Can Science Survive in the Modern Age?
Author(s): Harvey Brooks
Source: *Science*, New Series, Vol. 174, No. 4004 (Oct. 1, 1971), pp. 21-30
Published by: American Association for the Advancement of Science
Stable URL: http://www.jstor.org/stable/1731876
Accessed: 11-05-2016 02:20 UTC

Can Science Survive in the Modern Age?

Harvey Brooks

According to Caryl Haskins (*1*), "modern archaeological research leaves little room for doubt that the basic technological revolutions of mankind antedated the scientific revolution by many thousands of years." It is thus clear that "a technology of distinction can evolve and can even reach notable heights in a society of wholly pragmatic outlook," but what Haskins calls the "close and vital partnership" between science and technology is unique to the modern era and almost certainly essential to a sustained technological civilization at present levels of population and quality of life. In the words of Haskins:

> Only a cultural climate where the fundamental drives of curiosity and of the love of discovery for its own sake are understood and cultivated can a true science flourish. Paradoxically, it is only when such a science becomes deeply rooted as an element of high culture that a progressively innovative technology can be maintained over long periods, fusing eventually into the close partnership with which we are familiar today. And even when attained, that partnership can never be taken for granted. The maintenance of its health and vigor requires constant attention.

One of the questions I would like to raise in this article is whether in fact the conditions of modern society are generating a cultural climate which is no longer hospitable to the cultivation of a "true science" and whether the absence of such a viable science, in the sense expressed by Haskins, will destroy our ability to manage and control the technology which science has helped to create, and which is essential to modern civilized life.

The so-called scientific revolution which began 300 years ago was the most successful of all revolutions in man's history, yet it was quiet and without violence. It has changed the face of the world and the human condition more and in a shorter time than any other human institution or social innovation.

It is not a mere figure of speech to speak of a "revolution." To a larger extent than has been realized until recently, it was a conscious and deliberately planned revolution. Francis Bacon was its prophet, and laid out the blueprint for it with remarkable clarity and insight. The nineteenth century image of Bacon as an advocate of pure empiricism and mindless data gathering is wrong. He foresaw the importance of hypothesis, theory, and understanding, but above all he described accurately the cumulative and cooperative nature of the scientific enterprise and the fact that it was a social system for understanding nature which transcended the capability of individual men, or even one generation. If Bacon was the prophet, the members of the British Royal Society were the disciples who practiced the Baconian doctrine and propagated it, as shown by the recent interpretation by Margery Purver (*2*).

Since the scientific revolution every political revolution in the West has accorded a central place to science and has invoked it to justify and sustain its goals, as emphasized by Don Price (*3*). According to Thomas Jefferson, "the societies of scientists . . . form a great fraternity spreading over the whole earth" (*1*), and science was given a high priority in the early American republic until theoretical science was overwhelmed by the rising tide of pragmatic populist ideology in the Jackson era (*4*). Later both the Russian and Chinese revolutions accorded science a high place in their scheme of things and invoked the authority of science to legitimize their social prescriptions. Scientists, too, were given a high place by government, although with some ambivalence arising from the conflict between the intellectual freewheeling necessary for a "true science" and the political orthodoxy required in a Marxist state.

Today some believe that the new leftish student movements, which are worldwide, form the vanguard of a new revolution, the first true revolution in advanced industrial societies. If so, it will be the first in modern history which has not attempted to ally itself with science. So far as its ideology is discernible at all, it seems to be antiscientific and antirational, more akin to the early Christians than to the modern Marxists, despite its Marxist slogans.

In a real sense, in the most advanced industrial societies, man seems within sight of achieving the program which Bacon set for him three centuries ago, what Donald Schon has referred to as the "technological program" (*5*). Says Bacon (*6*):

> Nor can nature be commanded except by being obeyed and so those twin objects, human knowledge and human power, do really meet in one; and it is from ignorance of causes that operation fails. . . .

It is also true that pragmatic technology, if accompanied by the "ignorance of causes" of which Bacon speaks, is not viable today, and the greater man's "mastery" of nature, the more essential is his understanding of causes in order that mastery be disciplined to obedience. So, indeed, if the modern era has created social and cultural conditions in which the enterprise of science is no longer viable, it has sown the seeds of its own disintegration and decay, to be followed by the disappearance of a large fraction of the world's present population and a decline in the material conditions of human life. It is a mere detail whether this will come about first through some ecological disaster, through the decay and demoralization of the technological structure, or through a military holocaust.

The "disasters" that might be produced by modern technology can only be understood, contained, and controlled with the aid of scientific understanding. Even the occurrence of deleterious side effects of technology has only been revealed as a consequence of scientific research, much of it not initially directed to that end. It was more

---

The author is dean of the Division of Engineering and Applied Physics of Harvard University, Cambridge, Massachusetts 02138. This article is the text of the C. P. Snow Lecture delivered at Ithaca College on 19 January 1971. The author gratefully acknowledges permission from Ithaca College to publish the lecture.

than a century ago that Marsh (*7*) warned of the bad effects of man's exploitation of the planet, but it is only in very recent years that science has assembled the solid evidence from which the threat of man's activities could be defined and quantified, rather than merely speculated about as a possibility. In the early part of the 20th century spokesmen for the then burgeoning conservation movement predicted that the United States would run out of most of the natural resources essential to industry and agriculture within decades (*8*). But doomsday has long since come and gone, and the relative price of most resources has actually fallen in comparison with other products of the economy. Everywhere the industrial nations are struggling with agricultural surpluses, whereas coal mining is an ailing industry due to competition of substitute fuels. New technology—more efficient utilization of fuels, soil and forest conservation, new techniques of exploration, recovery from less concentrated ores and deposits, substitute materials—has altered the resource picture, so that few students of the subject any longer regard resource shortages as the most imminent problem of civilization. There is, of course, a problem on the scale of many decades, but there is considerable confidence that science and technology will, as in the past, change the outlook as the future arrives.

At the moment environmental pollution and the management of waste seems a more imminent problem than the depletion of resources, but it also seems likely that our environmental concern will recede into the future, much as did our concern about resources, as advancing knowledge and new or redirected technology provide us with the means of coping. There may be, however, a big "if" in this. This relates not only to the necessary political will—which seems to be developing—but also to assurance of the continued advance of the full interwoven fabric of science. Later on I shall try to detail why I believe this last is so necessary.

