• *ABSTRACT*

This paper examines an early phase of the controversy over the hazards of recombinant DNA technology in the United States, in the period 1976–78, during which agreement was reached within the biomedical community that these hazards were minimal. The proceedings of three scientific meetings that are generally agreed to have been central events in the emergence of this new perception of recombinant DNA hazards are examined. Techniques previously used to examine policy making on non-technical issues are applied here to analyze the formation of this scientific consensus. These techniques are used to show how certain social characteristics of the meetings – the sponsorship and organization of the meetings, informal processes affecting the scope of the proceedings, and the dissemination of the results – acted as 'social filters' for the complex set of perceptions of recombinant DNA hazards with which the scientific community started. In contrast to the received view of the recombinant DNA controversy, according to which the issue was resolved at a technical level, this paper argues that social dimensions of the decision process were crucial to the outcome.

Molecular Biology or Molecular Politics? The Production of Scientific Consensus on the Hazards of Recombinant DNA Technology

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One of the most remarkable aspects of the controversy about the hazards of recombinant DNA technology in the 1970s was the speed with which the whole issue faded away. Intensely debated in the period 1975–77, by 1979 the hazard question was almost a non-issue. Early warnings by prominent scientists about the potential hazards of combining the genes of unrelated species were quickly replaced by soothing reassurances that there was little or no cause for concern.

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Before 1960, it is likely that historians of science would have looked for an explanation of this development only in technical terms. But the assumption that technical issues can be resolved on technical grounds alone has come under severe challenge in a variety of arenas since 1960. On the one hand, debates on such issues as the safety of nuclear power plants, the impact of toxic wastes and the causes of cancer have shown that there is little or no politically neutral ground on which to resolve these issues. On the other hand, much recent work in the history and sociology of science has challenged the basic tenets of traditional empiricism and has demonstrated the 'social permeability' of the processes that produce scientific consensus.[1] These comments are not intended to suggest that an attempt to understand the formation of a consensus about the hazards of recombinant DNA technology should look only for social or cultural explanations. My purpose is to emphasize that both social and technical influences, and their interaction, must be considered. In particular, it is important to ask to what extent social or cultural considerations affected the generation, selection, and use of scientific evidence, the movement from evidence to conclusions, and the amplification of these conclusions as they were disseminated to other sectors of society.

Recombinant DNA technology (popularly known as 'genetic engineering') was surrounded by controversy about its possible impact on human health and environment almost from the time of the first experiments demonstrating the feasibility of transfer of genes between unrelated organisms in 1972. Generally it was feared that novel gene combinations would gradually be disseminated from laboratories or industries which used the techniques, would become established in organisms in the environment and provide mechanisms to generate new forms of disease. In addition, the ultimate social uses of the technology worried some scientists.

Much of the concern about the health hazards of recombinant DNA technology focused on the safety of the organism used at that time for all cloning work, a strain of the common intestinal bacterium *Escherichia coli* known as K12. *E.coli* K12 was not known to be pathogenic, nor could it survive easily outside the laboratory or in competition with other organisms. But it was feared that recombinant DNA techniques might inadvertently convert the bacterium into a pathogen, or that it might transfer 'foreign' DNA inserted into it to other more robust types of enteric bacteria which could survive effectively in humans and in the environment.

A period of debate and policy making followed, which led, by 1976, to the development of controls for recombinant DNA research in the United States, the United Kingdom, and in most other countries where research was under way. In the United States, strict guidelines for research supported by the federal government were developed by an advisory committee to the National Institutes of Health. These controls limited work in the field to a relatively small set of experiments that used *E.coli* K12 as the bacterial host. In addition, six classes of work were prohibited, including use of the techniques on a commercial scale.

But the NIH guidelines themselves became controversial. Would these controls guarantee effective containment of *E.coli* K12? Could this strain transfer 'foreign' DNA to other more robust organisms that could survive effectively in humans and in the environment? Further, could the controls really be effective when they applied in the first instance only to NIH grantees but not to the private sector or even to work supported by other government agencies?

Controversy erupted at a hearing on the NIH guidelines in February 1976 and spread quickly to some of the local communities which were centres of recombinant DNA activity: Ann Arbor, Cambridge, Princeton, Berkeley, San Diego. By July 1976, the Cambridge City Council had announced a three-month moratorium on recombinant DNA research within its boundaries while its own committee investigated the issues and assessed the adequacy of the NIH controls. There was also growing interest in Congress in passing legislation to regulate recombinant DNA technology. By the spring of 1977, roughly a dozen bills for that purpose had been introduced into the House and Senate, including a bill initiated by Senator Edward Kennedy which provided for external regulation of the field by a presidential commission.

At the same time, as the field began to demonstrate its practical and scientific potential, research efforts expanded rapidly and competitive pressures were increasingly felt at all levels of science, from the laboratory to the federal government. The accelerated pace of development was accompanied by an intensified fear of 'falling behind'. Scientists imagined that their work would be superseded by research pursued under less strict controls in other countries. Those responsible for national science policy feared that the United States would lose its lead in the field.[2]

It was during this period from 1976 to 1978 that leaders of the American biomedical research community began to argue that the

potential hazards of genetic engineering were exaggerated and that work pursued on *E.coli* K12 was, despite earlier concerns, quite safe, and that this new position was supported by new scientific information. This essay focuses on the change of scientific consensus that occurred, largely in the United States, at this time, and particularly on the role of three scientific meetings: the Enteric Bacteria Meeting at the National Institutes of Health in Bethesda, Maryland in August 1976; a workshop entitled 'Risk Assessment of Recombinant DNA Experimentation with *Escherichia coli* K12' held in Falmouth, Massachusetts in June 1977; and a meeting entitled 'US-EMBO Workshop to Assess Risks for Recombinant DNA Experiments Involving the Genomes of Animal, Plant, and Insect Viruses' held in Ascot, England in January 1978.[3]

Whatever else may be at issue, there is little doubt that these meetings were important sources of the 'new information' claimed as the basis for the change in perception of recombinant DNA hazards as well as crucial influences on its interpretation.[4] As shown later in this paper, the first meeting at Bethesda led directly to a decision to organize the second at Falmouth. And the Falmouth meeting strongly influenced the outcome of the third meeting at Ascot.[5] The results of all of these meetings, particularly the second and third, were subsequently cited repeatedly in scientists' testimony, in policy documents, and in statements of officials in the Department of Health, Education, and Welfare and the National Institutes of Health to justify the claim that there was little cause for concern.[6] Largely as a result, attempts to pass legislation aimed at regulating recombinant DNA technology were dropped and the NIH guidelines were revised and substantially weakened. Perhaps most important of all for the course of future policy making, a major change in the principles guiding NIH policy occurred: the burden of proof was transferred from scientists, to show that genetic engineering was safe, to the general public, to show that it was dangerous.[7]

However, while pressures to move quickly in the recombinant DNA field intensified, the controversy over its hazards continued. It was not a controversy that was easily resolved, largely because of the multidimensional complexity of the issues involved. First, there was an enormous variety of microorganisms which might potentially be used as hosts for cloning purposes and an enormous variety of genes which might be inserted into these organisms. The action and function of many of these genes was not well understood. In addition to these considerations, there was also the question of the interaction

of the genetically engineered organism with its environment and with other organisms. In the course of the controversy, dozens of possible risk scenarios were contemplated. A dearth of empirical evidence compounded the uncertainties which emerged in analysis of these possibilities.

