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IS SELF-REGULATION ENOUGH TODAY?: EVALUATING THE RECOMBINANT DNA CONTROVERSY

Charles Weiner*

THE RECOMBINANT DNA CONTROVERSY is sometimes cited today as relevant to current controversies about the control of new or proposed genetic technologies such as human cloning and human germ-line intervention. In the late 1970s, the concern was about the potential health and environmental hazards of novel research techniques that made it possible to manipulate genes, opening the path to genetic engineering. The researchers, their institutions, and their funding agencies developed a system of self-regulation to avoid hazards and to forestall legislative control. They focused on the means and not the ends; on the tools of genetic engineering rather than on the moral limits. I will outline the history of this process, with emphasis on aspects of it that are relevant to current concerns.2

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1 Paul Berg & Maxine Singer, Regulating Human Cloning, 282 SCIENCE 413 (1998) (asserting that anti-cloning legislation could inadvertently inhibit or delay research on diseases and the development of new therapies). See also Claire Nader & Stuart A. Newman, Human Cloning, 282 SCIENCE 1824 (1998) (disagreeing with the proposition that recombinant DNA research should be unregulated because of the biological and ethical issues raised by human cloning); Doris Teichler Zallen, The Public as a Partner at the Laboratory Bench, 12 TRENDS IN BIOTECHNOLOGY 107 (1994) (explaining how the current “vigorous state of health” of research into recombinant DNA is a result of the system of review developed in the mid-1970s, and how this is applicable to present-day issues arising due to advances in genetic technology).

2 This historical summary draws on my observations and documentation of the recombinant DNA controversy utilizing archival materials deposited in the Recombinant DNA History Collection available for study at the Institute Archives and Special Collections, Massachusetts Institute of Technology. See Charles Weiner, Anticipating the Consequences of Genetic Engineering: Past, Present, and Future, in ARE GENES Us? THE SOCIAL CONSEQUENCES OF THE NEW GENETICS 31 (Carl F. Cranor ed., 1994) (providing portions of this account). There are well-documented histories and analy-
At the Gordon Research Conference on Nucleic Acids in July 1973, invited specialists on DNA research heard fascinating reports of new techniques for manipulating and moving genetic material. The use of the newly discovered restriction enzymes made it possible to cut strands of DNA at specified, precise points and to insert them into the DNA of other organisms, combining the hereditary material of animals and bacteria. These recombinant organisms could be replicated in billions of copies through cloning. It was apparent to the scientists involved that they had a new tool for studying the structure and functions of genes and to probe the details of DNA and its transcription in cells of higher organisms. Biologists recognized that this would open up a new field of work, enabling the posing of fundamental research questions that would not have been feasible before, and to get answers that would help solve problems at the forefront of knowledge that would have important applications.

Amid the excitement about the potential of the new recombinant DNA techniques, some of the conference participants were alarmed over its possible hazards, not down the line when the applications were imminent, but in their own laboratories. They were concerned that the techniques might cause unforeseen hazards to human health and the environment. There was a possibility that harmless microbes could be unintentionally changed into human pathogens through the introduction of antibiotic resistance, which was part of the technique; through the production of dangerous toxins, which was a possible outcome; or through the transformation into cancer-causing agents of materials that previously were benign. In this relatively new field there was a great deal of uncertainty and little information about the risks.

The Gordon Conference participants asked for a special discussion of these larger questions. At that brief special session, they decided to write a letter to ask the National Academy of Sciences to study the potential hazards and to devise a plan to do something about them. They voted by a large majority to compose the letter

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and they approved the content of it. They also voted, this time by a slim majority, to send a copy of the letter to be published in the journal Science. The reluctance of many of the participating scientists to call public attention to the problem was an indication of a continuing conflict. They were concerned about a possible public health problem, and yet they feared that talking about it publicly might bring intrusion, as they saw it, into the scientific process. The Gordon Conference letter, replete with technical language, was intended for other scientists. It was published in the journal Science in 1973 and did not generate much public attention.

