# A NEW AWARENESS

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As the realities of science have changed, so our awareness struggles to keep pace. Some spokesmen for science try to pretend, or even to hope, that a bit more 'public understanding' will bring back some bygone times of serene prosperity. Angry young men among scholars are intent on demystifying all pretensions of science to be anything other than just another game, or business. A public, becoming increasingly concerned about threats to their well-being, and even more that of their children, repeatedly sees official experts exposed on television as determined to reassure, at all costs. All this is a long way away from the traditional image of science as being a sort of 'fountain of facts', to which all could come to collect what they needed for the solution of their problems. In my earlier book I reviewed all these problems as they appeared at that time, and then devoted myself to an analysis of the production of scientific knowledge of the traditional sort. Without such a basis, I felt, I could not make an effective systematic analysis of what happens to science when it is deprived of its traditional intellectual structures and political protection. The essays in this section represent my attempts to formulate a new understanding; at present they are all partial insights, for the reality is complex and ever-changing. But together they provide some elements which will necessarily be incorporated into any new synthesis.

The first essay here records my solving of an intellectual problem that had been with me since my undergraduate days. It concerns the philosophy of science, which in spite of its academic character is quite influential as an authoritative source for more popular conceptions of science. When I studied this as an undergraduate, it struck me that what was being described had little relation to 'science' as I was learning or understanding it. Yet the authors were clearly intelligent men, committed to some sort of understanding and truth. But what sort? Years later, through my friendship with the late Imre Lakatos, I came to renew the question, and then discovered the answer. Basically, these philosophers (those of the 'logical positivist' school, and their critics such as Popper) were not so much concerned with what working scientists do, as with Science as a symbol of the True and with it the Good. The peculiarities of their doctrines, and the character of their debates, then began to come clear. In those terms I could understand the great debates in philosophy of science of the 1960s, particularly the roles of Lakatos and Feyerabend; in them the ideological commitment was explicit. I could then develop the argument through successive drafts of an essay. But for a long time the philosophy of Thomas Kuhn withstood my analysis; and this was a severe weakness since his work has been so influential. But when I had the opportunity to lecture at length on this history, at Fudan University (Shanghai) and Wuhan University, in China, the final pieces of the puzzle fitted together.

More recently my focus has moved from scientists and science to the society in which they function; and the issues of commitment, and success and failure are also present on this larger scale. In this case the easiest way into the problem is through quality control. In the years since the publication of my book, this has become recognized as a serious problem, both in science and technology. In the former case, there are the well-known scandals of plagiarism, and worse, of cover-up by sponsoring institutions, mainly in the USA. For the latter, there is the Japanese challenge; through their focus on industrial quality control, they have taken the lead, now approaching a commanding position, in a wide range of industries. Is there something about modern Western societies that inhibits the maintenance of quality control? If so, the cyclical theories of rise and decline of civilizations. first articulated by the Muslim historian Ibn Khaldun, may become relevant; then it was luxurious living among the élite classes that led to a decline in national vigour; now the mild pleasures of consumerism are available to most, and serve as a model to all. Some might wish to interpret this essay as 'conservative', advocating the reversal of a trend which in America is described as going from the work ethic to the shirk ethic. But I do not think the trend is so simple, and less that it can be simply reversed. I am concerned to observe and analyse it, so that whatever is done about it is not the sort of reaction that only makes things worse.

The pleasures of consumerism may be mild in comparison to those of the extravagant luxuries of the rich of yesteryear; but there are now so many consumers that their combined impact on the planet threatens us all. Participating in a conference on the Gaia Hypothesis provided me with the occasion to reflect on how our science-based powers of destruction affect our approach to the traditional questions of the philosophy of science, or indeed of philosophy in general. Issues that have hitherto been explored mainly through the medium of philosophical science fiction are now appropriate for serious analysis. What emerged clearly for me was that not merely man's relationship with nature is now in question (are we some sort of pathogen that might destroy its planetary host?), but of humanity itself (are we something of a failed experiment?). Also, our scientific knowledge, if judged by the Darwinian criteria of success through survival into the future, becomes of a paradoxical status since by its means there may easily be such disruption of our habitat that civilization, and with it science, is injured or destroyed. All this may seem gloomy; but then many great earlier theories of man and the universe have served to modify our conceited view of ourselves; so perhaps Gaia, with these philosophical glosses, will help us towards a necessary humility.

This can be enhanced by a reminder of how our scientific knowledge, under modern conditions, does not protect us against ignorance and even fantasies. The delusion that we are so protected may be one of the more serious defects of our culture. In the brief concluding piece for the section (first produced for a Japanese anthology and then reprinted by Zia Sardar) I review the different ways in which our science can, and does, go wrong.

To outward appearances the academic discipline of 'the philosophy of science' has in recent times been an austere and abstract study. Its concerns have been with one major problem, to the near exclusion of all others. The truth-claims of completed scientific knowledge have been considered to be the only area of really worthwhile philosophical enquiry. The process of discovery, or the ethical problems of research or of applications, have traditionally been relegated to the status of non-problems or at best peripheral ones. Even now, as these other sorts of problems gain in interest among philosophers, the absence of a coherent framework of ideas for constructive study inhibits their development; while epistemology, the theory of scientific knowledge, still dominates teaching because it at least provides materials that can be taught.<sup>1</sup>

Furthermore, the sort of science considered worthy of study is very special. So special, in fact, that it might not even exist. The main tradition in the philosophy of science, including its variants and critics, has been devoted to considerations of matured 'exact' sciences, which combine quantitative experiments and mathematical laws to give the most assured knowledge to which humankind can attain. Other sorts of disciplines are deemed 'immature'; and their main assigned task is to find ways to approach or achieve the proper state. The obvious paradigm case for a genuine science is physics, whose solidity is attested by its triumphs both in theory and in application. It has been noticed that the theoretical end of physics has been in a state of continuous conceptual turmoil and revolution for nearly all of this century, and so its own credentials as a steady, perfected matured science are not beyond criticism. Imre Lakatos recognized this in a revealing footnote in one of his later papers, where he remarked:

This [when a tradition degenerates] seems to be the case in modern particle physics, or according to some philosophers and physicists even in the Copenhagen school of quantum physics.<sup>2</sup>

However, practitioners and defenders of this philosophical tradition can argue

that even if this philosophy-of-science describes no actual science it tells what any genuine science must be like. Its claims to special and unique status as a philosophical enquiry are not therefore dependent on whether its objects of study are precisely reflected in the imperfect world of human experience.

Such a conception of itself is quite legitimate for an academic discipline, particularly a philosophical enquiry. We do not ask geometers to go about measuring the earth, so we should allow philosophers-of-science a corresponding freedom to develop their own autonomous discipline. It is unfortunate that some people so misinterpret the field as to try to glean insight from it about the status and methods of confessedly immature descriptive sciences;<sup>3</sup> but that cannot be the responsibility of the philosophers. The philosophical task of showing how assured human knowledge can in principle be obtained in some sorts of natural science, is one that takes priority over merely practical

If all the foregoing argument for purity seems as reasonable as I have tried concerns. to make it, we are well prepared for an historical paradox. This is, that the founders and most of the main protagonists in the development of twentieth century philosophy of science have been deeply committed to causes directly involving humanity; and their doctrines of the philosophy of science were shaped with those broader ends consciously in view. The reason that 'science' in this tradition seems unlike ordinary practice is not because of its being a purified object of abstract conceptual analysis, but because of its being a symbol of the Good and the True in a certain ideologically engaged tradition of philosophical polemic. If, as I believe, it is time to move on beyond the insights and scholarly problems of that tradition, we should appreciate its sources of commitment so as to make an accurate and sympathetic assessment of its permanent achievements. Also, we will be better able to understand its particular weaknesses and thereby to remedy them in our own studies.

# The Vienna Circle: Proclaiming the True in Science

The focal point of the coherent tradition of philosophy of science was Vienna, of the 1920s and earlier 1930s. There flourished the Vienna Circle, a grouping of philosophers and other scholars that included Karl Popper on its periphery. While Popper's writings, philosophical and autobiographical, are clear on his deep and abiding political commitment, the better-known English-language writings of the members of the Circle do not overtly depict such an influence. Yet the connection was there; the school's founder and greatest philosopher, Mortiz Schlick, was assassinated in 1936. And Schlick was truly a martyr; his was a cause that extended back to the Enlightenment of the eighteenth century: a struggle against 'dogma and metaphysics' (the intellectual tools of reactionary clerical forces) and the invocation of 'science' as the unique way to truth and human improvement.

A manifesto issued by the Vienna Circle itself in 1929 makes all this quite plain.

The increase of metaphysical and theologizing leanings which shows itself today in many associations and sects, in books and journals, in talks and university lectures, seems to be based on the fierce social and economic struggles of the present: one group of combatants, holding fast to traditional social forms, cultivates traditional attitudes of metaphysics and theology whose content has long since been superseded; while the other group, especially in central Europe, faces modern times, rejects these views and takes its stand on the ground of empirical science. This development is connected with that of the modern process of production which is becoming ever more rigorously mechanized and leaves ever less room for metaphysical ideas. It is also connected with the disappointment of broad masses of people with the attitude to those who preach traditional metaphysical and theological doctrines. So it is that in many countries the masses now reject these doctrines much more consciously than ever before, and along with their socialist attitudes tend to lean towards a down-to-earth empiricist view. In previous times, *materialism* was the expression of this view: meanwhile, however, modern empiricism has shed a number of inadequacies and has taken a strong shape in the scientific worldconception.

Thus, the scientific world-conception is close to the life of the present. Certainly it is threatened with hard struggles and hostility. Nevertheless there are many who do not despair but, in view of the present sociological situation, look forward with hope to the course of events to come. Of course not every single adherent of the scientific world-conception will be a fighter. Some, glad of solitude, will lead a withdrawn existence on the icy slopes of logic; some may even disdain mingling with the masses and regret the 'trivialized' form that these matters inevitably take on spreading. However, their achievements too will take a place among the historic developments. We witness the spirit of the scientific world-conception penetrating in growing measure the forms of personal and public life, in education, upbringing, architecture, and the shaping of economic and social life according to rational principles. The scientific world-conception serves life, and life receives it.<sup>4</sup>

In support of this interpretation we have the personal testimony of the Norwegian social philosopher Arne Naess. He recalled:

The Vienna Circle was a nucleus of a movement for 'rationality' and against certain forms of metaphysics which at the time were closely allied with fascism and national socialism. It had all the missionary zeal of a movement, and it was touching but also somewhat alarming to watch Otto Neurath embrace aloof and aristocratic Polish logicians of various philosophical affiliations and proclaim, 'We agreel You are one of us! If Neurath sensed that one was *somehow* on the right side, one was identified as a sort of logical positivist. Protestations were of little use and disagreements were conceived as due only to 'unhappy formulations' (*unglückliche Formulierungen*) and there was always a remedy for that.<sup>5</sup>

There is a stylistic feature of the Vienna Circle's studies which supports the interpretation of their being prophets in analysts' clothing. For their vision of science was quite deliberately abstracted from the processes of a personal creation and historical development; and in this regard they were more extreme in their demarcations than their great predecessor, Ernst Mach. For in his own critical studies, as of mechanics,<sup>6</sup> Mach allowed for the maturing of a discipline through several phases, the earlier, anthropomorphic ones as important and valid in their own way as those which were appropriate to a more perfected state. The Vienna Circle showed no interest in such origins or their vestiges, being concerned solely with the establishment of the credentials of statements in fully matured sciences.

Why this aspect of the Vienna Circle's programme has not been made prominent is a matter beyond my present purposes to explain fully. Let it suffice that with the rise of Nazism in central Europe, the surviving members of the school dispersed to the Anglo-American cultural area. There, the ideological battles were in a totally different style and on different issues. It was only natural for the positive content of the scholarly work to be emphasized and its ideological commitments (themselves severely shaken by the defeat of the anti-Nazi forces) left in discreet obscurity.

There they remained, through the lifetimes of the founders of the school and the careers of their pupils. But in the present period, there is a renewed ideological relevance to the philosophy of science, related not so much to struggles against the traditional Right as to attacks from the new Left. Hence it is relevant and illuminating to see how, beneath the dry formalisms of the logical-positivist writers, there was an intense commitment to a political cause.

# Popper: Rescuing the Good in Science

In the case of Sir Karl Popper, one of the deepest and most influential philosophers of science of our time, the clues to ideological commitment are available in his best-known work. In a classic autobiographical essay, he describes how he came to conceive of the criterion of *falsifiability* in the demarcation of genuine science from its spurious imitations. Even allowing for the inevitable rationalization in the recollection of an event after a lapse of nearly four decades, the story has all the intensity and drama of a genuine conversion experience.<sup>7</sup> Put simply, in 1919 the young Popper was a radical

student who was inspired by four great thinkers who styled themselves as 'scientists': Karl Marx, Sigmund Freud, Alfred Adler (the personality psychologist) and Albert Einstein. After the defeat of the Central Powers in 1918, the way seemed open for the forces of scientific rationalism to achieve their goals in society as well as in nature. But things began to go wrong: failures and complications in the political struggle, doubts and confusions in the intellectual debate.

Popper began to sense that the pretensions to 'scientific' status (meaning, of course, embodying the good and the true) of socialism and psychology were not correct. Yet by the accepted criteria of the time, they were indubitably scientific. An adherent of Marx or of Freud could display numerous confirmations of their theories (very close to the principle of 'verification' that was at the heart of the Vienna Circle positivism). And Adler relied on the inductive evidence of his clinical experience for the development of his theories. Perhaps one of the most fateful moments in the philosophical thought of the century occurred when Popper queried one of Adler's instant diagnoses, and was assured of the psychologist's 'thousand-fold experience' of such cases. Popper reports that he could not help saying 'And with this case, I suppose, your experience has become thousand-and-one-fold.'8 This could be read as a sarcastic little joke; but actually it sends a searchlight beam into the weak centre of straightforward inductive reasoning. (It should be recalled that even when statistics are collected in an apparently inductive fashion in a controlled scientific experiment, the logic of the exercise, which should be reflected in all the techniques, is that of the testing of an hypothesis and not the confirmation of an inductive generalization.)

Popper makes one little remark on the background to these incidents, that calls out for historical investigation. This is, that he and his friends *already* knew that science is not infallibly true, and that scientists can err; hence a genuine demarcation of real science from the spurious would have to be independent of truth. Now, where these young radicals could have learned this lesson, is an intriguing and perhaps quite important question; I recommend it.

Popper's story is given added point by his example of astrology, as being no worse, methodologically, than the sciences which he had come to suspect. Now, this was not an example of an ancient and discredited pseudo-science chosen for its rhetorical effect. With the collapse of traditional authority in central Europe after the defeat of 1918, all sorts of fringe activities flourished wildly. Astrology was prominent among them, and supported self-appointed professors, institutes and learned journals. To such radical intellectuals as Popper, it could well have been the most vicious of the aberrations, because of its pretensions to the status of an empirical science. Hence to show that by the criteria of the Vienna Circle, the superstition of astrology could not be excluded, was to indicate the intellectual bankruptcy of the school.

Another implicit criticism in Popper's account concerns the dogmatism of the would-be sciences of man and society; and this would strike another blow at positivism's claims to be defeating the traditional enemies of reason. He describes how the practitioners of such fields, as the followers of Freud and Marx, use the doctrines in a particularly insidious fashion to protect themselves from criticism. The Marxist critical of the Party is deemed 'petty bourgeois'; the patient sceptical of Freud's interpretations is diagnosed as 'deeply neurotic' and so on. Thus immunized against criticism, and fortified by their 'confirmations', these essentially speculative, non-scientific studies could become really pernicious dogmatic pseudo-sciences. We notice that this defensive device is the same as that of traditional theology, which includes doctrines whereby all dissent is proved to be heresy. (I am grateful to Dr R. Sinsheimer, then at the University of California, Santa Cruz, for this

observation.) Thus, as I reconstruct Popper's problem-situation from his text, he had already given up Truth, and then found that the positivistic criteria admit both superstition and dogma. How to find an example by which real science can be demarcated from the suspect fields of Marxism and psychology, as well as the more patent pseudo-sciences? Einstein's bold theory of general relativity, and, more, his dramatic challenge to the astronomers to test it in the eclipse of 1919, provided that experience. For Einstein had argued mathematically that Sir Isaac Newton had been wrong, on a fundamental point of his system of the world. And now he was calmly inviting the scientists to test his claim, to determine whether he was greater than Newton-or himself only the author of a misconceived theory. That was real science-not fake confirmations, but bold conjectures ruthlessly put to the test. Popper concluded that what made a theory scientific was not that it was verifiable, but that it was falsifiable. But the heart of his insight was that what made a man a real scientist and not a fraud was the moral quality of daring to be shown to be wrong.

This is a very deep insight into the essentials of our science and indeed of our modern European civilization. If there is any doubt as to Popper's political commitment in its genesis and development, that can be removed by acquaintance with his influential works in political philosophy, such as The Open Society and Its Enemies and The Poverty of Historicism. The achievement had its own cost, reflected in Popper's use of the 'falsifiability' principle in the philosophy of science. For Popper was not content to leave it as an essentially ethical principle of genuine scientific behaviour; he needed to adapt it to function as a principle of epistemology and of method.<sup>9</sup> Severe problems were then encountered, for it turned out to be exceedingly difficult to demonstrate how knowledge could increase as a result of applying tests designed to falsify hypotheses: if such a test was successful we gained only the knowledge that some particular statement is false; while if it was unsuccessful we learned only that the statement was not yet proved false. As a principle of method, the projection of bold, very general hypotheses is not even a good caricature of the way scientists work. And, as an historic joke of the sort frequently associated with Einstein, the astronomical observations he suggested would not have been admitted by himself as a refutation of his theory even if they had gone against it.<sup>10</sup>

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The contemporary student derives from Popper's work a sense of urgency and commitment, unlike in the case of the technical writings of the Vienna Circle philosophers. It is not made clear what the urgency is precisely about, since the scheme of 'science' portrayed there is obviously unlike the practice of either the ordinary or the great scientists. But with the help of the autobiographical essay and the political writings, we can appreciate the kinship of Popper to the Vienna Circle, both participating in the tradition of central European rationalism, in which 'science' was not so much a particular social activity as a Cause. However, we should recall the strong difference between them. Whereas the Vienna Circle proclaimed the good news of Science in a thoroughly traditional Enlightenment way, Popper jettisoned the True of science in order to rescue the Good. Post-Popperian philosophy of science may be seen as a test of whether even this desperate measure would suffice for the ideological defence of Science in the later, troubled years of the twentieth century.

In the history of ideas, time does not run at all smoothly. The matured programme of the Vienna Circle was developed after the revolution in 'atomic' physics was well under way, and also after the insolubility of the 'foundations crisis' of mathematics had been proved by the most rigorous of mathematical arguments. Hence its confidence in the security and intelligibility of matured exact natural science was betrayed by events even before it became the basis of a programme. With Popper, time played other tricks: his insights of 1919 waited some fifteen years before appearing fully in print; and by the early 1930s the German-language market for politically liberal philosophy of science was drying up rapidly. So he spent long years in New Zealand preparing his *political* philosophy, on whose basis he came to London. Only in the later 1950s, nearly forty years after the initial enlightening experience, did his philosophy of science begin to affect English-language academic opinion.<sup>11</sup> It is a true mark of its quality that it was still fresh and stimulating; the long reign of the Vienna Circle philosophers and their associates and students was at last being challenged. Popper also had the pleasure of seeing a school develop around himself. But, inevitably, there soon appeared a threatening and in some respects sinister rival philosophy: that of Thomas S. Kuhn. The response to this engaged him, and even more his brilliant protégé Imre Lakatos, through the 1960s.

## Kuhn: Kicking Open Pandora's Box

Kuhn appeared on the philosophical scene in 1962; he was already recognized as a brilliant historian of the mathematical and physical sciences. His book *The Structure of Scientific Revolutions*<sup>12</sup> was an instant success. Although some philosophers of science felt that his ideas were incomplete in their novelty, originality and clarity of expression, there was no denying the popularity of the book or its lasting influence. The enormous influence of Kuhn's work is due not merely to the depth of his insights. More, he seems to be describing science the way it really is, and doing so with a mastery that comes only from matured historical knowledge and reflective personal experience. His scientists are neither the impeccable truth-gatherers of the positivist tradition, nor the heroic conjecturalists of Popper, nor yet the paradoxgenerators of Lakatos. They are, normally, just ordinary people, concerned only to solve research puzzles within an unquestioned framework of concepts and methods. Kuhn's own experience of science was in post-war America, where ideological struggles were very muted and science was well on the way to becoming a big business. His account, reaching its audience when a rapidly expanded world of science and science education had lost most of its earlier sense of adventure and commitment, reads like the plain unvarnished truth. Because of this close relation to a new, disenchanted common sense of science, its ideological significance is more difficult to discern and also more devastating.

Ideological communication

According to Kuhn, scientific progress alternates between 'normal' and 'revolutionary' phases, in which (respectively) scientists make piecemeal advances, or choose between rival grand systems. By this account, it appears that normal science is boring, and revolutionary science incomprehensible. He offers no methods or criteria for helping scientists decide in a revolutionary situation. Hence the genuine 'progress' of science (so vital for its traditional ideological message) becomes impossible to account for, and hence to guarantee, in both 'revolutionary' and 'normal' science alike. Indeed, Kuhn eventually reflected on the way that ultimate purposes are implicit in our idea of scientific 'progress', and wondered whether we couldn't dispense with it in the evolution of human knowledge just as we have done in the evolution of species.<sup>13</sup> With disarming candour, he describes normal scientific work as 'the strenuous and devoted effort to force Nature into the conceptual boxes provided by professional education'.<sup>14</sup>

Having casually dropped the True, he equally lightheartedly dismissed the Good of science. In his general account of the argument of his book he describes the response of established scientists to the crisis that precedes a revolution is such unflattering terms as the following:

Normal science, for example, often suppresses fundamental novelties because they are necessarily subversive of its basic commitments . . . when . . . the profession can no longer evade anomalies that subvert the existing tradition of scientific practice — then begin at last the extraordinary investigations that lead the profession at last to a new set of commitments, a new basis for the practice of science.<sup>15</sup>

Popper did well to entitle his own criticism of Kuhn as 'Normal science and its dangers'.<sup>16</sup>

The most striking evidence as to what was *not* worrying Kuhn comes from an exchange of the mid-1960s, when the mischievous Paul Feyerabend observed that Kuhn's idea of 'normal science' as 'puzzle-solving within paradigms'

provided no means of distinguishing between scientific research and other activities, even including organized crime.<sup>17</sup> The point of this remark was that the association of science with any sort of ethical consideration (either in goals or in methods) was completely obliterated on Kuhn's model. Kuhn's response was simply to remark that he never claimed his model to apply exclusively to science.<sup>18</sup> And there the matter rested.

Kuhn's work is an illuminating example of the way  $\hat{n}$  which a doctrine can have ideological consequences in near independence of the concerns and commitments of the author. It could be and was used for a denial of an objective, universal basis of scientific knowledge, for several purposes. On the one hand, it seemed to offer a behaviouristic criterion for the genuineness of a scientific field: one where debate on fundamentals is suppressed, and all work proceeds as puzzle-solving within a dogmatic framework. For insecure scientists in fields of human behaviour, this offered a justification of arbitrarily imposed Departmental conformity. But rebellious researchers and revolutionary students could utilize the relativity of 'paradigms' to struggle for a substitution of their favoured dogma against the officially sanctioned one. Both sorts of move are destructive of the open dialogue which is the essence of Western liberal democracy, of which 'science' had for generations been taken by its advocates as the great exemplar. Hence for those with any sensitivity to ideology, Kuhn's doctrines were a menace. All his colleagues distinguished Kuhn's doctrine from Kuhn himself; personally he is rather liberal in his politics. rather conservative in his philosophy of science, and not at all interested in ideology. Amidst all that central European intensity, he stands out as the American who just described it all the way it seemed to him, and then was amazed at the fuss made about it. Or is he? It is hard to imagine such an impact being made on the inherited image of science by some sort of accident. Is there more to Kuhn's message than meets the casual reader's eye?

Of course there is, and it shows in the repeated irony, sometimes quite savage, that appears in odd words and phrases in the text. When he remarks that a scientific training is more narrow and rigid than any except orthodox theology, or that the official disciplinary histories of science are like those of 1984<sup>19</sup> we need not assume that the analogies are unintentional. A more coherent set of clues is provided at the very beginning of the book, where he compares existing histories to tourist brochures or language texts, by which we have been 'misled in fundamental ways'.<sup>20</sup> These are, briefly, in the assumption that scientific progress is linear and cumulative; and that incorrect scientific theories are patently absurd. Kuhn seems to have believed such things right through his education, until he helped teach a course for nonscientists which involved reviewing old theories like Aristotelian mechanics, phlogistic chemistry and caloric theory of heat. To his 'complete surprise, that exposure to out-of-date scientific theory and practice radically undermined' his basic conceptions.

In a later autobiographical commentary<sup>21</sup> Kuhn actually described how it happened: 'My moment of enlightenment [sic] began in 1947, when I was

One memorable (and very hot) summer day those perplexities suddenly vanished. I all at once perceived the connected rudiments of an alternate way of reading the texts with which I had been struggling . . .

Thus we have a record of Kuhn's moment of enlightenment, analogous to Popper's of 1919 if not so dramatically retailed. But why should this produce the anger reflected in the irony? It seems most likely to me that at some stage Kuhn realized that he had been the victim of a deception; and we can identify the source of the deception in a root contradiction in the old, received ideology of science. This had two elements: that science is always true, and also always progressive. To explain those cases where progress seemed to have occurred at the price of exposing error, the old-time historian's technique was simple: to show that no real scientist could have believed that stuff. Kuhn discovered, by seeing the reasonableness of the discarded and discredited scientific theories, that the history he had trustingly imbibed was to some significant extent a pack of lies. Hence his anger, and also hence his extreme reaction, leading to the rhetorical flourishes about the 'arbitrariness' of what is believed in science at any time. The True of science had been betrayed in its falsified history; and so the Good is also compromised. All this is a speculative reconstruction on my part; but it at least explains the stylistic features of Kuhn's text and also the intensity that made it so readable and so significant for the ideology of science.

# Lakatos: The Dialectical Defence of Reason and Freedom

It was as ideology that Imre Lakatos read Kuhn's philosophy; and from its first appearance he devoted his main efforts to combating both its philosophical errors and its political implications. This challenge provided a renewed practical focus for Lakatos' work; otherwise he might have been too exclusively concerned with the technical debates between the Popper school and its old and new opponents. Because of his tragically early death Lakatos achieved only a modest bulk of publications; and his various papers are either difficult or controversial or both. But by his intensity, brilliance and wit, he kept alive the spirit of Popperian committed philosophy. Through it all, he was quite clear about his own ideological engagements. Indeed, much of the stimulus and support for this present essay of mine was derived from him.

From his student days onwards, Lakatos had been, successively: a member of the anti-Nazi underground; a Communist Party activist; a bureaucrat in the Hungarian State cultural apparatus; a minor victim of the Stalinist purges of the early 1950s; a candidate for a treason trial, whose name happened not to be reached; a non-rehabilitated (therefore document-less) ex-prisoner in preliberalized Hungary; a rehabilitated person, student and member of the Petöfi circle during the Hungarian 'spring' of 1955 and 1956; a refugee after the Russian intervention of 1956; a research student at Cambridge, England, completing a thesis on the philosophy of mathematics; eventually a member of the Popper group at the London School of Economics (LSE); and finally an embattled opponent of the 'new left' student revolutionaries who concentrated on the LSE in 1968.

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As Lakatos made clear in his published writing<sup>22</sup> the issue was plain: the defence of reason against its enemies, who (as Popper saw before him) could come equally well from the Left as from the old Right. But, working so much later than Popper and endowed with greater political and philosophical subtlety, he could appreciate those defects in Popper's system which required remedying. This apologetic work, undertaken directly as a response to the challenge of Kuhn, occupied the last years of his life and was of doubtful success. His earliest work, undertaken before he came under the direct influence of Popper, is more original and probably more significant. Its ideological commitments are not so open, but are thereby all the more worthwhile to explore.

Proofs and Refutations<sup>23</sup> is an essay in the philosophy of mathematics, in my opinion the first really new move in that field in the twentieth century. Previously philosophers and mathematicians had attempted to resolve the 'foundations crisis' in terms of mathematics being a fixed and rigid intellectual structure, consisting of clear concepts linked by unambiguous rules of inference. The various foundational programmes were devoted to exposing that structure in such a way as to eliminate the paradoxes and anomalies that had been discovered there. Lakatos saw a very different problem: as a preliminary to any genuine philosophy of mathematics, we must explore the dialectic of development both of mathematical concepts and a criteria of rigorous proof. For these are both historically conditioned, and any philosophy that ignores this fact perpetuates the bad tradition of dogmatism in mathematical thinking. His method was as radical an innovation as his doctrine: he expounded his philosophy through a classroom discussion of terrifyingly clever schoolboys, dissecting their hapless teacher's proof of a classic result in topology, the Euler Polyhedron Theorem.

The roots of Lakatos' philosophy of mathematics are clear: the strong Hungarian tradition of problem-solving mathematics, raised to an art and philosophy by G. Polya<sup>24</sup>; and a playful Hegelian style of dialectic, derived from a Marxism purified of its political content. His commitment was not so clear at the time of first publication of *Proofs and Refutations*; but it may be inferred from his life's work. One may imagine that the demonstration of the falsity of rigid and dogmatic thinking in mathematics, the most abstract of all sciences, could be applied *a fortiori* to the 'science of society' under which Marxist socialism was supposed to be constructed.

It could be that Lakatos's philosophy of mathematics was among the more significant intellectual achievements of the Petöfi Circle of the Hungarian Spring of 1955/6. There is even a conjecture that his criticism of 'proof' was born as a survival strategy under conditions of interrogation in Stalinist Hungary.<sup>25</sup> We recall the game played in Koestler's *Darkness at Noon*, where Rubashov had to admit guilt on any crime which he might *logically* have committed. In that game it mattered not that the accusations were, in the nonpolitical factual sense, false. We may imagine that for a more experienced interrogaté, the prime task was to prevent the interrogator convincing him that 'confession' was a personal duty that could be rigorously derived from the objective needs of Party and Revolution. Denying the cogency of even a mathematical proof could then provide an escape hatch from the rigours of Stalinist political logic.

The affinity in spirit and commitment between Popper and Lakatos is plain. They came together not long after Lakatos settled in England, and they then jointly met the challenge of the ideological consequences of Kuhn's apparently non-ideological analysis of science in his Structure of Scientific Revolutions (1962). The great monument of their endeavour is the report<sup>26</sup> on a symposium held in 1965, in which Kuhn and all the other leading philosophers of science participated. Lakatos's own published contribution was under revision for some years afterwards, and so it stands as a fully matured expression of his views. He recognized that the versions of methodology that can be read out of Popper's writings are all too naive to stand scrutiny; there could be no 'instant rationality' in scientific choice. His task was to construct a 'heuristic' that would allow both for the complexity of the cognitive problems (where testing of theories could be neither immediate nor decisive) and for the human qualities of scientists (rightly unwilling to throw away years of work at the sight of the first unresolved problem) while yet preserving the ethical and political commitments of Popper. His philosophical keenness led him into further problems (conveniently overlooked by most of his contemporaries) including the relations between the history and the philosophy of science, and also the location of the ultimate warrant for correctness of philosophical accounts of science (he put it in the successful practice, as distinct from the theorizing, of the élite scientists). The resulting edifice of ideas, further enriched by Lakatos's delight in polemic and paradox, was impressive but unwieldy. It was also very vulnerable to criticism in respect both of its historical reconstructions and its philosophical generalizations. And Lakatos, like Popper, failed to face up to the political consequences of his philosophical critique: if the dominant self-consciousness of science, as enforced by its élites, has indeed been false, reactionary and dogmatic (this is clear from his account of mathematics), what do we conclude about science as a social institution? Can it really be the embodiment of that rationality and intellectual integrity which we know to be at the core of a liberal, democratic, 'open' society? Thus the Good of Science is no easier to defend, once it has become problematic, than the True.

Ideological Commitments in the Philosophy of colorida

#### Ideological Commitments in the Philosophy of Science

Lakatos did not engage in his philosophical exercises for their own sake. While he was elaborating on his synthesis of Popperian idealism and Kuhnian pragmatics, he was also engaged in a political struggle with antagonists he considered as vicious and as dangerous as the Stalinist thought-police of Hungary. The rebellious students of the London School of Economics in 1968 were, in retrospect, a small and ineffective minority. But during their flourishing, they disrupted a distinguished educational institution, and announced their intention to capture it and much else beside. Even the native English academic staff at the LSE were caught up in violent struggles, ideological, institutional and personal. For Lakatos, it was the Red Fascists on the march again, and he reacted as if back in Budapest. This struggle convinced him that his version of Popperian liberal philosophy of science was central to the defence of civilization, and so gave his work a compelling intensity. But it took a heavy toll of his energies, and left him exhausted and ill.

It is conceivable to me that Lakatos eventually recognized that the great flexibility he had built into his model of rational scientific behaviour, for the sake of realism, had effectively undermined his political commitment and career. The crucial point is of time-scale; as he said, 'to give a stern "refutable interpretation" to a fledgling version of a programme is a dangerous methodological cruelty... [it] may take decades of theoretical work to arrive at the first novel facts and still more time to arrive at *interestingly testable* versions ...<sup>27</sup>. Decades of protection from critical judgement, even for an abstract scientific theory? How long then, for a new social system? By this criterion, the Soviet intervention of 1956 was quite possibly 'historically necessary' to protect the fledgling socialism of Hungary, scarcely a single decade away from war and Fascism. Thus Lakatos's lifelong exile was perhaps the result of a methodological error in the overly stern assessment of a fledgling version of a social development programme.

Only an intimate biography could tell whether Lakatos was aware of this latent contradiction. But since his methodological reflections were always guided by his political commitments, the practical implications of his strong denial of 'instant rationality' could not be hidden forever. What we do know is that one of the few comradely friends he retained from his earlier days in England exposed other crucial contradictions in his intellectual system, and effectively made himself rather than Lakatos the authority to be followed. This was Paul Feyerabend, in whose book *Against Method*, dedicated to Lakatos, the ideological aspects of the modern philosophy of science are taken to the ultimate in paradox and confusion.

### Paul Feyerabend and the End of Classical Viennese Philosophy of Science

Feyerabend is certainly the most confusing and paradoxical figure in the philosophy of science of that period. It is not at all easy to decide whether he is a court jester, Zen master, or Fascist. The first, because he still operates within the community of philosophy of science, engaging successfully in highly technical debates on problems within the dominant style. In this respect, he is more one of the club than even Lakatos ever was, to say nothing to Kuhn, whose real commitment is to interpretative history rather than exemplified philosophy. Conventional philosophers of science cannot dismiss him, for he is capable of publishing a fully expert and illuminating—or wounding—study of problems or persons at any time. Yet in what seems to be another incarnation, he has written wild and destructive criticisms of the whole programme of philosophy of science, that is explaining and justifying the methods whereby (idealized) scientists gain new knowledge. Some might hope to contain his influence by not taking the critical diatribes too seriously, and treating him as a court jester, who says impossible things as useful reminders of the human frailties to which even philosophers are subject.

Careful consideration of his arguments shows that they are not so easily reduced to jokes. If philosophy of science has any pretensions to help us understand the activity of science, then his studies of the behaviour of great scientists are troubling indeed. For he shows by example that for any explicit rule of method enunciated by philosophers of science, there is an important occasion on which it was broken by some great scientist. In his Against Method<sup>28</sup> he goes far towards demonstrating that Galileo was a precursor of Feyerabend, treating all the rules, including that of simple accuracy (or honesty) in recording observations, with fine anarchistic playfulness. The epoch-making description of the surface of the moon that Galileo saw through his telescope, reported in the Starry Messenger, gives prominent and important reference to a feature (a large round crater on the line bisecting the lunar disc)29 which can be made at all plausible only by the most skilful selection of modern lunar photographs<sup>30</sup>. And Galileo's struggle for the Copernican system can be considered 'scientific' only because he happened to be right; otherwise he broke every rule of the game.

be right; otherwise he broke every rule or any general second provided of a student for Now, this is the sort of thing that can easily 'blow the mind' of a student for whom (like so many) the authority of science is as absolute as theology ever was in the Middle Ages. After such an experience of shock and disillusion, the student may be ready to awaken to the truth that there is no truth to awaken to. In his role of awakener, Feyerabend may be considered as a Zen master. But the analogy is very imperfect: a traditional Zen master operated in an I-thou relation, so that the searcher would be genuinely enlightened and not destroyed. Providing an anonymous reading public with an exhibition of a batch of sacred images being sprayed by a philosophical machine-gun is a very different activity indeed.

For this reason, and another as well, Feyerabend may come under suspicion of being in effect (though certainly not in intention) a Fascist.<sup>31</sup> For what he offers to replace the old ideal of philosophy of science is confused and unconstructive. It is along the lines of allowing everyone to 'do his own thing' freed from the constraints of convention or of social or logical propriety. Those who

recall the connections of Nazi German-Folk ideology and religion with earlier currents of Romantic and anti-mechanical philosophies are justifiably troubled by Feyerabend even more than by the other counter-culture prophets. Feyerabend's prescriptions may be all very well after the anarchist Utopia has been achieved; but in the short run it may mean destroying the intellectual barriers to the victory of arbitrary will and brute force in intellectual and hence social matters.

