For the past century, progress in technology has increasingly depended on the applications of scientific research. Starting with synthetic chemicals and electricity, and proceeding through nuclear power and now micro-electronics and biotechnology, industry depends increasingly on substances and processes which have no precedent in traditional, craft-based manufacture. As this development continues, each innovation both promises more good and threatens more evil. Also, commensurate with the greater scientific and social sophistication of the whole context of innovation, the problems of possible risks are recognized at the outset. In its way this is great progress, since it becomes less likely that a new technology will become firmly entrenched in the productive system before anyone realizes its problems.

The path to regulation of such nascent technologies has some characteristic problems of its own, which might be taken as one of the main lessons of the recombinant DNA debates. One of the main difficulties at the early stages is in the identification of what risks might eventually prove salient. Since these will depend on how rapidly the different sectors of the technology develop in the future, and how they will interact with each other and their total context, locating the crucial point is necessarily very speculative. It is no wonder that there must be a lengthy learning period, during which there may be considerable confusion and error (as seen retrospectively). Even that shift from 'research cowboys' viral vectors' to 'Andromeda strain bacterial hosts', so fateful for the course of the debates of the 1970s and beyond, can be seen as nearly unavoidable, given the circumstances in which the leaders of the responsible scientists were grappling with these unprecedented tasks.

If there is one systematic point to be made, it would be on the problem of criticism. If the burden of proof is put upon critics, they will always be at a disadvantage, since the only thing that they can rigorously prove is our ignorance of the future. To have some effect they will then tend to concentrate on the two extreme ends of the spectrum, either publicizing particular cases in which they can interest the media and the public, or raising long-term fundamental (and necessarily inconclusive) issues of ecology, ethics and perhaps theology as well. Neither is conducive to a reasoned scientific debate; but if that has already been impaired by the implicit assignment of burden of proof by those who control the agenda, those whose function is to call for reflection must then seek other forums.

Acknowledgements

I owe thanks to many individuals, here and in the USA, with whom I had illuminating discussions about the problems of regulating recombinant DNA. I would like to make a special mention of Max Weintraub, then a student at the University of California, Santa Cruz, who researched the evolution of Robert Sinsheimer's ideas. My analysis of the 'cowboys' problem is based on Michael Rogers' article 'The Pandora's box congress', *Rolling Stone*, 19 June 1975, reprinted in J. Watson and J. Tooze, *The DNA Story*, San Francisco: Freeman, 1981.

Hardware and Fantasy in Military Technology

Those of us who have studied the philosophy of science are familiar with the way in which, earlier in this century, the practice of science revolutionized its philosophical analysis. After many decades in which science seemed to be accumulating permanent truths, by some infallible method, usually inductive, it all changed suddenly thanks to the revolutions in physics at the largest and smallest scales. Einstein's relativity theory showed that our deepest intuitions about space and time, and with them the axiomatic foundations of the science of mechanics, can be wrong. Shortly afterwards, the theories of quantum mechanics put causality, the continuity of natural events, and the presumably limitless extent of our knowledge of nature, into question. Rather later, the ethical questions of science, which for a very long time had seemed totally unproblematic, came to the fore, first with nuclear weapons and then with the deleterious side-effects of our sophisticated science-based technology.

In that latter case, there can be many difficulties in assessing actual and potential harm from particular industrial processes; and so ethics and epistemology can interact in (for example) environmental risk analysis. But hitherto there has been no occasion to question whether the material things that might be causing the harm do actually *exist*. Indeed, such a question, about a chemical or a factory, might seem quite nonsensical. That may well be; but we know that some (though not all) of the apparently nonsensical questions can lead us to new insights about the reality which we inhabit and construct. I shall argue here that the question of existence becomes salient, and indeed quite crucial for policies of all sorts, in connection with the weapons systems of the nuclear age. As my title indicates, the dividing line between hardware and fantasy in nuclear weaponry may turn out to be as culturally conditioned as that between matter and energy in pre-Einsteinian physics. Should my argument be correct, there can be important implications for all future debates on defence policies.

Up to now, however, there has been no occasion to question technology from

the perspective of our knowledge, or of the reality of the things claimed to exist. Indeed, our whole conception of reality is a 'materialist' one, in which things that used to be called 'spiritual' are of dubious status at best. In so many ways, the example of science, in particular the mechanics of small particles and all those sciences cast in that mould, has been taken as the paradigm of all knowledge. Correspondingly, there have been numerous attempts to establish the foundations of our values in our knowledge concerning the real, material things. The symbols of our civilization are the great technological achievements; and however problematic some of these may have become recently, there are certainly no effective challenges to them in the minds of the great mass of people both rich and poor.

Of course there are always those who denounce this or that area of technology, or perhaps even our technological reality as a whole, as misguided or evil. But even they are constrained to use it, and to live within it as a fish within water, flying to conferences at which they read word-processed papers on the beauties of the simple or spiritual life. So there can be no question of the reality and power of our system of technology as a whole. Some might argue that it is now on a self-destruct course, that with the uncontrollable pollution and degradation of the environment. Should there be some vast ecological catastrophe, leading to social upheavals and the disruption of our finely tuned systems, *then* eventually the survivors could look around and remark that those things once called (for example) sixteen-track stereos and compact disks are no longer 'real'. For even if some objects are still lying around, they can (in this post-apocalyptic scenario) no longer be used as intended. Some stray shaped bits of metal and plastic, which (with their strange iridescence) might find a use as magic pendant charms, are no longer the 'thing' (e.g. a record with superior qualities of sound reproduction) as described in its pristine state. The 'compact disk', as such, exists no longer.

This hypothetical example is far from sufficient for the establishment of my case; but we can use it for the insight about what is 'real' in the case of a sophisticated technological device. Let us try another example, rather more familiar. It is said that in certain cities of the USA it is unsafe to leave a car parked by the kerb. It may be 'vandalized' (actually, recycled), so that tyres go first, then perhaps wheels and brakes; the windows are smashed and any saleable parts removed; if it is the right sort of model, the engine itself might be lifted out. At the end of all this reprocessing, it is no longer a 'car', it is scrap, which once had been a car. Now, to determine the precise point at which it ceases to be a car and becomes scrap may well be impossible. But that is not a case for worry, for one of the oldest philosophical puzzles in the world is devoted to just this phenomenon of a continuous change between discrete categories, such as diluting wine with water until it stops being wine. If someone argues that the thing at the kerbside is still a car, then we can let it be towed away, further dismembered and finally the shell crushed into a block and the loose parts shredded. Somewhere along the line there is a phase at

whose end there is no car, and common sense dictates that this occurs before the total physical disintegration of the object.

The point of this somewhat lugubrious example is that a thing, even something inanimate like a car, can in its way 'die' and cease to be. When there is no practical prospect of its performing its function, or being restored to a state where it can do so, we begin to recognize that it is passing out of existence into non-existence. Another way of putting it is that the quality of the device is being degraded by stages (where quality relates to its performance and costs of maintenance and restoration); beyond a certain phase, the quality is so low that the thing no longer can be said to have any, and therefore cannot be said to exist. In the first example, we had individual copies of material things perhaps surviving intact, but in the absence of the total support system (such as high-quality electricity supply) they were useless and hence, as the named device unreal. In that case we could speak of a class of devices (compact disks) that becomes unreal, while the individual copies are unchanged, the reverse of our example of the single copy of an automobile being recycled at the kerbside while the class of automobiles still lives and flourishes.

My examples for all this discussion are necessarily somewhat out of the ordinary, since our total technological system has means for ensuring that quality, both of large subsystems and even of individual copies, is kept up, very far from those low, abysmal or abyssal limits where the degradation of quality threatens the very existence of the thing. Certainly complaints of low quality, both of copies and of systems, are legion; but the presence of a market, supplemented by regulation, ensures that people hardly ever find themselves having purchased a non-thing rather than an inferior thing. However, there is no automatic guarantee that all systems and copies will be 'good' in all relevant respects; certainly, there is no discernible lower limit on what is sold or foisted on to the poor and ignorant, locally and globally, in the way of shoddy, inappropriate or deleterious things.

To find an example where things are produced which have some of the properties of the post-apocalypse compact disk or the kerbside recycled automobile, we must therefore look in some special sector of the economy, where usefulness, consumer choice and regulation are so weak that there is no 'floor' at all under the quality scale, a sector where things can be so low-quality that they are not really the named 'things' at all. If this were in some odd, eccentric corner of the economy, then this would be just another essay in consumer advocacy, finding yet another evil, or aberration in the market economy, to be sorted out. But, as my title indicates, I am here talking about a large and significant sector of the economy, and one which has dominated much of our political life for a very long time. For a variety of special reasons, the sector of military procurement is lacking in the various protections of quality, to the point where it is realistic and relevant to say that we have been and are spending money on things that do not and cannot exist. Most notably, the Strategic Defense Initiative (SDI) is an example of this; and it is very useful

for my philosophical analysis since it is so bizarre that there will be less resistance to my paradoxical thesis in this case. What other important examples come under this category of zero-quality non-existent systems, will be for others, more familiar with the details, to decide.

The SDI as a Non-Thing

This is not the place to recapitulate a lengthy debate; one simple anecdote will have to suffice. This was a public debate on Star Wars, conducted between two American gentlemen, both of impeccably Establishment, military-orientated careers and views. The defender had conspicuously little to say; he showed that the problem has a history, that we now have no shield against enemy rockets, that he is concerned for peace, and that he too appreciates the inherent impossibility of some of the design features (as a ten-million-line computer program that must have no bugs on its first real run). The critic enjoyed himself hugely, and based his argument on considerations of quality: by examples, he showed how any possible design is in many respects hypersensitive to defects. For example, the mirrors that are to reflect the death-rays to the incoming rockets must be polished to a near-perfect smoothness lest they absorb some of that highly concentrated radiant energy and disintegrate instantaneously. Any sort of flaw could be fatal to their performance—a tiny crack, a piece of chewing-gum, etc. Of course that brought the house down; what is more likely, in America, than that some careless assembly-worker or bored soldier would place his chewing-gum on the mirror and forget to collect it?

All this was before the Challenger disaster, and the shooting down of the Iranair plane in the Gulf, so that the problems of quality are even more plausible now than then. But what might still cause difficulty is that a system can be deliberately *designed* to be so hypersensitive to quality, that under any remotely realistic circumstances it could not be said to exist, or to have a chance of existing, at all. Even if that could be imagined as an abstract possibility in some philosophical game, it is difficult indeed to imagine its happening, and on such a vast scale as the SDI. So my argument must not only show that these non-things are conceivable; I must also show that they really do happen. The non-things are there, and have an importance of their own as believed-in-things. Fantasy and hardware are not, then, in totally separate categories; in the nuclear age they can become indistinguishable. Out of such a paradox we might arrive at a deeper understanding of our technological reality as a whole.

As I say, the SDI is an excellent case in point as a non-thing. What is indubitably real about it is the extra money appropriated annually by Congress for the work. (To the extent that existing programmes are deprived of funds on behalf of this new one, and then existing work carries on with new titles so as to attract continued support, even that financial reality is relative.)

Beyond that we have an 'initiative' which could never (once the opposition had presented its case) be seriously defended in public, nor indeed even *defined* with any precision. It became a shifting assemblage of speculations and computer-art videos. Some of its imagined hardware had been considered and rejected; the rest was unfounded fantasy. There was a single experiment at its base, a highly improbable result concerning X-irradiation emitted from a small hydrogen bomb explosion; by some undreamed-of technology, *this* was to be aimed and focused in the instant of disintegration, thereby producing the death-ray against enemy missiles. There might, of course, be some potentially real things lurking behind it (which may be why the Russians have been taking it seriously); among these are communications satellite systems which might transform our present *ballistic* missiles to ones with on-course *guidance*, thereby greatly enhancing their accuracy.

But the SDI survived in good strength through the Reagan administrations because it was one of the President's fancies, along with Nicaragua. It not merely threatens the quality of civilian research into computers (the country's main line of defence against the Japanese), but also has all sorts of distorting and corrupting effects on technological R&D and scientific research. Those with extensive experience of federal programmes designed to eradicate this or that social evil within five years will find none of this surprising. But what is different in this case is that real hardware is being designed and built, at enormous expense in money and resources of talent, which is so palpably a non-thing. Fortunately, the self-correcting mechanisms of a (relatively) open society have come into play; and following on a growth of healthy scepticism among people and politicians about 'conventional' nuclear weapons systems as proposed, there is now little enchantment, outside Reagan and the looney right, over Star Wars. I am writing this before the election of 1988, so it is pointless for me to predict how the SDI will fare in the immediate future; but it is hard to imagine its carrying on unscathed into the 1990s (though this may be one of those predictions I shall come to regret!).

Quality Decontrol in Military Procurement

Nothing in politics happens in a vacuum; and so even in the Reagan administration there had to be some precedents and practices that gave the SDI some semblance of plausibility. This pre-existing context can be analysed into three phases: capture of quality control; the 'baroque' design effects; and what I might irreverently call 'Zen and the Art of Nuclear Deterrence'. To the first, the classic source is the book *National Defense* by James Fallows (New York: Random House, 1981). There he quotes case after case where even the most humble tools of the soldier, including his rifle, were the victims of distortion and degradation of quality through bureaucratic and commercial pressures. For those interested in the philosophy of quality control, there are fine examples of imposition of inappropriate criteria, such as testing a rifle by

its performance when used by marksmen under competition conditions, rather than in some simulation of battle. All this was in the cause of keeping the M15 (later M16) rifle, a foreign import, from being seen to be clearly superior to the traditional American product. Such practices go a long way back; in his *In the Name of Science* of 1966 (Quadrangle Books, Chicago), H.L. Nieburg described how defence contractors accomplished the 'throwing away of the yardstick' whereby their projects and products could be independently assessed.

It can be profoundly shocking to discover how the distortion and degradation of quality of performance can be imposed, in spite of what would seem to be its obvious consequences on the ability of one's own troops to fight and even to survive. The reasons for this can be as various as those of any corrupted state. We need not always invoke the 'Milo Minderbinder effect', from the *Catch-22* character who brought the principles of free enterprise, for which he was fighting, into the battlefield itself. The eminent physicist Freeman Dyson tells how he struggled vainly for many months to secure a slight modification of the standard Second World War British bomber, so that it would not be a death-trap for the fliers once hit by enemy fire. Those who opposed this were doubtless the well-educated, charming, highly principled mandarins who would be scandalized at the thought of the unsophisticated corruption by which the American system is bent from its stated objectives.

In all such cases, we can say with certainty that mechanisms of quality control are defective. The users of military equipment are remote in all ways from the purchasers, and hence the market is far different from that envisaged by Adam Smith. Also, the purchasers and regulators are subject to many pressures and inducements; since they must have some expertise in the materials on offer, they must therefore be familiar with, and in many ways useful to, the suppliers. However, quality control is not absent altogether, because in the end there is some hard reality testing in field conditions. Even if sample copies of a system perform miraculously well in 'controlled' trials, there can be notorious mishaps in practice, too significant to escape notice in any society where state control of communications is incomplete. The Aegis system for identification of aircraft is a recent case in point.

All these degenerative tendencies become more severe when the weapons system become 'baroque' in the sense discussed by Mary Kaldor in her book *The Baroque Arsenal*. Another way of describing the phenomenon would be hyper-sophistication, together with design by committee. Partly because of the enormous expense of new weapons systems and even of individual copies, there is always great pressure on their designers to make them perform optimally over several different sorts of functions. That this exercise is to a great extent a matter of bureaucratic power politics needs no saying. Then there are inevitable tendencies to changes of design in midstream, and to overly complicated designs in general. The result can be systems which were conceived and designed with the best and most honourable, uncorrupted intentions, and yet which have to be (or, better, should be) abandoned halfway

through the process because they become quite impossible to manufacture according to the specifications as demanded and promised.

Those systems which do survive to the stages of manufacture and use will always be sickly, in a sense. Design changes and retro-fits produce disharmonies and incompatibilities within the completed system. It is difficult to see a way to reverse this process, except by an international agreement to ban the gun. For, when they do work at all, each latest model is (in some contexts at least) vastly superior to its older rivals, so that victory can depend on its possession. So the world's military forces are in a sense 'hooked' on increasingly baroque weapons systems, in spite of their accelerating costs and increasing susceptibility. We may say that in this case, the mechanisms of quality control are further constrained and distorted by the sheer difficulty of specifying what is the design and what are the desired performance criteria.

Quality in Nuclear Weapons: Can the Unthinkable be Thought?

All these tendencies to the degradation of quality control operate even more strongly in the case of nuclear weapons. The dividing line between some of the 'classic' nuclear weapons systems and the SDI is, in retrospect, not so completely sharp; this is because of some logical properties of the theories under which nuclear weapons are justified, deployed and designed. First, we can say that there has been enormous progress for humanity over the last quarter century. The message first proclaimed by nuclear disarmament campaigners, and then derided by all the establishment intellectuals, is now a common-sense proposition that guides the policies of the two greatest powers. This is that there can be no such thing as major nuclear war. Here is another example of a non-thing, analogous to those I have discussed already. In our part of the world, we all accept the definition of von Clausewitz that 'war is the continuation of diplomacy by other means'. Now, a real exchange of nuclear weapons would most probably kill all the diplomats along with the rest of us; hence diplomacy would be discontinued, and the event would have been something other than a war. One could call it genocide, or a holocaust, or perhaps ecocide; but not war. Of course there is no *certainty* about this, but, especially since the 'nuclear winter' debates, the probability is so high that it is decisive for policy purposes.

This means that there is something unique about the various explosives and their systems for delivery: they cannot be used in a war. I should mention a possible exceptional case: sometimes there is reason to believe that their real use *is* contemplated, as in a 'counter-force first strike', which would be intended to disarm the enemy and keep us safe from retaliation. In that case a 'war' is contemplated, however one-sided it might be; and there are occasionally great debates on whether some development could make that option more attractive to one side, and therefore destabilize the arms race

further. But, given what is now publicly known about the deficiencies of existing weapons systems, that option, which would require extremely accurate weapons lest it rebound catastrophically, is always more of a theoretical, long-term concern rather than an immediate policy.

In the above example I have provided a hint of how convoluted the debates on nuclear weaponry can and do become. Because they threaten total annihilation, the logic of their possible uses is just very different from anything we have seen hitherto. In the normal scenarios, the weapons are designed, or perhaps better, intended *not* to be used. They are there as a 'deterrent' against some matching system on the other side. Essentially, their use is that of a bluff: one hopes it will succeed without being 'called', but one must be prepared for that most unwelcome eventuality. Now, how is there to be any quality control in that function? It would require some grave crisis, or a realistic simulation of one. The last real one was over Cuba; and the more we know about that, the less it seems possible to simulate such a thing realistically. Hence any quality control on particular designs of nuclear weapons systems, rests distinctly in the realm of uncontrollable speculation. Worse, we might consider the problem, how can there by any serious design at all? It is totally impossible to fine-tune the performance characteristics of a device of mass annihilation around some nightmare scenario of misunderstandings and panic among the world's leaders.

Fortunately for the nuclear weapons business, this difficulty has been circumvented; the procedures have been well described by Sir Solly Zuckerman in *Science Advisers, Scientific Advisers and Nuclear Weapons* (Menard Press, London, 1980). What starts the process is not a strategists' scenario involving mad Russians, but a weapons lab with a device they hope to market. Around its properties they imagine possible physical uses, and then the sorts of crises that could precipitate the threats to such uses. These would best relate to weapons already in existence on the other side or thought (or conveniently imagined) to be under development there. With such a sales pitch, all it needs is glossy brochures, realistic computer-art videos, and some sympathetic congressmen. Then the public learns of a new threat in the form of a 'gap' or 'window', to which our brave lads in the labs have fortunately in the nick of time dreamed up the answer, for only so many gigabucks. Thus the fantasy spawns hardware, whose only real function is to generate cost-plus contracts for itself and its offspring.

Of course there is a sense in which this is corrupt; for the public is being sold a succession of systems whose real function is not the sort of public benefit as stated, but rather a covert, private enrichment. But it is hard to see how it could operate otherwise, at least in a country that is leading rather than following in technological development in the field. When Herman Kahn wrote his famous book *On Thermonuclear War*, he referred to the calculating with megadeaths, in tens and hundreds, as being 'unthinkable' in its moral horror. He was not then aware that this property made any attempted rationality (such as his own) purely theoretical and speculative; once the test of

reality is brought in to the planning of nuclear warfare, its logical impossibility obtrudes on and then dominates the process. So, half-cynically one might say that the system of design as described by Sir Solly Zuckerman is as good or as bad as any, given that a society somehow believes in such a thing as rational policy for nuclear defence. From that fantasy of illogic, all others flow.

When such corruptions of reason and of process are so necessarily involved in the design and production of nuclear weapons, it is only natural, indeed inevitable, that their quality as physical systems, should totally lack controls. By analogy with the previous case, we may speak of them as hyper-baroque, lacking any chance of reality testing to contain or dilute the effects of unstable specifications and over-design. Indeed, before the SDI ever emerged from its murky origins, there were cases of weapons systems whose quality was publicly seen to be right down there near to the point of nullification. The most famous case in point was the MX system, which was to be carried around on trains in a gigantic network of underground tunnels, all to cope with a technicality of a treaty as yet unsigned. Faced with NIMBY (Not In My Book Yard) protests on a correspondingly large scale, the promoters shifted designs repeatedly, until it was quite clear that the function of MX was simply to keep the Air Force in the missiles business. The notorious Cruise missiles certainly played a part in the growth of a world-wide radical feminist movement; but their computer map-reading program never worked, the copies of the missiles performed but indifferently on tests, and the whole thing was abandoned halfway through its production run. Examples of 'nuclear junk' that never did work and never could work are there in abundance; the question is whether there ever was a nuclear strike force that could have inflicted serious damage on Soviet targets, rather than random genocide all around.

Thus, in the technical sphere just as in ordinary civil life, corruption is indivisible. If one part of an enterprise is rotten to the core, then the others will surely be infected. What makes nuclear weapons unique is that the corruption does not even depend on the (inevitable) moral frailties of the responsible agents; it is built into the very conception of the things, as devices whose use must be a non-use, and that non-use to occur under conditions that are strictly unimaginable. In another civilization, 'nuclear deterrence' might even become a Zen riddle, like making the sound of one hand clapping. But outside the monastery, in that big business of war, it had to become corrupt. Logical self-contradiction in these conditions leads not to instant enlightenment, but to intellectual fantasy and technical and social corruption. The SDI was only a natural extension of nuclear weaponry by other means, its fantasized hardware only a more pure version of what had gone before.

The Emperor's Bombs

Like anyone who is making a point they consider original and important, I must explain to myself and to my readers why it has not been taken up before.

There has been no lack of debate on nuclear weapons at all levels, and it is surprising that this point should have waited so long, perhaps until the problem is well on the way to resolution, before being made. One thing I can say about it is that I have indeed been making this point, in occasional lectures and articles, over the years; but I never found the slightest interest expressed in it. From this I concluded that either I am very eccentric indeed, or that the reluctance of my various audiences, even those in the anti-nuclear movement, to engage with this problem, is itself a topic for reflection.

First, I should say that my detailed observations have no novelty at all; everything I say about the deficiencies of nuclear weapons systems is and has been freely available in the public prints. Furthermore, my theories of the attitudes of those in charge of the weapons systems are supported by statements of knowledgeable people, reported in the press. Some years ago there was a minor scandal in America, about the 'electro-magnetic pulse' (EMP). This had been known since the mid-1960s at least, as a product of H-bomb explosions, that could derange even heavy-duty electrical switchgear, to say nothing of electronic systems. It was clear, to all who cared to know, that after a nuclear exchange of any significance, all the world's communication systems would be disrupted or destroyed. Moreover, there were techniques for 'hardening' such systems, making them less vulnerable to the pulse. But to institute these would have been very expensive indeed, and research on them would deflect funds from more glamorous projects. So the EMP was ignored, until there was evidence that the Russians were taking it seriously. Then there was a rather belated recognition; and at the time (1981) *Science* magazine quoted one defence official as saying: 'The philosophy that says a nuclear war is never going to happen has pervaded the military and its contractors to the point where they do not mind building self-defeating systems' (W.J. Broad (1981) 'Nuclear Pulse', *Science* **29**, 1009 ff, 1116 ff, 1248 ff).

Although this is an isolated statement, and as such not the strongest of evidence on its own, it fits so perfectly with the objective phenomena of the multitude (perhaps majority) of unworkable nuclear and hi-tech weapons systems that it is the obvious solution to the problem. And yet, what could be done, philosophically or politically, with such evidence, unless by someone who had already got to the same point as myself? Unless one has already gone beyond respecting nuclear strategy and nuclear weapons as the products of serious intentions, such a statement could only provide an invitation to further conundrums of the sort (usually involving comparisons of the dangers of various weapons systems and strategies) in which many anti-nuclear intellectuals have entrapped themselves. For we might ask, is the world safer with people like that running the show? Is this a case of 'laudable corruption'? Should we encourage them to keep on building self-defeating systems, as a contribution to peace? I do hope that such questions have an initial plausibility, for in my view they reflect a conception of the phenomenon that lacks roots in the analysis, logical and material, that I have been making, of hardware and fantasy. The reasons that there has never been a systematic appreciation

of the problem of 'nuclear junk' lie in the ways that nuclear weapons, in spite of all their moral horrors, are 'natural' to the ways of thinking of nearly all of us.

I believe that the nuclear weapons industry, culminating in Star Wars, could survive for so long in spite of being so fantasized, because it has an inherent plausibility in several basic aspects of our world-picture, and then also because its exposure would threaten some fundamental beliefs of ours. Those who admired the emperor's new clothes were not simply frightened and venal; social stability and social solidarity were at stake. Only a child could have been innocent of the subversive consequences of that excessively penetrating vision of hers.

First let me discuss the negative side of the protection of the plausibility of nuclear weaponry, extending for a while even to Star Wars itself. Here we may have a case of a taboo, an unwillingness to imagine incompetence, corruption and mendacity on a scale that staggers the imagination. For me there are two analogous cases. One is in committed scholarship of various sorts, where there are heated debates, persisting for decades or generations, over the true solution of some problem. In Protestant religion there was 'the historical Jesus' whose career *must* be capable of reconstruction from the scriptures that authentically described it. In the philosophy of science there was 'the scientific method', which *must* be capable of articulation so as to explain the unique truth and rationality of science. When both of these quests eventually lost their plausibility, the world as a whole did not suddenly turn upside down; but many individuals went through deep crises, and the next generation in those areas of experience inhabited a reality that was subtly but definitely different.