The National Academy asked Paul Berg, a distinguished biochemist and a principal researcher in the field, to organize a group of scientists to consider the issues. They met at Massachusetts Institute of Technology on April 17, 1974 and planned a conference for February 1975 to evaluate the hazards of the research and ways of dealing with them. Feeling a sense of urgency, they also drafted a letter to alert the larger community of biologists. Two months after the MIT meeting, Berg described these actions and the group's motivations in a letter to a colleague in England:

We met at MIT for a day and settled on the idea of calling a conference next February of those scientists working on methods of joining DNA molecules and particularly those involved in constructing hybrid DNAs. It was our plan that one of the major purposes of the Conference, besides a report on the scientific progress, would be a wide ranging discussion of potential hazards growing out of these types of experiments. Were there any experiments that should not be done? How could such a moratorium be proposed or enforced? In short, we expected a frank and searching review of what people were doing or wanted to do, particularly from the point of view of whether they should be done. But as we talked we realized that the pace of events might not wait for February and that some of the experiments many people would agree could be hazardous would be done (e.g., attempts to fuse portions of Herpes DNA to

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3 See KRIMSKY, supra note 2, at 70-80 (providing a detailed account of the development of the Gordon Conference letter).

4 Maxine Singer & Dieter Soll, Letter, Guidelines for DNA Hybrid Molecules, 181 SCIENCE 1114 (1973) (publishing letter sent to Phillip Handler, President of the National Academy of Sciences, and to John R. Hogness, President of the National Institute of Medicine).
appropriate plasmids for cloning in E. coli were imminent). Since the technology for constructing hybrids has become ridiculously simple, that fear was well-founded. Consequently we decided to devise a letter to be submitted to *Science* and *Nature* calling on scientists to defer certain kinds of experiments until these potential hazards could be better evaluated and certainly until there was an opportunity to discuss the issues at the February meeting.\(^5\)

Drafts of the Berg committee letter were circulated privately among the relevant scientists, and in July 1974, the final version was published in *Science*\(^6\) and *Nature*.\(^7\) Why did they go public? Because the committee felt it was the quickest way to bring the potential hazards to the attention of the community of researchers who would be likely to use the new recombinant DNA techniques. They felt that the situation was urgent because of pending experiments and because the power and fruitfulness of these research tools rapidly would attract many scientists to the field who were not experienced in handling pathogenic organisms. The letter called for a voluntary moratorium, a temporary deferral of those experiments which at the time were thought to be potentially hazardous. This appeal for self-restraint was linked to an end point, the conference scheduled for February 1975.

The February 1975 meeting at the Asilomar Conference Center in California evaluated knowledge in the field and its potential for research. It was the equivalent of an international review conference which ordinarily would be held well into the development of a research field and not at such a very early stage. The detailed review enabled the conference participants, who were the researchers and the potential researchers in the field, to consider the potential risks and ways to control them. The motive from the start was to avoid public interference and to demonstrate that scientists on their own could protect laboratory workers, the public, and the environment. Of course, there is that contradiction again: they

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\(^5\) Letter from Paul Berg, Chairman, Committee on Recombinant DNA Molecules Assembly of Life Sciences National Research Council, to Hans Kornberg (June 18, 1974), *available in Recombinant DNA History Collection*, supra note 2.


\(^7\) 250 *Nature* 175 (1974). The last paragraph was omitted in this version of the letter.
were dealing with a public health issue and were simultaneously attempting to keep the public out of it.

The organizers intended to exclude the press from the conference, but relented when a prominent science writer threatened to bring a Freedom of Information Act lawsuit against them since the meeting was funded by the National Institutes of Health. A deal was struck with sixteen individuals, most of whom were invited, that they would not report on the conference until it was over because things would be too much in flux. That pleased the reporters because they did not have to call in stories to their editors every day. Instead, the telephone booths were jammed with scientists calling their laboratories in Europe and the United States about the need to tool up for this very exciting new research. The conference gave them an opportunity to learn as much as possible about the recombinant DNA techniques, and it stimulated the growth of the field while producing a framework for pursuing it safely.

