Dr. Genelove:
How Scientists Learned to Stop Worrying and Love Recombinant DNA

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Dedication

The author wishes to dedicate this dissertation to everyone who has encouraged and supported by love of science and history.

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Abstract of Dissertation

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People are still debating recombinant DNA today, whether they know it or not. As the technology that gave birth to the biotechnology revolution, recombinant DNA initiated passionate debates from 1971 until 1978 which dealt with issues still central to biotechnology: biohazard safety; scientific freedom and responsibility; the standard of national policy preempting city and state initiatives to ensure the ability to conduct research uniformly around the country; the influence of scientific, business, and environmental lobbying on Congress; and, above all, who should be responsible for the creation, oversight, and enforcement of biotechnology advancements.

The years from the initial proposed recombinant DNA experiment in 1971 until the Asilomar Conference in late February 1975 were essential in determining the future of recombinant DNA research and setting the boundaries for the debates between scientists and in the public sphere. Despite the urgency of some scientists to move forward with recombinant DNA research during this period, there were times of little momentum and there certainly was no guaranteed outcome. It was in these essential years that fundamental questions were asked about the nature and safety of scientific inquiry, and where tough decisions were made that broke with the traditions of the scientific establishment and demonstrated an extraordinary degree of social responsibility at a critical moment in the history or molecular biology.

The dissertation explores the events that were occurring inside and outside of the laboratory that influenced the debates. These included the dramatic changes in molecular

biology, the rise of the environmental social responsibility among scientists, and radical science movements, as well as, the changing role of the scientific and lay press. These essential first years of the debate also created enduring networks that helped shepherd recombinant DNA through a myriad of scientific organizations and governmental agencies, as well as, respond to the criticism by other scientists and the public in the days, months, and years following the 1975 Asilomar Conference, which came to a consensus on the safety of recombinant DNA.

Table of Contents

Chapter 1: "Definitions are Arbitrary"

People are still debating recombinant DNA today, whether they know it or not. As the technology that gave birth to the biotechnology revolution, recombinant DNA initiated passionate debates from 1971 until 1978 which dealt with issues still central to biotechnology: biohazard safety; scientific freedom and responsibility; the standard of national policy preempting city and state initiatives to ensure the ability to conduct research uniformly around the country; the influence of scientific, business, and environmental lobbying on Congress; and, above all, who should be responsible for the creation, oversight, and enforcement of biotechnology advancements. Despite the effort of Congress and local and state governments, as well all of the individuals, scientists, organizations, and agencies involved in the debates, only one policy emerged for recombinant DNA in the United States, and it was not a uniform national policy. The policy was a set of National Institutes of Health Guidelines, [1] which only applied to researchers receiving NIH funds. What also resulted from the recombinant DNA debates were policies, such as the Ethical, Legal, and Social Implications (ELSI) of the Human Genome Project, where the scientists and federal government created programs to address the social impacts of biological advances.

The debates about recombinant DNA between 1975-1978 and the implications of the policy created for research using recombinant DNA are well worn by those who have written histories. There is unanimity in one aspect of the histories written about the

[1] The first set of NIH Guidelines was release on June 23, 1976. National Institutes of Health, "Guidelines for Research Involving Recombinant DNA Molecules," *Federal Register* 41: 27911-27943 (7 July 1976).

recombinant DNA debates from historians, scientists, journalists, and critics. They agree that events between the years between 1971 and 1975 had a sense of momentum and inevitability towards a positive outcome for the scientists involved in recombinant DNA research. Some of the histories spend a chapter on this time period, while a few cover it in a mere few pages. The years from 1971 to 1975 were essential in determining the future of recombinant DNA and setting the boundaries for the debates. Despite the urgency of some scientists to move forward with recombinant DNA research during this period, there were times of little momentum and there certainly was no guaranteed outcome. It was in these essential years that fundamental questions were asked about the nature and safety of scientific inquiry, and where tough decisions were made that broke with the traditions of the scientific establishment and demonstrated an extraordinary degree of social responsibility at a critical moment in the history or molecular biology. The other histories also downplay or ignore the influence of events that were occurring inside and outside of the laboratory that influenced the debates. This included the dramatic changes in molecular biology, the rise of the environmental and radical science movements, as well as, the changing role of the scientific and lay press. These essential first years of the debate also created enduring networks that helped shepherd recombinant DNA through a myriad of scientific organizations and governmental agencies, as well as, respond to the criticism by other scientists and the public in the days, months, and years following the Asilomar Conference, which came to a consensus on the safety of recombinant DNA.

Trying to explain how a nuclear fission chain reaction can be used to create a bomb of irrational destruction or as a source of energy is not simple. Neither is it simple

to explain many of the creations derived from chemistry laboratories. Recombinant DNA, however, is a different beast entirely. Recombinant DNA is as easy to explain, as it is to understand its potential benefits and consequences. Recombinant DNA is simply the removal of a gene (or genes) from one organism and insertion of the gene(s) into another organism. The simplicity of moving the genes from one organism to another, however, belies the difficulty of getting those transferred genes to express themselves in their new host. The smaller the gene(s) the easier it is to get it to express itself in a new organism. For example, it took scientists the better part of a year to get the gene for insulin, a single definable gene with a recognizable effect, to produce insulin when it was inserted into a simple organism, such as the bacterium *Escherichia coli*. Scientists still have not completely solved how traits relying on multiple genes (polygenic traits), such as size, strength, or intelligence, express themselves when inserted into another species. The complexity of getting genes to express themselves when inserted into another organism did not stop the fears of a Frankenstein-like organism from being created.

The idea of splicing genes from one organism into another fascinated molecular biologists for years with its potential to revolutionize their research. Yet when Paul Berg proposed the first recombinant DNA experiment in 1971 the reaction of the scientific community was not all positive.[2] The standard arc for histories of the recombinant DNA debates begins with the reactions to Berg's proposed experiment. The trajectory then follows that a year after Berg's proposal, the initial technique was soon simplified by

[2] David A. Jackson, Robert H. Symons, and Paul Berg, "Biochemical Method for Inserting New Genetic Information into DNA of Simian Virus 40: Circular SV40 DNA Molecules Containing Lambda Phage Genes and the Calactose Operon of *Escherichia coli*," *Proceedings of the National Academy of Sciences of the United States of America* 69, no. 10 (1972).

Stanley Cohen and Herbert Boyer to a degree that it could theoretically be carried out by a high school student at a standard lab bench. At a prestigious biology conference in the summer of 1973, the Cohen and Boyer method of creating recombinant DNA molecules was discussed, and the attendees expressed concern about the potential risks for experiments using this method. Their concern was expressed in a letter sent on their behalf to the president of the National Academy of Sciences (NAS),[3] which was later published in the journal *Science*.[4] The NAS then issued a report in June 1974 calling for a self-imposed moratorium on two types of recombinant DNA experiments, an international conference to discuss the safety of recombinant DNA, and for the NIH to create an advisory board for recombinant DNA research.[5] The culmination of their action was the Asilomar Conference in February 1975, where a select group of international biologists, lawyers, and representatives of the press at the Asilomar Conference Center met for four days in an attempt to reach a consensus about the safety recombinant DNA experiments. The conference yielded a consensus that allowed the majority of recombinant DNA research. The conference agreed that some experiments were too dangerous to carry out given state of physical containment facilities, and they would not be attempted until adequate safety conditions were created.

