Threats and Promises: Negotiating the Control of Research

In August, 1976, a citizen's review board was appointed in Cambridge, Massachusetts, to consider whether recombinant DNA research would have an adverse effect on public health. The board, composed of nonscientists, described itself as a "citizen's court." Its task was to review the safety procedures set out by guidelines from the National Institutes of Health (NIH) and the methods for monitoring their compliance. Evaluating conflicting arguments offered by scientists for and against the facility, the board presented its findings to the City Council of Cambridge as a basis for local policies governing the conditions of research.

During the same month, a presidential task force proposed an experimental science court to help resolve the factual dimensions of scientific disputes. Like the Cambridge group, the Science Court would arrange debates among opposing scientists, and evaluate their arguments on controversial technical issues. But, unlike the citizen's review board, it would be scientists, not laymen, who would make the judgments to serve as a basis for policy.

The assumptions behind these two efforts were similar; both the Cambridge Experimentation Review Board and the Science Court were intended to provide a rational basis for policy choices in troublesome areas of science and technology; both involved elaborate procedures to accumulate a range of conflicting viewpoints on questions of risk. Yet each represents a different approach to the resolution of disputes. Proponents of the Science Court assume that a major source of controversy lies in disagreement about the nature of evidence. They suggest that neutral judgments by scientists who can distinguish facts from values in controversial areas will help to resolve disputes. The Cambridge Review Board, assuming an inextricable relationship between factual disagreement and value conflicts, argued that "decisions regarding the appropriate course between the risks and benefits of potentially dangerous scientific inquiry must not be adjudicated within the inner circles of the scientific establishment." The contrast between these approaches highlights a crucial question: who should ultimately be responsible for decisions about controversial scientific and technical issues? In the recombinant DNA controversy, this question bears directly on the issue of freedom of inquiry as increasing demands for public control conflict with expectations of scientific autonomy.

The scientific community has persistently resisted public control. Only when research has direct technological applications are scientists willing to con-
cede the need for regulation. In a survey of 800 scientists, seventy-seven percent agreed that “The pursuit of science is best organized when as much freedom as possible is granted to all scientists.” This study cited some characteristic attitudes: “A pure scientist must not deny himself a discovery by worrying about social consequences.” “I would insist that no area of investigation be closed because someone feels that society is incapable of handling it.”

Wide consensus on the importance of autonomy has meant significant federal patronage of science with minimal public control. Even as political controversy has constrained technology, control over research has largely remained within the scientific community. More and more, however, concerns about technology are spreading to science. Arguing that basic research as well as its application may have undesirable environmental or social consequences, critics call for greater public scrutiny and sometimes control over research. This prospect is especially problematic for a community organized as a self-governing social system.

What has led to these demands for public involvement in decisions about research? What forms can such public participation take? And what are the implications of these demands for science? The recombinant DNA dispute casts some light on these questions as its participants grope for solutions to a difficult set of problems. But we must begin by briefly framing the case in the context of the present relationship between science and the political community with respect to the notion of freedom of inquiry.

The idea of constraining research calls forth accusations of “McCarthyism,” “Lysenkoism,” “the Scopes Trial,” “the Inquisition.” Many scientists feel that freedom of scientific inquiry is a constitutional “right”—like freedom of speech. “The accepted position is and should be jealously to guard the constitutionally guaranteed freedoms, both those expressed and those implied.”

The concept of freedom of inquiry has a venerable history and is widely taken for granted, but there is no constitutionally guaranteed right to pursue knowledge or to engage in scientific inquiry. Rather, it is more accurate to view freedom of scientific inquiry as the product of continued negotiations. An implicit compact emerged after World War II from the debates over postwar research policy. These debates first focused on the continued role of the military in the management of research as scientists bargained for less restrictive civilian control. Then, Senator Harley Kilgore, concerned with stabilizing the wartime productivity of science, proposed establishing a federal superagency to coordinate scientific work and to finance academic research. Many scientists objected, fearing that federal subsidy would threaten scientific freedom and prolong the wartime pattern of centrally directed research. Especially problematic was Kilgore’s notion that the agency should be run by laymen whose appointments could hinge on political criteria. Backed by professional societies, Vannevar Bush negotiated for an agency run by scientists themselves. The key and controversial point in this negotiation was the extent of political control to be associated with federal patronage of basic research. Like Kilgore, scientific leaders wanted stable funding, but only with safeguards against political interference. The negotiation thus hinged on questions of professional rather than lay control and on the degree of direct accountability to the political system and to the president. Truman favored Kilgore’s more politically responsive scheme, and it was not until 1950 that Congress finally approved the Bush proposal, insulating the National Sci-
ence Foundation from direct political control and thereby establishing the basis for a policy that provided federal subsidy for science with minimal public intervention. Science, claims one observer, is "the only institution for which tax funds are appropriated almost on faith, and under concordats which protect the autonomy . . . of the laboratory." This postwar contract was indeed unusual in its provisions for scientific autonomy and it has been subject to continued erosion precisely at the original points of contention, that is, over the role of laymen in governing the direction of research, the scope for political control, and the extent of accountability to the political system. Nixon's change of mind with respect to appointment of Franklin Long to the National Science Foundation directorship; the proposed Bauman Amendment to the NSF Appropriations Act, seeking congressional control over research projects; the Proxmire "Golden Fleece" awards; the increasing public controls over research methods that involve human subjects—all suggest the fragile character of the relationship. As a symbol of the growing tension between science and the political community, the recombinant DNA controversy has further eroded the contract and brought pressures to renegotiate the relationship and especially the terms of scientific autonomy.