The other taboo may be more acceptable outside America; I call it the 'Kennedy in Dallas' problem. Can there be places in the United States of America where it is simply unsafe for the President to go? The widespread need to find a lone, crazed assassin, rather than a web of persons involved actively or passively, reminds us how in America the Presidency, as distinct from any particular incumbent, *is* hedged about with a sort of divinity. Should the Presidency be exposed as just another part of the game (as was indeed the threat during the Watergate scandal), then something nearly sacred, that for many Americans has made their country something different and better, would be tarnished and corrupted. For this present case, I recall the title of that early book on the problem, *In the Name of Science*. That name, science, has a charisma in our culture analogous to that of the flag for Americans. Even though defence contracting is not traditional academic science, still there are enough connections, both in symbol and in practice, that the exposure of the universal degradation and corruption of quality in the one would necessarily rub off on to the other. The activities of the State in policy for defence science necessarily use some of the most eminent of the research community; if these should turn out to have been so gullible, or so complicit, then their standing, and then that of their community, would be compromised.

Perhaps in the management of the many individual scandals in the 'nuclear junk' field, we even see an analogy to the management of incompetence and

corruption in American politics at the highest level. So long as each crisis can be contained, treated on its own, and treated implicitly (though unrealistically) as an exception to a general rule, then the symbols are safe. In politics, it was that assassination in Dallas, and later that squalid little crime organized in the White House, that threatened the symbols themselves. In defence procurement, we have not yet had such a shattering experience. Perhaps the very illogic of nuclear defence protects itself: since there cannot be an occasion for reflection on the quality of the equipment when used, there is no way in which the awful truth can be forced upon the public. The closest approach was the Challenger disaster, but that was very specialized, and in the civilian sector as well. All this discussion has been in terms of the American situation; for there, government in general is more vulnerable to public disillusion than almost anywhere else, and also the scandals are more open. If fantasy and junk in nuclear weaponry can survive there, then elsewhere they are quite safe indeed.

At the present time we may be witnessing the containment and resolution of the greatest fantasy of them all, the SDI. In the closing months of the Reagan administration, there is very little said about it; and it is now some time since there were accusations that the basic experiments by which the whole scheme was justified were themselves highly dubious. There were leaks, protests, splits and finally resignations in the key weapons labs responsible for the core of the programme. It may be that reality testing was accomplished in this case, by an effective combination of the integrity of particular persons with the rivalry between particular organizations. What will happen after Star Wars, particularly if the decline in antagonism between the superpowers continues, is hard to say. Being optimistic, one could imagine a situation where people begin to ask what was it all about, and go on to look critically at the whole enterprise. At that stage, perhaps the cultural need for defences of the nuclear syndrome will be decreased; and so it will be possible to engage with the grounds for its positive support within our general view of the world.

Here we are dealing with what might be called inherent plausibility, which strongly constrains a culture's reception of an idea. Before Galileo and Descartes, the idea of the earth at the centre of the cosmos had all the inherent plausibility; after them, the balance shifted. Before the work of Martin Luther King Jr the idea of political action through Gandhian non-violence had no inherent plausibility in the West. Until recently, the idea that national security is *decreased* by an increase of armaments was implausible, even though this paradoxical property of weaponry was established in various ways of individuals. Thus although many Americans keep guns at home, not so many carry them around for instant use on the streets; even the guns lobby does not press for a *continuously* armed citizenry. The British have long been known to keep their police forces unarmed in ordinary conditions; and there is no pressure to change this apparent invitation to violence. Hence there have been a variety of precedents available, and some of them used in argument, to show that security can be improved through controlled disarmament. After only

some three decades, the balance of plausibility has been changing rapidly, and both the American and the Soviet leaders now know that (controlled) weakness brings strength.

This change in inherent plausibility might be quite an important one for our civilization, for it stands as an exception to the implicit logic whereby we relate ideas. For here we have a grasping of paradox, an appreciation that two apparently contradictory ideas have a deep inner relation. This is quite foreign to our mainstream literate culture, where logical entailment is the only accepted connection of ideas. Indeed, that thought-style, combined with the triumphant materialism of modern European civilization, is what made the development of nuclear weapons so seductively, nearly fatally plausible. Defence is secured by the ability to deter, repel and counter-attack; the greater these abilities, the better the defence, and hence the greater the national security. This is so logical, it could almost be a theorem in mathematics.

In spite of the abstracted megadeath calculations of the theory of games as applied to nuclear strategy, there was no real imagining of the thought-processes of the other side. The lesson in logic of the First World War, when both sides had the machine-guns that each had previously used so successfully against spear-throwing natives, was forgotten. This time around it was not merely the butchery of the best men of all the nations for a few yards of mud. Now we had the realization of the mad inventor's dream, 'a weapon so terrible that it would make war impossible forever'. But on all sides we still heard, 'If "they" have them, then we must have them too, and bigger'. It was beyond the wit of almost any expert, including the most erudite and authoritative, to imagine the properties and outcome of a duet in which each side was singing this same refrain. Of course, the reasons for this low-level awareness could be seen as largely political: if one has made the antagonist into a demon, then he is presumed to have no rights to self-defence, and by extension no thought processes either. But it is also possible to see a deeper cause, in an inability to confront a deeply paradoxical, self-contradictory situation that has arisen in the most natural way from ordinary circumstances and common-sense reactions.

We all know that the Bomb started as a deterrent against a possible Nazi bomb, and that once completed it was used both to finish the war with Japan and to make a show of strength to the Soviet Union. But once the Russians had followed suit, and especially once there were intercontinental ballistic missiles against which there is no defence, the logic of warfare, which had taken so many generations and centuries to elaborate, was rendered demented at a stroke. In the perspective of history, thirty years is not such a long time for leaders and theorists to turn their thinking around. While that was waiting to happen, all the corrupting tendencies of ordinary military procurement, aggravated by the baroque weaponry phenomenon, were heightened in the case of the nuclear weapons that had so suddenly become Zen riddles. Now at last we have learned that in the face of the technological ability to destroy civilization and much more, our inherited logic in political–military cause

and effect must be discarded, and a logic of paradox and reflexivity must be employed.

As that lesson sinks in, we may be ready for the next lesson, that not merely logic but also reality can be paradoxical and dialectical. The idea that a piece of equipment may be a non-thing, and may attain that status by a continuous conceptual extension from being a shoddy thing, is contrary to the ingrained materialism of our world-view as much as to our linear logic. Having been taught that the only reality is in the atoms (or their successors), and that things like 'values' are secondary, how difficult it is to imagine that what makes a device what it is, is the *function* around which it is designed. From that principle follows the appreciation that if a supposed device can perform no function at all, then it is not an object, no more than a pile of scrap is an automobile. In the persisting belief that such non-thing is a thing, we have the primary fantasy, from which all the others, and their associated corruptions, follow. Thus we learn that hardware and fantasy interpenetrate as much as do strength and weakness, and indeed good and evil.

In the case of nuclear weaponry, and particularly in the most instructive example of the SDI, we may have an important example of the way in which the development of ideas as realized in practice eventually modifies the reality around them, and so fosters the creation of a new, appropriate framework of ideas for effective practice and indeed survival in that new reality. There are analogous developments in other contemporary problem areas, most notably environmental pollution and degradation; we see it clearly in the concepts 'waste' and 'disposal', both of which are paradoxical and lead to illogicalities though of not such an immediately apocalyptic sort. If our civilization is to survive through the environmental, social and cultural consequences of the material powers it has spawned, then philosophical lessons such as these will be as important as any of our scientific or technological responses.

Adapted from a lecture first given at Crown College, University of California, Santa Cruz, in November 1985.

2

HOW WE GOT HERE

It is not easy to draw a boundary around the problem of the roots of our present predicament. Some see it only as a temporary imbalance in the technologies of production and of conservation; others lay it to the post-war affluent society, or to the industrialization of the Victorian age. At the other extreme, it is possible to argue that with the displacement of hunter–gatherers by agriculturalists the ecological imbalance was set in train; and one can even speculate that for hundreds of millennia *Homo sapiens* has been disturbing the ecosystem. My competence lies within the last few centuries, and so I can most usefully concentrate on an epochal event in our intellectual history, the 'scientific revolution' of the seventeenth century. This was a revolution within science, but even more a revolution *about* science, a relatively sudden revaluation of its objects, methods and functions. Taking the traditional term 'revolution' for this historical event, I use it to illuminate some of its essential features, including the simple, prophetic message which defined it, and then the complexities inevitably introduced by success. The dream of power for all mankind over a natural world conceived as disenchanted and dehumanized is unique to our civilization among all others; and so this revolution in ideas may help us to understand our predicaments in the material sphere.

The term 'scientist' is a recent invention, scarcely a hundred and fifty years old; and so we can say that there was science before scientists. Then they mostly called themselves natural philosophers; and this term conveys a difference in how the role was imagined. We understand the scientific revolution better if we see its founders as self-appointed prophets of a new path to the True and the Good. Admittedly, this is easiest in the case of Francis Bacon, for he devoted himself more to rhetoric than to research. But the volume of his writings on non-scientific subjects provides us with many clues to his essential vision. The title of the second essay in this section includes the term 'reform';

and this is ambiguous, since Bacon's own religion was the product of a Reform. I show how Bacon's religious commitment shaped, even determined, his conception of the path for science. There are some surprises there, such as in his relating of his endeavours to the millennium; and Bacon's perception of charity as the essence of religion has made a deep impression on me.

Such an exalted view of science has been for a minority, even among its practitioners. Linking the past with the present, I show how the tradition of criticisms of science is co-extensive with the history of scientific research and speculation. The Socrates idealized by Plato appears otherwise in a famous comedy by Aristophanes; there he runs a school that unites godless philosophizing with crooked logic. This conservative criticism is echoed down through the ages, to the Creationists of today; but there have been radical critics as well, condemning science as élitist and inhuman. It is possible to see all those past critics as futile or irrelevant, as science accomplished its triumphant advance. But in recent times the criticisms have increased in volume, diversity and standing of their proponents. The ambiguities in the ideas of 'science' and 'scientist' have now become a source of weakness, as the community of science is now in a position of creating great powers while being deprived of responsibility for their use; a paradoxical state, which leaves it confused and vulnerable.

Such a state of affairs would have been particularly dismaying to one of the last great prophets of science, who reinterpreted the rationalist tradition in Marxist terms; this was J.D. Bernal. His vision was formed in the 1930s, when capitalism produced unemployment, scientific and technical stagnation, and Fascism; while Soviet Socialism seemed to contain the promise of a science that was planned, for the benefit of science and society alike. Although he had great influence on a generation of scientists and politicians in England, his vision was clouded, by the Bomb and by the subsequently admitted errors and distortions of Stalinism, just as the planning of science was becoming a reality. He turned to history for guidance; and wrote a magnificent survey that enjoyed enduring popularity in capitalist as well as socialist countries. But his Marxism could not really explain the shape of events in the past, any more than in the future; and his life's work remains as a monument whose relevance will have to await some eventual rejuvenation of Marxist thought.

The following essays provide a perspective, however fragmentary and incomplete, on the background to our present problems. If there is any single simple lesson to be drawn from them, it is that science in history is very far from being the collection and application of facts, directed by some inner logic of discovery and need. Ideas, ideals and illusions, as tempered by criticism and fierce debate, are an important driving force in the shaping of the science that is done and the technology that

is effected. The lesson for the present and future is that science and technology can be similarly influenced, perhaps even more deliberately now than ever before, by public scrutiny and debate. This has already happened on a variety of salient issues, such as cruelty to animals and environmental protection; the task for the future is to ensure that accountability to the public is achieved by appropriate means, and does not degenerate into crude control by politicians.

What Was the Scientific Revolution?

From one perspective, the civilization we call Western, or better, modern European, is but one of a long sequence of civilizations, extending back for thousands of years towards prehistory. Indeed, it is prudent, particularly now, to consider it in that light, and to be aware of the possibility that it may now be turning towards decline and replacement. Although every civilization is unique in its own way, ours has special features that make it important not merely because it is our own and carries our fates with it. It has achieved a totally unprecedented degree of material power, enabling the earth to support previously unimaginable numbers of people in previously unimaginable conditions of material comfort and security. With this material progress has come an amelioration of social and cultural conditions for the great mass of people, so that even with all the continuing poverty in the world, there is at least a vision and a hope of a good and decent life for all. With this progress have come hazards; these affect not merely ourselves and our present civilization, but also the survival of any civilized life and indeed the whole living system of the planet. Hence our concern with understanding ourselves as a civilization now has a significance of genuinely cosmic proportions.

Such as understanding of ourselves will require all the perspectives we can achieve, from those of the poet and novelist to those of the scientist or philosopher. Somewhere in the middle, perhaps the broadest in scope but not in itself a complete synthesis, lies history: the study of the past, motivated though not dominated by the concerns of the present. In the case of modern European civilization, history is particularly appropriate as a path to understanding. The features that have made modern Europe unique, though discernible in retrospect in its various cultural roots, became decisive in the seventeenth century. Up to that point, one could (in retrospect) imagine Europe as just another empire, on the rise just when its rivals were decaying. But during that turbulent time, something happened to the educated common sense of the world in Europe, which interacted with the rapidly developing sciences of nature. Out of that union came the uniquely powerful body of

theories and methods that is epitomized in Newton's *Principia* of 1687.

Those achievements in the study of nature had consequences in all spheres of educated culture. The radical thinkers of the Enlightenment adapted them as symbols for their own programmes of reform of philosophy and society. The innovators of the Industrial Revolution used the example and the results in their rapid, though piecemeal transformation of the basic forces of production. Out of these two currents came contemporary Europe and its cultural colonies, an empire on which until recently the sun never set.

Revolutions

We get a special appreciation of how the Scientific Revolution has shaped our consciousness when we study the works of its masters, and feel how 'modern' they are. Galileo's *Starry Messenger* (1610) reads like first-class popularized science, and Descartes' *Discourse on Method* (1638) unfolds its story through the familiar device of an autobiography of an intensely self-aware intellectual. The writings of the third great prophet, Bacon, support this point, for they now have a somewhat antique flavour; Bacon wanted to reconcile the accepted learning with the new, rather than replace it. We can concentrate on these three authors as our examples, because of the commitment and clarity of their vision, making them prophets rather than just innovators; this was well-recognized in their own century. Such apparent modernity is, of course, somewhat deceptive; each of these authors was expressing personal concerns that are forgotten to use now, and in any event they were exceptional in their own time. However, the direct intellectual ancestry is unmistakable; and so when we look for the events that have made the world-view of modern Europe what it is, we can start there. All earlier events can legitimately be taken as roots, or anticipations, rather than the crucial thing itself.

For the purposes of this essay, I accept as an historical event something called the Scientific Revolution. It is concentrated inside the seventeenth century. Its earlier benchmarks are the classic writings, such as Galileo's *Starry Messenger* of 1610, Bacon's *New Organon* of 1621, and then Galileo's *Two World Systems* and Descartes' *Discourse on Method and Essays* of 1632 and 1638 respectively. Its culmination is recorded in Newton's *Principia Mathematica Philosophiae Naturalis* of 1687. Other contemporary great works of science, such as Gilbert's *De Magnete* of 1600, Kepler's *Astronomia Nova* of 1609 and Harvey's *De Motu Cordis* of 1628, do not count, because they only recorded the doing of science and did not call for its transformation. The 'revolution' was localized in place as well, starting in northern Italy, but soon moving to north-west Europe, including France, the Netherlands and England. Within those constraints, somewhat conventional as are all in history, we can try to see what happened and what it means to us.

The Copernican Revolution is the dramatic centre-piece of at least the earlier part of the scientific revolution; in it the earth was displaced from the

centre of things and set to spinning on its axis and orbiting about the sun. In cosmology it replaced the hierarchically nested universe of the Christianized Aristotelian scheme with infinite uniform space; and it also emptied it of the various intelligent agencies responsible for various theological and astrological functions, leaving it silent of meaning of its own. All this was accomplished partly by technical astronomy (Copernicus and Kepler) and partly by popularization (Kepler, Gassendi, Descartes and, most famous, Galileo). But a *proof* of the earth's motions was slow in coming; even Newton presupposed them rather than demonstrating them. Hence rather like the larger scientific revolution itself, the success of the Copernican revolution must be seen as much in terms of philosophy, common sense or ideology, as of scientific research.

The term 'revolution' now means the overthrow of something old and its replacement by something new; and this is how we understand the scientific revolution. The term was brought explicitly into the analysis of science by Thomas S. Kuhn in his seminal book *The Structure of Scientific Revolutions* (Chicago, 1962). For him, the scientific revolution in general, and the Copernican revolution in particular, functioned as crucial examples for his analysis. He also had an explicit picture of a reactionary or rigid old guard, unable to move quickly enough to resolve the contradictions (in his terms, anomalies) in its scientific practice. The analogy with political and social revolutions seems to be commonly accepted by scholars, and so we can call on it whenever it is fruitful for our analysis.

In general, we can imagine a revolution resulting from the maturing of various tendencies in an older order, which had previously seemed marginal or unimportant difficulties; but which rather suddenly come together and make it impossible for the existing structures to perform their necessary functions. In a genuine revolution, there is also an explicit ideology, a simple vision of a better world, that provided coherence and drive to the effort, inspiring the activists to their sacrifices. As the revolution consolidates, that ideology is rendered false or irrelevant, the new order becomes stable and conservative, and a new cycle begins. This is a rough model of revolution in the sphere of power; let us see how well it works for ideas and techniques.

The Ideology

In this case we can start with the ideology, for out of it came not merely the achievements then, but also the shape of everything since. This is a composite of the pronouncements of the three great prophets of the scientific revolution, Bacon, Galileo and Descartes. Very different in circumstances, style, doctrine and career, they offered complementary visions of what the revolution was about. In the words of Auguste Comte, the founder of the philosophy of 'positivism', the revolution was based on the precepts of Bacon, the concepts of Descartes, and the *expériences* of Galileo. They are a diverse set: the English

humanist and jurist, the French metaphysician and mathematician, and the Italian 'physicist' and cosmologist; with only indirect acquaintance or none at all; and all ending their careers in bitterness and failure; but between them creating the synthesis of ideas and commitments that revolutized our knowledge and power over the natural world.

In this case it might be more accurate to speak of an ideological commitment common to all three prophets, rather than of an ideology as such. For while there was very little in detail on which their programmes all agreed, there was a common core to their commitment that can be defined. This can be expressed as follows: *The most valuable and powerful new truths are to be achieved by following a certain new method of studying the natural world considered as dehumanized and disenchanted.* This formula is tightly packed, and so I should discuss its elements in some detail.

The *value* of the work is best expressed by Bacon's aphorism, 'for the glory of God and the relief of man's estate'; for him (and also Descartes) their work was a calling, the redemption of mankind in his material existence, a task with strong millenarian overtones. Such a commitment was not new among philosophical visionaries in Europe; the novelty here was the invocation of material *power* as the means to the divinely sanctioned end. Previously, the highest good had generally been portrayed (for the élite) as something inward or contemplative, the cultivation of wisdom or even of religious experience. Now the path is seen as something external and activist, even when (as with Bacon and possibly Descartes) the spiritual component is essential. This theme of power was also no novelty for Europe, for magic had co-existed, in a frequently hostile interaction, with learning and religion from the beginning. For magic as such, our revolutionaries had nothing but contempt and scorn; in modern terms, they created the list of pseudo-sciences that has stood unchanged to this day. (We may say that this *negative* definition of science exhibits the essential connection between their revolutionary vision and our orthodox scientific common sense.) But they took over its aspirations, in a certain refined form; and this paradoxical act (one of the two key moves in the ideology) was crucial for the revolution.

One way that the ideology could sanitize magic was to make the craftsmen the surrogates for the magicians. On this theme there is more unity among the revolutionaries than on any other; each in his own way lauded craft practice as a better approach to knowledge than the book-learning of the time. Some historians have seen this theme as a confirmation of a Marxist interpretation; here was the unity of theory and practice which the representatives of rising, progressive class (then, the bourgeoisie) would naturally promote; and also here was a harbinger of the sort of science that would flourish under socialism. The first point is quite plausible, although it is not easy to identify any section of a bourgeoisie particularly on the rise just at this point in the early seventeenth century. The theme of power can also be seen as a reflection of the changing nature and status of certain practical arts in the preceding century, mainly those depending on mathematics and showing their usefulness for

navigation, warfare and civic pride. Training in these mathematical arts was standard for young gentlemen of the time.

The theme of *method* was not particularly new; it had already been advanced in the previous century. Also, it is hard to find a common positive theme among the versions of the method that were advanced by the three prophets. Of course, they all proposed experience and reason, in some mixture, in opposition to the book-learning of the professors and the enthusiasm of the magicians. Perhaps the important unity here is the stress on method for *discovery* of truths, accepting that in their time mankind was largely ignorant; this was in opposition to those philosophers and theologians who believed in the existence of a satisfactory body of known truths, available to persons with the right training and attitudes.

The theme of *novelty* was somewhat daring in its own time; institutions of learning were explicitly charged with protecting tradition rather than challenging it; and in religion novelties had a very bad reputation, as they had led to the excesses of populist, radical religion during the Reformation. By contrast, Truth was a traditional goal in European thought; and this we understand as genuine, indubitable truth, known to be such with certainty. Previously there had been great debates between the adherents of theological truth against those supporting philosophy; but there was agreement that truth is there to be found. One element in the background to this new ideology was the rise of 'scepticism', in some important cases (notably Montaigne) a reaction against the idiocies and horrors of religious intolerance; and it is possible to see Descartes, and perhaps Bacon too, forming their philosophies in reaction to the challenge of sceptical denials of the possibility of achieving truth of any sort.

Perhaps the strongest novelty in this ideology was its commitment to the discovery of truth through the study of the *natural* world. For us it might seem strange that people should believe that they could solve problems of ethics and even of theology by this route; but for Bacon and Descartes it was quite explicit and programmatic (and at the core of their endeavours), and even Galileo gave hints in that direction. But after all, this is the dominant belief of our time: that the natural sciences, both by their accomplishments and by their methods, provide the example for all the others. Such a radical shift in priorities needs explanation. If we exclude the folk-history tale that the obvious successes of science and of the scientific method were the source for the new methodology, then we are left to find some motivation outside science itself. This could lie in a general disillusion with all traditional forms of knowing, achieved either through literature, philosophy or theology. These had failed to prevent the splitting of Christianity into perpetually warring factions; and the meaningful world that their symbols invoked was fading fast. Descartes' criticism of the pretentions of humanistic education in the *Discourse* is a masterpiece of destruction of a culture; Bacon, while more sympathetic and also more discursive, shows the same commitment.

Finally, we come to the principle that the natural world is to be considered

as 'dehumanized and disenchanted'. This is the core of the metaphysical reconstruction wrought by the scientific revolution. In the first rejection is the well-known reaction against the philosophical system of Aristotle as interpreted by the modernized scholasticism of the sixteenth century. Its 'final causes' were the prime target of ridicule by the innovators, along with its 'substantial forms' and 'occult qualities'. It has taken a full three centuries for the natural philosophy of Aristotle himself to be rescued from the dustbin of history, to which it was consigned in those early modern polemics.

The *disenchantment* was not so clearly proclaimed by the revolutionaries, and its significance has become appreciated by historians only recently. The term refers to the denial of the existence of any conscious agencies or meaningful events, anywhere in the world other than in humanity and in our unique God. With this goes the denial of the prodigious effects that such agencies can produce with small or negligible physical causes. In the enchanted cosmos, there were highly developed rational sciences, such as astrology; there were practices in which the purification of external matter and of the soul were indistinguishable, such as alchemy; and there were many forms of divination, ranging from the most refined down to the most gross and superstitious.

It was the historians and critics of literature who first observed the rapid change in style and in figures of speech that occurred around the end of the Elizabethan period. They saw that this signalled a revolutionary change in the educated common-sense view of the cosmos. There is even a great poem to mark the change, that by John Donne where he laments,

> The new philosophy puts all in doubt
> The element of fire is quite put out
> All the world is reduced to atomies

although the poem is so early (1610) that it is more likely to be referring to the Renaissance visionary heretic Giordano Bruno than to the corpuscular philosophy which had not yet been announced. In the later seventeenth century there was an explicit awareness of the affinity between the new disenchanted natural philosophy and the new literary style ('plain' for the English, 'classical' for the French); and this was exploited for propaganda purposes by the apologists for the fledgling Royal Society of London.

In this disenchantment we find a powerful contradiction in the ideology, for the scientific revolution shared the theme of power over nature with the ancient magical arts. Yet it did not even debate with their devotees, but simply dismissed them with contempt. Thus Galileo, in a crucial passage in his great work on cosmology, pitied the late Kepler for having believed the astrological nonsense that the oceanic tides are influenced by the moon! Many sorts of knowledge about the natural world that had been highly regarded (though also strongly contested) including astrology and alchemy as well as many varieties of divination, quite suddenly, in less than a century, became objects of ridicule among all the educated classes. In this respect the modern

European educated common sense is unique among all the world's literate cultures.

This dismissal was not so simple as it might appear in retrospect; for the prestige of the ancient enchanted arts was just beginning its rapid decline at the time of the inception of the scientific revolution. For Descartes and Galileo, the crucial move was not so much the discovery of mathematics as a way to knowledge, as the disenchantment of the Pythagorean mathematics that was already extant as a traditional path to wisdom and enlightenment. For a contrast, we have Kepler's lifelong search for the harmonies of the Creation, and even more strongly the Englishman Robert Fludd with his cabbalistical proportionalities. As usual, Bacon was the least rigorous in his rejection of the old enchanted learning; he did not ridicule it, but rather condemned it, on two sorts of grounds. First, it made men slothful and careless in their study of nature, since it promised easy results, unlike the Puritanical message of a just reward for honest toil that his way offered. And second, it was simply implausible that gross effects should be the result of the small or insubstantial causes that are invoked in magic; and this commensurability of effects with causes was a strong support for the essential beneficence of science, until the advent of nuclear weapons in our lifetime.