Feyerabend could reply to such an accusation with the rejoinder that for most of the world's peoples, aside from the mainly white, mainly male, mainly middle-class beneficiaries of high culture, that is precisely the unspeakable state of affairs already; and that this is both concealed and sanctioned by our dominant ideas of Science and Method. In his defence, in Science in a Free Society he describes the experiences in California which led to his conversion. Adopting a playful Feyerabendian style for the rational reconstruction of Feyerabend, we may take two episodes from the book, and combine them to imagine an 'epiphany of the yellow pencils' for his illumination.<sup>32</sup> These latter were the topic (along with black ravens) of the paradox which formed a principal concern of philosophers of science during the otherwise turbulent decade of the 1960s. So we may imagine Feyerabend at Berkeley during all the campaigns, teaching classical philosophy of science, including the paradox named above, when he became aware of his surroundings. These were first, a class of Californian 1960s types, including ethnic minority people whom he was expected to prepare for 'the wonderful chance to participate in the white man's manias'. Exemplifying these was (I imagine) the tear-gas that drifted into the classroom as the police broke up yet another student demonstration. Furthermore, having been abandoned by the best of the orthodox medical science that the University of California could provide, he was in the process of being saved by several unorthodox practitioners. This quintessentially 1960s combination completed the process of his disillusion with the official representatives of rationality and freedom, that had been growing for many years; and so he rejected the yellow pencils in favour of a radical democracy in all culture, including science. Thus enlightened, he turned on all scientific orthodoxies with the fierce delight displayed in Against Method.

Feyerabend is best understood in the context of the counter-culture which flourished most abundantly in California. His criticisms make sense when related to those of Ivan Illich and the other prophets of a new age. His political case against scientific medicine is supported by the chronicle of oppression and mutilation of subject peoples (including the whole female sex) at the hands of the certified experts.<sup>33</sup> Indeed, the only conclusive answer to his critique is the classic of a departing reactionary: 'Après moi, le deluge'—so that all but the most fanatical revolutionaries realize in retrospect the benefits of a rule of law that had at least been consistent, however harsh and unjust. There is a practical answer, of course, and that is to let time elapse and see what has happened to the message of the 1960s, and to the world which then for a moment seemed nearly in an apocalyptic state. In Feyerabend's polemics, the ideological motivation for the philosophy of science finally became fully explicit. This was because he was accusing the dominant Viennese tradition of complicity in the betrayal of the ideals for which it had historically claimed to stand. Their idea of rationality had showed itself a tool of class and cultural imperialists; he would then demonstrate the incapacity of such a rationality as an instrument of learning about the world. Though in the last resort Feyerabend argued from within the philosophical tradition to which the Viennese adhered, his work was devastating to that tradition. After him came a variety of social science approaches to scientific knowledge, all of which argued that scientific knowledge is a social construct, of a lesser or greater degree of arbitrariness. Although there remained a few apologists trying to rescue something of 'objectivity', with Feyerabend came the end of classical Viennese philosophy of

If that tradition had been truly 'positive' like the science it proclaimed, and science. had tough and resilient roots in a real understanding of its practice, it would not, I believe, have been so vulnerable to the assault of its critics. But, as I have shown here, the image of 'science' that was invoked in that programme was itself the product of an ideology, however unself-consciously applied: that science is uniquely the bearer of the True and hence also of the Good, in opposition to religion and other forms of knowing. When that image lost its plausibility, through changes in the ideological and institutional context of science, the technical articulations made by previous philosophers of science were discovered to be hollow and brittle. Two profound but simplistic thinkers, one, Popper, an eccentric Viennese ex-radical and the other, Kuhn, an unsubtle American conservative, achieved the insights that demolished the foundations of the old scientistic faith; and then in spite of Lakatos's heroic efforts to construct a dialectical defence of reason and freedom, the whole edifice was brought down by Feyerabend's 'Dada' critique.

# Conclusion: Where Do We Go From Here?

I do not wish to say that any philosophical system is only a tissue of rationalizations of an ideology, that enjoys some temporary plausibility. Although philosophy is very different in degree from the more 'positive' sciences that enjoy a more direct foundation in controlled experience, it too leaves behind a residue of achievement, in understanding rather more than in detailed knowledge, as each great movement or school passes through its cycle of growth and decay. But when all the signs point to a philosophical cycle nearing its end, it is time to see whether the world which was its passionate concern is still that which presents us with the problems that challenge and enrich us.

The ideology of the previous phase of philosophy of science was derived from a centuries-long battle with 'religion'. This lay not so much in the realm of individual faith, as in that of pretensions to exclusive knowledge, and of claims

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to political power partly on that basis. Now, in the later twentieth century, that old battle is over: the Christian Churches are in an excited and turbulent state that may indeed herald a great rebirth, but which certainly does not promise either the renewed obedience of the faithful or the deference of the secular powers. Instead, some at least of the clerical evils that motivated the endeavours of Enlightenment have now been inherited by the apparatus of anti-religious state power. And from science itself have come new evils. inconceivable once magic was discredited until the advent of the atomic bomb. So that those who still try to identify science with the humane, civilized values now find themselves in a confused night battle, where friend and foe are ever more difficult to distinguish.

My own retrospective assessment of the tradition would hinge on a distinction of three modes of discourse: heuristic, epistemology and ideology The logical positivists ignored heuristic and so were vulnerable when it was introduced. Popper invoked it but in a very caricatured version. Kuhn might be said never to have grasped the distinction between an insightful heuristic and a rigorous epistemology. Feyerabend uses heuristic to destroy epistemology. Of all these philosophers, Lakatos best appreciated the difference, but was lacking in a sufficiently sensitive touch to keep their relations harmonious.

The old epistemological problems of science are, therefore, no longer fruitful for our understanding of that great creation of the human intellect. As they have become isolated from their roots in committed experience, they can provide no effective defence against the suicidal application of reason in Feyerabend's arguments. I suggest that they be given a rest, and that new critical insights be applied to the analysis of science, not in a spirit of angry demystification, but as a complement to progress already being made in the history and the sociology of science. There, studies of the actual conditions and constraints on scientific work are providing a picture that is rapidly being enriched, of how science can have both successes and failures, and virtues and vices, without being the subject of one simplistic verdict on the degree of its adherence to the Good and the True.

The speculative and analytical styles of enquiry appropriate to philosophy could find an immediate rich harvest in the many areas of ethics that impinge on scientific and science-based work. For epistemology, there are the peculiarly challenging and urgent problems of 'trans-science', where the questions may look like ordinary experimental topics, but where the technical answers lie beyond the limits of feasibility. The philosophy of the mathematical sciences could be rejuvenated by deeper analysis of their inexactness in practice, as distinct from their perfection in an ideologically loaded theory. The criteria of demarcation of science from pseudo-science, essentially untouched from Descartes until Popper, could do with more scrutiny. For example, there are the policy-relevant disciplines dependent on mathematical models where the uncertainties in the inputs must be suppressed lest the outputs become indeterminate. Such GIGO-sciences (for Garbage In, Garbage Out) have a role in statecraft analogous to that of classical astrology.

Must we admit them as scientific in spite of their vacuity, merely because their underlying metaphysic follows Descartes and Hobbes? Even the field of ontology, long since relegated to the most obscure corner of metaphysics, takes on a new relevance. When both visible prodigious phenomena and inward states of enhanced consciousness are, continuously since the 1960s, on the agenda of debate, the concepts of reality decreed in the early seventeenth century may no longer be taken as unproblematical. These are only a few problems shaped in terms with which I am familiar; as philosophers enlarge their image of science from an idealized physics, to medicine, technology, and the fields of 'regulatory science', the problems are profuse in their challenge. I would not be so naive as to call for an end to ideology in the philosophy of

science. The new problems will have their own ideological motivations too; that is necessary and healthy. But we can look forward to the closing of a chapter in the philosophy of science in which the persistent rule of a particular ideology was, in its later stages, all the more damaging because it was unrecognized. That is why I have done this survey in the interests of an enriched understanding of our past, so that we may better shape our future.

Adapted from an essay that was first given as a lecture to the Department of Sociology at Leeds University in 1977, then, as revised, published (in German translation) in Versuchungen (essays on the work of Paul Feyerabend) (ed. H.-P. Duerr), Frankfurt, Suhrkamp Verlag, 1980, and further revised and republished, in Radical Philosophy 25, 1984.

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# Quality in Consumerist Civilization: Ibn Khaldun Revisited

Some cultures make good plumbing; some others, good space-rockets.

Who would lay down his life for General Motors; or indeed for General

Westmoreland?

In the nineteenth century it was the capitalists' commercial goods, as Marx observed, that battered down the Chinese Walls of the traditional closed societies. Now we may speak of a more sophisticated stage of the process, whereby not merely cheap necessities but cheap luxuries are increasingly available to all the world, and much desired by it. Old patterns of authority have long been under erosion; now old life-styles and values follow, all swept aside by consumerism.

Those who are concerned for values that are in any way 'alternative' to consumerism, be they ecological, socialist or religious, may watch with horror as young adults the world over increasingly find all life's meaning conveyed on the colour TV screen. Movements for realizing something of more genuine value seem regularly to degenerate either into fashion or into fanaticism, or at best to remain isolated on a political or cultural fringe. The triumph of materialism, after centuries of struggle on the philosophical and then political planes, now seems to be accomplished through the commodities of modern affluent living or its reasonable facsimiles.

But in this century of its success, the heartland of material progress, the Occident, does not seem to be enjoying the fruits of its victories. A variety of malaises afflict it; its optimism and confidence have given way to confusion and drift. Some reasons for this condition are quite obvious; thus the real victor in the struggle for the affection of the global consumer is Japan, one of the recent barbarians, now even more menacing to America in peace than in war. But there is more; on top of economic stagnation, ecological crises and terrifying financial instability, there is a sense of loss of power at the real centre. Military might and high-technology leadership (closely related through electronics, space and nuclear energy) are now both compromised,

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partly by external competition and also by internal decay. At this point in history, if you want to put a payload into space, you do best to go to China, or perhaps to Russia. American rockets are, it seems, no better than American cars. And Star Wars approaches the ultimate in ruinous absurdity.

Does all this mean that the 'unbound Prometheus' of hitherto dynamic Occidental technology is now revealed as a giant with feet of clay, at least in the culture of its origin? Has the triumph of the soft values of consumerism been achieved by the loss of the hard values of national strength? If so, then it is most important for the phenomenon to be understood. This might not make any difference in the short run; but an historical perspective might be crucial for understanding and effective action, should the present unstable balance of influence and power between different national cultures eventually become disrupted or deranged.

# Old Cycles of Empire, and their Social Cement

We all know the cycle articulated by Ibn Khaldun, starting with barbarian (perhaps 'hybrid'?) vigour, through stable prosperity, and finally corruption and decay from luxury. The story is at least as old as Saul, David and Solomon. Some empires repeated the cycle almost like clockwork, notably the Chinese. Others went through the process only a few times locally, and then succumbed to a global onslaught; such was the story of the Islamic communities.

About a half-millennium ago, a fateful shift occurred. The new barbarians did not come on horseback, ready to be tamed by the good things of palace life. Rather, they came in strange boats, bringing successively guns, Bibles, various poisons and diseases, and eventually things to sell and money to invest. World-wide they encountered no traditional culture in a vigorous phase, capable of resisting or of adapting creatively. It took a few centuries from the first easy conquests; but eventually all were penetrated and possessed, and became attached to the Occidental civilized world.

So the rise of the Occident was correlated with the enfeeblement of all the others. Was there a simple confluence of Ibn Khaldun cycles all over; or was there some larger, long-term shift affecting them all? On this I can, of course, only speculate. But we recall that what finally shattered the Inca civilization was not the Spanish invaders themselves, but the Great Inca's confession to his people that El Dorado, the Sun King, was nothing but a staged spectacle. The magic had already died; the land was a spiritual corpse waiting for the vultures. Similarly, the Aztecs, waiting for fair-skinned gods on winged vessels. China survived longer, with another cycle to go through; Japan shut the door and decayed quietly behind it until 1854. What of Islam? There the barbarians, with all their religious fervour, had already effectively destroyed that stable community that is the essence of Islam as a social possession of mankind.

The personal, visual experience of the monuments of those ancient civilizations is a great education in the relativity of cultures and also of technology. Vast social endeavours, and artistic craftsmanship of the highest pitch of excellence, were poured into edifices and furnishings designed for the glorification or perhaps even deification of a single individual. Such enterprises, often accomplished through the improverishment or enslavement of the masses, nearly defy explanation to the Occidental utilitarian mind. Was it only will, or caprice, or a desire to frighten or impress, that motivated the decisions for their construction? If so, that would represent a social practice, stable over many centuries, in which, regularly, very much was taken from society and nothing was given in return. Then the social structures apparently survived and were perpetuated and reproduced in spite of having no functions, only dysfunctions. To be sure, the oppression and exploitation could approach the absolute point; but then there could be uprisings, and at least in China, these would be a signal that the Heavenly mandate had been forfeited, and it was time for a change. The Ibn Khaldun cycle had completed one round.

Let us consider the possibility that that sort of production, with its characteristic excellence of technology, did have a social function, through which it provided a general benefit. This was obviously not on the material plane, unless all this extravagance was perennially accepted as necessary for the military and civil benefits of strong, stable government. It would be misleading to describe the monuments and their furnishings as 'symbolic', as if a dictionary of individual meanings would be necessary for their appreciation. Rather, perhaps that industry of rulership was organized around magic, in the sense of religious beliefs and experiences, adapted to the exercise of worldly power.

Experiences? Here the secular humanist (regardless of his or her profession of faith) starts nervously. Am I now invoking mysticism, superstition or perhaps psychedelia as the social cement for that highly developed technological form that dominated great civilizations for millennia? Let us not quibble about words; we can think of the *affect* that is now produced weakly and intermittently by the modern paraphernalia of patriotism: flags, songs, monuments, even some monarchs. As an intermediate case, we may recall the quasi-religious character of great or absolute rulers in recent times: the Czar, Stalin or Mao; or even (to some extent) F.D. Roosevelt and (for a time) Woodrow Wilson. In another age, with another consciousness, the systematically enriched subjective experience of participants, achieved through various technologies, some imaginable to us and some not, was the 'utility' of that charisma-industry. If we accept this thesis, then those other civilizations become at least comprehensible; otherwise they can only fill us with cosmic dismay, by their stupendous waste and abuse of human labour and talent.

We can speculate further: suppose that, however deformed or corrupted it may have become or been, this sort of charisma-industry did more than merely secure the passive obedience of the exploited masses. Perhaps, when it worked well, it combined the sacred and the secular, the sensory and the trans-sensory, so that (at least for those who were not totally excluded from its benefits) it provided an occasion and a motive for people to do and give of their very best,

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their labour and even their lives. In this way we can explain the otherwise incredible level of artistry routinely achieved in these productions. In our culture, it is hard to imagine such work being forced out of suffering slaves. Perhaps the religious/political matrix of belief, however much it violates *our* principles of equity and human realization, functioned to just that end.

This would have been the stable background to all the local cycles of dynasties. The successful barbarian conquerors (who came in anyway when the previous rulers had lost their charisma) could easily be recruited to the apparatus of social control accomplished through shared experience, and (as classically in China), the perturbations of life would soon diminish.

# The Last Half-Millennium: The New Game

As I have indicated, whatever degree of effectiveness and stability the charisma-industry may have achieved at different times and places, it has universally been in decline for some centuries. The empire of the Occident has broken the old matrix; it denies and then ignores the shared subjective realities on which the old system depended. Marx well expresses the new consciousness when he assumes the non-existence of such realities, and then tries to explain the whole history of production and of technology on straight Benthamite lines. In the political sphere, we learn from the American Declaration of Independence that people have the inalienable rights of 'life, liberty, and the pursuit of happiness', and that governments are no more than practical instruments for the securing of those rights.

We should recall that this democratic manifesto was articulated rather late in the full cycle of expansion of the Occident. Previously there had been an important period of 'absolutism', as in Spain and France. But a glance at history shows that in comparison to the real thing, European absolutism was temporary and feeble. The Sun King, Louis XIV, named after the Incas by Campanella, enjoyed scarcely a pretence of truly divine sanction, and still less of authentic participatory experience with his nation.

At first this destruction of the old realities, already enfeebled, was a great liberation in a multitude of ways. Material production could now be used for widespread personal enrichment through the production and sale of materially useful goods for an open market. Innovation quickened, so that by the nineteenth century the productivity of the textile mills would have seemed quite magical in any other culture. With higher productivity, and with the fetters of economic and political constraints completely shattered, the broad masses could, over a few generations, come to participate in increased wealth, in decent conditions of work and living, so eventually in citizenship as well.

So, finally, with most paid labour mild in exertion and duration, with more people possessing leisure to enjoy and also some means with which to make it enjoyable, we have arrived at the matured consumerist culture. First America in the 1950s, then other countries, in their own times and fashions, achieved this universal easy life-style, best symbolized perhaps by colour TV and Coke. (I had previously thought that the suburban house and car were essential; but given present trends rather more than a fifth of the world's people will soon be entering consumerism without them.) Who would dare to denigrate the mild pleasures, the absence of pressures to dehumanize one's fellows, and the many opportunities for real benevolence and solidarity which such a culture enables and occasionally fosters? Let anyone imagine and design a feasible alternative that genuinely promises the masses, now including women as well as men, a better existence than consumerism, however thin and precarious it still is for most of the world. Yet, as I observed at the beginning of this essay, there is now a darker side to

Yet, as I observed at the beginning of this essay, there is not a dather interest the picture. While its values suffuse through the whole world, the heartland of consumerism suffers this peculiar loss of strength. Perhaps, while consumerism is the highest point of social evolution now attainable, it is *also* merely the means to a shallow and artificial substitute experience of reality and meaning. Certainly, the revolt of the first-generation affluent youth of the 1960s was based partly on just that issue. Also, being based on the presumption that happiness can and should be pursued successfully, consumerism has no vocabulary for comprehending the complementary aspect of human existence, including struggle, evil, pain and death. In this respect, however pleasant and liberating it may be, it may also be incapable of performing the vital social functions of belief based on a shared deep experience. Thus consumerism might mark (and contribute to) the onset of a downward phase of a cycle of empire, reminiscent of Ibn Khaldun but of course of a different sort.

# Quality of Workmanship and the Morale of a Culture

Any analogy between the classic cycle of Ibn Khaldun and modern consumerist society must appear far-fetched. There are no longer any despots, and democracy in the social, economic and political spheres still spreads, in practice as well as in principle. Those masses whose lives are still full of want and sorrow may be said to be suffering nearly as much from neglect as from exploitation. And whichever external nations or leaders might claim the status of purifiers of the cultures, they too must establish their plausibility in the forum of international television; and so far none has passed the test. So if we are to establish a fruitful analogy with Ibn Khaldun, we must investigate more deeply than the superficial phenomena of politics.

Let us also leave aside, for the moment, the more obvious worries of the civilization, however much they may seem to contribute to its malaise. I choose to consider quality control, particularly in relation to American technology, in the nuclear, space and military sectors. Here the story is quite amazing, rendered credible only by long familiarity. Perhaps it is best expressed by the condition of American space technology. The Shuttle programme is now (only after the Challenger disaster) revealed to have been incompetent and corrupt;

where each sub-system was deemed safe until proved otherwise; and where finally management simply didn't want to know of any problems. Then, shortly after the Challenger disaster, misfortunes overtook each of the other models of American rockets; so that at the time of writing (late 1986) the United States is incapable of launching a space rocket with any degree of assurance that it will survive.

Here we may speak of a catastrophic collapse of quality control. The implications for the USA as a leading world power, though not discussed much in the English language media, are inescapable. And of course such a case cannot be isolated. American manufacturing industry in steadily losing out to Japan; and American military hardware includes many multibillion dollar boondoggles, with more coming all the time. Star Wars, ostensibly an experiment to test the feasibility of a system already proved impossible, now has its own financial and political momentum; it could well have been a plot hatched by the Russians or (more likely) the Japanese. Business and finance fare no better than manufacturing; we now learn that the Western banking community spent the 1970s in converting other people's money (the petrodollars) into bad debts in the poor countries. Was this neo-imperialism or mega-folly? And if America is in the lead in this doleful respect, its partners (notably Britain) cannot be far behind.

The decline, or collapse, of quality control in these crucial sectors has a significance beyond that of its implications for the strength of the afflicted nation. Quality control is particularly useful in exhibiting the inadequacy of the individualistic—utilitarian ethic for keeping a society together. In any operation, care for quality involves a sacrifice, perhaps small; an expenditure of time and effort which usually will have no perceptible effect. If things later go wrong due to inadequate quality now, it will only be sometimes, and anyway it will usually be far from the context of the operation. So it may be argued that the Benthamite 'total net happiness' function may actually be decreased by care for quality of workmanship; to make the point plausible, one may imagine cases where quality standards are imposed that are inappropriately severe for the product in question. Certainly, my net happiness, in utilitarian terms, is usually quite definitely the better if I cut to the operation.

What I have just sketched, with manufacture in mind, holds equally in any sphere of activity: design, administration, research, whatever. And if people doing the work systematically don't care, then no superior can enforce highquality work on them. In any event, the old Latin motto, 'who guards the guardians?' reminds us that normally slackness goes up and down the line uniformly.

Now, what is the relation between low-quality work and a consumerist culture? Quite simply, consumerism is by its nature hedonistic, Benthamite, however benevolently so in its easygoing way. The legendary American who asked, 'What's posterity done for me?' may have been at variance with the prevailing fashion of sympathy for the natural environment; but his position is essentially that of his culture. And modern orthodox economics reduces to the principle, 'For real love, pay cash.'

At this point my argument is becoming counter-intuitive. For everywhere we see around us evidence of high quality—in innovation, design and manufacture, as well as in marketing and advertising. Indeed, the great seductive power of the market economy is that by its 'hidden hand' it shapes its products to fit what consumers want. By contrast, bureaucratic control invariably yields consumer goods of low quality in every respect. But we as consumers only see directly what has been produced for us, on a reasonably competitive world market. Everyone knows that the provision of essential services, such as health and education, cannot be entrusted to such a market. And occasionally, through reports in the media or a live demonstration on TV, we witness those fiascos and disasters involving devices produced under conditions where market quality control is impossible. Such were Three Mile Island and Challenger. While the consumer markets relate to the popular values of a culture, those others (usually involving the State) reflect its real strength as a nation.

Furthermore, a refinement even within the market sector is possible. The Japanese manufacturing miracle, or conquest, can be understood precisely in terms of that particular culture *not* being totally individualistic and consumerist. Very traditional patterns of loyalties and commitments enable them to focus their energies, collectively and individually, and so to excel nearly wherever they choose. By contrast, when there is no such binding force in the productive life of society aside from consumerism, then no one makes a contribution beyond that which pays off directly, and quality inevitably suffers.

Another way of seeing the phenomenon is in the paradox, or joke, about plumbing and rockets. For a long time Western experts and observers were sure that the Soviet Union could never master the sophisticated technology for rocketry or nuclear weapons. The evidence of low-quality consumer goods, including even the plumbing in their best international hotels, seemed conclusive. What such observers missed is the cultural aspect of technology that I have expounded here. It is a commonplace to observe the affinities (political and cultural) between such centralized, totalitarian regimes and the despotisms of old. The core technologies of defence, together with the occasional grand display (such as the legendary Moscow Metro) have had all the available excellence quite deliberately concentrated into them. Housing, plumbing, clothing for the masses could wait, for in such cultures the masses will wait patiently, at least for some decades, provided that they get more security and national pride than hitherto.

Such successors to the charisma-industries of the old cannot survive indefinitely, especially once the Great Leader has departed. In these times, the example of the successful consumerist societies penetrates every curtain. And then, regimes of whatever formal character must come to terms with consumerism, or try with increasing urgency to fabricate a viable alternative in terms of their traditional culture or official ideology. The situation may well

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become desperate for some governments, for no viable alternative to consumerism has yet been found; and (as some are already aware) the shallowness of the consumerist ethic will eventually become manifest. In the absence of anything to supplement it, a nation, or culture, may well decline or die.

## Consumerism and the Survival of Nations

A long time ago the American philosopher William James spoke of the need for a 'moral equivalent of war', and for decades afterwards, liberals condemned him as a militarist. But they missed the point; it is not that war is necessarily good in itself; but that it brings persons and (sometimes) whole sections of a population to a place where the utilitarian ethic cannot apply. Comradeship and sacrifice, whatever their ethical foundation (in religion, patriotism or simple human solidarity) are difficult to justify on utilitarian, hedonistic grounds; witness the contortions of evolutionary biologists in explaining the selective advantage of 'altruism'.

The matter is not merely of theoretical interest; for in this age of democracy or consumerism, it becomes increasingly difficult for governments to persuade peoples to make the sacrifices involved in war or in some other heroic activity. Of course, this is a development that all good liberals applaud; never again could we have a phenomenon like the First World War, men marching off obediently to slaughter and be slaughtered.

But, for better or worse, it does create practical difficulties for statesmen, who occasionally need to display a credible threat of force to establish their policies in a hostile world. This requires that the young men of the nation are generally willing to lay down their lives, in the cause of religion, obedience, or patriotism. Americans, in spite of their rhetoric, are traditionally not eager for such challenges. To gain popular support there, a war must be embraced as a patriotic crusade. In this, Wilson and Roosevelt succeeded, Truman (over Korea) didn't do badly, but poor Lyndon Johnson failed. Then he had to promote and conduct a large military operation as if it were not a patriotic war, but rather a campaign to win 'hearts and minds'. For this the rhetoric of consumerism was the only resource, and so the operation was described, and perhaps eventually conceived, as if it were General Motors marketing cars. The surrogate for sales statistics became body-counts of corpses deemed to be Viet Cong. Computers ruled all, even the selection of targets for remotecontrol bombing. The results of this vast, bloody pretence were a catastrophe for the USA; it lost geopolitical strength, international credibility, its effectiveness as a military power (outside the fantasy context of nuclear war), and also the willingness of its people to support such adventures again.

It is very easy, and quite justified, to criticize the American governments for the Vietnam War. But they were caught in the historic contradiction of trying to run a great world empire, with all its inevitable material, personal and

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moral costs, without being able to admit as much to their own individualistic and consumerist population. Usually the dirty work could be done by small specialist forces; but when that failed, the choice naturally seemed between ignominious withdrawal or a larger-scale exercise. When that could not be promoted as a patriotic crusade, the leadership was caught in a cruel contradiction: it had to ask, or try to force, men to go to their deaths in the absence of any symbols or experiences that could produce the commitment which could make such sacrifices meaningful. All this is well known from the analyses of the traumas suffered by the Vietnam veterans, during and after the war. In some ways the moral horrors of Vietnam were worse than those of the trenches of the Great War; in the earlier struggle, it was after all men against men; while here they had to kill, or be killed by, anything that moved. By the end, morale had been corrupted beyond repair; the attempt to run a war without effective meaningful symbols showed itself a disaster.

Wat without that commitment of soldiers to their group, a war cannot be won. The future of the American empire is thus seriously called into question; if a colonial conflict is not popularly seen as vital to the nation, but only a matter affecting corporations or governments, there will not be a successful war, however much Presidents may scheme, sabotage or engage mercenaries. Nicaragua is a case in point. Good liberals may also applaud such developments, since we all agree that empires based on force are a bad thing. But, we may suppose, suppose it is not armed conflict but some political/economic equivalent of war; such as the sort that in some time of future world crisis to which the Americans may be subjected by the Japanese. Should a people not be able to see beyond their consumerist desires, should 'patriotism' then truly become what Dr Johnson called it two centuries ago, then the prospects for America's survival as a great nation would be dim.

This analysis of America may be justly criticized on the grounds that American individualism pre-dates the consumer society; and so the contradictions which are now becoming manifest may be characteristic of democracy as such, rather than its modern form in consumerism. This may well be; but my analysis is strengthened by consideration of a very different society indeed: modern China. For the first three decades after Liberation, there was certainly no question of consumerism. But Mao wanted to build a great nation from the shattered, impoverished hulk he inherited from centuries of decay. And for him, it had to be a *socialist* nation; otherwise it could too easily slip into colonial dependence on either America or Russia. But how to instil socialist consciousness, getting people to throw off the fendal habits and attitudes of centuries? Mere exhortation, propaganda, laws and Party control did not suffice. So there must be Campaigns, moral equivalents of revolution if you wish; most notoriously, the Great Leap Forward and then the Cultural Revolution. The earlier one was merely a disaster, the later one a catastrophe.

Whether some better design of these campaigns might have been successful is not the issue here. What we know is that the invocation of the symbols of

socialism and of the common good failed disastrously. For all his strong similarities to the great unifying emperors of China, Mao did not command the personal charisma necessary for the motivation and control of such a revolution in consciousness.

Now (late 1986) in China there is a near-vacuum in official ideological rhetoric involving socialism or the common good. The message is for each to enrich himself now, so that China will become modern, strong and great. The government will ensure that when that success makes it possible, those now left behind will be enabled to catch up. But such altruism is most definitely relegated to the future; it lives only in this vague promise, as well as in the social welfare programmes whereby the most disadvantaged are protected. And so in this tenth anniversary year of the end of the Cultural Revolution, socialist slogans are nowhere, and militantly socialist themes in the arts are bitterly denounced as dangerous relics of the ten-year catastrophe. By default, consumerism is the word in China. Peasants can get very rich; successful workers can buy their bits of electronic happiness; young people drink coffee, smoke, and take taxis on dates. Just now it seems to be exhilarating, at least for those who are making it.

Of course, there will be a price to pay, there is no doubt that this Chinese government, nearly unique in its public recognition of problems and shortcomings, past, present and future, will do its best to anticipate and alleviate them. Whether it will be able to control the effects of a sudden reversal of the rhetoric of equality, sharing and sacrifice on which a full generation was raised is not at all certain. But it can be argued that, analogously to the Americans trying to conduct a war in Vietnam without invoking the morality of patriotism, the Chinese Communist Party has no alternative but to reconstruct and modernize a society while avoiding the rhetoric of socialism. Those slogans had been completely discredited in the wreckage of the Cultural Revolution and the rule of the Gang of Four. Now they are raising a generation whose consciousness is being formed by the TV commercials, and whose urban working class (blue- and white-collar) will soon experience the vast wage differentials made inevitable by the influx of foreign capital and personnel.

At the moment, such problems are still on the horizon. Although there are still many of the inefficiencies and incompetences that are characteristic of the state of 'underdevelopment', still there is a sense of purpose (in making China great again), as well as a striving for more openness and criticism as well as more efficiency and expertise. There is still much inefficiency and confusion in the running of the Chinese economy, but it perceptibly improves and grows.

One sees the really ugly face of consumerism in those societies where 'developing' is a euphemism; where the traditional values are effectively dead, and 'modern' values sink to their most cheap and meretricious expression. There 'consumerism' can hardly be blamed as a cause; it is but one symptom of a social and spiritual pathology, otherwise manifested in universal corruption and brutality. In such context, it is a bad joke to speak of 'quality' at all, except perhaps in the techniques of oppression and vice. There we see what happens when the ancient symbols have totally lost their effectiveness, and the society cannot make a reality of the new ones, be they democracy or even consumerism. Such societies are rather like cruel, large-scale living laboratories, exhibiting what happens when values collapse. They serve as a reminder that we cannot take for granted that civilization as we know it will survive with its elementary decencies intact.

elementary determinants and the examples of America and China, because on them I can speak from personal experience. In those cases, appeals to patriotism or to socialism were, each in their own way, futile for the achievement of national socialism. Hence a form of consumerism was adopted; in the American case with goals. Hence a form of consumerism was adopted; in the American case with immediate catastrophic effects, and in the Chinese with profound long-term consequences as yet unpredictable. There may well be other cases, where religion has been invoked, with comparable results; but those I leave to others with more knowledge.

# Retrospect

I can now make it clear how this analysis relates to that of Ibn Khaldun. He analysed a repeated cycle of vigour and decay among rulers and consequently their societies. I suppose that this process was superimposed on a stable background of shared religious experience. This enabled the effective deployment of a charisma-industry, which used splendour and beauty as the visible part of the techniques for maintaining social solidarity. Thereby, obedience, loyalty and satisfaction of the masses could be achieved even under conditions which included evils that we can simply not comprehend. We have pale reminders of this in modern times, when even quite sophisticated societies exhibit patriotic fervour or veneration for a truly great leader.

I then suppose that for the past half-millennium at least, that formerly stable background has been universally in decline. The 'developing' societies suffer from more than the pathologies of the inheritance of colonialism; they are caught between the death of an old world, the only one in which their special cultures had any meaning, and the inaccessibility (by a multitude of causes) of the new one. China may escape from this trap, and there may well be some others. The 'developed' societies of the Occident are those which started their careers with an effective denial of that background experience; and so they have flourished as it has dwindled and decayed. But now they exhibit pathologies of their own, and some of them may well be due to the inadequacy of consumerism, a major source of their social solidarity, for the tasks of survival or even of national well-being. Making good plumbing involves different sorts of commitments and endeavours from making good rockets; and General Motors (or its symbolic surrogate) is not a cause for which men will lay down their lives.

When I considered the pathologies of consumerism at the level of

commitment and morale, I was drawing on the analysis of John Ruskin in Unto This Last. There he defines a profession as an occupation whose members should be prepared to die for its integrity. If someone would not, then he does not have a calling, and his work is utterly without significance. Ruskin's essay was in individualistic, moral terms; I have used his insight not to exhort but to analyse a social phenomenon. This is how I could argue that all my examples are part of the same phenomenon. China after Mao exhibits a recourse to consumerism, unavoidable in spite of all its obvious hazards, after the collapse of socialism as an inspirational ideology. America's failure in Vietnam shows its inadequacy as a substitute for patriotism in a war. And Challenger dramatically illustrates the fate even of industrial quality control in a society where consumerism is fully matured and dominant.

Considered in the light of social theory, this present essay is but another chapter in the long discussion of *Gemeinschaft* and *Gesellschaft*. I raise the question whether *Gesellschaft* can long survive with no moral foundation outside those of rationally calculated individualistic values, now realized on the mass scale as consumerism. It also evokes memories of Habermas's earlier writings, particularly his discussion of the *crisis of legitimacy* of modern governments; these must produce the goods of consumerism, or risk losing the consent of their governed. In the absence of that consent, then corruption and anarchy are the most likely consequences. In those terms, our benevolent, consumerist, democratic societies of the Occident are in a precarious situation indeed; they depend on a maldistribution of the world's wealth that cannot be justified, on an assault on the natural environment that cannot be sustained, and recently on the acquiescence of other nations with no grounds for a permanent loyalty to them.

If this conclusion seems gloomy, the cause lies not in myself but in our situation. I can analyse our predicament with my own experience and reason. To resolve it seems to me to require something more; and that I leave to others.

I am indebted to Isabel Phillips for the discussions in which many of these points were raised and clarified. This is a previously unpublished essay.

# Gaia and the Philosophy of Science

This essay is adapted from the transcript of a lecture given in October 1987 at a conference on James Lovelock's Gaia Hypothesis (first propounded in *Gaia: A New Look at Life on Earth*; Oxford University Press, 1979). In editing it, I modified the English style from the spoken to the written. But I felt that to remove the elements of dialogue in the text would deprive the essay of much of its liveliness. Hence readers will find themselves spoken to occasionally; this is not an uncorrected error, but a reminder of the origins of the essay in a very exciting and important encounter.

In thinking of what I could accomplish most usefully here, I decided that instead of giving you my opinions on various philosophical issues, I would try to clarify the issues on which other people can debate. I will therefore try to raise, in a rather condensed form, the different sorts of philosophical issues that have been in play in the discussions of the Gaia hypothesis. The only positive opinions that I shall offer will be not about Gaia but about philosophy.

# Philosophy and Gaia

One important thing about philosophy is that down through the ages it is always studying the same problems. Some people consider it inferior to science on that ground, for it never seems to get anywhere. But as humans we do face the same sorts of issues and dilemmas through the centuries; and an analysis given by a Plato or a Descartes can still be illuminating today. Of course, this is a matter of degree; if you are in a traditional Eskimo culture, or even a traditional Chinese culture, the formulation of the problems will be different to a significant degree. In our European culture, since the times of classical Greece, there has been a remarkable continuity of the great themes around which we do philosophy. It is quite reasonable to speak of the Good, the True, the Real, the Just, the Beautiful, and also the Human, while knowing that for

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each generation these root ideas must be seen somewhat differently and brought to life again.

It is against this background that I find the Gaia hypothesis so exciting. The hypothesis is that the earth is a gigantic homocostat, and one whose everchanging equilibrium states are created by life itself; so that our total environment, including the very rocks under our feet, is the product of the endeavour by life to maintain its environment. It seems to me that Gaia may become a very important event in our modern intellectual history. I am generally rather sceptical of new titles or new labels about scientific ideas, because most of them come and go very quickly, more like fashions than like truths. There are some grand organizing ideas that sound very exciting, but then they cannot be made into hypotheses for research science, and so they remain external to science. operating at the level of popularization or propaganda. By contrast, it now seems possible that the Gaia hypothesis will begin to give real coherence to what has hitherto been a rather complex and confused set of ideas about the natural environment. It will thereby become a very powerful organizing principle, analogous to continental drift in geology. It will suggest problems and regulate solutions, over a very wide range of natural phenomena, which had hitherto been considered too vast, and too complex, to be amenable to any approach other than crude and speculative simulations. With that strength, it will affect our perceptions of nature and therefore of ourselves, in a solid and determinate way. Thereby it will change the way in which we approach the perennial questions of philosophy.

In the discussions of Gaia that I have participated in and witnessed, I have sensed a variety of contrasting positions, and also the potential for conflicts among those committed to the Gaia hypothesis. In themselves, these are no bad thing, for they reflect the healthy diversity among our backgrounds and outlooks. But our debates will be more effective and constructive if we are clear about the issues that may divide us, and also understand that in these debates we are doing philosophy and not science.

