and now they are supplemented by high-technology hazards exported by ourselves in the form of many new practices and pollutants. Here I cannot go into the trade in 'bads' or 'dyscommodities' which now parallels the trade in 'goods'; at the present time it is partly an exchange of environmental toxicants (from the rich) for human ones (from the poor). Any serious discussion of risks in the global context would need to incorporate this aspect.

Concentrating my focus on the technological risks of the advanced societies, I will say that in general they defy an approach to their reduction that is based on scientific analysis and piecemeal hazards engineering. For examples I will consider two possible exceptions, the first real and the second apparent. With aircraft accidents we have a situation rather beautifully under technical control. The possibilities for mechanical failure and human error are scrutinized in the light of the rare accidents and incidents that occur; and, given the very large number of repeated hazardous situations, there are enough of these to support a probabilistic analysis. Hence they are controlled in order of severity, and the mode of transport becomes safer all the time. By contrast, the causes of road accidents are complex, and lie deep in political and personal attitudes and values; speed and drugs are still the main cause of death and injury on a scale which, if inflicted by a political enemy, would be totally unacceptable, and perhaps even destructive of the social order.

This contrast is most striking in connection with the risks to pedestrians, which are systematically underplayed in all official and industry pronouncements in spite of constituting an epidemic of violence; and particularly the risks to children, which are orders of magnitude more prevalent than the risks of the traditional diseases. Since strictly obeyed speed limits would significantly reduce the scale of these assaults, we have here a good example of a social 'acceptability' of a risk imposed on some for the sake of convenience and gratification for others.

The case of automobile accidents, then, is only partly a technical question; it is now even more a political, social and moral problem. But the same was true of the health hazards of the Victorian industrial cities; and by gradual, many-sided reforms (which, of course, required heroic campaigns for their accomplishment) they were eventually conquered.

However, the characteristic hazards of modern times, those which (to the

chagrin of the associated experts) cause more agitation than the endemic disease-type hazards as from automobiles, are very different. These are of the 'major hazard' sort, where the maximum estimated probability of an occurrence is very low indeed, but the possible damage from an occurrence is exceedingly, or unacceptably, high. It should be fairly well accepted on all sides that such hazards, particularly in their more remote effects, are in the realm of what Weinberg (1972) has so aptly called 'trans-science': where scientific data, however necessary for a partial exploration of the problem, will be impossible to compile for its total resolution. Moreover, the structure of the hazard, as a concatenation of possible causes and effects, is always complex and depends strongly on human vigilance and commitment. Hence the probabilities which are assigned at the nodes of an analysis depend more upon moral factors and judgements than they do upon repetitious events like the throwing of dice or the random variations of mechanical equipment; and they should therefore have very wide fiducial limits indeed. When one carries such quantities with their 'error bars' or estimates of inexactness through a complex calculation, the result may quite likely have such a wide band of inexactness as to lose all effective meaning as an estimate of an absolute risk. To pretend otherwise, and to use hyper-precise numbers (just the digits with no indications of inexactness) in a calculation, is to compromise scientific integrity. To proceed further, and to compare the results of two such exercises applied to quite different sorts of risks, involves losing political credibility as well.

Looking at these 'major hazards' as a class, we can see that, up to the present at least, there has been a tendency for them to increase in severity through causes arising out of our prevalent styles of science, technology and administration. In the first place, the division of intellectual and managerial labour in a large bureaucracy with its attendant constriction of vision from any one vantage point makes it possible for hazards to grow unnoticed from within the bureaucracy. Indeed, there are significant cases on record where bureaucracies either denied the existence of obvious hazards, or tossed the problem around from office to office, since a recognition and action would have been too costly in personal terms for any one individual.

Also, in this period we have witnessed the dominance of a 'gargantuan' style in technological design. This affected first oil tanker ships, and then nuclearpowered electricity-generating plants (outside France). The technological or financial rationale for this growth was always thin, particularly when design was considered in the context of construction and operation. But in the culture of bureaucracies, bigger has always been beautiful in a variety of ways; and so our major hazards have inevitably become more concentrated. This trend may now be reversed through changes of style and aesthetic; some of our major hazards may thereby be alleviated, but the essential problem will remain as one characteristic of our high-technology society.

The final technical aspect of hazards needing discussion is rather delicate, and my viewpoint may be considered contentious. This is that the competence of those responsible for managing hazards is liable (more than in ordinary

science or industry) to be inadequate to the task. Hence in those areas of our technological society where quality control is most crucial, it can be at its most difficult to achieve. Evidence for this is supplied by the published enquiries into disasters in the USA, such as at Three Mile Island and Challenger. Indeed, the continued occurrence of 'incidents' of various sorts in American civil nuclear reactors has been important in depriving the industry there of what was left of its credibility after the near-catastrophe of 1979. Why Americans should have these problems more severely than other nations is a problem that could be answered by 'the anthropology of quality control', a topic beyond my concerns here.

For an analysis of 'major hazards' we note first that they are liable to occur in technologies that are complex or novel to some significant degree. There have been no opportunities for the development of the craft skills whereby small. scale mishaps are monitored and contained, and a craft wisdom developed of how systems can go wrong and be righted. Also, new installations, with their combination of inexperience of operatives and instability of operating conditions, are particularly at risk. Further, the problems of analysing and containing hazards are as yet not standard or salient in the training and outlook of scientists and engineers; courses in such topics are conspicuous by their absence in formal curricula. Further, the bias of the leading schools in risk analysis, towards formal probabilistic models and away from craft experience and intuition, predisposes all concerned to concentrate on the scientific aspects of the problem and to ignore the human and moral aspects. Thus the major nuclear power accidents have all resulted from 'errors' of sorts that were not included in the models. One might say that a theoretical scheme for hazards that omits the category 'a disaster waiting to happen' is likely to be an abstract exercise, providing comfort to some but otherwise not much use.

This analysis would be incomplete without a reminder of the strains and distortions on practice to which the large-scale innovative technologies are particularly prone. Three Mile Island was finished in a hurry so that the owners could obtain a tax rebate; and with the Space Shuttle NASA had to prove that it could accomplish an impossible mission. In this latter case, the burden of proof of hazards was explicitly imposed on those who warned; if a system had operated safely two dozen times already, this was effective evidence that there was nothing to worry about this time. Finally, we should also keep in mind the tendency of bureaucracies to stick to decisions once taken, however insecure their foundation, because of the political costs of change and of public admission of error.

The same considerations hold in the case of hazards which affect the natural environment through pollution, either chronic or potentially disastrous. There it is common to find a 'David and Goliath' situation, where an embattled community, commanding resources in background education and self-taught expertise, engages with a slow-moving bureaucracy whose relevant expertise is inexpert in the ways I have described. My own education in these matters began with a consideration of the story of the proposed nuclear reactor at Bodega Head, California. In this pioneering case of environmental conflict, Pacific Gas and Electric, with the co-operation of the University of California, fought for years to defeat an opposition which maintained that the reactor was on a shelf of rock that sloped down towards the sea, less than half a mile from a branch of the San Andreas Fault. Asking myself how this could happen, I began to articulate the ideas that are developed above. In the case of major hazards, an analysis which treats them as a purely physical problem is bound to be misleading, ineffective and in the long run counter-productive.

Social Aspects

Having reviewed the difficulties in the way of a scientific approach to the analysis and control of major hazards, we can now see how these affect the social aspects of their management. In any problem of decision there will be one party (at least) which must go away dissatisfied; and for decisions to be accepted, those making them must be accorded legitimacy by all the parties to the debate. In the absence of such legitimacy, debates will tend to be resolved by simple coercion; or, alternatively, those who lose in a particular decision will feel entitled to carry on the struggle by whatever means. Thus the risks problem brings out in a particularly sharp form the phenomenon which Habermas (1976) has called the 'crisis of legitimacy' of modern society. For the authority of those who make decisions does not, as in former days, derive from a merit based on blood or wealth; it derives really from what the Americans have called 'the consent of the governed', based on the competence with which authorities exercise their functions. Now, we have just seen that in the case of risk problems, such a competence has a severe inherent limit. The transscientific character of the problems themselves, the immaturity of the sciences which occupy crucial positions in any argument, and the absence of a logic whereby inexact estimates can be translated into sharply defined conclusions and decisions, all seriously compromise any claims of a deciding authority to scientific competence of the traditional sort. Yet, in the absence of a scientific competence, what other basis is there for its necessary legitimacy?

The character of the outcome of realized hazards leads to difficulties in any negotiation on their accepted levels. After all, we are dealing here with the possibility of injury and death, perhaps on such as scale as to produce severe social dislocation and damage to the fabric of civilization. Few will now claim that harm of such a character can be reliably or usefully represented on a monetary scale to any degree of exactness.

This means that we cannot rely on what has been called the 'intersubjective comparability of utilities' on which our economic analysis has depended as a fundamental axiom.

Negotiations over hazards therefore cannot be easily reduced to the same sort as negotiations in economic affairs, even those which produce coercive outbreaks, as in strikes over pay. In that case, in the last resort, there will be a

compromise somewhere between the two positions on a basically quantitative scale, perhaps as modified at the fringes for political or psychological effect. Here the only quantitative scale is one of inexact probabilities, and these are qualifying an event which at a high probability is absolutely impermissible. Moreover, the juggling with probabilities can easily take on the substance of a ritual; very roughly speaking, a risk of one in a million (on some appropriate scale) is considered 'acceptable', while one in ten thousand is definitely not (except in the work-place). Now, that is a factor of a hundred, which is really not much of a definite band in which to negotiate, when the risks themselves are so imperfectly known.

Yet we must not ignore the economic aspect of any existing hazards. When we are reminded that the amelioration of a hazard would be impossible or counter-productive because the remedy would cost too much, we are thereby shown that someone is, in effect, putting lives and limbs at risk because it is cheaper to do so. I must hasten to say that the action need not at all be undertaken in a cold-blooded spirit; the basic contradictions of hazard analysis affect the reasonings of those who impose hazards on others just as much as those who inflict them on themselves. It could be that a more skilled management of uncertain quantities could ameliorate some of these problems; if experts could avoid the traps of hyper-precision, and reason in orders of magnitude when appropriate, then it could at least be clear when there is a scientific foundation for distinction among risks, and when the problem is in the ethical or political/economic realms. Regardless of the self-conscious understanding of any of the agents, it is possible to see cases of the imposition of risks on others as a form of oppression. Indeed, this style of oppression might be characteristic of our affluent high-technology society, as distinct from those others where the strong have oppressed the weak by simple overwork and underpayment, along with lethally hazardous conditions.