There was a significant overlap between the activities of the pioneers of the scientific revolution, and those of the proponents of 'natural magic'. This was claimed to be the production of strange and wonderful effects by purely natural means; and indeed much of the popularity of science then and now is on just such a basis. One of the most successful of the natural magic school had a career that touched that of Galileo in several ways; he even had a claim to have invented the telescope. Outside the part of Europe where the scientific revolution was victorious, natural magic remained the vehicle for new discoveries; thus was von Guericke's great experiment on the power of air pressure announced to the world. There were even transitional cases, such as when the young John Wilkins wrote on 'Mathematical Magic', full of innocent wonders; and later he became one of the founders of the Royal Society. The total silence of the adherents of the new philosophy concerning natural magic is as strong an indication as any of the deep difference between them. For the prodigious as such was of no interest to these philosophers, as indeed not to science; and this is another indication of the deep change in world-view that underlies that revolution.

The most natural substratum for the disenchanted world of nature was that of dead, particulate matter. But this had the problem that it was perilously close to a well-known heretical position, that of the ancient atomists, notably Lucretius. Then and in more recent times this philosophy had served as a vehicle for anti-religious ideas; if we are only atoms, then we have no immortal souls, to be judged and punished after our deaths. Hence the Christian philosophers who espoused this atomism needed to ensure that their theology was such as to neutralize its subversive implications. Why they should have adopted such a position, known to be dangerous, is one of the more intriguing

questions about the scientific revolution. One answer, provided by the French historian R. Lenoble, is that the whole movement was theologically inspired; and that in response to the threat to belief from the Renaissance natural philosophers (who could deny miracles on the grounds that *anything* is possible in Nature), there was a need for a world-view that was even 'harder' than that of Aristotle; hence the move to atomism, in spite of its recognized perils.

It should be noticed that Aristotelian ways of thinking did not die off so quickly; it turned out to be difficult, scientifically as well as theologically, to deny 'final causes' and design in the organic world. The ideological significance of Darwin's theory of evolution by 'natural selection' was that it was taken to complete the revolution in natural philosophy that had been started some two and a half centuries earlier; those who debated for and against 'Darwinism' in the later nineteenth century were under no illusions that this was merely a theory within science.

Reviewing this revolutionary ideology, or commitment, we see that the term scientific revolution needs to be interpreted properly if it is not to be misleading. What happened *in* science was an accelerated progress on several fronts, with foundations being laid for later achievements within the rising paradigm. But the deeper change in thinking was *about* natural science: its objects, methods and functions, in relation to its character as a means to knowledge and power. In this way we can understand how the Copernican theory became so popular among the learned, in spite of its scientific weaknesses and counter-intuitive perspective. Also, we can appreciate how this tendency in science, at first marginal to the enterprise of learning and even more so to that of industrial production, eventually came to dominate and indeed define them both.

Ideology and Practice

We are interested in the scientific revolution not so much as an expression of philosophies of the world, but for its significance in the creation of the science which now so dominates our culture and our life. Was this revolutionary ideology a consequence of the success of the experimental–mathematical sciences, a drawing of the lessons of what had made it possible? Or perhaps was it the rallying cry, after which the sciences were transformed into their present shape? Both these questions are deliberately simplistic in their phrasing, and so we should expect complex answers. For the first, a negative answer is a good first approximation. Bacon never did any science worth the name in our sense; Descartes formulated his grand designs on the basis of a period of work at the beginning of his career that was incredibly successful but also quite brief; while Galileo had rather more successful science and less programmatic talk, at least until he attempted the biggest job of all (proving the Copernican system) and failed.

As befits prophetic utterances, or revolutionary ideologies, they come before

the really hard work, with a simple vision that makes the commitment possible. In historical retrospect, it is easy to query the extent to which they made much real difference. The new approach to science worked best with the sciences dealing with matter in its most abstract and general form, amenable to mathematical descriptions, and simple observations and experiments. These were mainly mathematics itself, astronomy, mechanics (extending from statics to dynamics and hydraulics) and optics; but these had already been well developed in classical antiquity, further refined in the Islamic period and brought to a new excellence during the sixteenth century. The revolutionary transformations of *doctrine*, as of the Copernican system and of Descartes' co-ordinate geometry, have tended to obscure the continuity of subject matter and method into and through the seventeenth century.

Outside these 'mixed mathematical' sciences, as they had been called for centuries, the record of success is mixed. Chemistry was transformed conceptually, as the 'corpuscular' philosophy replaced alchemistic or vitalist conceptions, but practice developed steadily without any sudden breakthroughs. The story with biology is similar, and the line of progress even less clear. Harvey's discovery of the circulation of the blood was invoked as a great propaganda point for a 'mechanical' or 'corpuscular' philosophy, on the basis of the analogy of the heart with a simple pump. But historians have recently made it clear that Harvey himself was firmly in the Aristotelian tradition; and in any event his theory of circulation rendered the old Galenic unified physiological system implausible while not suggesting anything to replace it.

As to power over nature, we can recall that in the early eighteenth century, after the revolution was all over, Jonathan Swift could make a savage satire of the descendants of the scientific prophets in 'Laputa', as either addled philosophers or cynical 'projectors' who left things far worse than they found them on the estates of gullible country gentlemen.

Even accepting all these reservations about the accomplishments of the pioneers of the scientific revolution, it would be misleading and indeed unjust to dismiss their efforts as being more philosophy than science. Something immortal *was* achieved by the pioneers, each in his own way; and as the revolution consolidated in the middle of the seventeenth century, there was a 'generation of genius', including (among the English) Hooke and Boyle, and culminating in Newton. Afterwards the impetus flagged, so that by the end of the century English natural philosophy was largely a subject for theologians, eccentric gentlemen and satirists. Newton, semi-deified, was above and beyond it all as far as the public was concerned.

Thus it would be fair to say that outside a few favoured areas, what the scientific revolution accomplished for scientific research was a change in methods and explanations that *eventually* produced the edifice of established knowledge that we now take for granted. The fruits were delayed in their coming by anything up to two centuries or perhaps more. Hence we can say that the revolutionary ideology was not sufficient to bring about the instant transformation that it promised. Also we can query whether in the context of

those times it was strictly a necessary change, in order that scientific progress could occur.

The assumption (common from then to now) that the great scientific achievements of the scientific revolution required *our* sort of metaphysics of nature and conception of method is falsified by a close scrutiny of the works of some of the greatest innovators. At the turn of the century, from the sixteenth to the seventeenth, there worked three very great scientists: Gilbert had just completed his researches on the magnet; Kepler was engaged on his earlier work in astronomy; and William Harvey was completing his studies in anatomy and physiology. Each of them made a great scientific discovery: Gilbert of the earth as a great magnet, Kepler of the laws of planetary motion, and Harvey of the circulation of the blood. Yet each of them lived in a world that was in some degree enchanted, endowed with world-souls or life forces, that were soon to be declared anti-scientific by such as Descartes and Galileo, and that would remain in that category until our time.

Thus the ideology of the scientific revolution was framed in contradiction to what was scientifically successful in its own time, and then failed to produce immediate successes on a broad front outside the traditional matured mixed-mathematical disciplines. We might even question whether in those fields outside what we now call physics, the adoption of the new paradigm was on balance a 'correct' strategy for scientific advance in its time. But such a question, like all the big ones in history, rests on counter-factuals, and so cannot be effectively pursued.

Historical Interpretations

Now, some three and a half centuries on, we are left with the consequences of that creative period, in a science that has unprecedented power in its own terms, but which has created the possibility of evil on a scale commensurate with that of its good. We have mentioned how the heritage of the scientific revolution includes not only our common sense of the world of nature, but also the secularizing, critical enquiry of the Enlightenment, and finally the industrial innovation and eventually the science-based technology that brought Europe to world domination. With this perspective of hindsight, we can return to the question of the relation of the scientific revolution to Europe, and for the sake of our understanding of ourselves, try to understand the scientific revolution.

For this, the work of historians promises much but in the event offers tantalizingly little. We still have to contend with a sort of folk-history that played its part in the ideology of science from that time until ours, which is the story of heroes. These were such as Galileo and Descartes, who made those essentially simple discoveries which form a part of the core of elementary science. How did they do it? Their secret was simple common sense and sound scientific method, which in their day was a great achievement since most men's

minds were still captive to various distorting influences, either Aristotle or magic or metaphysics. There is a more sophisticated version of this tale, created by the first generation of critical historians of science. In this the great event is a change in Ideas, from the Aristotelian world-picture to a mathematical one, inherited mainly from Archimedes; and the transformation is exemplified in the mechanics of Galileo.

These folk-histories (with their variants) are now not so commonly told as they were just a generation ago; the recent transformations of science and of its consciousness make them appear naive in the extreme to those of us who know about the Bomb, ecology and acupuncture. In its place there have appeared some attempted alternatives, serving to demystify science and to exhibit it as part of the apparatus of social and political oppression. The most strident of these attempts is that of a feminist perspective. There, the scientific revolution has been interpreted through the theme of 'the death of Nature', where the disenchantment is seen as the destruction of feminine earth-consciousness by that of the alienated, phallic, patrial male. This works quite well at the level of consciousness, and even has its social correlate in the witchcraft craze (which lasted well into the seventeenth century) and the associated takeover of female medicine from the *sage-femmes* by men with university degrees. But it is difficult indeed to locate a strong feminine consciousness in previous ages in Europe, to say nothing of a feminist society existing within some millennia of the scientific revolution.

Rather more plausible at the moment are the sophisticated versions of Marxism. It is always useful to be reminded that literate culture has always been dominated by the classes possessing political and economic power; and that this culture will be deployed by them, on occasion quite self-consciously, for their material and ideological needs. The leading scholar in this new tendency has been M.C. Jacob; in her book *The Cultural Meaning of the Scientific Revolution* (New York, Knopf, 1987) she has shown how the 'new philosophy' of the seventeenth century was quite explicitly and unashamedly seen by its proponents as a means of protecting social stability during a period when it was quite precarious. In England it is most clear how the movement was of the centre, opposed both to the 'right', in the totalitarian Roman Catholic Church, and to the 'left', in the politically and religiously radical sectarians. Hobbes in particular is quite explicit on this, closing his *Leviathan* with a proof of the identify of the Kingdom of Darkness and the Kingdom of Fairies. For me the most telling incident of the whole episode, in this respect, is the Webster–Ward debate of 1654, just as the Puritan Revolution was winding down. In this, the radical John Webster called for an experimental science that was also Christian and Paracelsian; while his Oxford opponents had to admit that, in the last resort, universities were not primarily about advancing learning but about socializing the young élite.

This Marxist interpretation is given added strength by the political and cultural geography of Europe. The new approach to studying nature

flourished first in Northern Italy, just before economic decline and the counter-Reformation sent the area into stagnation. The centres of excellence then moved to the expanding economies of north-west Europe, including France, the Protestant Netherlands and England. The Habsburg Catholic countries of southern and central Europe, and the war-torn fragmented German states, were left behind for a century or even two. Thus a rising capitalism (including for these purposes its statist version in France) was the background for the development and consolidation of the scientific revolution. Certainly, no one in the seventeenth century had any conception of 'pure science', an activity irrelevant to commerce, statecraft or philosophy; that was an invention of German professors much later on.

But it must be admitted that the correlation of the rise of capitalist modern science works best on a broad scale in space and time. Attempts to show that a particular class interest or stated need led to a particular great discovery have so far proved fruitless. Also, the Marxist approach seriously undervalues the significance of the 'absolute state' in early modern Europe. The rulers of those, big and small, were the effective patrons of most of the mathematical practitioners of the time, rather than some section of the 'bourgeoisie'. Galileo himself said (although admittedly in a letter applying for a job back in Florence) that he preferred the patronage of a Prince to employment by a Republic. What may be called a simplistic Marxism applies best to the rhetoric of that time, though of course it fits well with much of scientific practice now.

This account would not be complete without a mention of religion and theology. For many years the Galileo affair was taken as a type-case, of how religious institutions (and hence, by implication, religious belief) are antithetical to the progress of science. The fact that Galileo and all the other great scientists were believers, and many of them Catholics, was just an anomaly to be adjusted. Then the story became complicated, as some scholars discovered affinities between certain Protestant principles and the scientific endeavour, following on Max Weber's identification of the 'spirit of Capitalism' as related to the 'Protestant ethic'. There even developed a revisionist thesis, to the effect that the uniqueness of Europe, enabling the scientific revolution to occur at all, was located in its Christian tradition. Certainly there is much to be said on this score; the tendencies to a voluntarist theology (emphasizing God's unrestricted will) seem to have been associated with other currents in the new philosophy. But my inclination tends away from looking at such specialized intellectual currents as independent agencies in history. For me the most appealing theological argument is the one I mentioned above, where the issue of miracles (which was a political question as much as theological) was instrumental in turning men towards a corpuscular philosophy.

Roots of the Scientific Revolution

For myself, the guiding principle is that the workings-out of history are complicated, and therefore single-cause explanations are sure to be oversimple. My preference is to identify several roots of such a development, to see which were more general and which more specific to the time and cultural milieu; and from that to derive an assessment of the particularity of the event. The practical consequence of such an approach is that it can enable us to see more clearly both the variations within the process as it occurred in Europe, and also the possibilities and problems of reproducing it in other times and cultures. Such extensions are, unfortunately, far beyond the scope of this present essay.

We can identify four roots of the scientific revolution, all capable of being traced back for some centuries previously in European history. Proceeding from the material aspects, we first have what can loosely be called 'capitalism': a productive economy dominated by a market of relatively unfettered operators devoted to self-enrichment, rather than by organizations created for the service of the power and glory of an absolute ruler through military might and religious culture. (Notwithstanding everything I said above about the absolute state, in the matter of organizing *charisma* for state purposes, these rulers were incompetent amateurs compared with those of the East.) The second root can be seen as a cultural reflection of this economic style: a new conception of the good life for the élite, away from the contemplative virtues of learning, wisdom and religious enlightenment, towards an activist, manipulative approach to nature and society not merely for practice but also for the highest good. Then there is (thirdly) the technical background, in the recovery and development of the sciences and arts of classical civilization, first through their adaptations in the Islamic civilization, and then directly through 'humanism'. Finally, there is the most subtle and pervasive change of all, which fortunately can be documented by crucial shifts in evaluation of forms of knowledge; this is the dehumanization and disenchantment of the external world, to a degree that makes Europe quite unique among all the major world civilizations.

To deal with these briefly, the period of early capitalism produced a rapid development of techniques in all fields, but also crucial changes in the social relations of intellectual property and hence also in its evaluation. For in this period there developed a market, of a somewhat special form, for useful knowledge. There was a new class of freelance (*sic*) experts, of which both Leonardo da Vinci and Galileo were members. They relied mainly on patronage of the great and wealthy, but given the insecurity of such support, they had to advertise their skills publicly. This was done partly by books that they published, which necessarily gave away some of their knowledge to any reader, but which also demonstrated their prowess and their promise for their next employer. Such knowledge extended over many fields; but most significant were the techniques associated with conquest and war. Sciences such as

navigation, surveying and fortification were crucial. They involved advanced mathematics, and so their practitioners could not belong to lower social strata; and they were also fields which gentlemen were expected to understand. Hence during this period there was a temporarily lowering of the barriers of snobbery between the 'liberal' and 'mechanical' arts. Descartes learned all the applied mathematical subjects at his Jesuit school; and Galileo taught them to his boarding pupils whom he took at Padua for extra income and future patronage. From this root could be seen to derive the respect for craftsmen among philosophers, and also their belief that through utilizing them, natural philosophy could accomplish what the magicians had always promised and never delivered.

An associated development in this same violent period was a change in the value system inculcated into the youth of the élite for the purpose of reproducing the social system. It had already been initiated in the Renaissance period with the famous 'discovery of man and nature' in the context of a professedly Christian culture. A symptom of this change is the work of Machiavelli, whose books replaced a centuries-old tradition of handbooks of advice to princes, which contained all the high-flown morality. Machiavelli was in his own way an idealist as well as a patriot; but he perceived that the realities of the baser human drives must be systematically mastered through fully explicit teaching, if there is to be any effective government at all. Francis Bacon summed it up when he announced the three grades of ambition for (élite) mankind, replacing the traditional set which were progressively less materialistic. For him it was a question of domination: at worst for oneself, better for one's nation, but best of all for the whole human race over nature.

All these attitudes would have been to no avail, in the absence of a technical basis for scientific advance. And this was there in good measure. Thanks to printing and to the market for expertise, published books in all subjects grew rapidly in number and in sophistication. At the beginning of the sixteenth century Europe was still translating and assimilating the scientific heritage of the Classical and Islamic civilizations; by the end of the century the work is fully matured technically, of a quality and style that can be read now without embarrassment or apology, in a variety of fields ranging from astronomy to anatomy. Hence when the new philosophical commitment was injected, there was some quite solid technical material for it to work on, at least in the more mathematical fields. With the steady development through the century in astronomy and mathematics, and the consequent transformations in cosmology and mechanics, the materials were available for Newton and his successors to create the vast edifice of science as we know it now.

The story as told along the lines of these three roots is self-consistent; and yet that driving commitment to a *particular* sort of experimental-with-mathematical explanation of nature, combined with that *particular* sort of belief in human power over a dead nature, is still to be explained. In other words, we can explain the rejection of the Christianized Aristotle in terms of a changing social and ideological function of knowledge; but the retention of

magic's aims, combined with the rejection of its means and world-view, still calls out for explanation.

We can put the issue in terms of the question, why the three prophets of the scientific revolution, with all the differences between them, were completely consistent in rejecting those ancient sciences which most of their contemporaries were still willing to entertain. It is one of the few themes which they all stated strongly; and more than any other it gives their work a common modern feel. The background in cultural context seems to have been a quite sudden acceleration of a shift of sensibility, which had been proceeding for some centuries previously. In their own time there are many cases of world-views of great scientists which to us seem bizarre mixtures; thus the astronomer Tycho Brahe disproved the reality of the 'crystalline spheres' which on the Aristotelian theory carried the planets around in the heavens; yet he was an enthusiastic practitioner of astrology and alchemy. One can find roots or anticipations of the scientific revolution in Luther's insistence that the Bible is a plain historical document rather than a mystical allegory; or even in Aquinas' definition of miracle in terms of interference with natural laws. If there is anything uniquely European about this transformation of consciousness, it is in this hardening of common sense, to the exclusion of both non-tangible causes and prodigious effects, first from philosophical or religious significance and then even from existence.

The Heritage of the Scientific Revolution

In this essay I have been able only to sketch some ideas about the background and initiation of the scientific revolution. As we might expect, its career was very different in the different parts of Europe; from a start in Italy, it moved to France and then England, following the shift in favourable environment, both in economy and in politics. In the German culture area it came late and partially; the struggle for the elimination of enchanted philosophies of nature was won there only in the mid-nineteenth century. Elsewhere the old symbols were picked up and adapted to new ideological struggles, as with the *philosophes* in eighteenth-century France. In the Catholic lands of the Counter-Reformation, Galileo has remained a living symbol (for both sides) until our own time.

Inevitably, as the revolution consolidated, it lost its ideological aggressiveness. At the outset the battle was over the message to reach the literate (hence élite) public, and the first tactic was to bypass the established educational institutions and challenge their monopoly. Since it was always an affair within the élite sectors of society, once that institutional battle was won, the doctrines of the new philosophy could be devoted explicitly to the service of the stability of the ruling social and cultural institutions. In eighteenth-century France, Descartes became the symbol for a new conservatism, and in England, Newtonianism was invoked in proofs of the wisdom of the creator in

His fashioning of the perfection of the natural, and by extension, of the social world.

In the perspective of centuries later, we can ask whether there was something uniquely European about the scientific revolution, with both historical and policy questions in mind. For history, if we were satisfied with its uniqueness in time and context, then we could readjust our approach to the science of other great civilizations, such as the Indian, the Chinese and the Islamic. We could drop the perennial question of why they failed to make it, and concentrate on evaluating them in their own terms. For policies for the future, we can enquire about the chances for the spread of science *as we have understood it* outside Europe. The relevance of this is the challenge of the new East-Asian nations, from the small to the large to the gigantic. Although the social and institutional conditions for the flourishing of the best creative science have changed through the centuries, and there is now no need for anyone to re-invent the scientific revolution, still we may say that if the initiation of this very special cultural product was somehow unique to Europe, then its transfer to other cultures is still problematic. This is not to deny the scientific and technological excellence which can be and is achieved abroad. Rather, we have to consider the possibility of the appearance of a generation of genius, which provides inspiration and examples for many generations that follow. So far, nowhere outside Europe has this occurred on a large scale, even within any field of the differentiated science of our time. But of course it is still early days in the maturing of modern non-European civilizations, and so there may yet be surprises to come.

Finally, there is the biggest question of all, raised by the ecological threats that, in their urgency at least, seem to be a product of the technology that has emerged from the scientific revolution. These give new life to the questions of excessive powers of knowledge over nature, of which the magical tradition was explicitly (if mistakenly in its own case) aware. We do now create prodigious effects with very small causes; and the new problem of control has now emerged as basic to our exercise of power. In these respects at least the assumptions underlying the world-view of the scientific revolution need correction. Whether the new consciousness produced to meet these new challenges will require a modification of both the activist ethic and the dehumanized and disenchanted cosmology of the scientific revolution, is something that only the future will tell.

Based on an essay published in J.R. Ravetz (1966), *Indian Journal of History of Science* 1 (1).

Francis Bacon and the Reform of Philosophy

Of all the great figures in the history of science, Francis Bacon is the most enigmatic and controversial. Some even deny him any place at all in the history, for he did no worthwhile science of his own, and on what we now see as the major issues of his day (the Copernican revolution, and the introduction of the mathematical approach) he guessed wrong. Yet for several centuries his memory was venerated as one of the founders of modern science, as important in his way as Galileo and Descartes in theirs. This mixed and contradictory reputation extends over his whole career, and indeed began in his own lifetime. For someone whose towering genius was recognized from his childhood, and who devoted his life to service of his country and mankind with hardly an evil thought, he had a strange power of attracting condemnation and even emnity.

In his lifetime he enjoyed the patronage of the greatest of English monarchs, Elizabeth I; and under her successor rose to the highest judicial position in the land, Lord Chancellor. His published essays were influential in his lifetime and for generations afterwards; and so universal was his learning that for a time a strong school of literary scholars argued that he was the only possible author of the plays attributed to Shakespeare. Yet when he was just at the pinnacle of success, he was disgraced on a charge of corruption, and spent the last five years of his life a broken man.

His ideas for science were adopted by the founders of the Royal Society of London; from him they learned the virtues of patient empirical research, done in a socially organized framework. Later, his 'inductive method' was taken as the model for disciplined scientific enquiry. But in recent generations historians found his organizational schemes irrelevant to the real work of science, and his methods applicable only to a small and not very important part of science. The rescuing of Bacon's reputation was started by Benjamin Farrington, who saw in him a 'philosopher of industrial science', and who later (as he himself matured) perceived the significance of Bacon's spiritual endeavour. My own study of Bacon was stimulated by both these works of

Farrington; and historians are now more willing to admit arguments such as ours than they were at the time of the first publication of this essay.

In recent years, historians of science have come to see that the establishment of the new style of investigating Nature in the seventeenth century was different in many important respects from the tasks of consolidating and extending this work in later centuries. The greatest men were concerned with philosophical problems as much as with 'scientific' ones, and indeed did not generally make any sharp separation between the two classes. The debates that took place were similarly a mixture of technical and metaphysical considerations. And engagement on the work was in many cases at least as much participation in a movement, as the following of a profession. With the new appreciation of this complexity of the work of the scientific revolution, the old opposition between internalist and externalist approaches to the history of science can be correctly seen as the reflection of general philosophies of history imported into this special study. Each historian will naturally investigate those problems to which his interests and skills direct him; but no one would now deny that the adoption of a new philosophy of Nature produced a qualitative difference between Galileo and earlier practitioners of the mathematical arts, nor that Boyle's and Newton's interests included the experimental philosophy of nature only as a special part.

This ecumenical approach brings many advantages, not least the freedom from choosing sides in a sterile debate. On the other hand, it has its characteristic dangers, in blurring the lines of definition of the subject matter of the history of science. This is not so serious when it comes to distinguishing (for purposes of historical analysis) between particular studies in a natural science, from those in general philosophy, and from craftsmen's empirical investigations. It does raise the deepest problems for historical enquiry, at those points where the fields of enquiry involved have been subsequently excluded from the domain of genuine science. To argue, for instance, that magic and alchemy, or generally the Hermetic tradition, played an essential and positive role in the establishment of modern science is to contradict a tradition of the conception of science which goes back continuously to the earlier seventeenth century. To admit mystics and Rosicrucians into the respectable ancestry of our modern science may seem to involve a betrayal of the long struggle for the establishment of reason as the foundation of judgements in affairs concerning both Nature and man.[1] But we now know that we cannot simply exclude from the earlier history of science any man whose philosophy of nature would have been unacceptable to late nineteenth century German analytical chemists. Long ago Dr Walter Pagel exhibited the rich mixture of motifs involved in the work of van Helmont and his school; and more recently he has restored William Harvey as a philosopher rather than an hydraulic engineer.

Reason itself requires that we should not run away from established facts merely because they are uncomfortable to our inherited prejudices. Also, in this later twentieth century, the focus and emphasis of the ideological struggles

involving science have changed suddenly from the versions that were current from the Enlightenment onwards. The completely natural powers provided to man by science are so great that sorcery has re-entered the vocabulary of discussions of science as a moral attribute.[2] And the Galilean style of applying 'disciplined experience and necessary demonstrations' to simplified and abstracted aspects of the natural world has in technical applications led to the micro-rationality of devices which each perform their assigned functions superbly but which in aggregate threaten the survival of our species. In reaction to these new problems, the long-submerged current of mystical thinking has surfaced again, not merely in the counter-culture of rebellious students, but even in influential currents in the new ecological thought and propaganda.

With these new experiences of the present, historians can and should have a new appreciative perception of the styles of thought that were suppressed in the seventeenth century. And as historians rather than propagandists, we can and should avoid a facile oversimplification of the complex and sometimes tragic interaction between styles of investigating nature which derived from opposed conceptions of that world and its relation to man and to God. In particular, the concept of influence (which more than any other carries the load of valuations) is a simple one only if we conceive intellectual history as a genealogy of ideas, hopping from book to book down through time. Rather, we should see the great philosophers as men grappling with the deepest and sometimes insoluble problems throughout their lives, adopting different provisional solutions, and thereby being open to different influences at different times; and also struggling with the relics of their own earlier thoughts as they change and develop.