A negotiated agreement has been thoughtfully defined as an exchange of conditional promises and threats. The scientific community has, in essence, bargained for substantial autonomy by claiming the inherent efficiency and, indeed, necessity of an unregulated scientific enterprise, and by promising practical contributions to economic progress in return for funding without intervention. Its bargaining strength comes from belief in the intrinsic value of knowledge, and from the promise of its contribution to the public good. Underlying the negotiation is the implicit threat that society will lose out on the benefits of science if excessive intervention accompanies government support. If the acquisition of basic knowledge is restrained by externally imposed limits, it is society which will bear the costs.

Negotiated agreements are maintained only if both parties have something to exchange that is valued. As long as the products of research were thought to be unequivocally desirable, the premises of negotiation remained stable. However, in the context of growing concern about the risks of research, attitudes have changed, bringing pressures for greater public control.

These pressures reflect a broader ambivalence about the value of science and technology and the legitimacy of decision-making authority. Technical decisions in many areas are confronting public scepticism, expressed in legislative or administrative controls, in legal action, and sometimes in overt protest. Former assumptions about "professional accountability" are increasingly translated into demands for direct "participation." It will be useful for our discussion of the negotiation over recombinant DNA to distinguish these two concepts, for the notion of accountability traditionally does not imply participation. Professional groups, for example, are answerable to the public and expected to account for their actions, but they regulate their own activities. Self-regulation, however, depends on public trust; it is the declining confidence in the adequacy of accountability procedures that inevitably leads to the concern about participation.

It is also useful to distinguish among various concepts of participation. The prevailing assumption is that citizens participate by choosing those they
wish to represent them in government. Direct participation in policy-making is
limited, and representatives are accountable mainly through periodic elections.
Alternatively, it is argued that direct participation by affected interests is neces-
sary in a democratic society. Awareness of the negative effects of certain poli-
cies, and a general declining trust in authority, have triggered interest in more
active and direct modes of participation. During the negotiations over recombi-
nant DNA research, we shall see both concepts of participation exercised as
elected representatives grope for ways to reinforce their legitimacy and compet-
tence to deal with a new set of issues, and citizen groups seek more direct in-
volved.

A final concept in need of clarification is that of regulation. Regulation oc-
curs typically either through economic incentives (funding to encourage useful
activities or penalties to discourage harmful practices), or through direct inter-
vention (setting standards or prohibitions). Scientists, like all groups, seek to
control the regulatory process, arguing that this is in the public interest. Their
ability to do so again rests on trust. Indeed, trust is the key variable in the
evolution of the recombinant DNA dispute as declining confidence in the ade-
quacy of conventional self-regulatory procedures in science has led to greater
restrictive regulation and demands for more direct public involvement.

I will examine these changing patterns in the negotiation over the autonomy
of science. Further, I will suggest the problems of accommodating the trend
towards greater public control, drawing from aspects of the recombinant DNA
dispute.14

*Negotiating Premises*

Negotiating relationships depend on the attitudes and expectations that are
brought to the bargaining process. That a laboratory research technique has
provoked such broad political interest reflects changing attitudes about both the
scientific enterprise and the appropriate locus of decision-making authority.

*CHANGING IMAGES OF SCIENCE*

Hecklers at the National Academy of Sciences forum on recombinant DNA
opened the meeting singing "We shall not be cloned . . ." They waved flags
reading "Don't tread on my genes" and "We will create a perfect race." They
questioned the desirability of seeking knowledge with possibly harmful appli-
cations and the morality of employing research procedures that pose potential
risks. Techniques of isolating and recombining fragments of DNA molecules
have opened the possibility of greatly expanding the understanding of genetic
inheritance. This knowledge could generate significant advances in the treat-
ment of disease, but it also removes some of the obstacles to genetic engineering.
It is this possibility, with its value laden implications, that accounts for the very
emotional reaction to recombinant DNA research.

Realistic or not, genetic manipulation is an overriding concern for the most
outspoken critics of this research. Although many of them are scientists them-
selves, they are absorbed in the religious and moral implications of a technique
that they perceive as allowing the creation and control of new life forms: "Once
the genie is out of the bottle, who can control its application?" "Scientists hold
our genetic future in their hands.” In the long tradition of intellectual criticism of science, these critics are convinced that science is distorting human values. They use anxiety-provoking images of Frankenstein and Faust. They warn of terrorism and of social corruption through the use of genetic information to justify present inequalities: “Is biology a social weapon?” “Can future generations cope with the possibilities science opens up today?”

The recombinant DNA debate has brought to a focus questions that are raised whenever connections are drawn between genetically mediated characteristics and human behavior. Research on the relationship between race and IQ, or between the incidence of an XYY genotype and criminal behavior, or on the genetic basis of alcoholism or addiction are bound to be controversial. Do we really want to know the genetic basis of intelligence or of behavior? Would this not allow the development of pernicious mechanisms of social control? If one identified XYY individuals or related IQ with race, would not this result in labeling and social stigmatization only to create the very behavior anticipated? Moreover, do we want the ability to control human qualities—to specify physical or mental characteristics? Anxiety about possible applications of research findings leads to questions about research itself. Ironically, it is the success of science, the expectation that knowledge is a compelling basis for social policy that encourages such concerns.

The long-term concerns about the applications of research brought emotional intensity to the recombinant DNA controversy. But the dispute was actually triggered by another issue; the fear of risks inherent in the techniques of research. This fear is not confined to the biomedical context; one might recall, for example, the community opposition to reactor research at Columbia University in the late 1950s.

In the case of recombinant DNA, it was feared that an experiment could inadvertently create genetic changes in known pathogens, or produce novel and dangerous forms of infectious microorganisms for which people have no resistance and medical science no cure. This possibility, if true, could be catastrophic. There was serious concern that risks were enhanced by the prevailing use of E. coli—a common bacterium that resides in the human digestive system—as a host for recombinant genes, although the common practice was to use enfeebled strains of E. coli that lack the capacity to colonize the human gut. The widespread research application of the technique because of the relatively low cost was expected to increase risks. Such fears have been exacerbated by the invisible nature of the risk. How does one know if a lethal gene is produced? It could take several years and the wide introduction of a dangerous gene in the population before problems were detected. Even as increased evidence has shown that accidents are unlikely, fears persist.