Given the complexity of the issues, as well as the fact that the techniques of engineering were evolving rapidly, the debate on the hazards of this field might well have continued indefinitely. In fact, it was soon restricted and ultimately closed down. The general phenomenon of the restriction of scientific controversy has been intensively studied in the last several years, with particular emphasis on intra-scientific mechanisms that produce closure.[8] In this essay, I take a different, though complementary, approach to the analysis of scientific controversy, one which has the advantage of suggesting a larger research programme for the analysis of connections between mechanisms of closure and the wider social context. I apply techniques of analysis used previously by political scientists in studies of decision making in political systems, particularly on controversial issues involving conflict and competition between social groups.[9] These authors have shown that for an understanding of the dynamics of controversy, it is generally not enough to examine only decisions taken in a formal decision process. Equally, perhaps even more, important for the outcome are such dimensions as the institutional context of decision making and the informal processes that affect the scope of the issues placed on the formal agenda. In this regard, their work suggests several conceptual approaches to the analysis of controversy.

First, these writers have emphasized the importance of examining the institutional environment in which an issue is addressed, as well as the effects of that environment on the distribution of influence in, and access to, the decision arena. In particular, they have proposed that restriction of participation in public affairs should be an object of investigation. As Robert Dahl notes: 'It is a reasonable preliminary hypothesis that the number of individuals who exercise significant control over the alternatives scheduled is . . . only a tiny fraction of the total membership.' Similarly, E.E. Schattschneider warns against the pluralist assumption that groups compete on equal terms in the policy arena: 'The flaw in the pluralist heaven is that the heavenly chorus sings with a strong upper-class accent. Probably about 90 percent of the people cannot get into the pressure system'.[10] In the case of technical controversy, it is no less important to examine how the decision arena is structured and who gains access to it.

A second dimension follows from the first, for restriction of access to the decision arena means that the kinds of issues that are placed on the formal agenda for consideration may also be restricted. The studies noted above emphasize the importance of investigating the tendency for participants in a decision arena to address only those issues and alternatives that do not disrupt the prevailing balance of power. In Schattschneider's words: 'All forms of political organization have a bias in favor ~~of~~ the exploitation of some kinds of conflict and the suppression of others because organization is the mobilization of bias. Some issues are organized into politics while others are organized out'.[11] Peter Bachrach and Morton Baratz take a similar position:

> Of course power is exercised when A participates in the making of decisions that affect B. But power is also exercised when A devotes his energies to creating or reinforcing social and political values and institutional practices that limit the scope of the political process to public consideration of only those issues that are comparatively innocuous to A. To the extent that A succeeds in doing this, B is prevented, for all practical purposes, from bringing to the fore any issues that might in their resolution be seriously detrimental to A's set of preferences.[12]

The same holds for scientific controversy. It is important to ask which aspects of a controversy dominate, and which are neglected, and to investigate the reasons behind these choices.

Finally, the form in which conclusions reached within a decision arena are disseminated can be a powerful means for reinforcing the prevailing bias. Particularly in science, where the full authority of the scientific community can be placed behind a conclusion, dissemination can greatly amplify the effect of the initial decision.

My principal purpose in the following is to use these approaches to show how various institutional, procedural and conceptual characteristics of the conferences at Bethesda, Falmouth, and Ascot acted as 'social filters' for the complex set of perceptions of recombinant DNA hazards with which the scientific community started. I shall argue that as a result, reservations about claims for decreased hazard were for the most part organized out of consideration, whereas claims that certain types of risk were minimal were organized in. Each stage of the process simplified the initial complexity until what was left was virtually a single argument: that whatever else might be done to it, it was impossible to convert *E. coli* K12 into an epidemic pathogen which could escape the laboratory and run rampant through a population. (I shall refer to this argument as the 'epidemic pathogen' argument.)

I shall discuss here three main mechanisms which were instrumental in this process of social filtration: first, the sponsorship and organization of the meetings, which determined the range of scientific and political participation in them; second, the informal processes that affected the scope of the proceedings and the reporting of results; third, the dissemination of the results and their use to justify weakening social control over the development of recombinant DNA technology.

Sponsorship and Organization

The organization that played a major role in the sponsorship of all of the meetings under consideration was the National Institutes of Health. NIH was the main source of government support for biomedical research in the United States. As such, it was the centre of a vast research network that connected its leadership closely with large research universities on the one hand, and the community of biomedical researchers on the other. As noted above, NIH was also responsible for the development of government controls for possible hazards of recombinant DNA technology. There is abundant evidence to show that university administrators and biomedical researchers overwhelmingly favoured this arrangement.[13] The Enteric Bacteria meeting was organized by two NIH virologists, Wallace Rowe and Malcolm Martin, one of whom, Rowe, was also a member of the Recombinant DNA Advisory Committee and of the RAC Executive Committee which advised the NIH Director on recombinant DNA policy matters. The Falmouth meeting was sponsored and funded by NIH. The Ascot meeting was jointly funded and sponsored by NIH and the European Molecular Biology Organization, a private scientific organization for the support of research in molecular biology supported by several European countries and a major grant from the Volkswagen Foundation. (EMBO was active in monitoring recombinant DNA controls in the United States and Europe and in making policy proposals of its own and generally favoured weakening controls developed in the mid-1970s.) The meeting was jointly chaired by Rowe, Martin, and John Tooze, executive director of EMBO. For all of these meetings, scientists close to institutions responsible for the sponsorship of recombinant DNA technology played important roles in deciding on the forms of the meetings.

The formal purposes of each of these meetings were scientific and

technical in nature. The Enteric Bacteria meeting was designed as an informal meeting with infectious disease specialists to open up analysis of recombinant DNA hazards to a wider spectrum of disciplines which had not previously been involved in the recombinant DNA issue. The Falmouth meeting had similar goals but was organized as a larger and more formal event with solicited papers. The Ascot meeting was designed to involve specialists in animal virus research in a detailed analysis of the hazards of cloning animal virus DNA.

