Despite all our research, a truthful response to questions such as "What's going to happen to the biosphere?" will most often be "We don't know, and we won't know." We go on to explore how, in the face of overwhelming ignorance, scientific inquiries in policy-related contexts can most responsibly and effectively be conducted. Better procedures for self-criticism and quality control in science are argued to be central to the construction of "usable ignorance." A key role is also assigned to the design of approaches through which incomplete science can be better integrated into policy debates.

Usable Knowledge, Usable Ignorance

Incomplete Science with Policy Implications

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For centuries the dominant theme of our science has been taken from Francis Bacon's aphorism "Knowledge and power meet in one." I need not relate here the transformation of humanity's material culture that science has brought about, nor the enhancement of human life, social, moral, and spiritual, that this has enabled through the conquest of the traditional curse of poverty (in at least the more fortunate parts of the world). But now we face a new, unprecedented problem: Along with its great promises, science (mainly through high technology) now presents grave threats. We all know about nuclear (and also chemical and biological) weapons, and about the menaces of acid rain, toxic wastes, the greenhouse effect, and perhaps also the reemergence of hostile species, artificially selected for virulence by our imprudent use of drugs

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and pesticides. It would be comforting to believe that each problem could be solved by a combination of more scientific research of the appropriate sort, together with more good will and determination in the political and technological spheres. Doubtless, these are necessary, but the question remains: Are they sufficient? The record of the first round of an engagement with these biospheric threats is not encouraging. For example, we do not yet know when, how, or even whether global temperatures will be influenced by the new substances being added to the atmosphere. This is why, we believe, a novel approach is called for if our science-based civilization is to solve these problems that are so largely of our own making.

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

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

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

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

What could such a new, appropriate sort of science be? Isn't science just science? In some ways, yes, but in others it is already differentiated. We are all familiar with the differences between pure or basic research on the one hand, and applied or R&D on the other. In spite of the many points of contact and overlap, they do have distinct functions, criteria of quality, social institutions, and etiquette and ethics. To try to run an industrial laboratory as if it were within the teaching and scholarship context of a university would be to invite a fiasco; and equally so in reverse. Now we face the task of creating a style of science appropriate to this novel and urgent task of coping with biospheric problems. Of course, there are many different institutions doing research with just this end in view. Sometimes they are successful, but success is more common when they have a problem where the conditions for success can be defined and met, and where the input from research is straightforward. To the extent that the problem becomes diffuse in its boundaries (geographically, or across effects and causes), entrained in crosscurrents of politics and special interests, and/or scientifically refractory, then traditional styles of research, either academic or industrial or any mix of the two, reveal their inadequacy.

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

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

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

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

Images of Science, Old and New

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

Insoluble Problems

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

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

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

Indeed, it is scarcely a decade since insoluble scientific problems have become "news that's fit to print." Alvin Weinberg (1972) brought them into recognition with the term "trans-science." Were these a new phenomenon of the troubled 1960s?—that period when environmentalists began to raise the impossible demand that science prove the impossibility of harm from any and all industrial processes and effluents. No, ever since the onset of the scientific resolution, science had been promising far more than it could deliver. Galileo's case for the Copernican Theory rested on his theory of the tides, where he contemptuously rejected the moon's influence and instead developed a mechanical model that was far beyond his powers to articulate or demonstrate. Descartes's laws of impact, fundamental for his system, were all wrong except in the trivial cases. The transformation of the techniques of manufacture, promised by every propagandist of the century, took many generations to materialize. In the applications of science, progress toward the solution of outstanding, pressing problems was leisurely; for example, the break-even point for medicine, when there came to be less risk in consultation a physician than in avoidance, seems to have occurred early in the present century.

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

"Atomic" Science

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

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

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

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

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

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

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

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

Quality Control in Science

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

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

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

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

Choice in Science

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

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

When we consider the criteria for choice governing mission-oriented projects, we find some components that are more or less internal to the process and others that are not. In the former category are feasibility and cost (this latter being measured against the demands of competing projects within some preassigned limited budget). For this we must take into account the aims or objectives of the project, which are necessarily exterior to it and different from the research itself, for they employ values.