In this historical framework, the real influence of those currents of thought soon to be damned as irrational can be established, without necessarily pitching the historian into the very deepest questions of judgement on the whole process of the establishment of the new philosophy of Nature. We need only imagine that some at least of the great natural philosophers achieved commitment to their life's work in a period of youthful enthusiasm and dedication; and spent their subsequent years in a struggle to retain what could be retained, and to achieve what could be achieved, in the face of the contradictions thrown up by harsh experience. Although such a pattern is commonplace in politics, it may seem entirely inappropriate to import it into the history of science. But the history with which we are concerned just now is of science in the largest sense; it concerns a movement for a Reformation in the philosophy of nature, in which the achievement of a particular sort of results by a particular style of research was only a part.

The career of each of the founders of the new philosophy of the seventeenth century can be studied in this way, and the differences in their achievements can be related to differences in their style and commitment. Thus, Galileo's Truth lay in a particular sort of realized mathematics, and his characteristic style can be seen in his very earliest production, the *Bilancetta*. On the deeper

problems of metaphysics and ethics, he left very little evidence of concern; and even his theological excursions were in the nature of defensive polemics. For Descartes, the evidence of enthusiasm is there, but it is suggestive rather than conclusive for a direct personal involvement with Rosicrucianism.[3] However, the autobiographical accounts of meditations and insights, and the 'three visions', leave little doubt that Descartes was started on the road to his extension-and-motion ontology by problems of philosophy, and by experiences, which touched the roots of the problems of human existence. For Bacon, the case is easier to argue, and correspondingly less disturbing in its conclusions. Bacon was neither a successful scientist, nor a consistent adherent of the new ontology of dead matter. A demonstration that his philosophy of science was influenced, and even shaped, by non-scientific considerations may even offer ammunition to those historians of science who have all along wanted to throw him out of the list. But since the list of thoroughgoing professional scientists in the seventeenth century becomes shorter with each advance of historical scholarship, such prejudices may be allowed to wither away of their own accord.

Strategies for Reform

There is no need to argue the case that Bacon saw himself not so much as a scientist, but as an agent of Reform, where the term is understood in its sixteenth century rather than twentieth century sense. By contrast, Descartes can be considered as trying to accomplish the sorts of task appropriate to both roles; and Galileo might be said to have become a reformer only when the unreformed state of his audience's minds became a nuisance and hindrance to him. We can therefore expect that Bacon's involvement in systematic philosophy, and in experimental science, would be casual and incidental; his achievements correspondingly less important; and the significance of his essays in these directions for an understanding of his basic philosophy, minimal. We have also got beyond the earlier tradition in British philosophy which constructed a Bacon who proclaimed the 'inductive' philosophy of the later eighteenth and nineteenth centuries. Hence we are entitled to approach an understanding of Francis Bacon by an analysis of his strategy for reform in philosophy, and to use this as an interpretative framework for his many and varied pronouncements on the state of the world about him.

Any strategy for reform must include several distinct components if it is to serve as a coherent guide to action. Without arguing on the absolute correctness of any particular taxonomy, we may conveniently distinguish the following aspects. First, there must be a description of the present lamentable state, and an explanation for its occurrence. Second, there must be some characterization of the desired state, and also a guarantee for its possible existence. Third, there must be a plan for changing the bad present into the good future. And finally, there should also be some indication that the time is

now ripe, so that recruits will come forward, and survive the inevitable temporary disappointments. To run this scheme on a familiar example, we may say that classical Marxism described the miseries of the proletariat and explained them in terms of the particular form of expropriation which defines capitalism; it was rather vague on socialism and on its variety of communism, but prided itself on its 'scientific' as opposed to Utopian analysis of their inevitability; its plan rested on the conclusions of the earlier phases of analysis, and involved the activation of the industrial proletariat rather than of other classes who might be oppressed or rebellious (peasants and young intellectuals, for example); and the ripeness of time was established both by the intensified struggle of the proletariat and by Marx's optimistic aphorism, 'Mankind sets only those problems which it can solve'.

For Bacon, a plausible analysis of the strategy for reform can be achieved without difficulty. The present ills are described in many places, in a variety of ways. Starting with the dichotomy between 'the Grecians and the alchemists',[4] we have the simile of the spider and the ant,[5] and the fine passage on the varieties of misguided endeavour in *Novum Organum* I, Aphorism 95.[6] For an explanation of present ills Bacon offers his Four Idols, perhaps the most original part of his entire philosophy. The guarantee of the possibility of improvement in philosophy comes from the example of the mechanical arts, both in their progressive character[7] and in their previous achievements in the absence of any contact with philosophy.[8] For the advancement of learning itself, the institutional form is sketched as Salomon's House in the *New Atlantis*; there we see a fine division of labour,[9] and a fully autonomous community of research workers, gathering and processing facts in a disciplined style. The plan for the improvement of philosophy is based on achieving 'colleges' where this sort of work can be done, and its correctness proved by its successes; and Bacon's own contribution is to provide the propaganda leading to the establishment of such an institution, on a Royal foundation. Finally, the ripeness of time is established by the numerous radical changes in the arts and in society over the few generations preceding Bacon's time; the frontispiece of the *Novum Organum*, with the Columbus of learning setting forth, symbolizes the argument.

Such a strategy for reform fits in well with what a twentieth century audience would expect, and as it is displayed in his writings it uses themes which must have been quite familiar to his own contemporaries. As evidence for Bacon's own views, it is not to be neglected; but it is evidence and not fact. All Bacon's published writings were propaganda; their function was to convert his audience, and their relation to his own private views was purely incidental. Indeed, the essays which he suppressed as unsuitable for publication have a style and content which is strikingly different from those of his published writings; and it was these essays that provided Farrington with the clue to the deeper interpretation of Bacon that he achieved in his second study.

There is strong evidence, from the history of Bacon's life and work, to indicate that the publicly announced strategy of reform was only a part, and not the deeper part, of his personal vision of the task. In the first place, on the matter of the mechanical arts, he knew not whereof he spoke. The three great inventions of printing, gunpowder and the magnetic compass were not first identified as such by him.[10] They were discussed by Cardano, and perhaps more significant, were dealt with in a popular French book, which was translated into English in 1594. The moral that Bacon drew from these inventions, that they were lighted upon by chance and owed nothing to 'philosophy', is not merely incorrect but would have been recognized as such by anyone then familiar with their recent history.[11]

Again, it is well known that Bacon's knowledge of the state of the sciences derived from his reading of books, and mostly general books at that. In retrospect, historians of science can discern fields in which great advances were being made in the period up to Bacon's life; in particular, anatomy, astronomy and mathematics. Bacon showed no recognition of these points of progress, but laid all of the sciences under his general condemnation. His conception of the ideal organization of scientific research could not have come from any examples within philosophy or the mechanical arts; but it is a natural extrapolation from the type of research appropriate to a programme for the rationalization of English law, one which was very dear to his heart. Indeed, his writings show an intellect trained in legal and literary skills, applied to this very different sort of work. His similes and rhetorical figures are nearly always taken from these fields and applied to natural philosophy and the arts; the cases where 'nature' provides the insights for 'man' are very few.

The conclusion of this line of argument could well be that Bacon was merely a literary showman, offering advice and instructions to people in a field of enquiry which he was too proud or too busy to learn properly. And, judging his published writings as propaganda exercises, such a conclusion would be hard to refute. But if we accept that with all the complexities and contradictions in his character, he was moved by a very deep commitment, this negative conclusion serves to throw up a new problem: what was he trying to do? If his programme for the sciences was not based on induction from personal experience of philosophy and the arts, wherein lay its driving force for him? Benjamin Farrington has provided the elements of an answer: that Bacon's deepest commitment was ethical and religious; and that the reform of natural philosophy was his choice of the strategic point for the achievement of the reform and redemption of mankind. To confirm and amplify this insight, I will show how a coherent and meaningful strategy for reform can be extracted from Bacon's affirmations in religion and ethics; and that this element is in fact essential for solving the historical problems raised by the technical and secular version of his strategy.

Bacon's Strategy for Reform

We need not dwell on Bacon's numerous criticisms of the state of natural knowledge in his time. The causes of this evil condition are in three classes: ignorance of means, corruption of ends, and inherent infirmities in human reason. In the first class we have the analysis of the sterility of school logic, and of the one-sided scientific efforts, either purely empirical or purely theoretical; and then the positive suggestions towards a method of true induction that comprise the second book of the *Novum Organum*. On the ends of the endeavour, Bacon describes the narrow and distorted ends then governing the various sorts of work,[12] and offers several formulations of the true ends of natural philosophy, as 'to establish and extend the power and dominion of the human race itself over the universe'.[13] As an explanation of the corrupted state of philosophy, he provides the Four Idols, which seem to be a deeper sceptical critique of human knowledge than the classical tradition provided, and indeed in some respects deeper than that with which Descartes grappled. Bacon starts with the defects of the mind itself, neither a *tabula rasa* of the empiricist tradition nor the true 'mirror' of the rationalists.[14] These imperfections are magnified in each individual, according to the peculiarities of his constitution and temperament. He is then subjected to the brainwashing of school, where he reasons with words that do not correspond to real things. Finally he comes to the theatre of higher education, where actors spout their lines devoid of all content.

At the naturalistic level, this explanation is self-sufficient, and indeed relevant to all times and places. But at the moral level, it has no meaning, except that of cynicism or despair. In itself, it certainly offers no clue to the possibility of reform; for any ordinary institutions would inevitably be corrupted by the prevailing tendencies to intellectual and moral decay. Bacon gave explicit recognition to the insolubility of the problem at this level, at the conclusion of his discussion of the Four Idols. There we read:

> So much concerning the special classes of Idols, and their equipage: all of which must be renounced and put away with a fixed and solid determination, and the understanding thoroughly freed and cleansed; the entrance into the kingdom of man, founded the sciences, being not much other than the entrance into the kingdom of heaven, where into none may enter except as a little child.[15]

Is this comparison a mere figure of rhetoric? It seems unlikely to be so, for two reasons. First, this call for a moral reform (the cleansing as well as the freeing of the intellect), the requirement of the innocence of the child, is Bacon's only answer to the sceptical challenge of the Four Idols. Second, and more important, the conception of human history which was a commonplace for Bacon and for his successors through Newton was that of a cosmic drama in which the successive acts were revealed in Scripture, and in which the

Almighty is ever-present. The analogy between the two kingdoms was not to be uttered lightly.

A stronger connection to the religious foundations of Bacon's vision is provided by a theme which is expressed in passages scattered through his published writings, and which dominates his unpublished essay, *On the Masculine Birth of Time*.[16] The absence of the true ends of philosophy is not merely an intellectual deficiency; it is a moral defect as well. In that essay Bacon runs through the list of philosophers, ancient and modern, calling them to the bar of judgement. He speaks of the 'sham philosophers' who 'debauch our minds', and of those who are worse still, 'the satellites and parasites of the great ones, the whole mob of professorial teachers'. Lest there be any doubt on this point, he concludes:

> But now I must recollect myself and do penance, for though my purpose was only to discredit it yet I have been handling what is unholy and unclean. What I have said against them is less than their monstrous guilt deserved.[17]

What is this 'monstrous guilt'? It is composed of spiritual pride, showmanship, dishonesty, and lack of true humility before Nature or pity for mankind. To put it in a single word, we may say, 'vanity'. Bacon mentions vanity in an important place in his published work, in the prayer which concludes the Plan of the Work of the *Instauratio Magna*:

> But man, when he turned to look upon the work which his hands had made, saw that all was vanity and vexation of spirit, and could find no rest therein.[18]

The same text is found in the *Meditationes Sacrae*,[19] and in the companion piece, *A Confession of Faith*, vanity comes into the cosmic drama:

> That upon the fall of Man, death and vanity entered by the justice of God, and the image of God in man was defaced, and heaven and earth which were made for man's use were subdued to corruption by his fall.[20]

A similar set of themes appears in a passage in the introduction to the *Historia Naturalis et Experimentalis*:

> For we copy the sin of our 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 Divine wisdom, or as they are to be found in fact.[21]

The need for curing this vanity, as a prerequisite to any progress in philosophy, is expressed in the Preface to the *Instauratio Magna*:

> Wherein if I have made any progress, the way has been opened to me

> by no other means than the true and legitimate humiliation of the human spirit.[22]

Later in the same section, he concludes his prayer:

> Lastly, that knowledge now being discharged of that venom which the serpent infused into it, and which makes the mind to swell, we may not be wise above measure and sobriety, but cultivate truth in charity.[23]

If we wish, we can dismiss all this as rhetorical high-mindedness, supported by conventional piety. But to do so would require a wilful ignorance of the religious sensibility of English natural philosophers throughout the seventeenth century. It would also require us to imagine Francis Bacon, a man conscious of his talents from his earliest years, and determined to dedicate himself to the service of God and of man, spending so much of his life on a purely technocratic fantasy.

Taking this ethical and religious concern seriously, we note in the above passages that there is a scriptural reference in the descriptions of the corrupted state. The 'sin of our parents' is that of Adam and Eve, and the 'serpent' is their tempter. Indeed Bacon sketched a history of the stages of the Fall of Man, relating the corruption of philosophy as he saw it to the scriptural account. Concerning the Fall itself, Bacon is quite sure that this did not arise from man's desire for natural knowledge;[24] but just as the angels fell from lust of power, so man fell from lust of knowledge:[25] a knowledge of Good and Evil conceived as independent of God's will.[26] Bacon believed that the Fall of Adam was not complete and absolute (in agreement with those who traced the *prisca sapientia* to Noah, such as Newton and the Masonic tradition); for then,

> the law was first imprinted in that remnant of light of nature which was left after the fall, being sufficient to accuse.[27]

Later the manner of revelation changed, to the written law, the prophets, and finally Christ. However, even at that first Fall, there was

> the curse, which notwithstanding was no new creation, but a privation of part of the virtue of the first creation.[28]

Bacon does not hope for the original 'virtue' of nature's workings to be restored; man must forever earn his rewards.

> In fact, there has been a second Fall; Wherefore our dominion over creatures is a second time forfeited, not undeservedly; and whereas after the fall of man some power over the resistance of creatures was still left to him—the power of subduing and managing them by true and solid arts—yet this too through our insolence, and because we desire to be like God and to follow the dictates of our own reason, we in great part lose.[29]

Bacon nowhere speaks explicitly of the time and character of this second Fall,

but it is likely that he gave some support to a popular doctrine that it occurred with the building of the tower of Babel,[30] and also that traces of the true wisdom survived to the times of the development of Greek mythology.[31]

In the terms of this deeper analysis of the causes of the corrupted state of philosophy, the problem of providing a guarantee of successful reform is easily solved within the same framework. On this, Bacon is quite explicit. He made strong use of the injunction of Christ, 'Ye err, not knowing the Scriptures, nor the power of God';[32] from this distinction, he can interpret the former as revealing God's will, and the latter, His works as studied by natural philosophy. Moreover, Bacon provides an abundance of points to prove that God intended man to discover the nature of his Created world. First, He left his 'seals and imprints'[33] on things, as well as his 'footprints' or 'vestiges'. And these are the true Ideas of the divine, which are so different from the Idols of the human mind.[34] It is through God's grace that man will 'write an apocalypse or true vision of the footsteps of the Creator imprinted on his creatures'.[35] Moreover, these 'vestiges', although not patent to the common view, were intended to be discovered. His hiding of the 'characters and impressions of his providence',[36] as in the final causes of natural processes, makes His wisdom 'shine forth more admirably', as that of the master politician 'that can make other men the instruments of his ends and desires and yet never acquaint them with his purposes'.[37] We are assured from scripture that God did not wish to keep these evidences concealed, for as Solomon said, 'The glory of God is to conceal a thing; the glory of the king is to search it out'.[38] This concealment was not intended as a trial for man; rather,

> Even as though the divine nature took pleasure in the innocent and kindly sport of children playing at hide and seek, and vouchsafed of his kindness and goodness to admit the human spirit for his playfellow at that game.[39]

Thus we can be sure that the secrets of God's creation are meant for man to discover; and we can be equally sure that man's dominion over the natural world is a 'divine bequest', as in the passage from Aphorism 129 that I quoted earlier.[40] This is supported by Bacon's references to man's partaking of the Sabbath with God, as in one of his famous prayers:

> Wherefore if we labor in thy works with the sweat of our brows thou wilt make us partakers of thy vision and thy sabbath.[41]

It also appears in the passage from the *Masculine Birth of Time* from which I quoted earlier; there Bacon promises his 'son' that his 'chaste wedlock' with things themselves will produce

> a blessed race of Heroes or Supermen who will overcome the immeasurable helplessness and poverty of the human race, which cause it more destruction than all giants, monsters or tyrants, and will make you peaceful, happy, prosperous and secure.

We might also enquire whether Bacon offered some hint of the character of this promised knowledge; for here the theological conceptions may throw some light on what, if anything, Bacon meant by 'form'. First, we must not forget that Bacon's concern, as much as that of Descartes, was with a universal science including all the arts of human and social behaviour. Second, the knowledge desired was an unmediated contact with 'things themselves'. These are the creations of God; man should establish direct contact with them through contemplation of them; he considered himself as having 'submitted my mind to things';[42] and that 'commerce of the mind of man and things' is 'more precious than anything on earth'.[43] This union with things[44] is not merely an intellectual act, but is the key to the whole sacred endeavour of the material redemption of mankind. Thus in *The Masculine Birth of Time*, Bacon speaks from his 'inmost heart', saying, 'My dear, dear boy, what I propose is to unite you with *things themselves* in a chaste, holy and legal wedlock . . .', whose issue will be the redeeming Heroes or Supermen described above. Although in his writings on method, he promises only to take the human reason up to *prima philosophia* or *sapientia*, achieving the most fundamental and general axioms,[45] his conception of the reform extended further. Thus, in speaking of the ends of enquiry in the *On the Interpretation of Nature*, he dismisses the ignoble and vulgar purposes as elsewhere, and asserts:

> but it is a restitution and reinvesting (in great part) of man to the sovereignty and power (for whensoever he shall be able to call the creatures by their true names he shall again command them) which he had in his first state of creation.[46]

The completion of Bacon's programme for philosophy is then no less than the redemption of mankind, to the extent that is possible, from the consequences of the original Fall.[47]

It is clear that a goal of such cosmic significance could not be achieved merely by the establishment of a scientific research institution. The task of discovering God's works must proceed hand in hand with that of interpreting His will; otherwise it will surely be corrupted. Bacon nowhere says this; and indeed one optimistic passage indicates otherwise:

> Only let the human race recover that right over nature which belongs to it by divine bequest, and let power be given it; the exercise thereof will be governed by sound reason and true religion.[48]

However, we are justified in considering this as propaganda; for even in the Utopian *New Atlantis*, the sages of Salomon's House took no chances:

> And this we do also: we have consultations, which of the inventions and experiences which we have discovered shall be published, and which not; and all take an oath of secrecy, for the concealing of those which we think fit to keep secret: though some of those we do reveal sometime to the state, and some not.[49]

This caution, combined with Bacon's unflattering view of the intellectual and moral condition of the scholars of his own time, makes it certain that his was a task not to be accomplished by administrative means alone.

To carry out his programme, Bacon would need men whose wits were not only sharp, but also cleansed. These would necessarily be set off from the common, corrupted society of the time, either by their being already reformed, or by being ready to reform. How was Bacon to locate and recruit those few who were ready to embark on the great work in a spirit of humility, charity and innocence? On this, the evidence is that Bacon planned to operate on two levels, in a time-honoured fashion. For society at large, there was an exoteric doctrine, cast in the terms that could be generally appreciated, with hints of the deeper message. But there was also to be a brotherhood of 'true sons of science'.[50] Bacon publicly[50] invited membership in this; and so in a sense it was not esoteric. But it was for those few who had been able to reform, and on one important point Bacon's esoteric teaching would have been radically different from his public statements.

This relates to a problem where his assertions seem insincere or self-contradictory: the value, and future role, of the philosophy then dominant. Bacon is at pains to deny hostile intentions towards it:

> For I do not object to the use of this received philosophy, or others like it, for supplying matters for disputations or ornaments for discourse—for the professor's lecture and for the business of life.[51]

Later, he protests the sincerity of his professions of affection towards the 'received sciences', citing his published writings, including the *Advancement of Learning*, as evidence.[52] But he protests a bit too much; for his disclaimers of general utility for his own philosophy turn into an affirmation of its innate superiority:

> It does not lie in the way. It cannot be caught up in passage. It does not flatter the understanding by conformity with preconceived notions. Now will it come down to the apprehension of the vulgar except by its utility and effects.[53]

For a long time I considered this to be an unresolved contradiction in Bacon's own thought, considering that his beloved studies of letters and the law fell within the class of inane works. But a reading of the unpublished *Refutation of Philosophies* gave the clue; there, the sage, 'a man of peaceful and serene air, save that his face had become habituated to an expression of pity', spoke to his 'sons', and advised:

> Therefore keep your old philosophy. Use it when convenient. Keep one to deal with nature, and the other to deal with the populace. Every man of superior understanding in contact with inferiors wears a mask. If I may, as my habit is, speak freely among friends, then I advise you: Possess Lais but do not let her possess you.[54]

The reference is to a famous courtesan; the distinction was made in a reply by the philosopher Aristippus, to critics of his personal behaviour.

Through an appreciation of the essentially *moral* aspect of the reform proposed by Bacon, together with a text such as this one, we can resolve the question of the nature of his esoteric teaching. In his Note B to Ellis's preface to the *Novum Organum*,[55] Spedding reviewed the texts which seemed to call for an esoteric teaching. His conclusion was that Bacon proposed to withhold the publication of his Formula,

> 'not as a secret of too much value to be lightly revealed', but as a subject too abstruse to be handled successfully except by the fit and the few.

This is almost correct; but for 'abstruse' one should substitute 'holy'. Bacon was sure that his method would 'level men's wits', but those wits must *first have been purified*, or (at a later period) protected from the contamination of the false and impious philosophies.

We can now come to the final problem, that of establishing the ripeness of time, so that recruits will come forward in good heart. My interpretation of Bacon's solution to this problem might appear farfetched or paradoxical, were it not for the support of his published texts, and the coherence of the religious framework of his strategy for reform as I have developed it up to this point. Bacon's discussion of this in the *Novum Organum* occupies the section from Aphorism 92 to Aphorism 114, giving the arguments for Hope, with some rambles *en route*. In Aphorism 92 he states that the greatest obstacle to progress has been despair; and by examples of his successes, he may with gentleness prepare men's minds with hope. The introduction to the section on Hope, Aphorism 93, provides a religious and theological foundation. It opens with:

> The beginning is from God: for the business which is in hand, having the character of good so strongly impressed upon it, appears manifestly to proceed from God, who is the author of good, and the Father of Lights.[56]

Elsewhere Bacon makes a strong use of the term 'light' as a synonym for knowledge. The passage continues with a scriptural sanction for his programme for a gentle reform proceeding from small beginnings:

> Now in divine operations even the smallest beginnings lead of a certainty to their end. And as it was said of spiritual things, 'The Kingdom of God cometh not with observation', so is it in all the greater works of Divine Providence; everything glides on smoothly and noiselessly, and the work is fairly going on before men are aware that it has begun.[57]

One might interpret this as an answer to the query by a sceptical reader, whether Bacon had succeeded in putting any of his fine words into practice. (In the previous Aphorism, he promised a set of particulars, admittedly the

strongest means of inspiring hope, for later parts of the Instauration: and for the sake of gentleness, offered only the plan of the work at this stage.

So far the claims for hope in his programme are rather general; but he concludes the aphorism with material that puts the present age in its true setting:

> Nor should the prophecy of Daniel be forgotten, touching the last ages of the world—'Many shall go to and fro, and knowledge shall be increased': clearly intimating that the thorough passage of the world (which now by so many distant voyages seems to be accomplished, or in the course of accomplishment), and the advancement of the sciences, are destined by fate, that is, by Divine Providence, to meet in the same age.[58]

In brief, Bacon believed that by his efforts he was helping to usher in the millennium. One quotation from a large book is but slender evidence for such a dramatic thesis; but supporting evidence is found on the frontispiece of the *Instauratio Magna* as published. For there, under the well-known picture of the ship clearing the twin pillars on its way to the open sea, is the motto, *multi pertransibunt et augebitur scientia*. Any reader familiar with scripture would recognize the text, from the apocalyptic Daniel 12, verse 4 which reads: 'But thou, O Daniel, shut up the words, and seal the book, even to the end of time; many shall run to and fro, and knowledge shall be increased'. Although the text itself has been questioned, and these connotations of Bacon's motto were doubtless lost on later generations (including those who chose the latter part for the motto of the University of Leeds), Bacon's readers would have been well aware of them. And in the context of thought of Bacon's time, a millennial belief supported by scripture is not at all surprising. Any great reformer must have the touch of the saviour about him; and through the seventeenth century, the Holy Writ was an accepted source of clues to the meaning of the revolutions of times.

Comments on Bacon's Strategy

It remains for me to deal with one outstanding problem in this interpretation of Bacon's strategy for reform; and then we can consider how this throws light on other aspects of Bacon's endeavour. The problem is that this interpretation apparently runs counter to Bacon's explicit statements of the separateness of theology and natural philosophy. The most extended account of the damage done by theologians and divines is in Aphorism 89 of the *Novum Organum*, Book I; there Bacon refers to the 'troublesome adversary and hard to deal with: namely, superstition, and the blind and immoderate zeal of religion'.[59] He then cites the story of the Greeks who were found guilty of impiety for giving natural explanations for thunder and for storms, and mentions the

Church Fathers who denied the earth's antipodes. He mentions also the defects of systematic theology, and then the fears of natural philosophy resulting from 'the simpleness of certain divines'. His defence of natural philosophy as the 'most faithful handmaid' of religion rests both on its power to dispel superstition, and on the text 'ye err in that ye know not the Scriptures and the powers of God'. None of this entails a secularization of the spirit of enquiry or of the conception of its ends; it is the sort of defence of natural philosophy against incompetent zealots which was continued in an apologetic tradition for science, at the hands of liberal churchmen, to the end of the nineteenth century.

