SCIENCE AND THE PUBLIC INTEREST

Recombinant DNA Research

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PARADIGMS AND POLITICS: THE ROOTS OF CONFLICT OVER RECOMBINANT DNA RESEARCH

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Introduction

Conflicts within the community of scientists are disorienting to the public. By and large people still harbor the notion that science is fully rational and that scientists share a responsibility to filter out personal bias and set aside vested interests in the execution of their craft. If we eliminate these exogenous factors what remains should be decision guided by principles of reason. It is on this basis, so the lay public is informed, that scientists can reach universal agreement on purely objective considerations.[1] The exceptions to these standards of professional responsibility are easily explained away as abberations.[2]

The fact that science has been used synonomously with rationality does not imply that conflicts within science are not rational.[3] Some philosophers, notably Karl Popper, have identified conflict as the motive force of scientific discovery.[4]

Ordinarily, the public gets very little opportunity to observe the internal debates carried on between members of the scientific community. When these disputes do reach the citizenry they are modulated by the media and consequently subject to considerable dilution. I had the unusual opportunity to be such an observer when, as a representative of the Cambridge (Mass.) community, I served on a citizen review board that was established to advise city officials on the risks of recombinant DNA research. I came to the citizen board with a special

interest in the anatomy of conflict. One of the first questions I asked was: Why after several years of debate is there a sustained disagreement among scientists over the hazards of recombinant DNA research? Is it strictly a technical controversy over the validation of scientific results, its laws and theories, or the appropriate data base from which extra polations are made? Are there perhaps extra-scientific factors that creep into the technical side of the debate?

The answer to this question has implications for the appropriate form of public participation in scientific controversies that have a bearing upon human welfare. If the risk-related issues of recombinant DNA research were easily divisible into technical and policy components, then it is justifiably argued that the appropriate entry point in the decision-making process for the public interest is in the policy sphere. On the other hand, if political and other extra-scientific factors cannot be isolated from the technical debate--a Gordian Knot of politics and paradigms--there is no obvious justification for excluding representatives of the public interest at any level of decision-making on issues of risk assessment and biohazards control.

My principal theme in this paper is that in the recombinant DNA controversy the scientific judgments related to the identification and assessment of risk cannot be fully understood in isolation from the socio-political context and motivations of those engaged in the risk assessment. Furthermore, the technical judgments which gave rise to the NIH Guidelines were shaped very early in the controversy by values and decisions of a non-technical nature. These factors were obscured from the public by proponents of the research in their overzealous attempt to characterize the internal decision-making process as fully rational and consonant with the public interest.

I shall discuss several critical areas where extra-scientific decisions guided the outcome of the risk assessment and the subsequent portrayal of the

issues to the public. Specifically these are: (1) the initial problem definition, (2) rationalizing dissident views to the public, (3) choice of the host organism, (4) taxonomy of risk, and (5) emergence of novelty.

The conclusions of the paper stand in direct opposition to the recent counterattacks launched against those who are promoting a more open system of decision making in the sciences. The key issues are whether there should be pluralism and advocacy in the process of risk assessment or whether it should be kept a closed process for those members of the scientific hierarchy. Those who advocate a closed system contend that it is a more rational process for providing the optimum conditions of objectivity.

Recently, the president of Columbia University was reported as saying:

> The adversary method for arriving at truth on which our legal procedures are based is, in simple language, not appropriate for arriving at sound public policy on scientific matters. Scientific questions simply cannot be settled by persuasive argument. The only effective method for resolving safety questions on nuclear or biological research is the objective analysis of results by our best scientific minds.[5]

If my arguments are sound then the grounds for justifying a closed system are substantially weakened.

Defining the Problem

As experienced social scientists are aware, a problem definition contains the seeds of its solution.[6] Similarly, in regard to the risk assessment of recombinant DNA research, the outcome at Asilomar and events thereafter were shaped by decisions made by a relatively small representation of the scientific community on how the problem of risk assessment was initially to be defined. At one point scientists who organized the conferences to identify the biohazard stood at a cross roads. They could have agreed upon a class of critical questions related to risk and then established a multi-disciplinary effort to carry out experimental programs that were designed to validate or invalidate

the unsubstantiated claims. All this was possible before calling for an end to the moratorium.[7]

On the other hand, they could gather a select group of participants in the field and related fields to offer a state-of-the-art biohazards analysis foregoing a commitment to a serious agenda of risk-related research. As it turned out, of course, they chose the latter and it is not difficult to understand why. Scientists had too much at stake in the research to spend several years waiting for results on biohazards. Furthermore, it is commonly recognized that research on laboratory risks is dull and unrewarding to those who identify themselves as pure scientists.