One further feature of technological and environmental risks should be mentioned, as a corrective to some simplistic ideas that are held by many theorists. This is that only rarely is it possible to make a simple 'decision' by which the problem is resolved once and for all. Rather, the management of any large-scale hazard is a continuously evolving process that may extend over decades. In the case of industrial plant, it can start with the first intentions to design and build, continue through construction and start-up, be modified but still present as a monitoring task during normal operation, and finally produce new sorts of problems in decomissioning and disposal. Each of these phases has its characteristic scientific, engineering and political aspects. And while bureaucracies, unlike individual protestors, are eternal, citizens' movements can persist a long time, and it only takes one victory on their part to stop a project forever. Hence we would do well to stop thinking of simple 'decisionmaking' on risks, and concentrate instead on the complex ongoing process of managing risks.

Consequences of Failure

The problems of risks, from technology and the environment, are not central to our societies; we still worry more about employment and inflation. But they have been growing, in size and severity; and all the indications are that they will continue to do so for the foreseeable future. Looking at them as a task for management by the institutions of society, I have argued that they present new challenges. For by their nature they deprive those institutions of the traditional supports by which their legitimacy is maintained; and yet the work of these institutions is uniquely sensitive to the trust placed in them by those who are affected by their decisions. This combination of importance and fragility of the institutional management of risks derives from the cognitive, technical and social aspects of the task.

Although the multiplicity and variety of risks problems precludes any simple assessment of success or failure (unless there should be some overwhelming threat that is coped with with greater or less success), overall it will eventually be possible to have some assessment of the success of the management of risks in any given nation. That success will be related to the achievement of a consensus on risk management, so that those who lose on any particular issue will not feel obliged or entitled to carry their grievance outside the constitutional channels. It would be as well to consider the consequences of a failure of effective management, and the resulting breakdown of consensus.

The failure of a consensus in any area of public concern may take some time to show its effects. For in the absence of a crisis, the routine work of government and society can continue more or less undisturbed. It is only when a new, serious challenge is presented, and cannot be met because of a lack of intellectual or moral resources, that there is a discovery of a degeneration that may be irreparable. In the meantime, there are only trends, any of which may be contested.

The most notorious example of the effects of a failed consensus on risks management is the nuclear power industry in the USA. Although some plants are still going forward, new orders stopped a very long time ago, and there are famous cases where protestors either stopped a reactor or even drove a utility into bankruptcy. Those responsible for the development of genetic engineering are well aware of the importance of the 'dread' factor in the public perception of risk, however much they may consider such fears to be groundless.

These localized technological and environmental risks have brought into being a new sort of politics, where NIMBY groups (Not In My Back Yard) join with ideologically based pressure groups to fight off LULU plans (Locally Unwanted Land Use). This might be merely an enlargement of the politically active consituencies, to match the development of the problems of managing technology, except that such groups can become militant and coercive to a degree that is inconsistent with their general social location. One reason for this is the breakdown in trust in the institutions that are planning the LULU,

and also in those that are supposed to be regulating the problem on behalf of society and themselves. Further, they see themselves as victims of a policy which imposes severe costs on them on behalf of an impalpable generalized benefit for all of society. It would be hard to assuage this grievance except by instituting a new principle of compensation for local risks; and no government is eager to open up such a new source of claims on the public purse.

One result of this new source of confrontation is that a new militancy in political action has developed, and spread even to traditionally deferential and law-abiding societies like England. The earliest manifestations of this activity were in connection with protests against motorway plans, and even more, against the rules under which enquiries were held. This brought a partial victory; and since then the major investment decisions, as on civil nuclear power, have all been accompanied by lengthy enquiries which gave at least a show of concern for risk and environmental factors.

So long as such struggles are focused on proposed new developments, they may be seen as marginal to the ongoing political and economic life of a society. They do become a serious nuisance when the disposal of wastes is in question. The sheer bulk of ordinary wastes, the menace of toxic wastes and the special horror associated with nuclear wastes make all aspects of this problem particularly fraught. We are all learning the truth of Barry Commoner's axiom of ecology, 'Everything has to go somewhere' (Commoner 1966); and there is no 'somewhere' that is free of political or environmental worries. Of course, it is possible that the rejection of such wastes, for deposit or even for transport, will eventually force the waste-producing authorities to think again about what they are doing. It would be no bad thing for such political struggles to induce a transformation of consciousness (it has already happened in the case of nuclear weapons); and following on that a change in the style and values of the technology itself.

The issue might become more serious, should there be some major challenge, perhaps on the analogy of a Chernobyl accident where the radiation was approaching the acute danger level, or perhaps in the framing and implementation of plans to move populations away from land likely to be flooded and then submerged through the rise of sea-level in the anticipated greenhouse effect. There are many other possible threats to the stability of any of our societies; and those concerning risks and the environment are by no means guaranteed to become the most salient. However, they are there, and will certainly grow before they decrease. Hence as a problem in government, they deserve serious consideration.

Remedies and Recommendations

Since the political dangers I have just described are in many ways inherent in the character of the technological risks we now face, there is obviously no simple way out of them. All that one can do is to suggest measures which could lead to a better understanding of the problem, and eventually towards its amelioration in various ways.

Starting at an immediately practical level, I believe that we could do well to have a close scrutiny of technological design as it has developed in the post-war period, and an examination of the sorts of styles, values, and fashions which have operated, perhaps quite unconsciously, in major spheres. The influence of vested interests and institutional pressures of various sorts could well be brought into such a study, so that we would have a better appreciation of the shape of that thing which must be submitted to social control, if we are to have a consensus, or indeed survival.

I see the problem of risks not so much as one of decisions as one of regulation, and I have also become keenly aware of the difference between the national styles of regulation in America, Britain and continental Europe.

I believe that attempts to transfer ideas and techniques from one culture to another may be frustrated in the absence of an understanding of the influence of social and cultural context. Certainly, a study of contrasting styles in some sample field (as I have seen in the case of recombinant DNA research) could be quite illuminating.

Stepping back slightly from everyday practice, we might review our attitudes to the 'participatory' aspects of decision-making on major technological risks. How much do those in authority still view them as a nuisance, a diversion from the real tasks that must be endured for strictly political reasons? I would suggest that we would do better to welcome them as an essential part of technological development in the modern world. For the problems involving technological risks *cannot* be reduced to exercises, puzzles for solution in one or two established exact sciences. The various sorts of fragmented certified expertise are no longer competent to predict and control all the manifold unexpected and undesirable outcomes of new developments. The problems should be seen necessarily as a dialogue rather than as a linear demonstrative style. The greater the variety of viewpoints, the more effective imaginative insights will be achieved.

The time and resources spent on such methods at the outset of developments may seem large at the time. But they are always a very small fraction of the total cost. And their sheer savings, in avoiding redesigned or aborted projects later on, can be significant.

This change of approach to the management of risks would involve important changes of attitude. In the first place, it appears that a set of attitudes which may be called 'scientism' may well be too rigid or even brittle to be allowed to survive unchanged in this coming period. Certainly, we should be able to recognize that in many important fields the certified (academically trained) expertise does not have a monopoly of competence in all aspects of the problems. In the USA (a country more open and pluralistic than the UK in many ways) they already give standing to local amateur 'public interest groups', lending them financial support and even helping them organize training programmes for their own home-grown experts. A similar process is just now

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starting in England, at least with respect to financial support. Of course, to relax the claims of certified expertise to a monopoly of competence in particular areas has its own dangers. However, when one is in a situation where all the problems are trans-scientific and where the relevant sciences are frequently immature, the pretence of a monopoly of expertise is one that can become counter-productive. Consequent on this, we might as well begin to revise our image of science itself, and reconsider which sort of natural science should be the paradigm example.

Relevant Science

For a very long time the 'real' science was considered to be physics, with its combination of precise experimentation with powerful mathematical theory. The triumphs of physics in these past three hundred years do not require any elaboration here. But it may be questioned whether this is the science whose characteristics are the most relevant to the problems we now face, and to the sorts of solutions we can achieve. If one were to consider toxicology, climatology or nutrition, or even energy forecasting, we would then encounter a very different picture indeed. Here we do not find scientific knowledge rolling back the frontiers with ignorance, and its applications steadily increasing human power, as on the old scheme. Rather, we find ourselves in situations where facts are uncertain, values in dispute, stakes high and decisions urgent. One traditional image of science was that of a map, where the representation was becoming more accurate without limit; now we have a reality that is changing more quickly, under human disturbance, than we can keep up with in our scientific mapping.

I would suggest that our conception of science should shift in the direction of these new problems and areas of expertise, in our work of popularization and teaching. This would of course be a vast undertaking, with many changes of attitude, style and power consequent upon it. But for an effective use of science in an era when risks are certain to increase before they abate, this would seem to be the only appropriate vision.

If I were to think of the most urgent well-defined educational task, I would suggest a reconsideration of mathematics as it is used for describing the world of experience. For a very long time we have been accustomed to thinking of mathematical statements as essentially precise, and admitting of no inexactness in content or formulation. Now, every competent scientist in a healthy experimental field knows that all of his quantitative assertions are inexact, and he or she will have conventions for expressing that quality. However, in most fields, particularly the mathematical social sciences, and in nearly all teaching in educational establishments, this feature of mathematics is barely recognized. An educational reform here would be as difficult and frustrating as in any other area, to be sure; but I feel that on this conceptual change, a great deal would depend.