The Climate for Science

Let me come back to the question raised in the beginning, namely, whether modern societies have created a social and cultural climate which makes unlikely the continuing generation and utilization of the knowledge to make such societies manageable. I refer to both generation and utilization, because failure could occur either from lack of adequate knowledge or from failure of society to accept and apply the implications of what is known by some of its members. I strongly suspect that these two conditions are not independent. A culture which accepts the primacy of the scientific method as a means of knowing, and provides political, economic, and psychological support for basic scientific activity, will also in the long run be prepared to accept and apply the knowledge gained. Conversely a society that fails to apply what it knows, will ultimately not want to know, and will repudiate the generation of knowledge, on the ostrich theory that what it doesn't know won't hurt it.

One aspect of this issue is the growing demand for participation in decision making by those affected. This goes considerably deeper than the ideology of "participatory democracy" advocated by radicals. It is generally conceded as a necessity today by spokesmen for almost all shades of political opinion. Almost every recent government report on environmental problems or technology assessment has emphasized the need for greater participation of affected interests at an early stage in the decision process. According to Moynihan (*9*), "Western democracies, perhaps especially the American democracy, seem continually to be evolving new forms of participation by citizens in the governing process, generally transforming experimental, ad hoc practices into more or less routinely acknowledged rights." Thus, he says, there is "a fairly steady evolution toward direct citizen participation in the actual workings of government, a movement that has somewhat lagged but otherwise paralleled the increasing professionalization of government service." We actually do not know the extent to which such wider participation is compatible with wise management of an increasingly complex technology. If in fact such management is strongly dependent upon scientific understanding, as I believe it is, then increased public participation will demand greatly increased public understanding of science, and appreciation of the nuances, limitations, and implications of scientific evidence—in other words, an increasingly rational and scientific culture. As society becomes more complex and interconnected, the "systems" effects of each decision spread more and more widely. Thus, each segment of the public, if it is truly to participate in the decisions that affect it, must become aware of a wider and wider range of activities. At the same time, the government official responsible for policy must become sensitive to a wider and wider variety of publics whose interests or value preferences may be affected by his decisions. We do not really have enough experience to know whether such a process is convergent. Are democratic participation and rational coherence really compatible? Participation, especially in decisions involving science and technology, poses a horrendous problem of what Herbert Simon has christened "attention management." There is a danger that random shifts of political attention, generated by overload of the political process, may simply inject an instability into the decision-making process which will cause it to "hunt" from one extreme to another, much like a mechanical or electrical servo system with too much feedback and not enough anticipatory control (*10*). According to Andrew Hacker (*11*), the trend toward participation may only mean that "most people estimate their opinions too highly to adhere to any consensus, let alone one involving common goals" with the result that "America has become an ungovernable nation whose inhabitants refuse to regard themselves as citizens of a social order in which the authority of government plays a principal role" (*12*). For participation to succeed, the participants must be prepared to accept a consensus at some sacrifice to their own interests and preferences, and the total nexus of sociopolitical decisions must possess some minimum degree of logical internal coherence. The very potency of our technology guarantees that it will soon call the bluff of inconsistent goals and preferences, as we are now seeing happen with respect to environmental protection and the power shortage.

Thus, widespread participation in decisions under modern conditions requires a widespread acceptance of rationality as a guide and an appreciation for scientific standards and criteria of judgment. This is probably a necessary condition for acceptance of a consensus by those participating. Unless there is some common standard of judgment, participation is merely a euphemism for a naked power struggle, a competition for a share of the scarcest commodity of all, the attention of decision-makers, with the tools of gaining attention being rhetoric, stridency, and, ultimately, violence. Such a struggle tends to escalate in time until all rationality disappears. In this situation science is seen as either an irrelevant or a hostile force.

Some Trends That Operate against Science

One can see a number of social trends that seem to operate against a continuing healthy scientific enterprise. It is difficult to decide on the true importance of these trends, for our society is also characterized by increasing diversity and scope for individuality, and since science is an activity of a small minority of people, it is not obvious that majority trends will be decisive. As a group, scientists are probably the most "inner-directed" (*12*) of the various character types who enter various occupations, and hence most immune to outside valuation of their activities and goals. Still, even scientists are not immune to the climate of thinking among their peers, and the status accorded to them through the majority's valuation of their contribution.

Let me, then, list impressionistically, some of the trends I see and their possible implications for the scientific enterprise.

1) The achievement of scientific excellence is highly dependent on the protestant ethic of work and individual achievement. Although the scientific community is one of the most open of all social systems in terms of all criteria other than its own internal standards of performance, its insistence on individual excellence and on rigorous interpersonal valuations runs strongly counter to contemporary egalitarian trends and rejection of all competition and comparisons between people, especially among youth. In the current jargon science is an inherently "elitist" activity, and its success as a social institution is highly dependent on a rigorous selection and ranking of its practitioners by their colleagues and seniors. As science has become professionalized in the last generation, its competitiveness has, if anything, increased. Some very able and talented people seem to be rejecting the "rat race." Although some of the more extreme forms of competitiveness caricatured in Watson's book (*13*) are certainly not necessary to a healthy scientific system, the advance of science does depend on a process of natural selection of ideas and people not unlike biological evolution, and without this selective pressure, truth cannot avoid being swamped by error in the long run. Just as biological evolution runs against the average trend of the second law of thermodynamics, so does science run strongly against the social second law of the least common denominator.

The other side of the coin is that rejection of competitiveness may affect mostly the marginal people in science, and not the very best and most highly motivated, who tend to set their own standards. These people are so far above the general run of men that competition is not a meaningful spur to achievement. But probably competition is important in setting the average standards of the enterprise, and hence its productivity. The answer here is not yet clear.

2) The increasingly close partnership between fundamental science and technology is leading to both public and professional disenchantment with science because of the misuse of technology. Beginning with World War II the general public was sold on fundamental science as a prime generator of technology, and the scientific community encouraged and promoted this identification of science with technology in order to gain financial support. Up until World War II there was a kind of popular culture of technical matters, typified by ham radio, automobile tinkering, and the garage inventor. This popular culture existed side by side with, and partly independently of, a small "high culture" represented by theoretical science. There were increasing interactions between the two cultures, but by and large the high culture was nearly invisible to the public, except for a few "culture heroes" like Einstein. Few Americans have ever heard of Joseph Henry, Albert Michelson, or Josiah Willard Gibbs, although Samuel Morse, Edison, Alexander Graham Bell, and Henry Ford are household words. Since World War II the two streams have intermingled; names like Oppenheimer, Teller, and von Neumann have become household words, identified with technology—vaguely military—in the public mind.