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

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

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

Ignorance

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

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

Elements of a New Understanding

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

Science in the Policy Process

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

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

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

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

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

Interpenetrating Opposites in Science

Science in the policy process is thus a very different thing from the serene accumulation of positive and ultimately useful factual knowledge, as portrayed in our inherited image. Indeed, given the intrusion of subjective elements of judgments and choice into a sphere of practice traditionally *defined* by its objectivity, we may wonder whether there can be any endeavor describable as science in such circumstances. To this problem I can only begin to sketch a solution, by giving two analyses, one static and the other dynamic. The former elucidates the paradoxical, or contradictory, nature of our situation, and the latter indicates paths to resolution of the paradox.

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

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

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

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

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

Varieties of Policy-Related Research

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

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

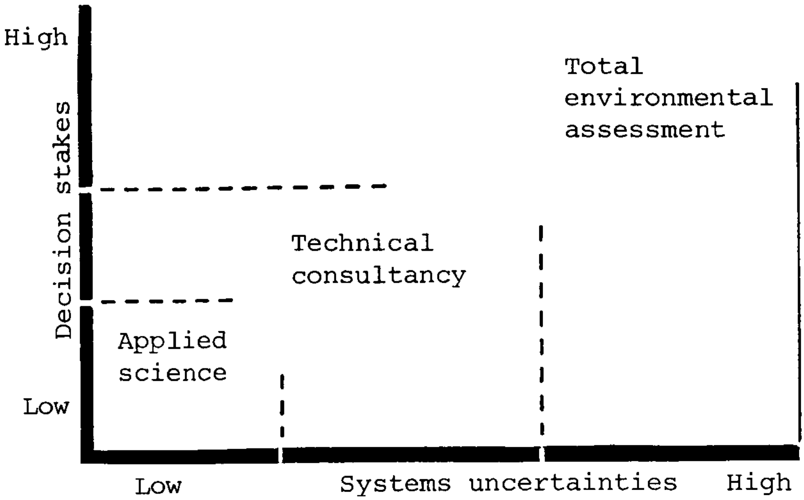

Figure 1: Interaction of Decision Stakes and Systems Uncertainties

power as a result. The public sees such struggles most clearly in notorious cases of pollution, when a beleaguered institution persists in harmful policies (such as poisoning its work force or the local environment), to the point of being irresponsible, immoral, or perhaps even culpable (industrial asbestosis is a notorious recent case in point). The outrage in such cases is fully justified, of course; but it is an error to believe either that those particular firms are uniquely malevolent, or that all firms casually and habitually behave in such a way. No, it is just when caught in such a trap, however much of their own making, that institutions, like people, will fight for survival.

Such cases are fortunately the exception. It is more common for both systems uncertainties and decision stakes to be moderate. Funtowicz and I have been able to articulate a model of consultancy practice, wherein the traditional scientist's ideal of consensual knowledge is sacrificed on behalf of a more robust sort of knowledge appropriate to the problem. We call it clinical, from the field of practice in which such a style has been developed successfully. In it we eliminate safety as an attribute (the term now has a largely rhetorical meaning anyway) and substitute good performance (which may include the possibility of failures and accidents). In the same vein, we generalize probability (with its mathematical connotations) to propensity, and measure to gauge;

and for prediction we substitute prognosis. In this way, we hope to express the degree to which nonquantifiable and even nonspecifiable expert judgments enter into an assessment. The outcome of the process (which is conceived as continuously iterating) is not a general theory to be tested against particular facts, but rather a provisional assessment of the health of a particular system together with the relevant aspects of its environment. I hope that this model will be useful in the biosphere project as it develops.

Passing to the more intractable case, where either dimension is very large, we have what we call a total environmental assessment. For here, nothing is certain, there are no boundaries or accepted methods for solving problems; the problem is total in extent, involving facts, interests, values, and even lifestyles, and total in its mixture of dimensions and components. Even here a review of history shows that in such cases a resolution can emerge. For a debate ensues, once an issue is salient; and while at first the debate may be totally polarized and adversarial in style, it may evolve fairly quickly. For both sides are attempting to gain legitimacy with the various foci of opinion, which ultimately represent power: special-interest groups, administrators, politicians, the media, respondents in opinion polls, voters. They therefore necessarily invoke the symbols of universality and rationality whereby uncommitted observers can be won over; and in however oblique and implicit a fashion, a genuine dialogue emerges. Most important in this process, new relevant knowledge is created by the requirements of the various disputants, so that the issue is brought in the direction of technical consultancy, if not yet science. For example, issue-generated research can eventually transform the terms of a debate, as in the case of lead in automobile fuel in Britain and Europe during the early 1980s. Events that previously had not been significant news suddenly became so: thus the various nuclear accidents of the 1950s and 1960s were of no great moment for policy purposes, while Three Mile Island was a mortal blow to the American nuclear power construction industry. Hence a problem does evolve; a dominant consensus can emerge; and then the losing side is forced into a retreat, saying what it can while the facts as they emerge tip the balance ever more decisively against it.