One biologist has speculated that the technique may also lead to violations of “genetic barriers” with profound and irreversible impact on natural evolutionary boundaries. Critics point out that the record of containment of biohazards is not flawless even under the most rigorous procedures. They emphasize the magnitude of uncertainty, the “total ignorance” about novel, self-perpetuating organisms.

Some prominent scientists, including James Watson, argue that these concerns are ill-founded. However, in a society deluged with warnings about risks from cyclamates, PCB’s, freon, and nuclear power, such fears are inevitable.
"We must control all these experimental procedures that are taking place and the effects that are coming out of sardine cans and tuna fish cans and even the milk you drink . . . I don't like to be contaminated," asserted one public official.  

Realistic or not, as the image of science changes, the case for unfettered research loses negotiating strength. For critics are increasingly asking questions about research that follow more from its external impacts than its internal dynamics. Does one place priority on what can be learned from a research project or on its possible negative social consequences? If risks and benefits are both hypothetical, which is to receive greater emphasis? But most crucial to the negotiation, if there is a conflict between scientific interests and public concerns, does one rely on professionals to assess and control risks in their own research or on those who may be affected?

CHANGING IMAGES OF AUTHORITY

Assumptions about the relative importance of technical competence as a criterion for legitimate decision-making authority are changing, as diverse groups claim social or political competence to evaluate the significance and acceptability of risk, and to decide on policies long regarded as technical and in the province of scientific expertise. The actors in the recombinant DNA dispute include several interests, each with different claims to legitimate decision-making competence based on their role as experts, as citizens, as affected interests, or as officials responsible for the public welfare.

Many laboratory researchers argue that scientific expertise is necessary and sufficient to assess problems of safety. Thus they seek to proceed with research under the old rules, relying on the judgment of their peers and resenting any external regulation as an intrusion on their autonomy. However, a group of molecular biologists at a Gordon Conference in 1973 expressed concern about potential biohazards in the technique of recombinant DNA research, and they proposed that the National Academy of Sciences and the National Institutes of Medicine evaluate the risks and take appropriate action. While these scientists acknowledged, and indeed called public attention to, the issue of biohazards, they too argued that expert competence is the basis of authority and that risks should be contained through procedures established and monitored by the scientific community. Thus, they supported the existing mechanisms of control that rely on self-regulation, seeking only procedural modifications.

A quite different perspective has been brought to the dispute by scientists representing such groups as Science for the People, and the Coalition for Responsible Genetic Research. Many of these scientists were politicized during the Vietnam War when they organized to oppose military research in universities. After the war the focus of their activity shifted to nuclear power and has now turned to genetic research. They start from a very different ideological base. Viewing the problem of recombinant DNA in political and moral terms, they feel that expertise is not a sufficient basis for authority. They question the ability of the scientific community to evaluate the risks of their own research and seek, not simply procedural adjustments, but basic systemic changes in the traditional organization of science and, indeed, of society.

Other actors in the recombinant DNA controversy include the many spokesmen for special interests, ranging from the pharmaceutical industry to
labor organizations concerned with occupational safety for technicians. As “affected interests” these groups also claim legitimate decision-making authority. Finally, public officials from federal bureaucracies, regulatory agencies, Congress, and state and local governments have perceived the issue as part of their responsibility to protect public health and safety.

Confronting such challenges, biologists have naturally sought to maintain as much autonomy as possible. But this has only reinforced their identification as a political interest group with powerful career concerns. Indeed a persistent theme throughout the dispute has been the conflict of interest implied by scientists regulating their own research. Metaphors have proliferated. To expect scientists to evaluate the risks in their own research is to ask “incendiaries to form their own fire brigade,” or “General Motors to regulate automobile safety,” or to ask, “Would the tobacco industry limit the manufacture of cigarettes?” These questions explicitly challenge the assumption that scientists can transcend their narrow private interests and career concerns in making decisions which affect the public welfare. Scientists, it is argued, lack the moral authority and legitimacy to regulate themselves when their work could shape the future of society. Direct societal intervention is necessary to assure that the public interest is properly served.19

The Negotiation Process

The changing images of science and appropriate decision-making authority have shaped the negotiation over recombinant DNA research, driving the issue to an increasingly public arena. In response to the concerns about biohazards expressed by biologists in 1973, the National Academy of Sciences formed an investigating committee which, in an unprecedented decision, called for a voluntary moratorium on any research that would improve the antibiotic resistance of bacteria and on any recombinations using tumorous or animal virus DNA. The committee recommended that the NIH develop a program to evaluate hazards, create guidelines for research, and organize an international meeting on the subject.20 That meeting was convened at the Asilomar Conference Center in California in February, 1975.

The Asilomar Conference was organized as a technical discussion of the scientific issues that defined biohazards and experimental safety.21 It was intended originally as a means of educating the international scientific community about the hazards of working with potentially pathogenic organisms. The organizers had invited a few members of the press, but were soon overwhelmed by the public’s extraordinary interest in their meeting, for many scientists did not regard the issue as a matter for public scrutiny at all. It was for scientists themselves to judge the extent of acceptable risk, and to take proper safeguards in the public interest. The important point for most scientists in this professional context was to manage the problem of risks so as to proceed vigorously with research that offered the promise of substantial benefits.

The meeting was motivated by concern that “If the collected wisdom of this group doesn’t result in recommendations, the recommendations may come from other groups less well qualified.”22 Thus, the participants agreed on interim guidelines based on the proposition that physical and biological containment must match the hazards involved, and sent recommendations to the NIH with
the expectation that their efforts would be acclaimed as a model of social responsibility. "You have seen scientists at work trying to do what they felt was right for the public, not for themselves," one scientist later testified.23

Nevertheless, Asilomar did not mark the end of controversy. The first to react was a group of scientists who had been concerned for some years with "the social responsibility of science."24 These activists questioned whether, in the climate of intense competition, one could trust scientists to comply voluntarily to recommendations that would constrain their work. Would it not be in the best interest of individuals to avoid compliance? Scientists might hesitate to call attention to problems that could bring external scrutiny. From their critical perspective, Asilomar appeared less a model of social responsibility than a means to protect vested interests.