We are moving into what Daniel Bell (*14*) has characterized as a "post-industrial society." According to Bell, "the ganglion of the post-industrial society is knowledge." More specifically:

> What has become decisive for society is the new centrality of *theoretical* knowledge, the primacy of theory over empiricism, and the codification of knowledge into abstract systems of symbols that can be translated into many different and varied circumstances. Every society now lives by innovation and growth; and it is theoretical knowledge that has become the matrix of innovation.

Yet, to the extent that Bell's assertions are valid, science finds it more difficult to claim neutrality in the political arena. The high culture and the popular culture have united, and the high culture has thus become contaminated in the minds of many with the militaristic, materialistic, and selfish features of the popular culture, that is to say, the culture of "middle America" with opprobrious connotations for the many highly educated people, who will become a near majority of our society in the next generation.

The average person—and indeed many young scientists—cannot distinguish between the content of science as a body of knowledge about the world and as a method of gaining knowledge, and the uses to which science is put by society. Nor can it distinguish between the behavior of scientists as individual actors in society, and the properties of science as a social institution for generating what John Ziman has called "public knowledge" (*15*). Above all, of course, it is the use of science for military purposes which has stimulated the reaction, and, to a secondary but almost as important an extent, the role of technology in causing the deterioration of the natural environment (*16*).

The danger in the identification of science with its uses is that it will place limits on inquiry which are not compatible with "the interconnectedness of the fabric of science." It is one thing to deplore work on biological warfare, which has little or no importance for the development of the conceptual structure of biology, and quite another to eschew work on molecular biology because it might in some not clearly foreseeable way be used for military or manipulative purposes. Yet the line between the kinds of activities is increasingly difficult to draw in practice. An extreme view argues that new knowledge can always be more readily used by those with political and economic power, therefore knowledge inevitably leads to concentration of power, and is thus inherently evil, at least in the present arrangements of society. Instead of "the truth shall make you free," the slogan is "beware of the truth, for it will be used to enslave you."

A corollary view is that there is no such thing as objective knowledge, that all rational inquiry is inherently biased by the sociopolitical environment in which it is imbedded, not only in its selection of projects, but even in its conclusions, and that the claim of objectivity is a cover for defense of the status quo, especially, but not only, in the social and behavioral sciences. Granted that complete objectivity is never possible, some turn this fact around and deduce that objectivity should not even be attempted, and that inquiry should be motivated by political commitment, specifically a radical commitment. This is, of course, an invitation to "double-think," to the warping of conclusions to fit preconceived assumptions, but it has growing persuasiveness to many people, and seems to me completely incompatible with any true science.

3) Coupled with this attack on the possibility of objectivity, and advocacy of a radical nonobjectivity, is a deeper disillusionment with rationality in general and a flight toward antirational cults. Whereas radical nonobjectivity is the cult of what seems to be a "lunatic fringe," antirationalism strikes an answering chord in a large number of people, even particularly young, well-educated people. To me this is a more disturbing trend than radical nonobjectivity. Astrology, once the refuge of the ignorant and the illiterate, is now gaining favor among many intellectuals, even young scientists, and is—God save the mark—being computerized. The national investment in astrology is between ten and twenty times that in astronomy. Eastern religions are enjoying a great vogue, and everywhere there is rising preoccupation with the emotional, the sensual, the affective aspects of human experience at the expense of the cognitive, systematic, and analytical aspects. Emotion-centered personality types are emerging as heroes to be emulated, again especially among the younger generation.

We do not really know to what extent all this represents simply a natural swing of the pendulum away from what was, perhaps, an overemphasis on the cognitive aspects of human personality, and an undue status for personalities which excelled in cognitive, verbal, and analytical skills. Some such reaction is probably healthy and was overdue. But to the extent that it implies that feeling and sympathy can substitute for reason and evidence in the management of human affairs, it is retrogressive and threatening.

4) Among some intellectuals there is also an attack on high culture in general, of which science is a part. One aspect of this is a glorification and imitation of aspects of what sociologists have called the "culture of poverty," a present rather than a future orientation, a low valuation of deferred gratification, and a high valuation of impulse and sensual enjoyment. This is in part a romanticization of the underprivileged and the dispossessed which has always characterized liberal and radical movements in the past. But it seems to go deeper and be considerably more widespread than in the past. Among scientists it has generated such slogans as "science for the people," and is closely related to but goes further than the egalitarian perspective mentioned earlier. In science it is bound to result in an emphasis on the pragmatic, and on the substitution of personal testimonial for evidence in the valuation of ideas.

5) The aspect of the climate for science that usually gets the most public attention these days, but may be the least important in the long term, is the political climate for public support of science. Compared with other constituencies that receive large subventions of public funds, the scientists have a very weak political base. Whereas the public regards them as a single community and a potential pressure group, scientists are in fact divided and at war with each other—a situation well exploited by those interested in decreasing or limiting public support.

Paradoxically, this situation may be more true in the United States, where in the recent past science has received more public support than in other countries, than it has in the United Kingdom and continental Europe where the support of basic science is much more directly tied to the entire public financing of higher education. In the United States fundamental science has prospered primarily by allying itself with various pragmatic goals of high political visibility, especially defense, national prestige, and health and, to a lesser extent, economic growth and the balance of payments. The origin of this situation is succinctly summarized by Vannevar Bush in his typically matter-of-fact way (*17*):

> To persuade the Congress of these pragmatically inclined United States to establish a strong organization to support fundamental research would seem to be one of the minor miracles. . . . There were some on Capitol Hill who felt that the real need of the postwar effort would be support of inventors and gadgeteers, and to whom science meant just that. [But] it was easy to make clear that the work of scientists for two generations, work that had been regarded by many as interesting but hardly of real impact on a practical existence, had been basic to the production of a bomb that had ended the war.

Bush points with pride to the result in the following terms (*18*):

> When large amounts of money flow, from taxes, into an effort which the public, and which, to a considerable extent, its representatives cannot understand, there is real danger present. It can take the form of support of the inconsequential, of bureaucratic control of universities, of waste, and of downright scandal. As we look back, I believe we can take pride in the fact that we escaped all these dangers to a truly remarkable extent over the years.