Risks and Their Regulation

Finally, we should recognize that science as a societal possession is ready for a change as deep as any in its history in modern Europe. Hitherto it has been the possession of a small, self-perpetuating élite, who have been wonderfully effective guardians of its quality and commitment. Now that science has become on the one hand an industrialized branch of the productive machine, and on the other a means to the containment of the risks inherent in production, it can no longer plead the innocence of its philosophical or academic neriods. The experience of technological and environmental risks has shown that outsiders, even self-taught citizens, can make a real contribution to science, if not in research findings then in the highlighting of problems. Conversely, the impotence of experts, as shown in the Chernobyl fall-out fiasco, as well as in the Challenger disaster, was an important object lesson in the limits of science when applied to real technological or environmental problems. It has been said many times recently that the hubris of particular groups of scientists has led to their embarrassment; perhaps the triumphalist ideology of science, so strong and so plausible for so long, has led them astray. Now is the time for a reconsideration of how science can best serve humanity, and the area of techological and environmental risks would seem particularly well suited for being the focus of such a reflective effort.

THE SAFETY OF SAFEGUARDS

Safety has become one of the most salient issues in technology policy. After a record of centuries of progress, in which our world became more safe as well as more comfortable and pleasant, we now find ourselves with large-scale, basic technologies where safety is critical, and yet where it seems increasingly difficult to guarantee it. Civil nuclear energy is the most prominent case here; and by reflecting on the problems of that technology, which few can now claim to have been definitively solved, we might gain some insights into the general problem of safety as a task where technical factors are inextricably combined with those of the social and moral spheres.

It must be hard for someone trained in an older school of science or engineering to admit that some problems are insoluble. Particularly in America, it had seemed that given enough resources, anything could be accomplished once the national will was there. The atomic bomb project set a precedent; and the moon landings showed that even the most visionary plans could be realized. But both of these projects were simple in their goals and also endowed with a special enthusiasm and commitment. Other sorts of projects, not enjoying such fortunate circumstances, were not similarly successful. Even as the moon-shots were proceeding, it was remarked that it seemed to be easier to organize a safe journey to the moon than one across Central Park in New York City. Again, organizing an ongoing civil nuclear power industry that would provide electricity that was clean, safe and cheap called for skills and

attitudes that were very different from those of the Manhattan Project.

Even within more traditional engineering fields, the present period has provided many examples of problems that cannot be solved, at least within any conceivable constraints on their social context and resources. Perhaps the first of these was traffic in towns. Nothing seemed simpler than to enable all automobile owners to exercise their democratic freedom to drive their cars wherever and whenever they liked; if too many were using a channel at its given capacity, then the planners and engineers could simply increase the capacity. This policy has seemed to work in Los Angeles, at least for traffic movement; but then that is a city without a centre, where commuter traffic is as diffused as all other. In the case of cities with centres, it was discovered after a couple of decades that providing storage and even access for all possible commuter cars would require a destruction of that centre to which they would be heading, on such a scale as to deprive it of anything worth going to. So the commuter-car problem was eventually recognized as 'effectively insoluble'.

The problem of safety is different from that of highway engineering; but if anything its management is even more dependent on the social and moral context of an ongoing operation. Whether it is soluble, to an adequate degree, in any given culture is a contingent question; there may well be some where it is, and there are doubtless some where it is not. My purpose here is to explore the elements of the safety task. For this I shall apply insights about the behaviour of people in an institutional environment and of technological systems in a natural environment. Since the problem of safety is largely one of control—including monitoring and, where necessary, applying corrections my argument depends on an analysis of 'safety control' as an institutionally organized human activity. I identify three principles of rule-governed behaviour which are relevant to systems of control intended to ensure safety.

We may start with the old question: Quis custodiet ipsos custodes?—Who guards the guardians? No system of control involving human agents is selfcontrolling as a whole. The hierarchy of control is, in a sense, uncontrolled or open-ended at the top. No matter how many elaborate checks and sanctions are formally built into a system of control, they will operate only to the extent that there is an effective commitment for them to do so, in spite of their inherent personal costs of the controllers. We then have the principle of the 'open-endedness' of such a system, and the need for commitment at the top of any hierarchy of control.

We draw the practical conclusion that there is no guarantee that the socially 'best possible' system of control in a given environment will be adequate to its intended functions. In the case of the global safety of nuclear energy systems, the point might be translated into a maxim: 'Do not circulate weapons-grade plutonium in a country where the chief inspectors can be bribed'. Whether even this restriction would eliminate safety control systems inadequate to their function is something about which experts might argue.

It follows from the first principle that every human control system needs a meta-system for its own control. Since this relation iterates without end we immediately see that 'ultimate' control in any human system must be informal, nersonal, even partly tacit.

That feature of control systems interacts in practice with another property of all systems of rule-governed human behaviour, namely, that it is necessary for operatives to violate the rules sometimes in order to accomplish their assigned tasks. Instead of arguing this in detail, I will simply point to the phenomenon of disruption through 'working to rule'. Hence, it is strictly impossible for a control group to enforce perfect adherence by operatives to any set of formal rules. They must be allowed initiative to accomplish their tasks as they see fit.

This inherent informality and imprecision of human control systems has important consequences for all work governed by standards of adequacy of results. The degree of quality which can be effectively achieved, even within a purely technical possibility, will depend on the commitment of the operatives. A management, however committed itself, cannot arbitrarily define and enforce standards. If the work-force refuses to pay the personal costs of achieving them, the standards will eventually be jettisoned in the interest of the accomplishment of more obvious goals, as formal—or formalistic—'completion' of tasks. Thus, we have the second principle: the 'incompleteness' of the controllability of tasks, and the need for commitment at the bottom of the hierarchy.

Thus, for there to be effective control, of quality or of safety, it is necessary for those both at the top and at the bottom to be committed. In regard to quality, the Japanese are fully aware of this; in their industries, all ranks share in the positive inducements and negative sanctions whereby group morale and commitment is fostered. In relation to safety, the combination of these two principles can explain the enormous variations between institutions, or even within one institution in its earlier and later phases; NASA is a good case in point here. This last example reminds us that no amount of external enthusiasm or pressure can ensure that control of quality, or even of safety, will be adequate for the performance of the required tasks. It is a matter of the organizational culture, one of those aspects that is impossible to quantify and yet which is crucial for the success of management.

The third principle relevant to systems of control is the 'degeneration' of routine tasks. There is not enough 'motivational capital' to go round to cover the multitude of boring, repetitious tasks on the diligent accomplishment of which all monitoring—and hence safety-engineering—depends. It is no answer to 'automate' them. This may reduce their quantity, but it cannot change their quality. Also, using iteration again, we see the need for the routine human task of monitoring the system of automatic control. Applying this principle, we can understand the otherwise astounding reports of lax security at American civil nuclear installations. It needs no arguing that here, as in the former-two cases, the institutional, social and moral aspects of the human environment are crucial in the containment of the degenerative effects of this situation.

The importance of these three principles of human behaviour in systems of control becomes clearer in the light of a fourth principle, relating to technological systems in their natural environment. In recent decades it has become common sense to think in terms of cycles, so that for any process of manipulation of materials, energy or information, there are phases both 'upstream' and 'downstream'. There are no infinite sources, nor any infinite sinks, available as solutions of problems in the real world. In the nuclear field, as in many others, engineering is coming to encompass the tasks of managing flows through all the phases, each one requiring its own appropriate systems of control. With study, some of the 'downstream' phases are revealing themselves as more problematic than those of the central, profitable operation. Thus we now have the problem, which is currently the topic of strongly conflicted politics in the USA, of constructing a repository for the storage of long-lived nuclear wastes that will be 'safe' for at least ten thousand years.

Partly under the influence of external pressures like those of the above example, there is developing as new appreciation of technologies in their natural environment. We are no longer confined to consideration of the safety problems of 'nuclear power stations'; rather the safety of the whole, complex 'nuclear fuel cyclic system' is the issue. The processing, handling, storage and transport of ores, fresh fuels, spent fuels, reprocessed fuels, temporary hot wastes and permanent cooler wastes, as well as the decommissioning of plants and disposal of *their* products, are all equally important in principle. At each phase, the hazards are different, including varied attractions for diversion of materials and terrorism. Perhaps more than any other technology, civil nuclear power has (partly through the operations of the political process) forced an awareness of these aspects, because its materials, particularly in the 'downstream' phases, are so particularly problematic.

These latter phases may yet come to present more problems than the 'central' ones, as part of the general problem of waste management which steadily becomes more acute in our industrial culture. Even though the engineers no longer completely dismiss wastes as 'garbage', a problem lacking in interest or prestige, we are still a long way from a general recognition that 'waste' is a pathological symptom of a technology. And certainly, all the reward-structures of our society act against a proper appreciation of the problems of wastes. Hence we can anticipate a period, some decades hence, when after a quite lengthy interval of trouble-free operation of nuclear reactors, the problems of decommissioning and disposal will obtrude. They may well be costly in all sorts of ways; and yet they will provide none of the rewards for reinforcement of safety control at the top, or the bottom, of the relevant institutions. Of course such a prospect is speculative, but it is not on that account irrelevant. The 'downstream' problems of nuclear energy will have to be solved; and that task will be undertaken in determinate contexts of commitment and morale.

When we consider the operation of such a system, we can do more than observe this or that particular hazard. On the basis of these, we can raise the question of whether the institutional, social and moral environment can sustain adequate systems of safety control. Hoping not to be culturally chauvinistic, I must say that I would be distressed at the prospect of the installation of such a hazardous cyclic system in a Third World country lacking in appropriate technical skills and political stability. What of a relatively unskilled and unstable European country? What of ourselves?

Nuclear reactors are—so far—unique in civil life in the degree to which the physical engineering of their matter and energy systems requires extraordinarily high standards in the social engineering of their systems of safety control. If these fail, now or at some time in the indefinite future, life on this planet will be endangered. Yet these systems of control are vulnerable to the effect of 'open-endedness' of control at the top, 'incompleteness' of controllability of tasks at the bottom, and to the 'degeneration' of routine monitoring tasks. All these possibilities are more acute, and no less serious, in the 'downstream' phases of the nuclear materials cycle, when there are fewer external supports for the morale and commitment, at the top and at the bottom, necessary for the maintainance of effective control. In these respects the hazards of civil nuclear energy are particularly sensitive to the quality of the culture in which the technology is located.

Relative to the conditions of society as we now have it, even in the most advanced centres, there is no guarantee that the problems of safety through the full cycle of nuclear energy are effectively soluble. This is of course an important issue in technological policy. In addition, it opens perspectives of a philosophical sort: for here we have a case where the 'hardware' of a technological system is so critically dependent, for its proper functioning, on the 'soft', social and moral, aspects of society. In this way, nuclear power may have contributed not merely to our comprehension of risks, but also to our understanding of the nature of technology as a human creation.