The translation of the Asilomar recommendation into NIH guidelines met some but not all of this criticism. NIH has budgetary ties to Congress and responsibility to elected officials. Through its administration of research funds in the area of molecular biology, the agency represents an authoritative institution able to set standards for research to which grant recipients are accountable. Policy decisions in the NIH are set by a council which includes public representatives. However, NIH decisions about research rely on the peer review system, so that its role in setting guidelines exacerbated concerns about self-regulation and further polarized the scientific community. The first draft of the NIH guidelines, weaker than the Asilomar recommendations, was promptly attacked by critics as "a means to protect geneticists not the public."25 Deluged with letters, the NIH added stricter requirements, only to be criticized by traditionalists as "terribly responsive to outside lobbying rather than being objective and thinking about it."26 However, the new guidelines, describing requirements for physical containment during experiments and prohibiting any research endowing bacteria with toxins or antibiotic resistance, were released.27

By laying out containment requirements, the NIH established standards of protection, and criteria for accountability. The procedures used to develop the guidelines, however, presented little challenge to the pattern of self-regulation and thus intensified concern about decision-making authority. Although the NIH meetings were open to observers and the committee received public testimony, the guidelines were written by scientists involved in research and concerned primarily with providing for safety without limiting the range of inquiry.28 Some observers expressed doubts about the discretionary nature of the procedures: "It is extremely unlikely that Congress and the public would be willing to rely solely on the moral suasion engendered by the guidelines and the peer pressure that they could carry . . ."29 Others questioned how the NIH, with responsibility to promote research, could also regulate it. They argued that the measures of accountability were insufficient, for the primary responsibility to assess potential biohazards still rested with principal investigators or local biohazards committees composed mainly of professionals likely to concentrate on facilitating research.

If dissenters cannot win their battles at one level, they typically seek to broaden their constituency.30 Thus, opposing scientists sought a wider framework for negotiation through greater public involvement. The negotiation quickly moved beyond the scientific community to engage environmentalists,
The hearings, public petitioned national community proposed covered dangerous and controversial. But non-biased procedures. Senator Edward Kennedy proposed a new national commission with powers that would overlap those of the NIH by licensing facilities, inspecting laboratories, and monitoring compliance to regulations. Others opposed the proliferation of new federal agencies. Kennedy's bill would allow state and local government rulings to prevail despite federal legislation. Other proposed legislation based on the NIH guidelines would preempt local community rulings.

When local government officials became aware that research was planned in their communities, they too entered the dispute. In 1976, the Harvard University Committee on Research Policy approved plans to upgrade an old biology laboratory to meet the new NIH standards. However, alerted by a newspaper article and by local activists, the mayor of Cambridge, Alfred Vellucci, a long time antagonist of the academic establishment, brought the issue to the city council. In two tense days of public hearings the council heard testimony for and against Harvard's laboratory. Discussion focused on enforcement procedures. Those satisfied with the NIH guidelines emphasized the technical adequacy of the containment procedures; others claimed, "The guidelines in and of themselves are meaningless unless we have some police force, hopefully of a non-biased point of view, which has the interest not only of the lab technician but the general public in mind." With this in view, the government of a city which would bear the costs of research in case of accident, asserted its political authority to enter negotiations about research.

Mayor Vellucci's hearings were rather more colorful than the usual discourse about science, yet his perspective reflected the prevailing view of the role of local government: that public servants have the responsibility to intervene in matters affecting the health of a community. Vellucci reminded scientists of his obligation as a representative "to make sure nothing is being done in the private or public laboratories that may be injurious to the health of the people of the city." The city council had forbidden the construction of slaughterhouses as dangerous to the public health and the mayor suggested that the city should have the same say about a research laboratory: "Why should one separate one health hazard as against another health hazard?" Obliged to enter decisions affecting his constituency, he felt that many scientists deliberately obscured their activities with unnecessarily technical language. "Refrain from using the alpha-
bet,” he told scientists at the public hearing. “Most of us in this room including myself are lay people; we don’t understand your alphabet.” He complained about secrecy of decisions about research: “If it wasn’t for some of these newspapers we wouldn’t have known nothing about this stuff—we caught Harvard just in time.”

Finally, Vellucci reminded scientists that public officials have a right to intervene in activities supported by public funds. Reflecting a more widespread concern with accountability (think of Proxmire’s monthly “Golden Fleece” awards and the proposed Bauman Amendment) Vellucci asked “Who the hell do scientists think they are that they can take federal tax dollars and do research work that we cannot come in and question?” He offered a political definition of academic freedom. “They don’t pay taxes and so they are free taxable institutions and that’s all the freedom they are gonna get . . .”

The city council established a moratorium on research in Cambridge, and asked that a review board evaluate the adequacy of NIH procedures. The city manager selected people from a cross-section of the community; all were non-scientists with no connection to Harvard or MIT. They included a former mayor, a nurse, a community activist, a Tufts University professor of urban policy, a former city councillor, a physician, and a social worker. The review board was more a jury than a representative body; indeed, board members saw themselves as a citizen’s court. They met four to six hours a week for four months, and members claimed to put twice that time into “homework,” educating themselves on the technical issues by reading articles (an estimated twenty pounds of reprints) visiting laboratories, and hearing seventy-five hours of expert testimony. In a mock trial, they examined the views of opposing scientists. Although most participants began with the assumption that any suspicion of risk should preclude research, in the end all agreed that the research should continue. They proposed, however, additional monitoring procedures, including broader public representation in the university biohazards committees required by the NIH and a Cambridge biohazards committee that would oversee research in the city. In February, 1977, the city council supported the board’s recommendations, which then became an ordinance.