# The Nature of Humanity

This is the first big issue to be raised by Gaia. In its old form, it involved the placing of mankind between the apes and the angels. Now it concerns our relation to non-human nature. Clearly, in some ways we are part of nature, and in some ways not; just as in some ways we depend on nature and in others we change it from outside. The debates can be on the ways in which these relations work out; or there can be a question of whether there is a decisive, essential answer on one side or the other, and if so which. We are interested in ourselves, wanting to know whether there is something very special about us as a species, different in some very important way from amoebas and dragonflies and cats. We *feel* as if we are different, and more important in some scheme of

things that is bigger than ourselves; and yet that is a philosophical position that can never be conclusively proved.

can never be conclusively protect. Gaia introduces a new element into this picture of ourselves in nature. Within the great dynamic homoeostat of the planetary system, we are a very little thing in physical terms, comparable to a culture in a Petri dish. We may not be very long-lived as a species, and so in another billion years there may be hardly any trace of our temporary presence. But while we are here, we can have quite significant effects on Gaia in her present phase. We may quite possibly be driving Gaia rapidly towards an unstable boundary, to where she must flip to a new phase, with very destructive consequences for ourselves and much else besides. It is even possible that the new phase of Gaia will be one that is at such an extreme of temperature that life as it has built up over the billennia will be extinguished or severely reduced.

billennia win be changement to look at humankind as not merely inter-Hence we are now forced to look at humankind as not merely interdependent with nature, or symbiotic with the rest of Gaia. Rather, in these terms we may be a pathogenic parasite on the whole planetary organism. It had done quite well without us for a long time, going through its cycles smoothly or roughly. Then *Homo 'sapiens'* arrives, and within a twinkling, on the scale of planetary time, he (should I use the masculine here?) does such things as to foul the whole system and destroy his nest and much else. To emphasize the point, let me try another analogy: ourselves (and particularly European man) as a weed. When a previously stable system is disturbed, the weeds invade and choke out everything else. Of course, after a while they create a new stability and are themselves squeezed out by a new flora. Is this the best that can be hoped for ourselves? If so, our pride in our accomplishments becomes rather muted, and we seriously wonder whether we are some sort of mistake.

Now, this pessimistic way of looking at humanity did not start with Gaia; ever since the Bomb and pollution, people, helped by science fiction writers and some scientists, have been aware of such possibilities. What is new with Gaia is that the issue now has a basis in science. The possibility that we are, on balance, a bad thing for our planet can now be stated in a precise, even partly testable form. This can cause a change in our image of ourselves comparable to those wrought by, say, Copernicus, Darwin and Freud. The first of these initiated a change in our picture of heaven and earth so that there is no longer a location for the angels up above, and for the damned souls down below. The second showed that no special creation was required to explain the origin of the many non-human species of living things, and so by analogy none was required for mankind. Finally, with Freud we discovered the unconscious, so that our reason, what really distinguishes us from the animals, turns out to be not so supreme and independent, but is partly governed by causes like the reactions of a goldfish to stimuli. Each of these scientific discoveries was opposed on the grounds that its philosophical interpretation would deny the dignity and uniqueness of mankind; yet humanity has survived them all, and

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(we think) with greater understanding and perhaps wisdom each time.

The contribution that Gaia makes to this sequence of discoveries is to take the process a step further. We are not merely an integral part of nature. depending on myriads of yet undiscovered natural processes for our very existence. But we are perhaps an unnatural part of nature, unique (to our present knowledge) among all species, in that we threaten to destroy the homoeostatic balance on which our existence depends. Hence the meaning of our existence on this planet is called into question. We can no longer assume that in some way we as a species are 'good' in the sense that each of us strives to be good to those around us. Perhaps collectively we are 'bad'; and if so, what is it all about, if anything? If by our own criteria of richness, diversity and complexity of organization and life, we as a species now threaten to destroy and degrade all of it on this planet, then our own value to - may I call it - the creation is seriously put into doubt.

Suppose that Gaia was doing just fine until we came along and introduced our increasingly unstable perturbations into the system, now possibly culminating in something quite planetismic within our lifetimes. What then of ourselves? Of course I am not predicting this; but since the possibility of such a man-made planetary catastrophe cannot be denied, then the philosophical, or existential, question is a real one. Thus, Gaia raises further disturbing questions about our place in a bigger scheme of things, if there is one; and given the scientific strength of Gaia, the philosophical question, a new form of an old issue, becomes all the more real and urgent.

# The Problem of Evil

This more general problem follows on naturally from those discussed just above. It is something that has been with us as a philosophical issue, since Biblical times; we have the book of Job and also the myth of Adam and Eve. Certainly in this century we have seen enough evil, either malevolent as with the Holocaust, or benevolent as with the Bomb. No philosopher or theologian has yet had a permanent success in showing that the apparent evil that pervades, even dominates, so much of human activity and human history is really good in a clever disguise. We have even had people who blame it all on civilization, who imagine what used to be called 'noble savages' as in the eighteenth century, or perhaps 'natural people' now, from Rousseau to Laurens van der Post, contrasting their purity and genuine realization of the values we profess, to our corrupted and sinful state. Others try to find some civilization which seems now, from its records, to have been in an harmonious and stable relation with its environment before some unfortunate accident terminated its life.

Such reflections are the negative reaction to the general optimism that has characterized European civilization for some centuries. We have applied science for the transformation of the means of production, and thereby

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achieved the solution of the curse of poverty at the material level. It only remains to reorganize our social arrangements, and then there will be enough food and fibre for all, together with an abundance of manufactured goods. And wherever there has been such progress in the material realm, there has been a corresponding improvement in the cultural and spiritual lives of people, with the driving out of the superstition and obscurantism on which thrive a reactionary clergy and their masters.

Here then, are the two contradictory views on the human meaning of industrial society. What contribution can Gaia make to this philosophical issue concerning the good society? In a general way, Gaia tells us that what has been going on over the last few hundred years cannot be extended to all mankind, or even sustained for very long. Regardless of its debatable merits for human advancement, it is only a temporary phenomenon. I like to make the point vividly with a question. Can the biosphere support a billion cars and also a billion air conditioners? This would be the load, if a Western standard of living, with instant transport and domestic climate control, were to be shared equally throughout the world. With the help of Gaia one could calculate the impact of the wastes in materials and energy that would be created by such a multiplication, fivefold or twentyfold, of these processes. Gaia reminds us that the biosphere is governed by interlocking cycles just as much as the biochemistry of a living body; and it can be poisoned in just the same way. But if such burdens on the downstream cycles of Gaia are not sustainable, then the vision of a just society for all mankind being achieved through our present technology is a delusion. We can keep our comforts for some time, and let the world's poor continue to rot; but that would be evil, and would sooner or later become destructive in physical as well as moral terms to us all.

Thus Gaia, as a sharpening of an ecological perspective, provides us with two philosophical issues arising out of the destructiveness of the ordinary operations of our modern industrial technology. In the long run (which may not be very long by planetary standards or even by human ones) the disruptive effects of our material culture will be producing vast and destructive changes; so that our own status as beings endowed with some superior qualities is called into question. Then, even in the short run, the impossibility of extending the current material benefits of our industrial system to all of humanity means that we are the Rich and they are the Poor; and the evil of injustice on a planetary scale is enforced not merely by consciously selfish politics, but by the exigencies of our productive machine.

### Knowledge and Ignorance

The last problem involved wastes, and that leads me naturally into my next topic. This is the theory of knowledge, frequently entitled by its Greek name, epistemology. For me the problem needs a new look; for we can no longer maintain the traditional view of science as rolling back the boundary with ignorance, perhaps even approaching truth asymptotically. The lesson of our industrial technology, as sharpened by awareness of Gaia, is that ignorance will always be with us (so long as things persist in their present form); and that indeed a *man-made* ignorance constitutes a great and ever-increasing threat to our survival. This is, I believe, a new move in epistemology.

I can illustrate this philosophical point by continuing my discussion of wastes; this is a paradoxical topic, perhaps even not in the best of taste, but I believe it to be relevant to the sort of philosophy that we need. 'Waste' is an increasingly urgent problem in industrialized societies; and yet we know very little about it. A few years ago I gave a lecture course on waste, and in my preliminary reading I scoured the catalogues of the University of California libraries, to find materials on the problem of waste. There was plenty on particular sorts of waste, but on Waste—nothing. This is probably because the industrial system does not yet recognize that there is a problem of Waste, only of particular wastes. The fact that it is being increasingly threatened with widespread poisoning by toxic wastes, and (in America at least) is becoming choked with nuclear wastes, is not yet seen as a systematic problem.

It can be argued that any culture needs to maintain ignorance, of some sort. about the things that threaten its integrity; we speak of taboos, in both the strict anthropological sense and also in the popular, social sense. Perhaps now waste, being so nasty, threatening and in the last resort unmanageable by our present approaches, is a taboo of late industrial society. Were we to consider seriously, systematically and publicly what is known of the environmental consequences of its activities, with waste primary among them but including other assaults as well, then so much that we now take for granted as benefits would be revealed as incurring incalculable costs as well. So the system maintains its plausibility by enforcing a sort of 'ignorance of ignorance'. All the concentration is upon our knowledge, of what we understand about nature, and how we can control her. The areas of ignorance, most easily seen in wastes and pollution, are left to the 'garbage sciences', starved of resources. prestige and influence. Mostly this can be accomplished by automatic means, since the whole social structure of science is organized away from effective work on such problems; in that sense we have 'socially constructed ignorance'. But in case that is not enough, many governments now ensure that there will be no trouble, by destroying the meagre research resources still available, on the principle that 'if business won't pay for research it's not worth doing'; here we may speak of 'politically constructed ignorance.'

One result of such tendencies is that we find an increasing number of urgent and intractable problems being thrust upon those with a competence to handle them, ranging from acid to CFCs, ozone and the greenhouse effect. In every case our knowledge, at least at the beginning of their study, is weak and paltry compared with our ignorance. And the policy implications of their uncertain conclusions are even more open-ended. When should we start making the investments for a planned removal of the world's major coastal cities, in anticipation of the rise in mean sea-level consequent on greenhouse heating? Such questions betray our ignorance, which now increasingly swamps our knowledge when we must make long-term decisions.

Of course, science has always had to cope with ignorance; and its progress over recent centuries has seemed so triumphant and inevitable because of the way in which the border with ignorance was rolled back in one field after another. But now we have a new phenomenon, which I call 'man-made ignorance'. This is an absence of necessary knowledge concerning systems and cycles that exist out there in the natural world, but which exist only because of human activities. Were it not for our intervention, those things and events would not exist, and so our lamentable and dangerous ignorance of them is man-made as much as the systems themselves. Most of our wastes are of this character; indeed we may say that the category 'waste' is itself a sign of a bad technology. (Lest I seem to have things too neatly sewn up, I may raise the question whether there is indeed waste in Gaia, such as the vast quantity of nitrogen in the atmosphere.)

Our man-made ignorance can extend quite dramatically to insoluble yet urgent engineering problems, such as the design of a repository for nuclear waste that will be safe for some tens of thousands of years. This is a very good example for illustrating the problem; I use it whenever I lecture on such issues. This impossibly long time-horizon in design is coupled with an urgently brief time-horizon in decision. The American federal authorities are increasingly anxious to 'solve' the problem of the nuclear wastes, at least in principle, lest there be some nasty accident at a temporary storage place when there is no remedy in sight. But, as Barry Commoner said long ago, everything has to go somewhere, and statistically negligible people can have political bite. So in the disposal of nuclear wastes (perhaps appropriately, given the general nature of the technology), we have at Yucca Mountain, Nevada, an exquisite interaction of knowledge, ignorance, Gaia and dirty politics: the classic case. Please excuse my aesthetic appreciation of what is a very nasty problem; I cannot help it.

When we consider the complexity and interrelatedness of the cycles by which Gaia maintains her balances, the massiveness of the disruptions which we now impose on her, the primitive quality of the scientific materials by which we attempt to decipher her clues; then truly we can speak of a man-made ignorance, criminal or pitiful depending on your point of view, in our relations with Gaia. Let me make it clear that I do not think that this ignorance is absolute or static; there is much that is being done by science, both inside and outside the Establishment, on all these problems. One of the enjoyable and exciting things about being at this conference is seeing science of such relevance, originality and excellence being reported and even being in the making. And certainly, more will be done, as the urgency of these problems becomes plain to us all, except perhaps for the most myopic or tyrannical of Gala and the Philosophy of Science

politicians. But the questions remain, will enough be done to sort out the existing damage to Gaia; and also will the scientists have the appropriate conception of their task?

### Methodology

When we reflect on the interaction of knowledge with ignorance in the scientific problems of Gaia, it becomes clear that a very new conception of what scientific work is about will become relevant and indeed necessary. This does not mean abandoning rigorous research using any appropriate method. be it quantitative, field research, simulation or what have you. But the relations of the scientists to the materials they explore, and to each other, will have to change. In his classic work on the philosophy of science, Thomas Kuhn described 'normal science' as the 'strenuous and devoted attempt to force nature into the conceptual boxes provided by [one's] professional education' With such a normality, it is not surprising that attempts to achieve genuine interdisciplinary research always founder. Nor is there any philosophical basis for resisting the inevitable trends for research to become atomized and fragmented, consequently and socially. How can there possibly be an integration of the various sorts of expertise relevant to any real Gaia problem. except when the research is done by those courageous individuals out on the margin, they and their work surviving precariously on the goodwill of supporters in funding agencies?

I believe that the recognition of ignorance can provide some basis for escaping from the atomism of the scientific life as we have experienced it hitherto. I might here paraphrase Winston Churchill's famous remark about greatness, and say that some research problems are invented (as in basic science), some are presented (as in mission-orientated science), and some are *thrust upon us* (as in problems of an assaulted environment). In this last case, scientists do not have the luxury of satisfying professional standards of rigour. Such problems may be described as having uncertain facts, disputed values, high decision stakes and urgent decisions. When we evaluate solutions to such problems, we broaden our perspective from 'correctness' (relative to the state of the art in experiment and theory), to 'quality' in a functional context that is partly technical and partly societal.

This is not the place to enter into a long discussion of the methodology of policy-related research. Let it suffice for now that I can see this as becoming a crucially important area of science, and one in which the assumptions about who is competent to do science and why, become drastically altered. In this, 'housewives' epidemiology' and TV investigative journalism will have their legitimate place, alongside the more conventional research. I could also argue that without the critical presence of such complementary sciences, it will be all the more difficult for the aware minority within the community of established scientists to make any impression on their colleagues and leaders. There is no need for me to labour this point here, since so much of what *The Ecologist* has published and fostered is in just this category of science.

As such science matures, there will be problems aplenty, practical and theoretical. One of the thorniest will be quality control. I wrote on this a long time ago, in my old book; and some of my gloomiest predictions seem to be coming true. We might ask, if the scientists themselves now have difficulty in maintaining good quality in their research, how can there be any chance of this when all problems are confused and conflicted? Well, I think there is an answer to that, not perfect by any means, but at least providing a mechanism. This is, public debate in all forums including those before a mass TV audience. Our system of trial by jury rests on the ability of ordinary people to see through the skills of advocacy, frequently employed on quite abstract and rechnical arguments. Without proposing any institutional forms at this point (for that would be wandering too far afield), I can imagine how an enrichment of the mechanisms for criticism (which as Karl Popper saw is the life-blood of science) could provide a means of ensuring quality control in this new sort of science, appropriate to the problems of Gaia.

When it is appreciated all around that a Gaia problem, either on a large or a small scale, is simply of a different type from that of atomized traditional science, then appropriate techniques will develop naturally. With them will come appropriate conceptions of the objects of enquiry of the sciences; the ruling assumption that 'anything larger than *E. coli* only serves to confuse the issue' in studying life will join other vulgar prejudices conceived in imitation of a conception of physics that died in 1905. Concepts such as integration and complex functionality will emerge from the backroom where biologists have entertained them somewhat shamefacedly, and be recognized as appropriate for Gaia just as much as consciousness is for humans. Thus, we may have a very exciting time ahead, in our thinking about what science is for, and is.

# Ontology

I cannot resist raising this last philosophical issue, even though it might make some people quite uncomfortable. 'Ontology' is the Greek term for the study of Reality; and with this I might seem to be introducing metaphysics or even religion into a scientific gathering. I should say at the outset that no particular conception of reality is entailed by adherence to the Gaia concept. Clearly, an old-fashioned atomist might have a lot of translating to do, back and forth between his concepts and those of Gaia; but for the human mind few such feats are impossible. At the other end, support of Gaia need not take a person further than acceptance of 'systems' and suchlike as real for the purposes of doing the science. And we all know that when Jim Lovelock chose the name 'Gaia' he was most definitely not implying that the earth is a goddess, or alive, or anything of the sort.

And yet, when we look at the earlier history, perhaps the prehistory, of the

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scientific conception of the earth as a great homoeostat governed by lifeprocesses, we see what must be called metaphysics. There were some quite amazing people around in Paris in the 1920s, and in their dialogues there could not have been any tight, defensive boundaries around their accepted realities. Vernadsky may have seen the whole thing as a vast harmonious hierarchy of systems; but then Bergson had his *élan vital*; and Teilhard de Chardin told explicitly of his experiences of something bigger and more meaningful than any merely perceptual events.

That was all long ago, and now we are all scientists rather than speculators. But, perhaps partly because of the playful name, partly because of the very deep human problems it raises, and also partly because of the approach to science that it fosters, Gaia is likely to make its contribution to the enrichment of realities that we are already experiencing. This is happening mainly on the medical side. When we know that ageing is a disease, and car accidents are an epidemic, heroic bacteriology is no longer the most effective paradigm for health problems. Also, when so many people are helped by acupuncture and allied techniques, it is hard to continue to say that *chi* energy is an Oriental superstition. What is to be done with the mind-body interaction, revealed in many manifestations from placebo effect through psychogenic disease and the practical success of healing therapies, is an exciting topic.

When I think of these enriched, perhaps nesting realities from the crudest atomism out to that of the visionary, I cannot help recalling that marvellous scientific satire of Victorian England, *Flatland*. There the realities were of dimensions; and the protagonist, a Square, was taken on a journey to see the busy Linelander and also the self-satisfied Pointlander. His education was in a third dimension, which (to his cost) he found impossible to communicate to his countrymen; and I must not tell you of the dénouement where awareness was shown to have its limits however high you go. We with Gaia can look down upon the old-fashioned atomist; but who is looking down on us?

I cannot make any prediction as to how Gaia will affect our perceived. accepted and (in a sense) socially constructed scientific realities. But science works in many ways; we know how it was the moon race, that essentially pointless extravaganza, that gave humanity its first effective vision of Gaia, blue, delicate and alone. It also impressed on Jim Lovelock that the earth's atmosphere is an unstable system; in this we have the distinction between the living earth and its dead neighbours, and hence the problem whose solution is Gaia. What sorts of perceptions and experiences will come now, is beyond my powers or interest to foresee. But once we have Gaia, it is difficult to keep the lines tightly drawn. I have in mind a metaphor that even Jim Lovelock has used, namely that Gaia can be 'sick'. Now I am sure that such an anthropomorphism can be translated back down into terms of stabilities, responses to shocks, and suchlike. But the term 'sick' is now in play, along with 'Gaia' herself. If we, so long accustomed to thinking of ourselves as the crown of creation, the only reality that really counts, come to see ourselves as guests of Gaia, and moreover bad guests who have been making our hostess sick, well then our reality will be that bit different, and the challenge of Gaia will have moved on another step.

This essay is the text, slightly modified, of 'Gaia and the philosophy of science', published in *Gaia, the Thesis, the Mechanisms and the Implications*, (eds P. Bunyard and E. Goldsmith) (Proceedings of the First Annual Camelford Conference); published in 1988 by the Wadebridge Ecological Centre, Worthyvale Manor, Camelford, Cornwall PL32 9TT, UK.

# Science, Ignorance and Fantasies

Our modern scientific technological culture is based on two articles of faith. The first, deriving from Bacon, is that knowledge is power, over our material environment. The second, from Descartes and his philosophical colleagues, is that material reality is 'atomic' in structure, consisting of simple elements denuded of interconnection and of causes relating to human perceptions and values. On that basis our civilization has achieved unparalleled success in theoretical knowledge and material power. But we all know that we are dangerously deficient in control. Viewed from *inside*, we may question whether we are only 'sorcerer's apprentices', capable of starting the magic engine but incompetent to control or stop it. Viewed from *outside*, our civilization may appear as a 'weed', dominating and choking out all other cultures in the ground disturbed by its material conquests.

In recent years there has been an increasing tide of criticism of this dominant world-view on all fronts. Following on the explosion of 'consciousness' among the affluent youth in the 1960s, the metaphysics of this civilization has been subjected to critical scrutiny, and many alternatives proposed. Some of these call for a return to world-views and religions which pre-date the rise of the great civilizations. More influential (so far) are those that draw on the cultural resources of the East, particularly the Taoist style of thinking. The presence of 'complementarity' in the structure of the most advanced theories of fundamental physics has been used strongly as evidence for the naturalness and 'scientific' character of this alternative framework of thought. The steady growth of 'alternative' or 'complementary' medicine, in fields where the 'atomistic' style seems ineffectual, counter-productive or positively barbarous, gives this other world-view a firm basis in successful practice and popular experience.

In the area of technology, the focus of Francis Bacon's dream, we have been coming to see more clearly how the solution of a problem at one level, such as in a 'technical fix', can produce more serious, perhaps insoluble, problems at other levels. The various forms of pollution, problems of the disposal of radioactive wastes, and the conversion of the former colonial world into a global slum and sweatshop for the 'advanced' nations are reminders of the inadequacy of a simplistic approach to 'power over Nature'. Here I shall develop some heuristic concepts whereby we may better comprehend such problems. I hope thereby to show how alternative styles of thought are as relevant to the control of material culture as to abstract physics or medicine.

# Value-Loading in Science, or the Social Construction of Ignorance

The optimistic philosophy of science of previous generations rested on a simple, linear scheme of the application of science to human benefit. Science produced facts, either in its own pursuits or in response to perceived social problems. In themselves these facts were value-free; the interests or prejudices of the individual investigator did not affect his conclusions, which were tested against the objective world of Nature. But in their totality, they embodied the highest human values. The miseries of mankind were easily seen to result from poverty, ignorance and superstition. The first two of these would be removed directly by the result of scientific enquiry; and the last would be defeated by the exposure of the real causes of human suffering, in material and intellectual culture. Those who espoused this philosophy were well aware that Science would not easily succeed on its own; there had to be a struggle against the institutions that profited from exploitation and oppression; previously, established religion and, more recently, an unjust social system.

The successes of this ideology, at least for the great mass of people in its homelands, must never be overlooked. Even now, when material poverty persists in the most advanced nations, there will be sharp practical contradictions between 'progress' (realized in the relief of drudgery and the production of jobs), and an 'ecological' awareness of the limits of 'growth'.

However, even within those highly developed economies, some systematic complications have been recognized. The theme of 'choice' has been appreciated as vital to the direction of science and technology. The image of the isolated, autonomous 'pure scientist', following his or her own curiosity and accidentally producing results of social benefit, is totally obsolete. Science is now a big business requiring choices for the allocation of limited resources. And technology cannot depend on an automatic mechanism of a market to turn inventions into successful innovations. In each case there must be 'policy', enabling direction to be given, and choices to be made, in accordance with general strategic objectives.

What is the source of such a strategy? It does not come from an immediate contact with Nature that is instantly and rigorously tested by results. Rather, it is found in institutions, which, since they embody power, must necessarily be closely aligned with the general political/economic structures of the society of which they are a part. The ultimate motive of such strategic planning may well

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be the improvement of the condition of mankind. But this aspiration will inevitably be filtered through the realities of power in any given context. Hence the science that is done (and perhaps more importantly, the science that is not done) reflects the values of a society as they are realized in its dominant institutions. In terms of this analysis, such slogans as 'science is not neutral' and 'science for the people' are not merely partisan rhetoric. They represent protests against the particular institutional arrangements for the productions of scientific knowledge, and also against the ideology of 'objectivity' by which it is still reinforced.

It might be thought that in spite of these forces shaping and (by some criteria) distorting the collection of scientific materials available to society, there must still be a hard core of 'facts' independent of these forces. This is a very delicate and sensitive question; for if we abandon all belief in our commitment to 'objectivity' in science, then there is no defence against charlatans or power-politicians deciding public policy on matters scientific and technological. Hence I only argue that 'objectivity' is by no means guaranteed by the materials or the techniques of science, but rather emerges partly from the integrity of individuals and partly from open debate on scientific results.

I can establish this point by an example from a common element of scientific technique: statistical inference. When statisticians test an hypothesis, they cannot possibly decide its truth or falsity; at best they work to within a 'confidence limit', which (roughly speaking) gives the odds (in terms of a mathematical model of the universe to which the given data are assumed to belong) that their conclusion is correct. Different problems conventionally are investigated to different confidence limits, say 95% or 99%. A more rigorous confidence limit requires a more expensive investigation. But a conclusion 'no evidence that . . .' is always relative to the pre-assigned confidence limit. A more searching test might have proved a positive result. Hence the values defining the investigation, the costs of 'false negatives' and of 'false positives', as well as the cost of the study itself, can determine the answer. A low-cost investigation can result in an effect remaining concealed. Knowledge is costly; but the price of economy is continued ignorance.

This general point of methodology can become an issue of political struggle in the case of suspected pollutants. When one considers all the methodological problems of field investigations, ranging from the inherent imperfections of data, through the weight to be assigned to indirect evidence (as from animal studies), the assumptions of 'normal' practice, and the implicit burden of proof in any regulatory decision, it is easy to see why at the present time methodology has become overtly political, at least in those countries (such as the USA) where procedures are required to be published and available for criticism. There, the typical situation is for 'the facts' provided by science to be the focus of debate in public forums, regulatory agencies, and the courts as well.

All this occurs only when a scientific issue has become salient, and there are

institutions for its public debate. Until then, and generally elsewhere, the public is ignorant of environmental hazards. The ignorance is not due to an essential impenetrability of the phenomena, but to social decisions (taken in leading institutions of state and of science) to neglect certain problems in favour of others. Such problems will usually *not* be those promising prestige and rewards to a scientific élite, but rather those involving diffuse, imperceptible, chronic or delayed effects of the unintended by-products of the industrial system. In that sense, our scientific-technological establishment moulds public awareness, by negative means, as surely as the theological establishment of earlier times did by indoctrination and prohibitions. The 'social construction of ignorance' is a phenomenon of our modern period, all the more important because it happens unnoticed and in contradiction to the received ideology of science as the bearer of Truth.

# Technological Blunders

Corresponding to the new uncertainties in science, we have the recent discovery of the possibility of massive blunders in technology. For a long time it had been recognized that the costs and benefits of technological advance are unequal in their incidence. The conquest and destruction of native peoples by those with superior means of production or destruction is no longer easily justified by 'the progress of civilization'. But we must now reckon with a new species of 'bad' arising from the supposed automatic 'good' of technological progress. This will occur most obviously where a technology is strongly innovative and lacking the automatic controls of a competitive market. Then it can happen that ignorance in the design process and incompetence in fabrication and operation can combine to produce a resounding failure. The most notorious present case is the civil nuclear power industry in the USA. There, cost and time overruns have produced crippling burdens of debt on utilities, even when plants are completed. And when they are abandoned after the expenditure of hundreds of millions of dollars, the victims (utilities and their customers) are left with massive debts and the real possibility of bankruptcy. And incompetence in operation, resulting from the power industry's being quite unprepared for the sophistication of the technology with which it was presented by science, produce even more crippling burdens.

Less obvious on the ground, but equally dramatic, are those cases where chemical manufacturers proceed for years to produce substances that are hazardous in all sorts of ways, choosing frequently to remain in wilful ignorance of the dangers to their workforces, consumers and the general public. When this socially constructed ignorance is eventually exploded, it appears that the guilty men were only ordinary people doing their jobs within the constraints of compartmentalized bureaucratic responsibility and generalized cost-cutting.

The question, how could all this happen? is a real one. Engineers and plant

managers of all sorts, presumably well trained and competent in their jobs, have as a group allowed major industries to cause great inconvenience and damage, and now to face destructive popular antagonism. A part of the answer may lie in the traditional education and outlook of such persons. It has been overwhelmingly restrictive, and reductionist, preparing for competence in routine operation, but providing no tools, technical or conceptual, for coping with the new problems of modern high technology. These problems include extreme sensitivity of plant to deviations from 'normal', so that simple, unavoidable errors can have costly or catastrophic consequences, most familiar in the case of nuclear power. Further, environmental impacts, no longer the gross, obvious pollutants of nineteenth century factories, lie totally outside the technical competence or experience of those who design and operate installations. Trained to solve simple problems in traditional ways, the engineers are far from being in control of the hyper-sophisticated technologies they have created.

# Quality Control—The Moral Element

Such problems in technology may be viewed merely as the growing pains of some industries where progress has been a bit too rapid for comfort for a couple of decades. That may well be; only time will tell. But these phenomena do raise the problem of the maintenance of quality control in these fields. The recent spate of publicized cases of fraud, plagiarism and the claiming of coauthorship on another's work show that the problem is also present in research science.

The maintenance of quality control in industrial production has become relatively straightforward. Once the quality of products is appreciated by consumers, quality control is understood by management to be essential for sales and survival; and techniques for employee participation are easily transplanted between such different cultural milieux as Japan and the United States. But in science it is otherwise; there is no external set of discriminating consumers, no hierarchical management, and no simple tests of quality of unit operations. Hence research science must be self-policing; and the wide variation in quality of work between different fields and different centres shows that the problem has no automatic solution. If we ask what motivates the individual scientist to invest the extra time and trouble to ensure the highest possible quality of his or her research, there can be several answers. The simplest is prudence; poor workmanship will be detected and rejected by colleagues. But this presupposes a collective commitment to high quality, and so, in effect, begs the question. Other reasons lie in the personal integrity, and pride of craftsmanship of the individual scientist, operating either as a researcher or as a quality controller in refereeing or in peer review. However, these are moral attributes; they are not automatic consequences of the

research process, nor can they be instilled by simple political or administrative means.

Scientific progress is uniquely sensitive to the maintenance of quality. Innovative work is hard and risky; the minority who dare and succeed can all too easily be smothered by an entrenched mediocrity that wishes to stay comfortable in old routines of problems and techniques. Thus, the maintenance of a generally good quality of research is a necessary background for the emergence of excellence and originality. Governments, even industries, can survive for a long time in a state of complacency and inefficiency, even enduring corruption. When such a situation exists in a field of science, the effects are not visible to the inexpert eye: teaching, research, conferences, grant applications continue smoothly; the one thing lacking is anything worthwhile happening.

Hence the value component of science has another essential element: the commitment by enough scientists, and particularly those in positions of political power in their scientific communities, to the production of good work, really for its own sake. Otherwise all of the world's research science would soon become like that recognizable in various backwater communities: much spurious activity, but no contribution to either knowledge or human welfare.

Similar phenomena can be observed in fields of technology where purchasers can be captured by producers, notably state (particularly military) procurement. It may seem outrageous and incredible that military authorities would endanger the lives of soldiers, and compromise the chances of victory in eventual wars, for the sake of bureaucratic convenience or advantage. But it is so; the examples are best known for the USA, but perhaps mainly because of the greater openness of government there.

Thus, even in the cases of the most 'hard' and 'objective' fields of human endeavour, we can discuss the effects of a 'moral environment'; if not enough people *care* about quality, then it will inevitably be lost. Cyclical theories of civilizations, usually cast in terms of political and military affairs, and standards of private morality, may be seen to apply to science and technology as well.

## Fantasy Hardware, the Ultimate Aberration

Before the advent of modern science, there was a well-recognized category of 'secrets too powerful to be revealed'. Whether they were actually so, we will never know. But in any event, the optimistic faith of the seventeenth century prophets of modern science rendered that category void. Although great material powers were promised through the new science, they were understood to be strictly limited. In the materialist world-view, effects were commensurate with causes; enhancements by spiritual or magical means were seemingly absurd.

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By the later nineteenth century, the technology of war was eroding that basic metaphysical assumption. Inventors were once again producing 'weapons so terrible that they would make war impossible forever'. Thus, nuclear weapons were not a totally new phenomenon; they were in a continuous development ideologically as well as technically. First seen as a cheap but very dramatic extension of means of quickly destroying a city and its inhabitants, they were indeed used, partly for their immediate effect and partly as an extension of diplomacy.

However, the second generation of nuclear weapons, involving enormously enhanced destructive power, effective means of delivery, and a sharing of the technique between the two major antagonists, did introduce a qualitatively new element into warfare. It was universally admitted that it was highly undesirable to use such weapons, even though only a critical minority argued that a nuclear war could not be 'war' in any meaningful sense.

The function of such weapons then shifted drastically: it became 'deterrence'. This concept was twofold: it referred to nuclear war involving an exchange of long-range missiles, but it also extended to the discouragement of a 'conventional' war in Europe. In the 'pure' case of intercontinental ballistic missiles, 'deterrence' introduced a very new sort of problem into military theory. Strategic thinking was concentrated on games of bluff and counterbluff, with models from 'the theory of games and economic behaviour', and with payoffs in mega-deaths. This was very quickly exposed as an idiotic pseudo-science by an eminent military scientist, Sir Solly Zuckerman. But he was ignored, by politicians, strategists and philosophers of science alike. Hence the gigantic machine of nuclear armament, distorting the economies and the politics of all the world's nations, and presenting an ever-increasing threat to the survival of mankind, had as its rationale a strictly nonsensical theory. What a fate for a civilization that so proudly bases itself on science!

Practical contradictions also afflicted nuclear strategy, though these took a couple of decades to mature. The 'defence' of Europe by the threat of its obliteration through American-controlled weapons led to increasing disquiet there. 'Civil defence' finally revealed its idiocy in American plans, for evacuations, requiring (for example) the inhabitants of each of the 'twin cities' Minneapolis-St Paul to seek refuge in the other!

'Independent' deterrents by second-rank powers as Britain and France could be only an expensive means of maintaining fantasies of national glory. And the spread of nuclear weapons to less-responsible ruling élites poses a sinister threat that cannot now be removed.

Such a situation might seem as bizarre as possible, until a new element was revealed in the early 1980s: the weapons themselves are unreliable. American missiles have been tested only on constant-latitude paths. Hence, any talk of 'first-strike', 'counter-force' attack (by missiles travelling over the pole and targeting with great accuracy and precision) is pure fantasy. Further, the coming generation of American missiles seem likely to impose a *de facto* 'freeze'. The MX system is totally devoid of any plausible function, except to keep the Air Force in the nuclear arms business. The Pershing II missile is a design disaster. The Cruise missile can fly sometimes under optimal conditions, but it is so plagued by difficulties that its production run has been seriously curtailed.

All these facts are in the public prints, and yet all sides in the nuclear debate choose to ignore them. Of course, existing weapons are in place, the spread continues, the threat to humanity is as menacing as ever. But it seems to be in no one's interest to make political use of this essential feature of nuclear weapons: as well as being absolutely evil, they are also absolutely insane, even to the point of becoming increasingly a matter of sheer fantasy.

# Conclusion

The metaphysics of our civilization is based on an absolute distinction between the 'primary' quality of things, taken from mathematics, and the 'secondary' ones, involving perceptions. 'Tertiary' qualities, involving values, are allowed metaphysical reality only on Sundays. This world-view has been dominant for some three centuries. Now its contradictions have matured. They are most manifest in plans to base a nuclear strategy on a future missile system that will certainly never operate. This complete interpenetration of fantasy and hardware could be seen as a sort of Zen koan; and perhaps some day it will.

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### References

The general problem of quality control, and the importance of morale and of moral imperatives, is discussed at length in my book *Scientific Knowledge and Its Social Problems* (London: Oxford University Press, 1971) (also published in Japanese translation).

The phenomenon of the misdirection of science, to the neglect of problems of human and environmental concerns, is discussed in *Quality in Science* (ed. M. Chotkowsky La Follette. Cambridge, Mass.: MIT Press, 1982), particularly in the essay by Harvey Brooks, 'Needs, leads and indicators'.

The most recent study of the provision of low-quality or inappropriate weapons to the American military is *National Defense*, by James Fallows (New York: Random House, 1981). His most striking example is the modification of the M-15 rifle into an ineffective weapon for use in Vietnam, in the interest of the preservation of a bureaucratic monopoly on design and testing.

For the history and institutional/political theory of the development of nuclear weapons and nuclear strategy, an eyewitness account is by Lord Zuckerman; see his *Nuclear Illusion and Reality* (London: Viking, 1982).

# CONSTRUCTIVE APPROACHES

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This section begins with a sample of my recent research, conducted with S.O. Funtowicz, on quality control of scientific information through the management of uncertainties. This is at its core a technical solution, through a notational system; it may not be immediately apparent what relation it has to the vast problems that I have been discussing hitherto. Perhaps the link can be established through the concept of 'ignorance of ignorance' which I have used on occasion in my writings. If we lack means to express the severe uncertainties that affect our information on the major problems, then they will not be expressed; and being unexpressed they will be ignored; and then we will persist in the illusion that we know (because the scientists have provided us with numbers precise to two or three digits) when in fact we have educated guesses at best. I used the term 'we' for the sake of brevity in a general analysis; but when we recall how many debates over environmental threats resolve into disagreements over the quality of official data, then an instrument for quality assessment that is simple enough for use by concerned citizens can make a significant contribution to the quality, and fairness, of such debates.