There is one point where Bacon identifies a particular error in the mixture of the natural and the divine, which might seem to argue against the use of scripture in any investigation of nature:

> In this vanity some of the moderns have with extreme levity indulged so far as to attempt to found a system of natural philosophy on the first chapter of Genesis, on the book of Job, and other parts of the sacred writings: seeking for the dead among the living: which also makes the inhibition and repression of it the more important, because from this unwholesome mixture of things human and divine there arises not only a fantastic philosophy but also an heretical religion. Very meet is it therefore that we be sober-minded and give to faith that only which is faith's.[60]

Fortunately, the mention of Job makes the identification of the culprits easy, at least for those who know something of the history of alchemical philosophy. Bacon's target here is the school of Paracelsus, and the 'heretical religion' would have been some variety of sectarianism, radical both in religion and politics.[61] Hence Bacon would speak with unusual severity about its dangers, and advocate means of control out of keeping with his usual gentleness in political and religious affairs.

Indeed, any opposition between 'natural philosophy' and 'theology' in Bacon's thought is to some extent an artificial construct, since he deeply distrusted systematic theology itself. He considered that there were very strict limits on the powers and rights of the human mind to attempt to penetrate the divine mysteries. In the *De Augmentis* he discusses the proper use of natural theology to 'refute and convince Atheism, but not to establish religion'. For the world is the work of God and not His image. From His works we can demonstrate that He exists and governs: and in His works we can demonstrate the basic properties of God's presence, rule and benificence; and also 'reasonably elicit' other 'wonderful mysteries'.

> But on the other side, out of the contemplation of nature and elements of human knowledge to induce any conclusion of reason or even any strong persuasion concerning the mysteries of faith, yea, or to inspect and sift them too curiously and search out the manner of the mystery, is in my opinion not safe.[62]

Bacon elsewhere gives hints that the corruption of theology has resulted from the same abuses of reason as the corruption of natural philosophy;[63] and also that the path towards true knowledge is the same in both cases: experience and aphorisms rather than argument and systems.[64]

We can safely conclude, then, that Bacon's conception of human knowledge of the divine did not entail a separation in spirit, methods or ends between inquiry into God's will and His works. In fact, if we look a bit more closely into Bacon's personal religion, we find the two endeavours brought into close relation. If we try to classify Bacon in respect to the problems of rational theology, we would probably call him a fideist. But this would be to apply a foreign and inappropriate scheme to Bacon's thought. His religion, like his philosophy of nature, was concerned primarily with practical works, and less with niceties of doctrine.[65] We express his view of the world very simply: man's corruption resulted from vanity; and man's redemption will be achieved by charity. For Bacon this was the message of Christ: he observed that all Christ's miracles were of mercy, not of judgement; each one was designed to help ordinary human beings with their ordinary problems. Similarly, in his meditation on Hypocrites, subtitled 'I will have mercy and not sacrifice', he makes charity to be the touchstone of true religion. The meditation opens with:

> The ostenation of hypocrites is ever confined to the works of the first table of the law, which prescribes our duties to God. The reason is twofold: both because works of this class have a greater pomp of sanctity, and because they interfere less with their desires. The way to convict a hypocrite, therefore is to send him from the works of sacrifice to the works of mercy.

This penetrating observation is followed by one even more so:

> There are some however of a deeper and more inflated hypocrisy, who deceiving themselves, and fancying themselves worthy of a closer conversation with God, neglect the duties of charity towards their neighbour, as inferior matters.[66]

On charity itself, Bacon discussed the various circumstances in which it is ordinarily applied, to an enemy repentant or at least defeated.[67] None of these satisfied him; even the feeling that virtue is proceeding from one may be a form of pride. No, the 'summit and exaltation' of charity comes only

> if evil overtake your enemy from elsewhere, and you in the inmost recesses of your heart are grieved and distressed, and feel no touch of joy, as thinking that the day of your revenge and redress has come.

Such a Christ-like conception of charity, encompassing a complete forgiveness and a complete love, should be kept in mind when we see the term in its frequent occurrence in Bacon's exhortatory passages. Pity for the sufferings of mankind comes out repeatedly in his various prayers, and his picture of a sage

is of 'a peaceful and serene air, save that his face had become habituated to the expression of pity'.[68]

Far from imagining a 'conflict between science and religion', Bacon saw the investigation of nature as a divine work. It not only served to reveal God's works, and to reform the intellect and soul of the enquirer, but also to imitate Christ in 'the relief of man's estate'. With this understanding of the connotations of his words, we may now see how Bacon's vision is encompassed in the final sentence of his prayer:

> Lastly, that knowledge being now discharged of the venom which the serpent infused into it, and which makes the mind of man to swell, we may not be wise above measure and sobriety, but cultivate truth in charity.[69]

With this interpretation of Bacon's conception of his task, we may be better equipped to approach the problem of his 'sources' and of the development of his ideas. It is well known that many of his aphorisms and illustrations are derivative from published sources; and a thorough search of the relevant sets of literature might well reveal Bacon to have been a sort of philosophical magpie, picking up ideas from everywhere and then publishing them, rearranged and slightly polished, as his own. But to condemn him for this would be to misconceive his task, which was not to do original research, but to plead for a cause. Also, the roots of his commitment, and his informal synthesis of ideas, could not be assembled from a set of index-cards. We know that from an early age he was aware of his talents, and was determined to devote them to the service of man and God. It is possible that his earliest endeavours were in a literary–humanistic direction, culling the literature of aphorisms and apothegms, and from them distilling 'axioms' on the nature of man. But this path was rejected, and by the age of thirty he had committed himself to finding the key in the study of nature. We simply do not know what person, or book, wrought this conversion. John Dee's departure from England probably came too early; and although Bruno was on hand during the crucial period,[70] he receives no mention whatever in Bacon's reflective writings, and his attack on institutional Christianity would be altogether too radical for Bacon's taste. Palissy the potter would probably have been only a self-educated workman for Bacon, not someone to provide him with his own particular version of the seventeenth century commitment to approaching God through Nature. The most likely source of Bacon's conversion seems to be some current of pietistic Paracelsian philosophy; there one would find the mixture of the themes of Christian charity and a manual interaction with the things of Nature. The sentiments of scepticism of official learning, pity for the sufferings of mankind, and dedication to a pure and holy reform that van Helmont shows in his autobiography[71] are strikingly similar to those of Bacon. This is not to assert any link of influence between the two; but to indicate that their common form of commitment may well have derived from a common source.[72] Bacon's vehement condemnation of Paracelsian scriptural natural philosophy is not

conclusive evidence against such an early influence; it is commonplace in politics for a man to derive his permanent ideals from a radical source, and eventually come to see the ongoing tradition of that source as the worst enemy of his matured programme.

Finally, we may consider whether this reinterpretation of Bacon has any significance for our understanding of the development of science from his time up to the present. Accepting the theological framework of his strategy for reform, we can add yet another criticism of his work to the already considerable stock. In one respect he was wrong: not merely was the millennium not at hand in the early seventeenth century, but the growth of scientific knowledge and of the power of its applications neither required nor produced such a moral reform in society as he had considered essential. Even though ordinary life for ordinary people is more peaceful and humane by far in the advanced societies than it was in Bacon's time, the twentieth century wars of ideology and empire have produced barbarities that match anything achieved during the wars of religion of the times before and after Bacon's career. Also, between Bacon's time and our own, his concern for the reform of the sciences could recede into past history as a curiosity of bygone times. Over the generations, natural science achieved appropriate methods of inquiry and viable social institutions for its work, so that it could progress to ever greater triumphs.

But very recently, it has been impressed on us all that science lives not by PhDs alone. The political problems of the management of a large and complex scientific community, internally and in relation to its sources of financial support and recruitment, become ever more demanding; and the moral problems of responsibility for abhorrent applications of scientific results are likewise intensifying. This is not to say that the times are again ripe for a prophet with Bacon's particular message, or indeed for any prophet at all. But the moral commitment, and pity for mankind, that drove Bacon to make his contribution towards the advancement of learning can no longer be dismissed as irrelevant or peripheral to the real business of science. Even if his scientific achievements are negligible, his elaborated methodology a bore, and his theological framework obsolete, yet in his aphorisms he may still speak to us.

This essay was first published in *Science, Medicine and Society in the Renaissance (Walter Pagel Festschrift)* (ed. Allen G. Debus), New York, Science History Publications, a division of Neale Watson Academic Publications Inc., 1972, pp. 97–119. A first draft was read to seminars at Leeds and at Cambridge in 1966. Its ideas have been developed in the course of teaching the *Novum Organum* at the Rijksuniversiteit te Utrecht. I am particularly grateful to Mr van Drunen, then a student at Utrecht, and to Ir. H. Peters, of Boxtel, for their discussions of this aspect of Bacon and the materials which they made available to me.

References

1 For a vigorous counter-attack on this tendency, see Hesse, Mary B. 'Hermeticism and historiography: an apology for the internal history of science', *Minnesota Studies in the Philosophy of Science,* V (1971).
2 See Wiener, Norbert, *God & Golem, Inc.*, MIT Press, 1964, 50–1.
3 For a generally sceptical interpretation of the evidence for Descartes' involvement with the Rosicrucians, see Gouhier, H., *Les premières pensées de Descartes, contribution à l'histoire de l'anti-Renaissance,* Paris, 1958.
4 See 'Mr. Bacon in Praise of Knowledge', Bacon, *Works,* ed. Ellis, Spedding and Heath, London, VIII, 124.
5 Bacon, *Novum Organum* (1620), Book 1, Aphorism 95, in *Works,* IV, 92–3.
6 *Works,* IV, 79–80.
7 *Novum Organum,* Bk I, Aph. 74, in *Works,* IV, 74–5.
8 *Ibid.*, Aph. 110, in *Works,* IV, 99–100.
9 *Works,* III, 156–66.
10 It is a commentary on the historiography of science that the immediate ancestry of Bacon's 'Three Inventions' was discovered by Joseph Needham, in the course of tracing them back to China. A possible direct source for Bacon was le Roy, Louis. *Of the Interchangeable Course or Variety of Things in the Whole World,* English translation, 1594. I am indebted to Dr P. Rattansi for the information on le Roy and on Needham's work.
11 In his earlier writings, Bacon had no doubt that the discoveries were chance. Thus in the *Temporis Partus Masculus* he mentioned gunpowder as an example of a 'lucky hit': 'If gunpowder had been discovered, not by good luck but by good guidance, it would not have stood alone but been accompanied by a host of noble inventions of a kindred sort.' (Translation from Farrington, B., *The Philosophy of Francis Bacon,* Liverpool University Press, 1964, 71). By the time of the writing of the *Novum Organum,* he was more cautious. In arguing for the possibility of new discoveries, he admitted that most of the standard examples 'may seem to depend on certain properties of things themselves and nature, there is at any rate nothing in the art of printing which is not plain and obvious', *Novum Organum,* Bk 1, Aph. 110, in *Works,* IV, 100. His description of the process showed no acquaintance with the device of cast interchangeable type which distinguishes Western printing after Gutenberg from earlier methods of stamping an image in ink.
12 *Ibid,* Aph. 81, in *Works,* IV, 79.
13 *Ibid,* Aph. 129, in *Works,* IV, 144.
14 See 'Plan of the Work', *Works,* IV, 26–7, for both points together.
15 *Novum Organum,* Bk I, Aph. 68, in *Works,* IV, 69.
16 For an English translation, see Farrington, *op. cit.*, 59–72.
17 *Ibid.*, 70.
18 *Works,* IV, 33.
19 Tr. *Ibid.*, VII, 243.
20 *Works,* VII, 222.
21 *Ibid.*, V, 132
22 'The Great Instauration, Preface', in *Works,* IV, 19.
23 *Ibid.*, 20.
24 *Ibid.*, 21.
25 *Ibid.*, 20–1.
26 'Confession of Faith', Works, VII, 222.
27 *Idem.*
28 *Ibid.*, 221.
29 'Nat. & Exp. Hist'. *Works,* V, 132.

30 Ibid, 133.
31 For a full discussion, see Rossi, P., *Francis Bacon, From Magic to Science,* London, 1968, Ch. 3, 73–134.
32 See the *Meditationes Sacrae,* tr. in *Works,* VII, 252; and *Novum Organum,* Bk I, Aph. 89; *Works,* IV, 89.
33 This figure is used many times; see 'Plan of the Work', Works, IV, 33; and 'Nat. & Exp. Hist.', *Works,* V, 132.
34 *Novum Organum,* Bk I, Aph. 23; *Works,* IV, 51; and Aph. 124, *Works,* IV, 110.
35 'Mag. Inst. Plan', *Works,* IV, 33.
36 *De Aug.*, Bk III, 5; *Works,* IV, 365.
37 *Ibid.*, 364.
38 *Novum Organum,* 129; *Works,* IV, 114.
39 'Mag. Inst. Pref.', *Works,* IV, 20.
40 *Works,* IV, 115.
41 'Mag. Inst. Plan', *Works,* IV, 33.
42 *Novum Organum,* 129; *Works,* IV, 115.
43 *Ibid.*, Aph. 113; *Works, IV,* 102.
44 The importance of this contact with 'Things' in Bacon's philosophy has perhaps been obscured by the translators' practice of using a variety of terms for '*res*', including 'fact', 'particulars' or 'nature'. Indeed, one may say that most of the times such terms appear in an English translation, the original is '*res*'. It could be argued that the Latin term had extended connotations for Bacon, as it does in legal usage; but in every instance I have tested, the translation of '*res*' as 'thing' improves the sense.
45 *De Aug.*, Bk III, 1; *Works,* IV, 337.
46 *Works,* III, 222. See also *Novum Organum, Preface; Works,* IV, 20.
47 This is hinted at in the Proem of the Great Instauration; I translate the third phrase as: 'he (Bacon) thought that all trial should be made, whether that commerce between the Mind and Things (which is the most precious of anything on earth, or at least of earthly things), might be restored to its integrity, or at least brought to improvement'. See *Works,* I, 121 and IV, 7, for original and translation.
48 *Novum Organum,* Bk 1, Aph. 129; Works, IV, 115.
49 *Works,* III, 165.
50 *Novum Organum, Pref.; Works,* IV, 42.
51 *Loc. cit.*
52 *Novum Organum,* Bk 1, Aph. 128; *Works,* IV, 113.
53 *Novum Organum, Pref.; Works,* IV, 42.
54 Farrington, *op. cit.*, 108.
55 *Works,* 107–13.
56 *Works,* IV, 91.
57 *Ibid.*, 91–2.
58 *Ibid.*, 93.
59 Ibid., 87–9.
60 *Novum Organum,* Bk 1, Aph. 65; *Works,* IV, 66.
61 I am indebted to Ir. H. Peters for this important identification.
62 *De Aug.*, Bk III, 2; *Works,* IV, 341–2.
63 *Novum Organum,* Bk I, Aph. 89; *Works,* IV, 88 and *De Aug.*, Bk IX, 1; *Works,* V, 111–19.
64 'Advancement of learning', *Works,* III, 487–8; and *De Aug.*, Bk IX, 1; *Works,* V, 118.
65 Farrington relates this emphasis to an English tradition, extending back through More and Colet. See Farrington, *op. cit.*, 17.
66 *Meditationes Sacrae, Works,* VII, 249.

67 *Ibid.*, 245–6.
68 *The Refutation of Philosophies*, Farrington, op. cit., 104.
69 *Novum Organum, Pref.; Works*, IV, 20.
70 Farrington, *op. cit.*, 27.
71 See the opening pages of the *Oriatrike, or Physick Refined* (1662).
72 One of the few 'moderns' for whom Bacon shows any respect is Petrus Severinus, a moderate Paracelsian of Denmark.

Criticisms of Science: From Past to Present

An Example from Classical Civilization

Although we shall soon see that the 'science' that was the object of criticism has been a complex and varied entity, the oldest example of detailed criticism known to me has a surprisingly modern tone. This is the comedy of Aristophanes, *The Clouds*, of around 420 BC. I shall discuss it at some length because of its usefulness in illustrating many of our concerns. A brief quotation will indicate the general style:

Student: What would you say then if you heard another,
Our master's own?
Strepsiades: Oh, come, do tell me that.
Student: Why, Chaerophon was asking him in turn,
Which theory did he sanction; that the gnats
Hummed through their mouth, or backwards, through their tails?
Strepsiades: Aye, and what said your master of the gnat?
Student: He answered thus: the entrail of the gnat
Is small: and through the narrow pipe the wind
Rushes with violence straight towards the tail;
There, close against the pipe, the hollow rump
Receives the wind, and whistles to the blast.
Strepsiades: So then the rump is trumpet to the gnats!
O happy, happy in your entrail learning!
Full surely need he fear nor debts nor duns
Who knows about the entrails of the gnats.

At one level we can see in the character of Strepsiades a precursor of those modern legislators who occasionally regale their colleagues with lists of ridiculous titles of research projects on which the taxpayer's money is being spent. Certainly the problem of justifying research whose only goal is 'positive' factual knowledge is one that defies easy solution. It is clear from the dialogue as well

as from the context that mere 'positive' knowledge of fleas is only ancillary to other goals. Strepsiades is a rustic who has come to Athens to learn how to argue with the 'wrong logic' in the law courts, and thereby escape the debts that his wastrel son has incurred. He has been directed to this school of 'Socrates' as a likely source of instruction. He is willing to put up with all these irrelevant facts as the price he must pay for mastering the techniques that will enable him to solve his practical problems. For the students and 'Socrates', however, such facts are serious business; they lead to the philosophical study of nature, and the achievement of wisdom. The discipline illustrated in the main body of the play is meteorology; the Gods are not abolished but the phenomena of thunder and rain are 'explained' by coarse jokes about digestive functions.

Thus Aristophanes blended the voices of the Sophists, who hired themselves out to teach debating skills, with those of the 'physiologues', who produced 'disenchanted' explanations of natural phenomena; then he named their representative 'Socrates'. It is likely that this was both inaccurate and unfair, but then Aristophanes was a writer of critical comedies, and in this one the moral is plain. The end comes when the son of Strepsiades displays a superior mastery of the 'wrong logic', to the extent that he is justified in beating up his father; the latter then burns down the school with its inhabitants. The chorus approves, as 'Socrates' and his group have blasphemed the gods. In this little drama the 'positive' facts derive all their significance from their ideological function; and this is seen as clearly by the 'scientists' as by their enemies. Only later do we find spokesmen for science claiming that embattled scientists (such as Galileo) should have both the privileges of an ideological combatant on the right side, and also the immunities of an encapsulated scholar.

The circumstances producing this early criticism of 'science' are worth mentioning. Athens was embroiled in the serious Peloponnesian War, having been led by Pericles through a cycle of patriotism, interstate co-operation and ultimately imperialism. The essence of the free Athenian polity, immortalized in Pericles' late oration, might well have been corrupted and destroyed before anyone noticed it was there. At the court of Pericles were 'freethinkers', including the philosopher Anaxagoras, who was eventually tried for impiety. Certainly there was plausibility in Aristophanes' implicit accusation that the 'demythologizing' of nature and of the city had led to a corruption of the people. The relation of the historical Socrates to all this is beyond my present concerns (Ferguson 1971).

We gain some idea of the rapidity of the change of cosmology in fifth century Athens when we consider the tragedy *Prometheus Bound*, by Aeschylus, only a half-century earlier. There Prometheus lists all his gifts to mankind: they are all techniques, with no 'pure' or philosophical science to be seen. What is startling to a modern reader, and perhaps embarrassing to some, is that the really advanced 'sciences' were those of prognostication by magical means. From our modern viewpoint we can perhaps imagine the intellectual and spiritual disturbance which the denigration and destruction of the old

cosmology must have caused more easily than could our predecessors in scholarship. In his classic work on Aeschylus, George Thomson (1941) omits the major part of Prometheus' speech which deals with magic; then he comments on 'the bold naturalism of the account'.

There is also a criticism of 'science' built into the Prometheus legend: the gods are jealous of man's powers. Whether this is simply one of the less enlightened responses of the Greek deities, or whether this reflects a deep fear of uncontrolled knowledge, like the Garden of Eden story, is matter for speculation. Of the prehistory of this sort of 'science criticism' I can offer only one fragment: for the early Hebrews, iron was an unclean substance and not to be brought into a holy place, because of its associations. Thus, a loathing of the evil effects of natural knowledge applied to technical problems can be traced back very far indeed.

Criticism in the Scientific Revolution

Although 'science' is an essentially complicated and confused term, I am here concerned with the cluster of activities and styles that are dominant at the present time. For brevity I can omit descriptions of debates over occult arts and scholasticism from the Medieval and Renaissance periods. However, the Scientific Revolution of the seventeenth century is so directly ancestral to our own situation, that a review of debates then can be helpful for perspective on ourselves.

The prophets of the scientific revolution had a commitment to a positive dream for a reformed natural knowledge; but not surprisingly, they were more articulate in their criticisms of the existing science and learning. From the criticisms which each of them made we may gain some insights into what he considered to be central to his own programme. Bacon's critique was the most broad-ranging, and also the most related to practice. He considered all the different sorts of men claiming to advance knowledge, and found them all wanting in their methods, attitudes and ethics. Although the professed men of knowledge were guilty of just about every one of the seven deadly sins, it was, in my opinion, pride that Bacon found most monstrous. There is little doubt that he saw himself as the inaugurator of a brotherhood of pure reformers of knowledge and then of mankind; the millenarian connotations of *Et Augebitur Scientia* would not have been lost on his readers (Ravetz 1971). Descartes probably also entertained some ambitions of a messianic character, and his interest in the shadow brotherhood of Rosicrucians is difficult to deny (Arnold 1958). But his lasting impulse to reform came from his experiences as an adolescent schoolboy. Having believed that books could reveal the Good and True, he truned in his disillusion on the entire syllabus, reserving special scorn for philosophy and theology, and allowing only mathematics a partial exception (Descartes 1638). By contrast to these two, Galileo seems to have been concerned mainly with natural philosophy, and his brief, disconnected,

critical analyses were made mainly in the context of polemical debate.

It is now generally recognized that there were three, not two, sides in the philosophical struggles of the early seventeenth century; against the 'mechanists' were arrayed Aristotelians with an 'organic' world view, and 'alchemists' with a 'magic' world view. Of course, there were factions and conflicts within each camp, and each exerted an influence on the other two (Kearney 1971). The debate between the two 'losing' sides is not well chronicled; van Helmont's sufferings at the hands of the Inquisition are nearly forgotten (Bonelli and Shea 1975). Nor do we have editions of counter-criticisms by Aristotelians (of the various sorts) to the 'atomists'. Fortunately, we do possess published documents from a great debate on the other front: between Paracelsians and Galileans, roughly speaking, in Civil War England. The occasion was an attempt by more radical educational reformers to include universities in their plans. The attack was led by John Webster, and in a war of pamphlets the defence was made by Seth Ward and John Wilkins. Webster denounced the universities for their supposed conservatism; in their reply the academics cast reflections on his competence, and denounced his alchemical recommendations as antithetical to all Bacon's precepts. But they had to admit that Bacon did advocate experiments as a way to learning, and that they refrained from forcing such things on their students, since the universities were still essentially élite finishing schools rather than centres for advancement of learning or diffusion of useful arts (Webster 1976).

With the decay into insignificance of its two rivals—outside Germany, at least—the school of the 'mechanical' philosophy of nature came to complete dominance by the end of the seventeenth century. But it had lost the prophetic zeal of its earliest proponents, and indeed in England, at least, experimental philosophy came to be regarded as a gentleman's eccentricity, at a time when manners and morals were the prime concern of the cultivated classes. In spite of—or perhaps because of—the near deification of Newton in early eighteenth century England, there was no corps of really talented men to continue his work. A savage satire of scientific academicians and industrial innovators was at the heart of the story of Gulliver's voyage to Laputa as told by Jonathan Swift. An equally pungent sociological analysis of scientific dogmatism was made by the philosopher George Berkeley in the course of his debate over the concealed obscurities at the foundation of the calculus (Ravetz 1971, pp. 219–23). But systematic and socially effective criticism of the dominant styles of science was by this time becoming exceedingly difficult, as 'reason' became the touchstone for all decisions in polite European society.

The Romantic Challenge and Its Descendants

It is useful to remember how deeply split was the Enlightenment movement on the question of science. All factions agreed on the necessity of doing away with the corrupt tyranny of the Church. But not all shared the faith of d'Alembert

and Condorcet that a Newtonian type of science, both natural and social, would itself bring reason and justice to human affairs. In particular, Rousseau and his followers, combining Arcadian, romantic and populist elements, raised a significant challenge to orthodox science during the revolution, appearing as forerunners of the cultural revolutionaries of recent times (Gillispie 1959).

The flourishing of romantic poetry in England also had its scientific aspects. Blake's contempt for atomism and 'single vision' is well known to today's counter-culture, and Coleridge's enthusiastic study of *Naturphilosophie* (Knight 1972) was more plausible than we now realize, given the exciting and chaotic state of chemistry and biology at the time. But the movement was short-lived; and the English combination of utility and inductivism kept their science rather more practical and less speculative and sensitive than in Germany.

Germany was, of course, the home and source of romanticism, particularly in science. Swedenborg, the engineer-turned-psychic, provided elements of continuity with earlier enchanted philosophies of nature. To the acute embarrassment of German men of science for a century afterwards, the great poet Goethe considered his work on optics as important as any other he did. Romanticism, in *Naturphilosophie*, had an ontological basis of opposition to the hard experimental science that was to replace it, a commitment to some sort of existence 'beyond reductionism'. Future historical studies may find a surprising number of people of such tendencies among known critical or eccentric scientists. Thus G.T. Fechner, the founder of psychophysics, was led to his classic empirical studies by the need to corroborate his panpsychical philosophy, as exemplified in 'Nanna, or the soul life of plants' (Jaynes 1972). Also we now find that A.R. Wallace broke with the Darwinian theory of the descent of man from apes because of the intensity of his spiritualist experiences (Kottler 1974). A direct link to the present-day counter-culture is provided by Rudolf Steiner, who combined Goethe with theosophy; and in spite of the apparent isolation of his established followers in 'anthroposophy', he indirectly provided inspiration and insights for the romantics of today.

A link to another contemporary focus of criticism can be found in Max Scheler, the brilliant though eccentric German philosopher of the earlier twentieth century. He did not merely mourn the passing of the 'organic' *Gemeinschaft* world in the well-known German style; he also examined modern science as the characteristic production of a peculiar, alienated consciousness (Staude 1967). Much of the later 'cultural' historical materialism—as, for example, the neglected essay by Christopher Caudwell, *Crisis in Physics* (1939)—seems to contain echoes of Scheler's analysis, though of course without his particular judgement on the phenomenon.