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

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

The Policy Process and Usable Knowledge

Now I discuss the policy process itself, in relation to these phenomena of the interpenetration of facts and values and of knowledge and ignorance. This is not the place to develop schematic models of that process, so I will content myself with a few observations. The first is that no decision is atomic. Even if an issue is novel, even if its sponsoring agency is freshly created, there will always exist a background, in explicit law, codes of practice, folkways, and expectations, in which it necessarily operates even while reacting on the background. And once an issue exists, it is rare indeed for it to fade away. It may become less salient for policy and be relegated to a routine monitoring activity; but it can erupt at anytime should something extraordinary occur.

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

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

Passing to the more contested issues, we mention enlightenment, which might involve enhancing awareness or changing common sense. Perhaps the most notable example of this sort of product in recent times is *Silent Spring* by Rachel Carson (1962). Through it, the environment and its problems suddenly came into existence for the public in the United States and elsewhere. We note that this function is performed partly through the mass media; the role of investigative journalism in the press, and especially television, in enhancing the awareness of the nonscientific public (and perhaps of scientists, too) should be more appreciated.

Once an issue has been made salient for the political process, then science can be a complement to interaction—that is, not being decisive in itself in any unreflective way, but correcting common-sense views, and providing crucial inputs when a debate is sharpened. To take an example from another field, the regulation of planned interference with the life cycle of embryo and fetus will not be reduced to the scientific determination of the onset of life and individuality. But, just as technical progress creates new problems of decision and regulation, scientific information can provide channels and critical points for the ethical and ideological debates on such issues.

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

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

Toward a Practical Approach

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

Scientific Quality— A Many-Splendored Thing

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

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

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

TABLE 1
Critical Criteria

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

This new and important skill has been called (by Bill Clark) "a critical connoisseurship of quality in science." One does not merely apply one's own specialist criteria blindly or unselfconsciously, however excellent or valid they may be for one's own scientific expertise or role. One must be able to assess productions from several points of view in succession, by means of an imaginative sympathy that involves seeing one's own role, one's own self, from a slight distance. It may be that I am here calling for the cultivation of attitudes proper to literary criticism, a prospect that to some may be even more alien than Zen riddles. But given the complexity of policy-related science, in response to the complexity of biospheric problems, I can envisage no easier alternative.

Usable Ignorance

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

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

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

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

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

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

Recognition of the need for monitoring entails that the decision process be iterative, responding in a feedback loop to signals from the total environment of the operating system. Also, the inclusion of ignorance in decision making via the explicit assignment of burden of proof, involves a self-conscious operation of dialogue at several levels, the methodological and regulative simultaneously with the substantive. All this is very complicated, of course, and the transaction costs of running such a system might appear to be very high, not least in the absorption of time and energy of highly qualified people. But if those costs become a recognized element of the feasibility of a project, let it be so; better to anticipate that aspect of coping with ignorance than either to become bogged down in endless regulator games, or to regress to a simplistic fantasy of heroic-scale technological innovation, thereby inviting a debacle sooner or later.

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

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

Conditions for Success

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

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

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

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

Conclusion and Perspective

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

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

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

The idea of using such applicable science as a significant contribution to the planned development of the means of production was first articulated in the socialist nations, and popularized everywhere by the prophetic writings of J. D. Bernal. It lost its ideological overtones during the Second World War, and now that planning is an essential tool even in the market-economy nations, science as "the second derivative of production" (in Bernal's phrase) is a commonplace (Ravetz, 1974). Even academic research is now strongly guided by priorities, set in the political process, and related to the requirements of the development of the means of production and of destruction. Boris Hessen's classic thesis on *The Social and Economic Roots of Newton's*

Principia may have been crude and over-simple for the seventeenth century, but for the twentieth it is a truism. There still remains a difference in slogans—in the socialist countries it is "the scientific-technological revolution," in the others it is "don't come last in the microelectronics race"—and only time will tell how these will work out in practice.

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

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