The interaction of knowledge, ignorance and policy has become an explicit concern among those grappling with the problems on a planetary scale; some years ago I participated in a conference intended to define an international, trans-disciplinary research programme on the biosphere. For this I was encouraged to explore ignorance as it relates to policy; and since I suspected that this would be a new concept for most of the scientists involved, I introduced the topic by easy stages, paralleling the experience of a scientist through his education and career. I was emphatic that this is not a question to be resolved by conceptual analyses; but rather that working scientists would need to revise their management of subject-speciality self-protection. Otherwise such efforts would amount to little more than a pooling of separate

### **Constructive Approaches**

uncertainties rather than an integration of common knowledge. It seems to be a general experience that the perils associated with interdisciplinary research are as yet more real to most scientists than the threats to survival that require such work for their control.

If the scientific communities are generally incapable of breaking out of the 'normal science' as defined by Kuhn (puzzle-solving within unquestioned, unquestionable paradigms), what can be done? In the next two essays I review the various alternative approaches that were spawned in the confusion of the 1960s, and which have moved towards stability and maturity ever since. Working first in the political and social dimensions, I review the old 'social contract of science' whereby science enjoyed the immunity of scholars in return for promising the benefits of inventors. That can no longer be sustained, as the old beneficent, omniscient image of science falls apart. A wider involvement of non-experts in 'science', in some sense of the term, is inevitable and appropriate. Here I indicate three sorts, labelled 'alternative', 'activist' and 'practical', very different in their functions and in their supporters; but all of them serving valid purposes. Should such enriched conceptions of science make their way into teaching, then there is a chance that the dusty decline of science education may yet be arrested.

Viewing the same phenomenon historically, in terms of 'orthodoxies, critiques and alternatives', I go back to the prophetic faith of the Scientific Revolution about the Way of Science, and show how its inherent contradictions, latent or manageable then and for centuries afterwards, have matured and become manifest. Most of the twentieth century has been needed for this process to achieve fulfilment; it started first in philosophy, only gradually extended to the social critique of science, and then most recently to a cosmological perspective. The sorts of 'alternative sciences' now flourishing have a variety of purposes, from the more political to the more private. I find 'alternative medicine' or simply 'healing' among the most significant, since it poses the greatest challenge to professional and metaphysical structures, in the most unobtrusive and non-violent way. As such tendencies gain in plausibility and acceptance, the common sense of science, set several centuries ago, must inevitably be modified, in ways that may have surprises for us all.

Finally I reprint (slightly modified) my first attempt at a unified conception of it all, in the discussion of 'critical science' in my previous book. There are many gaps in the vision I had then, which are easily seen by comparison with the recent essays. But in general I feel that it has worn well, and it is useful for showing both the development and the continuity of my thought over the last two decades.

# Qualified Quantities: Towards an Arithmetic of Real Experience

This study has been motivated by two problems at widely separated places in the methodology of the natural sciences. One is the crisis in the philosophy of science, caused by the continuing failure of all programmes to identify a logical structure which could explain the previously successful practice of natural science (Shapere 1986). The other is the failure of the traditional methods of laboratory science to encompass problems of risks and the environment in the policy process. Few people are aware of both problems and their possible connections. Here we will indicate their common root and, while not attempting a 'solution' cast as some formalism, we will exhibit a practical device whereby quantitative statements can be made in a clear and effective way.

The two problems actually come together, implicitly at least, on those issues where in one way or another the traditional methods of science have revealed their inadequacy. In the debates on environmental and occupational hazards, which are bound to increase greatly before they ever abate, popular conceptions of science tend to change drastically from naive trust to embittered cynicism. Having been told in school, in the media, and by all the accredited experts that science (in legitimate hands) can and will solve all our technical problems, citizens may then have a very different sort of experience, frequently involving procrastination, prevarication or even concealment and deception at the hands of the very experts employed to protect them against hazards. All scientific expertise then tends to become used as a debating tool, at the level of courtroom psychiatry. In debates on large-scale problems, such as engineering projects constituting 'major hazards' or major environmental intrusions, or in the speculative technologies of nuclear armaments, the dividing line between science, nonsense and fantasy becomes very difficult to discern. The traditional methodologies of scientific research offer insufficient protection against the corruption of reason that modern conditions encourage, even in our dealings with the world of Nature.

Our contribution is a new notational system (Funtowicz and Ravetz 1986)

for the expression of quantitative information, one which provides places for each of the judgements describing the different sorts of uncertainty with every quantitative statement is qualified. We call it NUSAP, an abbreviation for the categories Numeral, Unit, Spread, Assessment and Pedigree. The last three convey inexactness, unreliability and 'border with ignorance', respectively, Familiar analogues exist for the first two of these, spread and assessment, in variance or experimental error for the first, and confidence limits or systematic error for the second. The last one, pedigree, does not have a precedent in ordinary scientific practice or statistical technique; we define it as an evaluative history of the process whereby the quantity was produced. By means of that history, we characterize the state of the art of the production of the quantity. This exhibits the inherent limitations of the knowledge that can be achieved thereby, and in that sense demarcates the border with ignorance in that case. The first two places, numeral and unit, are close enough to their traditional analogues to need no explanation as yet. Within each place, or box, appropriate notations, depending on the applications, may be employed.

The usefulness of a tool like NUSAP for application to what we may call 'policy-related research' or 'public-use statistics' is not too difficult to imagine. If highly uncertain quantitative information were required to be written with all its qualifying places explicit, we could more quickly identify pseudo-precise or scientifically meaningless quantitative statements. In this respect the NUSAP notational scheme could function as an instrument of quality control, in an area where it is both urgently necessary and extremely difficult.

On the side of epistemology, the contribution cannot be so direct; but we hope that it will provide a basis for transcending the seventeenth century metaphysics in which geometrical reasoning was to supplant human judgement as the route to real knowledge. Instead of erecting some general, allencompassing, polar-opposite alternative to our dominant 'reductionist' science, be it in the form of a 'holistic', 'romantic', 'idealist' or 'voluntarist' philosophy, we can in a practical way exhibit the essential *complementarity* of the more quantifying with the more qualifying aspects of any quantitative statement. Human judgements are then seen, not as inhabiting some separate realm from exact mathematical statements, bearing a relation which is either hostile, mysterious, or non-existent; but rather as a natural and essential complement to the more impersonal and abstract assertions embodied in a numerical expression. When this insight, made familiar in everyday experience, is available for philosophical reflection, then we may be in a position to go beyond Galileo's (1632) classic pronouncement that the conclusions of natural science are true and necessary and that 'l'arbitrio humano' has nothing to do with them. Thus, NUSAP may make a practical contribution to the recently developed tendency in the philosophy of science, which gives some recognition to the informal aspects of scientific argument and rationality (Putnam 1981, Jiang 1985).

# The Problem: Uncertain Quantitative Information Represented by a 'Magic Number' Form

The problems associated with the provision and communication of quantitative information for policy-making in economic and social affairs are well known. It might be thought that the difficulties of producing 'usable knowledge' (Lindblom and Cohen 1979) in these fields are caused mainly by the inherent limitations of definition and measurement of their relevant aggregated statistical indicators. But it is increasingly recognized that in policy-making for technology and for the natural environment, similar difficulties arise. Planning for investment in technological and industrial developments is characterized by frequent uncertainty and occasionally by irremediable ignorance (Collingridge 1980).

The matter now takes on some urgency, in view of the growing proportion of scientific effort that is devoted to the understanding and control of the environmental and health consequences of technology and industry. Increasing space, both in the media and in research journals, is occupied by such topics as radioactive pollution, acid rain, agricultural chemicals and pharmaceutical products. A variety of research fields are called on to provide quantitative technical information which, it is hoped, will contribute to the resolution, or at least to the definition, of these practical problems.

Such issues are the subject matter for the policy-related sciences, whose function is to provide this new sort of usable knowledge. Because of the complexity and frequent urgency of some of these issues, the research communities do not always possess the knowledge and skills required for immediate effective solutions. Even experienced advisers may find it difficult to convey to policy-makers an accurate reflection of the scope and limits of the results that can be achieved under these constraints. Solving the problems of representing and evaluating technical information in these contexts, and also of identifying meaningless quantitative expressions, thus becomes of great importance for the proper accomplishment of public policy in these areas.

Policy-analysts have long been aware of this problem, and have searched for means of expressing strongly uncertain information. Thus:

One of the thorniest problems facing the policy analyst is posed by the situation where, for a significant segment of his study, there is unsatisfactory information. The deficiency can be with respect to data—incomplete or faulty—or more seriously with respect to the model of theory—again either incomplete or insufficiently verified. This situation is probably the norm rather than a rare occurrence. (Dalkey 1969.)

In spite of these manifest inadequacies in the available information, the policy-maker must frequently make some sort of decision without delay. The temptation for her/his advisers is to provide her/him with a single number, perhaps even embellished with precise confidence limits of the classic

statistical form. When such numbers are brought into the public arena, debates may combine the ferocity of sectarian politics with the hypersophistication of scholastic disputations. The scientific inputs then have the paradoxical property of promising objectivity and certainty by their form, but producing only greater contention by their substance (Nelkin 1979).

Indeed, there is now an increasing tendency for public debate to focus more on the various uncertainties surrounding the numbers than on the policyrelevant quantities themselves. This has happened most notably in the cases of 'the greenhouse effect' and acid rain. Such debates on the uncertainties will always be inherently more difficult to control and comprehend than those at the policy level. They unavoidably involve all aspects of the issue, from policy to methodology and even to state-of-the-art expert practice in the relevant scientific fields.

In all the fields of formalized decision analysis (e.g. Risk Analysis, Multi-Attribute Utility Theory, Operational Research, Decision Research, 'Hard' Systems Theory), practitioners are now searching for means of expressing subjective factors. This endeavour frequently confuses very different aspects of technical information, such as social value-commitments, group interests and personal judgements, as well as qualifying attributes of quantities. Innovations in statistics have not proved adequate to resolve such confusions. Under these circumstances, there is a real possibility that risk-analysis practitioners and those they advise will despair of objectivity, and in the resolution of policy issues will oscillate between emotional interpersonal contacts and ruthless power politics. Some even argue as if 'pollution is in the nose of the beholder'. and reduce all environmental debates to a conflict between sensible and sectarian life-styles (Douglas and Wildavsky 1982). We believe that the core of objectivity in policy decisions must be analysed and exhibited afresh, so that consistent and fair procedures in decision-making can be defended and further articulated.

Thus, the traditional assumption of the robustness and certainty of all quantitative information has become unrealistic and counter-productive. The various sorts of uncertainty, including inexactness, unreliability and ignorance, must be capable of representation. The task was well described by W.D. Ruckelshaus (1984), when Administrator at the US Environmental Protection Agency:

First, we must insist on risk calculations being expressed as distributions of estimates and not as magic numbers that can be manipulated without regard to what they really mean. We must try to display more realistic estimates of risk to show a range of probabilities. To help to do this we need tools for quantifying and ordering sources of uncertainty and for putting them in perspective.

The above reference to 'magic numbers' is not merely rhetorical. Our culture invests a quality of real truth in numbers, analogous to the way in which other cultures believe in the magical powers of names. The classic statement is by Lord Kelvin:

I often say that when you can measure what you are speaking about, and express it in numbers, you know something about it; but when you cannot measure it, when you cannot express it in numbers, your knowledge is of a meagre and unsatisfactory kind. (Mackay 1977.)

A quantitative form of assertion is not merely considered necessary for a subject to be scientific; it is also generally believed to be sufficient. Thus, the problems discussed here are not only related to the inherent uncertainties of the subject matter (as for example in risks and environmental pollutants); they originate in an inappropriate conception of the power and meaning of numbers in relation to the natural and social worlds. By their form, numbers convey precision; an 'uncertain quantity' seems as much a contradiction in terms as an 'incorrect fact'. But this image must be corrected and enriched if we are to grow out of the reliance on magic numbers; only in that way can we hope to provide usable knowledge for policy-decisions, including those for science, technology and the environment.

# Numerical Language: Pathologies and Pitfalls

The new requirements on quantitative information for policy-making have revealed inadequacies in the traditional numerical means of representation and in the implicit beliefs underlying them. But we should not think that a natural and faultless inherited numerical system is suddenly being stretched beyond its limits of applicability. Reflection on the history and existing uses of numerical systems shows that they contain many pathologies and pitfalls. These derive from the traditional basic conception of numbers as designed for counting collections of discrete objects. For measurement of continuous magnitudes, the traditional tool was geometry, with an 'analogue' rather than 'digital' approach. The combination of counting and measuring in practice involves estimation, for which no notational systems were developed until quite recently. But the uncritical use of numbers, with their connotation of discreteness and hence of absolute precision, still causes blunders and confusions at all levels of practice.

Such imperfections are not advertised by teachers adhering to a 'pure mathematics' pedagogical tradition. The subject of 'estimation' had indeed flourished in nineteenth century 'practical arithmetic'. But the influence of modern academic research in mathematics, culminating in the 'new mathematics', encouraged the teaching of the élite skills of manipulating abstract structures to schoolchildren. These did not complement traditional skills, but effectively alienated even arithmetic from practical experience (Kline 1974). (Such abstraction, perhaps based on disapproval of rote-learned

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practical craft-skills, had its analogue in the 'global method' for teaching reading while ignoring the alphabet. For several decades children emerged from the best schools unable either to count or to spell!) Such fashions in abstraction enable us to extend the insight of Gödel's famous theorem (1931) to the present context. As Kline (1972) expressed it:

Gödel showed that the consistency of a system embracing the usual logic and number theory cannot be established if one limits himself to such concepts and methods as can formally be represented in the system of number theory.

Here we are dealing with understanding rather than proof. In the 'new math', a more logical and complicated formalistic language for arithmetic was achieved at the expense of the loss of comprehension of the rich and contradictory world of the practical experience of quantity.

The confusions of arithmetic could be safely ignored so long as tacit craftskills were adequate for coping with the ordinary problems of application. The task of programming computers for calculations, where nothing can be left tacit, has forced some awareness of the practical problems of managing the uncertainties in all quantitative information. There is already a flourishing literature on 'numerical analysis' at all levels; but as yet no coherent and effective exposition of the management of the different sorts of uncertainty is available. Hence the ordinary practice of calculation is still afflicted with paradoxes and blunders, the sort that 'every schoolboy' should know, but doesn't.

For our first example, we may consider the representations of fractional parts of unity. We may say:

1/4 = 0.25 but 1/4 inch  $\neq 0.25$  inch

In the first case we are dealing with 'pure arithmetic', and the equality results from a simple calculation. But in the second case we are dealing with measurements, in this case inches; our objects are not 'points on the real line' but 'intervals of estimation', characterized by a 'tolerance'. Each representation has its own implied tolerance (or, as we shall call it, *spread*), and so 1/4 inch and 0.25 inch mean quite different things. In the former case, the next lower unit of magnitude (implying the interval of inexactness) is likely to be 1/16inch, while in the latter it is 0.01 inch, smaller by a factor of 6. Drawings of specifications in the different units have different implied tolerances, and thus mean very different things in practice. Managing such anomalies may be quite trivial to those involved in such work, but this is achieved by the adoption of implict conventions for interpretation, whose understanding may be restricted to a particular specialist group. (The traditional tables of decimal equivalents of common fractions, with entries such as 1/16 inch = 0.0625 inch, are examples of the deep confusion in this practical matter.)

A mention of tolerance (inexactness, error or *spread*) will usually provoke the response that all that is handled by the 'significant digits' (s.d.) convention. But to describe exactly what that is, turns out to be far from trivial. Indeed, the rule for preservation of s.d.s in a calculation is not at all straightforward. For example, if we wish to know the circumference of a circle whose radius is 1.2 cm, then we round off  $\pi$  to 3.1; but if the radius is 9.2 cm, then  $\pi$  should be 3.14, since the 'proportional error' in the second radius measure is of the order of 1%, while that in 3.1 is some 3%, inappropriately large. Thus the choice of the number of s.d.s to include in a numerical expression will depend on the calculation at hand, and the rules for choice will not be trivial.

The above examples may seem to relate only to unsophisticated practice. But the following subtle blunder has been observed even in high-level tables of statistics. Suppose (for simplicity and clarity) we have a population of just seven elements divided into three groups of 2, 2 and 3 respectively. A common tabular display would be:

| 1                           | %   |
|-----------------------------|-----|
|                             | 29  |
|                             | 29  |
|                             | 42  |
|                             | 100 |
| nelogi (Deletari e deletari |     |

There seems nothing wrong here, until we observe that 3/7 is strictly 42.8%; which should be rounded-up to 43%, just as 2/7 or 28.6% was rounded-up to 29%. But then the sum would be 101%; and how often do we see percentages summed to a figure other than 100%? Paradoxically, we may say that a 100% sum is most likely to be the result of fiddling the separate percentages! It reflects an incomprehension of the arithmetic of rounding-off, and is a more amusing example of educated confusion about quantities.

A particular unfortunate consequence of such blunders is that they impart an air of incompetence (however vaguely this may be articulated) to the reports in which they are manifested. Although explicit rules for the criticism of pseudo-precision are not widely diffused, many who use statistics are aware of the principle enunciated by the great mathematician Gauss:

lack of mathematical culture is revealed nowhere so conspicuously as in meaningless precision in numerical calculation. (Ravetz 1971.)

One simple way to avoid such blunders is to recognize units of aggregation in countings, and the possibility of 'swamping' one quantity by another. Paradoxically this phenomenon is more difficult to recognize because of a fertile ambiguity in the quasi-digit 0. This can function either as a 'counter' or as a 'filler'. Thus when we write '10', we understand 'zero in the unit place' as a digit distinct from the neighbouring digits 9, and 1 in 11, but when we write '1000', this usually refers to a count of 1 on a unit of a 'thousand', analogous to 'dozen' or 'score'.

By these examples we can see that imperfect quantitative information can be managed with greater or lesser skill. Its inherent uncertainties may be hidden, leading to confusion and blunders, and to doubts of the competence of the authors. Or the uncertainties may be clearly exhibited, improving the credit of the authors and providing more useful inputs to decision-making. One can never decrease the inherent uncertainties, or enhance the inherent quality, of any given information by such means; but as we have seen it is possible to transform the information into a more effective tool for decisionmaking.

## Notation, Language and the Concepts of Science

The examples of the previous section show how numbers may sometimes convey confusion rather than clarity; and a diligent search by any reader will reveal many instances where blunders in the manipulation or interpretation of quantities occur in all fields, and at all levels of expert practice. If we accept this phenomenon as real, we should reflect both on how it has come to be, and also why it has not been noted and analysed before now. We believe that such incompetence cannot be ascribed merely to 'bad teaching', when it persists in practice long after the end of formal schooling. Rather, we would say that such defects in practice, particularly because they are unnoticed, are indicators of unresolved contradictions quite deep within the 'paradigm' (Kuhn 1962) that defines that practice.

The paradigm in question is the metaphysical commitment to a certain sort of world of Nature (and by extension, humanity), and to the central role of a certain sort of mathematics in the structure of that world and in our knowing it. This is the world of the seventeenth century scientific revolution, where reality consists of the quantitative 'primary qualities', and where by appropriate methods we are to gain knowledge of those qualities, with no limit in principle to its extent and comprehensiveness.

We should be clear that this world-view, although one that (like any other) imposes a structure on reality as experienced, is far from being 'arbitrary' in the sense that an isolated individual can simply choose whether to adhere to it, or perhaps to switch to some other brand. It permeates not merely our conception of the role of mathematics in knowledge, but also what sort of scientific knowledge can and should be obtained. It was explicitly claimed by such seventeenth century prophets as Galileo and Descartes, and implicitly accepted ever since, that this approach to knowledge is not merely quantitatively exact, but also uniquely assured of truth in its results. Other approaches to knowing, ranging from the humanistic, through the imaginative, to the inner-orientated, have all been rejected with varying degrees of severity at different critical points in the development of the scientific philosophy. Now, some three and a half centuries later, the crisis in the philosophy of science, paralleled by the crisis in the policy sciences, becomes one of confidence. Numerical expressions, representing quantities derived by accredited scientists, cannot be guaranteed to protect us against vague, ambiguous, misleading or even vacuous assertions of a scientific appearance. Where then do we find the rock of certainty on which our scientific knowledge is supposed to be based?

Since that world is our paradigm, by its nature not to be questioned or even noticed in ordinary practice, its flaws will be revealed only occasionally; and they can then be dismissed as anomalies, or as mere anecdotes. Those who would enhance awareness of the problems of the dominant paradigm must then show how a previously unquestioned practice has defects (as we have just done). Or we may show how it reveals other significant features when examined critically. For example, we may consider the language used to describe the results of measurements in the world of experience. These are traditionally said to be afflicted by 'error', implying that a perfect experiment would yield a scientifically true value with absolute mathematical precision (This is reminiscent of the naming as 'irrational' by the Pythagoreans of certain magnitudes that broke their rules, such as  $\sqrt{2.}$  Even in sophisticated statistical theory, the crucial terms have a subjective cast, such as in 'confidence' or 'fiducial' (in our work we describe the analogous properties of information as 'reliability', relating to human practice to be sure, but to experience rather than to opinion). And among scientists of many sorts, the old ideal of objective, quantitative certainty has been dominant. Thus, from such dissimilar figures as Einstein and Rutherford, we have the dicta, 'God does not play dice', and 'If your experiment needs statistics you ought to have done a better experiment' (Mackay 1977).

Being a genuine crisis, this one does not manifest itself merely at these two dissimilar areas of experience: abstract philosophical reflection and craft arithmetical practice. The present century has seen the dissolution of many certainties in the mathematical conception of science. The revolutions in physics, particularly quantum mechanics, were explicitly philosophical in part; and similarly was the 'foundations crisis' in mathematics, leading through Gödel's theorems to a radical loss of certainty (Kline 1980).

This erosion of the previously unchallenged epistemological foundations of a scientific world-view has thus proceeded on many fronts. It has been accompanied by an erosion of the moral certainties of science, ever since the industrialization and militarization of scientific research became recognized. As yet there has been no effective presentation of an alternative paradigm, in the Kuhnian sense of a deep scientific revolution. The critical analyses raised in the 1960s (and echoed in Feyerabend's (1975) works) could not have a practical outcome in the absence of a wholesale transformation of society and consciousness. One modest philosophical alternative was suggested in the 1920s by Niels Bohr (Holton 1973), in his famous attempt to resolve the 'dualities' of early quantum physics by means of the essentially Chinese notion of complementarity. This remained a personal, almost idiosyncratic attempt at coherence, for the physicists were able to do quite nicely in making discoveries and inventions of unprecedented power, in spite of totally incoherent basic conceptual structures. Recent attempts to interpret physics in broadly 'oriental' ways have so far remained curiosities of popularized science (Capra 1975, Zukav 1979).

In the NUSAP system we put complementarity to work. Becoming familiar with the notational scheme, through use, entails acceptance of the idea that a bare statement of quantity, in the absence of its qualifying judgements, is scientifically meaningless. To paraphrase the classic formula of the logical positivists of the Vienna Circle (Ayer 1936), the meaning of a quantitative statement is contained in its mode of qualification as much as in its quantifying part. In this respect the NUSAP system makes a contribution towards an alternative approach to the philosophy of Nature.

We may ask, can notations really be so influential as we claim? The history of mathematics shows how they can encapsulate new ideas in such a way as to transform practice. This happened twice in the seventeenth century, first with Descartes' unified conception of algebra, geometry and their relationship, expressed through the symbols  $a, b, c \dots, x, y, z$ . Then Leibniz, with dx and  $\int$ , tamed the infinite in this new 'analysis'. At a less exalted level, the 'arabic numerals' democratized arithmetic in early modern Europe; previously calculation had been the preserve of those who had mastered the abacus, as supplemented by a variety of special tricks. Even when symbols are not designed for calculation, but only for effective representation, they can have a deep influence on a practice and how it is understood; the history of chemical nomenclature and symbolism provides many examples of this (Crosland 1962).

We conceived and developed the NUSAP notational scheme in full awareness of the complex interaction between tools (of which notations are an example), explicit concepts, world-views, and social practice. It is designed as an instrument of analysis and criticism, in an area of practice where such activities have been generally considered to be either unproblematic or even quite unnecessary. To the extent that there has been a mystique of quantities, and that this has been supportive of a mystique of exclusive scientific expertise, the NUSAP system also has functions in the societal aspects of scientific practice. There too, it can enrich the inevitable debates on quantities that enter into policy issues, avoiding the extremes of naivety and cynicism from which participants now have little protection. In that connection too, it can make its contribution to the development of appropriate new conceptions of science.

# Principles of the NUSAP Notational Scheme

The NUSAP notational scheme is a system whereby the various sorts of uncertainty contained in all quantitative information may be expressed concisely and also consistently with existing partial notations. It is designed to be applied to any expression given in the form of numbers or more generalized notations. By its means, nuances of meaning in quantitative statements can be conveyed clearly and economically; and various aspects of the quality of the quantitative information may also be expressed. Users need master only the very simplest skills, and the underlying ideas are familiar to all those with experience of successful practice in any quantitative discipline or craft.

Should it come into standard use, there will develop a more competent general level of criticism of quantitative assertions, among both experts and the interested public. Just as quality control is now recognized as an essential component of industrial production, meriting emphasis and appropriate organizational structures, so we can expect that with the adoption of the NUSAP system, quality control of quantitative statements will eventually become standard practice.

NUSAP was designed with several criteria in mind. In addition to the ordinary properties of a good notation (simplicity, naturalness, flexibility, etc.) it enables the distinction between meaningless and meaningful quantitative statements. Further, it protects against the misleading use of quantitative information by preventing the isolation of the 'quantifying' part of an expression from its 'qualifying' part. All this is accomplished because the notational system can distinguish among three sorts of uncertainty which characterize every quantitative expression. These are: inexactness of measurement and of representation; unreliability of methods, models and theories; and the border between knowledge and ignorance revealed in the history of the quantity.

The NUSAP notational scheme is a 'system' because it is not simply a collection of fixed notations. Rather, it is a set of determinate *categories*, each of which can be filled by particular notations appropriate to the occasion. The names of the five categories (or boxes, or places in a string) make up the acronym NUSAP. Considering the expression as proceeding from left to right, we start with those which are more familiar, the quantifying part of the expression; and conclude with those less familiar, forming the qualifying part of the expression. With such complementary aspects of the expression conveyed in a convenient and standard form, some of the classic dilemmas of subjectivity and objectivity in science can be resolved in ordinary practice.

Considered as a formal structure, NUSAP is more than a convenient array of symbols conveying uncertainties in technical information. It is a 'notational scheme' which provides a general framework so that an unlimited variety of particular notations may be employed unambiguously. It is a string of five positions corresponding to the categories of *numeral, unit, spread, assessment* and *pedigree*. By means of this place-value representation, each category can be expressed simply, without need for its explicit identification (this is a 'scheme' of notations at the most abstract level). For each category, there are many possible sets available for conveying particular desired meanings (thus in *unit* we may have Imperial, CGS, MKS or SI units). Any particular array of such sets, we call a 'notation'. Given such a notation, any particular case of representation will be an 'instance' of the notation.

Such distinctions enable great flexibility and power in the expression of
quantitative information. In this respect it is analogous to the notational system of 'arabic' numerals, where the meaning of a digit depends on its place, thereby enabling a small set of digits to be used for the representation of any possible integer. By means of this flexibility we can escape from the 'vicious circle' of digital representations, whereby even those notations used to qualify an expression are themselves afflicted by pseudo-precision (as '95% confidence limit').

The first category, in the left-to-right order, is numeral. We use this term rather than 'number' as a reminder of the flexibility of the system. The place can be filled by a whole number, a decimal expansion, a fraction, or even a representation of an interval, or a qualitative index. Next is unit, which can be a compound entry, consisting of standard and multiplier. This can be important for the representation of aggregated quantities, such as \$k, or perhaps \$10<sup>12</sup>. The middle category is spread, generalizing the traditional concept of error. Although this is normally expressed in arithmetical form (perhaps by  $\pm$ , % or fn, for 'to within a factor of n') there is a strong qualitative element about it. Spread cannot (except perhaps when given by a calculated statistical measure) be given precisely; it is always an estimate whose own spread is not a meaningful or useful concept. There is a way of qualifying the spread entry; it can be done by assessment, the fourth category in the NUSAP system. This may be seen most familiarly as a generalization of the confidence limits used in statistical practice. Assessment can be relevant in contexts where the problem does not admit of the calculation of confidence limits; and a great variety of notations can be deployed here, ranging from standard percentages, to a simple ordinal scale, such as 'high, medium, low'. The means of arriving at an assessment rating are equally various: it may be calculated statistically; it may be obtained by arithmetical operations from a conventional coding of the last category, *pedigree*; or it may be the result of a personal judgement.

Hitherto the categories have analogues in existing practice, ordinary or statistical; and it is natural to consider the NUSAP notational scheme as an extension and ordering of existing notations. But with the *pedigree* category, a novelty is introduced. By *pedigree* we understand an evaluative history of the production of the quantity being conveyed by the notation. Histories do not normally appear as part of notations; and for this category we have developed abbreviated schemes of analysis and representation. So far there are two, one for 'research information' and the other for 'public-use statistics'. In this paper we shall only introduce the *pedigree* for research information.

We said before that the contents of the *numeral* box need not be ordinary numbers. Thus, if a quantity is known only to within an 'order of magnitude', then an appropriate instance of *numeral* would be E6:. We remark that an instance 1:E6 denotes a determinate quantity, a million, very different from the 'order of a million' conveyed by E6:. (Representations in NUSAP have the boxes in the string separated by a colon; in reading them, we express the colon by 'on'. For example, an instance 1:E6 – where E6 is in the *unit* box – reads '1 on E6').

If one quantity is known only as an interval which lacks any preferred point of likelihood or of symmetry, then this should be the entry in the numeral place. Thus we could have (a, b): for an ordinary interval,  $(\geq a)$ : for an openended one. In the *numeral* place we may also find expressions of yet more general mathematical structures as numbers of a finite set representing an ordinal scale (as in much of social research), or numbers representing indices with a purely artefactual arithmetic. An extreme example of an ordinal scale with a qualitative notation of *numeral*, which is of direct practical use, is that for Geiger counter readings, such as 'click', 'chatter' and 'buzz'.

By unit we understand the base of the physical and mathematical operations represented in the numeral position. We distinguish two components of the unit. There is the standard, the common or generally used unit of the relevant operations; and the multiplier, relating to the standard to the particular unit involved in the expression. Thus we frequently see £342M, where the unit £M (with £ as standard and M as multiplier) is the actual basis of the calculations reported, as distinct from the £.p of strict accountancy practice. The meaning of the pair standard-multiplier may of course vary with context; thus kg is now a fundamental unit in the SI system, in which strictly speaking, 1 g should be written as 1 mkg. These two quantifying categories enable a refined description of topologies and scales of measurement.

Good practice in notation includes the indication of the *spread* of a quantity (which may also be called error or imprecision). For this the significant digits convention is common, as well as such statistical measures as standard deviation. In the case of highly inexact quantities, the *spread* may be conveyed by 'to within a factor of n'.

We can illustrate the application of NUSAP on some simple examples, where existing representations are inadequate. Suppose that we start with 'five million', and we add some smaller quantity. If it is very small, such as, say, 180, then the sum is normally understood still to be five million, since the latter quantity is not significant in the context. Writing the sum formally, we have  $5\ 000\ 000\ +\ 180\ =\ 5\ 000\ 000$ . In this sum, the last three zeros are interpreted as fillers rather than true digits; and so we use an artefactual arithmetic, adopting implicit conventions for the neglect of certain digits, just as in rounded-off calculations. But if the second addendum is 180\ 000\ it is not clear from the uninterpreted sum 5\ 000\ 000\ +\ 180\ 000\ just where the counter digits end and the filler digits begin. Only from the context can we know whether to apply a natural or artefactual arithmetic. A notation like  $5 \times 10^6$  may help, but even that is not conclusive.

Another useful example from ordinary practice is counting in dozens; this shows more clearly the influence of the process of production of the datum, since in this base there is no ambiguity between counter and filler digits. Thus 'eggs' will have, as a typical instance,  $4\frac{1}{2}$ :doz-eggs: rather than 54:eggs. This

example exhibits the phenomenon of pseudo-precision of a numeral in digits, when the process has consisted of counts by dozens and half-dozen.

In the NUSAP notational scheme, we can express five million in the alternative forms, 5:10<sup>6</sup>, 5:M or 5:E6. Here it is explicit that the unit is millions. Although some ambiguity remains, it can be resolved by the entry in the spread position. But it is quite clear that 5:M + 180 = 5:M is the correct sum, unless there is an explicit note to the contrary in the spread position. There is no need for an artefactual arithmetic, with all its ambiguities,

NUSAP can also convey some shades of meaning that may be important in particular contexts. Thus five million may be better represented as 50:E5 or  $\frac{1}{2}$ :E7, denoting different sorts of operations in the different aggregated units. We note that the use of fractions in the numeral position enables us to express the meaning of a rough cutting of an aggregated unit; thus a 'third of a million' is represented better as  $\frac{1}{3}$ : M rather than  $0.33 \times 10^6$ . It can be considered an advantage of a notation that a user can represent, and even calculate with, an instance which expresses a perfectly clear statement of a quantity that previously needed a verbal form.

When representing measurements, we must distinguish between the multiplier and the standard which make up the unit. For an example,  $5 \times 10^3$ : g represents a count of 5000 grams; and this expression implies that the measuring operations were performed in the old CGS system. Turning to 5:10<sup>3</sup> g, we are still in CGS, now operating in 'kilo' grams, of which there are 5. If we now write 5:kg, this is the expression of a count of 5 in the MKS system, or SI units, where kg is fundamental. Another example of the same sort exhibits a new feature; 5:g is clearly in CGS, while  $5:10^{-3}$  kg tells us that we have SI with a scaling in thousandths of a kg. We note that here the multiplier represents the scaling of the measuring instrument.

For an example of the spread category we return to aggregated counting, with the above-mentioned ambiguous case of 180 000 added to five million. It may be that the larger quantity here has such inexactness that even a tenth of it is insignificant. This could happen if it is part of a sum with much larger quantities, such as 32:E6: and 155:E6:. Then the spread would be understood to be as large as E6, the unit, and therefore the 180 000 or 0.18:E6 would be meaningless. In this way, the notation represents the practical situation of the swamping of a much smaller quantity in a sum; to be completely explicit we may express this as 5:E6:E6. The spread E6 indicates that no interpolation within the scaling has been done; equivalently, every quantity in this sum has an inexactness interval which is E6 in length.

Ey the use of this notation, the meaninglessness of a quantitative expression can be clearly exhibited. For example, where both unit and spread are E6, the quantity 180 000 would be expressed as 0.18:E6:E6. The 0.18 would be insignificant and the expression is vacuous. By contrast, if the 180 000 is being added to 'five million', and the spread is understood to be 0.1E6, then 0.18:E6:0.1E6 would be naturally rounded up to 0.2:E6:0.1E6; and this is a proper quantitative expression. The sums might read as follows.

First, 32:E6:E6 + 155:E6:E6 + 5:E6:E6 + 0.18:E6:E6 = 192:E6:E6 where the 0.18 is suppressed, as being meaningless in this context. If, on the other hand, our summands are, say, 3:E6, 7:E6 and 5:E6, then since these are small integers, it is likely (unless indicated otherwise) that the spread is less than E6, perhaps 0.1E6. In this case we may write 3:E6:0.1E6 + 7:E6:0.1E6 + 5:E6:0.1E6 + 0.18:E6:0.1E6 = 15.2:E6:0.1E6, where we have rounded up 0.18 to 0.2.

The notation enables us to identify pseudo-precision in measurements, even when this is forced by an accepted scaling. Thus in the SI, where 'cm' are officially suppressed, measurements which were formerly done in inches, with spread of  $\pm \frac{1}{2}$  inch, are now frequently expressed in 'mm' to the nearest ten. Thus 'five feet' will be rendered as 1 520 mm. In the NUSAP system, this would be properly represented as 152:10 mm. In this way we retain the standard required by the SI system, but modify by the multiplier 10, to express the practical scale of operation, equivalent to the illegal 'cm'. A somewhat less rigorous representation makes use of the spread category; we can keep the spurious last digit required by the SI, but show that in practice it is not a counter. Thus we would write 1520:mm:10, reminding the user that there is an effective 'spread' in the number as recorded.

Strongly inexact quantities are sometimes expressed 'to within a factor of n', such as '5  $\times$  10<sup>6</sup> to within a factor of 10'. The convention indicates multiplicative intervals above and below the given quantity; thus the given quantity here may lie between  $0.5 \times 10^6$  and  $50 \times 10^6$ . In the notation we write 5:E6:f10, meaning 0.5:E6 < 5:E6:f10 < 50:E6. By means of such notations it is possible to convey quantities of the sort characterized by 'the first law of astrophysics': 1 = 10. We can also express inexactness given in proportional terms; for example '5  $\times$  10<sup>6</sup> with a proportional error of 15%' is represented as 5:E6:15% or as 5:E6:[15 in E2].

In the policy context, fractions less than unity, expressed as percentages, are frequently used to indicate the division of some aggregate. The inexactness of such estimates is extremely difficult to represent in a compact notation, and a misleading impression of precision is all too often conveyed. Thus '40%' may mean 'less than half but more than one-third' or perhaps 'less than half but more than one-quarter'. These inexact estimates may be represented as  $\frac{1}{3} \cdot 1 \le \frac{1}{2}$  and  $\frac{1}{4} \cdot 1 \le \frac{1}{2}$  respectively. Another way of expressing such estimates involves using the variable x. If there is some unit U, we may have  $x: U: \frac{1}{3} < \frac{1}{2}$  or  $x: U: \frac{1}{4} < \frac{1}{2}$ . By this means, one can express quite fine distinctions among inexact estimates of fractions, avoiding the pseudo-precision of a two-digit percentage. The use of the variable x in the numeral place enables us to express clearly that the means for the production of the quantity do not provide us with information for distinguishing among numerical values. The class to which all the relevant values belong is represented in spread. We can refer to this as an 'indifference class', in the sense that no one numerical value can legitimately be taken as a representative of the class in preference to any other. In symbols, we write the general case as x: U:S.