The ontological criticism of modern materialistic scientism has flourished throughout the century, though until quite recently, at least, kept on the sidelines. The biological sciences have produced descendants of vitalism in 'holism'; this developed into the concept of 'levels of organization', as

characterized in Whitehead's 'organismic' philosophy, with continuous extensions to mysticism in Bergson—gloriously misunderstood by Bertrand Russell in his essay *Mysticism and Logic*—and in Teilhard de Chardin. From physics came a more 'spiritualist' tendency, most notably seen in Crookes and Oliver Lodge; later Eddington, Jeans and Milne continued the criticism of materialistic science from a more Platonic point of view. E.A. Burtt was the first to analyse seventeenth century science as the product of a metaphysical shift; his classic book (1924) opens with a contrast between Dante's hymn to the Divine Light and the schoolboy Epicurean heroics of Bertrand Russell's 'A free man's worship'. It was to be nearly half a century before professional historians of science could become sufficiently critical of science to appreciate this work.

Modern Radical Criticisms of Science

Although social criticisms of science on behalf of the non-élite classes were made in earlier centuries, they have gained coherence only recently. The course of Marxist criticism of science may indicate some reasons for this delay. To a large extent, Marxists have wanted only to inherit and purify bourgeois culture, rather than to transform it.

Lenin's vigorous book on philosophy of science (1909) accepted the facts and values of science as unproblematic; indeed, his version of materialism involved commitment to an impersonal, external world and cohered well with a scientism just a shade above the vulgar. It appears that the German Marxists of the 1920s, the first generation of Marxists who really enjoyed a collectivity of educated and academically employed scholars, were rather involved in debates with Kant, Weber and Freud. Marcuse and Mannheim reflected these concerns. Attacks on 'rational' service itself were then the property of a mystical, pre-Nazi Right (Forman 1971). Hence it was only in the 1930s that Marxist criticisms of science, mainly in England, emerged with intellectual force. This important movement has been studied by Werskey (1975) from several approaches, generally more social than doctrinal. There seems to be a common theme in all the criticisms, namely that science could produce peace and plenty for all—as well as culturally valuable knowledge from pure research—were it not for the 'fetters' imposed by a corrupt and destructive capitalist system. The most eloquent statement of this faith was in J.D. Bernal's *The Social Function of Science* (1939). The mixture of humanitarian, technocratic and reductionist–Faustian motives in Bernal's thought has not yet been fully explored, although B. Easlea (1973) has given some preliminary hints. The use of the Soviet Union as a shining example of the future became increasingly difficult as Stalin's regime became more oppressive; but it was only after the war that a major scientific scandal, the Lysenko episode, really upset the Marxist scientific community and provoked defections on a scientific issue.

In spite of a now lengthy experience of criticism in the West and practice in the East, the Marxist socialist criticism of science has not yet succeeded in articulating a coherent positive alternative. Although the slogan of 'socialist science' has been raised several times in the Soviet Union, it has usually been so entangled with crude and opportunistic campaigns within science as to gain no genuine credibility. The disillusion of J.D. Bernal with Soviet science, though only to be inferred from hints in his writings (Ravetz 1972), is likely to have been as severe as that of Max Born (Thompson 1953) or Kapitsa (Roszak 1969, 1972) with non-political 'industrialized' science. Indeed, the very possibility of a distinctively 'socialist' science now seems to be an open question for those who have the greatest personal commitment to the idea. Thus the avowedly radical British Society for Social Responsibility in Science once organized a conference to inquire: 'Is there a socialist science?' The outcome was far from conclusive.

A different dimension of radical criticism of modern science and technology can be traced back to the ethical and aesthetic writings of Victorians such as John Ruskin and William Morris. Though their ideas were neither stable nor always internally cohesive, they gave a reminder that the industrial system does more than exploit, it blights. Neither aspect is purely derivative of the other. A literary expression of this view was developed by D.H. Lawrence; the critical school of F.R. Leavis developed it further, and this at least as much as Marxism served as a basis for the political radicalization of a generation of English intellectuals (Thompson 1953). On the more overtly political side, the Ruskin influence—mixed with communitarian ideals and the studies of Kropotkin in social philosophy—has worked through many channels including that of Gandhi to the 'intermediate technology' of Schumacher (1973). Based on 'Buddhist economics', it invokes the increasingly powerful slogan, 'Small is Beautiful'.

Although the 'ethical' approach does not involve an enriched cosmology or enhanced experience for its criticism of science, its themes are shared by many approaches which do. The novelist Aldous Huxley emerged as an important critic of science with his *Brave New World*, which described a science-based utopia where happiness was enforced, and civilization was trivialized and debased. In the 1930s he became a pacifist communitarian consciousness-enhancing prophet; and by the 1950s his experiences with Eastern religion had prepared him for psychedelics. He then became a link to the Leary group, and thereby helped form the synthesis of the 'Politics of Ecstasy' which was so important for a brief period in the 1960s. This movement, identified and named by Theodore Roszak as 'the counter-culture' (1969), made cosmology and inner experience the 'base' in reality while the political–technical complex, the superstructure, was rotting away in our own time. A more anti-Marxist radicalism would be hard to imagine. Perhaps it was inevitable that the attempt to unite Marxism with a variety of romanticisms dating from the Freudian Revolution of the 1920s, inspired by Marcuse's *One Dimensional Man* (1964), should have had such a brief, though intense, career.

This review of radical critiques of science from the outside would be incomplete without one embarrassing example. Liberal intellectuals tend to

assume that all radical popular criticisms of science, as of other élite social institutions, must be from the Left. Philosophically reactionary populism is, if not a contradiction in terms, then at least an anomaly to be explained away. Yet the strength and persistence of the biblical fundamentalist attack on the teaching of Darwinian evolution in the United States should be a warning against scientific complacency (Grabiner and Miller 1974). Those responsible for this movement feel themselves excluded from a fair share of intellectual influence, just as do Marxist radicals. The more sophisticated defenders of biblical 'literalism' can argue in a very Lakatosian way about administrative suppression of their research program when it only *seemed* to be undergoing a degenerate phase; certainly neither side of the debate can rigorously prove the truth of its assumptions. Heavy-handed political tactics by scientist-politicians against 'creationist' propaganda have proved counter-productive, just as in the Velikovsky affair (which, although more bizarre in details, has not involved an overt challenge on the principles of scientific evidence). The issues of *Pensée*, published for a time by students in Portland, Oregon, showed a reasonable debate on scientific problems between Velikovsky and his critics (see also Gillette 1974). Even if the 'reactionary' criticisms of science are of a character with occasional chauvinistic denials by oppressed ethnic and cultural minorities of the originality or value of Western science, these criticisms should serve as a reminder that the dominant style in science, however great its intellectual power and social benefits, can yet be a tool of cultural oppression in many directions.

Science Policy Studies: From Publicity to Politics

A noteworthy feature of the present period is that sharply critical analyses of the scientific endeavour are made by established scholars, whose general radicalism may be mild or even non-existent. This respectable, 'inside' criticism of science reflects the new self-awareness of science, and the loss of its earlier assurance. Looking back on pronouncements of earlier spokesmen of science, we are impressed by their propagandist character. The 'man of science' was, for such as T.H. Huxley (1893), von Helmholtz (1893) or Karl Pearson (1892), a paragon of the best intellectual and moral virtues.

The earliest writings in a self-conscious tradition of sociology of science presupposed and reinforced this assumption. Thus Robert K. Merton, whose first work was historical studies of possible social influences on science, produced (1942) a theoretical eulogy for the idealistic scientist who shares an 'ethos' involving 'four norms' of behaviour. Such an image was consensus right through the early post-war period; such diverse figures as Polanyi, Popper, Bernal and Vannevar Bush could all agree that what science mainly needed was more latitude for doing its own thing. In itself, science was so innately good that there were no inherent problems of government that might endanger its progress. From earlier centuries through the post-war decade,

science policy studies were in essence little more thant science publicity pronouncements. The only critical voices from within established science were those of eccentrics like Leo Szilard (1961) and Norbert Wiener (1964).

Science policy analysis as such can therefore be said to begin with two studies whose limited, descriptive appearance belies their fundamental significance. Derek Price's fantastic straight-line, semi-log graph of growth (1963) did more than show a continuity with the past: it tolled a bell for the future. Those commentators who predicted that eventually we would need to give PhDs to dogs and cats were only partly ridiculous. Price showed that a situation of unequal growth-rates in scientific demand and societal supply could persist unchanged for only a limited length of time. If the supply of resources could not increase beyond all expectation, the demands of science would need to moderate. In his rigorous, boldly quantitative way, Price exposed one of the fundamental contradictions of 'big' science: that quantitative growth, previously so necessary for vitality, must stop soon. Indeed, one could define 'big' science as that whose claim on resources is so large as to be politically significant, and which is thereby constrained by general social priorities.

A neat contrast to Price's study was that of Alvin Weinberg on 'Criteria of scientific choice' (1967). Instead of impersonal statistics on quantitative growth, we have wise reflections on qualitative choice. For the affluent, post-war, big-science Americans, it was bad enough to remind the world that choices—and rejections—are necessary. But by challenging the absolute value of 'pure science', by including social concerns as legitimate components of any decision on investment in science, he seemed to be betraying the autonomy of the scientific endeavour. Emotive pleading aside, Weinberg's study did prepare for the exposure of an even more basic contradiction in 'big' science: the total confusion of the disparate goals of scientific research and of the appropriate social roles of scientists. We shall return to this later.

My own contribution to science policy studies began shortly after; in late 1964 my article on the Mohole scandal appeared (Ravetz 1964). In it I tried to define corruption in science, importing the norms of social behaviour appropriate to politics, business enterprise, or speculative technology. This led to the floating of a publicity stunt that quickly became a gigantic project, with inadequate study of goals, feasibility or costs. Around the same time I began to work up the ideas embodied in my book of 1971; and a problem posed by Derek Price's study became crucial at that early stage. What, after all, is the difference between 'little science' and 'big science' as social activities? All the indicators are continuous; how—in terms of Marxist dialectic—does quantity pass into quality? I recall that the question, phrased without the Marxist terminology, of course, was put by Jack Morrell. The answer to this question was suggested by another part of Marxism: a change in the capital-intensity of scientific research. The old craftsmen-producers who offered their finished products on a market of quality assessment are replaced by managers who must convince an investing agency to provide heavy capital for a future project. Much else follows from this, and is made coherent by it; thus I came to

the idea of the 'industrialization of science', and passed on from Marxism to other sources of insight.

Around the same time, Jean-Jacques Salomon saw the present period as one of techno-positivism, of the *savant* or aristocratic scholar being replaced by the *scientifique* or scientific worker (1973). In this insight he had been anticipated in 1960 by John Ziman, who had used the English cricket class distinction between amateur 'gentlemen' and paid 'players'; that is to say, between a 'vocation' and a 'career'. In his book, Salomon went on to identify the most cruel contradiction of all: that the noble ideals of the traditional scientific endeavour had rested on an illusion of innocence. Science, for so long the destroyer of ideologies, was revealed as a variety of false consciousness. How to preserve the ethics of a *savant* in the new disenchanted age is a problem Salomon left to the reader.

For the first really consistent, many-sided, Marxist analysis and criticism of science in this present period, we are indebted to Hilary and Steven Rose. After exploring the historical background in their book (1969), they moved to the attack in 1970 with their essay on the myth of the neutrality of science (Fuller 1971). With a host of suggestive examples, they showed how both scientific *choice* and scientific *concepts* are ideologically and politically influenced in various degrees. The political lessons of this may seem straightforward, but the Roses saw some tricky ethical problems, such as: Was Einstein guilty of the bomb? To save something of the functions of the discredited theory of the neutrality of science, they distinguished between a Kuhnian paradigm, subject to ethical judgements, and puzzle-solving within it, the last preserve of ethical neutrality. Whether this analysis would hold indefinitely may be doubted. Subsequently they have extended their analysis to describe the 'incorporation' of science in the bourgeois (and also the Soviet) state, wherein science functions as a means both of material production and of social control, while itself experiencing the social and political stratification and alienation of any techno-bureaucratic enterprise. The 'myth' of 'pure' science has been maintained only by a concentration of attention on the exploits of the academic élite; but this is now weakening, and the various titles implying some separation or opposition between 'science' and 'society' are themselves mystifying and obsolete. Although their analysis provokes queries and criticisms at many points, the Roses have had considerable success in their sustained endeavour to achieve a Marxist critique of modern science, all the time preserving the standards of civilized debate. (See Rose and Rose 1976.)

The formal sociology of science gradually emerged from the influence of Mertonism. Stuart Blume (1974) studied power in the scientific establishment, and found that it is but imperfectly correlated with scientific attainment. Rather, power—realized through the equivalent of patronage, or the allocation of research funds—derives to a strong degree from contacts in the bureaucracies that feed science. This arrangement is, of course, self-reinforcing. There was at least one large-scale empirical study, on some 400 scientists, to see whether they subscribed to, or had even heard of, Merton's 'four norms'

(Ellis 1972); the answer was disconcerting then but hardly needs stating now. An investigation of the scientists working on the first moon-rocks (Mitroff 1974) showed a fine mixture of traditional idealism with tough American realism; as one scientist said, if a result isn't worth being stolen it isn't worth producing.

By the mid-1970s the stream of 'critical policy' studies became a flood. We now face the problem of having too many approaches to the phenomenon and no inclusive theory for them all. Complementary to my theory of 'industrialization' is Harvey Wheeler's description of the 'bureaucratization' of science (1975). Where I saw the corruptions of 'entrepreneurial' science, he sees the onset of mediocrity and trivialization of research, paralleling the tendencies of high-technology consumer industries like pharmaceuticals and automobiles. Though this too had been anticipated by Leo Szilard long ago, it was then only an inspired guess (1961). Wheeler relates the present process to the current deceleration of growth, and anticipates a rapid ossification of the scientific enterprise. One solution, though more directed at the 'Andromeda Strain' problems of DNA research, is external political control through the 'constitutionalization of science'.

We can hardly expect the proposed critical alternatives to established science to be more coherent than the parallel movements in politics. But it is significant that in the realm of ideas, science lacks strong and confident defenses against these attacks. An idea which Dean Harvey Brooks proposed in 1975 indicates the state of ideology of established science; he suggested that American science could be rejuvenated by our making it the focus of another great national endeavour, providing a unifying purpose for the nation! Though this might well come to pass some time in the future, it is, in the wake of the moon-race and Vietnam, a forlorn hope. More symptomatic of the current atmosphere is a volume of essays produced at the University of Michigan in connection with the quincentenary of the birth of Copernicus. The title is *Science and Society: Past, Present and Future* (Steneck 1975). In it the social and human relevance of science is exemplified by a quote from the mother of an American black child; a visiting scientist met her on an educational aid project. She said:

> 'But you never show your white faces around here. You never say "I'm sorry. I'm sorry for what's happening. I'm sorry that we got our white folks walking on the moon while you black folks are falling on your beds sick with hunger and your stomachs rotting. I'm sorry that your boy is an epileptic . . ." Everything in the name of science. But any way you cut it, you're the master, we're the dogs, and I just got to wait and see whether a seizure some day will take my boy away from me . . .' (p. 227).

Doubts among the Scholars

The ideological motivations and functions of the various scholarly disciplines that study the natural sciences have had a shadowy history. On the one hand, the glorification or defence of science in general, or loyal praise for the founding fathers of some particular specialty, were quite legitimate concerns of the (usually amateur) philosophers and historians of science until recently. But since they nearly all shared in the defining assumption of our science, that it is simple truth, having no connection with ideology, they could not rise to self-awareness about their own efforts. Only now, given the combination of professional self-consciousness among some scholars, with the current mood of disenchantment with science, can a truly critical analysis of the past and of the present be achieved. This means that the way is now open for a genuine history and sociology of science; for until such reflective disciplines have some critical distance from their object, they cannot produce anything more than anecdote, chronicle or hagiography.

I deal with contemporary critical currents in other essays in this volume, and so there is no need for great detail here. Let it suffice that until the present century, the problem of error in science had a similarly shadowy existence among philosophers and spokesmen for science. Of course, all proclaimed the open and self-critical character of science. But nowhere was the problem of error in genuine science dealt with seriously. This is because the overriding ideological function of science was to show how science always got it right. Historians could nearly always explain the falsification of established scientific theories through myths of the failings of the side that was ultimately proven wrong.

Among traditional historians the adherents of phlogiston (a reagent in a theory of combustion) had 'ignored' the problem of negative weight; those of caloric (which is still measured in elementary physics experiments) had 'ignored' the cannon-boring experiment of Count von Rumford; and so on. Such stories required a considerable simplification and distortion of the historical record; on the case where a great scientist was simply wrong, as Newton on the corpuscular theory of light, the story was fudged. Incomplete knowledge at the time was never a sufficient excuse for honest scientific error; otherwise our own incomplete knowledge might be leading us astray. The possibility of honest, competent scientists being in error, the spectre of falsehood resulting from the application of scientific method, had never been seriously confronted.

Confirmation of this thesis of mine comes from the example of Popper (1963), who recognized the possibility of error with great boldness; and who indeed defined genuine science by its falsifiability. But his great example was the legendary Einstein, who (it might be imagined) earned the right to possess truth, by sincerely admitting that his theory might be false. Popper's whole logic is devoted to showing how his scheme of science is fundamentally

different from those of the verificationists, in spite of its concentration on theories that *pass* tests rather than fail them.

But once the rot had set in, nothing could stop it. With the loss of the faith that science possesses, to a unique degree, the True, and following on from that the Good, successive philosophers occupied ever more radical positions. The end of the epistemological tradition was accomplished by Paul Feyerabend. But a year before his last salvo, *Science in a Free Society* (1978), there appeared the first popular work in the new direction, where science has been studied for philosophical purposes as 'just another' social activity. This was *Laboratory Life* (1977), by Latour and Woolgar, a somewhat self-mocking anthropological essay on those strange creatures and their 'inscriptions'; we might call it the school of 'the scientist as fuzzy-wuzzy'. The members of the Science Studies Unit at Edinburgh University, notably Barry Barnes (see 1974, 1977) and David Bloor (see 1976, 1983), had been the pioneers in advocating a version of relativism with their 'strong programme in the sociology of knowledge', but their style and background were those of the traditional scholars; and their paradoxical theses never caught on with the public in the same way as the tales of the anthropologists and ethnomethodologists.

Even the historians of science are now sharing the general spirit of rebellion. Whiggery (referring to the 'Whig interpretation' of history as the story of progress) is now nearly a term of abuse among the more sophisticated professionals; they know that the events of the past were quite other than the brave precursors of the glorious present. One historian has argued, though on dubious evidence, that Newton 'fudged' his data on several crucial occasions (Westfall 1973). Also, the ideological sensitivity of genuine history, breaking the propaganda image of the Good Scientist and the teaching image of Cumulating Facts, has been advanced in a paper entitled 'Should the history of science be X-rated?' (Brush 1974). Indeed, the history of science is now reaching the point of maturity, where it need not stay at the level of muckraking or demystifying exercises.

Personal Interpretation

We have lived long enough with the new atmosphere of criticism of science to be able to grow out of our emotional reactions to it, and to begin to appreciate its meaning for the future of science. We all know that science is intimately linked to society, that science cannot be perfect in an imperfect society, and yet the future of science cannot simply be reduced to that of society. But we still lack a unifying theme to encompass the ongoing technical, social and ideological changes in science. I would like to make a tentative offering in that direction.

The clue to a deep problem is the ambiguities in the term 'science'. Does it

mean 'pure' or 'basic', or 'applied' or 'mission-oriented', or even 'R & D', or a bit of all of them in varying proportions at various times and places? The different names refer to very different activities, each with their own internal and external goals and ideologies. To let 'science' cover the whole lot, invoking only the technical puzzle-solving common to them all, leaves out some of the most important elements of science of the past and present. To distinguish among the various sorts of science, producing species of hyphenated scientists, might seem more conducive to clarity. But then we find that the categories blend into each other at so many points that paragraphs of explanation would be needed to establish each demarcation. So we can make a first conclusion: the complex multiplicity of roles and the consequent ambiguity of self-consciousness are now essential structural features of science.

Other social institutions doubtless have the same property; whether science suffers to an exceptional degree could be explored in a disciplined study. But, another feature of the situation of science aggravates its tensions and contradictions. Considering the many roles performed by scientists, we notice that one is almost always absent: that of the consultant professional, who acts on behalf of a client and takes personal responsibility for his decisions. The scientist may produce internally motivated results like a *savant* or scholar; or he may solve technically motivated problems in a corporate enterprise as a *scientifique* or research worker. Only rarely can he do what an independent engineer or physician does as a routine: solve problems and take decisions whose quality is soon tested by the welfare of a client. Now we know that the learned professions have plenty of problems of their own. But to a great extent these arise from a failure to honour a public trust that is embodied in a multitude of particular cases. The scientific community cannot even claim to have such a problem. There are no institutions for qualifying a scientist as a professional or—more significant—for disqualifying him or her.

In this feature of the social structure of science, we can see the sources of the strongly enforced alienation of scientists: from society; from the fruits of their work; from any effective sense of responsibility. Individually cut off from social decisions and their consequences, either by personal remoteness (such as academics) or subservience (such as research employees), they have neither the experience nor the opportunity to do anything to control the engine of total change that they are still fabricating. Even the engineer who insists that it's his duty only to follow orders is at a higher level of awareness, for he at least sees the problem, and can choose a policy. The community of science is then in a position of creating great power while being deprived of the responsibility for its use. This is a new twist on the old formula for corruption. It may help to explain the essential confusion, in practice and ideology, which has been revealed by the recent critical studies of science.

Clearly this is not a healthy situation. Yet it is built into the style of research, as is evidenced both by the necessary autonomy of some (not easily defined) portions of it, and by its externally directed applications, without which science would receive only small social support. Considered as a problem, it is

one that cannot be solved like a puzzle in social administration or education. Indeed, considered as a threatening challenge, it might well be classed as a 'contradiction'. Will it be a driving contradiction, forcing a restructuring of goals, ideas and institutions? Or will it be a crippling contradiction, where established interests cling to their bits of power and security, preventing any change before catastrophe strikes? Only time will tell. But I hope that we have here a useful element in the enormous task of understanding, criticism and action which will accompany the formulation of new science policy studies.

This essay was first published in *Science, Technology and Society. A Cross-Disciplinary Perspective* (eds I. Spiegel-Rösing and D.J. Price), London and Beverley Hills, Sage Publications, 1977, pp. 71–89.

References

Aristophanes (423 BC) *The Clouds*, tr. Benjamin B. Rogers, Bell, London, 1916.

Arnold, P. (1958) *Histoire des Rose-Croix*, Paris.

Barnes, B. (ed.) (1972) *Sociology of Science: Selected Readings*, Penguin.

Barnes, B. (1974) *Scientific Knowledge and Sociological Theory*, Routledge, London.

Barnes, B. (1977) *Interests and the Growth of Knowledge*, Routledge, London.

Bernal, J.D. (1939) *The Social Function of Science*, Routledge, London.

Bloor, D. (1976) *Knowledge and Social Imagery*, Routledge, London.

Bloor, D. (1983) *Wittgenstein: A Social Theory of Knowledge*, Macmillan, London.

Blume, S.S. (1974) *Towards a Political Sociology of Science*, The Free Press, New York.

Bonelli, M.R. and Shea, W.R. (eds) (1975) *Reason, Experiment and Mysticism in the Scientific Revolution*, Science History Publications, New York.

Brooks, H. (1975) 'Can science be re-directed?', lecture at CNAM, Paris, December 1975.

Brush, S.G. (1974) 'Should the history of science be X-rated?', *Science* **183**, 1164–82.

Burtt, E.A. (1924) *The Metaphysical Foundations of Modern Physical Science*, Kegan Paul, London.

Clagett, M. (1959) *Critical Problems in the History of Science*, University of Wisconsin Press.

Debus, A. (ed.) (1971) *Science, Medicine and Society in the Renaissance* (W. Pagel Festschrift), Science History Publications, New York.

Descartes, R. (1638) *Discours de la Méthode*, deuxième partie.

Easlea, B. (1973) *Liberation and the Aims of Science*, Chatto and Windus, London.

Ellis, N.D. (1972) 'The occupation of science', in Barnes (1972), pp. 188–205.

Ferguson, J. (1971) *Socrates*, The Open University, Milton Keynes.

Feyerabend, P. (1978) *Science in a Free Society*, New Left Books, London.

Forman, P. (1971) 'Weimar culture, causality and quantum theory 1918–1923: adaptation in German physicists and mathematicians to a hostile intellectual environment', *Historical Studies in the Physical Sciences* **3**, 1–116.

Fuller, W. (ed.) (1971) *The Social Impact of Modern Biology*, Routledge, London.

Gillette, R. (1974) 'Velikovsky: AAAS forum for a mild collision', *Science* **183**, 1059–62.

Gillispie, C.C. (1959) 'The *Encyclopedia* and the Jacobin philosophy of science: A study in ideas and consequences', in Clagett (1959), pp. 255–89.

Grabiner, J.V. and Miller, P.D. (1974) 'Effects of the Scopes trial', *Science* **185**, 832–7.