Several technical working groups met independently over a period of months in preparation for the conference. The most active was the Plasmid Working Group, focusing on the circular pieces of DNA which were the main tools for this new technique. They scoured the literature and their own knowledge, talked with other people in the field, and produced a very deep technical document. Reports of the working groups were presented and discussed at the meeting and one session was devoted to presentations of lawyers on policy and liability issues. Participants paid special attention to their legal responsibility for damage resulting from their laboratory work. The narrow technical focus of the conference was evident in the opening remarks of David Baltimore, one of the organizers. He first acknowledged that the techniques that were developed could have applications in a number of areas, including biological warfare, and that it had larger societal implications, but that such issues would be excluded since there was a full agenda of technical issues:

The issue that . . . [brings] us here is that a new technique of molecular biology appears to have allowed us to outdo the standard events of evolution by making combinations of genes which could be immediate natural history. These

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8 See PLASMID WORKING GROUP, PROPOSED GUIDELINES ON POTENTIAL HAZARDS ASSOCIATED WITH EXPERIMENTS INVOLVING GENETICALLY ALTERED MICROORGANISMS, (1975), available in Recombinant DNA History Collection, supra note 2.
pose special potential hazards while they offer enormous benefits. We are here in a sense to balance the benefits and hazards right now and to design a strategy which will maximize the benefits and minimize the hazards for the future.9

What happened at Asilomar? The recombinant DNA issue was defined as a technical problem to be solved by technical means, in essence, a technical fix. Larger ethical issues regarding the purposes and the long-term goals of the research were excluded, despite the rich discussions that had occurred among geneticists and other biologists in the 1960s about where to draw the line when it became possible to do genetic engineering. The 1960s discussions led to Congressional proposals for anticipatory study of the ethical limits of genetic engineering, which were resisted as premature by several leading biologists.10 Instead of those longer-term issues, the focus at Asilomar in 1975 was on safety of the newly developed technical tools for genetic engineering, on the means not the ends.

The Asilomar participants adopted provisional safety guidelines based on a two-part system of physical and biological containment of potentially hazardous recombinant organisms.11 The extent of physical containment was graded according to the anticipated level of hazard an organism might present if it escaped the laboratory, ranging from good laboratory technique for those experiments deemed to be of low hazard, to hooded glove boxes, negative pressure, and showers and clothes changes for laboratory workers dealing with organisms thought to be especially dangerous. Biological containment would introduce mutations in the organisms that were to be used in the experiments so that if they escaped they could not survive in the environment beyond the laboratory.

In November 1974, the National Institutes of Health had established the Recombinant DNA Advisory Committee (RAC), ad-

9 Audiotape of the International Conference on Recombinant DNA Molecules, Asilomar (Feb. 24, 1975) (on file with the Recombinant DNA History Collection).
10 Hearings on S.J. Res. 145 Before the Subcomm. on Gov't. Research of the Senate Comm. on Gov't. Operations, 90th Cong. (1968) (statement of Senator Walter Mondale) (introducing the idea that some scientific advances, such as gene manipulation, are potentially dangerous and must be looked at closely).
11 See Paul Berg et al., Asilomar Conference on Recombinant DNA Molecules, 188 SCIENCE 991 (1975) (explaining the measures adopted by the Asilomar participants).
visory to the Director of NIH. The first meeting was held immedi-
ately after the Asilomar conference at the end of February 1975. RAC appointees were knowledgeable researchers in the field who were asked to develop and extend the Asilomar provisional safety guidelines to control all recombinant DNA work at institutions receiving NIH funding of any kind. They were designing safety protocols that had the potential for restricting their own work. These controls were to be administered by the NIH, which funded and encouraged the research and, therefore, was itself in a position of conflict of interest. NIH officials acknowledged the potential conflict, and maintained that although NIH was not a regulatory agency, it had the best expertise in the field and needed to act in the absence of any other government group playing a role. Similar efforts were also underway in other countries.