As scientific events however, the organization of each of these meetings was anomalous, deviating significantly from the classic norm of openness. Each was unannounced, private, and thus known in advance only to a select group of scientists closely associated with the organization of the event. In each case, the wider scientific community and the public learned of the meetings only after the fact. In the case of the Enteric Bacteria meeting, even the identities of participants other than the two chairmen remain officially unrevealed to this day.[14]

This does not mean that a range of scientific positions was not represented at these meetings. Participation, however, was definitely controlled. In the first and third meetings, participants were invited by the chairmen. For the Falmouth meeting, a larger organizing committee was responsible for invitations, but even then, only two scientists known to be critical of NIH policy were present. One of those scientists, MIT biologist Jonathan King, later observed that he had to request admission to the meeting from the chairman. King also emphasized that '[the conference] was not announced by the normal procedure for announcing scientific conferences, that is, in the scientific journals, Genetics Society of America, American Society of Microbiology. It was private. It was by invitation of the organizing committee. Many people were rather upset . . . to find out that a risk-assessment conference was taking place and they didn't even know about it until after the fact'.[15]

At the Ascot meeting, held in the United Kingdom, members of the British Genetic Manipulation Advisory Group (GMAG) were not invited — an omission which caused many British eyebrows to be raised. As one member of GMAG later commented:

> It might be thought a discourtesy to run an international conference on an important policy question without involving the corresponding organization in the host country, particularly when that is the only one in the world to be setting standards that are used internationally. Indeed, it is hard to see why GMAG should

> have been excluded, except for the strong representation on GMAG of the members representing employees and the public interest. Had GMAG been invited to participate, some of these would certainly have attended, and would have supplied a critical presence.[16]

This evidence suggests that while technical expertise was well represented, there was much less diversity in representation of political positions on the question of control of recombinant DNA technology. This conclusion is also supported by the fact that at the more private Enteric Bacteria and Ascot meetings, discussions were characterized by informal understandings about the politics of the recombinant DNA controversy. A strong informal theme of the Enteric Bacteria meeting was a shared sense of a pressing need, beyond containing possible hazards of recombinant DNA work, to contain the spread of the controversy as well. There is a siege-like feeling about these discussions, a shared sense of threat, of polarization, of scientists versus society. Polarized categories — research scientists versus 'them', variously described as the 'sky-is-falling people', 'prophets of doom', those motivated by 'political interests' — characterize references to the recombinant DNA issue. In general, the transcript suggests that this group saw the recombinant DNA controversy as symptomatic of a general movement of the non-scientific public to bring biomedical research under external control.[17]

At the Ascot meeting, Wallace Rowe's introductory remarks similarly painted a picture of biomedical research as immensely threatened by external political forces, of the recombinant DNA controversy as a confrontation between the forces of rationality and the forces of antiscience, and of recombinant DNA controls as only the beginning of the progressive encroachment of bureaucratic restrictions on biomedical research. As he stated: 'There are very dreadful things on the horizon and it's not restricted to recombinant DNA. [The movement to restrict] recombinant DNA [research]... is only the beginning of... great dangers to freedom of inquiry'.[18] It is unlikely that such statements would have passed unchallenged in open meetings with a wider range of representation.

Informal Processes Affecting the Scope of the Proceedings and Reporting of Results

As political scientists have been pointing out for some time, who gains access to the decision arena strongly affects the values that

shape the proceedings. In this case, the restriction of political representation at these meetings meant that the assessment and analysis of recombinant DNA hazards happened in a specific political context characterized by strong informal interests in responding to and containing the recombinant DNA controversy. It is important to ask how this bias affected the definition of the issues under consideration and the scope of the proceedings.

The Enteric Bacteria Meeting

The tone of the Enteric Bacteria meeting was set by the chairman, Wallace Rowe, in his opening remarks:

> Part of the agenda today is to get you guys involved and get your voices heard, and maybe if the 'Infectious Disease Society of America' comes out and says, 'By God, if it's just insertion [of foreign DNA] you are talking about, nobody is worried about that mechanism.' That carries [a] tremendous amount of weight, at least to me. If I could say that to the prophets of doom: 'Look, these guys have come out and said that there is nothing to worry about here, so let's really start and get on with serious business.' That's what I hope we can accomplish.[19]

With this orientation to the problem, much of the conference was spent brainstorming the hazards of recombinant DNA technology. A wide and complex collection of issues was brought up, the general tenor of which was that unusual and possibly problematic combinations of genes could gradually be transferred into organisms in the environment where they might at some later time be expressed in a way that could cause eruption of novel disease. As one participant stated: 'there may be problems of low level endemicity. And, depending on what's created, in special cases, serious endemicity. The *Botulinus* [toxin], the growth-hormone producing *E.coli*. To me, those are frightening'.[20]

Consideration of these problems was greatly restricted, however, by the adoption of several assumptions which had the effect of focusing attention on a limited subset of hazards which were generally judged to be of much less concern.

The most important restriction placed on the discussion was the assumption that all recombinant DNA research would be conducted with *E.coli* K12, the strain of the common intestinal organism which had been weakened by many years of use in molecular genetic laboratories. As Rowe stated at the beginning of the meeting:

> Out of the infinite universe of combinations that DNA recombinant research can involve, the guidelines have narrowed it down, it seems to me, to a very advanced level. Of all the bacteria in the universe, we're only really talking about one particular bacteria with options to find parallel ones that are as laboratory restricted. Okay, so *E.coli* K12 is really the focus . . . of these experiments. No other organism is presently considered as 'licensed' under the guidelines.[21]

Rowe went on to qualify his statement because even in 1976, there was scope in the NIH guidelines for expansion to the use of other organisms in genetic engineering work. Nevertheless, *E.coli* K12 quickly became the focus of attention.

A second major restriction was the assumption that the assessment would be limited to hazards to communities *outside* the laboratory. Hazards to workers *inside* the laboratory were deemed relatively unimportant. In other words, the group took its concern to be not primary exposure but secondary spread. There appears to have been general agreement on this. As one participant stated: '[The question of epidemic disease] . . . seems to be much more important than infections of laboratory workers'; and as someone replied:

> I take that as a major condition. I am really not as concerned about lab workers as long as the infection is restrained in our midst. Introducing new things in the eco-system, into populations that are in no way involved in the lab, that is what I worry about. A case in the investigator — [or] the technicians — is bad, but that's not the major question.[22]

A third restriction was that recombinant DNA activities would occur only in technologically advanced countries with adequate public health and sewage treatment facilities. The implication was that epidemics in such environments would not occur under any circumstances. As one speaker put it:

> This kind of epidemic just doesn't happen and isn't happening in our society largely because of sanitation. We don't have house rats and we don't [have] house fleas and we don't have lice and we don't eat shit and that's what it comes down to. In our kind of society, this kind of epidemic just doesn't happen.[23]

With these restrictive assumptions, the participants focused their attention on the 'epidemic pathogen' scenario, which was generally agreed to be unlikely. Even so, not all concerns were put to rest. The following exchange is characteristic:

> — . . . What I want to know is, living in, say, Washington, can you make an epidemic in Washington? Can you make an organism so virulent that it will make an epidemic in Washington? . . .

> — I think the point is that your K12 could be carrying a new product that is quiescent as a genetic entity. It's got virulence, but is not expressing itself. As soon as by some accident of nature it then leaves that environment and gets superimposed . . .
> — Can you arrange this accident? Can you think of a circumstance in which you could make it spread? This is really the heart of the issue. Can it be done?
> — I can't answer that.