On the whole I believe this is a just assessment of the past, but it was still a shaky base for the support of science, and a base which now seems in the process of dissolution. So far the United States is the only developed country in which the support of fundamental research has experienced a cutback in real resources (variously estimated at from 15 to 25 percent down from its peak) during the last few years. The threat to the integrity of science lies not so much directly in the lack of resources (since they are still larger than in any other country) but in the negative response of people and plans in a system with large time lags, and in the lack of opportunity for young people on whom the system depends for its continued vitality. Peter Drucker (*19*) has referred to the "inventory crisis in careers," a crisis which is not confined to science, but applies in some measure to all careers requiring, or believed to require, advanced education. But the effects on science may be longer lasting than in other fields because of the great difficulty of scientific education and the accent on early flowering of scientific productivity.

6) There seems to be developing an increasing sense within science itself

that the most important discoveries may have been made, and that the pace of discovery and new insight of the 1950's and 1960's cannot be sustained much longer. In his retiring presidential address to the AAAS in Chicago last December, Dr. Bentley Glass (*20*) said:

> The great conceptions, the fundamental mechanisms, and the basic laws are now known. For all time to come these have been discovered, here and now, in our own lifetime. . . . We are like the explorers of a great continent who have penetrated to its margins in most points of the compass and have mapped the major mountain chains and rivers. There are still innumerable details to fill in, but the endless horizons no longer exist.

Apparently Dr. Glass was talking primarily about the field of molecular biology, but the implication was there that this was generally characteristic of the whole scientific enterprise. Glass's views are frequently echoed among other scientists. Although expressed mostly by the older generation of scientists, one also finds young people turning away from science for essentially these reasons, and this view of the finite compass of discoverable knowledge is likely to erode interest in science.

This sense of the completed edifice of science is not new in history. Lord Kelvin expressed much the same view toward the end of the 19th century, on the very eve of the explosion of new discoveries which launched the golden age of physics in the first half of the 20th century. Is this a quirk of the finiteness of human vision, brought on in part by the unusually rapid pace of discovery in recent years, or is it in fact something new under the sun, like the explosion of human population in relation to planetary resources? Only time will tell. My own belief is that this sense of finiteness is illusory, that there are still new frontiers which will open new domains of inquiry, but they are not necessarily in areas we can clearly foresee today. Perhaps one such frontier lies in the exploration and analysis of complex systems, for example, ecosystems or social systems or the integrative factors in highly organized biological systems. These are what I have called the sciences of organized complexity.

It is easy to exaggerate the meaning of having explained something "in principle." After all one could say that with the discovery of the fundamental postulates of quantum theory, all of chemistry and biology were explained in principle and therefore were no longer exciting. In fact this was even being said by the end of the 1930's. In practice, however, the richness of phenomena which lie in fields already understood in principle is a continuing source of surprise. In my own field of solid-state physics, the basic principles were already mostly understood in the 1930's, and the last great mystery, superfluidity, was clarified at the end of the 1950's. Yet this field has produced a whole series of unexpected discoveries in the last 10 years, which seems to belie the idea of "saturation." Most of these discoveries were of such a nature that in retrospect we can convince ourselves we should have been able to foresee them on the basis of principles already known, but in fact man's imagination is much too limited and the unexpected and exciting frequently springs out of fields already supposedly thoroughly explored. Thus, I find it hard to accept Glass's thesis.

7) The growth of scientific knowledge has resulted in greater specialization and finer differentiation of subject matter in research. This is said to reduce the sense of accomplishment and satisfaction in scientific discovery. A related phenomenon, also mentioned by Glass, is that the greater the volume of research, the less likely it is that any one project will produce truly original results. As specialization increases, the necessities for economy of thought and expression produce specialized languages and terminology, each unique to its own field, and this limits meaningful communication with neighboring fields. These effects reduce the sense of participation in a common scientific enterprise, and probably lessen the psychological rewards of doing science through attenuation of the sense of significance of one's own work. This problem is exacerbated by the length of apprenticeship required for a student to arrive at the frontier of contemporary research knowledge in any chosen specialty.

Undoubtedly, overspecialization and the information "explosion" have made the scientific enterprise somewhat less attractive, but I think it is easy to exaggerate this. Concern over the information explosion, for example, seems to be inversely proportional to the degree of active participation of the individual making the complaint in substantive scientific research. Journals such as the *Scientific American*, the *American Scientist, Science, Physics Today*, or *Psychology Today* have grown up to keep the harried specialist abreast of developments in science as a whole. The growth of knowledge is accompanied by compression and simplification which greatly extend the span of comprehension possible for an individual over a diversity of phenomena. New connections are constantly appearing between previously unconnected specialities, often illuminating whole new domains of inquiry. Depth of understanding in science frequently implies less rather than more specialization since abstract ideas find application in many different and unrelated areas. Science is not a series of independently filling watertight compartments. These compartments are constantly spilling over into each other. Careers of individual scientists, especially the most talented ones, reveal a surprising span of interest and contributions over a lifetime of work which often belies the image of narrow specialization. Bacon himself believed that the boundaries of the sciences should be matters of temporary convenience rather than "sections to divide and separate" (*21*). This still remains the attitude of the best scientists.

Interdisciplinary research has been growing in importance and scope in the universities at a time when it is fashionable for every pundit to take a passing swipe at disciplinary parochialism. Almost every scientist deplores the boundaries between disciplines, but it is always somebody else who is guilty of disciplinary narrowness, not the commentator. It is also at least possible that the present mania of interdisciplinary work will erode standards in the disciplines and thus in science itself.

In reviewing the preceding catalog of possible causes for the erosion of science, I find that I take most seriously the feeling that the edifice of science is nearly complete, and the demand not only from society but from scientists themselves that science always be "relevant." These two trends, in my view, pose a greater long-term threat to the health and integrity of science than does the shortage of funds, the growth of antirationalism, or other factors I have touched on. It is not that these trends of thought lack any legitimacy in reality. It is quite possible that the fields of science that have enjoyed such a spectacular rate of progress in the last 20 years are nearing the end of their greatest excitement and fruitfulness, but I am confident that new vistas will open, as they did after Lord Kelvin's famous pronouncement. Nor is it wrong that science should heed the demand for relevance, provided it does not heed it too exclusively and on too fine a scale. The matter has been well put by Sir Brian Flowers, the chairman of British Science Research Council (*22*):

> In the affairs of science there are two sets of forces acting: the external [forces] representing the aims of society and the internal forces representing the natural development of science: and there must be some balance between them, or the system collapses. . . . What happens when these forces get out of balance as they will from time to time? If the external forces are too strongly applied at some point the interconnectedness of the fabric of science is broken. The operation may become self-defeating. . . . But the internal forces can also prove too strong. When this is so, science develops in a manner unresponsive to wider needs and it also fails to benefit from the incentives which such needs generate.