At the time the National Institutes of Health first became involved in the issue, its director called for an aggressive program of research prior to the issuance of laboratory guidelines. But that research initiative had never been undertaken.[8] It was a key political decision that defined the nature and function of risk assessment. One has to understand the technical decisions within that context. With a broadening of the composition of the NIH committees the outcome of the risk analysis could have been weighted on the side of empiricism rather than on rationalism, or in other terms, more data could have been required before the potentially riskier experiments are permitted. For example, the Executive Committee of the Science Advisory Board of the Environmental Protection Agency established an ad hoc study group to examine the environmental problems of recombinant DNA molecule research.

It is of special interest to note how the EPA study group's findings and recommendations differed from the NIH committee's. Their emphasis was on strict monitoring of laboratory sites, more attention on licensing of laboratories and extensive tests on host organisms.[9] Different conclusions arrived at by a different set of scientists examining the potential risks reflected the nature of their involvement in the research, their scientific backgrounds and how they

identify their primary professional responsibility. A pluralistic appraoch toward risk assessment at its initial stages helps to mitigate the impact of a singular perspectival influence.

There was a second class of decisions related to problem-definition that set the stage for subsequent criticisms. The group of microbiologists who first alerted other researchers about the hazards of the new techniques believed that the investigators who are engaged in the research should have exclusive responsibility for monitoring it. The fear of public involvement or governmental intervention was a prominent factor in shaping how the Asilomar Conference was organized and developed. There was no attempt initially to use the normal scientific channels to reach members of other disciplines that quite evidently had something to contribute to the analysis of risk. It would seem reasonable under the circumstances (where the first self-imposed research moratorium was called) to issue a call for papers and conferences from the relevant scientific societies such as the Genetics Society of America, and the Infectious Disease Society.

As the controversy evolved it became evident why a broad disciplinary approach to risk assessment was warranted. For example, those whose expertise lies in infectious diseases clearly have something to contribute to the risk assessment of certain classes of experiments. But their contribution is far less significant if the concern is with the creation of new pathogens, the epidemiological effects of which have not as yet been studied. My conjecture is that emergent pathogenesis falls outside of the paradigm of orthodox epidemiologists. This is one explanation for why the infectious disease community was not overly critical of the research.

In addition, the early discussions prior to and during Asilomar that defined the problem placed an exclusive emphasis on imminent health hazards. Long term

effects of the research on laboratory workers and potential applications for genetic engineering were kept out of the problem definition. It is curious that while some scientists were busy justifying the exclusion of research applications from their scope of responsibility other colleagues were filing for patent rights on the newly developed techniques for genetic research or setting up independent consultant firms that could market applications to industry.[10]

The Single Route to Truth and Rationality

A principal dogma of scientific epistemology is that there can be only one truth. A corollary to it is that only one of two incompatible views can be rational. This is all good and well when there is a single path to follow or if the truth were to imprint its mark on our visual cortex. Without that certitude it would seem wise to respect the frailty of our claims for knowledge. In view of this it is interesting to examine how some principal actors in the recombinant DNA debate attempted to explain away the opposition. In their efforts to project a rational view of science to the public proponents of the research attacked their adversaries' views as irrational. One strategy that I was witness to on several occasions divided up the scientific opposition into three categories: those who oppose the research on religious or philosophical grounds; those who speak out of turn and do not possess expert knowledge and those who find the issue important merely to further their own political ends.[11] None of these categories addresses the content of the opposition's arguments. With these explanations one is supposed to understand that the controversy is a "virtual" one, and the rationality of science is saved in the eyes of the public.

In retrospect it is not difficult to understand why proponents for the research launched an offensive directed at their adversaries' rationality. If

the public is faced with a choice between two well reasoned arguments with mutually inconsistent conclusions that bear on the health and safety of a community, the public will choose more regulation and less risk. I would venture a guess that the likelihood is even greater when the benefits of the research are not imminent or the research (unlike the development of the polio vaccine) does not respond to a public health crisis. We already have evidence for this tendency in the citizen review processes held in Princeton, New Jersey and Cambridge, Massachusetts. The citizen committees of these respective communities requested additional safeguards to the NIH Guidelines. Strong advocates of the Guidelines explain this phenomenon psychologically. Reviewers must justify their task, consequently they must add controls.