THE POLITICAL ECONOMY OF RISK

The subject of risks is as varied as human experience itself; each variety of risk can illuminate another aspect of it. Here I will concentrate on those risks where it is possible to identify three parties: those who impose, those who endure, and those who regulate the risks. Clearly, these are 'roles' rather than individuals, since each of us takes chances, risks the consequences, and controls the hazardous actions. But there are some situations, notably industrial and occupational risks, where the three roles are relatively distinct. There, social and power relationships between workers, management and inspectorates are as important as science in any realistic analysis of industrial risk. A shift in this balance of power would be a prerequisite of any policy for the more effective control of industrial risks.

There is no doubt that quantitative studies of risks are essential for their

scientific analysis and effective control. But their results cannot be treated like weights, that are simply balanced against estimates of 'values' in the determination of 'acceptability'. Science, power and ethics are intimately related in every assessment of risks. The more we know about risks, the more we understand how their management is conditioned by the goals and values that are dominant in society. Through a greater awareness of the problems of controlling risks, we can come to scrutinize and eventually to refine them. And a realistic analysis of the relations involved in the 'political economy of risk' is necessary for any real improvement in their control. Such a study requires a new sort of scientific work, in which the committed amateur can make a contribution along with the certified expert.

Some risks, particularly those occurring in ordinary experience like car accidents, can be described quite well by statistical methods. By relating statistical indicators of personal and political reactions to risks, we can obtain some idea of the practical limits of 'acceptability' of the various sorts. But the more we know about these limits, the more complex and even bizarre they seem. For example, pressure groups concerned with particular industrial or medical risks will argue endlessly about hypothetical scenarios, while the twin drugs of speed on the road and alcohol are allowed (and, by official inaction, encouraged) to regularly claim thousands of victims each year.

Any effective policy of risk control must come to terms with such paradoxical features of risks. I believe that they can be expressed in terms of three 'unthinkables'. First, as a matter of personal psychology, hazards with a low probability are unreal; we have no intuitions to help us balance costs and payoffs when the odds against losing are ten thousand or one million to one.

Secondly, and related to this, is the severe difficulty of studying hazards scientifically. Elaborate calculations of the probabilities of complex harmful events are all too likely to be mere pseudo-precision, for accidents and disasters do not follow the statistical model of successively picking balls out of separate urns.

Finally, there is the moral dilemma of anyone who imposes a risk on another, through their activity as designer, inspector, supervisor or worker. A zero probability of harm is just impossible; but if harm occurs, there will always be the question, 'though I did my best, was it as good as it *should* have been?'

These three 'unthinkable' dilemmas cannot be suppressed in the day-to-day management of risks; for that reason risk management cannot become a 'matured science' where standard exercises suffice for ordinary competent practice. Although there is a great need for more competence in risk management, any attempt to create a closed world of certified expertise would be deceitful, and (in the present climate of public involvement in science) ineffective. Some might even question whether there is any chance of a rational dialogue on strongly contested risks (as those of nuclear power and its 'downstream' phases of reprocessing and disposal). Certainly, the subject is difficult; but I would argue that to ignore the political economy of risks, pretending that it is all a question of statistics and perhaps psychology, makes any effective progress on risk management all the more difficult.

The Risk Triangle

We can make a start on managing risks as a social phenomenon by recognizing that there are three sides involved in every hazard: those who create it; those who experience it; and those who regulate it. Sometimes all the 'sides' come together in the same person (say, a mountain-climber). But for most technological risks the sides are largely separate. Although all share in creating, experiencing and regulating risks, broadly speaking, in industry it is managers, workers, and health and safety inspectors whose basic responsibility and commitment lie on the three different sides of the triangle.

Since risks are so difficult to study objectively or even to imagine, it is only natural that the way each 'side' sees a hazard depends strongly on the values and expectations of its role, and that this perception will be very different from that of another side. Hence a manager need not be callous or inhumane to allow a hazard to persist even when warned about it; he just doesn't necessarily see it the same way as others. Nor need the worker take it too seriously, especially if he or she has real 'danger money' to compensate for an unreal slight chance of future harm, or, as is often the case, does not know the extent of the risk. This argument does not, however, excuse crimes: no doubt a murderer or rapist sees the crime in a different light to its victim or to a policeman!

Another consequence of the risk triangle is that the style of the control of risks will mimic, and be influenced by, wider social relations of power. A hazardous environment (at work or at home) is a part of social powerlessness. Hence all debates on risks have an inevitable and inescapable element of politics in them. Although bargaining on risks certainly involves the paradoxes of 'the three unthinkables', a risk policy that excludes such bargaining is all too likely to serve the interests of only one side of the risk triangle: that with the most power to shape perceptions and values.

Recognizing this basic structure makes it easier to isolate the influences that make the control of risks much more than a technical exercise. For example, the government agencies that regulate risks work in a bureaucratic context, where 'success' is assessed by many criteria besides the satisfaction of their publicly stated function — most notably the classic civil service aims of departmental well-being and a peaceful life. Such constraints, when added to the difficulties of imagination and analysis mentioned above, can lead to a total fragmentation of perception and responsibility among agencies charged with regulation. Further, mechanistic solutions, such as 'more flow of information' can be counter-productive. Faced with more memos, people throw away *all* the 'bumf'. The existence of such influences, analysed by Barry Turner (1978) in *Man Made Disasters*, indicates that the modelling of disasters as the

outcomes of series of independent random events is so misleading as perhaps to contribute to the mentality in which disasters occur.

We can better appreciate the actual role of science in the management of risks by putting aside the traditional 'public knowledge' image of science, and thinking instead of 'corporate know-how'. Management, and also regulators, desire (or are even legally required) to keep to themselves much of the knowledge about risks that gives them power. And the data that are collected, indeed the very categories in which they are cast, reflect a conception of the problem that is influenced by the perceptions and values of the 'side' that has the power to collect and process the data.

All sides of the risk triangle are involved in its dilemmas of 'designing for death'; but the official regulators are particularly vulnerable to corruption of their work. In reality, they depend on the goodwill of managers to get improvements carried out, despite the great formal powers they may hold. This forces them to accept unsatisfactory conditions in the short, medium and even longer terms. Yet, through all this, they must present themselves to the public as effective and sole guardians of safety. When the workers actually running the risks react cynically to the inspectorate's claims, the regulatory agencies are driven still closer to management, in order to preserve their public image and self-esteem. The end result of this process is that factory inspectors have often appeared to workers as little more than adjuncts—even adjutants—of management, trapped by their dependence on the philosophy of persuasion.

This is a harsh judgement, and I know that many sincere and dedicated personnel in the inspectorates will think ill of me even for publicizing it. It may be that the many successes of the inspectorates are all small-scale and quiet, and their few failures large and notorious. Be that as it may, there have been enough exposures of highly unsatisfactory conditions by journalists, in the press and TV, to establish the point. Certainly, the failure of the old Factory Inspectorate to protect workers against known asbestos hazards at Hebden Bridge, Yorkshire, and in east London, is something that cannot be denied or explained away except in terms of the corruptions of impotence. Of course this impotence is decreed and enforced by the political masters, who keep the inspectorates starved of personnel, funds and powers; but the personal dilemma of inspectors is no less cruel because of that.

The corruptions of impotence may also help to explain a phenomenon that is familiar, and disheartening, in the field of industrial hazards. This is the 'careless worker', who seems indifferent to the well-posted hazards, or might even seem to take changes deliberately with the firm's property and his or her body. Certainly, there will always be some, of whatever social position, who welcome risks; the prevalence of dangerous sports is proof of this. But we can also imagine the reactions of someone who knows that they are required to 'accept' risks, perhaps even some which are officially guarded against, in the interests of getting through the job on time. They may feel that no one, neither regulators or even trade union officials, has any real concern for their safety. Hence the reaction to helplessness is apathy, demonstrated in apparently irresponsible, even irrational behaviour.

With this approach we can understand the workings of the Superstar Technology phenomenon. (This is the name of an early report of the Council for Science, and Society, drafted by Professor John Ziman.) This refers to the problem of regulating an industry where all the recognized expertise is influenced by those being regulated. What happens then is not merely that the data become disputed, but the identification of the risk problem itself becomes a matter of institutional politics, with the powerful side eventually defining what seems to be a purely scientific problem. A clear example of this is the debate of the 1970s over the possible hazards of recombinant DNA research. There the problem was deliberately restricted to immediate laboratory hazards, rather than being extended to wider issues of environmentalism and ethics. Also, within the narrower problem, the burden of proof was imposed on objectors, so that unless they could demonstrate that a particular hazard was real, it was not taken seriously for policy, as distinct from regulatory ritual. This was confirmed by one of the more reflective of the 'insiders', Daniel Callaghan, at the forum of the National Academy of Sciences in March 1977. Since risks are so deeply embedded in the human condition, no simple

Since risks are so uccpity embedded in the handri therein only of the device will solve the problem of achieving effective or 'fair' control of risks. A change in the balance of power around the relevant risks triangle is a precondition of improvement for the regulation of any risk. Such a shift depends both on formal measures, and no changes of consciousness and of conscience; the two aspects interact. We should think not of a once-for-all solution, but in terms of cyclic processes, of advances on different fronts according to opportunity and effectiveness. And we should never forget that to every action that threatens existing relations of power and privilege, there will be a reaction. The Latin motto, *Quis custodiet ipsos custodes*? (Who guards the guardians?) should be displayed on the portals and letterheads of every inspectoratel

In this context we can see a function for strong and independent trade unions and community groups in the partial redressing of the imbalances of power in the risk triangle. The British Health and Safety at Work Act, providing for official safety representatives from among the union membership, was a major advance in this respect. Outside the work-place, risks are usually too diffuse for there to be any effective organization of those who endure them. Only on some special issues, notably traffic and pollution hazards, is there now a regular, recognized practice of communities organizing to protect themselves against those who impose the risks and the regulators also, if need be.