During 1975 and 1976, scientists on the RAC argued about whether the proposed guidelines were too strict or too permissive, and the document went through many drafts. All of this occurred in the absence of risk assessment experiments. At the same time, scientists at laboratories throughout the country were tooling up to use the new technique and were impatiently waiting for the green light that would allow them to proceed as rapidly as possible. They exerted a great deal of pressure on the RAC and the NIH. The process of establishing safety rules involved a series of compromises aimed at achieving a consensus within that portion of the scientific community affected by the guidelines while providing assurances to the public that they would be protected from possible hazards.

The long-expected NIH safety guidelines for recombinant DNA were approved by the director of NIH on June 23, 1976. On that day when the green light flashed, an extraordinary event took place in Cambridge, Massachusetts. Scientists from MIT and Harvard and representatives of NIH appeared at a special City Council hearing. They had been invited to explain to the citizens of Cambridge why the scientists themselves had been arguing about the safety of recombinant DNA and whether the guidelines were ade-

12 The Guidelines process, including drafts and revisions, is documented by NIH in a series starting in 1976. See NATIONAL INSTITUTES OF HEALTH, RECOMBINANT DNA RESEARCH (1976) (providing a multi-volume series of annual compilations of documents relating to the NIH Guidelines for Research Involving Recombinant DNA Molecules). See also KRIMSKY, supra note 2 (discussing the process of developing ethical and technical guidelines for genetic engineering, including the Asilomar Conference and the actions of the RAC through the early 1980s); WRIGHT, supra note 2 (detailing accounts of the process including the Asilomar conference and RAC’s actions through the early 1980s).
quate to protect the communities in which the research was to be done. Was there any danger to citizens? Who was going to monitor and enforce the safety standards? Could the scientists and their universities be trusted to regulate themselves? Testimony by several biologists that recombinant DNA techniques posed few risks and that they could be contained by the new guidelines was countered by testimony from other biologists who argued that the guidelines were inadequate and that they were formulated by advocates of the research. After a second hearing in July 1976, the City Council established a citizens' review board to examine the problem and, pending the outcome of the board's deliberations, placed a temporary ban on experiments classified in the guidelines as posing moderate to major hazards.

The nine-member Cambridge Experimentation Review Board met twice weekly for a total of more than 100 hours over a four-month period. About one half of the time was used for testimony by scientists on both sides of the issue. The Board presented its report to the City Council on January 5, 1977, recommending the creation of a city biohazards committee to oversee adherence to the NIH guidelines for all recombinant DNA work in the city whether funded by NIH or not, and several additional safeguards on experiment procedures, containment, and testing of organisms. These community confidence-building measures were incorporated in a City Council ordinance passed in February 1977, which was the first recombinant DNA legislation in the United States and was interpreted as a qualified public endorsement of the NIH guidelines.

The major fear of the recombinant DNA scientists was that their own early concern about laboratory safety had initiated public scrutiny of the new research. This was emphasized by the events in

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14 For a critical examination of the praise for the Cambridge experience as a model for decision making and public participation with respect to recombinant DNA safety guidelines, see Rae S. Goodell, Public Involvement in the DNA Controversy: The Case of Cambridge, Massachusetts, SCI. TECH. & HUM. VALUES, Spring 1997, at 36 (describing a number of failures in the City Council's attempt to regulate recombinant DNA technology, such as control of the debate, underlying assumptions, and "composition of opposing sides" by the scientists and a lack of understanding of the technical issues by City Council members).
Cambridge and in other communities such as Ann Arbor, San Diego, New Haven, and Princeton where academic biologists were tooling up to use recombinant DNA techniques. By 1978, sixteen separate bills had been introduced in Congress to regulate recombinant DNA safety standards by making the NIH guidelines mandatory for both publicly and privately funded research and providing enforcement and punishment provisions for any violations. Research universities and scientific organizations saw this local and national activity as public "overreaction" threatening their control of laboratory safety procedures and their research funding. They vigorously lobbied to oppose or influence legislation. Several prominent biologists who had shared the early concern about possible safety hazards of the research publicly recanted, and a resolution to Congress signed by most of the participants in a 1977 Gordon Conference stated that they previously had overstated the risks and now could provide reassurance that the work was safe. In the end, no legislation was passed by Congress.