It stands as an open question whether or not the amount of regulations reaches a limit as the number of independent citizen reviews approaches infinity. However, there is evidence of a reverse effect when the review panel is comprised of individuals professionally close to the research. The narrower the interests the fewer the regulations and controls.

Choice of the Host Organism

One of the principal areas of disagreements is on the use of *E. Coli* as a host organism for the genetic recombinantions. There has not been much dispute that safer strains could be found. The arguments that justified staying with *E. Coli* K-12 were not *exclusively* scientific. It is another instance of decision hybridization, in this case a mixture of science, economics and politics. Members of the Boston Area Recombinant DNA group suggested that a thermophilic bacterium be substituted for *E. Coli*.[12] During hearings held by the National Institutes of Health on a preliminary draft of the Guidelines, a *B. Subtilis* host vector system was cited as a promising alternative to *E. Coli*.[13]

The choice of a substitute host to *E. Coli* meant experiments could not proceed at as fast a pace. Time was needed, perhaps one half decade, to learn as much about this alternative host as was known about *E. Coli*. So the question is, how much time is worth trading off for the additional margin of safety? If you start off with the idea that you are well within the margin of safety, the law of diminishing marginal returns takes effect and there is clearly no appreciable gain in trading off time for additional safety. But if the base line safety is in dispute, the decision over how much additive safety is worth, how much saving in time, is clearly a political question not a scientific one.

As a lay person reviewing the controversy over risk versus benefit I found myself faced with an enigmatic situation. On the one hand, microbiologists understand many of the technical problems best, and a sensible citizenry should rely upon these scientists to interpret the risks. On the other hand, the scientists have special vested interests in the research and consequently in seeing that it proceed swiftly. Whether consciously or not these researchers may be willing to accept more risk for the public than the public is willing to bear. At the same time their enthusiasm for the science can distort their interpretation of the benefits.

There is another facet to the controversy over the host organism that is worth mentioning. Several scientists interpreted the conflict over the use of *E. Coli* as an issue of bad public relations. They were frustrated with those who identified *E. Coli* K12 (the attenuated strain) with other members of the class of enteric organisms. Some argued that if some test results show *E. Coli* K12 would not survive or colonize in the human gut, as do other enteric organisms, it was appropriate to change its name. Once it becomes disassociated with other forms of *E. Coli* public anxiety would diminish.[14] It is the Madison Avenue theory of scientific change. I cite this as another example of the extra-scientific influences on the science. There are other interesting cases of the

use of linguistic therapy to assuage the public anxiety.[15] A fuller discussion of the impact of language in the debate will have to await another study.

Taxonomy of Risk

The process by which potential recombinant experiments were divided up into categories of potential risk illustrates two things. First, it was a wonderful expression of science operating in its grand Cartesian tradition breaking down a complex issue into manageable parts.[16] Secondly, it was an illustration of how personal vested interests shaped the outcome of the decision making process. The classification of permissible experiments along with the construction of a biohazards containment space was only partially based upon scientific justifications. At Asilomar the conference and committee meetings had much in common with a national political platform committee. It was a process of hard negotiation with scientists protecting their own research turf. One of the scientists who helped organize Asilomar described the debate over classification in the following way: "... it was a scientific matter with political overtones, clearly, because there were people who had done experiments with cold-blooded animals who wanted to continue."[17]

The distinction between what experiments were to be classified as P-2 and what should be classified as P-3 was a source of continuous criticism. Should the line be drawn between warm-blooded and cold-blooded animals or between insect and bird DNA? A leader of one of the three working groups at Asilomar was quoted as saying: "The consensus here is that people want guidelines and containment so they can go and do their experiments, but no one will come out and say it--they're all chicken."[18] Even such avid proponents of the research as James Watson agreed that there was something irrational about the process by which the risks of classes of experiments were relativized.

> The heart of our trouble comes not from silly leftwing agitation but from the fact that the molecular genetics establishment at Asilomar put the weight of its authority behind guidelines implying we could honestly predict that one form of recombinant DNA experimentation might carry more potential danger than another.

> In contrast, I, for one, saw no way to decide whether work on fruit fly DNA, or yeast DNA, or mouse DNA should be more or less restricted, if at all, and so found the Asilomar experience an exercise in the theater of the absurd.[19]

Emergence of Novelty

During this debate it was not unusual to find the skepticism of the critics striking at the central core of biological knowledge. They questioned the ability of biologists to anticipate novelty or to generalize from empirical data built upon a narrow base of experimental evidence. The biohazard issue was the catalyst which helped the epistemological issues surface. The research advocates cited experiments which tested *E. Coli*'s ability to colonize in the human gut. Results on a small sample of human subjects indicated that the survival of this laboratory strain of *E. Coli* in humans was limited to within a week, while further colonization failed. Critics took issue with the confidence level of these experiments. They questioned whether the results could be extrapolated over a wide range of human stomach flora. One scientists struck right at the heart of the issue, when he questioned the degree to which biology was a predictive science.