Another participant, however, cited the occasional large-scale *Salmonella* epidemics, such as an outbreak in Riverside, California involving 20,000 people. As someone summarized the concern: 'The point we are trying to make is, if you already have an organism that can cause epidemics and if it receives the genes from, say K12, you can get an epidemic of organisms with *those* genes'.[24]

Clearly not everyone was persuaded that the new technology posed no new problems. But as the discussion continued, outstanding issues — such as the question of low-level seepage of novel gene combinations into organisms in the environment — tended to be factored out of consideration rather than confronted. Instead, the sense mentioned earlier that biomedical research was threatened came increasingly into focus. When several people noted that other aspects of biomedical research might pose hazards as serious as those of the new biology, they were warned that scientists must be careful not to stimulate the spread of regulation to other research fields. 'Science,' someone announced, 'is under very serious attack'. 'But where is the attack coming from?' it was asked. 'From ourselves,' came the answer. 'One has to be very careful about the tack one uses and should not say, "Well, gee, we have been doing much more dangerous experiments for years." That's murder! You have to use a very positive approach'.[25] In the same vein, someone else (or possibly the same person) warned that:

> we have a serious political disease . . . [and that] you have to be careful in these arguments that you don't spread it to other people. The big danger about the argument 'But look! something else is much more dangerous than what we do already' is that the 'something else' all of a sudden gets in with a big bag of red tape at the very least.[26]

Visions of laboratories swathed in red tape dominated the later stages of the morning session. Within the context of concern about the spreading regulation of science, the argument that *E.coli* could be converted into an epidemic pathogen came to be seen, not simply as one consideration among many others associated with the problem of

defining potential recombinant DNA hazards, but as a leading argument and furthermore, an argument which could be developed specifically for the purpose of defusing the growing controversy. As the chairman, Wallace Rowe, expressed this sense:

> Why I got you here is that I think if somebody acquires data that convinces important people, they'll say, 'It's a bunch of nonsense; you cannot change *E.coli*; you've tried, so and so has done this until he is blue in the face and I can't see and a thousand other Infectious Disease people can't see any danger in working with a *Salmonella* donor into *E.coli* and a *Drosophila* into *E.coli*.'[27]

Further exchanges following this statement show that others present accepted this political strategy:

> — Well, who do I have to impress? How does it come to pass that I have to write an application to do a standard genetic cross?
> — That's really where it's at . . . The point, as I understand it, is that the ingredients for infectious disease with *E.coli* K12 are simply not there and the number of unknowns that you have to specify is very large and each probability is very small. You multiply them altogether and you come out with nothing. You know, numbers that are comparable to 10^{-n}; negative numbers that are comparable to the number of the atoms in the universe.
> — You are going on the argument that people say you are going to create drastic epidemics, everyone bleeding to death . . .
> — Right, but that's what people are being scared with; that is what the other side is winning with. They are *not* winning with the idea that a few lab technicians or a few scientists are going to get sick. They don't care about that. Nobody cares about that.[28]

When someone at this point attempted to make scientific distinctions about hazards, they were told that the political dimension had to be emphasized. Here is the exchange:

> — [We must] really separate these [issues] out and somehow . . . try to get the word going around that informed people are really not worried about epidemics; that there may be problems of low grade endemicity, and depending on what's created, in special circumstances, serious endemicity — the *Botulinus*, the growth hormone producing *E.coli*. To me, those are frightening.
> — The Mayor of Cambridge doesn't know the difference. What the Mayor of Cambridge is worried about, besides not being re-elected the next time, is the possibility of an epidemic. It's exactly the same issue as the nuclear people have to face: that there are serious arguments being made at the level of low levels of contamination, but their popular image is that of explosions. And it's exactly parallel. Serious arguments are about this kind of low level thing, but in terms of the PR, you have to hit epidemics, because that is what people are afraid of and if we can make a *strong* argument about epidemics and make it stick, then a lot of the public thing will go away.[29]

The select participants at the Enteric Bacteria meeting, sharing an interest in protecting 'free inquiry' in recombinant DNA research, thus carefully concentrated their attention on developing arguments that would convince the public that research hazards were exaggerated. The issue was not *whether* the 'epidemic pathogen' argument was technically acceptable but *how* it should be used politically. As someone summarized the sense of the group at the end of the morning session:

> I think [the problem of convincing the public] is what you have to deal with. It may not mean a thing, but that is very easy to do. It's molecular politics, not molecular biology and I think we have to consider both, because a lot of science is at stake.[30]

The Falmouth Meeting

Wallace Rowe reported on the Enteric Bacteria meeting to the NIH Recombinant DNA Advisory Committee in September 1976. Rowe conveyed the view that, in the opinion of the participants, 'enteric epidemics are extremely remote' and that 'concepts such as this should be discussed in a public forum'.[31] An organizing committee chaired by Sherwood Gorbach, a specialist in enteric disease at Tufts University, was established and the outcome was the two-day workshop held at Falmouth, Massachusetts in June 1977.

The agenda of the Falmouth meeting was limited to the hazards of use of *E.coli* K12 as a cloning host. According to the proceedings, published almost a year later, three basic questions were posed: first, could the addition of 'foreign' DNA convert *E.coli* K12 into a pathogenic strain that could either cause disease in an individual or spread through a population? Second, could DNA inserted into the K12 strain be transferred to other microorganisms or to the somatic cells of a host? Third, could 'foreign' DNA inserted into *E.coli* K12 encode for harmful products such as toxins, hormones, or proteins capable of inducing an allergic response?[32]

The published proceedings show that the Falmouth meeting's response to these questions produced mixed results. Some research appeared to be reassuring although by no means definitive. For example, efforts to establish the K12 strain in the human intestine showed that the organism generally survived no longer than four or five days.[33] However, the significance of this result was not altogether clear. Feeding experiments had also demonstrated the failure of other, more robust strains of *E.coli* to establish themselves.[34] Further

reassurance was claimed for experiments which showed that efforts to enhance the pathogenicity of the K12 strain by traditional breeding techniques were unsuccessful.[35] However, this work was limited in scope, did not use genetic engineering, and was based on limited knowledge of the location and role of genes controlling pathogenic properties of *E.coli*.[36]

Other research and analysis was clearly inconclusive. For example, troublesome questions were raised about the capacity of *E.coli* K12 to transfer 'foreign' DNA to other more robust organisms which could survive more effectively in the environment.[37] As Bruce Levin, a population geneticist who attended the meeting, later described the state of the issue:

> There was considerable discussion about the transfer of bacterial plasmids and a general feeling that the rate of infectious transmission in the intestines of healthy mammals would be low. However, in my impression there was absolutely no consensus reached which suggested that the probability of transfer of chimeric DNA by plasmids was sufficiently low to be disregarded. Furthermore, there was very little consideration about transfer via transducing phage or as free DNA.[38]

Further questions were raised about the impact of *E.coli* bacteria which were genetically 'reprogrammed' to make novel proteins. At this point in the development of genetic engineering this possibility was not realized, but it was a major research goal. Jonathan King of MIT and Sydney Brenner of the Cambridge Laboratory for Molecular Biology raised the theoretical possibility that such reprogrammed bacteria might generate new forms of auto-immune disease in which secreted gene products might cause human or animal hosts to make antibodies against their own proteins.[39]

Clearly Falmouth did not produce a definitive interpretation of recombinant DNA hazards. Indeed the inconclusiveness of the discussions was underscored by a primary outcome of the meetings: the development of a set of detailed protocols for further risk assessment research. Gorbach, in an introduction to this list of proposals in the published proceedings, stated that 'there must be a beginning, even if it serves to create a focus for disputation; from the cauldron of vigorous scientific debate will finally emerge critical experiments to assess the potential hazards in recombinant DNA technology'. The need for further experimental work appears to have been generally accepted.[40]