The study of risks is a clear example of the sort of science that is now emerging as salient in the management of our high-technology society. Here, as in the fields of resources and environmental problems, the established scientists have lost their professional monopoly of legitimate expertise. This does not affect the position of the traditional academic scientists, who have generally engaged in such problems only on an individual basis. What has become questioned is the scientific authenticity and authority of those who work for agencies as employees, or who sit on expert committees in the belief that the problems are not substantially different from those in the laboratory. Many of the important issues have been opened up, and indeed much of the

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relevant research done, by journalists, students, activists and pressure-groups. In this sort of science, the political commitments are open and self-conscious rather than being smuggled in through values concealed as 'common sense'. The maintenance of quality is achieved through public debates in a variety of forums, no less effectively than through private peer-review and journal refereeing. Thus the management of risks is, as well as being important in itself, an example of how there could emerge a 'critical science', combining objectivity with commitment in the study of nature, as already occurs in other fields of learning.

'Risk assessment – a science of uncertainties' was adapted from an article first publised as 'Risk assessment – a science in controversy', J.R. Ravetz (1982) *Physics Education* 17, 203–8.

'Public perceptions of acceptable risks' was adapted from an article first published in *Science and Public Policy*, October 1979.

'The safety of safeguards' was adapted from an article first published in J.R. Ravetz (1974), Minerva 12 (3), 323-5.

'The political economy of risk' was adapted from an article first published in *New Scientist*, 8 September 1977.

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Recombinant DNA Research: Whose Risks?

The debates over the hazards of recombinant DNA research (or, under its British title, genetic manipulation) were among the most important in the field of the social relations of science in the 1970s. I was fortunate in being a participant observer, my knowledge of the political and social aspects compensating for my ignorance of the biological technicalities. My involvement began when I was invited to join the Genetic Manipulation Advisory Group (GMAG), probably because of the reputation I had gained from my work with the Council for Science and Society on risks. This was in late 1976; I attended one meeting of GMAG, and then went on a study visit to the Institute for Advanced Study, Princeton, which had been planned long previously. There I found the hazards debate in full swing; and using my American contacts, I soon found out who was who. I even attended the classic public meeting at the National Academy of Sciences in March 1977, where Jeremy Rifkin launched his campaign, in the style of the 1960s, against the research. Shortly thereafter I returned to England, and played my part as a member appointed to represent the public interest on GMAG, for the duration of my two-year term of appointment. My writings on my experiences were mainly transcripts of lectures; from these I have now produced this account.

The coming of age of physics was marked by the development of nuclear weapons, with the horrors of Hiroshima and Nagasaki, and later the tragedies of those scientists, like Oppenheimer, who could not keep on the right side of the power structures. With biology, it happened on a smaller scale, in the absence of any perceptible threat to humanity or indeed the environment; only a few distinguished careers were destroyed, and it was all over in five years. At the beginning, molecular biology was an exciting and well-known field of science, thanks partly to the great discovery of the DNA double helix and thanks also to the effective publicity about that discovery. There was the usual speculative talk about what these techniques could eventually do for

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humanity, but nothing was, or could be, promised. By the end, the community of scientists found themselves embattled and somehow tarnished, having been publicly accused (however falsely) of forming a self-serving élite; and the excitement of these techniques was then being expressed by venture capital coming in on a large scale, so that leading scientists were simultaneously performing the two roles of scholars and entrepreneurs. All this happened most openly and dramatically in the United States; the contrast with Britain, wellmannered and well-managed, is striking; and the contrast with Continental countries, where the scientists simply regulated themselves as they saw fit, is more striking still.

This brief episode forms a fascinating history in its own right, and has been studied by several competent scholars. My purpose here is to review it briefly in the light of some important lessons that it can provide in connection with debates on risks. In some ways it was like a revolution, not a neat replacement of old by new, but a sequence of events that, once triggered, followed out a logic of its own until the forces of innovation (or destruction) were exhausted and a new normality could be created. Perhaps because for a time no one was really in control, the debates were more open even if less conclusive; and from the succession of issues that formed the subjects of the debates among the various parties, we may appreciate an important property of such episodes. This is that when (as in this affair) the science is not conclusive, then what is actually debated will depend to a great extent on who sets the agenda of debate, and therefore who controls the forum. Furthermore, if the issue is contentious then this may be recognized at the time. The debate is then conducted at two levels: on the substantive issues as chosen; and on the question of the choice, its scientific appropriateness, political fairness, and procedural justice. This multilevel debate occurred openly at a crucial point in the history of the American effort to control recombinant DNA research, Since such clarity is relatively uncommon in risks debates, the DNA episode is particularly instructive.

This history also has implications for the philosophy of science. For in such debates it is impossible to make a neat separation of an objective, scientific core from its subjective, political meanings for interpretation and policy. All the actors in the affair had their own agendas, none of which was completely determined by such scientific facts as may have been available. Of course, this does not imply that the actors on either side were anything less than honourable and competent in their own sphere. Nor does it imply that there is never anything there in the science other than the products of a negotiation. What we have in these difficult risks debates is a mixture of the objective and subjective, the scientific and the political, which by its nature is inextricable. Perhaps a study of such debates will help us to get beyond the either/or thinking that so hampers our attempts to understand the social aspects of science, and to appreciate the need and the possibility of an enriched logic for studying such problems.

The other important feature of this episode is the neat contrast it offers

between the American and British styles of administration. In this regard it might be something of a minor classic case, since the issue is quite selfcontained, without any complications of party politics or diverging political philosophies. The styles of management of the problem in the two administrative cultures were so characteristic of each that the whole affair might have been written as a script.

The outcome of the affair is not particularly encouraging for those who consider greater public involvement in discussions on science policy as a good in its own right. The sporadic character of the American interventions, and the nearly total absence of public debate on the British scene, serve as a reminder that citizens' activity in relation to science can occur only when there is some issue so urgent that the wall of incomprehensible expertise must be breached. This may be happening at the present time in some places (such as West Germany) in connection with the large-scale genetic engineering industry. But that is another story, whose history is yet to be told.

The Beginnings: Classic Microbiological Hazards

We must remember that when the trouble first began, in the early 1970s, this was after nearly two decades of the most exciting period in living memory for biology. From the initial discovery of the genetic code (the term is not merely a metaphor, as we shall see) in 1953, there had been great progress in actual manipulations with the material of heredity, splitting and re-joining it at will. As yet, it was still only on the easiest of materials, mainly in well-known microorganisms such as *Escherichia coli*, a common gut bacterium, and then mainly on some free-floating rings of DNA called plasmids. It was a slight worry that the easiest way to mark particular sites was with a gene conferring resistance to particular antibiotics; but it was not difficult to avoid the risk that these could somehow spread resistance to antibiotics in medical use. However, problems were being noticed, particularly when other micro-organisms, more obviously pathogenic or even possibly so, were used.

To explain this, I must introduce a few technicalities. The manipulation, or splicing, of genes is not done on individual copies, but by the mixing of many organisms in or on a nutrient medium. Furthermore, to effect this operation, it is necessary to have strings of DNA that are as small and simple as possible; even the main hereditary material of a bacterium, in a nucleus, is usually too large for the operation to succeed. Hence the use of the small extranuclear plasmids, with a few thousand or even a few hundred base pairs. Alternatively, there are viruses, also with a simpler genetic structure, and some of them are quite well studied because of their importance for medicine. These simpler structures, on which the actual manipulations are performed, are called the vectors. When after a mixing some of them are presumed to have the desired recombinations, they are then introduced to a host, most commonly *E. coli*, which can be easily and quickly multiplied, or cloned up. In that way, millions

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or billions of copies of the vector, with its recombined DNA, can be produced; they can then be extracted and analysed.

The only catch is that viruses are somewhat mysterious organisms in their behaviour, especially when compared with the classic bacteriological pathogens. Some are plainly pathogenic; with others, it is not so clear, and it is not at all easy to be sure that any virus that cohabits with humans is truly 'safe'. Hence the earliest concern about the hazards of this research was in connection with the viral vectors, not the bacterial hosts. This concern was developing in the echo of a near-catastrophe in which the National Institutes of Health had been involved. It had been discovered retrospectively that some twenty-five million Americans had been inoculated with an attenuated polio virus which had been contaminated with another virus, SV40, which causes tumours in simians (monkeys). If this had been pathogenic in even a small minority of cases among humans, the consequences would have been dire. Hence the officials were justifiably nervous about any manipulations that might lead to a spread of pathogenic viruses, or even a threat or hint of such an event.

Then as the pace quickened, and increasing numbers of researchers joined the work, these generalized worries began to take real and unpleasant form. Some researchers were worried that the micro-organisms in their cultures really required trained and careful researchers for their safe handling. In the classic fields involving dangerous pathogens, this was no problem; people would not presume to involve themselves with materials that were beyond their competence or their facilities. (In British there already existed a Dangerous Pathogens Advisory Group to monitor research done with the most lethal organisms.) There were well-known craft skills and safety routines whereby an experienced worker could quickly decide whether to allow someone in their lab; the classic example is going to a basin to wash one's hands after a preparation: does the candidate look for the elbow-taps? If not, they are out. In this way, a club of experienced and committed researchers had generally maintained quality and safety in the work. But in this new research these organisms were not being studied in their own right, but simply as convenient carriers of the genetic information that was of scientific interest to the researchers whose experience and commitment lay elsewhere.

Hence by 1973 there was an inceasing concern about the safe handling of the viral vectors, with the knowledge that the inevitable fringe of 'cowboys' could possibly do something really irresponsible and dangerous. Attempts by individuals to control who received samples had proved futile or even counter-productive. There was a growing sense of unease, not very sharply focused, about all the various problems of this new research. It is important that many of the researchers were young, with a political and social awareness that had been sharpened by living through the events of the 1960s. There was a sense of determination to avoid being drawn into a situation that would be analogous to that of the scientists who co-operated with the military in the Vietnam War. But just what to do, and how to assess the problems of such risks as there might be, was not at all easy to decide.

The Andromeda Strain Hosts Hazard

The affair went public in 1974 with a classic statement, in the form of a letter to *Science*, signed by Paul Berg and ten other leading researchers, which described several classes of potentially hazardous research, covering most of the interesting work in progress, and called for a moratorium on research in those areas until the hazards had been assessed and safety standards adopted. It was quite unprecedented in the whole history of science for a group of scientists to call a halt to their work, and trust to the force of consensus to ensure that colleagues in other countries did not cheat. If Leo Szilard had been successful in his efforts to get such a moratorium among the much smaller group of atomic scientists in 1938, the subsequent history of the world would have been much simpler and safer.