Coming to Terms

POLARIZATION AND MISTRUST

At least three levels of dialogue are woven into the dispute over recombinant DNA research: a technical discussion of safety, a philosophical discussion of values, and a political discussion of authority and trust. For most biologists, the issue is one of safety to be controlled by careful containment procedures; for critics, recombinant DNA is a sensational technology with ethical and social implications requiring greater public control. For many lay observers, this debate is a symbol of declining trust in science and its governing institutions to represent public values—“People don’t trust the authorities to run this thing; we have lost our capacity to trust.”

As the dispute evolved, drawing increased public attention, attitudes polarized nearly to a point of noncommunication. The very use of language reflected
the diverse perspectives of those engaged in the dispute. The scientists at Asilomar, calling for greater self-regulation, had initially talked of "recombinant DNA technology" and even "plasmid engineering." Later, when threatened with external control, their language shifted, and they referred to the research as "a science" directed to basic understanding of the human genotype. Critics, on the other hand, consistently called it a "technology." There were other linguistic manipulations. Several biologists suggested that the E. coli used for recombinant DNA research be given a new name so as not to confuse it with unattenuated strains. A scientist critical of the research objected to the use of the word "molecule" for DNA fragments since it implied chemical rather than biological manipulation.

Threats and promises escalated. Especially bitter about the schisms within the scientific community, biologists repeatedly emphasized the benefits of their work and warned that excessive public control would retard research at enormous public cost. "It will be contrary to the public interest if this should lead to a decision by the public to direct the scientific course of such investigations." The same scientists who opened the discussion about recombinant DNA at Asilomar had second thoughts as the issues became politicized. They perceive their critics as irrational and hysterical, committed to the destruction of science, or at best possessed of a Chicken Little mentality that amplified speculative risks into expectations of disaster. However, when biologists pointed to the limited possibility of an accident, their critics argued that the possible magnitude of an accident, no matter how unlikely, was an overriding consideration. When biologists claimed the benefits of genetic research for future medical technologies, critics responded that this claim was exaggerated because disease is related more to environmental than genetic factors. Most scientists felt that regulation, appropriate with respect to technology, should not be extended to basic research where future applications are unpredictable. However, their critics argued that concerns about technology inevitably bring into question the implications of knowledge itself, and that the moral implications of genetic research as well as immediate risks call for public intervention. Finally, the most profound disagreement centered on the extent to which scientists themselves can be trusted to evaluate the consequences of their research, and impose their own constraints.

THE PARTICIPATORY IMPULSE

For those who are unwilling to trust the authority of governing institutions or established administrative procedures, direct participation becomes a means to assure accountability. The NIH guidelines were not in themselves a sufficient guarantee that scientists would be accountable for the hazards of their research, so critics demanded procedures for more direct public involvement and local control. The mayor of Cambridge clearly took special pleasure in comparing Harvard to a slaughterhouse; but his perspective about the need for greater public involvement in science policy is more widely shared. The mayor of Ann Arbor, Michigan, argued, for example, "I will not abdicate my responsibility as mayor of the city to any federal, state, or private institution within our political jurisdiction. Just as scientists must reexamine the whole question of
academic freedom and right of inquiry, so must the various units of government reexamine their respective goals and relationships." The mayor went on to call for a greater public voice in monitoring and evaluating research with ethical or social implications.42

Similarly, an attorney, Harold Green, has argued that participation is needed "to stir up controversy," for scientists represent only a narrow spectrum of social values. Science policy, like tax policy or federal fiscal policy, he contends, should be subject to a democratic process including bruising political debate: "I do not see anything that is inherent in science that ought to distinguish it from any other aspect of our society in terms of the operation of the political process. Everything else is subject to the adversary process and debate, why not biomedicine?"43

To many scientists, such claims for broader participation and political debate are a challenge to science itself—a manifestation of growing antiscience sentiment. In fact, the Cambridge Review Board decision belies that assumption. Unlike many citizen actions, it did not bring a stalemate to the negotiation. The question of abandoning the research was not considered as board members explicitly decided to focus on the adequacy of containment procedures and the provisions for their implementation, rather than the ethical implications or potential future applications of the research. Although several members in fact were personally concerned about these broader ethical issues, they agreed to follow the instructions of the city manager and to limit their considerations to questions of safety. This decision suggests that these citizens shared the premises of the scientific community about the value of research. Rather than an antiscience demonstration, the participatory impulse in this case represented a search for ways to establish a more appropriate relationship between science and those affected by it. Essentially, the Cambridge Review Board was an experiment on how a lay citizen group could participate in decisions concerning science. The group wrote,

Knowledge whether for its own sake or for its potential benefits to humankind, cannot serve as a justification for introducing risks to the public unless an informed citizenry is willing to accept those risks . . . . we wish to express our sincere belief that a predominantly lay citizen group can face a technical scientific matter of general and deep public concern, educate itself appropriately to the task, and reach a fair decision.44

This participatory experiment reflects the changing political context of decision-making in other sectors. The recombinant DNA conflict is only one of the many controversies over science and technology. Similar disputes rage over the siting of nuclear power plants and the implementation of biomedical innovations, as well as over the methods of research. Many decisions once defined as technical have become the focus of adversary politics. In the 1960s, for example, a decision to build a power plant was a matter of closed negotiation between a utility, the industry, and its regulatory agencies. By 1970 changing values required greater public accountability in order to limit social and environmental impacts.45 Participatory procedures have been integrated into administrative processes; they have been institutionalized in sunshine laws, in provisions for public access to documents, and in strategies to involve the public
in decision-making. In lieu of trust in responsible authorities to act in the public interest, these procedures are an adaptive response to demands for accountability in an increasing number of policy areas.46