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[3] "Maxine Singer and Dieter Soll to Philip Handler, 17 July 1973," (ASSEM: Life Sciences: Com on Synthetic Nucleic Acids: Ad hoc Proposed, 1973; Central Policy Files; NAA).

[4] Maxine Singer and Dieter Soll, "Guidelines for DNA Hybrid Molecules," *Science* 181, no. 4105 (1973). Cohen and Boyer's journal article on their successful recombinant DNA was published following the letter written on behalf of the Gordon Conference by Maxine Singer and Dieter Soll. Stanley N. Cohen et al., "Construction of Biologically Functional Bacterial Plasmids in Vitro," *Proceedings of the National Academy of Sciences of the United States of America* 70, no. 11 (1973).

[5] Paul Berg et al., "Potential Biohazards of Recombinant DNA Molecules," *Proceedings of the National Academy of Sciences of the United States of America* 71, no. 7 (1974).

Days after the Asilomar Conference, the NIH began work on a set of guidelines for NIH funded scientists carrying out recombinant DNA experiments. The resulting NIH Guidelines in June 1976 nearly replicated the majority of recommendations of the Asilomar Conference. Following the publication of the NIH Guidelines, the recombinant DNA debates became public. A critical cover story in *The New York Times Magazine* the week after the release of the NIH Guidelines fueled these debates, as did a vocal minority of scientists,[6] notably the left-leaning radical scientists, like Science for the People, environmentalists, and politicians. The public stage of the debate was primarily concerned with the safety of the experiments, the role of the public in science policy, and battles of local, state, and federal preemption laws.

Congress was slow to get involved in the debate. There was one hearing in the spring of 1975, and one in 1976. After six years from the beginning of the debates within the scientific community, the debates reached their apex in 1977 with the first session of the ninety-fifth Congress. Subcommittees in the House and Senate sponsored wide-ranging hearings that resulted in weeks worth of testimony, which included scientists, policy makers, mayors, governors, representatives of business interests, environmentalists, and the Secretary of Health, Education, and Welfare, many of whom made appearances at the different hearings throughout the year. By the end of the year nearly ten bills were drafted and put into the Congressional Record. These bills varied in content from extending the NIH Guidelines to cover all research using recombinant DNA to a bill that closely mirrored the 1946 Atomic Energy Act with recombinant DNA

[6] Liebe F. Cavalieri, "New Strains of Life–or Death: Scientists Have Learned to Rearrange the Basic Genetic Material of Living Things, and So Have Opened an Exciting New Research Frontier. But, One Biologists Warns, 'the Future Will Curse Us for It.'," *The New York Times Magazine*, 22 August 1976.

replacing atomic energy and HEW replacing the Department of Energy, and variations between the two.[7] Early in 1978, Paul Rogers and Harley Staggers cosponsored a bill that was final attempt to pass legislation on recombinant DNA. The bill was essentially an extension of the NIH Guidelines to cover all recombinant DNA research, but contained more administrative provisions as HEW, and not NIH, would be responsible for oversight and enforcement. No future bills to regulate recombinant DNA research were sponsored in Congress, and the NIH Guidelines was the only federal policy in the United States.[8]

Not Spliced

This dissertation is constrained by dates, and constrained by subject matter. Two prominent topics related to recombinant DNA, are not discussed in this dissertation because of their tangential nature to my argument. They include the birth of the business side of biotechnology, and the federal funding of science.

The growth of biotechnology is an interesting subject, but my research found that the established agricultural or pharmaceutical community largely followed the lead of the scientists. Though not required to follow NIH Guidelines of 1976, they did so voluntarily. For established businesses or biotech start-ups there were two problems with using recombinant DNA. The first problem was legal, and the second problem was scientific. The first was concern about protecting intellectual property rights over any unique organism created using recombinant DNA. There was little point from the perspective of

[7] President Harry S. Truman signed the Atomic Energy Act, also known as the McMahon Act, into law on August 1, 1946, and it went into effect January 1, 1947.
[8] Donald S. Fredrickson, *The Recombinant DNA Controversy: A Memoir: Science, Politics, and the Public Interest 1974-1981* (Washington, D.C.: ASM Press, 2001), Burke K. Zimmerman, *Biofuture: Confronting the Genetic Era* (New York, NY: Plenum Press, 1984), 169-77.

a business or a venture capitalist in heavily investing in the creation of a recombinant DNA molecule that could not be patented. The second was purely scientific. Although scientists could insert specific genes into bacteria, they were initially unable to have any of those genes expressed. That is, using recombinant DNA techniques, scientists could insert the gene sequence for the production of human insulin into bacteria and have it successfully reproduce, but they could not get the bacteria to create insulin. The expression of inserted genes into the plasmid of bacteria could turn the bacteria into small factories for pharmaceuticals.

In 1972, Ananda Chakrabarty, a scientist at General Electric, filed a patent application for a bacterium that could digest crude oil, a property that was possessed by no other naturally occurring bacterium. Chakrabarty planned to create this new bacterium from genes from four existing bacteria using recombinant DNA. The patent officer rejected his patent application on the basis that living things were not patentable. Chakrabarty appealed this decision to The Court of Customs and Patent Appeals, who reversed the decision of the patent officer. Sydney Diamond, Commissioner, US Patent and Trademark Office, then appealed this decision to the Supreme Court of the United States. On June 16, 1980, the Supreme Court ruled in favor of Chakrabarty in a 5-to-4 vote. Chief Justice Burger wrote in the majority opinion that the production of “a new bacterium with markedly different characteristics from any found in nature and one having the potential for significant utility. [Chakrabarty’s] discovery is not nature’s handiwork, but his own; accordingly it is patentable subject matter.” All of the patents on

unique organisms that did not occur in nature were approved following the *Chakrabarty v. Diamond* decision.[9]

In *Invisible Frontiers*, Stephen Hall detailed the events of the race that led to the first successful production of insulin produced by bacteria created using recombinant DNA.[10] Hall's narrative account of the race involved three teams – led by Walter Gilbert (Harvard University), Herbert Boyer (University of California, San Francisco), and William Rutter (University of California, San Francisco) and Howard Goodman (Eli Lilly). Insulin production was seen as the low hanging fruit of potential benefits of recombinant DNA because the gene sequence was already purified and there was a demand for insulin. The team led by Boyer was the first to produce insulin, and the result was that Genentech, Boyer's startup biotech company, became the industry leader. Rutter went on to found the biotech company Chiron Corporation, and many other academic scientists followed suit in the years to come. The issue of patents, especially at universities, and the commercialization of recombinant DNA technology by academics is the focus of "Making Dollars Out of DNA" by Sally Smith Hughes.[11] In *Shaping the Industrial Century*, Alfred Chandler explains how and why the agriculture and

[9] *Diamond V. Chakrabarty*, 100 U.S. 2204 (1980).
[10] Stephen S. Hall, *Invisible Frontiers: The Race to Synthesize a Human Gene* (New York: Atlantic Monthly Press, 1987).
[11] Sally Smith Hughes, "Making Dollars out of DNA: The First Major Patent in Biotechnology and the Commercialization of Molecular Biology, 1974-1980," *Isis* 92, no. 3 (2001). For a more critical view of the relationship between academic scientists and biotech companies see Martin Kenney, *Biotechnology: The University-Industrial Complex* (New Haven, CT: Yale University Press, 1986).

pharmaceutical companies changed their business models to commercialize recombinant DNA technology.[12]

The other topic that was tangential to my research, but nevertheless affected recombinant DNA research was the change in how the federal government funded research beginning the late 1960s. The most obvious changes occurred when the budget for funding was cut or raised – with the economic downturns in the late 1960s through early 1970s there was much more cutting of research and development budgets. Federal funds also began to shift away from basic research toward mission-oriented research (also called directed research). Two examples from the late 1960s and early 1970s were the Mansfield Amendment and President Nixon's War on Cancer (a slogan Nixon never used.)