> I should say that this (reproducibility of experiments) still is, to a large extent, true of physics and most other disciplines, but it no longer holds for many areas of molecular biology and especially for "gene" transplantation. One has only to look at many papers in this field to see how repugnantly unique and indescribably private much of the experimentation has become. I wonder how many of the specialized findings have been, or can be duplicated.[20]

Another prominent scientist testifying before subcommittees of the Senate raised the same issue: "I do not believe that we can predict the properties of these organisms--created by the fusion of genes from disparate species--or their subsequent evolution, or their impact, present and future, on the existent biosphere."[21] I would venture another guess. The sensitivity to biohazards on the part of some scientists had been a factor in raising the epistemological standards for the validation of certain results. This is clearly the case with

one of the central principles of molecular genetics--the relationship of gene structure to gene product. In simplified terms it states that one gene is matched with one protein. That result restricts the possibilities of novelty through recombinations and thus has a bearing on the issue of risk assessment.

A pure gene is said to have a fixed product. Such a gene fragment when transplanted to a new host will either code for its fixed product or lay dormant. This isomorphism between gene structure and gene product should allay some fears that bizarre transformations can take place from innocuous elements. I raised this issue on many occasions to scientists who appeared before the Cambridge citizens board. Among the staunch proponents of the research there was unqualified acceptance of the result. Others who expressed deeper concern over laboratory safety conditions were more skeptical. They argued that even if the one gene-one protein result were universally true, there could be other mechanisms for emergent properties to develop in the cell. This is especially significant when strains have impurities or when biologists are experimenting with two pure innocuous DNA fragments that combine in a unique way to produce an infective product.

> The emergence of virulence has been substantiated in the joining of viruses. Scientists from the Southwest Foundation in San Antonio reported...that they created (a cancer virus that might be able to infect humans) by combining a harmless baboon virus with a mouse cancer virus that, alone, also is harmless. The product was a virus with the genetic information of the mouse agent with the coating of the baboon virus. Unanticipated products from laboratory combinations of organisms are reported from time to time in journals to alert researchers of potential hazards. In one recent case scientists created a potentially infective cancer virus from two component viruses that by themselves were deemed harmless to humans.[22]

To complicate the matter even further a recent study published in *Nature* cites evidence that a single gene sequence can produce two distinct proteins. The result is explained by the fact that certain nucleotide sequences can be read in more than one way.[23]

It is rather curious how little impact this result has had on re-examining the likelihood of emergent properties. The only citation I could find of the relevance of this result to recombinant DNA hazards was made by the Princeton citizen committee. They cited the new results in their final report to underscore the potentiality of additional hazards unanticipated in the NIH Guidelines.[24]

Conclusion

By the time the issues of genetic recombinations were raised in local and state jurisdictions, scientists who struggled for self-regulation presented themselves in a united front. As a result, the public received a distorted view of how the Guidelines had been reached. The Asilomar process and beyond was offered as a significant achievement in rational scientific decision making. In fact it was a highly politicized process built upon complex motivations of actors, many of unquestionable integrity, but who had much invested in the research. My point is simple. Rather than seeing the linkage between paradigm and politics as anathema to science, we should acknowledge its existence. I do not see how it could have been otherwise.

The recombinant DNA controversy is not unique in this regard; it simply illustrates the symbiosis. It is commonly held that the issues became political when the lay public first became involved. The historical record shows that the issues were already politicized in the early stages of the controversy. Extra-scientific decisions bearing on the public safety were shrouded in technical terminology and camouflaged by the austere atmosphere of professional scientific meetings. It is not a sign of disrespect for the deities of science, only a sign that they are closer to us earthly beings.[25]