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The assessment category expresses the reliability of the information, generalizing not only the confidence limits of classic statistics, but also those of Bayesian statistics, interpreted as 'degree of belief' (Keynes 1921) or 'betting odds' (Savage 1954). Such formally defined measures are properly applicable only in special cases, and are not free of conceptual problems of their own. The assessment category is not to be formalized in the logical sense, but is designed to convey judgements of reliability in a convenient form. Where statistical notations are familiar and appropriate, they may be freely used. Otherwise, a more qualitative notation, such as, for example, an ordinal scale, should be adopted. Thus, we may have the set (*Total*, *High*, *Medium*, *Low*, *None*), perhaps codified as (4, 3, 2, 1, 0) to convey this kind of judgement. As in the cases of numeral, unit and spread, a great variety of notations are available for assessment.

A familiar case from scientific research is that of a number which historically belongs to a sequence of experimentally derived results describing the 'same' physical quantity. It is well known that elements of such a sequence may well jump about by amounts far exceeding the *spread* of any of them; this is described as systematic error as distinct from random error. A reader of technical literature may estimate a numerical entry for the *assessment* place, by an examination of the published versions of such a variable 'physical constant'. With *spread* representing average, a sample case might read  $4.32:\mu U: \pm 0.17: \pm 0.3:$ .

In traditional statistical practice, the *assessment* (or confidence limits) is closely associated with the *spread* (or variance). Unfortunately, this association tends to conceal the radical difference between the two categories, and to inhibit the understanding of either. When we generalize *assessment* from the simplest notion of reliability, the independence of the two categories becomes apparent. For example, consider a statistical distribution where we are interested in estimating the 95th upper percentile, or the top 5%. The entry of the *numeral* place is then qualified by the expression '% 95' in the *assessment* place, the order being inverted deliberately to distinguish this from the more traditional '95%' confidence limit. In such a case, the *spread* will depend on the number of trials or of simulations of the same process. So, if we are comparing the results of two different experiments involving different numbers of trials or simulations (as for instance obtaining the top 5% of a distribution of experimental coin-toss results), we can have *spreads* varying with the size of the sample while the *assessment* entry is always '%95'.

Another illustrative example is of a case where the *spread* box is empty, but where a definite (though qualitative) *assessment* is appropriate. This can happen in a 'back-of-envelope' calculation, where the basic *unit* is expressed through a *numeral* entry of a small integer number. In such cases, *spread* is meaningless; but the calculation can be qualified by, say, '*Upper Limit*' (or *U*) in the *assessment* box. This is not an ordinary sort of reliability as calculated in traditional statistical practice; but it provides the user of the information with an appropriate interpretation for reliable use in practice. The flexibility of the system is further enhanced by the use of combinations of entries in boxes to convey nuances of meaning. A particularly direct case of this is in a trade-off between 'strengths' of entries in the *spread* and assessment places. This also generalizes statistical practice; so that we may describe a distribution more tightly by its range over the 25% - 75% percentiles or more broadly over the 10% - 90% percentiles. This translates directly into a lower *spread* with lower assessment, or higher in both categories. In NUSAP, this can be expressed by  $\mu$  (the mean in the N place),  $S_Q$  and  $S_D$  (the interquartile and interdecile ranges in S). The notations would read  $\mu: U:S_Q:50\%$ : and  $\mu: U:S_D:80\%$ :. For an example, we imagine a distribution with a mean of 46,  $S_Q = 12$ , and  $S_D = 20$ . Alternative representations would be 46::12:50% and 46::20:80%; the percentages in the assessment box relate to the amount of the total distribution represented in the *spread* place.

total distribution representation are directly involved in a policy process, the When uncertain quantities are directly involved in a policy process, the flexibility of the system can be very useful indeed. An illuminating example is cited by Mosteller (1977): estimation of the number of American men who emigrated during the Vietnam War. These ranged from 2.5k through 30k to 70k or even 100k, though the higher figures were less reliable. If the absolute number is not critical for policy purposes, then a convenient NUSAP expression would be  $\geq 3:E4::Good:$ . With a one-sided interval in *numeral*, it is appropriate to leave *spread* empty. If the lower bound on the estimate is very sensitive for policy, the *numeral* entry could be reduced; and the expression reads  $\geq 2\frac{1}{2}:E4: + 20\%$ :High:. In this way, a policy-maker is told that she/he is unlikely to go wrong in acting on the basis of an estimate in the range  $2\frac{1}{2}$  to 3 on a *unit* E4.

These examples show how the system can be used to provide alternative communications, each valid in its own right, for a single statistical result. Each version focuses attention on a different aspect of the distribution, corresponding to different needs of users.

# Pedigree for Research Information

In the NUSAP notational scheme, the most qualifying category, located in the far right position, is *pedigree*. This expresses the most extreme of the various sorts of uncertainty conveyed by the notation: its border with ignorance. The previously discussed categories can be seen as a preparation for the introduction of this one. Thus, *spread*, expressing the inexactness of quantities, served as a reminder that a quantitative expression is not 'clear and distinct'. Even if there is some realm of ideal mathematical entities (such as lines without breadth), represented in necessarily true mathematical statements (such as exp  $(\pi i) + 1 = 0$ ), the world of empirical objects and their measurements always involves 'more or less', or 'tolerances', about quantities possessing a fringe of vagueness. In that sense, the specification of an object in respect of its

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quantitative attributes implicates the rest of the world, things other than our particular object of attention, as it shades into them.

This can be seen clearly by reflection on the normal practice of indicating *spread* or the misnamed 'error'. When we write  $4.32 \pm 0.05$ , that extra term must surely be other than perfectly precise. How is its imprecision to be conveyed? Is 0.05 drastically different from 0.04 or from 0.06? In normal practice, we simply record an *assessment* of confidence, which is a very different kind of judgement. We do not ordinarily attempt a 'spread of the spread', for many practical reasons; and also because if we were to iterate once, then why not twice or more? Hence we satisfy ourselves with an informal, tacit convention on the formal, misleadingly precise representation. Once aware of this we see how the simplest and most common of conventions for expression of the lack of perfect exactness in quantities leads us into paradoxes of infinite-regress. The border between the measured thing and its environment, or between our knowledge and our ignorance, can never be specified precisely.

Thus, our quantitative knowledge can never be fully exact or perfect, even in itself. When considered in the context of its usefulness, further qualification is necessary. Even a simple assertion carries an implicit claim to be true; and therefore also to be completely reliable in use under appropriate conditions. But every statement of fact needs some sort of *assessment*, since it is impossible to achieve perfect reliability any more than perfect truth. As we have seen, technical statements involving probability and statistics include notations for the expression of their confidence limits, which can be interpreted as the odds against a 'failure in use' of the information. (This interpretation is closer to practice, and also less paradoxical, than that of 'confidence in its truth'.)

Of the three sorts of uncertainty expressed in NUSAP, ignorance is the most novel and complex, and also the most difficult to convey explicitly. In ordinary scientific practice, ignorance of a special sort is vital to the enterprise: the interesting problems which can be stated, but whose solubility is not assured. In this sense, science deals with controllable ignorance; successful science involves, in the classic formula, 'the art of the soluble'. Not all ignorance comes in such convenient packages; in contemporary science/technology policy, the most important problems are frequently those of 'trans-science' (Weinberg 1972): problems which can be stated, whose solution can be conceived, but which are unfeasible in practice because of scale or costs. Such trans-science problems may involve ignorance that is quite important in the policy realm, such as when decisions must be taken before there is any prospect of the relevant information being produced.

In the *pedigree* category, we do not characterize information (or ignorance) in technical detail. Rather we exhibit the mode of production of the quantitative information being represented, through an evaluative history. This defines the border with ignorance, through a display of what more powerful means were *not* deployed in the production of the information. Thus, if we report a 'computation model' as the theoretical structure for the information, that implies that there was no 'theoretically based model' available, and still less 'tested theories', involved in the work. Thus, in each phase we are comparing existing results with conceivable alternatives of greater strength. As research fields develop through practice, early pioneering efforts may be superseded by stronger work in such a fashion as this. Hence we may imagine the choice of modes in a *pedigree* matrix as indicating the border between what is currently feasible and accepted as known, and that which is unfeasible and unknown.

In this respect a *pedigree* code is analogous to the statement of a proved theorem in mathematics. Such a statement includes more than the result; equally important are the conditions under which it holds. As to other possible conditions, there is ignorance; and the statement of a theorem constitutes an implicit challenge to explore that ignorance. Although quantitative information is not 'true' in the same sense as a mathematical result, there is this analogous border between knowledge and ignorance in the specification of its production.

We may describe the three qualifying categories of NUSAP in terms of the various contexts to which they apply. In practice, they operate in interaction, so that no one is truly prior. By abstracting somewhat we may speak of contexts of production of information, of its communication and of its use. These correspond to the categories of *pedigree*, *spread* and *assessment* respectively. In production, the border with ignorance is shown by the limitations of each chosen mode in the *pedigree* matrix. In communication, the 'unknown' is that into which the stated quantity blends by means of the (non-iterated) *spread* term. In use, the implied testing by future experience, revealing possible ignorance, is conveyed by the reliability rating of *assessment*. The order in which we have discussed these categories is not the same as that in NUSAP; in the scheme we adhere more closely to existing usages, where a notation starts with the quantifying part and proceeds towards the more qualifying.

For the evaluative history of the quantity as recorded in the *pedigree* matrix, we analyse the process into four phases. These indicate, by their various modes, the strength of the different constituents of quantitative information resulting from a research process. We have theoretical, empirical and social phases, the last being split into two in order to encompass all the sorts of evaluation that we may want to provide. In order, the phases are: *Theoretical Structures*, *Data Input*, *Peer Acceptance* and *Colleague Consensus*. The *pedigree* matrix is displayed as follows (with corresponding numerical codes and abbreviations):

Discussing the separate phases in order, we have first *Theoretical Structures*. Following the traditional scientific methodology, we accept that the strongest mode here is *Established Theory*. The general term 'established' includes such modalities as: tested and corroborated; or theoretically articulated and coherent with other accepted theories. Thus Einstein's General Theory of Relativity was in this sense already 'established' when it was tested by the famous astronomical experiment of 1919. When the theoretical

|        | Theoretical Structures  | Data Input   | Peer Acceptance                | Colle  |
|--------|---|--|--------------------------------|--|
| 4<br>3 | Established<br>Theory (TH)<br>Theoretically<br>based Model<br>(Th.bM) | Experimental<br>Data (Exp)<br>Historic/<br>Field Data<br>(H/F) | Total<br>(Tot)<br>High<br>(Hi) | All but cranks<br>(All)<br>All but rebels<br>(All-)    |
| 2      | Computation<br>Model (Mod)<br>Statistical<br>Processing<br>(St)       | Calculated<br>Data (Calc)<br>Educated<br>Guesses<br>(Ed.G)     | Medium<br>(Med)<br>Low<br>(Lo) | Competing Schools<br>(Sch)<br>Embryonic Field<br>(Emb) |
|        | Definitions<br>(Def)  | Uneducated<br>Guesses (Gues)                                   | None<br>(Non)                  | No Opinion<br>(No-O)                                   |

component lacks such strength, and is perhaps rudimentary or speculative, then its constructs must be considered as in a 'model', but one which is theoretically based; we have then the mode *Theoretically Based Models*. Although still involved in explanation, such a model makes no effective claim to verisimilitude with respect to reality. In this latter respect it is similar to a *Computation Model* which is some sort of representation of the elements of a mathematical system by which outputs are calculated from inputs. In such a case, there is no serious theoretical articulation of its constructs; the function is purely that of prediction. Such a mode is particularly common in the mathematical behavioural sciences; a well-known example is IQ. This mode, *Computation Model*, characterizes the use of high-speed computers for simulations where real experiments are difficult or expensive.

Important research can exist where neither articulated constructs nor elaborated calculations are present; this is the case in classic inductive science. Then, with techniques varying from simple comparisons (formalizing J.S. Mill's Canons of Induction) through to very sophisticated statistical transformations, we have *Statistical Processing*. Such forms of *Theoretical Structure* can provide no explanation and only limited prediction; but used in exploratory phases of research, they can yield interesting hypotheses for study. Epidemiological work of all sorts, leading to identification of likely causes of known ill-effects, is a good example of this mode. Finally, we have those situations where data which are gathered and analysed are structured only by working *Definitions* that are operationalized through standard routines. This will be the case with field data, frequently destined for public-use statistics. A *pedigree* for public-use statistics has been developed by the authors but it is not discussed in this paper.

The normative ordering among these modes is clear; the higher generally includes the lower as part of their contents. But this does not imply judgements on craftsmanship, effectiveness, or on the quality of the investigators or of a field. We do not share in the traditional judgement that all science should be like physics. However, if (in its present state of development) a field can produce only relatively weak results (as gauged by the modes of this scale), that should be an occasion neither for shame nor for concealment.

The other phase deriving from traditional scientific methodology is called *Data Input*. We use this name rather than 'empirical', to include certain inputs (quite common in policy-related research), whose relation to controlled experience may be tenuous or even non-existent. Starting again with the classical and strongest mode, we have *Experimental Data*. Not so strong, our next entry is *Historic/Field Data*; data of this sort are 'accidental' in the sense of being taken as they occur, and lacking tight controls in production and/or strict reproducibility. *Historic Data* are those that were accumulated in the past, out of the control of the present study; *Field Data* are produced by large-scale procedures of collection and analysis.

Historic/Field Data have at least the strength of a relatively straightforward structure, so that its possible errors and deficiencies can be identified. But sometimes Data Inputs are derived from a great variety of empirical sources, and are processed and synthetized by different means, not all standardized or reproducible. The numbers are then themselves 'hypothetical', depending on untested assumptions and procedures. Even to estimate the spread and assessment in such cases may be quite difficult. Hence we assign Calculated Data to a weaker point in the scale even than the Historic/Field Data mode.

Traditionally, the last mode discussed would have been considered the weakest in a scientific study. But with the emergence of policy problems calling for data inputs regardless of their empirical strength, formalized techniques were created whereby opinion could be disciplined so as to provide a reasonable facsimile of facts. Such were subjective probabilities, Bayesian statistics, and other ways of eliciting quantitative estimates from experts. These we call *Educated Guesses*. Sometimes even such a mode is absent; *guesses* can be simply uneducated, and yet accepted as data, hypotheses or even facts, whichever seems plausible. In this respect, *Data Inputs* in modern times have come a long way from the relative certainties of the classical methodological framework for science.

The social aspects of the *pedigree* are here given in two phases: *Peer* Acceptance relates to the particular information under evaluation; and Colleague Consensus describes that aspect of the field in relation to the particular problem area. These are the phases to which users (and those who advise them) could turn first, for preliminary evaluations of possible effectiveness of the technical information. Thus, if there is weak Colleague Consensus and a research field is seriously divided (with Competing Schools or perhaps only Embryonic) then there will be no security in any piece of quantitative information (Funtowicz and Ravetz 1984). Even the sampling of expert opinions, to obtain Educated Guesses, can lead to a bimodal distribution or worse; from this the policy-maker learns the important lesson that scientific ignorance still dominates the problem. Stronger Colleague Consensus, as with All but rebels or All but cranks, may well be time-bound. Since, as T.H. Huxley said: 'It is

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the customary fate of new theories to begin as heresies and to end as superstitions' (Mackay 1977), who is a 'rebel' or even a 'crank' depends on circumstances. There is a real distinction between the two cases; rebels have some standing among their colleagues, whereas cranks have none.

At the other extreme from scientific orthodoxy, we have the mode  $N_0$  *Opinion*, where there is simply no cognitive framework or social network in which the proffered information can make any sense when it appears. This may be from its apparent lack of substance or of interest, or both.

Once we have an appreciation of the context in which peers can receive and evaluate a piece of information, it is useful to characterize that process. The modes of *Peer Acceptance* range in linear order from *Total* to *None*. It is important to realize that the significance of any given degree of *Peer Acceptance* depends critically on the state of *Colleague Consensus*. Thus, if there is a strong general *consensus* and weak *acceptance*, the information must be judged as of low quality of craftmanship (given trust in the general competence of the field). But if *consensus* is as weak as *acceptance*, even such an adverse judgement is not proper; and ignorance rules again. The degree to which consensus can be weak, even in 'matured' scientific fields, is generally underestimated quite seriously by outsiders. Hence low acceptance is liable to be interpreted in a misleading fashion, as a well-founded adverse judgement on the technical information and by extension on its author as well. We have split the 'social' phase into these two parts, partly to avoid such errors as this.

We now discuss various instances of quantitative information that were important in the development of science, and which illustrate significant features of our *pedigree* category.

Not all quantitative information is appreciated on its first publication; the classic example is Mendel's simple arithmetic ratios between frequencies of different sorts of hybrid peas. For the first thirty years after its publication, the pedigree was, as seen retrospectively by historians: (Th. bM, H/F, Non. No-O) or (3, 3, 0, 0). Of course, any contemporary who might have scanned Mendel's paper would not have been so complimentary on the cognitive side. A (reconstructed) pedigree code for that period would be (St, Calc, Non, No-O) or (1, 2, 0, 0). The *Calculated* mode conveys the suspicion that the simple ratios were the result of a coincidence or of 'massaged' data. In the earlier twentieth century, the rediscovery of Mendel changed the pedigree to (Th. bM, H/F, Tot, All) or (3, 3, 4, 4). With the further development of genetics, the ratios themselves are strengthened to have a *pedigree* (Th, Exp, Tot, All) or (4, 4, 4, 4). But greater sophistication in statistics and its application to experimental design led to a scrutiny of the aggregated numbers by R.A. Fisher, who found them 'too good to be true'; and so the modern historians' judgement of Mendel's own work in his own time now has *pedigree* (Th. bM, Calc, Non, No-O) or (3, 2, 0, 0) (Olby 1966).

A sort of inverse example was provided by T.S. Kuhn (1961) in his seminal essay on measurement in science. This was an experimental value for a constant of crucial importance in the caloric theory of gases: the ratio of the two sorts of specific heat. The setting for the production of this number was quite dramatic: the Laplacian Theory of Gases could explain the experimentally known velocity of sound in air, if (and only if) the constant in question had a certain predicted value. The *Academie des Sciences* devoted its annual essay award competition to this topic in 1819; and the desired value was duly obtained by Delaroche and Berard, whose work won the prize. All was perfect; and here we have a *pedigree* (Th, Exp, Tot, All-) or (4, 4, 4, 3), the only reservation being among the nascent scientific/political opposition to the Laplace school. Unfortunately, the result was simply incorrect; and its background theory became discredited for many reasons. A retrospective *pedigree* for the result, a decade on, could be (Th. bM, Calc, Non, All-) or (3, 2, 0, 3); here the *Colleague Consensus* embraces the victorious anti-Laplacian party, comprising nearly all save the lonely disciple Poisson (Fox 1974).

There two examples of the rise and fall of *pedigree* ratings for quantitative information provide a warning that the evaluation of scientific results is a matter of judgement, which can change drastically. What is effectively scientific knowledge at any one time is very much liable to subsequent revision by the wisdom of hindsight. The reliability of quantitative information in practice does not require it to be continuously confirmed and corroborated. In this respect it can be like theories which, in spite of being superseded or perhaps refuted are still reliable in particular contexts of use ('caloric' being one good example; Newtonian mechanics is another). The *pedigree* coding, by analysing the different phases of the history of the production of the relevant quantity, can assist in the description of such changes, and perhaps thereby also contribute to the resolution of the philosophical problems of such 'fallible

knowledge'. Use of the *pedigree* code will also help to clarify relations between providers and users of technical information. Frequently there is a clash of interests and perceptions. Users want unambiguous and certain facts (of the sort science traditionally promises) as inputs to their decisions. This would relieve them of the burdens of evaluations of inputs and of responsibility for decisions that turn out to be 'wrong'. Typically, the world still expects science to define a 'safe limit' for toxicants of all sorts. The scientists, however, are keenly aware of the imperfections of their offerings in such contexts (unless they are partisan experts in a dispute). Their interest is to hedge their statements with disclaimers and alternatives. With the *pedigree* evaluation, there is a means whereby the most radical uncertainties can be clearly expressed, and then form part of a reasoned discussion of the reliability of the available quantitative information.

#### Conclusion

We have indicated how NUSAP can contribute to the resolution of the two urgent problems in the methodology of natural science. In epistemology the

#### **Qualified Quantities**

problem is effectively transformed away from the need for a logical structure. independent of human judgement, whereby uncertainty and ignorance can be conquered. With the notational scheme, these complementary aspects of our knowledge are exhibited in a coherent form. Thus, the experience of successful practice in the quantitative sciences is codified; and the management of the uncertainties becomes a definable task.

In those areas of policy-related research where severe uncertainty prevails, NUSAP provides a standardized means for communications. Debates on the necessarily imperfect and contentious quantities that are invoked will then have a structure and a discipline. The acceptance of NUSAP will also enhance clarity of understanding among those who provide quantitative information and contribute to the improvement of quality control. In such ways, it will increase familiarity with 'uncertain quantities' among all who use them; and in that way enable a shift in 'scientific common sense', so that a more mature understanding of the scope and limits of science may be achieved.

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# Usable Knowledge, Usable Ignorance: Incomplete Science with Policy Implications

For centuries the dominant theme of our science has been taken from Francis Bacon's aphorism 'Knowledge and power meet in one'. I need not relate here the transformation of humanity's material culture that science has brought about, nor the enhancement of human life, social, moral, and spiritual, that this has enabled through the conquest of the traditional curse of poverty (in at least the more fortunate parts of the world). But now we face a new, unprecedented problem. Along with its great promises, science (mainly through high technology) now presents grave threats. We all know about nuclear (and also chemical and biological) weapons, and about the menaces of acid rain, toxic wastes, the greenhouse effect, and perhaps also the reemergence of hostile species, artificially selected for virulence by our imprudent use of drugs and pesticides. It would be comforting to believe that each problem could be solved by a combination of more scientific research of the appropriate sort, together with more goodwill and determination in the political and technological spheres. Doubtless, these are necessary, but the question remains: Are they sufficient? The record of the first round of an engagement with these biospheric threats is not encouraging. For example, we do not yet know when, how, or even whether global temperatures will be influenced by the new substances being added to the atmosphere. This is why, we believe, a novel approach is called for if our science-based civilization is to solve these problems that are so largely of our own making.

Indeed, we may see the issue not merely in terms of science, but of our industrialized civilization as a whole, since it has science as the basis of its definition, the science defined by the motto of Francis Bacon. And the problem that faces us is that the sum of knowledge and power is now revealed to be insufficient for the preservation of civilization. We need something else as well, perhaps best called 'control'. This is more than a mere union of the first two elements, for it involves goals, and hence values, and also a historical dimension, including both the remembered past and the unknowable future.

Can our civilization enrich its traditional knowledge and power with this

new element of control? If not, the outlook is grim. There are always sufficient pressures that favour short-term expedients to solve this or that problem in technology or welfare, so that the evaluative concerns and long-range perspectives necessary for control will, on their own, lose every time. That is what has been happening, almost uniformly, in our civilization until quite recently. Only in the last few decades have scientists become aware that control does not occur as an automatic by-product of knowledge and power. Our awareness has increased rapidly, but so have the problems. And we are still in the early stages of defining the sort of science that is appropriate to this new function.

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We might for a moment step back and look at this industrialized civilization of ours. It is now about half a millennium since the start of the Renaissance and the expansion of Europe. That is roughly the standard period of flourishing for previous civilizations; will ours prove more resilient to its own characteristic environmental problems? It seems likely that some of the ancient 'fertile crescent' cultures declined because of excessive irrigation, and in various ways the Romans consumed great quantities of lead. What would our be our auto-intoxicant of choice?

In some ways our material culture is really rather brittle; our high technology and sophisticated economies depend quite crucially on extraordinary levels of quality control in technology and on highly stable social institutions. Whether these could absorb a really massive environmental shock is open to question. The real resilience of our civilization may lie not so much in its developed hardware and institutions, as in its capacity for rapid adaptation and change. It has, after all, continued to grow and flourish through several unprecedented revolutions: one in common-sense understanding of Nature in the seventeenth century, another in the material basis of production in the eighteenth and nineteenth centuries, and yet another in the organization of society over much of the world in the twentieth century. Perhaps it could be that the latest challenge to this civilization, resulting from the environmental consequences of our science-based technology, will be met by the creation of a new, appropriate sort of science. We can only hope so, and do our best to make it happen.

What could such a new, appropriate sort of science be? Isn't science just science? In some ways, yes, but in others it is already differentiated. We are all familiar with the differences between pure or basic research on the one hand, and applied or R & D on the other. In spite of the many points of contact and overlap, they do have distinct functions, criteria of quality, social institutions, and etiquette and ethics. To try to run an industrial laboratory as if it were within the teaching and scholarship context of a university would be to invite a fiasco; and equally so in reverse. Now we face the task of creating a style of science appropriate to this novel and urgent task of coping with biospheric problems. Of course, there are many different institutions doing research with just this end in view. Sometimes they are successful, but success is more common when they have a problem where the conditions for success can be defined and met, and where the input from research is straightforward. To the

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extent that the problem becomes diffuse in its boundaries (geographically, or across effects and causes), entrained in cross-currents of politics and special interests, and/or scientifically refractory, then traditional styles of research, either academic or industrial or any mix of the two, reveal their in the

either academic or industrial or any mix of the two, reveal their inadequacy. This is the lesson of the great biosphere problems of the last decade. Faced with problems not of its choosing (though indirectly of its making), science, which is the driving force and ornament of our civilization, could not deliver the solutions. When asked by policy-makers, 'What will happen, and when?' the scientists must, in all honesty, reply in most cases, 'We don't know, and we won't know, certainly not in time for your next decisions'.

If this is the best that science can do, and it seems likely to be so for an increasing number of important issues, then the outlooks for effective policymaking and for the credibility of science as a cornerstone of our civilization are not good. Yet, I believe, so long as scientists try to respond as if they face simple policy questions determined by simple factual inputs, the situation cannot improve.

But what else can scientists do except provide facts for policy? I hope that we can define the task in new terms, more appropriate to our situation, and *that* is an important component of the goal of this project.

My work on this project has already involved me in an intellectual adventure; recasting my earlier ideas about science had led me into paradox and apparent contradiction. Rather than leading colleagues into them by gentle and easy stages, I have chosen to exhibit them boldly in the title. We all , know what is 'usable knowledge', although it turns out to be far from straightforward in practice (Lindblom and Cohen 1979). But 'usable ignorance?' Is this some sort of Zen riddle? I hope not. But if we are to cope successfully with the enormous problems that now confront us, some of our ideas about science and its applications will have to change. The most basic of these is the assumption that science can indeed be useful for policy, but if and only if it is natural and effective, and can provide 'the facts' unequivocally. So long as it seemed that those facts would be always forthcoming on demand, this assumption was harmless. But now we must cope with the imperfections of science, with radical uncertainty, and even with ignorance, in forming policy decision for the biosphere. Do we merely turn away from such problems as beneath the dignity of scientists, or do we learn somehow to make even our own ignorance usable in these new conditions? In this exploratory essay, I hope to show how even this paradox might be resolved, and in a way that is fruitful for us all.

# Images of Science, Old and New

If I am correct in believing that our inherited conception of science is inappropriate for the new tasks of control of these apparently intractable biospheric problems, then we shall all have to go through a learning experience, myself included. Scientists, scholars and policy-makers will need to open up and share their genuine but limited insights of science, so that a common understanding, enriched and enhanced by dialogue, can emerge. My present task is to call attention to the problem, and to indicate my personal, rough, provisional guidelines toward a method.

# Insoluble Problems

I may well seem to be speaking in paradoxes, so I will suggest a question that may illuminate the problem. For background, let us start with the historical datum that in the year 1984 we cannot predict when, or even whether, the Earth's mean temperature will rise by 2°C due to an increasing  $CO_2$  content in the atmosphere. Yet this prediction can be cast as a scientific problem, for which there are both empirical data and theoretical models. Why these are inadequate is a question I must defer; but we can (I hope) all agree that here is a scientific problem that cannot be solved, either now or in any planned future. And this is only an example of a class that is growing rapidly in number and in urgency.

I believe that such problems are still very unfamiliar things, for our personal training in science progressed from certainties to uncertainties without any explicit, officially recognized markers along the path. Almost all the facts learned as students were uncontested and incontestable; only during research did we discover that scientific results can vary in quality; later we may have come across scientific problems that could not be solved; and only through participation in the governing of science do we learn of choices and their criteria.

Now I can put the question, for each of us to answer for himself or herself: When, at what stage of my career, did I become aware of the existence of scientific problems that could not be solved? My personal answer is not too difficult. As a philosophically minded mathematician, early in my postgraduate studies, I learned of classic mathematical problems and conjectures that have defied solution for decades or even centuries. I have reason to believe that my experience was exceptional for a scientist. Certainly, I have never seen an examination in a science subject that assumed other than that every problem has one and only one correct solution. Some such problems may well exist, but they will be a tiny minority. Similarly, research students may learn of the tentativeness of solutions, the plasticity of concepts, and the unreliability of facts in the literature. But this is a form of insiders' knowledge, not purveyed to a lay public, nor even much discussed in scholarly analyses of science.

Indeed, it is scarcely a decade since insoluble scientific problems have become 'news that's fit to print'. Alvin Weinberg (1972) brought them into recognition with the term 'trans-science'. Were these a new phenomenon of the troubled 1960s?—that period when environmentalists began to raise the impossible demand that science prove the impossibility of harm from any and all industrial processes and effluents. No, ever since the onset of the scientific

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revolution, science had been promising far more than it could deliver. Galileo's case for the Copernican Theory rested on his theory of the tides, where he contemptuously rejected the moon's influence and instead developed a mechanical model that was far beyond his powers to articulate or demonstrate. Descartes' laws of impact, fundamental for his system, were all wrong except in the trivial cases. The transformation of the techniques of manufacture, promised by every propagandist of the century, took many generations to materialize. In the applications of science, progress toward the solution of outstanding, pressing problems was leisurely; for example, the break-even point for medicine, when there came to be less risk in consultation of a physician than in avoidance, seems to have occurred early in the present century.

None of this is to denigrate science; however slow it was to fulfil the hopes of its early prophets, it has now done so magnificently, nearly miraculously. My aim here is to focus our attention on a certain image of science, dominant until so very recently, where the implicit rule was 'all scientific problems can be discussed with students and the public, provided that they're either already solved or now being solved'. Each of us (including myself) has this one-sided experience of science as 'the facts' embedded deeply in our image of science. That is why I think it is a useful exercise for each of us to recall *when* we first discovered the existence of insoluble scientific problems.

## 'Atomic' Science

If I am still struggling to find a new synthesis out of earlier ideals and recent disappointments, in spite of having earned my living on just that task for 25 years, I cannot really expect colleagues or members of the general public to provide immediate insights that will neatly solve my problems. All I can do is to offer some preliminary ideas, to share with colleagues from various fields of practice, and to hope that out of the resulting dialogue we may achieve a better understanding of the practice and accomplishments of science, as a mixture of success and failure, and of our achieved knowledge and continuing ignorance.

It appears to me that we must now begin to transcend an image of science that may be called 'atomic', for 'atoms' are central to it in several ways. The conception of matter itself, the style of framing problems, and the organization of knowledge as a social possession—all may be considered atomic. I believe that such an image inhibits our grasping the new aspects of science, such as quality control, unsolvable problems and policy choices, that are essential for an effective science of the biosphere.

The idea of atomic was at the heart of the new metaphysics of Nature conceived in the seventeenth century, the basis of the achievements of Galileo, Descartes and Newton. The particular properties of the atoms were always contested, and are not crucial. What counts is the commitment to Nature being composed of isolated bits of reality, possessing only mathematical properties, and devoid of sensuous qualities, to say nothing of higher faculties of cognition or feeling. Such a basis for experimental natural science was quite unique in the history of human civilizations, and on that metaphysical foundation has been built our practice and our understanding of science.

That practice is best described as analytical or reductionist. It is really impossible to imagine laboratory work being done on any other basis. But we can now begin to see its inadequacy for some fields of practice that are largely based on science, such as medicine. To the extent that illness is caused by social or psychological factors, or indeed by mere ageing, the atomic style of therapy through microbe hunting is becoming recognized as inadequate or even misdirected.

With the atomism of the physical reality goes an atomism of our knowledge of it. Thus, it has been highly effective to teach science as a collection of simple hard facts. Any given fact will be related to prior ones whose mastery is necessary for the understanding of it; but to relate forwards and outwards, to the meaning and functioning of a fact in its context, be it technical, environmental or philosophical, is normally considered a luxury, regularly crowded out of the syllabus by the demands of more important material. This is not just yet another deficiency to be blamed on teachers. In his important analysis of 'normal science', T.S. Kuhn (1962) imagines an essentially myopic and anticritical activity, 'a strenuous and devoted attempt to force nature into the conceptual boxes provided by professional education'.

Our conception of the power based on scientific knowledge is similarly atomic. Engineers are trained to solve problems within what we can now see to he exceedingly narrow constraints: operational feasibility within commercially viable costings. The environment hit engineering practice with a sudden impact in the 1970s because of protective legislation, generally first in the United States and then elsewhere. It is understandable that engineers should find it inappropriate for the fate of important dams to depend on the breeding habits of a local fish; but it does reflect on their training and outlook when they repeatedly plan for nuclear power stations in the state of California without first checking for local earthquake faults. To be sure, the calculation of all environmental variables, including the cultural and psychological health of affected local residents, does seem to take engineering far from its original and primary concerns; but the demand for such extreme measures arises from a public reaction to a perceived gross insensitivity by engineers and their employing organizations to anything other than the simplest aspects of the power over Nature that they wield.

Now we have learned that power, even based on knowledge, is not a simple thing. It is relatively easy to build a dam to hold back river water; there is power. But to predict and eventually manage the manifold environmental changes *initiated* by that intrusion is another matter. The flows and cycles of energy and materials that are disrupted by the dam will, all unknown to us, take new patterns and then eventually present us with new, unexpected problems. The dam, strong, silent, and simple, engineering at its most classic, may disrupt agriculture downstream (Aswan, the Nile), create hydrological imbalances (Volga), or even be interpreted as imperialism (Wales)! Hence the constant need for continuous, iterative *control*, lest an atomized knowledge, applied through myopic power, sets off reactions that bring harm to us all.

We may say that a sort of atomism persists in the social practice of science, where the unit of production is the paper, embodying the intellectual property of a new result. This extends to the social organization of science in the erection of specialties and subspecialties, each striving for independence and autonomy. The obstacles to genuinely interdisciplinary research in the academic context, hitherto wellnigh insuperable, point up the disadvantages of this style for the sorts of problems we now confront. It is significant that when scientists are operating in a command economy, being employees on mission-orientated research or R & D, and not in a position to seek individual advancement as subject specialists, an effective exchange of skills *is* possible. Thus, the atomic ideal of knowledge is not an absolute constraint; it can be suspended in the pursuit of knowledge as power; our present task is to see whether it can be transcended in the attempt to apply knowledge, produced by independent scientists and scholars, to the new tasks of control.

# Quality Control in Science

We may now begin to move outward from this previous atomism, to enrich our understanding of the scientific process. Here I am trying only to make explicit what every good scientist has known all along. I may put another question concerning the personal development of each of us: when did I become aware of degrees of quality in scientific materials presented ostensibly as complete, uncontestable facts? I know that for some, either exceptionally independent, or having a gifted teacher, the awareness came very early, even at school. For me, the moment was in my final year at college, when I studied a table of basic physical constants. There I saw alternative values for a single constant that lay outside each other's confidence limits. I realized then that the value of a physical constant could be quite other than an atomic fact. Among the discordant set not all could be right. Was there necessarily one correct value there; or was it a matter of judgement which cited value was the best?

The issue of quality is at the heart of the special methodological problems of biospheric science. Hard facts are few and far between; in many areas (such as rate constants for atmospheric chemical reactions), today's educated guesses are likely to appear tomorrow as ignorant speculations. The problem of achieving quality control in this field is too complex to be resolved by goodwill and redoubled efforts. Later I build on Bill Clark's ideas on making a first analysis of the task.

The problem of quality control in traditional science has quite recently achieved prominence, but still mainly in connection with the extreme and unrepresentative cases of outright fraud. The enormous quantity of patient, unrewarded work of peer review and refereeing, where (in my opinion) the moral commitment of scientists is more crucial, and more openly tested, than in research itself, has received scant attention from the scholars who analyse science. Yet quality control is not merely essential to the vitality and health of normal science. It becomes a task requiring a clear and principled understanding, if the new sciences of the biosphere are to have any hope of success. The inherited, unreflected folkways and craft skills of compartmentalized academic research are inadequate here; and here we lack the ultimate quality test of practice, realized mainly through the marketplace of industrial research and R & D.

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I envisage a major effort in our project being devoted to the creation of appropriate methods and styles of quality control. I hope that this will emerge naturally from reflection on their own experience by scientists who have already been engaged in such work; but it cannot be expected to form itself automatically, without explicit attention and investment of the resources of all of us. I return to this theme in the final section.

## Choice in Science

My next theme is that of 'choice': here too it was Alvin Weinberg (1963, 1964) who first raised the issue, early in the 1960s. Previous to that, the ruling assumption, one might almost say ideology, had been that real science required an autonomy that included choice of problems and the setting of criteria for that choice. But with the advent of 'big science', the public that supported the effort through a significant burden on state expenditure was inevitably going to demand some voice in the disposition of its largesse. This is not the place to discuss the detailed arrangements, or the deeper problems, of that new 'social contract of science'. Anyone involved with this biosphere project is fully aware that biosphere problems are not to be solved without massive investment of funds, in which public and private corporate agencies are inevitably, and quite legitimately, involved.