The Mansfield Amendment to the 1970 Military Procurement Authorization Bill was the first signal that Congress was breaking from the post-Second World War funding of science initially espoused by Vannevar Bush in *Science, The Endless Frontier*.[13] The Mansfield Amendment required that all research funds for Department of Defense had to be for projects that had specific military function or operation. The result was that the DOD could not fund basic research unless it was intimately tied to a specific military need. The scientific community was stunned by the amendment and its easy passage. Historian David Dickson argued that "the amendment encapsulated a growing feeling both inside and outside Congress that a lack of direct accountability meant that society

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[12] Alfred D. Chandler, *Shaping the Industrial Century: The Remarkable Story of the Modern Chemical and Pharmaceutical Industries* (Cambridge, MA: Harvard University Press, 2005). Particular focus is paid to the role of biotechnology in Chapter 10, 260-279.
[13] Office of Scientific Research and Development (US) and Vannevar Bush, *Science, the Endless Frontier. A Report to the President* (Washington, DC: US Government Printing Office, 1945).

was not getting the returns to which it was entitled from its two decades of investment in basic research, and that often federal agencies were being take for a ride by the scientific community."[14]

President Nixon's fourth "great goal" in his 1971 State of the Union Address was "to offer a far-reaching set of proposals for improving America's health care and making it available more fairly to more people."[15] His fifth proposal for this goal was to "ask for an appropriation of an extra $100 million to launch an intensive campaign to find a cure for cancer, and I will ask later for whatever additional funds can effectively be used. The time has come in America when the same kind of concentrated effort that split the atom and took man to the moon should be turned toward conquering this dread disease. Let us make a total national commitment to achieve this goal."[16] Later that year, Congress passed The National Cancer Act, which appropriated over $25 million, $35 million, and $45 million for cancer research, treatment, and prevention for fiscal years 1972, 1973, and 1974, respectively.[17] Nixon signed the bill on December 23, 1971. The bill resulted

[14] David Dickson, *The New Politics of Science* (Chicago, IL: University of Chicago Press, 1988), 122. For an analysis of federal funding for science see also Susan Wright, *Molecular Politics: Developing American and British Regulatory Policy for Genetic Engineering, 1972-1982* (Chicago, IL: University of Chicago Press, 1994). The scientific community, especially those receiving funding from the DOD, were still concerned about the Mansfield the following year and the future of research and development funds. See Rodney W. Nichols, "Mission-Oriented R&D," *Science* 172, no. 3978 (1971).
[15] Richard Nixon, *Public Papers of the Presidents of the United States. Richard Nixon: Contains the Public Messages, Speeches, and Statements of the President, 1971*, 6 vols., vol. 3 (Washington, DC: FU.S. Government Printing Office, 1972), 53.
[16] Ibid.
[17] Public Law 92-218, *The National Cancer Act of 1971*, 92nd Cong., 1st sess., 23 December 1971.

in one of the largest mission-oriented biomedical research programs in American history.[18]

What's in a Word?

Terminology is important in science because one word in science can have extremely specific definitions while others can be broadly defined. The use of some non-specific scientific terms can carry political or cultural baggage depending on its use, such as eugenics, while others, such as germ, are just vague. The term recombinant DNA is used in this dissertation because the molecular biologists carrying out recombinant DNA research used this specific term to describe their experiment and their work. They rarely used the term genetic engineering because it was not a particularly descriptive scientific term by the late 1960s when it could mean a multitude of different things to biologists and to the public. During the essential years of recombinant DNA, the term genetic engineering was also loaded, and to a degree it always had been a loaded term. Its general vagueness allowed it to be wielded by its user to imply confidence or dread in biological advances. The *Oxford English Dictionary* defined genetic engineering originally as "the production of desirable genotypes by selective breeding; the application of eugenics."[19] After the invention of recombinant DNA and the growth of biotechnology the definition came to mean what we think of today: "the alteration of the genome of an organism by

[18] James T. Patterson, *The Dread Disease: Cancer and Modern American Culture* (Cambridge, MA: Harvard University Press, 1987), Robert Proctor, *Cancer Wars: How Politics Shapes What We Know and Don't Know About Cancer* (New York, NY: BasicBooks, 1995). Not all cancer research was on the molecular level, for a history of radiation treatment see For an analysis of US medical research policy see Stephen P. Strickland, *Politics, Science, and Dread Disease: A Short History of United States Medical Research Policy* (Cambridge, MA: Harvard University Press, 1972).
[19] "Genetic Engineering," in *Oxford English Dictionary* (2009), 521. Genotype is t

laboratory techniques, esp. by the insertion, alteration, or removal of a gene; the techniques involved in this process."[20] The term genetic engineering was first used by Curt Stern, a pioneer in human genetics, in a 1949 article in *Science* titled "Selection and Eugenics."[21] At the end of his article arguing for the benefits of positive eugenics, Stern wrote

> Eugenic thinking has always emphasized the well-being of mankind, though much eugenic counseling was based on inadequate knowledge and has been harmful. In the future more knowledge will be gathered and will aid wise planning. Then genetic and eugenic counseling will become the foundation of human *genetic engineering*.[22]

From its initial use, the definition of genetic engineering was entangled with eugenics, a term that, when Stern wrote his article, was wrought with suspicion by the public and criticism by scientists. The problem for Stern and all future geneticists was that the origins of genetics could not be untied from eugenics, which to the public had a negative connotation, including forced sterilization, immigration restrictions, and marriage restrictions. [23]

Stern was part of the post-Second World War movement by biologists, especially human geneticists, in the United States and Britain who "fought—by and large

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[20] Ibid.
[21] Curt Stern, "Selection and Eugenics," *Science* 110, no. 2852 (1949).
[22] Ibid. Emphasis added.
[23] For the role of eugenic in American culture see Daniel J. Kevles, *In the Name of Eugenics: Genetics and the Uses of Human Heredity* (Cambridge, MA: Harvard University Press, 1995), esp. 96-112, Stephen Jay Gould, *The Mismeasure of Man*, Rev. and expanded. ed. (New York, NY: Norton, 1996). In the United States, negative eugenics quickly took hold scientifically and legislatively, including forced sterilization, immigration restrictions, and marriage restrictions. Notable among these is the Immigration Act of 1924 and the *Buck v. Bell* Supreme Court ruling [*Buck V. Bell*, 274 U.S. 200 (1927).]