In American science the balance has usually favored the external forces, whereas in the United Kingdom and in Europe it has tended to favor the internal forces. Uniquely, for a time in the postwar era in the United States, the internal forces attained explicit public recognition, possibly even to too great a degree. This came in part as crumbs from the lavish tables of mission-oriented research and development. Many now feel that the external forces are again gaining the ascendancy, and with the too active connivance of some scientists.

In the remainder of this article I should like to discuss the question of the responsiveness of science to societal priorities, and to review some of the ways in which a strong interconnected fabric of basic science, governed by its internal forces, is necessary to the fulfillment of societal needs.

Science and Social Priorities

The basic thesis—indeed a restatement of the Baconian program—has been well summarized by Bernard Barber, the sociologist of science, in the following words (*23*):

> However much pure science may eventually be applied to some other social purpose than the construction of conceptual schemes for their own sake, its autonomy in whatever run of time is required for this latter purpose is the essential condition of any long run applied effects it may have.

The difficulty in answering the question of how responsive contemporary scientific activity should be to current social priorities (which is the only kind there are) is complicated both by the long time that often, but not always, intervenes between fundamental scientific activity and its social consequences, and the large uncertainty which exists in the linkage between any given area of inquiry and its possible social impact. The linkages within science and between science and society are complex, and conventional political wisdom is an insufficient guide to the determination of relevance. In fact, judgment as to what science is relevant to what social goals is itself a difficult and complicated intellectual problem, not to be solved casually by offhand judgments. Science is linked to its ultimate social effects in many nonobvious ways. Here are a few points to bear in mind:

1) In discussing relevance, one must distinguish between "strategy" and "tactics" in science. It is sometimes, though not always, possible to determine broad strategic emphasis in terms of potential applicability, but the finer the scale of choice within a broad field, the more necessary it is that the choices of problems and projects be made in terms of internal criteria if science is to advance efficiently and economically, whether measured in terms either of its own goals or those of society.

For example, it seems fairly clear that the environmental sciences—atmospheric chemistry and dynamics, oceanic chemistry and dynamics, overall material and energy flow in the biosphere, the chemistry and physics of the solid earth, the interaction between the solar wind and the upper atmosphere—all have great potential relevance to the problems of environmental pollution and depletion of resources with which man will increasingly be faced. But the precise scientific questions to be addressed, the order in which they should be attacked, and the methods of approach must be determined by the requirements for understanding, not application. Even for the application of science an ounce of "theory" is often worth a pound of "information," and facts which contribute to the creation of significant generalizations which in turn suggest where to look for other facts, are frequently more important than facts of greater apparent immediate relevance to a particular social problem. Thus, many detailed scientific investigations may be relevant to a social problem primarily through their contribution to the formulation or verification of a hypothesis which is applicable, rather than directly. It is thus that the biologist often chooses apparently esoteric or primitive organisms having no apparent connection with human life to work out biological principles which, once understood, can be applied to more complicated systems more evidently relevant to a biomedical or an environmental problem. Most of the remarkable elucidation of molecular genetics has been worked out with bacterial and virus systems of little practical importance, but the models and theories developed have tremendous potential applications to human biology (*24*).

2) An important process by which basic science is applied is by flow through other sciences which are themselves applied. There is a sort of hierarchy of sciences beginning with mathematics and running through physics, chemistry, biochemistry, biology, and medicine. At each level the science has a higher degree of potential applicability, and yet all are highly interdependent, both conceptually and for instrumentation and experimental techniques. For example, probably the most important social impact of nuclear physics in the long run will come, not from the atomic bomb or nuclear power, but from the use of radioactive tracers to study the biochemistry of living systems. Molecular biology would not have been possible without tracers, and the whole panoply of laboratory technology and instrumentation that goes with the detection anl measurement of radioactivity. Tracers have already proved enormously important in following material flows through ecosystems. This is of great significance even without referring to the literally hundreds of direct clinical applications of radioisotopes, particle accelerators, and radiations from nuclear reactors.

An example of the impact of one science on another is beautifully described in Wigner's essay, *The Unreasonable Effectiveness of Mathematics in the Natural Sciences* (*25*). Wigner opens this essay with a story of a conversation between two friends, one a statistician working on population trends. The friend looks over the statistician's shoulder and sees the symbol π. "What is that?" he says. "The ratio of the circumference of the circle to its diameter." "Well now you are pushing your joke too far, surely the population has nothing to do with the circumference of the circle." What better epitome of the problem of relevance?

Wigner cites a different example in the law of gravitation. He points out that, with the data available to him, Newton was only able to establish the law of gravitation to within 4 percent, yet two centuries later we know it to be verified to better than 1 part in 10^6. Wigner cites this as an example "of a law, formulated in terms which appear simple to the mathematician, which has proved accurate beyond all reasonable expectation."

The modern laws of quantum electrodynamics are another example of the "unreasonable accuracy" of a theory, originally selected on mathematical grounds, which proved startingly accurate.

As another example, the concepts of quantum theory have spread from physics through all of chemistry and much of biochemistry. Computational techniques in quantum chemistry have placed us on the threshold of being able to predict from first principles the properties and reactions of quite complex chemical systems as reliably as by doing the experiment.

Chemical and biochemical concepts increasingly underlie most aspects of biology, even extending to the identification of species and the specification of ecosystems (*26*).

The essential point is that laws and principles that are simple and general in one domain of science frequently turn out to be unreasonably applicable in other domains of science having only the remotest connection with the original inquiry that first led to their formulation. This happens to an almost uncanny degree; it is a phenomenon that has never been fully and satisfactorily explained, but which happens so frequently that it comes to be taken for granted.

3) An important impact of basic science arises from the development of laboratory instrumentation which later finds its way into industrial process control, environmental monitoring, or other operational or developmental uses. Furthermore, basic science, by challenging contemporary technology in unique ways, leads to technological developments which it is doubtful would ever have occurred directly in response to more obviously "applied" needs. An interesting recent example is the superconducting magnet. The earliest motivation for the construction of a magnet came, of all things, from radio astronomy, where a lightweight, high-field magnet was needed for a traveling wave maser to be operated at the focus of a steerable radio-astronomy "dish" as an ultra-low-noise amplifier for the detection of very faint radio signals from space. The success of this application soon led to much more massive support of the development of superconducting magnets by the Atomic Energy Commission because such devices appeared ideal as deflection magnets for large bubble chambers in high-energy physics, much more economical than electromagnets.