All this may seem so natural that we must remind ourselves how new it is, and also how little impact it has made on the philosophical accounts of science to which we all go for enlightenment and guidance. There is a real gap between conceptions here: if science consists of true atomic facts, whose value lies in themselves, then what possible genuine criterion of choice can there be for research? Of course, the experience of research science is that not all facts are of equal value; they vary in their interest and fruitfulness, as well as in their internal strength and robustness. Hence policy decisions on research are possible, however difficult it is to quantify or even to justify them with conclusive arguments.

When we consider the criteria for choice governing mission-orientated projects, we find some components that are more or less internal to the process

#### Usable Knowledge, Usable Ignorance

and others that are not. In the former category are feasibility and cost (this latter being measured against the demands of competing projects within some pre-assigned limited budget). For this we must take into account the aims or objectives of the project, which are necessarily exterior to it and different from the research itself, for they employ values.

In considering these external values, I make a distinction between functions and purposes: The former refers to the sort of job done by a particular device, and the latter to the interests or purposes served, or the values realized, by the job being done. Functions are still in the technical realm, while purposes belong to people and to politics. It is at the intersection of these two sorts of effects that policy-making for sciences and technology is done.

The question of feasibility, while mainly technical, is not entirely straightforward. The assessment of feasibility depends on a prediction of the behaviour of a device or system when it is eventually created and in operation. To the extent that the proposal involves significant novelty or complexity, that prediction of the future will inevitably be less than certain. Indeed, it is now clear in retrospect that the great technological developments of recent decades were made under conditions of severe ignorance concerning not merely their social and environmental effects, but even their costs of construction, maintenance and operation. There is an old and well-justified joke that if a cost-benefit analysis had been made at the crucial time, then sail would never have given way to steam. But many American utility companies might now reply that a proper analysis, made on their behalf, of nuclear power might have protected them from the financial disasters that now threaten to engulf them.

This point is not made by way of apportioning blame for the troubles of that once supremely optimistic industry. It can be argued that, say, 15 years ago, it was impossible to predict which of the possible mishaps would afflict the industry, and how serious they would be. But in that event, we should recognize the ineradicable component of ignorance, not merely uncertainty, in forecasting the prospects for any radically new technology.

#### Ignorance

The pervasiveness of ignorance concerning the interactions of our technology with its environment, natural and social, is a very new theme. 'Scientific ignorance' is paradoxical in itself and directly contradictory to the image and sensibility of our inherited style of science and its associated technology. Coping with ignorance in the formation of policy for science, technology and environment is an art that we have barely begun to recognize, let alone master. Yet ignorance dominates the sciences of the biosphere, the focus of our project.

The problems of applying science to policy purposes in general have been given a handy title, 'usable knowledge'. For those problems of the imminent future, we would do well to remind ourselves of their nature by using a title like 'usable ignorance'. Its paradoxical quality points up the distance we must travel from our inherited image of science as atomic facts, if we are to grapple successfully with these new problems. How we might begin to do so is the theme of my discussion here,

# Elements of a New Understanding

To some extent, the preceding conceptual analysis follows the path of the maturing understanding of many scientists of the present generation. First, as students, we mastered our standard facts; then, in research, we became aware of quality; as we became involved in the government of science, we recognized the necessity for choice; involvement in environmental problems brought us up against functions of devices and of systems, and the frequently confused and conflicting purposes expressed through politics. Still, we could imagine that there was a hard core to the whole affair, in the sort of basic, incontestable facts that every schoolboy knows. Hence the intrusion of ignorance into our problem-situation did not immediately raise the spectre of the severe incompetence of science in the face of the challenges—or threats—produced by the environmental consequences of the science-based technology on which our civilization rests.

# Science in the Policy Process

This rather comfortable picture is analogous to the traditional model of science in the policy process. We may imagine this as a meeting of two sides. The public, through some political machinery, expresses a concern that some particular purposes are being frustrated or endangered, say through the lack of clean water. Administrators then devise or promote devices and systems, physical technology, or administrative agencies to perform particular functions whereby those purposes may once again be protected. For this they need information about the natural process involved in the problem, for which they turn to the scientists. The scientists provide the necessary facts (either from the literature, or produced by research to order) that either determine the appropriate solution, or at least set boundaries within which the normal processes of political bargaining can take place. In that way, the problem is solved or, at least, effectively resolved in political terms.

However well such a model has fitted practice in the past, it no longer captures the complexity and inconclusiveness of the process of policy-related science in the case of biospheric problems (Otway and Ravetz 1984). Indeed, we may define this new sort of policy-related science as one in which facts are uncertain, values in dispute, stakes high, decisions urgent, and where no single one of these dimensions can be managed in isolation from the rest. Acid rain may serve as the present paradigm example of such science. This model may seem to transform the image of science from that of a stately edifice to that of a can of worms. Whether this be so, the unaesthetic quality is there in the real world we confront and with which we must learn to cope somehow.

It may help if we employ another model: how problems come to be chosen for investigation. In the world of pure or academic science, problems are *selected* by the research community. If a particular area is not yet ripe for study, available techniques being insufficiently powerful, it is simply left to wait, with no particular loss. (The adventurous or foolhardy may, of course, try their luck there.) In the case of mission-orientated work, they are *presented* by managerial superiors, though these are expected to have some competence in assessing feasibility and costs of the research in relation to the goals of the enterprise. But in policy-related science, the problems are *thrust upon* the relevant researchers by political forces that take scant heed of the feasibility of the solutions they demand. Indeed, it will be common for such problems not to be feasible in the ordinary sense. Drawing on low-prestige and immature fields, requiring databases that simply do not exist, being required to produce answers in a hurry, they are not the sort of inquiry where success of any sort can be reasonably expected.

It may be that our traditional lack of awareness of the interaction of ignorance with scientific knowledge has been maintained because science could proclaim its genuine successes and remain at a safe distance from its likely failures. Through all the centuries when progress became an increasingly strong theme of educated common sense, science could be seen as steadily advancing the boundaries of knowledge. There seemed no limit in principle to the extent of this conquest, and so the areas of ignorance remaining at any time were not held against science—they too would fall under the sway of human knowledge at the appropriate time.

Now we face the paradox that while our knowledge continues to increase exponentially, our relevant ignorance does so even more rapidly. And this is ignorance generated by sciencel An example will explain this paradox. The Victorians were totally ignorant of the problem of disposal of long-lived radioactive wastes. They had no such things, nor could they imagine their existence. But now we have made them, by science, and the problem of guaranteeing a secure storage for some quarter of a million years is one where ignorance, rather than mere uncertainty, is the state of affairs. Thus, we have conquered a former ignorance, in our knowledge of radioactivity, but in the process created a new ignorance, of how to manage it in all its dangerous manifestations.

# Interpenetrating Opposites in Science

Science in the policy process is thus a very different thing from the serene accumulation of positive and ultimately useful factual knowledge, as portrayed in our inherited image. Indeed, given the intrusion of subjective

To begin with it is necessary for us to transcend the simplistic picture of science that has been dominant for so very long. For generations we have been taught of a difference in kind between facts and values. The latter were seen to be subjective, uncertain, perhaps even basically irrational in origin. Fortunately, science supplied facts, objective and independent of value judgements, whereby we could attain genuine knowledge and also order our affairs in a proper manner. Those who protested that such a sharp dichotomy was destructive of human concerns were usually on the romantic or mystical fringe, and could be ignored in the framing of curricula and in the propaganda for science.

Similarly, the opposition between knowledge and ignorance was absolute. A scientific fact could be known, simply and finally. It could, of course, be improved upon by the further growth of science; but *error* in science was nearly a contradiction in terms. The boundary between knowledge and ignorance was not permeable; it simply advanced with each increment of science, bringing light to where darkness had hitherto reigned. Of course, there have been many disclaimers and qualifications tacked on to this simple model; we all know that science is tentative, corrigible, open-ended, and all the rest. But the idea that a fact could be understood imperfectly or confusedly, or that a great scientific discovery could be mixed with error, has been brought into play only very recently by historians of science.

Hence we are really unprepared by our culture to cope with the new phenomenon of the interpenetration of these contradictory opposites. The impossibility of separating facts from values in such a critical area as the toxicity of environmental pollutants is a discovery of recent years (Whittemore 1983). And the creation of relevant ignorance by the inadequately controlled progress of technology is still in the process of being articulated by philosophers (Collingridge 1982).

An immediate reaction to these disturbing phenomena can be despair or cynicism. Some scholars have elaborated on the theme that pollution is in the nose of the beholder, and reduce *all* environmental concern to the social-psychological drives of extremist sects (Douglas and Wildavsky 1982). Politicians and administrators can take the easy way out and treat scientists as so many hired guns, engaging those who are certain to employ technical rhetoric on behalf of their particular faction. Such solutions as these, if considered as cures, are really far worse than the disease. If dialogue on these urgent scientific issues of the biosphere is degraded to thinly veiled power politics, then only a congenital optimist can continue to hope for their genuine resolution.

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Viewed socially, these oppositions or contradictions show no way through, But the situation is not desperate once we appreciate that decision-making is not at all a unique event requiring perfect inputs if it is to be rational. Rather it is a complex process, interactive and iterative; the logical model for it is perhaps less demonstration than dialogue. Seeing decision-making (or policyformation; I use the two terms interchangeably) as a sort of dialectical process. we may imagine those central contradictions of usable knowledge and usable ignorance being transcended, or synthesized, through the working of the dialectical process.

## Varieties of Policy-Related Research

First, I show how these problems of policy-related research may be differentiated, and in such a way that the natural tendency of their dynamics is toward a resolution. Drawing on recent work by myself and my colleague, S.O. Funtowicz (Funtowicz and Ravetz 1985), I distinguish two dimensions of such problems: systems uncertainties and decision stakes. The former refers to the complex system under consideration, including aspects that are technical. scientific, administrative and managerial; the uncertainties are the ranges of possible outcomes, corresponding to each set of plausible inputs and decisions. The decision stakes are the costs and benefits to all concerned parties, including regulators (both field employees and administrators) and representatives of various interests, that correspond to each decision. In each case, we have complex sets of ill-defined variables for aggregation into a single index. hence each of the dimensions is only very loosely quantitative. We distinguish only the values low, medium and high (Figure 1). When both dimensions (systems uncertainties and decision stakes) are low, we have what we may call applied science; straightforward research will produce a practical band of values of critical variables within which the ordinary political processes can operate to produce a consensus.

When either dimension alone becomes large, a new situation emerges; we call it technical consultancy. This is easiest to see in the case of system uncertainty; the consultant is employed precisely because his or her unspecifiable skills, and his or her professional integrity and judgement, are required for the provision of usable knowledge for the policy process. It is less obvious that, even if uncertainties are low, large decision stakes take the problems out of the realm of the routine. But on reflection, this is the way things happen in practice. If some institution sees its interests seriously threatened by an issue, then no matter how nearly conclusive the science, it will fight back with every means at its disposal, until such time as further resistance would cause a serious loss of credibility in itself as a competent institution, and a damaging loss of power as a result. The public sees such struggles most clearly in notorious cases of pollution, when a beleaguered institution persists in harmful policies (such as poisoning its work-force or the local environment) to the point of being



irresponsible, immoral, or perhaps even culpable (industrial asbestosis is a notorious recent case in point). The outrage in such cases is fully justified, of course; but it is an error to believe either that those particular firms are uniquely malevolent, or that all firms casually and habitually behave in such a way. No, it is just when caught in such a trap, however much of their own making, that institutions, like people, will fight for survival.

Such cases are fortunately the exception. It is more common for both systems uncertainties and decision stakes to be moderate. Funtowicz and I have been able to articulate a model of consultancy practice, wherein the traditional scientist's ideal of consensual knowledge is sacrificed on behalf of a more robust sort of knowledge appropriate to the problem. We call it clinical, from the field of practice in which such a style has been developed successfully. In it we eliminate safety as an attribute (the term now has a largely rhetorical meaning anyway) and substitute good performance (which may include the possibility of failures and accidents). In the same vein, we generalize probability (with its mathematical connotations) to propensity, and measure to gauge; and for prediction we substitute prognosis. In this way, we hope to express the degree to which non-quantifiable and even non-specifiable expert judgements enter into an assessment. The outcome of the process (which is conceived as continuously iterating) is not a general theory to be tested against particular facts, but rather a provisional assessment of the health of a particular system together with the relevant aspects of its environment. I hope that this model will be useful in the biosphere project as it develops.

Passing to the more intractable case, where either dimension is very large, we have what we call a total environmental assessment. For here, nothing is certain, there are no boundaries or accepted methods for solving problems;

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the problem is total in extent, involving facts, interests, values and even lifestyles, and total in its mixture of dimensions and components. Even here a review of history shows that in such cases a resolution can emerge. A debate ensues, once an issue is salient; and while at first the debate may be totally polarized and adversarial in style, it may evolve fairly quickly. Both sides are attempting to gain legitimacy with the various foci of opinion, which ultimately represent power: special-interest groups, administrators, politicians, the media, respondents in opinion polls, voters. They therefore necessarily invoke the symbols of universality and rationality whereby uncommitted observers can be won over; and in however oblique and implicit a fashion, a genuine dialogue emerges. Most important in this process, new relevant knowledge is created by the requirements of the various disputants, so that the issue is brought in the direction of technical consultancy, if not yet science. For example, issue-generated research can eventually transform the terms of a debate, such as in the case of lead in automobile fuel in Britain and Europe during the early 1980s. Events that previously had not been significant news suddenly became so: thus the various nuclear accidents of the 1950s and 1960s were of no great moment for policy purposes, while Three Mile Island was a mortal blow to the American nuclear power construction industry. Hence a problem does evolve; a dominant consensus can emerge; and then the losing side is forced into a retreat, saying what it can while the facts as they emerge tip the balance ever more decisively against it.

There is, of course, no guarantee that any particular total environmental assessment will move down scale in this way, or will do so quickly enough for its resolution to prevent irreparable harm. But at least we have here a model of a process whereby a solution can happen, analogously to the way in which great political and social issues can be (but, of course, need not be) resolved peacefully and transformed.

Debates on such issues are usually very different from those within a scientific community. They cannot presuppose a shared underlying commitment to the advance of knowledge nor presuppose bounds to the tactics employed by the antagonists. In form they are largely political, while in substance ostensibly technical or scientific. Confusion and rancour of all sorts abound. Yet, I argue, such apparently unedifying features are as consistent with effective policies for science and technology as they are for political affairs in general. And they *must* be, for the great issues of the biosphere will necessarily be aired in just such forums; there are no other forums to render them unnecessary.

# The Policy Process and Usable Knowledge

Now I discuss the policy process itself, in relation to these phenomena of the interpenetration of facts and values and of knowledge and ignorance. This is not the place to develop schematic models of that process, so I will content

myself with a few observations. The first is that no decision is atomic. Even if an issue is novel, even if its sponsoring agency is freshly created, there will always exist a background, in explicit law, codes of practice, folkways and expectations, in which it necessarily operates even while reacting on the background. And once an issue exists, it is rare indeed for it to fade away. It may become less salient for policy and be relegated to a routine monitoring activity; but it can erupt at any time should something extraordinary occur.

Indeed, when we look at the duration and complexity of those dialectical processes whereby a total environmental assessment problem (its common initial form) is gradually tamed, we see the necessity for a differentiation among the functions performed by the facts - or better, the inputs of technical information. Here I can do no better than to use materials recently developed by Bill Clark (personal communication). He starts with authoritative knowledge-the traditional ideal of science, still applicable in the case of applied science issues. This is supplemented by reporting – not in newspapers, hut in the accumulation of relatively reliable, uncontroversial information on a variety of phenomena of no immediate salience, but crucial when a crisis emerges. This is the descendant of natural history, popular in past epochs when clergymen and other gentlemen of leisure could gain satisfaction and prestige through their mastery of some great mass of material, perhaps of a locality, perhaps of a special branch of nature. The decline of this style of science, under the pressure of changing institutions and the dominant criteria of quality, is a clear example of what I have called the social construction of ignorance. Harvey Brooks (1982) has recently shown what a price we now pay for our ignorance, in the impotence of what I call the clean-up or garbage sciences in the face of our various pollution problems.

When science is involved in the policy process, particularly in the technical consultancy mode, then impersonal demonstrations give way to committed dialogue, and no facts are hard, massy and impenetrable. They are used as evidence in arguments, necessarily inconclusive and debatable. In this case we invoke metaphors to describe their nature and functions; Steven Toulmin (1972) has suggested the term 'maps' (not pictures, or we might say dogmas, but rather guides to action). I have developed the idea of a tool, something that derives its objectivity not so much through its correspondence with external reality as through its effectiveness in operating on reality in a variety of functions and contexts (Ravetz 1984).

Passing to the more contested issues, we mention enlightenment, which might involve enhancing awareness or changing common sense. Perhaps the most notable example of this sort of product in recent times is *Silent Spring* by Rachel Carson (1962). Through it, the environment and its problems suddenly came into existence for the public in the United States and elsewhere. We note that this function is performed partly through the mass media; the role of investigative journalism in the press, and especially television, in enhancing the awareness of the non-scientific public (and perhaps of scientists, too) should be more appreciated. Once an issue has been made salient for the political process, then science can be a complement to interaction—that is, not being decisive in itself in any unreflective way, but correcting common-sense views, and providing crucial inputs when a debate is sharpened. To take an example from another field, the regulation of planned interference with the life-cycle of embryo and fetus will not be reduced to the scientific determination of the onset of life and individuality. But, just as technical progress creates new problems of decision and regulation, scientific information can provide channels and critical points for the ethical and ideological debates on such issues.

Finally, Bill Clark mentions ritual and process: since science is the central symbolic structure of modern industrialized society, the invocation of science to solve a problem has a political power of its own. But such an action, if abused or even abortive, may lead to a wider disillusionment with the secularly sacred symbols themselves, with consequent harm to the social fabric. W. D. Ruckelshaus (1984), sometime Administrator of the Environmental Protection Agency, has identified this danger clearly, in his warning of chaos if his agency is perceived as not doing its job. Analogously, we may say that the best thing to happen to the American nuclear power industry was the outstandingly independent and critical Kemeny report (1979) on Three Mile Island. If such a report had been widely and effectively denounced as a whitewash operation, the loss of credibility of the industry and of its governmental regulatory agencies could have been catastrophic.

With this spectrum of different sorts of usable knowledge, and their corresponding variety of institutions and publics, we begin to see a practical resolution of the abstract dichotomies of fact and value, knowledge and ignorance. Of course, the system as a whole is complicated, underdetermined and inconclusive. But that means it's like social life itself, where we have many failures but also many successes. The only thing lost, through this analysis, is the illusion that the scientist is a sort of privileged being who can dispense nuggets of truth to a needy populace. Seeing the scientist as a participant, certainly of a special sort, in this complex process of achieving usable knowledge provides us with some insights on how to make his or her contribution most effective.

## **Towards a Practical Approach**

Here I hope to be constructive, and I can start my argument with a topic mentioned early in my analysis of the enriched understanding of science that every researcher develops: the assessment of quality. This is frequently the first exposure of a scientist to the essential incompleteness of any scientific knowledge—not merely that there are things left to be discovered, but that the border between our knowledge and our ignorance is not perfectly defined. Even when scientific statements turn out to mean not quite what they say, they are not necessarily the product of incompetence or malevolence; rather, they reflect the essential incompleteness of the evidence and the argument supporting any scientific result. In a matured field, the assessment of quality is a craft skill that may be so well established as to be nearly tacit and unselfconscious: we *know* that a piece of work is really good (or not), without being easily able to specify fully why. By contrast, one sign of the immaturity of a field is the lack of consensus on quality, so that every ambitious researcher must become an amateur methodologist in order to defend his or her results against critics.

# Scientific Quality-A Many-Splendoured Thing

When we come to policy-related science, that simple dichotomy of the presence or the absence of maturity is totally inadequate to convey the richness of criteria of quality, with their associated complexity and opportunity for confusion. Here I can only refer to the deep and fruitful insights of Bill Clark, in his taxonomy of criteria of quality among the various legitimate actors in a policy process involving science. In his table of critical criteria, he lists the following actors: scientist, peer group, programme manager or sponsor, policy-maker, and public interest group. For each of these, there are three critical modes: input, output and process. Mastery of that table, reproduced here (Table 1), would, I think, make an excellent introduction to the methodological problems of policy-related science.

It may well be that, as this project develops, we will need to go through that exercise, if only to the extent of appreciating that the research scientist's criteria of quality are not the only legitimate ones in the process.

However different or conflicting may be the other criteria of quality, they must be taken into account, not only in the reporting of research but even in its planning and execution. Now, any one of the actors in such a process must, if she or he is to be really effective in a co-operative endeavour, undertake a task that is not traditionally associated with science: to appreciate another person's point of view. This need not extend to abandoning conflicting interpretation of facts (for a fruitful debate is a genuine one), nor to empathy for another's life-style or world-view. But for strictly practical purposes each participant must appreciate what it is that another is invoking, explicity or implicitly, when making points about the quality of contested materials.

This new and important skill has been called (by Bill Clark) 'a critical connoisseurship of quality in science'. One does not merely apply one's own specialist criteria blindly or unselfconsciously, however excellent or valid they may be for one's own scientific expertise or role. One must be able to assess productions from several points of view in succession, by means of an imaginative sympathy that involves seeing one's own role, one's own self, from a slight distance. It may be that I am here calling for the cultivation of attitudes proper to literary criticism, a prospect that to some may be even more alien than Zen riddles. But given the complexity of policy-related science, in and the mode, usuale ignorance

#### Table 1 Critical Criteria

| Critical Role                      | Input  | Critical Mode   |  |  |
|------------------------------------|--|---|--|--|
| Scientist                          | ,  | Output Process  |  |  |
| Poor                               | Hesource and time<br>constraints; available theory;<br>institutional support;<br>assumptions; quality of<br>available data; state of the art   | Validation; sensitivity analyses;<br>technical sophistication; degree<br>of acceptance of conclusions;<br>impact on policy debate;<br>imitation; professional<br>recognition  | Choice of methodology (e.g.<br>estimation procedures);<br>communication;<br>implementation; promotion;<br>degree of formalization of<br>analytic activities within the<br>organization |  |
| group                              | Quality of data; model and/or<br>theory used; adequacy of<br>tools; problem formulation;<br>input variables well chosen?<br>Measure of success specified<br>in advance?                                | Purpose of the study;<br>conclusions supported by<br>evidence? Does model offend<br>common sense? robustness of<br>conclusions; adequate coverage<br>of issues  | Standards of scientific and<br>professional practice;<br>documentation; review of<br>validation techniques; style;<br>interdisciplinarity  |  |
| Programme<br>manager<br>or sponsor | Cost; institutional support<br>within user organization;<br>quality of analytic team; type<br>of financing (e.g. grant versus<br>contract)   | Rate of use; type of use<br>(general education, programme<br>evaluation, decision-making,<br>etc.); contribution to<br>methodology and state of the<br>art; prestige; can results be<br>generalized, applied elsewhere? | Dissemination; collaboration<br>with users; has study been<br>reviewed?  |  |
| 'olicy-maker                       | Quality of analysts; cost of<br>study; technical tools used<br>(hardware and software); does<br>problem formulation make<br>sense?   | Is output familiar and<br>intelligible? Did study generate<br>new ideas? Are policy<br>indications conclusive? Are they<br>consistent with accepted ethical<br>standards?   | Ease of use; documentation;<br>are analysts helping with<br>implementation? Did they<br>interact with agency personnel;<br>With interest groups?                                       |  |
| ublic-interest<br>oups             | Competence and intellectual<br>integrity of analysts; are value<br>systems compatible? Problem<br>formulation acceptable?<br>Normative implications of<br>technical choices (e.g., choices<br>of data) | Nature of conclusions; equity;<br>analysis used as rationalization<br>or to postphone decisions? All<br>viewpoints taken into<br>consideration? Value issues  | Participation; communication o<br>data and other information;<br>adherence to strict rules of<br>procedure   |  |

response to the complexity of biospheric problems, I can envisage no easier alternative.

## Usable Ignorance

The preceding analysis has, I hope, made us familiar with the richness of the concept of usable knowledge in the context of incomplete science with policy implications. Now I can attempt to make sense of that paradoxical category, usable ignorance; for in many respects this defines our present task as one that is qualitatively different from the sorts of science with which we have hitherto been familiar.

First, I have indicated one approach to taming ignorance, by focusing on its border with knowledge. This should be easily grasped with an experience of research. Indeed, the art of choosing research problems can be described as sensing where that border can be penetrated and to what depth. Similarly, the art of monitoring for possible accidents or realized hazards, be they in industrial plant or environmental disruption, consists in having a border with ignorance that is permeable to signals coming from the other side, signs of incipient harmful processes or events that should be identified and controlled. Thus, the technical consultancy problem is one where ignorance is managed, through expert skill, in just this way.

Where ignorance is really severe, as in total environmental assessment, then it is involved in the problem in ways that are both more intimate and more complex. For if ignorance is recognized to be severe, than no amount of sophisticated calculation with uncertainties in a decision algorithm can be adequate for a decision. Non-quantifiable, perhaps non-specifiable, considerations of prudence must be included in any argument. Further, the nature and distribution of a wider range of possible benefits and costs, even including hypothetical items, must be made explicit. Since there can be no conclusive or universally acceptable weighting of these, the values implicit in any such weighting must be made explicit. In terms of a dialogue between opposed interests, this effectively takes the form of a burden of proof: in the absence of strong evidence on either side do we deem a system safe or do we deem it dangerous?

By such means we do not conquer ignorance directly, for that can be done only by replacing it with knowledge. But we cope with it and we ensure that by being aware of our ignorance we do not encounter disastrous pitfalls in our supposedly secure knowledge or supposedly effective technique.

The preceding account is prescriptive for future practice rather than descriptive of the past. Had ignorance been recognized as a factor in technology policy, then, for example, the nuclear power industry would today be in a far healthier state. The easy assumption that all technical problems could be solved when the time came has left that industry, and the rest of us on this planet, with such problems as the disposal of long-lived radioactive wastes. In this case we must somehow manage our ignorance of the state of human society some tens of thousands of years into the future. How many professional engineers have been prepared by their professional training for such a problem?

Coping with ignorance demands a more articulated policy process and a greater awareness of how that process operates. Great leaps forward in technology require continuous monitoring to pick up the signals of trouble as they begin to arrive, and both physical symptoms and their institutions should be designed with the ignorance factor in mind, so that they can respond and adapt in good time. (This point has been amply developed by Collingridge (1982).)

Recognition of the need for monitoring entails that the decision process be iterative, responding in a feedback loop to signals from the total environment of the operating system. Also, the inclusion of ignorance in decision-making via the explicit assignment of burden of proof involves a self-conscious operation of dialogue at several levels, the methodological and regulative simultaneously with the substantive. All this is very complicated, of course, and the

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transaction costs of running such a system might appear to be very high, not least in the absorption of time and energy of highly qualified people. But if those costs become a recognized element of the feasibility of a project, let it be so; better to anticipate that aspect of coping with ignorance than either to become bogged down in endless regulator games, or to regress to a simplistic fantasy of heroic-scale technological innovation, thereby inviting a debacle

Coming now to an idea about the biosphere project itself, I find the category of usable ignorance influencing it in several ways. First, it should condition the way we go about our work, for we will be aware that just another programme of research and recommendations is not adequate to the solution of biospheric problems. Also, the concept of usable ignorance may provide topics for a special research effort within the project. What I have described above is only a rudimentary sketch of some of the elements of a large, important and inherently complex phenomenon. With colleagues at Leeds University, I have begun to articulate themes for a co-ordinated research effort involving the logic of ignorance, studies of how some institutions cope with the ignorance that affects their practice, as it reveals itself in error and failure, and more studies of how institutions cope with the threats posed by their ignorance when their monopoly of practice, or their legitimacy, is threatened.

More directly relevant to the immediate concerns of colleagues on the biosphere project is the way in which we will need to make our own ignorance usable. We are, after all, inventing a new scientific style to respond to the new scientific problems of the biosphere, simultaneously with the special researches that are at its basis. We have various precedents to remind us what is not likely to work. The simplest is a scattered set of groups of experts, each doing their own thing and meeting occasionally to exhibit their wares. Synthesis of the efforts is then left to the organizers of the meeting and the editors of their proceedings. At a higher level, we have the experience of multi-disciplinary teams, where each member must protect his or her own private professional future by extracting and cultivating research problems that will bring rewards by the special criteria of quality of his or her subject subspecialty. Here, too, the whole of the nominally collaborative effort is only rarely greater than the sum of its parts. Nor can we turn with much hope to the task-force model, which does bring results in technology, for that depends critically on the simplicity of the defining problem, and on an authoritarian structure of decision and control. Our problems are multidimensionally complex by their very nature, and trans-national co-operation is achieved more by cajoling than by command. Hence, none of the existing styles of making knowledge usable is appropriate for ignorance.

# Conditions for Success

It appears, then, that we need some sort of dialectical resolution of the contradiction between the auto-archy of academic-style research and the dictatorship of industrial-style development. There seem to be two elements necessary to make such a new venture a success. One is motivation. Enough of us on the biosphere project must see it as a professional job, developing a new sort of scientific expertise in which we can continue to do satisfying work after the completion of the project. I have no doubt that if this project succeeds, it will become a model for many others, enough to keep all of us busy for a long time. The other element is technique: devising means whereby the genuine mutual enhancement of ideas and perspectives can be accomplished. I indicated some of these at the very beginning of this chapter, in describing some ways in which the biosphere project will be novel.

We may well find ourselves experimenting with techniques of personal interaction that have been developed for policy formation, but that have hitherto been considered as irrelevant to the austere task of producing new knowledge. But since we, even in our science, are trying to make ignorance usable, we should not be too proud to learn about learning, even in the research process.

The crucial element here may lie in quality assessment and the mutual criticism that makes it possible. Can we learn, sufficiently well for the task, to have imaginative sympathy with the roles and associated criteria of quality of others in different corners of this complex edifice? We will need to comprehend variety in scientific expertise, in methodological reflection, in organizational tasks, and in policy formation. If so, then we can hope to have what Bill Clark has called a 'fair dialogue', in which we are each an amateur, in the hest sense of the term, with regard to most of the problems on which we are engaged.

I believe that such a process is possible and that it is certainly worth a try. The environmental problems that confront us, as residents of this planet, are now global and total. We in this group cannot hope to legislate for all of humanity over all the salient issues. But we can at least indicate a way forward, showing that our civilization is genuinely resilient in meeting this supreme challenge.

## **Conclusion and Perspective**

As an historian, I like to find support and understanding in the pattern of the past as it may be extended into the future. In this connection, I can do no better than to quote from an early prophetic writing of Karl Marx. In the Preface to his *Critique of Political Economy* (1869), he gave an intensely concentrated summary of past human history as he understood it, in terms of class structures and class struggles. His concluding motto was, 'Mankind only sets those problems that it can solve'. We must try to justify his optimism in the case of this present challenge. We may understand it as our civilization's characteristic contradiction: the intensified exploitation of nature through the application of knowledge to power, which threatens to become self-destructive unless brought under control.

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For my historical perspective on this, I would like to review the evolution of science as a social practice, as it has developed to create new powers and respond to new challenges. In the seventeenth century, the scientific revolution had two related elements: the disenchantment of nature, and the articulation of the ideal of a cumulative, co-operative public endeavour for the advancement of knowledge. With the decay of the ancient belief in secrets too powerful to be revealed came a commitment to a new style of social relations in the production of knowledge. This was promoted as both practically necessary and morally superior. From this came the first scientific societies, and their journals provided a new means of achieving novelty while protecting intellectual property.

As this system matured in the nineteenth century, with the creation of complex social structures for the organization and support of research and researchers, the early dream of power through secular, disenchanted knowledge took on reality. For this there were developed the industrial laboratories and applied research institutes, first in Germany, but eventually elsewhere. From these came the high technology of the present century, on which the prosperity and even survival of our civilization now depends.

The idea of using such applicable science as a significant contribution to the planned development of the means of production was first articulated in the socialist nations, and popularized everywhere by the prophetic writings of J.D. Bernal. It lost its ideological overtones during the Second World War; and now that planning is an essential tool even in the market-economy nations, science as 'the second derivative of production' (in Bernal's phrase) is a commonplace (Ravetz 1974). Even academic research is now strongly guided by priorities, set in the political process, and related to the requirements of the development of the means of production and of destruction. Boris Hessen's classic thesis on *The Social and Economic Roots of Newton's 'Principia'* may have been crude and over-simple for the seventeenth century, but for the twentieth it is a truism. There still remains a difference in slogans—in the socialist countries it is 'the scientific-technological revolution', in the others it is 'don't come last in the microelectronics race'—and only time will tell how these will work out in practice.

Our present concerns are centred on the new problems of the biosphere, involving an ecological vision that runs counter to that of Bernal, and the tradition to which he was heir. The 'domination of nature', the driving vision of our science-based civilization, may turn out in retrospect to have been just a disenchanted variety of magic (Leiss 1972). The recently discovered fact that we cannot dominate, though we can destroy, may be the decisive challenge to our civilization. The solution of the problem of world-wide poverty through the development of material production in imitation of the West, even if possible in the social sphere, could become ecologically devastating. Can the biosphere provide the sources and sinks for a world-wide population of a billion private automobiles? Hence, I believe the new task for science is a total one, requiring new concepts of its goals in human welfare as well as new methods of achieving knowledge and wielding power over Nature under appropriate control.

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# A New Social Contract for Science

In this essay I am viewing science in the perspective of several hundred years of continuous internal growth and external support. During this period the 'material' side of science had been doubling every fifteen years, with remarkable constancy; and science enjoyed general prestige and the confidence of a variety of publics. For some, science (in its discoveries and methods) promised a Truth that was genuine and reliable, unlike opinions derived from arguments about words or from obedience to authority. For others, science promised the means to the conquest of Nature for the achievement of general welfare as well as private profit. For many, there was the sheer delight and fascination in sharing the discovery of the structure and workings of the natural world. Whatever its function, science gave satisfaction. Although there were always some who opposed it, in part or in whole, they were a steadily decreasing band. All the different aspects and images of science, appealing to its different publics, were in harmony.

This picture, holding roughly for all the eighteenth and nineteenth centuries and a bit beyond, now seems a bit too good to be true. Yet the historical record shows that science *was* nearly universally accepted as the embodiment of progress, itself the symbol of our secular civilization. Also, the steady growth of 'pure science' in size and effectiveness is evidence of a sort of 'social contract' mentioned in the title. Science enjoyed ever-increasing support, complete freedom in choosing its problems, and considerable autonomy in setting its criteria of quality. In return, it was not constrained to provide direct benefits for any particular client. It was sufficient for science to promise indirect benefits in ideology (its particular form of truth), in industry and in education. In the later part of that period, a particular aspect of science became accepted as representing its essence, that of 'discovery' rather than, say, 'invention' or learning. In our times, that has become modified to 'research'; so that for these closely associated with a university, and hence near the centre of the endeavour of science, research is what it is all about.

In retrospect all that seems a golden age. From the very opening of the

twentieth century, complications set in. Philosophers of science know of the unsettlement caused by Einstein's work. The image of science as the cumulation of Truths never recovered from those intellectual revolutions. The Great War, its aftermath, then the Second World War culminating in the Bomb, brought evil into the life of science. Since then, problems and complications have increased, so that 'science' is blamed for our afflictions, as indiscriminately as it was formerly praised for our blessings.

All that is quite familiar, yet there is a new and very troubling element that has recently become noticeable. It is not merely that science must now endure many critics. Worse, science now seems to have no effective champions, who can speak from inner conviction, to bring a doubting public back to their traditional confidence in science. As a result, science is increasingly vulnerable to any and all criticism and attack, whether from anti-Establishment intellectuals, or from an anti-intellectual Establishment. This is just now worse and more obvious in Britain than elsewhere; but it is not at all unique to that country. Certainly there is plenty of money for science in the USA. But this is increasingly in the form of contracts for specified research, from the federal government or from private industry, so that it is more in the nature of longterm R & D than the scholarly pursuit of knowledge.

This is the symptom which I shall use to introduce my study of science in its social setting: the old social contract of science seems to be weakened, indeed discredited; and there is as yet nothing to take its place. I will *not* here offer a clean and tidy solution to this problem; for I do not know of any. Rather, I will offer some examples and ideas, as an invitation to a discussion; only that, and no more. It may be that such a style, rather than theories and blueprints, might even be appropriate for an eventual 'New Social Contract for Science'.

# Why Science Has No Champion

There is no need for me to run through the doleful tale of attacks on scientific research, both within and outside the universities, that are the hallmark of the present (Thatcher) UK government. Nor need I remind you of the silence of the other major parties on this issue. In the next government, there might well be more money for science and education, but at a political price, in reorganization or redirection to someone else's priorities. The old British social contract, so well epitomized in the hallowed principle of the old Medical Research Council to 'back chaps' (selecting for self-defined excellence), is no more.

Elsewhere the hostility and contempt are not so obvious, but the end of an era is unmistakable. In America, biology has for some years been increasingly under the sway of the commercial interests that are developing nascent technologies. And the physical sciences there have needed to sup at the table of the promoters of Star Wars, in spite of the obvious mendacity and corruption of that programme. In these English-speaking countries, the struggle to maintain the health of the scientific enterprise is especially sharp, at times nearly desperate. It is not made any easier by those critics, generally from within the educated sector of society, who attack science for its alleged lapses from morality or integrity. This goes beyond the common practice of blaming an undifferentiated 'science' for all the threats to humanity, from nuclear weapons to environmental pollution. The scientific experts employed by state agencies and private corporations are routinely treated as hirelings, paid to reassure the public that their organizations can and would do no wrong. Worse, the conduct of research, even within universities, is condemned on ethical grounds as lacking in any humane sensitivity to the interests of its sentient subjects, mainly but not exclusively non-human.