successfully—to emancipate human genetics" from the social prejudices of the eugenics movement.[24] According to historian of science Daniel J. Kevles, the goal of the post-war biologists was to establish human genetics as a purely scientific field that could explain "the complexities of human heredity and assist medicine by illuminating the relationship of genetics and disease."[25] Stern's article reinforces Kevles's argument because Stern was trying to reclaim eugenics as an agent for positive change for future generations. Stern based his argument for positive eugenics on the increased knowledge of human genetics as a means for genetic counseling unbiased by social, cultural, and political prejudices. He concluded his article by stating "Although eugenic problems are not as urgent as the pessimists believe, their ultimate importance can hardly be overestimated."[26]

Though first used by science fiction writer Jack Williamson in his 1951 book *Dragon's Island*,[27] the public associated the term genetic engineering with Aldous Huxley's classic *Brave New World*, which was written and published in 1932. In his book, Huxley painted a picture of a dystopian future where the state tightly controlled reproduction to perpetuate a caste system, and a drug (soma) was ubiquitous and its use encouraged by the state in order to propagate a seemingly utopian world. Written as a response to the excesses of the industrial revolution and the horrors of the First World War, the book has endured, in part as a cautionary tale (its discussion of state engineered artificial breeding techniques to create particular castes) and as a forecast of the possibility of eugenics and genetic engineering. Though written before the term genetic engineering was coined, Huxley was closely connected to world of biology and eugenics

[24] Kevles, *In the Name of Eugenics: Genetics and the Uses of Human Heredity*, ix.
[25] Ibid.
[26] Stern, "Selection and Eugenics," 208.
[27] "Genetic Engineering."

in England. Aldous was the grandson of biologist Thomas Henry Huxley (known as "Darwin's Bulldog"), and brother of Julian Huxley, an evolutionary biologist and prominent member of the eugenics movement in Britain.

The stain of eugenics or the easy analogy to *Brave New World*'s dystopian future loaded the term genetic engineering with so many negative connotations that by the 1960s geneticists avoided the term altogether. Advances in genetics in the 1960s began turning the hypothetical ability to alter specific genes into a reality. During the first half of the decade, debates ensued among scientists about the potential new genetic techniques that could alter specific genes. These debates generated conferences, which in turn produced published statements and testimony for public consumption and debate. The books published regarding the genetic engineering debates of the 1960s included *Man and His Future* (1963), *Control of Human Heredity and Evolution* (1965), and *Biological Aspects of Social Problems* (1965).[28] Geneticists themselves tried separate the term from their work by creating new terms.

In 1965, renowned biochemist Rollin Hotchkiss wrote "Portents for Genetic Engineering."[29] First delivered as a keynote lecture at the annual meeting of the American Institute of Biological Sciences and subsequently published in *The Journal of*

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[28] G. E. W. Wolstenholme and Ciba Foundation., *Man and His Future* (Boston, MA: Little, Brown, 1963), T. M. Sonneborn and Ohio Wesleyan University., *The Control of Human Heredity and Evolution* (New York, NY: Macmillan, 1965), J. E. Meade, A. S. Parkes, and Eugenics Society (London England), *Biological Aspects of Social Problems: A Symposium Held by the Eugenics Society in October, 1964* (Edinburgh, UK: Oliver & Boyd, 1965).

[29] Rollin D. Hotchkiss (1911-2004) was an influential American biochemist best known for his work on isolating antibiotics, and demonstrating the transfer of antibiotic resistance between bacteria. Hotchkiss spent the majority of his career at the Rockefeller Institute of Medical Research, and was elected a member of the National Academy of Sciences (1961).

Heredity, the paper argued for "genetic intervention." For Hotchkiss, "the new potential program, 'genetic engineering', raised by the exploits of molecular biology, was a genetic intervention that could be practiced in private and in secret on the individual genes of individual persons."[30] The only reason he provided for not using the term, genetic engineering, was his preference "to call it intervention because it may never be really engineered."[31] Hotchkiss continued using his term for the remainder of the article while detailing the potential benefits as well as, the potential consequences of genetic intervention.[32]

The following year, geneticist and Nobel laureate Joshua Lederberg addressed the potential ability of the specific alteration of human genes in his article "Experimental Genetics and Human Evolution." Lederberg, who defined eugenics as selective breeding, dismissed eugenics because of the potential for social and cultural biases, the lack of knowledge by scientists, and the "relatively inefficacious" speed at which it might change the genes of a population.[33] He argued that the "realization of applied biology, is, simply medicine," and "a more effective slogan" to eugenics would be "euphenics."[34] ("Applied biology" was a general term Lederberg employed to describe the multiple methods that molecular biology could be employed to alter an organism.) Lederberg created the term euphenics and defined it as "all the ameliorations of genotypic maladjustments, including liability to any disease that could be brought about by treatment of the affected

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[30] Rollin D. Hotchkiss, "Portents for Genetic Engineering," *The Journal of Heredity* 6, no. 5 (1965): 198.
[31] Ibid.
[32] Ibid.: 197-202.
[33] Joshua Lederberg, "Experimental Genetics and Human Evolution," *The American Naturalist* 100, no. 915 (1966): 530.
[34] Ibid.: 521.

individual, more efficaciously, the earlier in his development."[35] Though a broad definition, euphenics was his attempt to redefine the role molecular biology could play in directing human evolution.

Publicly, scientists did use the term genetic engineering, notably in Congressional testimony in the late 1960s. An example of this came during the testimony of Nobel laureate and biochemist Arthur Kornberg before the Senate Subcommittee on Government Research on the National Commission on Health and Society in 1968. In his opening statement Kornberg testified that if the pace of advancement in biology continued for the next twenty years "genetic engineering would then come more into prospect. This prospect fascinates people. It also frightens them and I understand why it does. Any knowledge or any invention can be used for good or evil. Fire, the wheel, gunpowder, nuclear power. They serve us and destroy us." Kornberg explained that "at some future date" genetic engineering could be used to cure genetic defects, including diseases with a genetic component. Kornberg's use of the term genetic engineering, forced him to answer some sensational questions, including: "Do you see this work of yours leading to the creation of a master race?" "Now, once you develop and are successful in genetic engineering, what course does it take – with Hitler, with Stalin, in a democratic society, with a Mao-tse Tung – where does it go?"[36] Kornberg answered the first in the negative and stated that the "dire consequences of genetic research are still remote," though it is always a possibility.[37]

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[35] US Congress. Senate. Subcommittee on Government Research, *National Commission on Health and Science Policy*, 90st Cong., 2nd sess., 1968, 42.
[36] Ibid., 45,47.
[37] Ibid., 47.