As demands for accountability descend on the scientific community, we find a similar groping for appropriate accommodation. Both the proposed Science Court and the Cambridge Experimentation Review Board can be viewed as efforts to win public acceptance of controversial decisions. Each has its own problems. The task of the Science Court would be to seek “neutral” scientific judgments about the factual dimensions of disputes, assuming that this neutrality would contribute to policy choices. However, this and similar proposals (for computer mediation,47 or a new profession of certified public scientists48) are simply elaborations of the self-regulating mechanisms of science. Supporters of such proposals assume, as did the Asilomar organizers, that conflict is an aberration, failing to recognize that basic value differences underlie disputes over the acceptability of risk. And they ignore the central concern with vested interests when scientists judge themselves.49

At the other extreme, the Cambridge Review Board and similar politically based groups are often composed entirely of laymen from the local communities where research is proposed. These groups may have mixed motives, for their deliberations may reflect past experiences (e.g., town–gown relationships) more than the problem at hand. With no scientific representation, they are unlikely to win the confidence of the scientific community unless, as in the Cambridge case, they make a decision compatible with scientific interests.

Other social inventions such as the human experimentation review boards required in institutions supported by HEW funds include both scientific and lay representation. A survey of these institutional review boards found substantial differences between the perspective of the laymen and the scientists who were participating.50 The scientists emphasized the medical contributions of research, and defined their purpose as weighing the protection of human subjects against the need to develop new knowledge (implying that greater risks can be tolerated in high priority research). The first concern of lay participants was the adequacy of informed consent, and they perceived the purpose of the review procedure to be the protection of the subjects. Although lay members on the boards were generally less active than scientists, and the boards were relatively uncoercive, university investigators by no means fully accepted this mechanism of accountability, in large part because of the lay involvement. Nearly half of those interviewed felt that board members were making judgments beyond their technical qualifications and that the procedures were a bureaucratic intrusion on their work.

THE SCIENTISTS’ RESPONSE TO PUBLIC PARTICIPATION

The search for forms of public participation proceeds with frustration and ambivalence, reflecting the persistent tension between the ideal of participation and its pragmatic implementation. As a concept, participation is a source of legitimacy, but as a procedure it may be inefficient and obstructive. Among scientists it is feared that widespread public involvement in decisions concerning science would virtually paralyze the conduct of research. For beyond
the usual procedural complications involved in establishing participatory reforms, science poses special problems that are apparent in the recombinant DNA dispute.

To assess research procedures requires evaluating complex technical material. Lay groups have significant problems coping with the knowledge required to evaluate research and may in the end fall back on whatever expertise is available. In large part, the Cambridge Review Board relied on the existing NIH guidelines and on the judgment of one of its members who had both pedagogical and community organizing skills.

Beyond the problems of complexity, however, research that probes the frontiers of knowledge may present risks that are vague or even hypothetical. There is seldom full and conclusive evidence that could serve as a definitive basis for predicting the effect of research, or its potential harmful applications. And even if it were possible to predict certain risks, there is no calculus by which to evaluate their public acceptability.\(^{51}\) Inadequate understanding of the nature of risk could bring about unrealistic demands for risk-free research. Mayor Velucci, for example, wanted “an absolute 100% certain guarantee that there is no possible risk which might arise from this experimentation.”\(^{52}\) It was only after months of intense study of the details of recombinant DNA research that members of the Cambridge Review Board, inclined initially to demand risk-free research, realized that this was not realistic.

Greater public participation may also disrupt patterns of behavior accepted as normal for the practice of science. The pressures of responding to the media, the tactical political manipulations, and the local or specific interests of most lay groups are alien to the way scientists perceive themselves. Mayor Velucci insisted that scientists testifying in the Cambridge public hearings identify their political base: “I represent the lay people of the city, who are you here to represent?” Similarly, hecklers at the National Academy of Sciences forum on recombinant DNA demanded that speakers identify their source of support as evidence of potential bias. In each case the demand created discomfort and irritation among scientists who perceive themselves more a part of an international community than a local political interest group.

Although openness is important to the scientific ethic, the demand for greater public information threatens the normal practice of science. When the City Council of Cambridge wanted access to research proposals, scientists sought to identify them as proprietary and to keep them from open circulation. Suggestions that the Freedom of Information Act be extended to provide open access to proposals horrified scientists who felt that such public access would allow theft of ideas by other scientists in fields where intense competition for priority in discovery leads to self-protection, would disrupt the peer review system and the unbiased evaluation of scientific work, and would provide opportunities for outside intervention.\(^{53}\)

Aware of these problems, many scientists have responded to the recombinant DNA dispute with dismay or bitter disdain. Fears are “science fiction” in the imagination of “a few ideologues” concerned with furthering their own political views. Critics are “kooks, shits and incompetents.”\(^{54}\) Their views reflect “antiintellectualism” and “political misbehavior.” One scientist compared the fear of physical contagion from pathogenic microbes to the seventeenth century
fear of moral contagion by soul corrupting books—both reasonable in their way but both pernicious.55 Many scientists felt public hearings to be unnecessary, and regulation to be destructive—"the first step towards government control," the "camel's nose under the tent."56

The overwhelming concern with maintaining self-regulation became explicit whenever regulation appeared to be imminent. In the spring of 1977, for example, the United States Environmental Protection Agency (EPA) sought opinions from professional societies about its potential role in monitoring the research. The Genetics Society of America wrote to forty eminent molecular geneticists, and fourteen replied.57 Thirteen asserted that research would pose minimal environmental hazards or none and that therefore there was no reason for regulation. Interference would "border on the ridiculous." The EPA should stick to "known hazards such as radiation exposure from nuclear power plants," they believed. The other four geneticists suggested that it was not really known whether or not there would be environmental risks, but two argued that, in any case, EPA should not be involved until the scientific community itself established evidence of risk: "The whole question of regulation and monitoring is abhorrent especially when done by a government agency and not by scientists." Above all, these scientists were concerned with the trend towards local government regulation. Indeed, as the participatory impulse was expressed by the interest of local communities in the issue, uniform federal legislation appeared increasingly desirable.