A sense of the full scope of the Falmouth proceedings was not what

reached either the wider scientific community or the general public, however. The public image of the Falmouth results was shaped primarily by a letter sent by Sherwood Gorbach to the NIH Director, Donald Fredrickson, immediately after the conference. The 'epidemic pathogen' argument dominated Gorbach's account to the virtual exclusion of other issues. There was, Gorbach emphasized, 'unanimous' scientific agreement, backed by 'extensive' scientific evidence 'all of which provides reassurance that *E.coli* K12 is inherently enfeebled and not capable of pathogenic transformation by DNA insertion'. What emerged was an essentially soothing view of the evidence, one in which uncertainties and unresolved issues were obscured by the emphasis on the remoteness of possible hazards.[41]

The arbitrariness of this result is brought out by the scepticism with which it was received in countries where the political relations of the research laboratory differed from those in the United States. In Britain, for example, where trade unions played an important role in the development of policies governing laboratory safety, the 'epidemic pathogen' argument never became a major focus of debate, nor was it used as a rationale for changing policy. As Sydney Brenner, Director of the Cambridge Laboratory for Molecular Biology stated in 1980: 'We were never concerned about creating an epidemic pathogen. In the first instance, our concern was the health and safety of people at work'.[42]

The important point emerging here is that how scientists responded to the information developed at the Falmouth meeting depended not on the scientific validity of the epidemic pathogen argument (as I have noted earlier, there was little *technical* disagreement about this argument) but on whether they saw this result as central to the question of recombinant DNA hazards. Their judgments on the latter issue were *social* rather than *technical.* Scientists who accepted the social validity of the 'epidemic pathogen' argument saw Falmouth as a scientific meeting that produced a scientific judgement. Scientists who did not accept the social validity of the 'epidemic pathogen' argument also saw Falmouth as a scientific meeting — with the difference that they perceived the results as 'choreographed' (to use the description of one of my respondents) — in other words, aimed at emphasizing a preconceived result. As one of the scientific members of GMAG expressed this view: '[Falmouth was] a real set-up . . . not a comprehensive scientific debate . . . [The 'epidemic pathogen' argument] was developed by people who wished to produce a certain conclusion'.[43]

The Ascot Meeting

Gorbach's summary statement of the Falmouth result proved highly influential for the third scientific meeting to be discussed, which was held at Ascot, England, in January 1978. A major reason for the Ascot meeting was growing discontent among virologists in the United States and elsewhere with restrictions on the cloning of animal virus DNA in *E.coli*. These procedures had stimulated the original concerns about the potential recombinant DNA hazards and had subsequently been classified as 'high risk' in the NIH guidelines. The need for a meeting to reassess these controls was emphasized by Paul Berg in a letter to the NIH Director in October 1977 and reinforced by John Tooze, executive director of EMBO, at an NIH hearing in December 1977.[44] Apparently these concerns were heard sympathetically. The three-day meeting occurred in January 1978 and was jointly sponsored by NIH and EMBO.

As at Falmouth, the Ascot participants addressed only the risks of cloning in *E.coli* K12. Again, the 'epidemic pathogen' argument, now validated as the result of the Falmouth meeting, proved influential. To some extent however, a focus on epidemics was diffused as a result of the presence of participants from European countries such as Sweden and the United Kingdom where hazards to individual workers were a primary issue in the formation of recombinant DNA policy.

In classifying hazards resulting from the use of animal virus DNA in cloning, the meeting explored two major classes of use — cloning of viral DNA in bacteria, and use of viruses as vectors to insert foreign DNA into animal cells. In each case, they analyzed hazard scenarios based on the type of viral DNA used, how this might be released, and how it might gain access to the cells of a human or animal host. The complexity of this task resulted in part from the variety of types of animal virus (DNA versus RNA; segmented versus nonsegmented; single stranded versus double stranded), their mode of replication and their mode of action in a host organism.

In some cases, there was general agreement that the production of hazards was virtually impossible. For example, it was agreed that, because of the differences between the genetic regulatory machinery of bacteria and higher organisms, particularly the inability of bacteria to splice out the recently discovered intervening sequences in animal virus DNA, bacteria carrying whole viral genomes would be unable to make infectious viral particles and so would be unable to provide a new route of viral infection.

Other possibilities were not so easily eliminated, however. For example, if the gene for a viral coat protein were introduced into bacteria, and if the bacteria made this protein, would human hosts exposed to these bacteria become tolerant to the virus and unable to raise an appropriate immunological response? Or, if an entire DNA copy of an RNA virus such as polio were inserted into a bacterium, could the bacterium produce the intact virus, and thus provide a new route of transmission? Or if a gene known to be responsible for tumour formation were inserted into *E.coli* bacteria and if the bacteria colonized the gut and later died and released this DNA, would this tumourigenic DNA transform exposed cells and cause tumours?

The transcript shows that it was impossible for this group to eliminate such scenarios on theoretical grounds, although the probabilities were generally considered to be low. One participant summarized the conclusions of the group as follows:

> There were certain things that just molecularly seemed [as if] . . . they could not happen, according to our present knowledge of animal viruses . . . And, so, these were of no concern even if . . . what we know about the safety of K12 and the implausibility of each step for transfer broke down . . . And there were others where we felt a little bit more uneasy because we could conceive of proteins being expressed or whole viruses being reconstituted, if all the biological safety mechanisms broke down, which is extremely unlikely but not inconceivable. And those I suppose are the sarc genes and similar genes of oncogenic viruses, and I would include whole genomes of positive strand viruses in the same category —things where they can, conceivably, be reconstituted, if all the safety mechanisms that we've built in, biological safety mechanisms, broke down, and if there was full expression, and so on.[45]

The tenor of these discussions also shows that at many points, predictions were speculative. Too little was known about the mechanisms of viral infections and transformation to be able to predict the effects of cloning these genes. As one participant remarked:

> We do not know that a certain gene product of Marburg or Lassa [virus] is, in fact, highly toxic and is not responsible for the extraordinary . . . pathogenicity of this virus. So, if you had one of these genes making a protein product, I am not sure that I would be willing to say today that it should be reduced to P2. I mean, that is something that we simply do not know.[46]

As another participant summed up the essential problem of making these assessments:

> You see, the whole discussion has [the feeling of] a sort of Aristotelian academy because we are really just discussing extremely theoretical things and we're deriving models which are based on no experiments whatsoever. I mean, that's why we're talking so much.[47]

A further issue that emerged during the meeting was that if the high physical containment levels required by the 1976 NIH controls were lowered, access to the cloning of viruses would be greatly increased, and containment barriers would be more likely to be broken. As one participant stated:

> If there's any concern at all in allowing [the genes of higher organisms or animal viruses] to get into the general environment, you can be sure that if these K12 organisms carrying the clones are generally available in all labs, that they will get out, that they will be mobilized into other strains sooner or later.[48]

As at the earlier meetings, what eventually neutralized concerns such as these was a shift in focus away from hazards to individuals in the laboratory to the issue of secondary spread in communities outside the laboratory. The shaping of the Ascot assessment is most evident in the final day of the meeting when the group drafted a summary statement which purported to represent a consensus. Among other things, the draft referred to the conference's recognition that clones of bacteria carrying certain types of viral DNA might 'bypass the natural barriers to infection by the virus particle, because it is a conceivable, but extremely remote possibility that all the biological containment barriers might break down'.[49]