A 1970 Congressional Research Service (CRS) report attempted to define the term genetic engineering and its implications for the future. The author of the report stated that there was "good reason" for the "generalization" of the term because "many of the advancements in molecular biology which are producing rapid progress in genetic engineering also are providing the basic information required to solve other problems."[38] In addition to the curing and other elimination of genetic diseases, genetic engineering also encompassed "eugenics, artificial insemination, 'test-tube' babies, cloning, counseling, medical genetics and euthenics."[39] The report concluded that there were ethical, legal, and political implications for genetic engineering. With each new advancement that made genetic engineering a reality, there was a "reaction from the public bordering on real fear…that the possibilities inherent in genetic engineering will be abused as have many aspects of nuclear energy."[40] The resulting response from the public was usually for a legal "prohibition of the research."[41] The report warned that although there was the potential need for regulation and, therefore, legislation, writing laws as "new developments occur is not always so simple."[42]

Another Congressional Research Service report two years later bore similar conclusions. The report acknowledged that there was "not a simple task to furnish a precise definition", because genetic engineering was "discussed from so many different

[38] James M. McCullough, "Genetic Engineering: A Brief Review and a Bibliography," (Washington, DC: Congressional Research Service, 1970), 2.
[39] Ibid., 12.
[40] Ibid., 31.
[41] Ibid.
[42] Ibid., 12, 35.

viewpoints."[43] After a review of other published definitions, the author returned to the definition used by the 1970 CRS report. The 1972 CRS report provided more details in the examples it illustrated for the benefits and controversies of the research related to genetic engineering. The report did recommend a thorough evaluation of genetic engineering, including rate of scientific progress, costs of genetic treatment, legal questions about human experimentation, and the ethical impact of eventual decisions. The alternative to a comprehensive study "would include an acceptance of the haphazard evolution of the techniques of genetic engineering, to hope that the issues will resolve themselves, or to continue to face such issues under pressure without the detailed knowledge that forethought and evaluation could provide."[44]

Molecular biologists were not immune to using the term when addressing broader audiences. On October 25, 1974, Bernard Davis's editorial titled "Genetic Engineering: How Great is the Danger?" was published in *Science*. In his editorial Davis argued that there were two types of genetic engineering. The first was the replacement of defective genes, and the second was the cloning of an organism. The former was more complicated because the replacement of a single gene would likely cause other problems. This was because most genes are polygenic – the gene is responsible for more than one single trait. For cloning, Davis stated that there were more potential benefits and argued that the idea of a master race would be more feasible with "selective breeding" than cloning. The purpose of the editorial was not to explain the complexities of genetics to a scientifically

[43] James M. McCullough and Congressional Research Service Science Policy Research Division, Library of Congress, "Genetic Engineering: Evolution of a Technological Issue," ed. Research Subcommittee on Science, and Development, Committee on Science and Astronautics, and US House of Representatives (Washington, DC: US Government Printing Office, 1972), 1.
[44] Ibid., 44.

literate audience, but to ask for their help. “It seems important for scientists to help the public to sort out these complex issues and avoid anxiety over improbably or distant developments. Such anxiety could lead to pruning of valuable major limbs on the tree of knowledge, rather than of branches with dangerous fruit.”[45]

That fall Davis, who supported recombinant DNA research, attempted without success to get a longer version of his *Science* editorial published in *The New York Times*.[46] Davis explained in a letter to the Philip Handler, president of the National Academy of Sciences, that he wrote the article for the *Times* in anticipation of “the need to balance the ill-informed articles that are bound to continue to appear on genetic engineering, and recognizing that it is hard to get public exposure for sober analyses by responsible scientists.”[47] In November, he wrote to a colleague lamenting his decision to use the term genetic engineering in his *Science* editorial. “Definitions are arbitrary. I don’t like ‘genetic engineering’ at all. However the term seems to be her to stay.”[48]

Though titled “Genetic Engineering, 1975,” the first Congressional hearing on recombinant DNA used the term genetic engineering only once. As the first of four witnesses in an hour-long hearing before the Senate Subcommittee on Health, chaired by Edward Kennedy, Stanley Cohen set the linguistic tone of the meeting from the start. By

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[45] Bernard Davis, "Genetic Engineering: How Great Is the Danger?," *Science* 186, no. 4161 (1974): 309.

[46] _____, "Genetic Engineering: How Great Is the Danger?," (ALS: D Med: Recombinant DNA Molecules, Organizing Com for int'l Conf on, Report, Synthetic Nucleic Acids, 1974-1975; Central Policy Files; NAA.), "Bernard Davis to Philip Hander, 27 November 1974," (Public Relations, General 1974; Central Policy Files; NAA).

[47] "Bernard Davis to Philip Hander, 27 September 1974," (Public Relations, General 1974; Central Policy Files; NAA).

[48] "Bernard Davis to Roger Master, 25 November 1974," (Public Relations, General 1974; Central Policy Files; NAA).

stating “The lay press, and some of the scientific press, have fancifully termed the methodology ‘genetic engineering.’ While technically correct, the choice of this term is unfortunate, since the term has long been associated with ethical and religious issues of human experimentation which are quite peripheral to the biological safety questions considered at Asilomar. It should be clearly stated at the outset that the engineering of the heredity of man was not the subject of the Asilomar meeting, nor will it be the subject of my testimony today.” Cohen did not address the issue of genetically manipulating human genes, nor did the rest of the witnesses, nor any of the senators.[49]

Unfortunately for Cohen and other scientists performing recombinant DNA research, the debates of the 1970s about recombinant DNA did not abide by the boundaries he set at the 1975 hearing. There were other terms that were put forward to disentangle recombinant DNA from genetic engineering. The one that still remains today, though not nearly as ubiquitous as genetic engineering, is synthetic biology. Coined in 1974 by Polish geneticist Waclaw Szybalski, synthetic biology is the design and creation of new elements and models of biological systems. For Szybalski recombinant DNA provided molecular biology an opportunity to enter a new phase of creation and not description. Recombinant DNA, however, did change the definition of genetic engineering to what we understand it to mean today as “the alteration of the genome of an organism by laboratory techniques, esp. by the insertion, alteration, or removal of a gene.”[50]

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[49] US Congress. Senate. Subcommittee on Health, *Genetic Engineering, 1975*, 94th Cong., 1st sess., 1975.
[50] "Genetic Engineering."

Organization of this Dissertation

Chapters one, two and three set the scientific and social context in which the recombinant DNA debates originated. It is important to place the scientists who debated the future of recombinant DNA properly within their scientific and social contexts to understand what the changing dynamics, both internally and externally, were that influenced their decisions. The 1970s were a time of great change in the field of molecular biology. The role scientists also changed particularly their role in social activism.

Chapter two consists of two interrelated topics: the rise of both molecularization of biology and the environmental movement in the 1960s through the early 1970s. Though not dependant on each other, the concurrent growth of the two proved mutually beneficial. Molecularization was a process in the late 1960s by which advances in molecular biology techniques were being adopted and used in other fields of biology. This in turn expanded the field in size and scope. Although the conservation movement in the United States stretched back the early decades of the twentieth-century, the environmental movement was a broad coalition of citizens that is the one of the most enduring social movements of the late 1960s. Their rise in the mid through late 1960s had little national impact until increased lobbying efforts yielded the National Environmental Policy Act (1970) – the first significant piece of environmental regulation. The act set federal standards for acceptable pollution levels and required all federally funded projects to complete environmental impact studies (EIS). The EIS was a public document of a thorough scientific evaluation of the potential effects that the project may have on the environment an agency's or federally funded proposed project. Research by molecular

biologists provided scientific legitimacy and evidence to the environmental movement. The legitimacy and evidence translated into boosts in members in environmental organizations as well as a popular momentum to pass federal environmental laws and regulations. The environmental movement, in turn, allowed molecular biologists, like Joshua Lederberg, a relatively non-controversial issue for public expression of social responsibility. What the two also shared was that they both worked within traditional organizational boundaries.