By 1979, the NIH Recombinant DNA Guidelines had been made far more permissive than the original 1976 version. More than ninety percent of U.S. research in the field was either no longer covered by the guidelines or was subject to only minimal controls equivalent to standard laboratory practice. By 1982, most experiments subject to the guidelines were controlled at the local level through institutional biosafety committees and RAC reviewed only research that had the potential for special safety problems. No demonstrated harm had been caused by the research as conducted under the guidelines. A limited amount of risk assessment research had been done during that period and several small consensus workshops of scientists in the field were held to review existing knowledge and to refute the earlier concerns. NIH's approach to the guidelines was that they would be flexible enough to respond to new scientific knowledge. That also opened them up to flexible response to pressures from researchers and their interests, pressures from industry, and pressures from national policy priorities and political interests.

15 The conference resolution opposing legislation was described in Walter Gilbert, Letter, 197 SCIENCE 208 (1977).
16 See generally WRIGHT, supra note 2, at 228-55 (examining three scientific meetings held in response to the recombinant DNA controversy); KRIMSKY, supra note 2, at 213-84 (discussing initiatives designed to reassess the hazards of DNA experimentation due to complaints of some scientists that overly restrictive guidelines were, perhaps, unnecessarily burdening researchers).
Downgrading of the guidelines coincided with rapid commercialization of the field and the involvement of academic scientists in biotechnology companies. In November 1974, during the moratorium period, a patent for the recombinant DNA technique was filed by Stanford University and the University of California on behalf of two of the scientists who developed the technique. The patent was granted in 1980 after the Supreme Court decision allowing patenting of human-made organisms.\(^{17}\) Biologists and their universities became involved in what soon became an almost complete commercialization of the work. In the 1980s' political climate of deregulation, the U.S. biotechnology industry was promoted as a national priority. Emphasis was on government, industry, and media claims of medical, practical, and economic benefits of the research and the need to develop the industry. Critical questions about the health and environmental safety of research techniques and products were met by arguments that if the United States did not move forward rapidly in biotechnology, the country would lose out in international competition. The "gene gap" argument was deployed to resist special regulation of the field.

As the guidelines faded away for most laboratory work, attention shifted from the accidental escape of genetically engineered microorganisms to the intentional release of these organisms into the environment for agricultural purposes. The United States Department of Agriculture (USDA) and the Environmental Protection Agency (EPA), the agencies who would ordinarily become involved, initially claimed that they did not yet have the expertise to evaluate the possible hazards and they urged NIH to provide safety oversight for these applications through the RAC. Evaluation by the RAC seemed like a very comfortable approach for scientists and companies who had been working with it. In the absence of federal legislation for recombinant DNA, industry had been in voluntary compliance with the NIH guidelines. It was not until 1984 that the EPA issued an interim policy statement on field testing of genetically engineered microbial pesticides. By that time NIH had approved proposals for small-scale field testing of a genetically modified organism that was to be sprayed on strawberry and potato plants to prevent frost damage. The "ice-minus" controversy of the mid-1980s involved approvals by NIH, EPA, and California agencies, legal challenges by genetic engineering critic Jeremy Rifkin,\(^{17}\)

congressional hearings, and protests and demonstrations by citizens in the community where field testing was to occur. As in Cambridge several years earlier, the citizens asked, "Why are we the last to know?" The test plot was definitely in their back yard, but they were not informed of its exact location. They were also concerned about unresolved safety questions raised by ecologists. By the time the tests were finally conducted in 1987, RAC's role in the approval of the environmental release of genetically modified organisms had been superceded by the EPA.18