This draft was energetically resisted by some who argued that the Falmouth conference had shown that no hazards would materialize under *any* circumstances. Here is the exchange between Wallace Rowe and Harold Ginsberg, chairman of the Department of Microbiology at Columbia University College of Physicians and Surgeons:

> *Ginsberg*: I have one concern, if you'll pardon the expression. When this report becomes public and you talk about the breakdown, the simultaneous breakdown of biological barriers, and . . . when you're before Senator Kennedy's committee and he asks you what does that mean, and then he relates it all the way back to all other recombinant DNA [scenarios] what do you answer? What is this simultaneous breakdown of biological barriers?
> *Rowe*: The transfers [to other enteric bacteria] that have selective advantage . . .
> *Ginsberg*: Yes, but you see, the whole Falmouth meeting said that couldn't occur, and yet you don't make that explicitly clear that this can't occur. You say it can occur and everything up to this moment in history has said no . . .[50]

Some at the meeting challenged Ginsberg's position. 'If we ignore [hazards to laboratory workers], we end up looking like a bunch of virologists with a completely callous and unrealistic approach to human error,' one person commented.[51] Nevertheless, Ginsberg's position, with its emphasis on the Falmouth result, eventually prevailed. As one of the European participants later commented:

> The trouble with the Ascot meeting was that the moment one raised a scenario, one would be shouted down by [those] saying that the Falmouth meeting had said that the clones were not mobilizable, that they could never get out of *E.coli* K12 or χ1776, and could not become an epidemic strain.[52]

The final 'consensus' statement which appeared in the *Federal Register* in March 1978 finessed the issue of hazards to laboratory workers and focused attention on the question of hazards to the community. The latter, the report emphasized, were 'so small as to be of no practical consequence'.[53]

The overwhelming impression produced by the report on the Ascot conference was one of reassurance. Almost all hazard scenarios were considered 'remote,' 'most unlikely,' or 'impossible'. In general, it was concluded that the cloning of viral DNA would 'pose no more risk than work with the infectious virus or its nucleic acid and in most, if not all cases, clearly present less risk'.[54] Since the sole risk assessment experiment designed to test the hazards of cloning viral DNA, the Rowe-Martin polyoma experiment, was a year away from yielding results, these conclusions were surprisingly emphatic. The scientific community's response to earlier fears about the cloning of viral DNA had come to be essentially an attempt to tell the public they had nothing to fear.

Dissemination of Results

At the end of March 1978, the Falmouth report was still unpublished and the Ascot report was available only in the *Federal Register*. Details of the proceedings of both meetings were thus only known to the small groups of scientists who participated directly in these meetings. The standard methods of communication with the larger community of biologists — publication of the proceedings and discussion of their assumptions and results at open scientific meetings — were not used. (The full report of the Falmouth meeting was not published until May 1978; the Ascot report was never published in a

scientific journal.) Instead, the form of dissemination of the results of these meetings provided a third mechanism for 'social filtration' of their content.

In the case of the Falmouth conference, the Gorbach summary focusing on the 'epidemic pathogen' argument was widely circulated in the summer of 1977. Several national newspapers covered the Falmouth story and relied on the Gorbach summary for information. The message in almost all of these accounts was the same: researchers at Falmouth had 'unanimously concluded that the danger of runaway epidemics [was] virtually nonexistent'.[55] Recombinant DNA technology would generate 'No Sci-Fi Nightmare After All,' as the headline in the *New York Times* put it.[56]

This view of the Falmouth results quickly achieved scientific respectability. Its influence can be seen in the extensive lobbying effort against legislation aimed at regulating recombinant DNA research that influential sectors of the biomedical research community mounted at this time. The 'epidemic pathogen' argument proved to be a crucial tool in this campaign. For example, it was used at an important meeting in July 1977 between Senator Edward Kennedy and representatives of scientific societies with more than 500,000 members.[57] The purpose of the meeting was to convey the biomedical research community's reservations about Kennedy's proposed recombinant DNA legislation. (Significantly, Kennedy later cited 'new evidence' on recombinant DNA hazards as a reason for his withdrawal of legislation.) The argument was also used by Philip Handler, President of the National Academy of Sciences, Paul Berg, one of the leading pioneers of recombinant DNA technology and chairman of biochemistry at Stanford University, and Donald Fredrickson, the NIH Director, at hearings on the need for regulation of the recombinant DNA field before the Senate Subcommittee on Science, Technology and Space in the fall of 1977.[58] In August 1977, an editorial in *Science* which cited the Gorbach summary at length conveyed the view that the risks of work with *E.coli* were now deemed to be minimal. This extensive use of the 'epidemic pathogen' argument by leaders of the biomedical research community strongly reinforced the sense of its validity.[59]

The Ascot and Falmouth results were disseminated even further at international meetings which addressed the scientific and practical implications of genetic engineering. In these forums, the 'epidemic pathogen' argument tended to be generalized into the much more extensive claim (implicit in the *New York Times* headline quoted

above) that recombinant DNA technology posed no significant hazards at all. One such meeting was held in Milan in March 1978, sponsored by the World Health Organization and the Fondazione Giovanni Lorenzini. At this meeting, scientists and industrialists involved in recombinant DNA research and development repeatedly assured the audience that the hazards of recombinant DNA technology were no longer significant. Irving Johnson of the Eli Lilly Company claimed that there had been 'a steady and persistent decline in concern by informed and participating scientists for any biohazards'.[60] Molecular biologist Waclaw Szybalski claimed that the cloning of 'practically any DNA fragment in *E.coli* K12' posed 'no significant risk'.[61] And John Tooze — who undoubtedly was seen as an informed participant from the Ascot meeting — insisted that recombinant DNA was 'no more hazardous than many other, now routine, biological techniques whose development — unheralded by well-meaning but nevertheless alarmist public statements by those who invented them — rightly excited no concern amongst the general public and entailed no dangers for it'.[62]

Claims such as these went virtually unqualified. The Falmouth conference discussions were cited without reference to the conference's call for further assessment of the hazards of work with *E.coli*. The Ascot results were cited without reference to any need to investigate further any aspect of the cloning of viral DNA or to the fact that the Rowe-Martin experiment had yet to yield results. As a writer for the British science journal *Nature*, reporting on the Milan conference, observed: 'One must now accentuate the positive. The new evidence, however, does not seem substantial: those at Milan witnessed some unseemly clutching at straws'.[63]

Finally, the new 'consensus' solidified as it was used repeatedly in official reports and statements to justify weakening controls for recombinant DNA technology. A report prepared in March 1978 by the House Subcommittee on Science and Technology emphasized that 'the immediate benefits of recombinant DNA research appear to be more imminent than the risks which have been hypothesized' and that 'types of research currently permitted under the recombinant DNA research guidelines do not seem to pose significantly greater risks than natural diseases routinely confronting the medical community; in many instances they appear to present less risk'.[64] That a programme of hazard assessment was still a year away from being launched seems not to have bothered the authors. By the time the NIH Director appeared before his recombinant DNA advisory

committee in April 1978 to argue that the burden of proof on recombinant DNA hazards should shift from those who wished to promote the technology to those who wished to restrict its use,[65] the Falmouth and Ascot results had been raised to the status of fully sanctioned scientific generalizations.