Notes

1. In Norman Campbell's popular essay *What is Science?* he defines science as "the study of those judgments concerning which universal agreement can be obtained." (New York: Dover, 1952), p. 27.
2. The most grievous sin in science, falsifying data, was allegedly commited by biologist Paul Kammerer and more recently cancer researcher William T. Summerlin of the Sloan-Kettering Institute. See Arthur Koestler, *The Case of the Midwife Toad* (New York: Vintage, 1973); also Barbara J. Culliton, "The Sloan-Kettering Affair: A Study Without a Hero," *Science,* 184 (1974), 644-650.
3. As an example see *Science,* 185 (1974), 925.
4. Karl Popper, *The Logic of Scientific Discovery* (New York: Harper, 1959) identifies negation (falisfication) rather than affirmation (confirmation) as the key to rationality in science.
5. *Science,* 198 (1977), 275.
6. Gunnar Myrdal, *Objectivity in Social Research* (New York: Pantheon Books, 1969).
7. Friends of the Earth criticized the NIH Guidelines on the grounds that an initial commitment to testing the hazards was not fulfilled before the Guidelines were released. See Francine R. Simring, "Folio for Folly: N.I.H. Guidelines for Recombinant DNA Research", *Man and Medicine,* 2, No. 2 (1977), Journal of the Columbia University College of Physicians and Surgeons.
8. "Survivability studies with the host strain prior to insertion of new genetic materials should precede all recombinant DNA research with that host." The quotation is from a draft letter to Dr. Emil M. Myak, Chairman, Executive Committee, Science Advisory Board, U.S. Environmental Protection Agency, from Martin Alexander, Chairman, Ad Hoc Study Group on Recombinant DNA Policy,

Science Advisory Board, EPA, dated April 28, 1977. *Science* reported on October 15, 1976 that EPA research director Wilson K. Talley told a Senate subcommittee "We believe that recombinant DNA research should be performed on organisms which would be unable to live outside the laboratory environment and which are less ubiquitous than *E. Coli*."

9. On October 7, 1974, the director of the National Institutes of Health, Robert S. Stone, announced the establishment of the Recombinant DNA Molecule Program Advisory Committee. He set the following as its primary objective. "The goal of the Committee is to investigate the current state of knowledge and technology regarding DNA recombinants; their survival in nature, and transferability to other organisms; to recommend programs of research to assess the possibility of spread of specific DNA recombinants /and the possibility of spread of specific DNA recombinants/ and the possible hazards to the environment; and to recommend guidelines on the basis of the research results". The announcement appeared in the *Federal Register*, 39, No. 215 (Nov. 6, 1974) 39306.

10. The Cetus Corporation of California was set up by scientists to find industrial uses for recombinant DNA techniques. In a corporation statement that appeared in October 1975 the goals of the firm were spelled out. "We propose to do no less than to stitch, into the DNA of industrial micro-organisms, the genes to render them capable of producing vast quantities of vitally needed human proteins." Scientists are speaking directly to the potential investors of the research. *The New York Times* reported on March 9, 1977: "Last week, nearly 200 people from the investment industry heard Dr. Matthew Meselson of Harvard University, a noted molecular biologist, say that this field of research has been brought into a new phase by the new techniques." *The Times* titled the story: "Technology: Investing in Molecular Biology." Dr. Meselson was extremely cautious about promising

anything. He spoke merely of potentialities. *Technology Review* reported on May, 1977 (p. 12): "Lured by the prospects of new products and profits and goaded by commercial competition, every major pharmaceutical firm has put biologists to work on recombinant DNA. The issues no longer center upon small-scale, university-based fundamental research."

11. James Watson was quoted in *Time Magazine* (April 18, 1977) as labelling the opponents of recombinant DNA research "Kooks", "Shits", and "Incompetents." An editor of *Chemical & Engineering News* questioned Watson on the meaning of those terms. Watson replied that by "incompetents" he meant prominent opponents who have been total failures themselves at DNA research. By "shits" he refers to those with political motivations and by "kooks" he refers to those who oppose the research on "religious, phony environmental and other grounds. See *Chemical & Engineering News,* "Forum on Recombinant DNA", 55 (May 30, 1977), 26. Salvidor Luria is more sensitive in his account of the critics of the research. He divides the criticism into three classes: mystical, sanitary and political and takes the last two seriously despite his disagreement with opponents. "The Goals of Science," *Bulletin of the Atomic Scientists,* 33, No. 5 (1977), 32-33.

12. This recommendation was made to the Cambridge Experimentation Review Board and at the National Academy of Science Forum on Recombinant DNA, March 7-9, 1977, Washington, D.C.

13. National Institutes of Health, *Recombinant DNA Research,* Vol. 1, Documents relating to NIH Guidelines for Research Involving Recombinant DNA Molecules, Feb. 1975-June 1976, p. 104.

14. These issues were raised in one of the workshops held at the National Academy of Science Forum on Recombinant DNA. March 7 to 9, 1977, Washington, D.C.