Helmholtz, H. von (1893) 'On the relation of natural science to science in general', in

Popular Lectures on Scientific Subjects (ed. M. Kline), Dover Publications, New York, 1962, pp. 1–28.
Huxley, A. (1932) *Brave New World*, Chatto and Windus, London.
Huxley, T.H. (1893) 'On the advisableness of improving natural knowledge', in *Methods and Results*, Macmillan, London pp. 1–28.
Jaynes, J. (1972) 'Fechner, Gustav Theodor', *Dictionary of Scientific Biography*, Scribner, New York, Vol. IV, pp. 556–9.
Kearney, H. (1971) *Science and Change, 1500–1800*, Weidenfeld, London.
Knight, D. (1972) 'Chemistry, physiology and materialism in the Romantic period', *Durham University Journal* **64**, 139–45.
Kottler, M.J. (1974) 'Alfred Russel Wallace, the origin of man and spiritualism', *Isis* **65**, 145–92.
Latour, B. and Woolgar, S. (1977) *Laboratory Life: The Social Construction of Scientific Facts*, Sage, Beverley Hills and London.
Lenin, V.I. (1909) *Materialism and Empirio-Criticism* (tr. D. Kvetko). In *Collected Works*, Vol. 13, Laurence, London, 1930.
Marcuse, H. (1964) *One Dimensional Man*, Beacon Press, Boston.
Merton, R.K. (1942) 'The institutional imperatives of science', reprinted in *Social Theory and Social Structure*, The Free Press, New York, 1967.
Mitroff, I. (1974) *The Subjective Side of Science*, Elsevier, Amsterdam.
Pearson, K. (1892) *The Grammar of Science*, Scott, London.
Popper, K.R. (1963) *Conjectures and Refutations*, Routledge, London.
Price, D.J. (1963) *Little Science, Big Science*, Columbia University Press, New York.
Ravetz, J.R. (1964) 'The Mohole scandal', *The Guardian* 27 October.
Ravetz, J.R. (1971) *Scientific Knowledge and Its Social Problems*, Oxford University Press.
Ravetz, J.R. (1972) review of *Science in History* by J.D. Bernal, in *Technology and Culture* **13**, 664–6.
Rose, H. and Rose, S. (1969) *Science and Society*, Allen Lane, London.
Rose, H. and Rose, S. (1976) 'The incorporation of science' in their *The Political Economy of Science*, Macmillan, London.
Roszak, T. (1969) *The Making of a Counter-Culture*, Doubleday, New York.
Roszak, T. (1972) *Where the Wasteland Ends*, Doubleday, New York.
Salomon, J.J. (1973) *Science and Politics*, Macmillan, London.
Schumacher, E.F. (1973) *Small Is Beautiful*, Abacus, London.
Staude, J.R. (1967) *Max Scheler, an Intellectual Portrait*, The Free Press, New York.
Steneck, H.H. (ed.) (1975) *Science and Society: Past, Present and Future*, University of Michigan Press.
Szilard, L. (1961) *The Voice of the Dolphins and Other Stories*, Simon & Schuster, New York.
Thompson, D. (1953) *Science in Perspective: Passages of Contemporary Writing*, John Murray, London.
Thomson, G. (1941) *Aeschylus and Athens*, Routledge, London.
Webster, C. (1976) *The Great Instauration: Science, Medicine and Reform, 1626–1660*, Duckworth, London.
Weinberg, A.M. (1967) 'Criteria of scientific choice', in *Reflections on Big Science*, Pergamon, Oxford.
Werskey, G. (1975) 'Making socialists of scientists: whose side is history on?', *Radical Science Journal* (London) **2/3**, 13–50.
Westfall, S. (1973) 'Newton and the fudge factor', *Science* **179**, 751–8.
Wheeler, H. (1975) 'Science's slippery slope', *The Center Magazine*, Santa Barbara, California, January/February, 1975, pp. 64–72.
Wiener, N. (1964) *God and Golem, Inc.*, MIT Press, Cambridge, Mass.
Ziman, J. (1960) 'Scientists: gentlemen or players?', *The Listener* **68**, 599–607.

The Marxist Vision of J.D. Bernal

The idea of 'progress', which more than any other single element defines modern European civilization, became associated with science only rather recently, in the eighteenth century. Before then all human affairs were conceived largely in religious terms, even science; and the rise of a purely secular idea of science, as the embodiment of progress, occurred in Britain only in Victorian times. The present century has seen repeated blows to the assumption of progress, though up to now it has proved remarkably hardy, always adapting its content to changing circumstances. My own dialogue with the ideas of J.D. Bernal, as the leading representative of a humanistic Marxist vision of science, has developed through my witnessing of the tragedy of his lifelong commitment to a faith in science as progress.

If we consider the fortunes of the idea of progress during this century, we are impressed by the frequency with which people could experience disillusion. 'Losing faith' is supposed to be a problem that afflicts the religious; but on reflection we see that belief in progress and science is also vulnerable in its own way. Much of the creative thinking about science in this century can be interpreted as a struggle to retain faith; and I shall use this idea to analyse J.D. Bernal and the significance of his work. The fact that Bernal was a Marxist and personally committed to the Communist movement serves here to emphasize the conflicts he experienced. Although the guilt suffered for association with Stalinism is different from that for the atomic bomb, at root they are manifestations of the same rationalistic optimism gone wrong.

I can view Bernal's endeavour with particular sympathy, coming a generation after him and starting early in life with a political commitment roughly where he was then. In my case it was relatively easy to disengage myself from the constraints of political commitment and of intellectual style in which he was eventually trapped. My private debate with Marxism began in my early teens, when I sensed a shallowness in the writings of Marxist philosophers who were clearly dedicated human beings and sometimes also distinguished scientists. It was further shaped by my political development then and into the

1950s, whereby I formed my own assessment of socialism as it was then being produced in Eastern and Central Europe. Somewhat later, when I came into the history of science, an urgent item on my personal agenda was to see what, if anything, could be done with Marxism as a source of insights. In this connection I studied Bernal's work, and even had the good fortune to meet him once for a private conversation.

I came to see Bernal as a tragic figure, one whose life had been so compromised and corrupted by the struggle to retain his faith that by the end he could not really make sense of it. But I had already discovered that that sort of 'failure' is not at all the same as life wasted. When occasions presented themselves, I welcomed the opportunity to work out my thoughts on Bernal. One such came in an invitation to contribute a chapter on Bernal as an historian of science, to a proposed commemorative volume. When this did not materialize, the essay was available for a special issue of the journal *Isis*, marking the fiftieth anniversary of the Second International Congress of the History of Science. On that occasion, a delegation of Soviet intellectuals thrust the Marxist interpretation of the history of science before the world, and thereby changed the lives of a number of brilliant young English scientists. As it appeared in *Isis*, my essay was followed by a critical appraisal by Richard S. Westfall.

The following year I had an invitation to lecture from the Science Policy Foundation, whose Director, Maurice Goldsmith, was a (not uncritical) follower of Bernal in his social vision of science. The proposed titled was 'The social functions of science'; and I adapted that to a review of Bernal's classic *The Social Function of Science*,[1] whose third edition had appeared just forty years previously. It was, I admit, a rather thin excuse for speaking of the past rather than of the present and future; but the lecture was well received, and points from the discussion were incorporated into the published text.

J.D. BERNAL AND THE SOCIAL FUNCTIONS OF SCIENCE

Regardless of its insufficiency as the solution to a personal quest, Bernal's historical writing provided edification and inspiration to readers in many lands. His direct influence on the construction of socialist policies, in Britain and elsewhere, came through his personal contacts and his essays on contemporary science. A collection of these was published as *The Social Function of Science*.[1] It went through three editions in the early war years in Britain, a testimony to its popularity and significance.

Re-reading it after many years was as exciting for me as its first discovery. In this book I could see a really distinguished mind at work. How effortlessly could Bernal identify, classify, analyse and solve problems! With a magnificent sweep, his surveys run through the history, sociology, political critique and the future of science. His was a coherent vision, one deriving from a great tradition

of progresive thought about science which first matured in the mid-eighteenth century but was, I think, enriched and deepened by Bernal's own intense concern for science and democracy.

Reflecting on the forty years since that edition, we can see how in many ways his vision has been realized. We have had a planning of science, and also the applications of science to human welfare. These have truly relieved ordinary people of discomfort, pain, deprivation and squalor to a degree which might have been unimaginable to people who didn't have Bernal's optimistic faith in science. In Norbert Wiener's terms, science has made possible 'the human use of human beings'. We may, with Bernal, speak of the next phase, a 'scientific–technological revolution'. Through all problems and criticism, let us never lose sight of those positive achievements.

Beyond Bernal

Recently there has developed a sense that the problems are rather deeper than those which Bernal could see. Although he identified many infirmities and abuses of science in his time, there are new problems which one feels could not have been analysed in his framework of ideas. Perhaps the best example of such a problem is given by the accusation made by Lord Zuckerman, who was a younger contemporary of Bernal's.

Zuckerman has gone on public record accusing particular kinds of scientist of being 'the alchemists of the arms race'.[2] His model of the nuclear arms race is one where teams of scientists, not in universities but in defence establishments, dream up devices based on the application of known physical principles. Then, acting like denizens of bureaucracies anywhere, they try to aggrandize their own little empires by selling their devices in the normal bureaucratic way to their superiors.

But these devices happen to be not increased education provision or welfare programmes, but weapons of mass destruction. Thus, these scientists produce the impetus for weapons development. This impetus goes right through the system and results in politicians believing that there is a *need* on strategic grounds for such and such a weapons system to be available in a few years' time. The weapons system is then ordered and the scientists on the 'other side' then say with increasing plausibility 'Ah, this system that we are developing is just what *our* side will need to counter that system which *theirs* is now deploying!' So Zuckerman has blamed such scientists as being among the prime movers in the arms race. Zuckerman is a man who speaks from knowledge and experience, as a former governmental scientific adviser. Yet he feels that there is a deep moral corruption in some sectors of science, by which this particular phenomenon is produced.

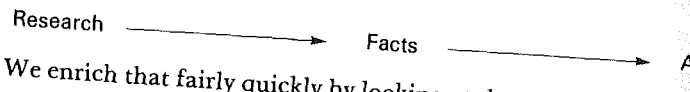
There are other cases, which I will discuss in passing, where we have grounds for unease about the behaviour of scientists in the social side of their work, and in the management of the applications of science. I reflected on all of this as I

looked at Bernal's book, and I had a wondrous feeling of puzzlement. Because as I read the book it was all so coherent, so convincing, so self-contained that it was impossible to see where he got it wrong. This was a striking phenomenon, of the type that can present the historian with the occasion for framing and studying a significant problem. For when a deep and coherent set of ideas about the future turn out to be inaccurate, the error will lie less in particular details than in something basic. So I found myself conducting an historical exercise just to see how Bernal could have missed those things which have now become quite crucial problems in the management of science and technology, given the depth of his understanding and his personal shrewdness about the society of science.

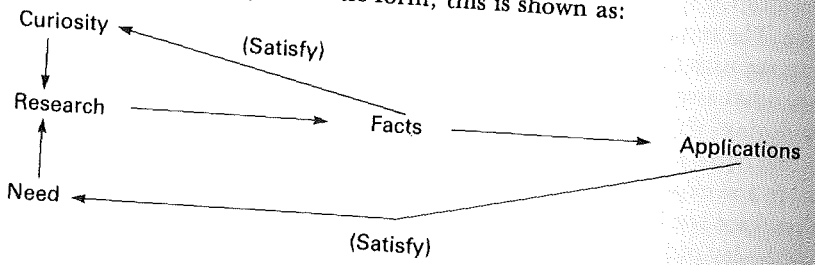
Bernal's vision, and its limitations, may thus provide us with clues for the development of an enriched appreciation of the social functions of science, appropriate to our time which are even more complex and problematic than his own.

A Way Forward

I try to do this by constructing some simple charts of the process of science; and by their successive elaboration to trace the development of insights into the problem. For the first version, we have the basic model for 'autonomous' science; from the research activity flow results leading to the True and also the Good.



We enrich that fairly quickly by looking at the motivations for the research. We start off with curiosity driving the scientist. You can say that the scientist has curiosity, he does research, and gets some facts which satisfy his curiosity. Of course we know that one set of facts throws up more problems, leading to more curiosity, and the process iterates. On the other hand, there is social need. A perceived social need can stimulate research which produces the relevant facts; the facts then provide the basis for an application, and thus the need is satisfied. In diagrammatic form, this is shown as:



It was part of Bernal's greatness that he saw that science needs *both*

motivations. Pure curiosity leads to 'ivory tower' science, with only haphazard applications to human needs, while excessive concentration on applications can lead to the trivialization of research. But the combination of the two, first proclaimed by Francis Bacon in his call for experiments 'of fruit *and* of light', provides the stimuli for a balanced and healthy science.

We are already in a position to analyse how things can go wrong. First there is the old, simpler explanation of how things can go wrong: scientific curiosity is blocked by 'dogma and superstition'. The appreciation of this as a barrier goes back at least to the Enlightenment; it was recognized through the nineteenth century. In those times, the main agency of 'dogma and superstition' was the institutions of religion. In that epoch the struggle of science against the stifling influence of institutionalized religion was, as Bernal saw, part of the struggle for human freedom in general.

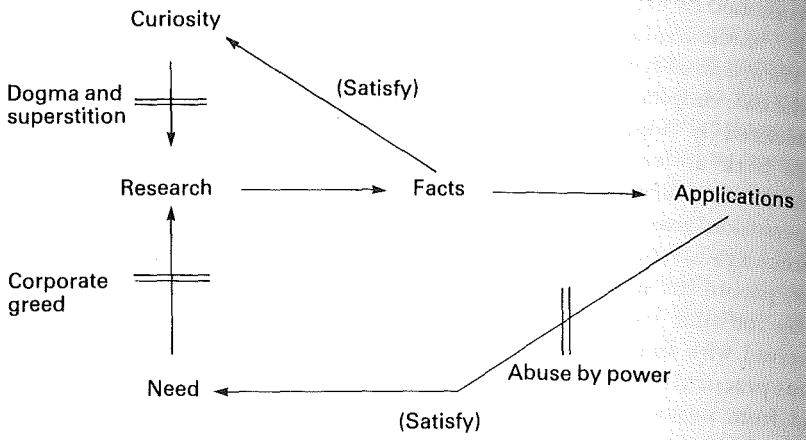
By the time we come to the twentieth century, when science is closely involved with industry, and where finally the applications of science are of great practical importance, a new kind of problem arises: the cycle leading to and from human needs can be interrupted, or distorted, or destroyed by secular institutions. I think Bernal was original in achieving this appreciation, for it requires a radical political and social viewpoint, unlike that of the traditional 'rationalists'. As Bernal pointed out, applications of science can be blocked or distorted by commercial greed. It may be that this was more obvious during the time of the Depression than now; Bernal quotes a case where large firms simply buy up patents so as to prevent that competition which, through innovation, would threaten their investments. This has been less obvious since the Second World War; but the distortion of technology into anti-social directions has been well documented.

Another 'abuse' of science is applications which are themselves deformed and evil; the worst is war. Instead of being an application serving social need, these harm and frustrate social need. War-science is little studied by sociologists of science, and not at all by philosophers of science. This uniform silence of the academics only heightens our appreciation of Bernal's clarity of vision. For such science, now estimated to absorb something like half the corps of 'scientists' world-wide, deforms and corrupts science and technology alike. Conducted in secret, largely beyond the control of legislatures, it distorts the direction of R & D, and (even in the USA) starves less glamorous sectors of science and of industry.

We are now in a position to enrich the original, somewhat simplistic analysis, and to depict the problems as Bernal saw them. I have tried to lay out the elements so that the obstacles to the completion of the cycles are clearly seen as external to science itself.

I should say that Bernal himself saw and described all such things as they occurred in his own time. His conclusion was simple: this is what you must expect when you have an unjust and inefficient social order kept going by greed and warfare. Change the social order, and you will be able to change

science. And he indicated here and there in his book, writing in the mid-1930s, that there were signs that in the USSR, these problems were beginning to be solved. I comment on some of those examples below.



By using my model a little more, we can see ways in which the process can be further elaborated. I hope thereby to see the kinds of problems which Bernal himself would have appreciated, had he been living and working a bit later, when the evolution of our kind of science had proceeded further.

Unrealism

The first new problem I can point to is in fact quite old; it was discussed by Jonathan Swift in his Voyage to Laputa in *Gulliver's Travels*. For a long period of optimism it was forgotten by proponents of science. But now we find that one may identify a social need, call for research whose facts will provide relevant applications, get it done, and then discover that the research is ineffective. There is a promise which cannot be fulfilled; more than that there is a promise in which great investment has been made, which still cannot be fulfilled. Perhaps the easiest example to cite is the American war on cancer: Richard Nixon said, 'if Jack Kennedy can get a man to the moon, I can cure cancer'. He decided to throw money at the cancer problem as Kennedy had decided to throw money at the space problem. Kennedy had won—they got a man to the moon and back; but Nixon failed—cancer was not cured.

We may then think of all the many problems which the proponents of social science believed it could solve; one must remember that Bernal was part of the movement for the extension of the scientific method into all spheres of life. We can now see that in some way or other the characteristics of social problems, even problems of poverty and disease, do not easily yield to that style of attack, based on the physical sciences, which people confidently believed they would. My best example here is a very simple one; those who created the British

National Health Service imagined that it would be a self-reducing government department. Once we caught up with the arrears of ill-health, due to past poverty and neglect, there would be less and less need of a health service! It is fair to say that we did catch up in many respects, but the problem did not go away. We can say that we now have problems at a higher level of effectiveness for humanity: but expensive problems, there still are.

Ecology

Ineffective science leads at worst to disappointed hopes; but we must now face new kinds of problem resulting from the very power of science-based technology. It is possible to make applications of science which have very great effects indeed, some of which are intended but others quite unintended. And these latter may be adverse or even catastrophic.

I think it important that this category of *blunders* in science was not recognized by Bernal and indeed was recognized by hardly anyone of his generation. For this I quote from one paragraph of his book, on page 379, describing the dreams of a science-based society.

> There are large tasks still for mankind to undertake—the ultimate conquest of space, of disease, and death, most of all their own ways of living together. We get a kind of foretaste of this activity by the work of the Soviet Union in the conquest of the Arctic. With a fully organised world society such tasks could be pushed far further. It will no longer be a question of adapting man to the world but the world to man. For instance, the present Arctic with its wastes of tundra, glacier, and sea-ice is a legacy of the geological accident of the Ice Age. It will disappear in time, leaving the world a much pleasanter place, but there is no reason why man should not hasten the process. By an intelligent diversion of warm ocean-currents together with some means of colouring snow so that the sun could melt it, it might be possible to keep the Arctic ice-free for one summer, and that one year might tip the balance and permanently change the climate of the northern hemisphere.

It thus appears, with the wisdom of hindsight, that Bernal and his generation were utterly lacking in 'ecological' sensitivity. This is by no means a criticism of scientists in Bernal's field or of his political persuasion. Even those who were out there coping with the environment were possessed of a 'magic bullet' mentality, as Kenneth Mellanby recorded in *Pesticides and Pollution*: in the 1950s the applied entomologists expected that soon there would be poisons that would kill the nasty bugs and leave the nice ones.[3]

Thus Bernal and his contemporaries, and indeed his predecessors, were the victims of what we can now see as an illusion, from which we only now are recovering. That is, that the consciously benevolent applications of science

cannot do harm. This assumption, or rather faith, has a long history, back indeed to the seventeenth century. We can see it in Francis Bacon, who really believed that magic and the idea of 'powers too great to be revealed' were not merely sinful, because you were getting something for nothing, but also implausible, because things do not really happen like that.[4] As the vision of the world (for European peoples) lost its quality of enchantment, it became common sense that science was really safe—effects could only be proportionate to their (material) causes. The concepts of a trigger reaction, of a non-linear, synergistic reaction, of a complex and unstable ecological *system*, were effectively absent from mainline scientific thinking, including that of Bernal, until well into the post-war period. In the absence of such concepts one cannot imagine blunders, and one cannot imagine some things with which we are now confronted as urgent problems of survival.

Ethics

Finally, I come to the third major problem which is not to be found in Bernal's book—the consequences of the human frailties of scientists. Bernal certainly knew of the imperfections of the community of science; and in the book he discusses various organizational problems with great shrewdness and insight. But as to the scientists themselves, he maintained a commitment of extraordinary intensity. The very last paragraph of the book (a *most* significant location) consists of a *credo* of Science as Communism. Thus

> Already we have in the practice of science the prototype for all human common action. The task which the scientists have undertaken—the understanding and control of nature and of man himself—is merely the conscious expression of the task of human society. The methods by which this task is attempted, however imperfectly they are realized, are the methods by which humanity is most likely to secure its own future. In its endeavour, science is communism. In science men have learned consciously to subordinate themselves to a common purpose without losing the individuality of their achievements. Each one knows that his work depends on that of his predecessors and colleagues, and that it can only reach its fruition through the work of his successors. In science men collaborate not because they are forced to by superior authority or because they blindly follow some chosen leader, but because they realize that only in this willing collaboration can each man find his goal. Not orders, but advice, determines action. Each man knows that only by advice, honestly and disinterestedly given, can his work succeed, because such advice expresses as near as may be the inexorable logic of the material world, stubborn fact. Facts cannot be forced to our desires, and freedom comes by admitting this necessity and not by pretending to ignore it.

> These are things that have been learned painfully and incompletely in the pursuit of science. Only in the wider tasks of humanity will their full use be found.

So science is the model of the classless society, and scientists themselves are somehow purified by the activity of research. Here again, Bernal follows the prophetic vision of Francis Bacon very closely. Bacon himself had a Utopian vision of what could be done by men of science. His theory for it was cast in theological terms, although he did appreciate its social aspect sufficiently to write the prophetic *New Atlantis*. With Bernal the vision was cast in secular terms, but still motivated by the highest idealism.

Francis Bacon had already worried about the problem of avoiding the production of evil by the powers of science. His own solution to the problem was, in my paraphrase, 'it needs good men to do good science'; of course this is a pun on the word 'good'. For Bacon had seen how easy it is to do rotten, shoddy or selfish scholarship or research. He deduced that it requires people of a superior moral stature even to start effective work; and then by that very activity they would be further purified.[5]

Bernal shared this idealistic ethical vision. He has many deep and penetrating discussions of the organization of science, its ailments, how they can be cured. He said very shrewd and penetrating things about democracy and authority in laboratories; but still he was basically quite idealistic about the society of science.

One confirming clue to this idealism may be seen in the contemporary works of the sociologist Robert K. Merton. In his classic early papers on science, he included a 'norm' of 'communism' (or alternatively 'communalism'), which enjoined the free sharing of results. Much later Merton came to recognize, and to grapple with, the social and ethical problems resulting from the quite real relations of property that are embodied in scientific results. Such a 'property' is rather subtle, and can be overlooked by someone who is not desirous of the vulgar rewards of fame and power. But the many disputes over priority and authorship that have disfigured the life of science show that even the greatest of scientists are not immune to such considerations.

In fact, it is easy and natural to draw parallels between the individualistic conception of the achievement of new, atomic 'facts' and the capitalist social order in which such a view took root in early modern Europe. The property inhering in an authenticated scientific result is more subtle than real-estate but it is equally profitable. It draws 'rent' through the prestige derived from citations. Bernal's ignoring of this (now obvious) aspect of science under capitalism seems to reflect a personal determination to imagine science as the unconditioned agent that will yield the Truth of nature and the Good for mankind.

Now that we are aware of the property relation, which conditions and partly motivates the work of individual scientists, we can see how the community of science necessarily reflects (though it does *not* precisely copy) the structures of

property and power of the wider society in which it is embedded. Bernal's vision of an essentially classless, communistic science, only waiting for society to catch up with it for its full potential to be realized, must be recognized as rather more Utopian than Marxist in its socialism.

Institutionalized Conflicts

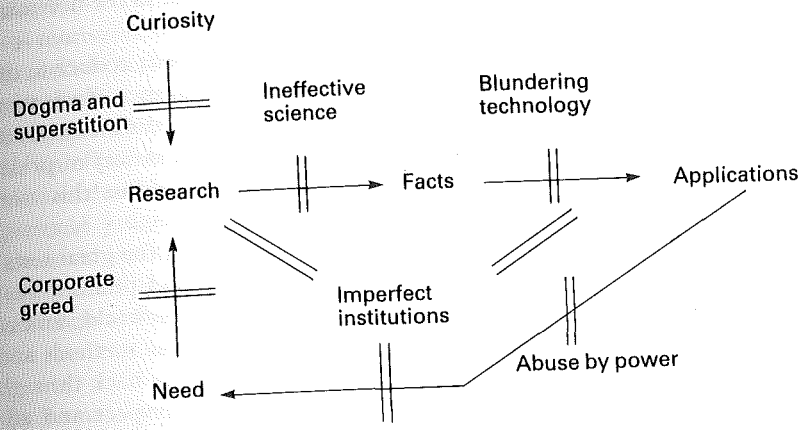
Thus our third new problem, after ineffective research and blundering technology, is the imperfections in the social institution of science. How are they realized? I cannot give an analysis here; a few examples must suffice. One has already been shown—Lord Zuckerman's analysis of the role of scientists as promoters of the nuclear arms race. Some might object that such people, working in secret on technology, rather than the acquisition of new knowledge, are not really 'scientists'. This becomes a matter of definition; but those who would restrict the honorific term to those who pursue old-fashioned independent, inapplicable research are really more in the spirit of Polanyi than of Bernal.

Another manifestation of the problem (which I owe to Sir Alex King's mention in the discussion after the original lecture) is 'bureaucracy'. Large-scale projects require organizations that are also large-scale, but which are in addition complex and which possess a tendency to take on a life of their own. Those who have never experienced this phenomenon may find it hard to imagine how people can completely lose sight of the original 'mission' of an institution, concentrating solely on immediate problems of position, prestige and patronage. But it happens, with deadly regularity; and institutions devoted to 'research' are as susceptible as any other. Lack of awareness of this problem has been responsible for the disappointment of very many hopes in the post-war period, from those of a new international order in science, to those of a new social order in particular countries.

Finally, I mentioned problems of 'quality control' in science and technology, again problems which might have been inconceivable as *systematic* problems to a dedicated scientist of Bernal's generation. This is a large topic, which I discussed, perhaps prematurely, in my book of 1971.[6] Now we see the sprinkling of cases, which may be more notorious than significant, of outright fraud in science. More significant for me at present are analogous problems in technology. To cite only one salient example: we should ponder the significance of the fact that it took a *student* to discover that the niobium used as an alloy in the steel of PWR pressure-vessels has such intense and long-lasting radiation that the decommissioning of such reactors will be enormously more difficult and expensive than previously assumed.[7] Where were the scientific experts who should have checked on such a possibility during the previous decades? Working for a bureaucracy that preferred not to know, is the answer.

Conclusion

We can now make the final full elaboration of the earlier model. It is relatively easy to locate 'ineffective research' and 'blundering applications'. But 'imperfect institutions' have such pervasive effects, that we must simply put them somewhere as a reminder.



Applying the spirit of Bernal himself, it is possible to explain the emergence of such new problems by the changes in science itself, and we must never forget that they appear alongside the brilliant successes of science and its applications. In my own book I remarked on the ways that science has become capital-intensive, so that decisions on research represent an investment of resources rather than a preference by a lone craftsman. Consequent to this, the community changes to large, hierarchically organized laboratories; and with the interpenetration of science and industry, the 'society' of science cannot survive as an uncontaminated haven of non-material values.