From there the device moved to application in plasma containment in controlled fusion research and to the development of efficient guide field magnets for high-energy particle accelerators. It is not at all clear whether controlled fusion will ever prove practical, although hopes for it have soared dramatically within the last few years as a result of the Russian Tokamak experiments. What is abundantly clear, however, is that the possibility of fusion would have been much dimmer in the absence of high-field superconducting magnets. They also appear to be important in connection with possible magnetohydrodynamic power generation, and experimentation is going on with the construction of highly efficient electric motors using such magnets.

Another example comes from the field of pesticides. Until the appearance in 1952 of the various forms of chromatography as laboratory analytic tools for organic molecules, there was no means of detecting chlorinated hydrocarbons at the levels of concentration at which they appear in the environment as residues from pesticide use. As a result a recent report is able to state (*27*):

> It is safe to say that the means of examining pesticide residues in the environment are more sensitive and specific than [for] any other major contaminant group. Existing analytical chemical methods can determine such residues in the range of parts per billion or parts per trillion. Consequently the role of pesticides as environmental contaminants is better understood than that of any other major class of contaminants.

Indeed both chemical and physical methods of analysis are vital for almost all forms of pollution monitoring and control. It is doubtful whether most of these methods would or could have been developed from scratch for pollution monitoring alone. The flow of information has usually been the other way around. It has been the availability of sensitive analytical methods developed for other purposes in basic research that has alerted us to many environmental problems and stimulated further applied research and instrument development related to them.

As another example, instruments are now being developed which can measure particulate concentration in air remotely over a long path by the use of lidar—light detection and ranging by use of a pulsed laser source. Through the use of different wavelengths and comparison of them, information can be obtained on particle size distribution and on chemical composition. Here again is an instrument based on a physical tool which would never have been conceived for that applied purpose (*28*).

The conversion of a laboratory method into an instrument for operational use, of course, requires extensive applied research and development, often at considerably greater cost than the development and use of the original laboratory device. But without the original technique, which emerged from basic research, the subsequent applied work would have been futile and wasteful.

4) Basic science is an important input to decision making about technology and technological policy and also is important as an early warning and identification of problems as well as opportunities. In the past the support of science has usually been defended as a generator of technology—as a sort of substrate or nutrient on which technological development feeds. But science does, can, and should serve also as the basis for a critique of technology. As I have already hinted, a number of our present concerns about the environment were first identified as a result of basic research for other purposes. It is no accident that Rachel Carson, who wrote *The Sea Around Us*, was also one of the first people to popularize the ecological side effects of the use of pesticides in her more famous book *The Silent Spring*. Miss Carson was in close contact with oceanographers, and much of the earliest evidence for the occurrence of pesti-

cides in the environment and their concentration in the food chain came, rather accidentally, from basic research in oceanography. It would be going too far to say that the pesticide problem would never have come to light without this basic research, but it certainly did do a good deal to focus applied research attention on the issue.

Research can be looked upon as a sort of search strategy. Basic research often provides the clues from which the search strategy in applied research can be narrowed and focused. Thus, through the use of the corpus of available scientific understanding, past and contemporary, the search strategies employed in applied research and technological development are much more efficient and economical in effort and money. The body of scientific knowledge may seldom produce the precise information needed to launch a desired application, but it provides the clues that tell us where to start looking. Mathematical theories of search strategies tell us that even minor deviations from a random search at the beginning can enormously increase the rate of convergence of a search process. But this sort of effect of basic science cannot show up in efforts to trace the origins of technological development in discrete scientific "events," as the authors of Project Hindsight for the Department of Defense attempted to do (*29*). Most of the "events" identified in Hindsight were technological rather than scientific. This result was probably to be expected, but really proves nothing about the relevance or importance of basic research. Such studies can never reveal the blind alleys that were not pursued because of judgment based on available scientific knowledge. It is important to observe that the knowledge required for the assessment of technology and for judgment regarding strategy of development is usually much more basic than the knowledge required to invent specific solutions to technological problems. Even this, of course, is not simple since there are also examples in which contemporary scientific perspective has delayed or discouraged radical technological innovations.

An interesting example of the unexpected relevance of basic research recently came to my attention in connection with optical astronomy. This example is still highly speculative, and there may turn out to be nothing to it, but it still illustrates my argument even if this particular case does not turn out as expected. A group of astronomers and atmospheric scientists at the University of Washington conceived the idea of reexamining old spectroscopic plates used in connection with spectrographic analysis of stars. Astronomers are well aware that this analysis is plagued with "contaminating" absorption lines due to absorption by molecules and atoms in the atmosphere through which the light reaches the telescope. Substances present only in trace amounts still have significant effects, especially when very faint astronomical objects are being studied. Great effort has been devoted to making elaborate corrections for this contamination, but nobody had previously tumbled to the practical significance of the contaminating lines themselves. Now it seems possible that this may be a powerful new tool for studying minor atmospheric constituents, both gaseous and particulate. Furthermore, the plates have been stored back 50 years, and thus can provide a baseline for observation of secular changes in the composition of the atmosphere as a result of man's activities, especially in plates taken with telescopes located near urban areas (*30*). The scientists concerned have even proposed the construction of new telescopes near urban areas to deliberately monitor atmospheric pollutants on a remote and continuous basis. All this represents a result which could never have been planned. Now that we are suddenly realizing what man might be doing to the atmosphere, we cannot decide to go back and measure the atmosphere as it was 50 years ago. We have to make use of the information, largely unpublished, developed as a result of basic research done for a purpose which appeared to have no practical relevance whatsoever.

5) Basic science provides a source of intellectual standards and "taste" for applied work. This is a point which has been particularly emphasized by Weinberg (*31*). The intellectual issues posed in basic work are often simpler and hence sharper than in applied work which usually has to deal with complex or "messy" systems. Nevertheless, the standards of judgment are often transferable from the simpler cases. This is why great importance attaches to the overlap in personnel and communications between the basic and applied communities, an overlap which has been particularly strong in the United States and which probably accounts in part for our success in exploiting the results of basic research even when the initial discoveries were made in other countries.