15. Dorothy Nelkin, in an unpublished paper entitled "Threats and Promises: Negotiating the Control of Research," provides some insights on the use of language in the controversy. "The very use of language reflected the growing polarization. The scientists at Asilomar, calling for greater self-regulation, had talked of "recombinant DNA technology" and even "plasmid engineering." Later, when threatened with external control, their language shifted, and they consistently referred to the research as "a science" directed to the basic understanding of the human genotype."

16. Descartes described his second Rule of Method as follows: "to divide up each of the difficulties which I encountered into as many parts as possible, and as might be required for an easier solution." Rene Descartes, *Discourse on Method and Meditations,* trans. Laurence J. Lafleur (New York: Bobbs-Merrill, 1960), p. 15.

17. The quotation appeared in "Science that Frightens Scientists," by William Bennett and Joel Gurin, *Atlantic Monthly,* 239 (Feb. 1977), p. 55. Maxine Singer is the scientist quoted.

18. Nicholas Wade, *The Ultimate Experiment* (New York: Walker, 1977), p. 48. "The battle of the frogs demonstrated how particular interests as well as general principles shaped the framing of the NIH Guidelines. Another demonstration was shotgun experiments with higher plants, which on the tumor virus rationale are rather considerably less threatening to man than are fruitflies, were left in the stricter containment of P2 + EK2. Why? Because no one at the Committee was particularly anxious to shotgun higher plants, whereas many researchers were interested in frogs and fruitflies. Higher plants have since been downgraded to the same level as fruitflies." (p. 96). "Elena O. Nightingale was a participant at Asilomar. At the time she was associated with the Division of Medical Sciences, National Research Council, National Academy of Sciences. She also served on the Recombinant

DNA Molecule Program Advisory Committee. In a letter to Dewitt Stetten which has been included in *Recombinant DNA Research*, Vol. 1 (Aug. 1976), U.S. Dept. HEW-NIH, she wrote: 'There seems to be no biologic rationale for considering insects to be safer than cold-blooded vertebrates. This point was raised at La Jolla by Dr. Brenner, but the Committee voted to keep a distinction between the cold-blooded vertebrates and other cold-blooded animals, including insects'." (p. 42)

19. "In Defense of DNA," *The New Republic*, (June 25, 1977), 13.
20. Erwin Chargoff, "Recombinant DNA Research: A Debate on the Benefits and Risks," *Chemical and Engineering News*, 55 (May 30, 1977), 41.
21. Statement of Robert Sinsheimer, California Institute of Technology, before a joint hearing of the Senate Subcommittees on Health of the Committee on Labor and Public Welfare and on Administrative Practice and Procedure of the Committee on the Judiciary. "Oversight Hearing on Implementation of NIH Guidelines Governing Recombinant DNA Research," September 22, 1976, p. 73.
22. *Los Angeles Times*, front page, May 28, 1976.
23. F. Sanger et al, "Nucleotide Sequence of Bacteriophage X174 DNA," *Nature*, 265 (1977), 687-695.
24. The following appeared in the report submitted by the citizen committee in Princeton that studied the issue: "...recent evidence reported by Professor Sanger's group in Cambridge, England, presents a surprising new fact about the DNA of a bacterial virus. It has been widely believed until now that each sequence of three nucleotides in a DNA molecule codes for a specific amino acid which means that a particular section of the DNA molecule contains the genetic information for a single protein. Sanger's group has shown that a "pure" DNA fragment may contain overlapping information that could specify a second protein and thus produce an unexpected

result." *Report of the Princeton Community Biohazard Committee* (May 1977).

25. It is comforting to find scientists who are open about the issue of self-interest. Richard P. Novick is Chief of the Department of Plasmid Biology at the Public Health Research Institute of New York. He was chairman of the "plasmid group" that was responsible for drawing up one of the working statements for guidelines at the Asilomar Conference.

> I would like to say, quite frankly, that my own personal interest in these experiments colors my view inevitably. In fact, given that I would like to do certain experiments involving recombinant DNA--experiments that I believe to be non-dangerous--I am quite unable to distinguish between the following two alternatives as the basis for this belief.
>
> 1. I am convinced they are not dangerous, and therefore it is okay to do them; or
>
> 2. I have convinced myself they are safe precisely because I want to do them.
>
> And I would deny anyone whose self-interest is involved in a particular line of activity to make that distinction. *Bulletin of the Atomic Scientists*, 33, (May 1977), 16.