Chapter three explores the emergence of radical scientists and the impact that they had on the changing relationship between science and society. The radical science movement was an extension of the New Left and the scientists protested about the use (or misuse) of science and technology by the Department of Defense, especially in the Vietnam War. The first major protest by scientists was the March 8, 1968 panel discussions, lectures, and events open to the public that occurred in universities across the country. The chapter focuses extensively on Science for the People. Science for the People was a New Left group in organization, structure methodology, and ideology. Originally founded by a University of California, Berkeley theoretical physicist in 1968, Charles Schwartz could not get his professional organization to address social concerns, specifically the Vietnam War. The group quickly rose to national prominence with its public protests by the Boston Chapter, and with its radical stance against traditional scientific organizations. The chapter also focuses on the how, during this period, there were changes within the scientific establishment leading to the first biological weapons ban, as well as, scientific societies allowing dissenting view to be presented at their annual meetings.

Chapter four provides scientific background to understand the pioneering work and discoveries that led to Paul Berg's proposed first recombinant DNA experiment. The first half of the chapter centers on Joshua Lederberg, whose work with *Escherichia coli* provided the foundation for bacterial genetics. The second half of the chapter centers on the role discoveries in molecular biology in 1960s had in providing the tools for Berg to propose the first recombinant DNA experiment. The chapter concludes with the first proposed recombinant DNA experiment.

Chapters five and six examine the reactions to the first proposed and first successful recombinant DNA experiments. Chapter five begins with the criticism of Berg's proposed experiment and why he determined to postpone it. The concerns about the experiment also initiated at debate in the molecular biology community about the safety of experiments that use biohazards. Just as concern about Berg's experiment began to wane, Stanley Cohen and Herbert Boyer design and successfully perform a simplified method for creating a recombinant DNA molecule.

Chapter six begins with the reaction to the Cohen and Boyer experiment at an exclusive scientific conference. The attendees were so concerned about the potential dangers of recombinant DNA experiments that they broke with the traditional private nature of the conference and wrote a letter to the president of the National Academy of Science and the letter was also meant for publication in the journal *Science*. The majority of the chapter follows the slow and halting work of the Academy as it responded to the concerns expressed in the letter. The result was a press conference and publication of a report detailing a voluntary deferral of two types of recombinant DNA experiments, and recommendations for an international conference, as well as, recommendations for the

NIH to create a committee to evaluate the safety of recombinant DNA experiments. The chapter concludes with an analysis of the immediate reaction by the scientific and lay press to the Academy report.

Chapter seven picks up with the role the press played in the early weeks following the release of the Academy report. Specifically it deals with the attempts the lay press made to explain the science of recombinant DNA, as well as potential benefits and hazards. It also discusses what scientists were choosing to do or not do in the face of a voluntary moratorium on two types of recombinant DNA research.

Chapter eight focus on two of the recommendations made by the Academy report. The chapter begins with the difficulties in planning and hastily arranging an international conference to essentially determine the fate of recombinant DNA research. The second half of the chapter details the constant and slow movement by the NIH to complete their specified recommendation.

Chapter nine deals with the events in the fall and early winter that occurred outside of the NAS and NIH, which influenced the Asilomar Conference, Three events, however, occurred outside of the NAS and NIH that would influence and help shape the Asilomar Conference. The first was an address by an eminent physicist. The second was the renewed interest in recombinant DNA at year's end by the scientific and lay press. And third, the Ashby Committee, which formed by the British government a few weeks after the publication of the CRDM Report, released their findings.

The concluding chapter is exclusively the culmination of the essential first years of the debate. The chapter makes extensive use of the audio recording of the Asilomar Conference. The focus of the chapter is on the role that non-scientific arguments had in

shaping the eventual consensus. The chapter will also evaluate the consensus reached at the Asilomar Conference, and how that marked the end of the initial debates and how it impacted the public debates between 1975-1978.

Finally, there is a bibliographic essay at the end of the dissertation. It discusses in detail the various sources available on recombinant DNA.

Chapter 2: "Nothing Short of Revolutionary"

The question of where to begin a study of the recombinant DNA debates is a problem every author faces. Previously, almost every author answered this problem by starting with the creation of the man-made recombinant molecule or a brief primer on basic genetics. Beginning the story with the creation of the man-made recombinant molecule, however, misses an important aspect of the tension leading to that event. It misses the movements inside science and outside of science that drove the recombinant DNA debate public. It misses the events that then drove the, at times very contentious debates. These prior histories largely ignore the state of molecular biology prior to development of recombinant DNA techniques. They fail to explain why molecular biology spread rapidly to other segments of the biological community, and how it had positive and negative effects within the biological community. To truly understand how recombinant DNA became such a contentious issue, recombinant DNA and the initial steps taken by scientists must be evaluated in the context of larger social and scientific changes.

The second part of this chapter deals with the rise of the environmental movement, which occurred concurrently with the molecularization of biology. The environmental movement grew out of the conservation movement of the early part of the century, but owed a great deal to the research and the increasing social consciousness of scientists, especially biologists. Research was a double-edged sword for scientists. It exposed the dangers of pollutants, but it also buttressed the argument for environmental regulations that could have the effect of constraining future research.

Chemicals, radiation, and other byproducts of industrialization were polluting the environments. Beyond rivers on fire, oil slicks on beaches, and smog engulfing cities, scientists were discovering the damage pollution was doing on a molecular and genetic level. DDT was the cause the shells of eggs to be too brittle, and geneticists were discovering that these pollutants were causing irreversible genetic mutations. The rise of the environmental movement was in reaction to these problems. The movement was a large and initially bi-partisan. Solutions to environmental problems included simple things that could be done at the personal level to lobbying for federal regulations. Though not anti-science, the environmental movement serves as a measuring stick for the public's willingness to get directly involved in science through the legislative and regulatory process. The most fundamental regulator change occurred with the enactment of the National Environmental Policy Act (NEPA) in 1970. NEPA's greatest impact was creating a set of uniform environmental standards throughout every federal agency, and requiring every federally funded project to carry out a publicly available environmental impact study.

The Rise of Molecular Biology

The mid 1960s through the early 1970s marked a time a dramatic change inside and outside of biological laboratories. These changes affected the way molecular biologists thought of their research and that research's relationship to society. These years created an atmosphere of fear, anxiety, and social consciousness about the surrounding molecular biologists. This uneasiness played itself out first within the scientific community and then publicly during the recombinant DNA debates of the

1970s.