This was indeed the finest hour of these scientists; and it is understandable that they should have become bitter when, so soon afterwards, the public (or at least those parts made visible) turned their idealistic move against them. This happened for a variety of reasons. It seemed by the force of the announcement that there was some genuine hazard, even if it was not specified in detail. Then, when a scant two years later they announced that it was all under control, again with an absence of scientific detail, that public was puzzled and (given the inevitable changes in attitude) suspicious. There is a deeper background here; especially in America, generations had been raised in fear of 'germs', which were always lurking, ready to attack from such places as toilets, sink-drains, even one's mouth. To be told by the scientists that some germs were so dangerous they wouldn't handle them was reasonable; to be told soon afterwards that it was all right, they're safe now, was not.

Further, there is the question of what sorts of hazards were being advertised by the scientists. These had nothing to do with the issues of incompetent or careless handling of pathogenic organisms as such. Rather, the fear was focused on the possibility of pathogenic features being transferred across species, since DNA had been shown to survive and function in new host species. The areas of concern involved the enhancement of the pathogenicity of organisms, either by antibiotic resistance or by the insertion of genes capable of causing cancer. Although viruses were mentioned, it was only in connection with the cancer problem. Thus the focus of concern was now clearly concentrated on bacterial hosts, carrying the risks either of enhanced resistance to antibiotics or of some carcinogenic property.

The leaders of the research community were thus facing a cruelly paradoxical problem: how to reassure the public that they were going to restore safety to a set of totally hypothetical microbiological hazards? In the letter they were very honest about the weakness of theories and the paucity of data on hazards; and they stressed the need for research. But there were systematic difficulties as well. In order for these to constitute a genuine health hazard, there would need to be a most improbable sequence of events, involving the escape of the affected micro-organisms, followed by a transfer of

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their undesirable properties to wild strains, and then for these to become vigorous and infectious to humans. It was all rather remote, and in its way unreal. From the outside, one could have compared such risks with those of viral infection in cowboy labs of researchers and technicians, who were largely women, and (in America) totally lacking in the protections at work that trade unions can provide.

Once the moratorium was announced, it was obviously urgent to organize research to assess those hazards; but before this, there had to be some means of creating a consensus on what those hazards were. Running recursively backwards, this would first require a conference, and (earlier still) some consensus about its organization and agenda. Quite soon some leading researchers went against the moratorium idea, and criticized and indeed ridiculed all hypothetical hazards when they were articulated in any detail. On the other side; the environmentalists and radicals were beginning to stir. The researchers' shift of problem, to Andromeda strain bacterial hosts from research cowboys' viral vectors, had probably occurred without any single conscious decision, and had the great merit of substituting a neat, scientific problem for an embarrassment on which one might well be reluctant to confide in the public. But it was now well on its way to imposing its own logic on any attempted solution, scientific or political. (The term Andromeda strain comes from a famous science fiction story about an infection from another galaxy, by its nature impossible to control or predict.)

Another feature of biological hazards made their self-imposed task yet more difficult. As the discoverers of the double helix had said, the essence of the genetic mechanism is not so much a substance as a code: information, rather than matter or energy, is the crucial agent in this system. In physical and chemical systems the dangerous things are material, and can be traced and. wherever found, reasonably assumed to stay put, or to move by recognizable channels, or to disintegrate harmlessly. In biological systems, the information constitutes the hazard, and it is carried on self-replicating units; the problems of assessment and control of the hazards are therefore enormously more difficult. Here the information can move in small numbers of invisible carriers; if even only a few copies survive passage through some barrier, they can multiply again, and moreover pass their messages on to hardier relatives. So dilution and attenuation are effective only in the most extreme degree. All this was common knowledge to those working with dangerous pathogens; experience of cases of smallpox and other infectious diseases spread by unlikely routes in labs and hospitals had driven home this lesson. To attempt to impose the rigours of a dangerous-pathogen lab on DNA research would be ridiculous; but there was no calculus of quantifiable risks available which could provide guidance for the defining of controls and barriers of graduated levels of severity.

The Berg letter opened the way for discussion of further hazards, which were to provide a bridge between the medical risks it discussed, and other, speculative risks whose discussion at that point might well have sent the whole affair out of control. The basis of the scientists' alarm was the discovery that genetic material from one species could be inserted into another, and not merely be reproduced but also function there. This meant that the barrier between species, which had been thought to be nearly absolute in nature, could now be bridged at will, by mankind. The adverse consequences of this are difficult to specify in detail, and no one claimed that monsters and chimaeras were going to be produced overnight. But there was among some a sense of awe that something like a central taboo of the natural world was being breached; and we should at least stop to think about it before rushing in to exploit it. This wider problem was mentioned by others, and when it was introduced, the researchers' response was simple: first, that such things have occurred in nature; and secondly, that it is hard to imagine such a man-made creation being sufficiently hardy to become a hazard.

In the discussions of such issues we have the beginnings of a struggle over the choice of the salient problem, rather than in the unnoticed, probably unselfconscious shift from viral vectors to possibly pathogenic bacterial hosts. The scientists could label the broader concerns as speculative and unrealistic, but this was in respect of a programme for biology that was artificially narrow. Some biologists, including the most distinguished among them, had been publicly speculating for decades in a Faustian mode about the modification of life, for the improvement of humanity under scientific guidance. When the rhetoric of promise of recombinant DNA approached the earlier visions, it is not surprising that critics took the more extreme versions seriously as the *eventual* outcome of the research.

Hence by the time that the conference on risks was organized, the issues were already complex, and the lines of future conflict taking shape. It was held in February 1975 at the beautiful conference centre at Asilomar, on Monterey Bay in California, and is remembered by that place-name. It was remarkable for happening at all; but the things that did not occur there were of comparable significance for the unfolding of the story. First, there was a paucity of experts in the fields relevant to assessment and control of the hazards; even a most enthusiastic bacteriologist who supported the research and ridiculed the Andromeda strain hazards, Dr B. Davis of Harvard, later complained that his talents had not been utilized. A leading public health official in California told the press that he learned about the conference from the newspapers. The one distinguished public health official who was there, an Englishman, had always said publicly that this hazard is minuscule compared with the breeding of resistant strains of pathogens through the widespread inappropriate use of antibiotics. Hence it is fair to say that at Asilomar the hazards problem was kept under the control of the DNA scientists, who in this area were amateurs.

Furthermore, the problem was kept within the confines of the Berg letter's statement. One participant from Europe who had hoped that this conference would act in the spirit of Leo Szilard, and at least open up discussion on the questions of the future of this nascent technology, went away embittered. Since every researcher who wanted to stay in the game, or race, knew that he or

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she had to be there, the pressure on the organizers for places was severe; at first they tried to economize on space by excluding the press, but the attempt failed, leaving a certain amount of ill-will. In the space of a few days the organizers had to create the theory and framework of a programme of risk assessment and regulation, and secure the consensus of a research community of highly ambitious, individualistic scientists. A measure of acquiescence was achieved after a sobering lecture by a lawyer on the sorts of legal liabilities that might fall on institutions or scientists, should some of the hazards be realized. And the elements of a solution were provided by another British member, Sydney Brenner, with a proposal for 'crippled bugs' that would grow in the artificial conditions of lab cultures, but die off with convenient promptness as soon as released.

All that accomplished, the researchers went home, and the most concerned scientists, mainly those at the National Institutes of Health, embarked on the Herculean task of contructing guidelines. These were to define standards of safe practice in terms of the two sorts of containment, physical (equipment and standard procedures) and biological (degree of crippling) appropriate for each possible class of experiment; and to produce a comprehensive catalogue of classes and their containments. Since practically nothing was known about the infectious behaviour (in this context) of most of the organisms being used, all the categories had to be based on conjecture about the hazards and about the degree of conservatism necessary to cover uncertainties. Naturally, any such category could be criticized as too severe to allow research to proceed, or too lax in protection. By this time the American style of regulation was in operation, with ample facilities for observation and comment by all interested parties, including those critical or hostile in any way. The environmentalists were becoming steadily more alarmed at the way the affair was being managed; and their criticisms were being relayed to community activists wherever they might be preparing for struggle.

At this point, in 1975, reassurance might have been achieved if there had been a 'crash programme' of large-scale research in risk assessment, as recommended in the Berg letter. But this did not happen, in spite of money being made available by the NIH. After Asilomar, several labs started work on the engineering of Brenner's crippled bugs; and as the difficulties in this became apparent, it was abandoned nearly everywhere. The paucity of serious risks research, in spite of the advertised official good intentions, is a constant theme throughout this whole history; a study of the reasons for this could be illuminating.

The Political Struggle for the Agenda

By this time, the environmentalists and radicals were becoming alarmed. It seemed that problem and solution were firmly in the hands of the professionals. Americans are quite sophisticated about 'the regulation game', and know well that self-regulation can be primarily devoted to self-protection by a professional group. Moreover, at those universities which decided to look into the risks problem on their own, it was soon clear that for the administration and leading scientists the question was not whether, but how soon, DNA research would be promoted. In a pattern which has become characteristic of American environmental politics, there was developing a coalition of citizens concerned for their localities (the NIMBY—Not In My Back Yard groups) with ideologically orientated national pressure groups, against a threatened LULU (Locally Unwanted Land Use). This could feed on traditional townspeople's resentment of universities, as being arrogant and overbearing as well as exempt from local property taxes.

It did not help the researchers' public image when in 1975 a book was published that was a sort of sequel to the famous The Double Helix by James Watson (1968). Watson was well known as an aggressive type, given to vociferous and rude comments about any and all who opposed him, as well as occasional remarks that gave offence to women. He was also one of the earliest defectors from the Berg letter, and an abrasive critic of all attempts at regulation. In this new book, the great discovery, and Watson's relations with Rosalind Franklin and her project manager Maurice Wilkins, were viewed from a rather different perspective in the book Rosalind Franklin and DNA by Ann Sayre (Norton, New York, 1975). Briefly, Sayre's argument was that Watson had used the gullible Wilkins to obtain, secretly, X-ray photographs that were the confidential property of Dr Franklin. This is uncomfortably close to academic theft; and the author argued further that this information was crucial in Watson and Crick's making the discovery before Franklin herself. Watson's book had already caused a great stir by its frank display of his all-toohuman emotions and motives; now the demystification, indeed degradation, of the scientific calling was carried a step further.