PRINCIPLES OF ACCOMMODATION

While many scientists remain firm in their resistance to outside regulation, the degree of autonomy from political interference that was negotiated during the period of post-World War II optimism no longer seems feasible in a far more sceptical age. Demands for public involvement and societal control imply new relationships—a new "concordat" which reestablishes the position and status of scientists with respect to the larger society. If the autonomy of science is viewed as a negotiated contract, not an absolute right, several principles follow.58

First, to establish viable relationships, bargaining parties must have a sense of political efficacy. Those who feel excluded from decisions they regard as controversial and who have no power to exercise sanctions are more likely to obstruct than to accommodate. In highly technical areas, political efficacy rests on availability of information. The Cambridge Review Board had good access to technical information and conscientiously educated itself. As an informed group with a sense of potential influence, it was able to take a constructive mediating role.

Second, negotiating relationships must be based on some mutual understanding and shared values—an esprit communauté which emphasizes mutual objectives, if only the need to maintain future relationships. The Cambridge Review Board shared with scientists a respect for the scientific endeavor; it focused more on the appropriate means to implement containment procedures than on the value of the research itself. Much greater difficulties of negotiation arise when basic ethical and value differences prevail. The prospect of genetic
engineering, like abortion or fetal research, tends to polarize opinion on the basis of conflicting values. Thus, the split within the scientific community, based on concerns about the moral implications of genetic research, appears non-negotiable; for when values are not shared, no facts can be brought forth beyond a certain point to change people's minds.

Third, a viable process of negotiation requires that controversial issues be stated in terms of problems to be solved rather than solutions to be accepted. This implies candid disclosure of the potential risks as well as benefits of scientific research. If research in a sensitive area is initiated as a noncontroversial technical exercise, public mistrust is likely to increase. The aftermath of Asilomar suggests the dangers of disclosing potential risks, but the scientists' initiative may have deflected harsher criticism; if the same questions had been raised by the public rather than by scientists, subsequent constraints on research could have been far more obstructive.

Fourth, negotiations must recognize and deal directly with issues of public concern. This controversy and others reflect more than simply fear of immediate risks. Authority is a major issue as the debate increasingly centers on the choice of appropriate controls. Asilomar demonstrated that scientists are attentive to risks, but it failed to deal with the concern about self-regulation; evidence of social responsibility simply did not answer the lingering questions about the locus of control.

Finally, negotiations must leave open some possibilities for compromise. Compromise is an ambiguous word. It may carry positive implications of agreement by mutual consent, or unsavory overtones of weakness and unprincipled concession. Scientists, unaccustomed to bargaining and compromise, are inclined to take the negative view. Yet there is room for compromise since much of the debate has to do with the level of containment required for specific types of research.

The recombinant DNA controversy has called attention to the importance of establishing a viable relationship between science and society that will allow continuing negotiation about specific problems of research. To simply argue the "right" of free inquiry is unrealistic, for there are already many restrictions on science. Since the 1946 Atomic Energy Act, all research activities using fissionable materials have had to comply to the security regulations and licensing procedures of the Atomic Energy Commission. During the 1960s, federal regulations virtually prohibited scientists from using psychogenic agents to elucidate basic biochemical mechanisms of brain action. Restrictions on the use of human subjects for biomedical research are by now routine. The recombinant DNA issue differs mainly in the extent to which it has become a focus of wide public scrutiny. Given the policy importance of scientific research and its concern with basic life processes, such public scrutiny is inevitable. The negotiation is no longer over whether there will be greater public control of science, but over who will participate in establishing controls, how controls will be organized, and how much they will influence detailed decisions concerning the nature and procedures of research.

References

1Research for this paper was financed in part by a joint NSF-NEH EVIST grant. I appreciate extensive comments provided by the MIT discussion group organized by G. Holton and R. S.
Morison. In addition, Rae Goodell, Clifford Groebstein, Philip Handler, Sheldon Krinsky, Richard O'Brien, and Vivien Shelanski provided important criticism.

1 James Sullivan, City Manager, letter to the City Council of Cambridge, August 6, 1976; Cambridge Experimentation Review Board, Guidelines for the Use of Recombinant DNA Molecule Technology in the City of Cambridge, submitted to the Commissioner of Health and Hospitals (December 21, 1976).


3 Cambridge Experimentation Review Board, op. cit.

4 Marlan Blissett, Politics in Science (Boston: Little Brown, 1972), chap. 3.


12 Scientists assume that external constraints would have a basic impact on scientific progress. Michael Polanyi argued that "any authority which would undertake to direct the work of the scientists centrally, would bring the progress of science to a standstill," in Michael Polanyi, "The Republic of Science," Minerva, 1 (Autumn 1962): 68. Similar arguments pervaded the recombinant DNA dispute. "We could not have penicillin or any of the other benefits of medical research over the past century," stated A. Pappenheimer in "Hearings on Recombinant DNA Research," (City of Cambridge, July 7, 1976), p. 204. See → Stanley Cohen, "Recombinant DNA: Fact or Fiction," Science, 195 (February 18, 1977): 654–659.


14 The above concerns were vividly expressed at the National Academy of Sciences Forum on Recombinant DNA (March 7–9, 1977).


16 Department of Health, Education, and Welfare, op. cit., p. 233. In testimony during the NIH hearings Dr. Emmett Barkley summarized studies of laboratory-acquired infections; there were 3,921 reported cases in the last three decades. At Fort Detrick 423 laboratory infections and three deaths were reported from 1943 and 1970. See National Research Council, "Report of the Panel on Risks and Benefits of Recombinant DNA Research" (November 1, 1977).