During this time period, new advances within molecular biology came with increasing momentum that some prominent molecular biologists began actively referring to it as a revolution in biology. Describing the changes in molecular biology in testimony before Congress in 1968, Nobel laureate Arthur Kornberg described the events in molecular biology as "nothing short of revolutionary… And because of these tremendous advances, the essence of biology has become comprehensible to the high school student and nonscientist…Progress during the past decade has been more rapid than we expected. We have a firmer grasp of what the gene is chemically, and how to understand its function than we did 10 years ago."[51] The following year, molecular biologist and Nobel laureate Salvador expressed the view of other biologist that what was occurring was not a revolution, but the "scientific fulfillment" of modern biology that began with Darwin and was ending with the "understanding of the nature of, function, and changes of the organic substrate of evolution, *the genetic material*."[52] Whether it was a revolution or fulfillment, molecular biologists were excited and concerned about the prospects for their research. Some biologists viewed their ability to understand and harness the mechanisms of genetics as having both the potential to solve genetic problems, while others viewed it ass a "terrifying power" capable of unintended consequences.[53]

Historian of science Michel Morange has defined this period as the "molecularization" of biology. Laboratories and research programs sharply reoriented

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[51] US Congress. Senate. Subcommittee on Government Research, *National Commission on Health and Science Policy*, 90st Cong., 2nd sess., 1968, 40, 53.
[52] Salvador E. Luria, "Modern Biology: A Terrifying Power," *The Nation*, 20 October 1969, 406.
[53] Ibid.

themselves towards the study and better understanding of higher organisms, their development, and mechanisms for different biological pathways involving those organisms.[54] Due to an increase in the size and scope of molecular biology by the early 1970s, this so called molecularization led to breakthroughs in understanding the form and function of the organisms at their molecular level. The molecularization also brought about growing pains.

Molecular biology is considered a very modern science. During its formative years, molecular biology was not even its own academic discipline. Instead, molecular biology grew out of other established fields in biology, such as genetics, microbiology, biochemistry, and biophysics. Historian of molecular biology Horace Freeland Judson, found perhaps the first attempt at a definition of molecular biology by one of its earliest members and intellectual fathers, Francis Crick. In a Medical Research Council (UK) grant application in 1947, Crick defined what he intended to study as:

> The particular field which excites my interest is the division between the living and non-living, as typified by, say, proteins, viruses, bacteria and the structure of chromosomes. The eventual goal, which is somewhat remote, is the description of these activities in terms of their structure, i.e. the spatial distribution of their constituent atoms, in so far as this may prove possible. This might be called the chemical physics of biology.[55]

Despite the mouthful of a definition, Crick's work with James Watson in determining the structure of DNA in 1953 led directly, in the words of Ernst Mayr, to the "phoenix-like

[54] Michel Morange, *A History of Molecular Biology* (Cambridge, MA: Harvard University Press, 1998), 179.
[55] Horace Freeland Judson, *The Eighth Day of Creation: Makers of the Revolution in Biology*, Expanded ed. (Plainview, NY: CSHL Press, 1996), 88.

rise" of molecular biology.[56] Molecular biology borrowed heavily from biochemistry, biophysics, and genetics, but was (and is) without firm disciplinary boundaries because of the way it understood different structures and biological pathways in a cell at the molecular level. Molecular biology came with different set of tools and a different style of analysis than older scientific disciplines.[57]

It also came with a different way of training graduate students. Traditionally fields that worked with pathogens required courses on the safe handling of biohazards. For example, microbiologists, who regularly researched a lethal viruses and bacteria, were required to take courses in medical epidemiology as well as proper laboratory safety. Due to the breadth of research topics available to molecular biologists, however, biohazard and laboratory safety training was limited. The results of which were that many of the newly minted PhDs were unprepared to properly handle biohazards that they might research.

The granting of the first PhDs in molecular biology was a defining characteristic of the growth of molecular biology as a unique field. Prior to the first PhD in the field of microbiology, biologists who called themselves molecular biologists were trained in a particular biological discipline and typically used the tools and techniques of molecular biology to carry out their experiments. The first PhD in molecular biology was awarded in 1969. The number of PhDs awarded in molecular biology remained relatively constant until 1980. After 1980, molecular biology began a rapid growth. By the last decade of the twentieth century, molecular biology became one of the top three PhDs awarded in

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[56] Ernst Mayr, *The Growth of Biological Thought: Diversity, Evolution, and Inheritance* (Cambridge, MA: Belknap Press, 1982), 574.
[57] Judson, *The Eighth Day of Creation: Makers of the Revolution in Biology*, 177.

biological sciences.[58]

The granting of the first PhDs in molecular biology occurred in the middle of Morange's molecularization of biology. Scientists characterized this time period from, 1965 to 1972, by the "number of groups and laboratories working on molecular biology, the creation of institutes devoted to the subject, and above all an expansion of the molecular vision."[59] This molecularization happened in a few distinct ways throughout a gradual process. First came a general increase in funding towards research at the molecular level at the expensive of other disciples. This led to a shift in grants, which was followed by a shift in experimental emphasis in the field. Eventually the shift in experimental emphasis resulted in a shift in the subject matter of professional journals toward molecular biology. As mentioned above, the discipline became a unique field during this period. Departments and courses at universities were set up expressly for the purpose of teaching molecular biology. The resource allocation at a university inevitably shifted towards molecular biology at the expense of other biological fields. If a department was not created, molecular biology was added as a change or update in curricula of established fields such as genetics or biochemistry.[60]

During this same period federal funding for basic and applied research began contracting. In 1970, geneticist and Nobel laureate Joshua Lederberg called this situation "the fiscal emasculation and political dismemberment of health research now under way

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[58] National Science Foundation and Division of Science Resources Statistics, "U.S. Doctorate in the 20th Century," ed. Lori Thurgood, Mary J. Golladay, and Susan T. Hill (Arlington, VA: National Science Foundation, 2006). The NSF statistics do not Biochemistry was the biological field with the most PhDs awarded during these years. Why there was rapid growth in throughout the last two decades of the twentieth century is explored in chapter 8.

[59] Morange, *A History of Molecular Biology*, 167-72.

[60] Ibid., 173.

in [the Department of Health, Education, and Welfare]."[61] Historian Susan Wright documented that the "overall decline in federal funding in the late 1960s pinched the universities," and the federal money spent between 1967 and 1974 "came with strings attached" in the form of tangible results.[62] Molecular biology was a field yielding results during this period that allowed scientists to [63]

The eve of molecularization brought the dawn of an age of biotechnology. Spawned by the creation of the recombinant DNA technique, was a technique so simple that almost any researcher could employ it to yield tangible results and funding. Funding, however, was not the driving factor. James Watson described recombinant DNA as "no ordinary scientific advance…Wide new classes of experiments were made feasible, possibly allowing research to be accomplished over the next decade that might not otherwise be accomplished over the next century."[64] Experiments with eukaryotic organisms were one class on new experiments of great interest to scientists. Due to their relative genetic simplicity and quick reproduction, the majority of molecular biological experiments used bacteria and viruses. Recombinant DNA opened up the possibility of understanding how higher organisms functioned and developed.