Another aspect of the standards provided by basic research is related to the problem of objectivity. Because technology inherently has a purpose, it also frequently has a built-in tendency toward wishful thinking, and toward subconsciously overlooking awkward or inconvenient facts which might throw doubt on the validity of a technological goal on which great energy and effort have been lavished. This may apply particularly to the secondary consequences of technology. It is notorious that chemists from the chemical industry and engineers from the auto industry were slow to accept the evidence of environmental pollution resulting from their products. This was not out of greed, or the profit motive, or even "just obeying orders from above," as some now seem to be asserting. The skepticism was usually quite sincere and consciously disinterested even when subconsciously biased. It is very difficult for anyone to accept criticism of his brain children, and this may be particularly so in the case of engineers or technologists who may have committed a large slice of their careers to a single goal. The standards and traditions of basic research provide an "ethic" which helps scientists to be more objective in such matters. It is also true that the more open communications system of basic science, and its hospitality to criticism from all qualified quarters, tends to make science a natural source of critique for technology. The communications system in technology is less public, more based on personal contacts, and the engineer is judged by the object which he designs, whereas the scientist is judged by his contribution to truth. This is perhaps why, during the 1950's and early 1960's nuclear physicists were often used as critics and evaluators of military technology on government committees. On the one hand they were generally familiar with the theory and concepts needed to master the details of military weapons, but on the other hand they had no personal commitments to the success of these projects. In short, a scientific climate produces a general environment which is more objective and critical about technology.

6) Finally, one cannot discuss the impact of science without referring to its general cultural effects. A strong science propagates and popularizes the

belief that the world is intelligible and hence ultimately subject to man's control (not to be confused with unlimited exploitation). This belief in intelligibility extends to human societies as well as to the natural world. Such an attitude is essential to general political acceptance of the rational management of technology.

Of course there are dangers, often arising from variations on the aphorism that "a little knowledge is a dangerous thing." In the past, misleading analogies with scientific laws have been exploited to justify or rationalize harsh social arrangements, as in the theory of "social Darwinism" and the rationalization of unbridled laissez faire at the end of the 19th century. The law of supply and demand was enthroned like Newton's law of universal gravitation, and used to preclude any form of collective intervention in the "natural" workings of the economic system, which neglected the fact that this system was a human construct, with ground rules built in by man-made laws and political actions, not facts of nature.

Another cultural effect of science is as a model of progress. It is the one area of human activity which can incontrovertibly be said to progress, not only despite, but because of, the fact that the definition of progress, unlike that in other areas, is not anthropocentric. In every other area the notion of progress is subject to question because it depends on the values against which the change in the human situation is to be measured. Thus, we cannot agree as to whether advancing technology constitutes progress, but in science knowledge and understanding do grow cumulatively independently of how they may subsequently be used.

But we do know that models and metaphors are important in setting human goals, and the mere existence of basic science as a social subsystem which progresses cumulatively provides a kind of intimation of the possibility of progress in other areas. Perhaps this is what C. P. Snow had in mind when he spoke of scientists as constituting the most "future oriented" segment of society.

Conclusion

A recent newspaper account of the 1970 annual meeting of the AAAS was headlined, "Science's Blank Check Bounces." I am not, however, advocating that giving a "blank check" to science will solve all our problems. The discussion of science policy in the last three decades has too often confused necessary with sufficient conditions. A strong basic science is a necessary condition for a strong economy, a livable environment, and a tolerable society. But it is by no means a sufficient condition. That a vital science is an indispensable tool of human welfare in the present stage of evolution of man on the planet does not mean that it is the only tool or that it cannot also produce the opposite. Indeed, there seems almost to be a complementarity between the power for good and the power for evil inherent in science. Nuclear energy poses the possibility of nuclear holocaust, but is indispensable to a continuing supply of energy after fossil fuels run out. The computer threatens us with "big brother," but seems indispensable to the rational management of our complex social structures. Molecular genetics could be used for frightful purposes, but opens up the prospect of the final conquest of human disease and food supply. Drugs which control human behavior have opened up frightful possibilities for abuse and self-destruction, but they also offer the hope of conquest of mental illness. What I have referred to are really technologies, not science, but science is needed to use them wisely, although it will not guarantee their wise use.

Although science cannot ask for a blank check, there is a part of it which must have the autonomy to "do its own thing" if it is to continue to serve society. How much of science should have this autonomy, and what sort of accountability should be required of it will be matters of continuing debate. Some accountability outside the scientific system itself is essential, as in any other human activity, but the degree of external accountability which is necessary will depend also on the success with which science maintains its own system of internal accountability, guaranteeing the intellectual excellence and integrity of its results. Although I do not believe scientists can be held accountable for the uses which society makes of the knowledge they produce, they do have an obligation to make clear the implications of this knowledge insofar as it is within their special intellectual competence to do so. However, I believe that the highest allegiance of science must continue to be to truth as defined by the validation procedures of the scientific process itself, and that the distortion of scientific results or the selective use of evidence for political purposes, no matter how worthy, is unforgivable insofar as it is presented cloaked by the authority and imputed objectivity of science.

That science should have a measure of autonomy does not mean it cannot also respond to new social priorities. As in the past, new social missions can open up exciting new scientific questions, as fundamental as any generated by the internal workings of science. However, what is important is that no matter how much the broad strategy of science might be influenced by social priorities, the tactics should be largely governed by scientific criteria. Furthermore, it is essential that some science be supported and cultivated for its own sake alone. Here the primary criterion must be excellence as judged scientifically, that is, by internal standards. The fraction of the total technical effort that is supported in this way should have some degree of constancy over the long term.

You are no doubt wondering what is the answer to the question posed by the title of this article. I cannot give a definite answer one way or the other. The threats to the integrity of science, both from within and from without, are probably greater than at any time in the past, because science is much more a part of the total social and political process, no longer the semi-hobby of a few dedicated and somewhat eccentric individuals. But I am an optimist. I do not think that the scientific enterprise is going down the drain. It will change, as science has always changed. It will respond to new social priorities, but, like an organism responding to disease, it will develop antibodies which will fight and finally contain excessive control by external criteria, and in fact will transform these external pressures into new opportunities and new fundamental fields of inquiry. But I could be wrong!