Conclusions

There is a direct line of development from the Enteric Bacteria meeting in August 1976 to the new consensus on recombinant DNA hazards that emerged in 1978. Having discovered that their very success in achieving a powerful technique for producing novel substances and organisms had led to two distinct threats to their free pursuit of these methods (costly containment procedures and regulation of research), molecular biologists closely associated with the National Institutes of Health and the biomedical research establishment organized a defence of their interests. They brainstormed (under Rowe and Martin) extensively about what *could be perceived* as potential hazards of their techniques; they came up with a way of approaching the hazards question which could convince both the public and a fair number of their colleagues that their research was not at all dangerous; and at the Falmouth and Ascot meetings they succeeded in carrying the day with their approach.

The dominant image of these meetings, as portrayed in press coverage and in official reports, is one of 'scientific' meetings with 'scientific' agendas. In fact, this analysis has shown that a principal motive for the meetings was the protection of biomedical research from external regulation. At the Enteric Bacteria meeting, the most private of the three, this motive was made quite clear, and numerous comments — with no explicit dissent — suggest that all of the participants accepted it. The same disparity between image and motive characterized the Ascot meeting. It was 'an entirely scientific, analytical process,' Rowe later asserted. However, others at the meeting disagreed. 'It was very obviously a political meeting,' one of the European participants later recalled.

> The science was not too bad but I had a strong distaste for the way it was managed ... We were being used in the name of being a disinterested group of virologists but it was fairly clear by the end of the meeting that [the organizers] wanted to go back with a result that could be exploited for deregulation.

In the achievement of this consensus, the available scientific data were rarely in question: 'the science was not too bad'. The analysis was politicized at a different level — namely, through the introduction of restrictive assumptions which allowed a selective and reassuring interpretation of these data. The persistent focus on the question of the conversion of *E.coli* K12 into an epidemic pathogen allowed other considerations to be factored out.

It may be argued that the 'scientific consensus' arrived at in the late 1970s has in fact been borne out by the experience of the mid-1980s: a great deal of virtually unregulated cloning has been conducted in laboratories all over the world, in countries without modern sewage systems, and using many bacterial hosts — by no means only the weak *E.coli* K12. As far as we can tell (and this is a critical qualification because there has been virtually no organized effort to find out) none of this activity has resulted in hazardous biological agents with at least short-term effects.

But the refusal of the scientific establishment in the United States to call for hard experimental evidence that recombinant DNA research would not produce pathogenic substances or organisms, and the alacrity with which biomedical researchers in general rallied round to promote to the public results of brainstorming sessions as 'new evidence', both suggest that the most immediate concern of biomedical researchers at the centre of the cloning controversy in the 1970s was neither public safety nor scientific rigour. In fact, the history of the controversy indicates something entirely different: the insistence of research scientists that their freedom of investigation take precedence over the competing needs of the public and of laboratory workers.

• NOTES

This paper will form the substance of a chapter in *Molecular Politics: The Development of Policy for Recombinant DNA Technology in Britain and the United States*, forthcoming from the University of Chicago Press in 1987. An earlier version was presented at the XVIIth International Congress of History of Science, Berkeley, 1985. I would like to thank Robert Sinsheimer and Bruce Levin for their comments on drafts. Research for the paper was supported by NSF Grant No. SES 78-26618.

1. See, e.g., H.M. Collins (ed.), 'Knowledge and Controversy: Studies in Modern Natural Science', Special Issue of *Social Studies of Science*, Vol. 11, No. 1 (February

1981). The phrase 'social permeability' is taken from an article in this issue by Trevor Pinch, 'The Sun-Set: The Presentation of Certainty in Scientific Life', 131.

2. See, e.g., US House of Representatives, Committee on Science and Technology, Subcommittee on Science, Research and Technology, *Science Policy Implications of DNA Recombinant Molecule Research* (Washington, DC: US Government Printing Office, 1978), ix.

3. I do not examine here events leading up to scientists' perceptions of recombinant DNA hazards before 1976 or the further development of scientific consensus after 1978 in the United States, the United Kingdom, and other countries. For a detailed analysis of technical arguments used in the course of the controversy, see Sheldon Krimsky, *Genetic Alchemy: The Social History of the Recombinant DNA Controversy* (Cambridge, Mass.: MIT Press, 1982). The relation between the evolving scientific consensus and the wider social context from 1972 to 1982 is examined in my forthcoming study of the development of policy for recombinant DNA technology in the United States and Britain.

4. The main evidence used here comes from the meetings themselves. The first and third meetings are recorded in lengthy transcripts, neither of which, unfortunately, is entirely complete. The transcript of the first meeting lacks the names of speakers. Of the transcript of the third meeting, which amounted to roughly 1200 pages, about 600 survive. The remainder have been destroyed. Nevertheless, what remains — much of the record of the first and last days of the meeting — is certainly more than enough to establish some general characteristics of the meeting. The third meeting also resulted in a published report. The second meeting produced a summary report and published proceedings. The written record has been supplemented where necessary by interviews with participants at the meetings. The arguments developed at the Falmouth meeting are analyzed by Krimsky, op. cit. note 3, Chapter 16. However, the social and conceptual relationships between the Falmouth, Ascot, and Bethesda meetings have not been examined. Nor have the transcripts of the Bethesda and Ascot meetings been analyzed.

5. There were certainly other developments that influenced this consensus as well. For example, J. Jelsma and W.A. Smit, in 'Risks of Recombinant DNA Research: From Uncertainty to Certainty' (to be published in *Impact Assessment Today*), discuss the views of microbiologist Roy Curtiss and the results obtained by molecular biologist Stanley Cohen purporting to show that genetic engineering occurred spontaneously in nature. However, it seems reasonable to assume that the *collective* positions developed at the Falmouth and Ascot meetings eventually had a larger impact than the positions of individual scientists. (In addition, Curtiss attended the Falmouth meeting and contributed to the final report.) The frequency of citation of the reports of these meetings also supports this conclusion.

6. See, e.g., Office of the Director, NIH, 'Background on the Proposed Revisions (9/27/77) of the NIH Guidelines for Research Involving Recombinant DNA Molecules' (November 1977), 29–34; Department of Health, Education, and Welfare, National Institutes of Health, 'Recombinant DNA Research: Revised Guidelines', *Federal Register*, Vol. 43, No. 247 (22 December 1978), 60086–87; and sources cited below, notes 57, 58 and 59.

7. See, e.g., *Science Policy Implications of DNA Recombinant Molecule Research*, op. cit. note 2, vii; National Institutes of Health, Minutes, Meeting of Recombinant DNA Molecule Program Advisory Committee, 27–28 April 1978, 3 (remarks of NIH Director, Donald Fredrickson).

8. H.M. Collins, 'An Empirical Relativist Programme in the Sociology of Scientific Knowledge', in K.D. Knorr-Cetina and M. Mulkay (eds), *Science Observed: Perspectives on the Social Study of Science* (London: Sage Publications, 1982), 95–96.