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Now, I am sure that every one of these criticisms can be countered as being misguided, inaccurate or unfair. But at this point in history we seem to lack of conviction that they are all beside the point. We cannot simply dismiss them as impertinent, resting on our assurance that science does not need to justify the details of its conduct or of its consequences to unsympathetic sectarian critics. Under the old social contract, such would have been the defence, all the more effective for being implicit. Now such a point cannot even be stated publicly. What has happened?

Clearly, the image of science, before its various publics, has changed drastically over the last generation. This change can be ascribed to the growth of science, and the problems raised by the applications of its results. For myself, I have been able to understand it through the idea of 'industrialization'. This has several aspects. Most obvious is the union of science with technology, and the great increase in the aggregate size of the scientific enterprise. With these developments, science has become more like industry, and has necessarily and inevitably lost some of its independence and innocence, But the process of industrialization also penetrates into the life of science itself. Formerly scientists were independent craftsmen, whose equipment costs were of an order of magnitude commensurate with their means, or at least with those of a patron. In this respect, their situation approximated to the ideal of 'intermediate technology' as first defined by E.F. Schumacher, Their standing as members of a community then depended on what they did with that equipment, as seen by the quality of their accomplished work. Now, the assessment by 'output' has been seriously modified, for research cannot begin until some funding agency has decided to invest in it. Scientific research is now a capitalintensive enterprise, rather than a craftsmen's community, in this important respect.

Once that science, or even an individual scientist, needs to justify a claim on someone else's resources, then that someone else's values inevitably enter the endeavour. With industrialization has thus come a decisive shift in the balance between knowledge and power in the goals of scientific effort. Formerly, 'science' was devoted to the pursuit of knowledge; it was thereby 'pure' in several senses. The application of that knowledge to power was the task of others; science derived credit for making the means available, and escaped blame when something went wrong. That happy state of affairs is with us no longer. Science, as a socially organized activity, is no longer insulated from the consequences of its applications. The supposed 'neutrality' of scientific knowledge, whose good or evil consequences are the responsibility of the user, has lost plausibility. Now the 'industrialized' scientist usually gets some agency to invest in his research only by promising that its applications will help their missions, commercial or military. Hence the disinterested scientific seeker after truth, ignorant—and hence innocentl—in relation to the morality of the applications of his work, is no longer credible.

Hence, the traditional sorts of power achieved indirectly through science, in the industrial and military fields, reflect their moral ambivalence back on to scientists and science. Worse, some new sorts of power, indeed those that promise to realize some of the greatest humanitarian aspirations of science, show themselves to be even more ambivalent. Here I refer to 'biomedical engineering', achieving ever deeper intervention in human reproduction, disease and life and death. This whole field is characterized by the paradox that each innovation increases the happiness of some client group, and so can be justified in terms of medical ethics. Yet as a whole these developments raise many troubling problems. In the public discussion of these, we now witness an amazing inversion of roles from those in a traditional debate. For, from the time of Galileo through that of Darwin and beyond, 'science' has been displacing 'theology' and 'philosophy' as genuine human knowledge. But now that scientific power has invaded the areas of the private and the sacred, science alone cannot prescribe bounds to what is proper; and moral philosophy and even theology win places at the conference table on ethical

issues in biomedicine. Thus, the powers achieved by science have become compromised in the moral sphere. And still worse, they have produced a new sort of ignorance, something we might even call science-based ignorance, which threatens our very survival as well as our faith in science. For examples, I may remind us of some questions and problems concerning the environment. 'Will there be a "greenhouse effect"?' 'can forest death from acid rain be reversed?'; 'what will happen when the tropical rainforests are destroyed?'; and in the engineering field, 'how can we design a repository for nuclear wastes that will be safe for

10 000 years?' Such grand insoluble questions are paralleled by quite mundane problems that can be classed under industrial reliability and quality control. Thus we may ask how to prevent a repetition of Challenger, Chernobyl, Bhopal, the poisoning of the Rhine, and so on. In *each* case there were identifiable failures in the management of the system; but can all possible failures be prevented in advance by scientific management skills? We do know 'good' management tends to produce a reliable, safe operation; but achieving 'good' management is a problem more in the political and moral sphere than in the scientific or technological. This state of science-based ignorance is revealed to the general public not only in the great disasters, but also in the daily debates over local hazards. Local militancy of the sort more familiar in America or on the Continent has been successful here in England in forcing a complete re-planning of radioactive waste disposal. The official experts, like those monitoring radioactivity from the British Nuclear Fuels Ltd plant, and those monitoring fall out from Chernobyl, were revealed as only partially in command of real scientific knowledge. To us in the universities, the unfortunate technical experts who are interrogated by journalists on TV may not count as real scientists; but to the viewing public they are the scientists who matter. The loss of the aura of objectivity and certainty from such experts then reacts back on 'science' in general.

Returning now to science as we understand it, I must also mention those academic scholars who analyse science, philosophically, sociologically, or whatever. Their consensus filters out to schoolteachers and the public within a couple of decades; and their images then come to dominate public discourse. What do we find there? It is just one generation since Kuhn published his classic *Structure of Scientific Revolutions*; and since then, for the defenders of science, it has been downhill nearly all the way. Increasingly, scholars become more sceptical, more relativist, and more disenchanted with the received verities of science. These are people who, in general, do not support any particular external or social criticism of science; they are content merely to corrode its heart, from within.

In such a context, the writings of eminent scientists about the pleasures of research, or the promise of science for human welfare, can seem like the ramblings of old men about their bygone happy youth. At this time, to study science as a scholar is to criticize, indeed to attack and deny, its past pretensions to merit. Should such developments continue, and there is no sign of their abating, it will become increasingly difficult to find anyone who can make an effective case *for* science to an increasingly disenchanted public. As science needs a champion ever more, he will be ever less likely to appear.

# What Sort of New Image?

I am arguing that the *malaise* of science, its inability to dismiss its enemies and detractors, reflects the obsolescence of the old social contract of science. And with this comes the irrelevance of the old dominant image of science, as the provider, directly, of the True and, indirectly, of the Good. The improvement of the state of science, in its self-confidence, morale and integrity, will require a creative response to its new discussion.

a creative response to its new circumstances. What options are available? The easiest course to follow is to try more of the old mixture, perhaps modernized by some market research into what the public particularly wants. Of course there will be an admission that science does not have all the answers; and that values necessarily enter into policy decisions on technological and industrial questions. But the message will be, that 'science', meaning the activity of the leaders of the research community, is still at the centre of things. Trust them to continue managing, pressure the government to provide them once again with the prestige and perquisites they so sadly miss, and all will be well. To accomplish the enlightenment of the public to appreciate so obvious a message, it only needs more and better-trained schoolteachers, and more and better-disposed journalists (and fewer of those nasty TV investigators). With a complacency befitting just such a cause, this case is advanced by our surviving scientific élite.

The other approach is that of tough realism. There was a famous advertisement in the 1960s, by one of the leading aerospace contractors for the Vietnam War. This displayed the proud motto, 'North American Rockwell, where science gets down to business'—an exquisitely designed ambiguity, so expressive of the current social contract of science. We could say that since the seduction of industrialized science by its external clients, in business and the state, is historically inevitable, why not lie back and enjoy it? Already, the 'pure' research sector has been renamed 'basic', and anyway occupies a shrinking portion of the total effort. How much funding of research is now devoted to sheer scientific curiosity? And certainly, the rate of innovation in key sectors of technology and medicine is evidence that enthusiasm and creativity still flourish.

In this proposed social contract, science becomes the servant of society. Its work can be planned, at least in outline; by negotiation there could be derived the proportions of total societal support to be spent on, say, civil technology, defence, medicine, environment, 'basic', and odds and ends. As such a situation stabilized, new foci of power and prestige would emerge. The old 'pure science' image, corresponding to the old social contract, could be allowed to wither away. Indeed, in the heavily bureaucratized societies, with a scientific tradition deriving from the Académie des Sciences of Paris rather than from the Royal Society of London, such a social contract has been a strong, sometimes dominant pattern. So what would be wrong with it here? One thing wrong is that it is not in our traditions; it presupposes a strong, centralized state which confidently intervenes and directs in many other sectors of civil society. To try to accomplish a complete, self-conscious 'incorporation' of science (I owe this felicitous term to Hilary Rose) in the context of a weaker, self-limiting central state apparatus as in the Anglo-American tradition could produce the sorts of problems of interfering yet ineffective control that plagued the nationalized industries in Britain. Also, it is important in our political and social traditions to have universities, not technical training schools, as the foci of excellence in education and learning; for these to be kept healthy under modern conditions requires that they do their teaching in the atmosphere of research. Furthermore, the experience of the centralized administrations of science, even in the market-economy countries such as France, does not suggest that this 'incorporated' social contract provides all the answers.

More to my present point, which is about the social problems of science, even such an absorption of science into the state would not resolve any of the problems of criticisms and morale. Science could become seen as even more to blame for environmental problems, as it became yet more monolithic in its support, active or passive, of policies of the governments in power.

Let us look forward. Suppose that schoolchildren get a constant diet of criticisms of science—where it is blamed for all our ills—for decades to come. Then a steadily decreasing number will experience that excitement and fascination which is essential if they are to make the choices, and possess the commitments, to enter creative careers as scientists. I have known personally, and I have stated as the cornerstone of my philosophical analysis of science, that that excitement and fascination is both a highly rewarding individual experience and also an element in the social life of science that is necessary for maintaining its health and integrity. Should disillusion and discouragement set in, now in science teaching and then in research, then there could develop a vicious cycle of decay in morale which would be very difficult indeed to break.

Hence, I shall argue that neither a cheap nostalgia, nor an easy acquiescence of present pressures and tendencies, will suffice. What will? I confess that, in detail, I do not know; I cannot provide you with a 'blueprint for survival' for science. But I can offer an analysis of the problem in terms of the social and institutional history of science. On that basis, I can offer some ideas, by way of an invitation to a discussion of possible solutions.

# The Social Constitution of Science

So far I have argued that science, in its present partly industrialized and incorporated state, will not be able to maintain its integrity and its cultural meaning until it achieves a new understanding of itself that coheres with its real situation. This is not merely a matter of passive reflection, for a scientific enterprise that is *merely* the servant of industrial firms and state agencies will *not* command the popular respect and enthusiasm that science needs if it is to remain healthy and vital. Hence, our attempts to achieve an understanding of the present state of science must be guided by our commitment to help the forward evolution of science, through the present into the future, beyond simple industrialization.

It is in this sense that I speak of a new 'social contract': some new appropriate understanding of what science is and how it relates to its context in society. Before this can be accomplished, we must be clear about the present state of science in this respect. As a contribution to such a clarification, I want to suggest certain ideas that may strike you as paradoxical as well as unsettling. I shall argue the following thesis: although the connections between 'science' and 'democracy' are manifold and deep, in some important respects Science retains traces of the time of its origins, and has important features of hierarchy and of absolutism resembling those of the type of Church and of State to which it has always been considered antithetical.

Let me first remind you why this thesis is paradoxical. A democracy of culture was an integral part of the programmes of the prophets who created our modern European science. Indeed, nearly the only positive feature common to Descartes, Galileo and Bacon was an appreciation of the practical knowledge of craftsmen, and a commitment to the unity of that practice with philosophical theory. They were quite explicit on this in their writings. For their lay audience, already using the vernacular for their intellectual work, this may not have been shocking. But for *our* institutional ancestry, the scholars and learned professionals of the universities, it must have seemed to be a degradation of learning, a dilution of culture, with dangerous consequences for knowledge and society.

Then as science began to fulfil its promise of material power over nature, another important connection appeared. It was the *applications* of science that transformed material culture, and then social and political life, so that 'democracy', in our sense, became possible. Norbert Wiener's phrase, 'The human use of human beings', reminds us that, so long as the productive process, on farms or in factories, is such as to make the life of ordinary people 'nasty, brutish and short', there could be no real democracy in society. There may be some *forms* of democracy, and perhaps too some protections of personal liberty; but genuine democracy, where ordinary people have a real share in the power of shaping their lives, is absent or illusory, Hence, as one sees in any developing country, there is a great respect for science as *applied*, for the improvement of the material conditions of life and thereby the eventual achievement of democracy.

There is also a great tradition of popularization of science, frequently led by leading scientists who wanted to share their exciting discoveries, or to enlist a broader public on their side in struggles against the enemies of science. Those were broadly labelled as the promoters of 'dogma, metaphysics and superstition', or theologians, philosophers and priests respectively. They were seen as fostering ignorance and illusion, in the service of outworn institutions. Thus, science had a real relation, however complex, partial and ambivalent, with movements towards greater democracy in society. Popularization enabled people of quite humble origins to feel that they were participating in a great adventure, and indeed sometimes to do so actively as amateurs. Furthermore, science found a large proportion of its most distinguished recruits outside the privileged classes, and so too close an identification with the élite would have been damaging to its own activity. So well diffused was the positive image of science that movements of reform or even of revolution would make their social analyses in the name of 'science' and derive assurance thereby.

Democracy is also inherent in the processes of research science. Research results are (in principle) evaluated without any regard for the personal characteristics or social location of the author. Entry into science, and rewards for excellence, are based on merit, not on personal connections.

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Power in the scientific community is diffused among members (through peerreview of proposals and refereeing for journals); and positions of professional leadership are awarded for excellence and wisdom, rather than for political connections. All this is of course more strongly characteristic of preindustrialized science; and it has provided inspiration for scientists as widely different in their political outlook as Michael Polanyi and J. Desmond Bernal.

In view of all this, it may well seem paradoxical, as well as unsettling, if I say that in some important respects modern science bears strong traces of the times of its origins, when hierarchy in society and absolutism in religion and knowledge were still dominant.

Absolutism and hierarchy—these may seem very inappropriate as descriptions of science. But the points are not new with me. As to absolutism, we find in Kuhn's classic work *Structure of Scientific Revolutions* a vivid description of an absolutist regime in scientific knowledge. The 'paradigm' is the unquestioned, indeed *unquestionable*, framework of current research. To secure its permanence, students are indoctrinated, history is distorted, and difficulties in research practice are, as he says, 'suppressed or evaded'. The world of open criticism and free debate, so prized by Popper in his account of science, is emphatically conspicuous by its absence in Kuhn's picture of 'normal science'. Small wonder that Popper described it as a 'danger to science, and to our civilization', though tending reluctantly to agree with it as a description of science education.

Kuhn's account of the research process has been widely criticized; but no one, to my knowledge, has argued that science education is Popperian, critical and democratic, rather than Kuhnian, dogmatic and absolutist. There *are* some final Honours examinations that include questions with the instruction: 'critically evaluate' a theory; but they are only a minority. In our science teaching, we have a formal curriculum that generally purveys hard incontestable facts; and a hidden curriculum that moulds students' thinking into the ruling assumptions on what sorts of problems, solutions and even ways of analysing problems are 'truly scientific'. This seems to be as absolutist as any doctrines imposed by ecclesiastical or political authorities in the past.

Well, you may say, there *are* some problems in realizing the critical spirit in science teaching. But this teaching, as well as research practice, is uniform and open to all; how could one possibly conceive it as hierarchical? Of course, the form and content of natural science is abstracted from all social considerations. But the practice of science as a social institution cannot be so abstracted. There are enough well-documented accounts of the history of sexism and of racism in research communities that I need not labour the point here. Such unfair practices are indeed regrettable, but is this 'hierarchy'? No; these examples were introduced merely to establish the point that even 'pure' science does not necessarily have a 'pure' social practice.

Hierarchy comes in more subtly, in the dominant assumptions of what is 'real' science, in what institutions and by what people it is done, and also how it relates to the 'less real'. This point does not require political radicals for its expression; for many years we have heard complaints that 'applied science' and 'engineering' enjoy significantly less prestige than 'pure science' in our country. The effects of such differences in status operate in many ways; the less favoured activities tend to accept their inferiority and try to ape their betters. In America, 'physics-envy' is a well-known neurotic disorder of the behavioural sciences.

The perspective here, particularly as seen from the educationalist's viewpoint, is of a pyramid of prestige, with the Royal Society and its special style at the top, and 'technology' somewhere near but not at it. Teaching is orientated towards getting the pupils as high up that pyramid as their effort and talent will take them. The skills of comprehending and controlling one's own personal environment are generally (though with an increasing number of important exceptions) relegated to sub-academic courses in schools, and to independent self-help organizations for adults (tending to reach those whose need is in some ways least severe). Some of us know of the uniformly negative response to requests for funding for development of 'adult science literacy'. This does not mean that there is a conspiracy to keep most adults scientifically illiterate. For none is needed; by the hierarchical assumptions on 'real' science and its social location, there is simply no interesting problem to which 'adult scientific literacy' provides a solution. Science, in the sense of the institution enjoying official prestige and support, is the property of our power and social élite, no less effectively so because the status is implicit and unofficial.

I am far from being the first to recognize this situation. Whenever, in modern times, there has been conflict and instability in relations between the different orders of society, science has been brought into the arena. The rather abstract intellectual democracy proclaimed by the founders of modern science was quite quickly given its limits in the world of real politics. The most famous instance of open conflict occurred here in England in the 1650s, when some of the radical 'Puritans' demanded a democratic education in practical, Paracelsian, Christian natural philosophy for students at Oxford University. In their reply, the future founders of the Royal Society made it very plain that their job was to provide a finishing-school for the sons of the élite; and thus the social location of the new science was explicitly and firmly settled. There were similar exchanges during the French Revolution; and the Lysenko episode in the Soviet Union can be understood, partly at least, in the same light.

All these earlier attempts at 'science for the people' were bound to fail, because there were simply so very few people with sufficient literacy to comprehend, let alone apply, science. These early failures were analogous to those of the campaigns in the political and social spheres, like free elections, abolition of slavery, trades unions, generalized civil liberties and equal civil rights, which were quite Utopian when first proposed, but are now commonplace. Perhaps now, with the widespread diffusion of education and of political activity, the extension of science outside élite culture could in its

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turn cease to be Utopian. I shall now discuss some examples that indicate that this may be starting to happen.

I must make it clear that any significant change in the social character of science will depend on prior changes in many aspects of our social and cultural lives. It is quite beyond my topic here to discuss how such changes could ever come about; hence when I offer these examples, it is only as illustrations of what might take root in the event of some general change in the social relations of knowledge. But it is worthy of reflection that our society is one of those now considered as 'developed', a static condition of perfection to which the rest of humanity aspires, even though roughly a mere quarter of our adolescent population finds it rewarding to remain studying beyond the date of formal release. This is not to propose an even longer incarceration on the American model; but to remind us how the skills of literacy and numeracy are still effectively the possession of a privileged minority.

# Science as Experienced from Outside

As I have said before, the problems of maintaining and enhancing the health and vitality of science cannot be resolved until science as *experienced* by its many publics is in harmony with science as proclaimed by its official leaders and propagandists. Science is still portrayed as essentially 'pure' knowledge when it is now predominantly 'applied' power; and science is still portrayed as thoroughly democratic when it has such strong traces of absolutism and hierarchy. So long as such anomalies persist, science will not again enjoy full public prestige, and the necessary protection that it brings. It will remain vulnerable to sectarian attacks and to criticism and contempt, from *every* quarter in society that has grounds for hostility to some aspect of it.

How can this change? Only by education; but this is to be understood in the widest sense. Obviously those now outside science will need help in developing the skills and the clear understanding necessary for self-confidence, if science is to develop as an integral part of a democratic society. And we on the inside can also benefit from education, perhaps getting some help in seeing ourselves as others see us.

Perhaps the beginning of such a re-education will have to be conducted mainly outside the classrooms. At the start there must be groups of people, acting on their own initiative, independent of, or perhaps even in opposition to, established authorities; forging their own conceptions of science as knowledge and power. Then these can eventually be synthesized, and expressed in a form suitable for teaching. As a contribution towards the enhancement of our perspective on science, I would like to offer three examples. These might be labelled 'alternative', 'activist' and 'practical' science, respectively.

For the first, let me remind you about the state and significance of 'alternative medicine'. If you say, 'but that's not science', you are revealing

your preconceptions of what is really science. Modern 'mainstream' medicine claims, with considerable justification, to be based on science; and traditionally it had adopted just the absolutist, hierarchical style that I have identified for science in general. While not always claiming infallibility for itself in its cures, it has certainly demanded the exclusive power to *decide* what is real and legitimate in the healing arts, and what is not. In all this, its professed basis is science: scientific knowledge as the foundation, and scientific method as the warrant for its claims.

Hence, when a steadily increasing number of people defy the bans and proscriptions on 'alternative' medicine, they are implicitly rejecting the exclusive claims of mainstream medicine in some respect or other: for them either it is not truly scientific, or its idea of science is itself defective. To some extent the latter must be the case, especially when the patient invests her or his practical trust and tentative belief in a treatment whose theoretical basis is utterly at variance with science as we know it, such as acupuncture or homeopathy.

Alternative medicine is a useful example for us in forming a perspective on the evolution of science, for it forces us to think again about what we mean by 'science', in relation to the lives of people and also to its own essential character. For brevity I want to consider another example at the opposite extreme: campaigning by local groups on environmental issues that involves an intimate mixture of science and politics. This is what I mean by 'activist' science. In this case, the character of science is not challenged; but its public manifestation as official expertise is held up to sharp, critical scrutiny.

Such groups, sometimes called NIMBY (Not In My Back Yard), are found world-wide; as yet they have no formal unifying organization or ideology. But their campaigns, assisted by special-interest pressure groups, have already caused important changes in the thinking of industry and government about 'the environment' and its proper care. In this country the movement started with the anti-motorway action groups of the 1960s and 1970s; and it is now most visible with those opposing the storage of dangerous wastes in their neighbourhoods. Up to now, the leadership has come from the USA, where traditions of strong local politics, of citizens' initiatives, and of a helpful judicial system have combined to enable the growth of movements of considerable strength and sophistication.

Through their struggles they have come to their own awareness of what science is, in the context of its employment in the control of technology. The picture is not flattering, but it is important for us to comprehend it, as a symptom of the present difficulties of science, and of the way to their resolution. I shall summarize an article in the journal *Everyone's Back Yard*, published by the Citizens' Clearinghouse for Hazardous Waste Inc., in the issue of Winter 1986. It's called 'Lessons we've learned'; and there are four. The first is that 'science and technical information *alone* will not solve problems', mainly because government agencies would rather *not* know about problems lest they be required to find the money to do something about them. A Hew Social Collinger IOL Science

Then that 'There are only a *few* answers to the *many* scientific questions raised by dump sites', because science out in the raw, confronting disturbed and degraded natural systems, is a totally different thing from science in the teaching or research laboratory. Third that 'often scientists don't admit that they don't know', lest they lose credibility; instead they argue for the 'acceptability' of supposedly 'small' risks. Finally, it was a particularly hard lesson for the author to learn that 'scientists are not objective', but have their biases like anyone else. Perhaps in the old-fashioned lab, where scientists enjoy control over their experiments and are insulated from the economic and political consequences of their work, 'objectivity' is possible. But out in the world of policy, where scientists encounter great uncertainties in their research results *and* experience direct pressures from their employers, they require exceptional strength to withstand the interests that are concerned with power rather than either truth or welfare.

We should notice that this account, unlike some from the extreme 'green' fringe, appreciates that scientists may mean well and do their best. But the new problems of science in the environment, or policy-related research, strip scientists and science of those protections which had previously enabled the endeavour to seem 'pure' in so many ways. Now the innocence is lost, as that of a vanished childhood; the question is whether, or rather how, science can attain a mature understanding of itself in its complex and contradictory social setting. It seems to me that to approach the members of the Citizens' Clearinghouse for Hazardous Waste, or even the clients of alternative medicine, with the standard proposals for more and better schoolteachers and journalists would be somehow missing the point.

Perhaps the most important lesson of the preceding examples was one nearly implicit aspect of them both. This is, that 'science' in each case means something quite different from the activity centred on original research, which we in the universities generally take for granted as defining real science. Alternative medicine is, nearly by definition, not science; some would even call it anti-science. Similarly, debates between hired or partisan experts on the hazards of a rubbish dump may seem best kept quite distinct from what goes on in the university lab. Yet such are examples of people's direct, personal experience of science. Other direct experiences might be in their jobs, where 'science' can make their tasks better, or worse, or perhaps even non-existent; alternatively, in their homes, where it appears as nutrition, gardening, do-ityourself, hobbies, first aid, advice on illness, counselling on medical problems, child psychology, marriage guidance, and so on. Of course hardly any of this 'practical science' is 'science' as understood in the context of British university Honours degree courses. However, some courses at polytechnics include such practical matters; and at American universities all sorts of 'science' can be found. Perhaps we in the universities have in some ways been living in an ivory tower, not being reminded of the differences between our rather precious, esoteric conception of science, and that of the broad public on whose goodwill our survival ultimately depends.

Perhaps in this discovery of the varieties of scientific experience, we can find some clues to the eventual recasting of the social contract of science. The first is that such 'practical science' (as distinct from the 'popular science' purveyed from on high) is neither hierarchical nor absolute. It is mainly a handbook literature, commercially successful where it is felt to be useful, and embodying much disagreement between sources. This 'science' generally lacks institutions for direction, quality control and adjudication of debates. Yet it survives and flourishes, as the background to the more self-conscious, intellectually demanding activities like alternative medicine or environmental campaigning.

Second, there is an increasing continuity of content between such 'practical' materials and syllabuses everywhere outside universities. This is the result of many pressures, not least the need to make science more attractive somehow, so as to keep up the numbers of students. At the same time, the media provide many discussions, at a good intellectual level, of the open-ended problems raised by science, ranging from medical ethics to environmental protection. These are used to good effect, again outside universities, to enliven science teaching and ameliorate its Kuhnian dogmatism. Hence the separation between science as taught more generally, and science as experienced by the public, is far less extreme than the traditional university syllabuses would lead us to believe.

Third, in all this endeavour we witness creativity, and personal growth, in spite of the absence of 'discovery' as defined in establishment science. It is all too easy for scientific discovery itself to become routine, and devoid of, or even inimical to, creativity; such is a very common situation in 'industrialized' scientific research contexts. In this 'practical' science, just as in orthodox science studied as hobby or avocation, lies a resource of creativity and enjoyment which could provide that élan, enthusiasm and commitment without which science of any sort cannot long survive. Finally, all of this 'practical' science has a very important function, only imperfectly realized in institutionalized education, that of enabling people to control their own personal environments and hence their own lives. In this sense it is profoundly democratic.

This large body of literature and practical skills, generally ignored in polite discussions of 'science', offers some important lessons for us. It is not hierarchical, nor absolute, and it is genuinely 'enabling', to use that term in its new sense. Perhaps it is all the more interesting in that it was not designed that way, but just happened. These three sorts of science, the 'alternative', 'activist' and 'practical', are only roughly sketched examples. In one obvious sense they are not 'science'. But why not? They all involve investigations of Nature, for human understanding and control; and that is as good a definition as any. Of course, they are not disciplined research, and so they do not yield the sort of knowledge as a social possession, that we ordinarily consider to be science. I would only say this: perhaps our definitions are in need of revision, so that we could overcome the barriers, social, cultural and intellectual, between our

#### A New Social Contract for Science

mainstream science, with its tendencies to hierarchy and absolutism, and these other sorts of endeavour.

## **Conclusion and Perspective**

Through all this I have preferred to cite examples rather than to articulate theories. This has had a double use; it has (I hope) made the matter more comprehensible and interesting; and it has also enabled the argument to proceed in spite of the rudimentary state of development of my theoretical ideas. As I have said, this is only an invitation to explore a problem.

Hence, here I can be quite modest in my claims for these other forms of experience of science, including 'alternative', 'activist' and 'practical' science. I need not claim that these are a panacea for our problems of education and of science. I doubt that they are. But they can serve as suggestive examples of resources, and of activities, whose significance has hitherto been insufficiently appreciated.

The main function of my examples is to remind us of the possible usefulness of *diversity* in any new social contract for science. Rather than a pyramid of prestige, explicitly defining what is real and valid, and implicitly defining what is not, we could enjoy a diversity of activities and experiences. Each would have its appropriate institutions and images of science, and its appropriate publics. Some would be very similar to those we have now, serving 'basic' or 'industrial' research; others (as we have seen) could relate to education, leisure, health or politics. In society at large, both religion and politics survived the transition from hierarchy and absolutism to diversified, more democratic forms. Perhaps, some centuries later, science will soon manage it too. Such could be the basic idea of a 'new social contract for science'.

Let me now recapitulate briefly. Over the previous centuries, science enjoyed a 'social contract' whereby it obtained societal support and protection. Until recently, its patrons were largely *within* the élite section of society, though the image of science always and necessarily had a broader appeal. In these modern times, with its industrialization, science has been transformed both as a social activity and in its social contract. This new state is not stable, nor is it one in which science can easily flourish. The next change in the social contract *may* involve only some shuffling among the various state and corporate patrons and paymasters, accompanied by some putting out of more flags for science. Or we could engage on a really new look at science in society, the sort of self-scrutiny that becomes possible when, and only when, complacency is shaken and the scientific community's leaders do not know who are their friends, if any.

In this unsettled and therefore potentially creative situation, we can look again at science, and think again about its future. I hope that the perspectives I have offered, on the industrialized state of science, the present remnants of hierarchy and absolutism in science, and the diversity of perspectives and activities in science, can provide materials for a discussion of the shape of a new social contract for science.

Finally, let me briefly defend my style of argument, of offering examples rather than advancing a theory and a plan. For some, this may well be disappointing, as if I am shirking my duty to argue in a systematic, scientific way about this important problem. As I have already indicated, this approach seems to me to be coherent with my conception of any new social contract for science, and of its means of achievement. For this I have an example from recent personal experience, in the way that in the People's Republic of China the government and Party organize their discussions and activities towards the creation of a new society. For them it is an accepted and public fact that they are as yet ignorant of the character of their desired state, and of the means for achieving it. They *expect* to make mistakes, and to need to retrace their steps along the path. Such honesty, and the philosophical perspective underlying it, can provide us with the occasion for useful reflection on the knowledge achieved by science, now and in its possible new social contracts.

The Chinese also have a valuable perspective on themselves: they know that their nation is poor, and that their culture has many deficiencies. I almost said 'underdeveloped', in contrast to our supposedly 'developed' state. Certainly the rest of the world sees us as 'developed', essentially as having arrived and with nowhere to go. Perhaps that illusion of perfection is at the root of some of our present ills.

Suppose that we accept that our society is still very 'underdeveloped' culturally; and that the continued absolutist and hierarchical character of science is one manifestation of our backward state. It is difficult to imagine 'science of the people' as things are now; any detailed scheme is necessarily Utopian, and any practical initiative must be small scale and tentative. But with such a realistic humility about ourselves, analogous to that of the Chinese, we at least have a hope of proceeding forward with facts rather than fantasies.

This Chinese attitude is not a perennial, unchanging Oriental wisdom. Only a few decades ago the leaders of China were sure that they had a science of society which provided all the correct answers to their problems; and then they lurched from crises to catastrophes. Their version of Marxism was, like so many others of its time, both absolute and hierarchical, just like the image of natural science on which it was modelled. Now, through all their very real, passionate debates on extremely difficult problems, they know that free discussion and diversified experimentation are their only security against another disaster.

In the same spirit, I could remind us that the absolute, hierarchical character of science under its old social contract has given us a very one-sided sort of progress; and that the myopic, hubristic attitudes it has fostered among scientists and experts have brought us to the very brink of ecological disaster. If we are to think about a new conception of science appropriate to the future,

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then I would rather start with an awareness of our ignorance, mine as much as anyone else's. Otherwise, the sins of scientific pride may be our final undoing, both as members of a scientific community in a social context, and as members of a total civilization, which will live or die with its science.

Based on an invited public lecture at the University of Leeds, March 1987; an edited version of that text was published in the Bulletin of Science, Technology and Society 8, 1988, 20-30.

# Note Added in Proof

My comments about China were based on my experiences there through the first student demonstrations in 1986. This essay had gone to press when the tragedies of June 1989 occurred. I have decided to leave the text unchanged, partly as a reminder of my own fallibility, and partly as a gesture of goodwill to the Chinese people, hoping that even now progress must continue.

# Science: Orthodoxies, Critiques and Alternatives

The concept of 'alternative science' has been current for a very brief period. about two decades at most; hence an historical survey of the movement lacks the normal preconditions in prior scholarly productions and separation in time. But, however recent, it is of great importance for any projection of the shape of science in the future; and the period in which the idea was born, the 1960s, is definitely in the past. Also, a genuine history, rather than a mere chronicle, is made possible by the essential feature of this movement: its roots lie in the establishment of our sort of science in the seventeenth century. Its ideology was then given a very clear expression, partly in programmatic terms and partly in contrast to other conceptions of natural knowledge then prevalent. The contradictions within that ideology, some latent and others then capable of resolution, could subsequently, with the advance of science, be suppressed or ignored. With the recent full maturing of science in its organization, effectiveness and power, these contradictions have become manifest. This explains the apparent paradox that in a period of the greatest triumphs of science, its opponents became most strident and effective. Out of the movement of criticism on all issues, new foci of practice and reflection have come to exist and to find stable niches in society. These are what we call 'alternatives'.

#### Early Contradictions and Their Resolution

The early vision of modern science was explicitly millenarian in Bacon; and implicitly so (within the limits of their respective styles) in Galileo and Descartes. From their writings, we may distil the prophetic message: that through the study of an abstract aspect of nature, with a style of enquiry that was alienated from its object but open to all persons, error would be banished, ignorance abolished, and truths easily achieved that would be powerful, beneficial and safe. Thus a straight and narrow path of enquiry into Nature

#### Science: Orthodoxles, Critiques and Alternatives

was to be the gateway to the material and moral redemption of mankind.

For analysing the contradictions in that programme, we may rephrase it in terms of certain themes. That is, this style of science promised the security of gaining truth (and avoiding error) through discovery within a particular reality; its social practice was one of openness (to all participants and also in its results); to its external patrons it promised ideological innocence in its teachings and the practical beneficence of its powers in application. All this is an ideology; and it was an essential part of the endeavour, in the Scientific Revolution and for some three centuries afterwards. The aspect of the ideology of science that was later to become its greatest strength, security, was the weakest point in the early programme. Galileo's attempt at a scientific proof of the Copernican system failed disastrously; Descartes' general physics was obviously speculative; and Bacon successfully induced very little indeed, Nor did the initial protestations of innocence carry sufficient weight, particularly with those Roman Catholic authorities who had cause for concern. The claims of openness were more successful, although (perhaps because of) being restricted to the more polite orders of society.

The problems of reality also solved themselves; although some of the great earlier discoveries of modern science (such as those of Kepler, Gilbert and Harvey) were made within the framework of 'animated' world-views, the accelerating secular change in common-sense consciousness soon made such 'alternative' world-pictures implausible and obsolete. The progress of discovery within the new paradigm, in the seventeenth century and beyond, seemed to guarantee beyond doubt that this is the one and only secure way to the True. Although the practical beneficence of the new science took a long time to materialize, it seems that its public were generally prepared to take that on trust. Jonathan Swift's portrait of addled natural philosophers and corrupt 'projectors' of Laputa (in Gulliver's Travels) was only part of his general denunciation of secularized eighteenth century high society. The powers of the new science also had a quality of innocence: with the decline of the magical arts, there were no longer secrets too powerful to be revealed. All effects were proportionable to their natural causes, and so the idea of science producing real evil was nearly a logical impossibility, until our own times.

## Early Challenges, Resolved and Unresolved

Thus did the ideology of modern science gain its form, and increase steadily in strength through the eighteenth and nineteenth centuries. One of the greatest strengths of that ideology was that it saw science as simple and absolute, the antithesis to mere belief or to 'ideology' itself. The earliest conflicts involving science were easy victories. The perennial struggle about openness surfaced in the French Revolution, with vain complaints that Lavoisier's chemical nomenclature made a barrier against all those artisans who lacked the erudition to master his classicisms. The issue of reality erupted with Naturphilosophie; and with its downfall, the hardest of world-views generally ruled supreme. The triumph of Darwinism was due only in part to the overwhelming weight of his separately inconclusive arguments; equally it was the conviction of his audience that no other sort of explanation could be 'scientific'.

By the sort of double-think that is possible only within a well-established ideology, science's propagandists could continue to proclaim its innocence (as the vehicle of simple truth) while vigorously attacking what for the unlettered majority of people was the foundation of their personal morality: religion revealed through sacred texts. The beneficence of science was equally secure; while the propagandists of industrialization lauded science as their own, those who spoke for the suffering masses were equally determined to enlist it; thus Marx called his the 'scientific' socialism, which would replace the futile 'Utopian' varieties.

The security of scientific knowledge grew to the point of becoming a new dogma. Those who debated such questions as the nature of 'force' in the eighteenth century, or of infinity in projective geometry, or atomism in chemistry in the nineteenth century never doubted that there was a unique true solution. Outsiders who criticized the foundations of a science, such as Bishop Berkeley on the calculus, were dismissed as not possibly being really serious. Even the great 'critical' philosopher Kant took Newton's mechanics, along with Euclid's geometry, as the necessary framework for our experience of the world.