The crisis came to maturity in 1976, itself the centennial year for the United States. And it was natural for the great confrontation to occur in Cambridge, Massachusetts, the home of Harvard and MIT but otherwise a working-class suburb of Boston with a strongly ethnic population. There the coalition comprised local politicians with many grievances against the great universities, together with university-based radicals whose campaigns dated back to the 1960s or even further. There had to be an incident that triggered the confrontation: this was a plan to construct DNA research facilities in an old building suffering from an ineradicable infestation of ants; a plan which, moreover, had not been notified to the civic authorities.

In some memorable public hearings and offstage debates, the issues were both of the safety of the DNA techniques as to be practised, and the attitudes and behaviour of the scientists and institutions (or of the interfering, demagogic politicians, depending on your point of view). A leading scientist at the NIH, Dr Maxine Frank Singer, came to the hearings with a set of the official guidelines hot off the press, assuming that these would demonstrate the sense of responsibility of the research community; instead she was asked at

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the outset, 'Whose side are you on?' As it eventually turned out in practice, the Boston area was the one place where citizens' involvement in regulation was attempted seriously and where it was successful. A committee of laypersons was appointed to review the regulations, within the confines of the narrow hazards problem, and eventually came up with a broad approval of the official scheme.

By this time, later 1976, the original concern with scientists who were 'cowboys' or simply incompetent in handling potentially dangerous pathogens, had been nearly forgotten. As the debate polarized, the issues went correspondingly to the extremes. There was a conference on hazards at Falmouth, Massachusetts, at which a consensus was obtained informally on all sides that the chances of an accidental disease epidemic were extremely low; this was then advertised as universal scientific agreement that the techniques are 'safe'. On the side of the critics, public leadership was being assumed by Jeremy Rifkin, of the People's Bicentennial Commission, now retitled People's Business Commission. His battle-cry was that the scientists were not merely playing God, creating new life-forms, but worse yet, doing so for private profit and concealing their conflicts-of-interest when they made public pronouncements on safety.

The achievement of a crippled bug, a version of the common *E. coli* that needed a variety of artificial nutrients for survival, did not soothe the critics' feelings. It was significant that this creation, named chi-1776 in honour of the bicentennial, was the only such successfully engineered organism, and that its creator was not a mainstream DNA researcher, but a bacteriologist at the University of Alabama Dental School, Roy Curtiss III. True genetic engineering, as opposed to changing genes where it happened to be convenient, was still in its infancy.

The climax came in March 1977 with an all-American event, an open forum at the National Academy of Sciences in Washington, at which proponents and opponents were paired off in public debate. By this time tensions were running very high, and the leading scientists were feeling quite embattled. Newspapers and television were replete with jokes and cartoons depicting the scientists as manufacturing new monsters daily; and a liberal senator from Arkansas introduced a bill that by implication treated all this research like germ warfare. Worse, Rifkin's rhetoric had been shifting from radicalism to populism with a religious tinge. Even his Cambridge radical colleagues became a bit nervous at his repeated references to church-goers. The separation of church and state, or rather the exclusion of organized religion from matters of intellectual policy, was an issue that polarized America between liberals of all religious views, and the religiously conservative who were later to be named 'the moral majority'. Biologists in particular had the threat of creationism hanging over them; and after that, who knows what could come. Rifkin was playing a very dangerous game, perhaps much more so than he realized or cared; but for the biologists, liberal, intellectual, and many of them Jewish, his brand of populism aroused very primal fears. Hence the issue of

debate was threatening to move right away from biology in whatever form, and into some deep and unresolved tensions in American public life. (As an historian's footnote, I must say that at the time I lacked sensitivity to this dimension of the affair, perhaps because of my long absence from America.)

At the NAS symposium itself, Rifkin attempted a replay of the 1960s. In press statements the weekend before the meeting began, he denounced the agenda, the organizers, and their public and covert sponsors. Thus his leaflet with an alternative agenda started with: 'What are the moral, ethical and theological implications involved in the artificial creation of new forms of life? He also made vague threats about disruption if his demands for a changed agenda were not met. There was a compromise on these, so that a representative of his group could make a statement at the very beginning of the conference. Undeterred, he had his young activists planted in the hall so that at the official opening, they unfurled banners at various strategic locations. (One of them, quoting Adolf Hitler, 'We will create the perfect race', was held in front of the speakers' table; a mischievous press photographer caught it at such an angle that it could seem to be the drop-cloth, and more mischievous editors printed it.) The speech by his colleague Ted Howard was more measured; he simply pointed out that this issue would be with us for a long time, and the ethical and humanistic dimensions could not be willed out of existence by the research scientists.

The debates themselves had a rather tired air, since the paired participants had been boxing in similar rings too many times already. The scientists were very strong on their assurances of present microbiological safety; the critics were equally strong on their concerns for future dangers of all sorts. The scientists also did not resist the temptation to wrap themselves in the mantle of freedom of enquiry and the quest for truth; although as good citizens they agreed that technological development should have no immunity from regulation of its hazards. In all the debating, there was no real dialogue, only a public display of both sides being outwardly polite to each other.

At the conference was one genuinely tragic figure, a sort of Brutus character. This was Robert Sinsheimer of CalTech, who had been a leader as researcher, administrator and statesman in the community. But a few years previously, at the peak of his career, he had begun to ponder on the question of whether scientists could guarantee that these discoveries, which would eventually yield great power, could be controlled for human benefit. After exploring this theme in a number of lectures, he found that he could not solve the problem, and became progressively more disillusioned with his colleagues although not with the ideals of science. His research output dwindled, and a visitor from Cambridge solved the big problem that had guided Sinsheimer's researches and earned a Nobel prize thereby. By the time of the NAS symposium, he found himself in an unlikely alliance with the radical, antiscience critics. His former colleagues and students felt betrayed, for his presence among the ill-assorted opposition figures transformed their public standing. There was much bitterness about his defection; and unpleasant rumours about his motives circulated. Soon afterwards he resigned his Chair, and became an administrator in the University of California system.

Re-Entry, and Transformation

After 1977, the debate dwindled. The critics had had their chance, and in the absence of a demonstrable present hazard, their vague and generalized warnings and complaints were ineffective. Intensive lobbying on Capitol Hill ensured that no unwelcome legislation would get through the Congress. Successive conferences were devoted to displaying the consensus among the scientists that the Andromeda strain risks were indeed minimal, and that the original guidelines could safely be relaxed. The earlier fears about the cowboy scientists were effectively allayed by the guidelines, which prescribed equipment and lab discipline for the more hazardous operations which, even if not always obeyed to the letter, did at least serve to curb any really outrageous excesses. In 1979, the operation went international, and a conference at Wye College, England, worked on the relaxation of the standards for viruses. At that event there were detailed reports of experiments on hazards; and by their paucity, simplicity and crudity they conveyed an impression almost of contempt for the whole exercise.

One result of all the public agitation, certainly unintended by the critics, was that this field became more notorious than it would have done on the basis of its current scientific achievements (or indeed its palpable hazards) alone. The claims of its practitioners, that it would soon be producing substances yielding great benefits to humanity and commensurate profits to investors, eventually became plausible. At first there were only the glowing statements by the early entrepreneurs, who had been among the pioneering researchers in gene-splicing. But by the later 1970s, industrial investment in respectable amounts was coming in, and an increasing proportion of the leading researchers were doubling as businessmen.

For a time this seemed to be causing considerable concern among those raised up in traditions of 'little science'. To be sure, it is always possible for an individual scientist to wear several hats, and to speak freely about his academic research while being discrete about the other. But when a community becomes involved with two radically different systems of intellectual property, value and etiquette, it is hard for it to retain its integrity. Whereas high-energy physics became bureaucratized through the sheer scale of the necessary equipment, DNA research tended to become commercialized through these entirely natural and unavoidable developments. Seen in historical perspective, it is part of the transformation of the social activity of science, from its 'academic' phase to 'industrialization', where the size of the enterprise, the scale of individual projects, and the proximity to profitable technological development all combine to produce corresponding changes in the social relations and then inevitably in the ideology of the work itself.

The British Model-Consensual and Closed

I personally experienced the culture-shock of rapid transition from the American system to the British; only a few weeks separated the NAS forum from my next meeting of the Genetic Manipulation Advisory Group. Going on an early morning train to London rather than to Washington, I found my way to the Giba Foundation, just up the street from the hallowed BBC, was allowed in by the concierge, sipped coffee with my new colleagues, and then went into the closed, windowless meeting room. One or two meetings afterwards, we found on entry that our table spaces had been provided with texts of a declaration about the Official Secrets Act, awaiting our signatures. (This statement itself may make me liable to prosecution under the Act.) I decided not to sign, leaving my conscience free to blow the whistle if my duties as representing the public interest required it, and also knowing that I would be liable under the Act regardless of my signature.

The Genetic Manipulation Advisory Group was the outcome of the British approach to regulation in this field. Soon after the 1974 letter, a committee of enquiry was formed, consisting of eminent and independent scientists under the chairmanship of Sir Eric Ashby; it recommended that there be a supervisory body so that the research could proceed safely. This was planned by a second committee, chaired by Sir Robert Williams; and GMAG came to be in late 1976. As an example of design of regulatory agencies, it had several interesting features. In the British mode of minimal regulation, it functioned by the voluntary co-operation of researchers; it had no enforcement powers of its own. However, all its work was done in the closest collaboration with the Health & Safety Executive; so anyone who ignored the Group's 'advice' or otherwise contravened its recommendations would immediately have the HSE down on them. Thus, the Group could have the reality of power without its outward trappings or formal responsibilities; a device truly worthy of a mandarin.