But I would not like to leave the matter there, as if it could be simply 'reduced' to a 'material' base. I think attitudes and commitments are important too. Among those which I think made science particularly vulnerable to the stresses and temptations of post-war 'big science' was an optimism, bordering on hubris, concerning the powers of the applications of science for human benefit. But there was another belief, less obvious but equally fundamental, with which I shall conclude.

This faith is that the community of science cannot get things seriously wrong. Of course individual research is partly speculative and always corrigible. Of course the march of progress renders old theories obsolete. But—and this was the working experience of academic scientists—all change was *progressive*; errors and mistakes were winnowed out by honest criticism, and new theories generally include and explain their predecessors. The idea

that science could collectively get something wrong for a long enough period for it to be significant simply lay outside Bernal's framework of possibilities.

Of course it becomes a matter of degree; if blunders in technology are revealed, by whatever means, within one or two decades, does not that prove the self-correcting nature of the scientific process? In a sense, yes. But for practical purposes, the time-lag (increased by the bureaucratization of science) may be uncomfortably long. It may be long either in the lifespan of an individual whose investment of time and talent is nullified, or in the necessary response-time to a maturing technical or ecological crisis.

In either case, confidence in corrections 'in the long run' may reduce to the only such certainty, which is the ending of life. The self-assurance of scientists that outsiders could not possibly be right, which (it is now admitted) was so strong in the nuclear power industry, can now be seen as a variety of pride. Science was traditionally believed to provide a defence against this vice; perhaps that made its onset even more difficult to detect.

Bernal's *Social Function of Science* was perhaps the last of the great testaments of science in which a person of broad intelligence and philosophical depth could argue coherently that the social problems of the world, and of science itself, could be solved simply by the application of the methods and approach of science. With him, a line of prophets extending back through Huxley and Condorcet to Bacon, came to an end. With respect and admiration, we re-read it, partly to recapture that optimism and human commitment, and also to gain clues as to how our own times present challenges which require new insights on fundamentals. As a scientist, Bernal would, I am sure, have considered this too as progress.

J.D. BERNAL'S HISTORY OF SCIENCE

J.D. Bernal was one of that small group of brilliant British radical scientists whose vision of a Marxist science of society, based partly on a new history of science, was shaped by the Soviet contribution of 1931. The most significant products of his vision were the monumental *Science in History* and the more specialized *Science and Industry in the Nineteenth Century*.[9] The latter work is a thoughtful and careful exploratory essay, almost as by a scholar for scholars. The broad sweep of *Science in History* and its continuing popularity, wide diffusion, and ideological significance make it the work by which Bernal's Marxist endeavour is to be assessed. I shall concentrate on it here.

Bernal was fortunate not only in having an encyclopaedic mind, but also in living in a social milieu that did not automatically relegate those who exercised broad interests outside research to the status of eccentrics. The achievement of *Science in History* is not at all reduced, but is better understood, when we recall the analogous works by J.B.S. Haldane, Lancelot Hogben and Joseph

Needham.[10] We can look on Bernal and his colleagues as late participants in a tradition of 'philosophy of nature', before specialization and fragmentation had finally conquered science, leaving only 'popularization' as the link between reflective research scientists and the lay public.

Science in History may be analysed for its contribution to the history of science and used as evidence for the evolution of Bernal's thinking on basic problems. My conclusion is that the book has made a disappointingly small contribution to the history of science; that this is because while essentially it was motivated by post-war problems, it attempted pre-war solutions; and that Bernal's historical work suffered particularly from its origins in the Cold War period, when even the ecumenical Joseph Needham was isolated and strident. Bernal's lack of success is a reminder that the theoretical maturing required for an effective Marxist analysis of science in modern societies is even now a hope rather than an achievement. This negative assessment is not at all a personal judgement on Bernal or on the commitment that produced his historical work. It could be argued that every great thinker fails, in part, on her or his life's project; but the incidental achievements are a permanent enrichment for humankind.

By Bernal's account in the preface to the book, the occasion of its composition was an invitation to give a series of lectures in 1948. Before then his historical concerns, though long-standing, had been insufficiently urgent to go beyond private notes or the sort of brief synoptic account that appeared in his 1939 *Social Function of Science*. When *Science in History* was finally complete, he admitted (perhaps too modestly): 'It is only now that I am beginning to understand what are the problems of the place of science in history'.[11] Although (as I shall argue later) the work was conceptually and politically obsolescent by the time it appeared, its magnificent scope and coherent outlook yielded an excellent publication career. It ran through four editions in England alone, between 1955 and 1969, as well as through two in the United States. There were translations into some fourteen foreign languages, covering all the socialist countries and all major culture areas. By a strange irony, the last edition came out in four illustrated paperback volumes, marketed in the 1970s in England and the United States to a readership to whom Bernal's commitments and concern must have been utterly remote.[12]

Science in History is really two books in one binding. The first five parts take the story from the Paleolithic to the Victorian age. This historical material was little altered through successive editions, though a series of notes at the back of the third edition (the only one with substantial revisions) record Bernal's reactions to new discoveries and to critical reviews. The massive sixth part, comprising nearly half the bulk of the book, deals with 'science in our time'. It draws mainly on Bernal's own experience, scientific and political, and it was extensively reworked, for scientific and political reasons, between editions. At first a cold-war tract in praise of socialism and its works, it later became a quite

balanced essay in socialist apologetics. The concluding seventh part was allowed to remain substantially intact from its first version.

Professional historians of science, even those who disagreed deeply with Bernal, admired his daring and his vision in attempting to write a global and synthetic history.[13] But they were wrong to compare his work to their own rather limited excursions in synthesis. The model for Bernal was not that of the specialist historians, who at their best could tell only the history of progress of scientific ideas. Rather, the tradition of histories of the progress of knowledge, arts and culture, starting with Condorcet and extending to H.W. van Loon and H.G. Wells, should be seen as the source and inspiration for Bernal's work.[14] Themes, assumptions and even materials were available for him there, particularly in the relation of intellectual and cultural progress to its social and material context. And he could make a significant enrichment to the theoretical setting of these works through his own version of Marxism. It was not only that he could provide a plausible real cause for progress more substantial than an instinct for achieving the Enlightenment values of reason and liberty. With his scheme he could offer a certain measure of historical imagination by seeing the past as more than a story of gradually decreasing error and negativity. He assumed that progress depends on the needs of a ruling class, which at the beginning of its rule desires to improve human knowledge and power, becoming stagnant and reactionary only towards the end. Thus even the bourgeoisie is not totally or simply bad; rather it has outlived its historical usefulness. In this mode of explanation, Bernal could assert that ancient priesthoods promoted magic rather than rational enquiry 'when the early temple establishment decayed, and the priests became increasingly dependent on the offerings of the faithful'.[15]

Bernal's explanatory framework, which looked beyond the content of scientific ideas to the broader interests of relevant producers and consumers of those ideas, helps explain the lack of contact between him and established historians of science. The latter generally dealt with 'scientific ideas', at their best in a broad and sympathetic way (as did Charles Singer and W.P.D. Wightman), at their worst in a narrow, dogmatic or precious way (as did Alexandre Koyré).[16] The bad influence of Koyré should not be underestimated. In his 'idealist' reinterpretation of Galileo, he denied Galileo not merely a social context but even his experiments! When he achieved eminence in America, he insisted on a totally superficial interpretation of the scientific revolution as a choice between particular styles of geometrical and mechanical thought.[17] Anything suggestive of social influences on science or scientists Koyré dismissed as 'Marxist'.[18] Thus a generation of young historians of science, who might well have been receptive to Bernal's 'externalist' approach if not to his conclusions, was kept firmly in abstracted intellectual history.

Were that the whole story one could view, and judge, *Science in History* as a production in a great tradition of amateur philosophical popularization,

related only indirectly to the narrow, frequently pedantic professional research in the field. But the weakness, indeed the tragedy of the work, lies in Bernal's neglect of post-war Marxist thought in the history of science, which was then being developed by a lively group including S.F.Mason.[19] The lack of contact between Bernal and these people poses an important problem and in itself provides a clue to the character of *Science in History*, for one of the main functions of the book, whether or not consciously intended by the author, was to reinforce the faith of those who were already committed to a Marxist interpretation of the world. They saw the majestic sweep of the survey, combined with a multitude of convincing and detailed facts and further enhanced by the scientific eminence of the author. All this could provide a powerful argument in support of a commitment to Marxism as the truly scientific philosophy of our age. It really works: here is human history explained in Marxist terms. In this respect the achievement of *Science in History* is genuine. A look at one later work in a non-Marxist rationalist tradition, *The Ascent of Man* by Jacob Bronowski, shows how much more real explanation can be accomplished within a Marxist perspective.[20] But for those who were already questioning the adequacy of pre-war Marxism (as I was in the 1960s), the book was not particularly inspiring or reassuring.

The historical section of the book (as opposed to the political Part VI) had three quite discrete divisions, with very different problems and style. The earlier part of the history, before the Renaissance, moves rapidly and with a sure touch through the standard material. While convinced of the ultimately decisive role of the economic foundation and equally certain of the distinction between real science and false paths to knowing, Bernal is by no means mechanistic or unimaginative in his interpretations. He even spares Christianity some of the blame assigned to it by Gibbon. He sees the Decline as a cyclical process, reflecting the inability of classical civilization to solve the contradictions basic to its characteristic social relations of production. In all this section one sees Bernal's mind performing at its best: reworking a mass of material into a synthesis that is plausible and coherent. Its only failing is that it solves somewhat too much, leaving nothing for puzzlement and wonder, in all that vast spectacle of growth and decline. But it is a matter of taste whether or not 'the Greek miracle' should be left partly as such, or totally reduced to natural causes; and Bernal's audience would certainly prefer historical science to mystery.

Jumping over to the other successful sections, we can admire Bernal's reviews of the technical aspects of contemporary science. It is well to remember that his was not merely an encyclopaedic intellect; his profound achievement in applying physical methods to the elucidation of living systems required scientific insight of a rare order. So in this section we have surveys of twentieth century science that should become historical sources in their own right. His account of physics is noteworthy in this respect.

The weaknesses of Bernal's approach become most apparent in the middle part of the story: the creation of modern European science from the sixteenth

century onwards. In broad outline his story is quite plausible. First, it was nascent capitalism in the later Middle Ages that enabled the developing productive forces (including science) to escape the stagnation suffered previously in the Chinese and the Islamic cultures. Then there were the three distinct phases, terminating in 1540, 1650, 1690, which he calls Renaissance, Wars of Religion, and Restoration. These involved, respectively, progress in 'uses of science', the 'first great triumphs of the new observational, experimental approach', and their consolidation after 'the overthrow of the feudal–classical theories in the previous hundred years'. As the developments described in the narrative move further from the sphere of production more exclusively into that of thought, the plausibility of the reduction to the economic foundation weakens drastically. Several crucial problems are glossed over: the role of a specifically capitalist class in fostering science; the use of science and scientists as agents of social oppression; and the causes of the 'disenchantment' of the world of Nature.

Bernal's neglect of the first issue, the support given science by a specifically capitalist class, derives from his neglect of political aspects of power in favour of its economic aspects, a neglect shared by traditional Marxist historians. Bernal (always sensitive to complexity and paradox) remarks at the beginning of his discussion of the middle period that it has no convenient name; and so he labels it with the surprisingly un-Marxist 'Wars of Religion'. He does not mention that political historians have discussed a phenomenon of absolutism, where the state, whose power was then concentrated in the crown, asserted itself against other institutions. Because of their preference for economic at the expense of political explanations, Marxists have tended to search for the relations of 'science' with 'the bourgeoisie' rather than with the state. But the latter relations were vital, first in France and then in Central and Eastern Europe, and available in the latter case for adaptation to the needs of socialism. 'Capitalist' and 'statist' post-feudal social contexts each had their contribution to make. Evidence for the importance of the latter is provided by these words of Galileo:

> It is impossible to obtain wages from a republic, however splendid and generous it may be, without having duties attached. For to have anything from the public one must satisfy the public and not any one individual; and so long as I am capable of lecturing and serving, no one in the republic can exempt me from duty while I receive pay. In brief, I can hope to enjoy these benefits only from an absolute ruler.[21]

Thus he decided to return to ducal Florence from republican Venice, in spite of the benefits and security he enjoyed there.

There is no explicit sign that Bernal sensed how inadequate the classical (economic rather than political) Marxist framework was on this period. Perhaps quite unself-consciously he used a crucial term with damaging equivocation, to enable his essential point to be stated: 'The new experimental philosophers, or scientists as we would now call them, . . . appeared more as

individual members of the new bourgeoisie, largely lawyers . . .; doctors . . .; a few minor nobles . . .; churchmen . . . and even one or two brilliant recruits from the lower orders . . .'.[22] To be sure such people belonged to cities; but we must remark that none were in roles related to the bourgeoisie as defined by Marx. I do not mean to criticize Bernal personally for this gap at the centre of his story, but rather to show how here his Marxist framework does not carry him through, either to fruitful solutions or even to interesting problems.

On the second problem, science as a means of oppression, the Marxist tradition at least allows for its recognition, while for the rationalists it is a pure embarrassment. The genocidal horrors practised by European imperialism on its conquered lands (what we now call the Third World), involving the technologies of warfare, of primary-products exploitation and of various addictive poisons, did not interest Bernal greatly. Closer to home, the relation of Paracelsian alchemy to radical politics was already becoming familiar to Marxist historians of the English Civil War period. But Bernal did not consider these contradictions within a class-linked natural science, as well as the incidental costs of the advance of a class in its 'progressive' phase, to be historically significant once the ultimate goal has been fixed. The result of this approach is that Bernal's history is externalist-whig, differing only in detail from the internalist-whig style of the historians of science dominant in his time.

Finally, the really big problem of the seventeenth century is the exceptionally rapid rate then of the 'disenchantment' of nature. As a whole, this was a complex process extending through many centuries of European history. If one's unit of time is a quarter millennium, then it is clear that 'rationality' and 'capitalism' rise simultaneously. But on a finer scale the correlations become very difficult to maintain. Although he had no doubts that the ancient pseudosciences were nonsense, Bernal was not uncharitable toward them. He even recognized, in connection with Paracelsus, that '. . . owing to the intrinsic complexity of chemistry, it was this intuitive and mythical approach rather than the rational mechanical one that was to be most successful in advancing chemistry until its revolution in the eighteenth century'. This bold statement puts the scientific revolution in an unusual perspective. For if chemistry was so complex, then *a fortiori* so would be biology, medicine, and most of craft production. Hence we may say that for the greater part of science as it affected life, the 'mechanical' approach was as counter-intuitive as the supposed motions of the earth. Also, it was largely unsuccessful; as Bernal remarks, 'In fact, the only parts of the external world where their methods succeeded were those already cultivated by the Greeks.'[23]

The rise of the 'scientific' world view in the seventeenth century was therefore *not* a reflection on the successful practice of 'disenchanted' science. Rather, even by Bernal's account it would seem to be something that was, in Lenin's phrase, *von Aussen hineingetragenes*. Perhaps it came from the rise of the bourgeoisie and its world view (on which Max Scheler and Christopher Caudwell speculated[24]), but it is doubtful whether the appropriate sort of bourgeoisie was then rising with sufficient strength to do the job. Further,

there is the awkward fact that the progressive 'corpuscular' philosophy was introduced to Protestant bourgeois England as an import from Catholic Absolutist France by such Royalist emigrés as Walter Charlton and Thomas Hobbes. Thus neither 'scientists' nor 'bourgeois' provide the basis for an adequate 'rational' explanation of the diffusion of the scientific world view as it actually happened in the seventeenth century. This has remained a puzzle and a source of contention among historians to this day; but it passed unrecognized by Bernal in spite of his sharp perception of particular points.

For a rationalist history of science written in the heroic 1930s, such criticisms as these would have been quite inappropriate. Although Hessen and his colleagues in 1931 were by our standards naive and simplistic, they were pioneers, throwing speculative bridges across the gap between 'science' and 'society'. But by the 1950s there had been some development. The surge of left-wing and Marxist thinking in the wartime and early post-war period produced a few people seriously concerned with the social history of science. They tended to be isolated both from their technocratic comrades of the Old Left and (later) from the mainly Leavisite radicals of the New Left; both these were uncritical about science, the former in accepting it, the latter in rejecting it. However, the new Marxist scholars were there, and a monument to their achievement is S.F. Mason's *Main Currents of Scientific Thought*. Had its rich insights been cultivated and developed, our critical understanding of the past and the present of science would have been greatly accelerated. In particular, I believe that the Leeds group of scholars in the 1960s could have made an even more powerful synthesis of the intellectual and social aspects of seventeenth century English natural philosophy. But it was not to be. Mason and Lilley were academically isolated, and neglected by Bernal, whose sources outside the pre-war Marxists seemed to be the leading academic historians. Only by accepting the assessments of the latter group could he state the paradox (needless because it was incorrect) that the scientific innovators 'from Copernicus to Newton, were the most conservative in their religious and philosophical outlook'.[25]

In some respects this is a cautionary tale. Bernal was not the first, nor the last, of those who when they eventually discover the unravelled perplexities of their present situation turn to past history for the answer. Rarely do they reflect that history, though capable of fruitful study by amateurs, is nonetheless a discipline requiring respect. There, as anywhere else, enquiry is not so much discovering facts as solving problems. And if the problem is neither clearly defined initially nor encouraged to grow in dialogue with the evidence, the result will be flat. This seems to have been the case with Bernal, in spite of all the qualities of *Science in History* that enabled it to compete for a popular audience with the best of the professional historians' work. One sign of his failure to grow as a historian is his dropping of historical studies once the first edition of the book was complete. The later editions show only slight evidence

of continued interest in the subject: very occasional textual changes, and the notes on scattered topics added at the end. The contrast with the material on contemporary society, almost entirely rewritten, is striking.

In fact Bernal did not stop reflecting or trying new approaches to the problems of science. From the history of science he turned to 'the science of science'. Given his later awareness of the virtual absence of worthwhile social science in the socialist countries and his generally low opinion of its state under capitalism,[26] this would seem to be a rather weak reed to clutch. It is as if Natural Science, previously the great motor of progress, subject only to the needs of the Developing Forces of Production, were now to be guided into its final consummation of the Scientific–Technological Revolution by a few practitioners of the most mechanically materialist, ideologically blind, statistical social science. This is a far cry from the problem-situation that motivated the original work on the book: 'the troubles of the times, and the inescapable connection between them and the advance of science' and the task 'to release the new forces of science for welfare rather than destruction . . .'.[27]

The full story of the evolution of Bernal's practical and philosophical thought in the post-war period is not my concern here. In my review of the fourth (illustrated) edition, I remarked that Bernal then had apparently less hope for solving mankind's problems and also less faith in the socialist nations.[28] I may speculate that until the threat of nuclear annihilation became real (that is, some time *after* Hiroshima) Bernal could see no *essential* problem in the advance of science, which he had always believed would lead by logical necessity both to material plenty and to social rationality. His reflections on the new problems of science were conditioned by the sharp polarization of politics in the early 1950s; for his accounts of contemporary science he could not afford the luxury of objectivity or even of public fraternal criticism. Perhaps even the ongoing English Marxist speculations about the political history of science, however orthodox their authors, were also fraught with dangers for Bernal as the leading Western spokesman for an embattled socialist camp.

All these particular circumstances may have done no more than give precise shape to Bernal's endeavours. Joseph Needham could move through his vast encyclopaedic study towards a statement of his own combination of radicalism and mysticism, for that is what he had believed in all along. Bernal started as a prophet of a disenchanted magic, where men's reason would accomplish all imaginable good things.[29] In the 1930s his faith in reason was enriched by a keen social conscience, and he turned to history only when the realization of his dreams seemed threatened, following the Second World War. He then slowly learned that for all its intrinsic interest, history spoke only in riddles: the past presented paradoxes wherever it was closely scrutinized, and it also offered little reassurance or guidance for the novel problems of the present. So Bernal moved on, or back, to a technocratic style of approach, in the 'science of science'.

Since Bernal's time there has arisen in the West a new current of social and

institutional history of science, now growing rapidly in strength and sophistication. It would be a kind tribute to Bernal's memory to describe it as a development from his work, but unfortunately that would be true only in a minor part. For the new studies of this kind are far from Marxist in their orientation, but rather partake of the new, tough, demystifying attitude to science. At one main centre, Pennsylvania, the focus is on institutional history and power politics; at another, Edinburgh, it is on the spuriousness of 'objectivity' and on 'the strength of social interests'. My appreciation of Marxism, in particular dialectical materialism, is enhanced by study of these newer works. For I believe that Marxism could provide a considerable enrichment to the present practice of social history. However, professedly Marxist history of science is now practised in the West on a very small scale. The Roses' social history of modern science, *Science and Society*, came at the end to confront the contradiction of the evils created by the science of today. One of the authors, while still supporting Marxist materialism against the subjectivists and mystics, has moved to a position that would have stunned Bernal: she feels that scientific research may now be too tainted to be an honest occupation.[30]

This consideration of Bernal may seem an epitaph on his endeavour, reducing it to error and futility. But history, unlike Bernal's sort of science, has a meaningful place for tragedy.[31] Another way of describing the seventeenth century would show how the great prophets and philosophers of the age, including Bacon, Galileo, Descartes, Harvey and Newton, ended their careers with the failure of their life's project—and some in deep disillusion as well. Bernal's historical work, with all the limits resulting from its circumstances, has a touch of greatness about it. Should Marxist scholarship become revitalized at some future time, a grateful and critical appreciation of that work will be an important foundation for further progress.

The first part of this chapter, 'J.D. Bernal and the social functions of science', was adapted from an essay first published under the title 'The social functions of science: A commemoration of J.D. Bernal's vision', *Science and Public Policy*, October 1982. The second part of the chapter, 'J.D. Bernal's history of science', was first published under the title 'Bernal's Marxist version of history', *ISIS* 1981, **72**, 393–402.

References

1. J.D. Bernal, *The Social Function of Science* (3rd ed., London: Routledge, 1942).
2. Zuckerman, Lord, *Nuclear Illusion and Reality* (London: Viking, 1982).
3. Mellanby, Kenneth, *Pesticides and Pollution* (London: Collins, 1967), p. 122.
4. Bacon, Francis, *De augmentis scientiarum*, 1620, trans. in *Works* (Ed. Ellis and Spedding), Vol. IV, Bk 3, Ch. 5, p. 367–368.
5. J.R. Ravetz, 'Francis Bacon and the reform of philosophy', 1972, see this volume.
6. J.R. Ravetz, 'Quality control in science', *Scientific Knowledge and Its Social Problems* (Oxford University Press, 1971), Ch. 10.
7. Norman, Colin, 'A long term problem for the nuclear industry. Worn-out reactors may remain radioactive too long to entomb. A student discovered what the experts missed', *Science*, 1982, **215**, 376–379.
8. N.I. Bukharin, *et al.* (1931) *Science at the Cross Roads: Papers Presented to the International Congress of the History of Science and Technology held in London, June 29 to July 3, 1931, by the Delegates of the USSR* (London: Kniga, 1931; 2nd ed. London: Frank Cass, 1971).
9. J.D. Bernal, *Science in History* (London: Watts, 1954); J.D. Bernal, *Science and Industry in the Nineteenth Century* (London: Routledge & Kegan Paul, 1953).
10. See G. Werskey, *The Visible College* (London: Allen Lane, 1978).
11. Bernal, *Science in History* (1954), p. vii.
12. 2nd and 3rd eds: London: Watts, 1957, 1965. Paperback (4th) ed: London: Watts/Penguin, 1969; Cambridge, Mass.: MIT Press, 1971.
13. L. Pearce Williams, review of *Science in History*, *Isis*, 1957, **48**, 471–473.
14. Condorcet, *Esquisse d'un tableau historique des progrès de l'esprit humain* (Paris, 1794); H.W. van Loon, *The Story of Mankind* (London: Harrap, 1922; with 27 reprints to 1961); H.G. Wells, *The Outline of History, Being a Plain History of Life and Mankind* (London, 1920).
15. Bernal, *Science in History*, p. 90.
16. Charles Singer, *A Short History of Science* (Oxford Univ. Press, 1941); W.P.D. Wightman, *The Growth of Scientific Ideas* (Edinburgh/London: Oliver and Boyd, 1950).
17. A. Koyré, 'Galileo and Plato', *Journal of the History of Ideas*, 1943, **4**, 400–428.
18. Henry Guerlac, 'Some historical assumptions about the history of science', in A.C. Crombie, ed., *Scientific Change* (London: Heinemann, 1963), pp. 797–812, on p. 810.
19. S. Lilley, *Men, Machines and History* (London: Cobbett Press, 1948); S.F. Mason, *A History of the Sciences: Main Currents of Scientific Thought* (London: Routledge & Kegan Paul, 1953).
20. Jacob Bronowski, *The Ascent of Man* (London: BBC, 1974).
21. Galileo (letter of 1610) quoted in Stillman Drake, *Discoveries and Opinions of Galileo* (New York: Doubleday, 1957), p. 65.
22. Bernal, *Science in History*, pp. 287–288.
23. *Ibid.*, pp. 273, 350.
24. Christopher Caudwell, *The Crisis in Physics* (London: John Lane, 1939), Ch. 2, 'The world as machine'.
25. Bernal, *Science in History* (1965), p. 350 (not in 1st ed.).
26. *Ibid.*, pp. 870–872, 833–834, 840–842.
27. Bernal, *Science in History* (1954), p. vii.
28. J. R. Ravetz, rev. of *Science in History* (1971), *Technology and Culture*, 1972, **13**, 664–666.
29. J.D. Bernal, *The World, The Flesh and the Devil* (1st ed. 1929; London: Cape, 1970); see the discussion in Brian Easlea, *Liberation and the Aims of Science* (London: Chatto and Windus, 1973).
30. Hilary and Steven Rose, *Science and Society* (London: Penguin, 1969); Hilary Rose, 'Hyper-reflexivity: A new danger for the counter-movements', in Helga Nowotny and Hilary Rose, *Counter-movements in the Sciences* (Dordrecht: D. Reidel, 1979).
31. J.R. Ravetz, 'Tragedy in the history of science', in Mikulás Teich and Robert Young, *Changing Perspectives in the History of Science* (London: Heinemann, 1975).