9. See, e.g., P. Bachrach and M. Baratz, 'Two Faces of Power', *American Political Science Review*, Vol. 56 (1962), 947–52, and 'Decisions and Non-Decisions', *American Political Science Review*, Vol. 57 (1963), 632–42; R. Cobb and C. Elder, *Participation in American Politics: The Dynamics of Agenda-Building* (Baltimore, Md: The Johns Hopkins University Press, 1972); F. Frey, 'Comment: on Issues and Non-Issues in the Study of Power', *American Political Science Review*, Vol. 65 (1971), 1081–101; M. Crenson, *The Unpolitics of Air Pollutions* (Baltimore, Md: The Johns Hopkins University Press, 1971); E.E. Schattschneider, *The Semi-Sovereign People: A Realist's View of Democracy in America* (New York: Holt, Rinehart and Winston, 1960); Robert Dahl, *A Preface to Democratic Theory* (Chicago: The University of Chicago Press, 1957).

10. Dahl, op.cit. note 9, 72–73; Schattschneider, op.cit. note 9, 35.

11. Schattschneider, Ibid., 71.

12. Bachrach and Baratz, op. cit. note 9, 948.

13. The support of university administrators and biomedical researchers for NIH controls of the policy process is reflected in the strength of their opposition to the transfer of responsibility to another body: see, e.g., N. Wade, 'Gene Splicing: Senate Bill Draws Charges of Lysenkoism', *Science*, Vol. 197 (22 July 1977), 348–50; B. Culliton, 'Recombinant DNA Bills Derailed: Congress Still Trying to Pass a Law', *Science*, Vol. 199 (20 January 1978), 274–77; D. Dickson, 'Friends of DNA Fight Back', *Nature*, Vol. 272 (20 April 1978), 664–65.

14. Interview with Malcolm Martin, 22 November 1983.

15. Quoted in Krimsky, op. cit. note 3, 216.

16. Unpublished manuscript circulated in 1978.

17. Enteric Bacteria Meeting, 31 August 1976, Transcript, 2, 39–40, 42.

18. US–EMBO Workshop to Assess the Containment Requirements for Recombinant DNA Experiments Involving the Genomes of Animal, Plant, and Insect Viruses, 27–29 January 1978, Ascot, England, Transcript, 16.

19. Enteric Bacteria Meeting, Transcript, op.cit. note 17, 6.

20. Ibid., 44.

21. Ibid., 3.

22. Ibid., 15.

23. Ibid., 32.

24. Ibid., 34.

25. Ibid., 40.

26. Ibid., 43.

27. Ibid., 43–44.

28. Ibid., 44.

29. Ibid., 44–45

30. Ibid., 45.

31. National Institutes of Health, Recombinant DNA Advisory Committee, Minutes of Meeting, 13–14 September 1976, 9–10.

32. 'Risk Assessment of Recombinant DNA Experimentation With *Escherichia Coli* K12' (Proceedings of a Workshop Held at Falmouth, Massachusetts, 20–21 June 1977), *The Journal of Infectious Diseases*, Vol. 137, No. 5 (May 1978), 615–16.

33. For example, H. Williams Smith, 'Is It Safe to Use *Escherichia Coli* K12 in Recombinant DNA Experiments?', ibid., 655–60.

34. See Rolf Freter, 'Possible Effects of Foreign DNA on Pathogenic Potential and Intestinal Proliferation of *Escherichia Coli*', ibid., 624–29. Freter noted that 'feeding experiments tell us little about the ability of bacteria to grow in the human intestine. It has been known for some time that the feeding of bacteria, especially those grown under the usual laboratory conditions, rarely results in implantation because the normal intestinal flora is antagonistic to the growth of invaders.'

35. Samuel B. Formal and Richard B. Hornick, 'Invasive *Escherichia Coli*', ibid., 641–44.

36. Later research carried out with the benefit of more precise knowledge of these properties and with the use of genetic manipulation techniques showed that the 'invasiveness' of *E.coli* K12 could be substantially enhanced: see Sansonetti, Copecko and Formal, *Infection and Immunity*, Vol. 34 (1981), 75, and Vol. 35 (1982), 852.

37. See, e.g., E.S. Anderson, 'Plasmid Transfer in *Escherichia Coli*', *The Journal of Infectious Diseases*, Vol. 137, No. 5 (May 1978), 686–87.

38. Bruce Levin to Donald Fredrickson, 29 July 1977.

39. Jonathan King, 'Recombinant DNA and Autoimmune Disease', *The Journal of Infectious Diseases*, Vol. 137, No. 5 (May 1978), 663–66.

40. 'Risk Assessment Protocols for Recombinant DNA Experimentation', ibid., 704a–08.

41. Sherwood Gorbach to Donald Fredrickson, 14 July 1977.

42. Interview with Sydney Brenner, 6 May 1980.

43. Interview with member of GMAG, April 1980.

44. Paul Berg to Donald Fredrickson, 13 October 1977: in Department of Health, Education and Welfare, National Institutes of Health, *Recombinant DNA Research*, Vol. III (September 1978) (Washington, DC: US Government Printing Office, 1978), Appendix A, 12–14; John Tooze, Testimony, Meeting of the Advisory Committee to the Director, NIH on the Proposed Revision of the NIH Guidelines on Recombinant DNA Research, 15–16 December 1977, Transcript, ibid., 306, 351.

45. US–EMBO Workshop to Assess the Containment Requirements for Recombinant DNA Experiments Involving the Genomes of Animal, Plant, and Insect Viruses, 27–29 January 1978, Transcript, 1002.

46. Ibid., 1006.

47. Ibid., 284.

48. Ibid., 1013. See also 1010, 1009, 1016.

49. Ibid., 1039.

50. Ibid., 1050–51.

51. Ibid., 1061.

52. Interview with Robin Weiss, 18 June 1979.

53. *Federal Register*, Vol. 43, No. 63 (31 March 1978), 13749.

54. Ibid., 13751.

55. Victor Cohn, 'Scientists Now Downplay Risks of Genetic Research', *Washington Post* (18 July 1977).

56. *New York Times* (24 July 1977).

57. Peter R. Day, 'Kennedy Bill Unchanged by Scientists' Visit', *BioScience*, Vol. 27, No. 9 (October 1977), 594.

58. US Senate, Committee on Commerce, Science and Transportation, Subcommittee on Science, Technology and Space, *Regulation of Recombinant DNA Research* (2, 8 & 10 November 1977), 13, 37, 161.

59. Philip Abelson, 'Recombinant DNA,' *Science*, Vol. 197 (19 August 1977), 721.

60. Herbert Boyer and S. Nicosia (eds), *Genetic Engineering* (Proceedings of the International Symposium on Genetic Engineering: Scientific Developments and Practical Applications, Milan, 29–31 March 1978) (Amsterdam: Elsevier, 1978), 224.
61. Ibid., 254.
62. Ibid., 282.
63. Peter Newmark, 'WHO Looks for Benefits from Genetic Engineering', *Nature*, Vol. 272 (20 April 1978), 663–64.
64. US House of Representatives, Committee on Science and Technology, Subcommittee on Science, Research and Technology, *Report on Science Policy Implications of DNA Recombinant Molecule Research* (March 1978), 4–10.
65. Taped recording of eleventh meeting of the Recombinant DNA Molecule Program Advisory Committee, National Institutes of Health, 27–28 April 1978.

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