Doubtless because of its creation under a Labour administration, with Mrs Shirley Williams as the Minister responsible, GMAG had some uniquely progressive features. First, the Group itself had the researchers as a minority presence: eight places, with two for employers, four for employees and four for those nominated to represent the public interest. Thus the workers and public between them had a representation that balanced the researchers and comprised nearly half the Group. In practice, the categories overlapped considerably, since two of the 'employees' were active researchers, one of the researchers was an industrial manager, and one of the 'public' representatives was the polymath editor John Maddox. Hence there was never a polarization on an issue based on conflicting perceptions of what a problem was about.

It is worthy of reflection that three of the members were American in their origins, and another was frequently in America and had an American wife. It goes without saying that considerations of the sort raised by the Cambridge (Massachusetts) radicals, to say nothing of Jeremy Rifkin, were totally absent from the agenda at GMAG. But since there were also absent from public discussion in the country at large, one could hardly expect GMAG to go looking for trouble by taking them up. As a public interest member, I was representing a constituency that was totally unorganized, and perhaps even non-existent! I saw my job as monitoring the regulatory machinery as it was being constructed and put into operation, including such things as checking that people in positions of responsibility (such as Biological Safety Officers) were already trained or committed to getting training. I learned that one of the most important techniques of my task was checking the 'matters arising' against the minutes of previous meetings. It was all too easy for small items to be forgotten from one meeting to the next!

It could be that it was easier to promote this innovative model for GMAG against the inevitable resistance just at that time, because in 1973 there had been a scandal over a smallpox outbreak in a hospital near one of the few laboratories licensed to work on that virus. The whole affair was extremely confused for a long time; but it was clear that the self-regulatory Dangerous Pathogens Advisory Group had not succeeded in preventing this accident. Hence, the radical idea of giving the workers and public interest some say in connection with this hypothetical hazard, even as a reassurance exercise, was less vulnerable than it might ordinarily have been.

A second progressive feature of GMAG's constitution was that the Genetic Manipulation Safety Committees which each centre was required to institute were to have less than half their members from 'management'. This term was offensive to many academics who, in spite of having responsibilities, power and pay on a managerial scale, still wanted to think of themselves as just first among equals in a community of scholars. This hint of a trade unionists' conception of a laboratory was strengthened by another provision, that the committees were to consider the scientific merit of proposals as part of their assessment in passing them to GMAG for advice. The origin of this seems to have been the point that if you are asking people to do something dangerous, it is only proper to show them why it is worthwhile. But since the hazards covered by GMAG were agreed to be only 'hypothetical' at worst, it seemed to some that this provision was an invitation to mischief. However, the Group was not inclined to stir things by changing the regulations with which it was constituted.

As the meetings ground through their lengthy agendas of assignment of categories to proposed experiments, it gradually became clear to me that the scientifically competent members of GMAG considered the hazards to be not very real at all. Early on I took it on myself, as public interest representative, to suggest that GMAG sponsor research into the risks that it was regulating; this was actually the second point of its remit. This proposal received no support, and raised some influential opposition. This was based on the very sensible point that if you tell the public you are researching into the risks, then they will believe that there are some, and become alarmed, all unnecessarily. I then wondered what people were doing on GMAG, especially the scientists who would meet for a long day's work once a month to engage on long, earnest discussions about the category in which to place some particular ambiguous experiment. Eventually I decided that it was largely a cosmetic exercise, designed to reassure the public and also ensure that researchers kept a generally clean shop. In that way scandals would be avoided, which is the great desideratum of any Civil Service, and of the British in particular. (Much later, GMAG learned that the HSE had commissioned a research project on the supposed fragility of the American crippled bug chi-1776. Also, a subcommittee of GMAG commissioned research on the transfer of some plasmid vectors in common use, with results that I discuss below.)

It should be said that GMAG did its bit to keep the labs up to scratch. Even if the categorizations of proposed experiments were to some extent arbitrary, the site visits were most definitely not. There we could see how at even the most prestigious lab, with the most safety-conscious staff, little (and sometimes big) things could go wrong unnoticed. To some extent (though in my view insufficiently) we functioned like a good safety inspectorate, being a conduit for complaints that could be made to us without fear of victimization; we could then make enquiries without disclosing the source of our particular interest.

Scientific questions did become very significant at a crucial juncture for GMAG. One of the subcommittees was charged with assessing and then validating the safety of hosts and vectors. They were able to commission experiments which demonstrated the decay rate of particular populations under particular circumstances. But whether any such number could stand for safety was a question that they (perhaps imprudently) faced squarely, and then promptly admitted defeat. This put GMAG into a crisis; the basis of our categorizations hitherto had been a table which, according to Sydney Brenner, had been drafted by him and inserted into the Williams report over his protests that it would be accepted as definitive in spite of being nothing of the sort. The table was being questioned as more information came in; and now it seemed impossible to create any sort of scientific basis for categorizations. Just at this point (late 1977) it seemed as if the Americans were set to dilute their standards to the vanishing point; and only GMAG would be there to hold the line for safe and proper operation. (No other country then had both the resources and the inclination for such an exercise.) So Sydney Brenner was prevailed upon to overload himself to the danger point yet again and produce a scheme for categorization. This he did within a few weeks; and with interpretations and modification by various GMAG members, it became the standard biohazards analysis scheme for the UK and remains so to this day.

It may be significant that the only really serious disagreements on GMAG were concerned with institutional politics rather than science. The first concerned the status of the trade union members. At one point they said that they wanted to consult with the Trades Union Congress before agreeing to some policy; this was challenged on the grounds that they were at GMAG as independent members representing an interest, not as mandated delegates of the organization that had been asked to nominate them. The issue was taken up to the Minister, and the 'gentlemen's' status confirmed. The most serious struggle concerned confidentiality. Given the traditional, semi-paranoid concern of British industry with secrecy, it was not surprising that firms were reluctant to have their proposals viewed by the whole membership of GMAG. They did have a point, in that if one of us merely mentioned something about a proposal to a rival, that could count as 'prior disclosure' and nullify their patent rights. And the sanctions over us, consisting only of the Official Secrets Act, were not really effective. There was a proposal to have a small chairman's committee review such proposals; and the employees' representatives were adamant in their opposition to this, on the grounds of the remit of GMAG and also of their responsibility to their members. This issue, with its ramifications, came close to splitting GMAG, until an acceptable British compromise was found.

The political temperature was finally raised in late 1978, near to the end of my two-year term. First, the most prominent trade union in the industry, the Association of Scientific, Technical and Managerial Staffs, organized an open meeting on genetic manipulation. This was well attended, but the only critics were a very small group from the British Society for Social Responsibility in Science; and they were not on the platform but on the floor with a single strident leaflet. Shortly afterwards there was a television programme, the second on the subject. For the first, in early 1977, the producers had needed to come to America to interview me, as the only mildly critical person with a British accent that they could find. In this second, the critical voices came from America; the programme could be considered as sensationalist, and was ill-received on GMAG. And then in December 1978 I was rotated off GMAG, for reasons that may be discovered in 2008 when the thirty-year rule permits.

I was later told that after that first two-year session, GMAG settled down to a routine, to the point that the public interest representatives eventually found themselves with not much to do. This clarified my own ideas on what that initial period of GMAG was all about. Its function was only partly in the performing of ritual categorizations; more important was the work done on the margins of the meetings' agendas, in the piecewise construction of a framework for regulation. This was done formally through the drafting and approval of guidance notes on a variety of topics for safe and proper procedures, and informally through the establishment of precedents and understandings on a great many detailed issues. Since, as it turned out, there were no realized hazards which would test, or strain, the machinery of GMAG, there is no proof that the machinery was effective or even necessary. But as an exercise in the enlargement of the idea of regulation in the British context, it had a significance beyond its size or original remit. Not long after GMAG was established, the regulatory climate in the UK changed drastically, so that the example of GMAG was not likely to be followed up. But providing that memories are kept, the example of GMAG will be there, when the tide turns again.

Conclusion

Early in 1979 I was invited to do a brief front-page article for the journal *Trends in Biochemical Sciences*. The editor, although a strong proponent of the research, clearly thought it good journalism to have a piece from me. He could even have intended a minor scandal, since the issue would appear just when everyone was awaiting news of the Wye conference on relaxation of safeguards for research with viruses. For that, I coined the term 'high-intensity science'; and although my definition got lost when the text was cut by the editors, I recall that it involved manipulations of information rather than of matter or energy. Because of this, relatively modest inputs of capital and labour would be sufficient to produce results of great power, in all the aspects of knowledge, applications and hazards.

Equivalent to my thousand words was a cartoon, showing a scientist and a layperson both looking at a packing case labelled 'Environmental Risks— High-Intensity Science'. The one is holding his telescope backwards, and proclaims, 'Ah, yesl Precisely as my scientific colleagues anticipated! The risk is vanishingly small.' The other, viewing frontwards, shows great alarm, and shouts, 'Good grief! It's even worse than I feared! The risk is COLOSSAL.' Nearby is a journalist with a press-card from the 'Daily Trash', whose tabloid front page consists of the headline 'NEW CANCER SHOCK HORROR REPORTS are totally untrue'.

The world of academic science was doubtless turbulent and stressful for its practitioners in all sorts of ways. But in relation to its environment it was insulated and peaceful to a remarkable degree. That is all gone now; in retrospect the insulation of academic science may be seen as a transitional phase, between its predecessor in natural philosophy, never far from ideology and hence politics, and its successor in industrialized science, so very close to industry and (through its hazards) to another sort of politics.

The cartoon in TIBS symbolizes the new sorts of debates that take place around science. They may well determine the course of research that is done (unless the research communities can always maintain total control of the regulation of their hazards); and yet they cannot be conclusive scientifically. Is it all a matter of prejudice, through which end of the telescope one chooses to view? Is there so little possibility of consensus that the gutter press has a free run? In terms of the system of ideas that grew up for explaining and promoting academic science, these questions cannot be answered. If we need some absolute criterion of truth or objectivity in science, then we will surely be disillusioned and fall prey to subjectivity and prejudice. But if we can develop a new understanding of science, that sees it as part of life and society, then debates like those on the safety of recombinant DNA, which are bound to recur so long as we have a science worth pursuing, can be better comprehended and hopefully better managed as well. That is the purpose of historical sketches like this one, indicating by example the sort of philosophy of science that we will need to have.