By the later nineteenth century some independent spirits were beginning to uncover obscurities and contradictions at its base. Their intent was not at all destructive; they wished only to strengthen science against certain weaknesses that had developed through its years of easy triumphs. But directly and indirectly they prepared the groundwork for the revolutions, philosophical and scientific, of the next century. Ernst Mach's critical history of mechanics (1883) showed that Newton's idea of 'force' was confused and anthropomorphic, his 'mass' was incomprehensible, and 'absolute space' non-scientific. Thus, for nearly two hundred years scientists had been living in an illusion of security; their paradigm science could then be seen to be resting on very shaky conceptual foundations. Similar developments afflicted mathematics. Non-Euclidean geometries created a schism between 'intuition' and mathematical truth; while a series of interrelated developments in theories of sets, of infinite numbers and aggregates, and of logic, led to a full-blown 'foundations crisis' at the century's end.

Within the space of a very few years, Albert Einstein made discoveries which would soon revolutionize the foundations of the world-picture of physics, and also of scientific truth; hence this greatest triumph of discovery would fatally weaken the traditional security of science. The combination of his theoretical work with that of the revolutionary 'atomic physics' eventually led to the

atomic bomb, which shattered the beneficence of science as well. The first philosopher to appreciate the full significance of Einstein was

Popper; with his 'falsificationism' he jettisoned the True of science to save the

Good, as realized through the intellectual integrity of the (legendary) Einstein who in 1919 dared the world to prove him wrong (Popper 1963). Popper was far ahead of his time; through three decades of his career he witnessed the dominance of the last 'triumphalist' philosophy of science. This was logical positivism, born in anti-clerical struggles in Vienna, and transplanted by refugees in ideologically neutralized form to the English-speaking world. By the time Popper came into prominence, his message for science was obsolescent. The revolution within philosophy of physics of the earlier twentieth century had given way to a revolution of consciousness and experiences, in which the old ideology of science was a principal object of rejection and contempt.

# The Radical Critique of the 1960s

Although the millenarian aspirations of the 1960s, in politics and in experience, are now reduced to an object of historical study, the permanent changes achieved then should not be underestimated. The concept 'alternative', including science, is a mark of these. The conditions for that revolution in consciousness were multiple. First, there was a new class, of 'affluent' youth, enjoying incomes to spend and markets organized around their desires. They were also free of the bondages of parental control, of fear of poverty, and of ambition for advancement. They could cultivate new experiences ranging over idealistic politics, communal life-styles, intense aesthetic experience, and altered states of consciousness. In relation to science, this 'counter-culture' was full of contradictions. Its devotees would cheerfully utilize all its benefits, including the standard equipment of post-war consumerism, high-technology music and synthetic mind-expanding drugs. Yet on the ideological plane science was a prime focus for their attack. All the contradictions in the ideology of science that had been latent through the centuries of triumph now became manifest.

Developments in philosophy of science were at first unrelated to the 'counter-culture', but they soon interacted. Motivated by his disillusion with the standard 'accumulationist' vision of science, T.S. Kuhn produced his epochal *Structure of Scientific Revolutions* (1962). This was so influential perhaps because of its confusions, ambiguities and ironies. Its effective message was of a science whose content is strongly 'arbitrary', 'where 'progress' consists of an alternation between anti-critical puzzle-solving within paradigms, and anti-rational combats between paradigms. In vain did Popper protest that Kuhn's 'normal [*sic*] science' is a menace to civilization; equally vainly did Lakatos try to blend Popperian idealism with Kuhnian realism in his 'methodology of scientific research programmes' (Lakatos and Musgrave, 1970). The security of science was lost, irretrievably, for some generations to come.

The executioner of scientism was Paul Feyerabend, who in Against Method

(1975) showed that for every principle of method or even of intellectual integrity, there was a violation committed by some great scientist, usually Galileo. Although his professed message was 'playful anarchism' he formed the link between epistemology and radical activism. He had been in Berkeley in the late 1960s, experienced cultural imperalism in the classrooms and also benefited from 'alternative medicine'. Thenceforth, for him science was a white, male, middle-class racket, protecting itself by a dogmatic orthodoxy as intolerant as any other in history.

Although Feyerabend was in a small minority among philosophers of science, his message of denial of the beneficence of science had already been expounded on many fronts. Ecological consciousness among the reading public was created suddenly with Rachel Carson's Silent Spring (1963); and within a remarkably few years, the American government had environmental legislation drafted and enacted. More radical ecological messages came from Paul Ehrlich, with his Population Bomb (1968), and Barry Commoner, with Science and Survival (1966), who blamed post-war high-technology consumerism rather than just people. Most radical of all was the communalist-Christian Ivan Illich, in his broadside attacks on all the institutions of Western science-based intellectual culture; these included De-Schooling Society (1971), Energy and Equity (1974) and Medical Nemesis (1975). In a more practical vein, E.F. Schumacher showed that 'aid' to the poor nations was counter-productive, materially as well as ethically. His vision was of 'intermediate' (later 'appropriate') technology, described as Small Is Beautiful (1973) but founded on his 'Buddhist economics' conception of the meaning of work and ultimately on his own private religious experience.

With the beneficence of science falling into disrepute, its innocence could not be far behind. It was in the public record that with the A-bomb, science had tasted sin, and that with the H-bomb it had found it sweet. The evil and insanity of nuclear 'deterrence' were appreciated by only an eccentric few until the Cuba crisis of 1963; thenceforth this greatest production of the scholars brought back visions of the sorcerer's apprentice, and worse. The complicity of American science in some of the most reprehensible dirty tricks of the dirty Vietnam War was signalled by dissident students and researchers, culminating in a one-day research strike at MIT itself. And even within the world of 'pure science', the image of the slightly eccentric other-worldly searcher of oldfashioned academic science gave way, in the age of industrialized science, to 'Professor Grant Swinger' (immortalized by Dan Greenberg in Science magazine) (1969), and the real-life swashbuckling opportunist Jim Watson. Some fifteen years after the great event, Watson cheerfully revealed the squalid side of his Nobel prize-winning achievement (1968). Further, problems of quality control, with the implication that many scientists will not or cannot do work of adequate quality, have intruded into the governing of science in an age of restricted support; and there has been no shortage of cases of flagrant, even flamboyant, fraud and plagiarism in prestigious fields and institutions.

This loss of innocence also affected scholarly reflection on science as a human activity. Up to the 1960s, historians of science, as led by such as Sarton, and sociologists of science, as led by such as Merton, were at one with the great popularizers and propagandists in presenting a picture of science, and of scientists as well, in which anything but the Good and the True, in consequences and in behaviour, was nearly inconceivable. But, after the messages of Kuhn and Feyerabend had been assimilated, historians eagerly lifted the lid off questionable scientific practices among the great, so that the situation was eventually summed up in a classic paper, 'Should history of science be X-rated?' by Steven Brush (1974). I attempted to comprehend the positive and negative aspects of science as a social activity, combining a Polanyi-ite theory of craft knowledge of research with a Marxist conception of 'industrialized science', and concluding with a call for a 'critical science' (Ravetz 1971).

A new generation of epistemologically radical social scientists soon found their target in the old faith that science proceeds by *discovery* of something objective out there. Scientific knowledge was shown to be the product of social construction, of negotiation among interests, or to be merely 'relative' to a professional consensus, or capable of being illuminated by the approach of cultural anthropology (the seminal work in 'the scientist as aboriginal' being *Laboratory Life* by Latour and Woolgar (1977)). The collapse of the old positivistic faith among philosophers of science was complete by the end of the 1970s: though of course there would always be those in the mathematical-behavioural sciences who had not heard of Kuhn any more than they had of Heisenberg.

The inherited ideas on discovery in science were further eroded by the movements of environmental activism that got under way in the later 1960s. Hitherto, no one had seriously considered the prospect of the impotence of science as worthy of serious reflection. To be sure, in previous generations the limits of our knowledge, as of disease, had been painfully obvious; but there was the sense that the progress of knowledge would eventually eliminate all such ignorance. But with the environmental crises of modern times, a new category appeared, which we may call science-based ignorance. The new technologies, particularly nuclear power, created problems of risks and pollution for which no available body of scientific knowledge was adequate. The great statesman of nuclear engineering in America, Alvin Weinberg, exposed the problem with his paper on 'trans-science' (1972). For this his paradigm case was the determination of the number of mice necessary for the assurance of the safety of environmental radiation at federal standards: some eight billion ( $8 \times 10^9$ ) would be required. Other problems, such as those called 'zero-infinity risks', and (again from Weinberg) 'Faustian bargains' in which future generations are to cope with our pollutants, have emphasized the radical insufficiency of the scientific inputs to urgent policy issues. The contradictions are both cognitive and social. On risks questions, the official task of scientific reassurance is either to prove the impossibility of the undesired event, which is logically impossible; or to prove its 'acceptability' to a suspicious

public, which is practically impossible. Worse, the awareness of technologies that are 'unforgiving' or 'brittle' spread more quickly among protestors and critical scientists than among designers and expert-apologists. Finally, the prevalence of very ordinary weaknesses of morale and discipline among managers and operatives in extraordinarily sensitive and dangerous installations deprived such enterprises of all credibility among their critically concerned publics.

Environmental politics also punctured another element of the old faith of science, that of its openness. For in such struggles, only a part of the relevant information is 'public knowledge', produced by academic scientists whose rewards are derived through the conventions of citation by others. Crucial information will be 'corporate know-how': data on processes or pollutions which are the property of institutions, private or state. In this sort of contested science, the art is to provide non-information, dis-information, misinformation, anything but the real thing, to those standing in the way of this particular manifestation of progress. Even within the traditional university research sector, the 'open society' of science is in retreat, as more funding for research comes in contracts rather than in grants and (as in fields like biotechnology) scientists become inventors and entrepreneurs as well as discoverers. Other aspects of the traditional openness of science have also failed the test of critical scrutiny. Entry or advancement has been no more immune to the effects of prejudices based on class, race or sex than in other fields of human endeavour. Even if such regrettable practices are now less tolerated than in the past, their becoming known represents a change in the public image, the self-image and the ideology of science. These are themselves as real, and as important for the activity, as the social practices that they reflect.

Reality itself came up for effective questioning in the 1960s, for the first time in several centuries. This was not then in the form of a competing research programme, or paradigm, for mainstream science itself. Rather, altered states of consciousness, made possible on a mass scale by the achievements of modern chemical science, were invoked in a challenge to the billiard-ball universe that constitutes the metaphysical orthodoxy of science. This formed the basis for a wide-ranging critique of the supposed inhumanity and corruption of the modern scientific enterprise, in the name of Roszak's 'counter-culture' (1969). In such an intellectual environment, venerable pseudo-sciences moved in from the margins of respectability, to capture the interest and commitment of even the best-educated young people.

Thus, in that decade of the 1960s, many aspects of science that were previously unquestionable were subjected to criticism, on a large scale, in public, and to some extent from within the community that supplies science with its recruits and with its principal audience and social support. One decade of convulsions in the realm of ideas is far from sufficient to effect a rapid radical change in the large-scale social enterprise to which they relate. But in spite of the subsiding of the ferment of the 1960s, many of the ideas that achieved plausibility and power then have survived, maintaining a stable

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existence on the margins, some remote but some quite close, to the mainstream of the contemporary scientific-technical enterprise.

# Some Effective 'Alternative' Approaches

Even during the 1960s, there was a variety of positive, practical initiatives devoted to resolving particular problems revealed in the general critique. These took permanent shape during the following decade, along with critical movements that appeared quite suddenly at the end of the decade of ferment (such as radical feminism); and now there is a goodly spread of stable, partly institutionalized activities that in one way or another can be called 'alternative'.

The least impact on science has been made by the more traditional socialist, or Marxist, critique. To see how the 'development of the means of production' can be systematically evil (as in warfare and pollution) requires a perspective not to be found in the Marxist canon; and the continued failure of the established socialist societies to provide an example of success in science could not but weaken the force of the Marxist critique of capitalist science. The quaintly named 'British Society for Social Responsibility in Science', which quite rapidly transformed itself from a club of left-of-centre academics to a ginger-group of young radicals, settled down to providing a valuable service in the field of occupational hazards, and also in providing a base for young professionals protesting the incompetence and corruption of their established state-welfare institutions. But there never appeared a mass base, or even an effective organized constituency, in any of the groups to which such a movement necessarily appeals. The contradiction of a movement for the workers which was not by the workers was never resolved. The movement for 'alternative technology' did not fare much better in terms of recruits and successful designs. Windmills and methane digesters could not fit in with modern industrial systems; and industrial process that were small-scale, nonpolluting, humane and profitable have been elusive in practice.

By contrast, the issue of 'the environment' has found a broad and stable constituency, though not as yet a single mass institutional base. The issue is well expressed by American acronyms: NIMBY (Not In My Back Yard) groups opposing LULUs (Locally Unwanted Land Uses). These have all the strengths, and weaknesses, of special-interest activist movements. For them, the beneficence and openness of science are in discredit; as well as the innocence and integrity of the corporate 'experts', where their local interests are affected. They derive much of their strength from ideologically committed national pressure groups, such as Friends of the Earth, or (in the USA) the Citizens' Clearinghouse for Hazardous Wastes, Inc. An essential element in their struggles is a new sort of 'scientific discovery': that of investigative journalism, usually TV, that exposes the callous inhumanity of selected corporate offenders and the impotence or complicity of state regulatory agencies. Local 'environmental' campaigns are symbiotic with a militant 'ecological' movement, which interprets high-technology catastrophes (recently, Bhopal, Challenger, Chernobyl, the Rhine poisoning) as symptoms of a deep sickness in the style and values of modern science-based civilization. Through magazines (such as *The Ecologist*) and activist groups (such as Greenpeace) they drive home the message of the corruption of established science, be it on the whales, civil nuclear power, or the tropical rainforests. Their positive programme calls for a transformation of life-styles and values, along the lines of mystical-communitarian prophets such as Gandhi and Schumacher. As yet they have an effective political base only in West Germany; but unless the problems they address are either resolved or are overwhelmed by much worse ones, they will not go away.

In response to the ecologists' political challenge, a cynical analysis is that there are no votes in sewage. But there are votes in the home, where children, growing or as yet unborn, are exposed to insidious hazards. Through such issues, women's movements escape the contradictions inherent in their standard complaints about science: is it bad because it discriminates against women, or is it the sort of sexist, soulless grind that no sensitive person would want to go into anyway? 'Housewives' epidemiology' uses disciplined methods, sometimes quite inventive, to supplement and expose official statistics that show 'no evidence of harm' from suspected pollutants. Although on a relatively small scale as yet (after the first flush of enthusiasm in the 1970s) women's 'selfhealth' groups constitute a radical alternative to prevailing medical ideas about what is significant, and what is 'normal', in the functions and problems of women's bodies. In that sense, they are unavoidably political; and to the extent that they make the subjective feeling of being a woman into a self-aware and shared experience, they plant the seeds for a demystification of maledominated knowledge and ways of knowing, of which modern science is the paradigm case.

The success of 'alternative' approaches is perhaps best seen in medicine. Largely through the triumphs of bacteriological medicine (perhaps owing more than is generally admitted to soap, sewers and window-screens), the classic infectious diseases of temperative climates have been brought under control. Now health hazards are known to relate as much to life-style as to 'germs'. The legendary ancient Chinese principle of paying a doctor to keep one healthy is reflected in the American Health Maintenance Organizations. Psychogenic disease, forgotten for some centuries, has become respectable again. Different approaches to healing, until very recently dismissed and denounced as the province of charlatans and quacks, are now given grudging respect for their accomplishments if not for their theories; such are homeopathy, herbalism, chiropractic and acupuncture. This last, involving the manipulation of chi energy, may be a meeting-point for orthodox and alternatives, as for East and West. Practitioners and researchers, in China and elsewhere, apply a scientific approach to the study of *chi*, and let the two styles complement each other in a single course of therapy.

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# Science: Orthodoxies, Critiques and Alternatives

All such developments are still on the margins of regular medical practice: and as marginal activities they are conducted in a very different social style They are more 'open' not only in the sense of presenting fewer barriers in the form of lengthy training, but also in exhibiting none of the exclusiveness that the 'medical sects' of earlier times employed to maintain their shreds of prestige. The openness extends to varieties of the healing art that are 'alternative' in the extreme; indeed some which in England had been classed as witchcraft until the 1950s. Healing by laying on of hands, with or without contact, and with or without theories of orthodox religion or of unorthodox spirituality, is now regularly administered by some thousands of persons. It is of course possible that their achievements will follow on those of chi energy in being explained within a slightly enriched scientific world-picture. But in the meantime, such a practice constitutes a challenge to the reality defined by the prophets of the scientific revolution, and accepted unquestioningly in the world of science ever since. It is all the more effective for being quiet. non-antagonistic, and outwardly consistent with any life-style or medical treatment. Its practitioners and clients need not think of themselves as metaphysical revolutionaries; individually, they believe themselves simply to be giving and receiving help. It is thereby less vulnerable to being outlawed on the one hand, or to being commercialized or co-opted on the other. But given its cultural context it is likely that its adherents will need to learn all over again that even 'spirituality' can be as materialistic as any other attachment.

With this last activity we have come a long way from what is currently accepted as 'science' in any sense of the term. But the challenge raised by the 'alternative' approaches is that the prevalent idea of science is itself a product of history. In that history, coinciding with the course of modern European civilization, the original contradictions, so long latent under all the successes, have now matured and become manifest. What sorts of interactions eventually develop between orthodox science, its critics, and its alternative approaches, will be for future historians to study. But we can be sure that any new orthodoxy will never be the same as in its triumphalist centuries up to the middle of our own.

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# Towards a Critical Science

We can now permit ourselves some final speculations on possible trends in the future of the natural sciences. The process of industrialization is irreversible; and the innocence of academic science cannot be regained. The resolution of the social problems of science created by its industrialization will depend very strongly on the particular circumstances and traditions of each field in each nation. Where morale and effective leadership can be maintained under the new conditions, we may see entire fields adjusting successfully to them, and producing work which is both worthwhile as science and useful as a contribution to technology. Recruits to this sort of science will see it as a career only marginally different from any other open to them; and it is not impossible for men of ability and integrity to rise to leadership in such an environment. This thoroughly industrialized science will necessarily become the major part of the scientific enterprise, sharing resources with a few high-prestige fields of 'undirected' research, and allowing some crumbs for the remnants of smallscale individual research. A frank recognition of this situation will help in the solution of the problems of decision and control. Since the criteria of assessment of quality will be heavily biased towards possible technical functions of results, they will thereby be more easily applied, and less subject to abuse, than those which are based on the imponderable 'internal' components of value.

Thus, provided that the crises in recruitment and morale do not lead to the degeneration and corruption of whole fields, we can expect the emergence of a stable, thoroughly industrialized natural science, responsible to society at large through its contribution to the solution of the technical problems set by industry and the state. Scientists, and their leaders and institutions, will be 'tame': accepting their dependence and their responsibilities, they will be unlikely to engage in, or encourage, public criticisms of the policies of those institutions that support their research and employ their graduates. Such a policy of prudence is not necessarily corruption; whether it becomes so will depend on many subtle factors in the self-consciousness of this new sort of science, and the claims made to its audiences. But not all the members of any group are easily tamed, and the emergence of a 'critical science', as a selfconscious and coherent force, is one of the most significant and hopeful developments of the present period.

There have always been natural scientists concerned with the sufferings of humanity; but with very few exceptions they have faced the alternatives of doing irrelevant academic research to gain the leisure and freedom for their social campaigns, or doing applied research which could benefit humanity only if it first produced profits for their industrial employer. The results of pharmaceutical research must pass through the cash nexus of that industry before being applied, and that process may on occasion be an unsavoury one. Only in the fields related to 'social medicine' could genuine scientific research make a direct contribution to the solution of practical problems, of protecting the health and welfare of an otherwise defenceless public. Now, however, the threats to human welfare and survival made by the runaway technology of the present provide opportunities for such beneficial research in a wide range of fields; and the problems there are at least as difficult and challenging as any in academic science. These new problems do more than provide opportunities for scientific research with humanitarian functions. The response to this peril is rapidly creating a new sort of science: critical science. Instead of isolated individuals sacrificing their leisure and interrupting their regular research for engagement in practical problems, we now see the emergence of scientific schools of a new sort. In them, collaborative research of the highest quality is done, as part of practical projects involving the discovery, analysis and criticism of the different sorts of damage inflicted on man and nature by runaway technology, followed by their public exposure and campaigns for their abolition. The honour of creating the first school of 'critical science' belongs to Professor Barry Commoner and his colleagues at Washington University, St Louis, together with the Committee for Environmental Information, which publishes Environment.

The problem-situations which critical science investigates are not necessarily the result of deliberate attempts to poison the environment. But they result from practices whose correction will involve inconvenience and money cost; and the interests involved may be those of powerful groups of firms, or agencies of the state itself. The work of scientific enquiry is largely futile unless it is followed up by exposure and campaigning; and hence critical science is inevitably and essentially political. Its style of politics is not that of the modern mass movements or even that of 'pressure groups' representing a particular constituency with a distinct set of interests; it is more like the politics of the Enlightenment, where a small minority uses reason, argument, and a mixture of political tactics to arouse a public concern on matters of human welfare. The opponents of critical science will usually be bureaucratic institutions which try to remain faceless, pushing their tame experts, and hired advocates and image-projectors, into the line of battle; although occasionally a very distinguished man is exposed as more irresponsible than he would care to admit.

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In the struggles for the exposure and correction of practices damaging to humanity and the environment, the role of the state is ambiguous. On the one hand, every modern government is committed in principle to the protection of the health of its people and the conservation of its natural resources. But many of the agencies committing the worst outrages are state institutions, especially the military; and in any event the powerful interests which derive profit or convenience from polluting and degrading the environment have more political and economic power than a scattering of 'conservationists'. It sometimes occurs that two state agencies will be on opposite sides of an environmental struggle; but the natural tendency of regulatory agencies to come under the control of those they are supposed to regulate can make such a struggle a one-sided affair.

The presence of an effective critical science is naturally an embarrassment to the leadership of the responsible, industrialized, tame scientific establishment. Their natural (and sincere) reaction is to accuse the critics of being negative and irresponsible; and their defensive slogan is along the lines of 'technology creates problems, which technology can solve'. This is not strictly true in all cases, since nothing will solve the problems of the children already killed or deformed by radioactive fall-out or by the drug thalidomide. Moreover, this claim carries the implication that 'technology' is an autonomous and self-correcting process. This is patent nonsense. We have already seen that a new device is produced and diffused only if it performs certain functions whereby human purposes can be served; and if the intended beneficiaries do not appreciate its use, or if those injured by its working can stop it. the device will be stillborn. The distortions of technological development arise when the only effective 'purposes' in the situation are those of the people who believe themselves to derive pure benefit from the innovation. On the selfcorrecting tendency of technology, one might argue that no large and responsible institution would continue harmful practices once they had been recognized; but this generalization is analogous to the traditional denial of the cruelty of slavery, along the lines that no sensible man would maltreat such valuable pieces of property. And the history of the struggles for public health and against pollution, from their inception to the present, shows that the guilty institutions and groups of people will usually fight by every means available to prevent their immediate interests being sacrificed to some unproven public benefit. If the campaigns waged by critical science come to touch on some issue central to the convenience of the state or other very powerful institutions, we may yet experience a polarization of the community of natural science, along the same lines as occurred on the Vietnam issue in some of the human sciences in America. In such a situation, it will not be possible for a leader of science to be both honest and tame; and if an establishment within science chooses to serve its paymasters rather than truth, it will be recognizably corrupt.

Such extreme situations may be a long time in developing, if for nothing else than that critical science is still in its infancy. As it develops, it will be at risk of

applied to practical problems, and partly those of radical and reforming political movements. Perhaps the most obvious will be an accretion of cranks and congenital rebels, whose reforming zeal is not matched by their scientific skill. But there are others, arising from the contradictory relations between critical science and the relevant established institutions of society. As true intellectuals rather than a technical intelligentsia, individual members may find some sinecures within the interstices of bureaucratized intellectual systems; but there will need to be some institutions providing a home for the nucleus of each school, and external sources of funds for research. Hence, especially as critical science grows in size and influence and society becomes more sophisticated about the problems of runaway technology, some accommodation between the critics and the criticized will inevitably develop. We can even expect to see critical research being supported, critical slogans being echoed, and leaders of critical science being rewarded by institutions whose basic destructive policies still are unchanged. Such phenomena have already occurred in the USA, in the politics of race; and on this issue, where the interests concerned are mainly major institutions which can hire talented and enlightened experts at will, it is even more likely. The movement of critical science would then face the pitfalls of corruption as soon as, or even before, it had skirted those of impotence. But this is only a natural process, characteristic of all radical movements. It is easy to maintain one's integrity when one's words and actions are ineffective; but a long period of this can produce a sectarian or a crank. If one begins to achieve power, and one's policies affect the interests of many others, one must decide where one's responsibility lies. If it is to the ideal alone, then one is set on a course towards tyranny, until overthrown by the host of enemies one has raised up. And if one accepts responsibility for the maintenance of a general welfare, including that of one's opponents, one is on the path to corruption and impotence. This may seem a gloomy prognosis: but a society which does not present such hazards to radical movements of every sort is not likely to retain its stability; and a radical movement which cannot resolve such contradictions does not deserve to succeed. I see no reason why critical science should be less exposed to them than to any other reforming movement.

encountering many pitfalls, partly those characteristic of immature sciences

A cautionary tale that should be read by all who are embarking on political activism based on 'critical science' is the play by Ibsen, *The Enemy of the People*. Superficially, it is about an honest doctor who is hated by the corrupt forces of his town for his determination to expose the scandal of polluted waters being used in the town's profitable baths, as a result of economies in their construction. But on closer reading, it can be seen that Dr Stockmann's misfortunes were also due to his own naivety and egoism. I found it significant that in his own version of the play (Viking Press, New York, 1951) Arthur Miller strengthened its 'progressive' message by transposing the passage where the town meeting declares Dr Stockmann to be 'an enemy of the people'. In Miller's version it comes at the very beginning of the meeting, before he has spoken; in the original it comes after the Doctor's harangue, concluding with

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'Let the whole country perish, let all these people be exterminated'. It is true that he had been goaded by implacable enemies and false friends until he reached this extreme position; but the reaction of the town in the original version is then not a simple case of McCarthyism. After studying the play with a class at Harvard, where this modification was discovered, I was struck by the idea that a worthwhile sequel could be written, entitled 'The People's Friend', in which the entrenched forces, if only a bit less stupid and venal than in the original, could corrupt the good Doctor without difficulty. I recall being told later that scientific tests of the sort that convinced Dr Stockmann of the pollution of the baths are themselves far from conclusive.

We can expect, then, that the future political history of critical science will be as complex and perhaps as tortured as that of any successful radical and reforming movement. But if it does survive the pitfalls of maturation, and so contributes to the survival of our species, it can also make a very important contribution to the development of science itself. For if the style of critical science, imposed by the very nature of its problems, becomes incorporated into a coherent philosophy of science, it will provide the basis for a transformation of scientific inquiry as deep as that which occurred in early modern Europe. The problems, the methods and the objects of inquiry of a matured and coherent critical science will be very different from those of academic science or technology as they have developed up to now; and together they can provide a practical foundation for a new conception of humanity in its relations with itself and the rest of nature.

The work of inquiry in critical science involves an awareness of craft skills at all levels, and the conscious effort of mastering new skills. The data are obtained in a great variety of ways, from the laboratory, from the field, and from searching through a varied literature, not all of it in the public domain. Much of it lacks soundness, and all of it requires sophisticated and imaginative treatment before it can function as information. Indeed, since the problemsituations are presented in the environment, and much of the crucial data must be produced under controlled conditions in the laboratory, work in critical science may overcome the dichotomy between field-work and lab-work which has developed in science, even in the biological fields, over the past century. In the later phases of investigations of problems, the same challenges of variety and novelty will always be present. The establishment of the strength and fit of each particular piece of evidence is a problem in itself; and the objects of inquiry (including the measures of various effects and processes, as well as conventional standards of acceptability in practice) are so patently artificial that there should be little danger of critical scientists' being encased in them as a world of common sense. The establishment of effective criteria of adequacy for solved problems is possible, for the work will frequently be an extension and combination of established fields to new problems, and so critical science can hopefully escape the worst perils of immaturity. Also, any critical publication is bound to be scrutinized severely by experts on the other side, so high standards of quality are required because of the political context

of the work. Indeed, a completely solved problem in critical science is more demanding than in either pure science or technology. In the former, it is usually sufficient to obtain a conclusion about those properties of the artificial objects of inquiry which can be derived from data obtained in the controlled conditions of experiment; in the latter it is sufficient for an artificial device to perform its functions without undue disturbance by its natural environment; while here the complex webs of causation between and within the artificial and natural systems must be understood sufficiently so that their harmony can be maintained.

The social aspects of inquiry in critical science are also conducive to the maintenance of its health and vitality, at least until such times as the response to its challenge becomes over-sophisticated. The ultimate purpose which governs the work is the protection of the welfare of humanity as a part of nature; and this is neither remote, nor vulgar. Critical science cannot be a permanent home for careerists and entrepreneurs of the ordinary sort; although it may well use the services of bright young people intending eventually to serve as enlightened experts. Those who want safe, routine work for the achievement of eminence by accumulation will not find its atmosphere congenial; its inquiries are set by a succession of problem-situations, each presenting new challenges and difficulties. Hence although critical science will doubtless experience its periods of turbulence, both political and scientific, it is well protected from stagnation and from the sort of creeping corruption that can easily come to afflict industrialized science.

Finally, the objects of inquiry of critical science will inevitably become different from those of traditional pure science or technology, for here the relation of the scientist to the external world is so fundamentally different. In traditional pure mathematical-experimental natural science, the external world is a passive object to be analysed, and only the more simple and abstract properties of the things and events are capable of study. In technology, the reactions of the uncontrolled real world on a constructed device must be taken into account, but only as perturbations of an ideal system; the task is to manipulate it or to shield the device from its effects. But when the problem is to achieve a harmonious interaction between man and nature, the real world must be treated with respect: both as a complex and subtle system in its own right, and as a heritage of which we are temporary stewards for future generations. Hence, even though studies of our interaction with the environment will necessarily use all the intellectually constructed apparatus of disciplined inquiry, their status and their content will inevitably be modified. They will be more easily recognized as imperfect tools, with which we attempt to live in harmony with the real world around us; and although this attitude may seem conducive to scepticism, it will be the healthy one which recognizes that genuine knowledge arises from lengthy social experience, and that such knowledge depends for its existence on the continued survival of our civilization. The objects of enquiry themselves will include final causes among their essential attributes, not merely the limited functions appropriate to
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technology, but also the judgements of fitness and success already developed in classical biology and ecology. All this is work for the future; but if it is successful, the opposition between scientific knowledge and human concerns, characteristic of the sciences derived from the dehumanized natural philosophy of the seventeenth century, will be overcome.

Postscript, nearly two decades on: When I first wrote the above section, I knew and said that critical science was in its infancy; and the institutions where it was fostered were, and would be, scattered and vulnerable. There is still no settled institutional form, nor a large organization, created around this concept. Whether the eventual transformation of science, should it occur, explicitly takes this form, is of little concern to me. In my more recent essays I have concentrated on various aspects of what needs to be done, rather than calling for a particular form of campaign. This is because I believe that what will come cannot be hurried, and I would rather devote my energies to understanding what is to come, than spending them on a special-interest advocacy.

If I were to revise the above text significantly, it would be in connection with a remark about industrialized science, near the beginning of the section. There I mentioned crises in recruitment and morale as a contingency that was outside my analysis; and now I believe that these will need to be confronted quite directly by anyone who is concerned for the health and indeed the survival of industrialized science in our part of the world. I sense that over the next two decades the triumphalist ideology of science, with which I grew up and which in its philosophical and political expressions provided me with a great intellectual challenge, may pass into oblivion. What will happen to technology, to education, to the conduct of research in other disciplines, and other learned activities that have taken science for their model is far beyond the scope of this comment. But we may expect that then the blanketing scientific orthodoxy of the present will have become enfeebled and confused; and for a generation who have grown up with Greenpeace as we grew up with the Bomb, the world-view of critical science may become a commonplace. Its own divergent and contradictory tendencies will then have full play; and the challenges presented by critical science to the established order will be presented explicitly and reacted to as such; until eventually a new equilibrium, with its new latent contradictions, may be achieved. All this is speculation, which may yet mock me when I read it later; but my account would be incomplete without this personal glimpse into an unknowable future.

Adapted from the Conclusion of Scientific Knowledge and Its Social Problems, Oxford University Press, 1971.

# Epilogue: Science and Charity

In the study of the history of science, we are no longer embarrassed by the presence of styles of work that are very different from the one defined by the 'disenchantment and dehumanization' of Nature, which has been dominant since the Scientific Revolution of the seventeenth century. Among all the varied currents in the endeavour to understand and control the natural world, we can identify an alternative philosophy that has provided a vehicle for a politically radical folk-science that challenges the dominant, bureaucratized science of its time. In this tradition, the study of nature is explicitly seen as a social and also spiritual act; one dialogues rather than analyses; and there is no protective cover of belief in the 'neutrality' or 'objectivity' of one's work. Such a philosophy of nature becomes articulated and advanced, as part of a general reaction against the formal, dry style that pervades the official version of the activity. There is an analogous tendency in religion, and indeed the two sometimes interact. Looking back into history, we can find an affinity of doctrine or style, and sometimes a linking tradition, as far back as the Taoists of ancient China, through St Francis of Assisi, to Paracelsus, William Blake and the 'counter-culture' prophets of the 1960s.

Not every one of these figures would claim to be a natural scientist of any description; but as philosophers, poets or prophets they must be recognized as participating in and shaping a tradition of a certain perception of nature and its relation to man. Granted all the variety of their messages and styles, certain themes recur. One is the 'romantic' striving for immediacy, of contact with the living things themselves rather than with book-learned descriptions. Another is 'philanthropy'; the quest is not for a private realization, but for the benefit of all men and nature. And related to these is a radical criticism of existing institutions, their rules and their personel. Looked at from the outside, each upward thrust of the romantic philosophy of nature is doomed to failure. Mankind will not be transfigured overnight; and the romantic style has its own destructive contradictions. Whereas the 'classic' style degenerates gradually into an ossified form and a sterile content, the 'romantic' style goes off much

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more quickly, through chaos of form and corruption of content. But even in disciplined scientific inquiry, the categories of 'success' and 'failure' are neither so absolutely opposed, nor so assuredly assignable in particular cases, as the traditional ideology of science assumed. And the failure to achieve Utopian dreams, in science as well as in social reform, is not at all the same thing as futility.

The dreams of the romantic, philanthropic philosopher-prophets cannot move towards realization by the accumulation of facts or of battalions. Rather, they exist through a discontinuous, perhaps erratic, series of crises and responses. Sometimes they have the good fortune of producing a creative tension in a man brave enough to attempt the synthesis of a prophet's vision with a world managed by priests. He too will fail, almost certainly; some problems are insoluble. But this message, perhaps in a particular science or walk of life, perhaps of a generalized wisdom, will speak to men in later ages, coming alive whenever it has insights to offer. In this present period, we may find Francis Bacon speaking to us more than Descartes the metaphysiciangeometer or Galileo the engineer-cosmologist. As deeply as any of his pietistic, alchemical forerunners, he felt the love of God's creation, the pity for the sufferings of man, and the striving for innocence, humility, and charity; and he recognized vanity as the deadliest of sins. To this last he ascribed the evil state of the arts and sciences.

For we copy the sin of our first parents while we suffer for it. They wished to be like God, but their posterity wish to be even greater. For we create worlds, we direct and domineer over nature, we will have it that all things *are* as in our folly we think they should be, not as seems fittest to the Divine wisdom, or as they are found to be in fact.

The punishment for all this, as Bacon saw it, was ignorance and impotence. It might seem that the problem is different now, for we have so much scientific knowledge and merely face the task of applying it for good rather than evil. But Bacon assumed his readers to believe themselves in possession of great knowledge; and much of his writing was devoted to disabusing them of this illusion. Perhaps the daily reports of 'insufficient knowledge' of the effects of this or that aspect of the rape of the earth, and our sense of insufficient understanding of what our social and spiritual crises are all about, indicate that in spite of the magnificent edifice of genuine scientific knowledge bequeathed to us, we are only at the beginning of learning the things, and the ways, necessary for the human life.

Bacon was a shrewd man, fully sensitive to the weaknesses of the human intellect and spirit. He was aware of the superficiality of ordinary thought and discourse, at whatever educational level; and he also distrusted the extraordinary enthusiast, in religion or politics, for the damage he could cause. His life's endeavour was to overcome this contradiction somehow, and to bring about a true and effective reformation in the arts and sciences of nature. For him, this was a holy work, a work of practical charity inseparable from spiritual redemption. His audience was inevitably among the literate; and so he tried, by scattering hints and half-concealed invitations, to call together his brothers, who would gently and silently show by their example that a good and pure way into Nature is also the practically effective way. Of course he failed, in his philosophical reform as in his political career. There was no English audience for his particular message during his lifetime, and at his death he was alone and neglected.

Shortly after his death, however, there was a stirring; and Bacon's message of 'philanthropic' science began a career of its own. For a while, his followers knew what he was about; but with the passage of decades and disillusion, this was forgotten, and only the vulgar fact-finding Bacon survived. Yet when we now come back to read Bacon, perplexed and worried as we are by the sudden transformation that science has wrought upon itself as well as upon the world, we can find relevance in passages like the following:

Lastly, I would address one general admonition to all; that they consider what are the true ends of knowledge, and that they seek it not either for pleasure of mind, or for contention, or for superiority to others, or for profit, or fame, or power, or any of these inferior things; but for the benefit and use of life; and that they perfect and govern it in charity. For it was from lust of power that the angels fell, from lust of knowledge that men fell; but of charity there can be no excess, neither did angel or man ever come in danger by it.

Adapted from the concluding pages of Scientific Knowledge and Its Social Problems, Oxford University Press, 1971; full references will be found there.

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