The RAC also played a transitional role in the oversight of experiments in human gene transfer, generally referred to as gene "therapy" to reflect the, as yet, unrealized hopes of its advocates. In 1983, the RAC responded to the report of the President's Commission on Bioethics' study of genetic engineering which considered several approaches to the oversight of future human genetic engineering. The commission's study was initiated after the leaders of the three major U.S. religious groups wrote a letter to the President stimulated by the 1980 Supreme Court decision permitting patenting of genetically engineered organisms. They called for the study of the ethical issues associated with genetic engineering and observed that "no government agency or committee [w]as currently exercising adequate oversight or control, nor addressing the fundamental ethical questions in a major way."19 RAC's response to the Commission's report was to establish a Working Group to consider procedures for reviewing proposals for human gene therapy. In 1985, RAC's "Points to Consider in the Design and Submission of Human Somatic Cell Therapy Protocols" was issued by NIH. They said they would be willing to review proposals for human gene transfer protocols for somatic cells, but would not "at present entertain proposals for germ-line interventions."20


19 President's Commission for the Study of Ethical Problems in Medicine and Biomedical and Behavioral Research, Splicing Life: A Report on the Social and Ethical Issues of Genetic Engineering With Human Beings 96 (Nov. 1982) (providing a reprint of a letter from three general secretaries of national religious organizations, dated June 20, 1980, posing questions and answers regarding the dangers and benefits that may result from the growth of genetic engineering).

20 For accounts of the development of RAC's role in human gene transfer, see LeRoy Walters & Julie Gage Palmer, The Ethics of Human Gene Therapy, 148-51 (1997) (showing the role of the RAC's Working Group in guiding researchers with human gene therapy); see also Ira H. Carmen, Debates, Divison, and Decisions: Recombinant DNA Advisory Committee (RAC) Authorization of the First Human
pressed by the Council for Responsible Genetics to specifically ban human germ-line engineering, the committee refused. Leroy Walters, the bioethicist who had been for many years a member of RAC and was the head of its human gene therapy subcommittee, subsequently argued that voluntary programs of germ-line genetic intervention are "ethically acceptable in principle."

The first human gene transfer proposal was received by RAC in April 1988 and about 100 proposals were reviewed and approved by 1995. Gene therapy became the primary task of the group, and since then it has dealt with the scientific validity of proposals as well as risks for human subjects, the adequacy of informed consent, the role of local institutional review boards, and the liability of researchers. RAC nurtured the development of human gene therapy by applying the clinical standards of biomedical ethics, but bypassed the larger ethical issue of whether it should be done at all. The role of RAC remained an advisory one to the director of NIH. In 1995, the Food and Drug Administration became the regulatory oversight agency for human genetic engineering, with RAC playing an advisory role in reviewing proposals involving novel techniques or applications.

Throughout RAC's history - from its creation in 1974 to deal with initial concerns about laboratory safety to its current role in human gene transfer experiments - it has been friendly to re-

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Gene Transfer Experiments, 50 AM. J. HUM. GENETICS 245 (1992) (arguing that to understand how the RAC should proceed in orchestrating a human gene therapy policy agenda, competing vantage points must be analyzed).

21 WALTERS & PALMER, supra note 20, at 91 (evaluating the ethics of allowing tests of germ-line genetic intervention with humans).