References and Notes

1. C. P. Haskins, *Foreign Affairs* **49**, 237 (1971).
2. M. Purver, *The Royal Society: Concept and Creation* (M.I.T. Press, Cambridge, 1967). According to Purver, "The Royal Society claimed to be an innovation without precedent in the history of science, and insisted on its uniqueness in no uncertain terms" (p. 21) or, again, "it also claimed that as an innovator its importance to posterity was incalculab.e" (p. 21).
3. D. K. Price, *Science* **163**, 25 (1969).
4. A. H. Dupree, *Science in the Federal Government* (Harvard Univ. Press, Cambridge, 1957), pp. 23, 44 ff.
5. D. Schon, *Technology and Change* (Delacorte, New York, 1967), see especially chap. 8.

6. ———, *ibid.*, p. 190.
7. G. P. Marsh, *Man and Nature* (Scribner, New York, 1864).
8. For a discussion see H. H. Landsberg [*Daedalus* **96**, 1034 (1970)].
9. D. P. Moynihan, "Counsellor's Statement," in *Towards Balanced Growth: Quantity With Quality*, report of the National Goals Research Staff (The White House, Washington, D.C., 1970), p. 8.
10. H. Brooks, *Daedalus* **94**, 66 (1967).
11. A. Hacker, *End of the American Era* (Atheneum, New York, 1970), p. 146.
12. ———, *ibid.*, p. 142.
13. J. D. Watson, *The Double Helix* (Atheneum, New York, 1968).
14. D. Bell, "Notes on the Post Industrial Society (I)," *The Public Interest* (Winter 1967), pp. 24–35, quote from p. 28.
15. J. M. Ziman, *Public Knowledge, The Social Dimension of Science* (Cambridge Univ. Press, Cambridge, 1968).
16. A striking example of the identification of science with technology by the press is the following quotation from the *Wall Street Journal* (8 Jan. 1971), in an article otherwise generally friendly to science: "Politicians and pressure groups have been challenging, with notable effect, several projects that are in the vanguard of scientific (sic) development, such as supersonic transport, nuclear power, chemical and biological warfare, and space exploration."
17. V. Bush, *Pieces of the Action* (Morrow, New York, 1970), p. 65.
18. ———, *ibid.*, p. 64.
19. P. Drucker, "The New Markets and the New Capitalism," *The Public Interest* (Fall 1970), pp. 44–79, especially p. 66.
20. Quoted by W. Sullivan in the *New York Times*, 29 Dec. 1970.
21. M. Purver, *The Royal Society: Concept and Creation* (M.I.T. Press, Cambridge, 1967), p. 51.
22. B. Flowers, "Science in Universities," public lecture delivered at Nottingham University, Nottingham, England, 6 March 1970.
23. B. Barber, *Science and the Social Order* (Collier Books, New York, rev. ed., 1962), p. 139.
24. For a dramatic recent example, see M. Harris [*Science* **170**, 1068 (1970)].
25. E. P. Wigner, "The Unreasonable Effectiveness of Mathematics in the Natural Sciences," in *The Spirit and Uses of the Mathematical Sciences*, T. L. Saaty and F. J. Weyl, Eds. (McGraw-Hill, New York, 1969), pp. 123–140.
26. See *Biology and the Future of Man*, Philip Handler, Ed. (Oxford Univ. Press, New York, 1970); also, *The Life Sciences* (National Academy of Sciences, Washington, D.C., 1970), especially chap. 3.
27. American Chemical Society, *Cleaning Our Environment, The Chemical Basis for Action*, (Washington, D.C., 1969), p. 203.
28. ———, *ibid.*, p. 84.
29. C. Sherwin and R. Isenson, "First Interim Report on Project Hindsight (Summary)" (Office of the Director of Defense Research and Engineering, Washington, D.C., 30 June 1966).
30. P. W. Hodge, *Nature* **229**, 549 (1971); "ASTRA Project Monitors Atmospheric Pollution" *Physics Today* **24**, No. 1, 20 (1971).
31. A. M. Weinberg, *Bull. Atomic Sci.* **22**, 8 (1966).

Change in Argonne National Laboratory: A Case Study

The impact of altered management and objectives transform an AEC national laboratory.

Ann Mozley

For many years the American research and development community has been the envy of the world. Overseas researchers have flocked to it; presidents of all persuasions have endorsed it; and, most recently, President Nixon, noting the need to maintain the country's scientific leadership against challenging competition from abroad, reminded his electorate that "We support a strong program of research in the sciences with protection for the independence and integrity of participating individuals and institutions" (*1*, p. 145). The retreat from these goals and the crisis engendered in the scientific and engineering community by severe cuts in national funding have become the study of administrators, scientists, politicians, and sociologists. Their inquiries have been directed largely to the general climate of change (*2*). My study is an attempt to examine one major national scientific institution, the Argonne National Laboratory, Argonne, Illinois, in a context of organizational and national change and, from a review of its historical and contemporary situation, to offer a case study of the effects of altered administrative and conceptual objectives on a specific community of science.

The background data for the study was drawn from federal government reports and papers, annual and special reports from the Argonne Laboratory, and contractual and other documentary sources. The greater part of the evidence, however, was collected over a period of several months during 1970 and 1971 from informal interviews with personnel at Argonne—from division directors and associate directors, senior and associate scientists, administrators, short- and long-term scientists and engineers, to the Laboratory director, two former Laboratory directors, the vice president for programs and projects of the University of Chicago, and the president of the Argonne Universities Association. I thank all of them for their forthright cooperation.

Origins and Early History

Argonne, a multiprogram national laboratory of the Atomic Energy Commission (*3*) for the pursuit of peaceful uses of atomic power, grew directly out of the wartime Metallurgical Laboratory of the Manhattan Engineer District based at the University of Chicago from January 1942. The first successful, controlled self-sustaining nuclear chain reaction (carried out under the direction of Enrico Fermi on a squash court of the University) was achieved in December 1942, and work was set for the construction of nuclear reactors for the production of plutonium, the process of separation and isolation of plutonium, and for related research in physics, chemistry, metallurgy, and biology. During 1944, the first heavy water–moderated reactor was placed in operation at an early Argonne site in Cook County Forest Preserve. After the federal government's plan to establish the Atomic Energy Commission under the Atomic Energy Act of 1946, Argonne was selected to become a principal, permanent, national laboratory devoted to research in the long-range development of atomic power, and was formally constituted on 1 July 1946. By formal agreement with the government on 31 October 1946

The author is lecturer in social history at the New South Wales Institute of Technology, Sydney, Australia.