23 They were mainly affiliated with Science for the People, a group of scientists who organized during the Vietnam War over the issue of military research in universities. Expression of their concerns appears in testimony by Jonathan King, Cambridge Hearings, op. cit. (June 23, 1975), pp. 135–145; Richard Lewontin, ibid. (July 7, 1976), p. 188; and Dr. Silverstone, Department of Health, Education, and Welfare, op. cit., p. 286. Proposals have included eliminating the possibility
of a Nobel Prize in this area in order to reduce competition, and restricting research to only a few isolated laboratories.


27The guidelines (Cambridge Hearings, op. cit.) described four levels of physical containment for experiments, ranging from adherence to standard laboratory practices to requirements for the use of special buildings and extreme decontamination procedures. They also outlined levels of biological containment based on the survival rates of the recombined bacteria used in experiments; and bacteria were to be developed with mutations that would prevent survival outside the laboratory. See Department of Health, Education, and Welfare, op. cit.

28The NIH Committee had included one nonscientist, Emmett Redford, a political scientist, in 1975. A year later a second nonscientist, Leroy Walters, an ethicist, was added.

29Peter Hutt to Donald Fredrickson, Department of Health, Education, and Welfare, op. cit., p. 483.


31Public hearings were held by the U.S. Senate Subcommittee on Health, of the Committee on Labor and Public Welfare, on September 22, 1976 and April 22, 1975; and the U.S. House Subcommittee on Research, Science, and Technology in April, 1977. In addition, state and local legislation to curb the research has been proposed in many states, including New Jersey, New York, California, Wisconsin, Indiana, and Michigan. → Nicholas Wade, "Gene Slicing: At Grass Roots Level a Hundred Flowers Bloom," Science, 195 (February 11, 1977): 558–560.

32In a letter to the NIH, the Friends of the Earth wrote, "We have been particularly struck by the small, preliminary steps being taken to deal with genetic engineering problems, with the parallels to the nuclear power controversy, which of course received no public debate or scrutiny for the first twenty years of its existence. Both nuclear power and genetic engineering seem to be proceeding on the assumption that they must proceed, yet no public debate had been initiated on genetic engineering even now as the impetus grows." Cf. letter from Lorna Salzman to Donald Fredrickson (May 17, 1976), Department of Health, Education, and Welfare, op. cit., p. 542.

33Cambridge Hearings, op. cit. Vellucci also held an outdoor Saturday morning “market” of scientific ideas in a public square where scientists with conflicting views aired them to the public.


36Mayor A. Vellucci, NOVA interview, op. cit., p. 21.

37Cambridge City Council Experimentation Review Board, op. cit.

38Mayor Vellucci fought the decision to the end, threatening to appoint his own committee. In the end he supported the recommendations but wrote a letter denying all personal responsibility.

39Statement by a participant at a workshop on citizen participation at the National Academy of Sciences Forum, March 8, 1977. This attitude reflects a general decrease in public confidence in institutions. A Harris Poll found that between 1966 and 1973, the proportion of the public expressing a great deal of confidence in the leadership of institutions declined as follows: federal executives, 41% to 19%; Congress, 42% to 29%; major companies, 55% to 29%; higher education, 61% to 44%; medicine, 72% to 57%.

40Stanley Cohen, op. cit.


43Harold Green, op. cit.

44Cambridge Experimentation Review Board, op. cit.

45James Creighton, "The Limitations and Constraints on Effective Citizen Participation," in an address to Inter-Agency Council on Citizen Participation, December 8, 1976, argued that a representative government cannot deal directly with demands for issue-by-issue accountability. Citizen participation or public involvement is an adaptive response to these demands. Similar adaptive responses are taking place in European countries where experiments attempt to bring informed public involvement to controversial technical decisions so as to "reconcile contradictions between expertise and democracy." In Sweden, the government supported study circles involving some 80,000 people who met regularly in small groups to discuss aspects of the energy program. In Austria the Ministry of Industry developed a program to publicly debate the most controversial technical dimensions of nuclear energy. The assumption in these efforts is that open discussion can help to reestablish the trust necessary for stable negotiating relationships. For discussion of these

It is usually assumed that participation increases trust in the political process. However, the small amount of data available suggests that participation is not necessarily a significant determinant of trust. A survey of voters, nonvoters, and those who participate in political meetings found little difference in their trust in government. Participation is, however, associated with a sense of political efficacy, the feeling that one can actually influence the action of governments. Yet here too, cause and effect relationships remain vague for initial expectations about efficacy may have inspired the participation in the first place. See Robert K. Yin et. al., *Citizen Organization*, RAND Corporation report for U.S. Department of Health, Education, and Welfare, R 1196 (April, 1973).


J. C. Glick, in "Reflections and Speculations on the Regulation of Molecular Genetic Research," in Lappé and Morison, op. cit., pp. 189–190, proposes a profession of certified public scientists who would perform independent audits of scientific research. They would belong to a professional organization (an Institute of Certified Public Scientists) which would set standards for review.


Mayor Vellucci, in Cambridge Hearings, op. cit. p. 55.

For example, the animal welfare controversy at the American Museum of Natural History in New York City began because an antivivisectionist used the Freedom of Information Act to obtain a research proposal describing the use of cats as experimental subjects. See *Science*, 194 (October 8, 1976): 162–166.

James Watson, from a public speech cited in *Chemical and Engineering News* (May 30, 1977). Other labels have been culled from assorted speeches, letters, and editorials.


Note that industry is less concerned about regulation than academic scientists. They are organized to deal with it, and their main concern is that regulation protects proprietary information. See Testimony before the U.S. House of Representatives, Subcommittee on Science, Research and Technology (May 3, 1977).

The Genetic Society of America claimed it took no official position on political questions and instead asked forty of its most eminent members to respond. Letters were made available through the Environmental Protection Agency.