22 For the debate on what role RAC should play in human gene transfer review, see Henry Miller, Bureaucrats' Bloat in the Laboratory, WALL ST. J., May 10, 1994, at A-18 (explaining the duplicative reviews that gene therapy clinical trials must undergo, including approval by two separate NIH committees which includes the RAC); see also Leonard E. Post, Editorial, RAC's Review of Gene Therapy: It's Time to Move On, 5 HUM. GENE THERAPY 1311, 1312 (1994) (arguing that while RAC was "an enormously successful instrument of public policy" because it allowed gene therapy to gain public support with minimal controversy and provided greater flexibility regarding oversight than legislation or Agency regulations, RAC's oversight of gene therapy is now outdated); Sheldon Krimsky, Editorial, Response to Editorial by Leonard E. Post, 5 HUM. GENE THERAPY 1313, 1314 (1994) (arguing that regulatory agencies like the FDA are not in a position to address the ethical decisions regarding human genetic engineering because they arguably do not have the necessary independence, public trust, or ability to have intense public scrutiny currently afforded by RAC); Doris T. Zallen, Public Oversight Is Necessary if Human Gene Therapy Is to Progress, 7 HUM. GENE THERAPY 795, 796 (1996) (supporting the RAC's efforts to develop guidelines regarding the use of recombinant DNA research techniques).
searchers and dominated by their interests. At the same time, the work of the committee has been relatively open and visible. NIH made efforts to create a full public record of the RAC deliberations and documents in addition to the announcements of meetings, proposed changes in the guidelines, and decisions required to be published in the *Federal Register*. However, very few citizens read that relatively inaccessible, small-print publication. Nor do many people have the opportunity to travel to Bethesda to sit in on committee meetings. The RAC minutes list the non-committee members who attended the meetings. As the commercialization of genetic engineering increased from 1980 forward, the record shows that representatives of companies were consistently present to follow the deliberations and look after their interests.

Public participation on the RAC was broadened in 1978 in response to complaints that it was dominated by self-interested researchers. Yet there were built-in limits and constraints to this participation because most of the issues placed before the committee were technical and were often beyond the expertise of the non-scientists. Another problem was that RAC was increasingly asked to review industry proposals. Biotechnology companies were in voluntary compliance with the guidelines and sought NIH approval for their recombinant DNA work with the condition that proprietary information would be kept confidential, as was the practice with federal regulatory agencies, even though NIH was a research-supporting agency. As a result, public representatives on the committee frequently were not able to report to the public about information relevant to environmental and public health.

The development of genetic engineering clearly involves more than the safety issues that have been the major focus of RAC's mandate and activities. The larger ethical concerns about where to draw the line in applications of genetic engineering were occasionally discussed when raised by some members of the committee or at the request of outside groups. RAC, however, resisted taking a stand against the use of recombinant DNA techniques for biological warfare and refused to recommend an unambiguous ban on the review of proposals for human germ-line intervention. Instead, RAC's emphasis was to develop safe procedures for the research, focusing on how to do it rather than whether it should be done.23

23 An example of RAC's resistance to efforts by public interest groups to broaden its approach is documented in the minutes of the January 30, 1989 meeting. *Recombinant DNA Advisory Comm., Nat'l Institutes of Health, Minutes of Meeting 19-37* (1989).
As Leon Kass observed in 1997, "the piecemeal formation of public policy tends to grind down large questions of morals into small questions of procedure." Recombinant DNA research was safer as a result of the NIH guidelines developed by RAC. The biologists at Asilomar in 1975, and the subsequent generations of RAC members, raised important safety issues and set standards for good laboratory practice.

Despite the success in improving the safety of research, the quasi-self-regulation model developed in the recombinant DNA controversy is not adequate for expressing and enforcing societal and moral limits for potential genetic engineering applications such as human cloning or human germ-line interventions. These potential applications are not inevitable and they raise profound issues beyond laboratory and environmental safety and patients' rights. They occur in a context of increasing genetic determinism, pervasive commercialization, and aggressive efforts to sell genetic intervention as a cure-all for medical and even social problems. Separation of the technical issues from the ethical issues, and the narrowing of ethical concerns to clinical biomedical ethics limits meaningful public involvement, and obscures the larger picture.

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