Geneticists had long known about the mutagenic properties of radiation and certain chemicals, as causing mutations was a regular fixture in classical genetics

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[61] Joshua Lederberg, "Politics Nullifies Science in Environmental Studies," *The Washington Post*, June 13 1970.
[62] Susan Wright, *Molecular Politics: Developing American and British Regulatory Policy for Genetic Engineering, 1972-1982* (Chicago, IL: University of Chicago Press, 1994), 28-29.
[63] See Chapter 4 for the detailed account of the discoveries that were happening during this period.
[64] James D. Watson and John Tooze, *The DNA Story: A Documentary History of Gene Cloning* (San Francisco, CA: W.H. Freeman and Co., 1981), 529.

experiments. Experimental programs, such as Thomas Hunt Morgan's famous experiments on *Drosophila*, created and studies mutations to understand how genes worked.[65] In the late 1960s and early 1970s many molecular biologists, particularly geneticists, began to publicly express their concerns about the increase in mutations caused by pollution. Joshua Lederberg was one of the most outspoken, and he occasionally used his weekly *Washington Post* science column to warn about environmental dangers to the human genome.

In a 1969 column on the basics of human mutations, he first pointed to "many environmental factors are either known to add to our burden from mutations mishaps, as in the case of radiation, or suspected of doing so, as in the case with chemicals."[66] In a column a year later, Lederberg focused on the need for a better science and support financially and politically from lawmakers for "many of our environmental dilemmas."[67] Lederberg's focus on the environment led to James Watson, who co-discovered the structure of DNA in 1953, to chide him in a 1971 article in *The Atlantic Monthly*. Watson claimed that Lederberg ("Stanford's celebrated geneticist") now seemed "bored with further talk" with some aspects of genetics "implying that we should channel our limited influence as public citizens to the prevention of the wide-scale, irreversible damage to our genetic material that is now occurring through increasing exposure to man-created

[65] For how *Drosophila* became a model organism in classical genetics see Robert E. Kohler, *Lords of the Fly: Drosophila Genetics and the Experimental Life* (Chicago, IL: University of Chicago Press, 1994).
[66] Joshua Lederberg, "Human Mutation Can Scramble the Coded Genetic Message of DNA," *The Washington Post*, October 4 1969.
[67] Lederberg, "Politics Nullifies Science in Environmental Studies."

mutagenic compounds."[68] Despite Watson's friendly chastisement, Lederberg and many other biologists continued to provide the environmental movement with new research and the academic support it needed to succeed.

The Environmental Movement

On April 22, 1970, millions of Americans participated in the first Earth Day. Activities ran the gamut from teach-ins to non-violent rallies to clean up projects to concerts to even a shrimp trawler bringing a petition to Washington, DC for the Secretary of the Interior.[69] Schools were closed. Congress took the day off. In the words of Denis Hayes, the 24 year-old Harvard Law student and organizer of Earth Day, "traditional characterizations of left and right are irrelevant at this stage of the environmental movement."[70] Earth Day was the largest public expression of concern for the environment in American history, with an estimated twenty million participants, who stretched across the political spectrum from college students to housewives to Nixon's Silent Majority. The idea to have a dedicated teach-in day for the environment was originated by Sen. Gaylord Nelson, a Democrat from Wisconsin committed to the environmental cause, in 1969. Soon after, Environmental Teach-in Inc., a Washington based student run organization, began organizing Earth Day.[71] By eschewing a confrontational stance and remaining nonpartisan the first Earth Day, the organizers

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[68] James D. Watson, "Moving toward the Clonal Man: Is This What We Want?," *The Atlantic Monthly*, May 1971.
[69] Gladwin Hill, "Earth Day Goals Backed by Hickel," *The New York Times*, 22 April 1970, 36.
[70] ———,"Earth Day Sponsors to Stay Together," *The New York Times*, 22 April 1970, 35.
[71] ———, "Activity Ranges from Oratory to Legislation," *The New York Times*, 23 April 1970, 3.

gently excluded radical movements from the left and the right, as well as anti-war protesters, and largely avoided any organized violence or arrests.[72] Earth Day also allowed the participation of industries potentially hostile to the environmental movement, such as Scott Paper Company, to announce a $36 million project to control pollution at one of its plants.[73] By all accounts, the first Earth Day was a great success. After such a great success, it appeared that the environmental movement with its broad spectrum of support was going to play a large role in the nation's politics for years, if not decades, to come.[74]

Although its actual political influence never reached the high euphoric expectations shortly after the first Earth Day, the environmental movement was one of the one of the most enduring social movements of the 1960s. In many ways, 1970 was its most successful year. A major piece of legislation passed late in 1969 and the President signed it into law on the first day of the new year. The nation celebrated the first Earth Day in numbers that were never reproduced. The government created an agency in the executive branch to centralize the disparate environmental functions scattered throughout all of its numerous agencies and administrations. The years leading up to 1970 saw the environmental movement gain a broad and increasingly organized group of adherents that energized the movement and helped push bi-partisan legislation easily through Congress. The years immediately following 1970 saw the flowering of many of the national and

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[72] The most notable disturbance was a group of fifteen protestors who were arrested when the blocked a corridor at Logan International Airport in Boston. They were there to protest supersonic transport planes and their creation of noise pollution. Ibid., 30.
[73] Richard Harwood, "Earth Day Stirs Nation," *The Washington Post*, 23 April 1970, A1.
[74] For an account of how President Nixon approached and dealt with the first Earth Day see, J. Brooks Flippen, *Nixon and the Environment*, 1st ed. (Albuquerque, NM: University of New Mexico Press, 2000), 1-16.

international environmental groups that make up the majority of environmental movement today. These organizations using the new environmental laws, especially the National Environmental Policy Act, enabled public and legal challenges to new scientific advances, including recombinant DNA. In understanding how these organizations formed and the impact of environmental laws in the early 1970s, one must understand how the environmental movement formed and influenced the passage of legislation.

The rapid rise of the environmental movement was a product of its time. The unbridled post-war economic growth permitted an increasing segment of the population to look at the environment new ways. The most notable of these was to see that prosperity and the post-war ethic of economic and technical progress had consequences for society. One of the most noteworthy of these became the environment. The physical manifestations of growth and industrialization became increasingly more frequent and obvious. Air pollution, always a problem of heavy industrialized cities such as Pittsburgh, became more problematic throughout the 1950s and into the 1960s as there was little federal action.[75] The safety of surface water pollution became more problematic as the Great Lakes, together the largest body of fresh water in the world, were, by the late 1960s, polluted almost beyond description. The introduction of non-biodegradable synthetic detergents created plumes of suds in local sewage plants and appeared in local drinking water as well as in rivers and lakes. The Cuyahoga River in Cleveland, which previously caught on fire, again erupted in flames in 1969.[76] The landscape of America

[75] As an example, in October 1954 the smog was so heavy in Los Angeles that the city's public school system closed for most of that month.

[76] Hal Rothman, *The Greening of a Nation?: Environmentalism in the United States since 1945,* ed. Gerald D. Nash and Richard W. Etulain, Harbrace Books on America since 1945 (Fort Worth, TX: Harcourt Brace College Publishers, 1998), 98-100.

was literally littered to the point of distraction by garbage as well as by man made items such as billboards.[77] The progress that allowed for more leisure time to enjoy the outdoors did not merely alter, but began destroying the American wilderness.

The environmental movement of the 1960s built upon the Progressive Era conservation movement in America. Exemplified by John Muir, the Sierra Club,[78] and the National Audubon Society,[79] the goal of these groups was to preserve nature from the excesses of the exploitation of the nation's natural resources through industrialization and development. Although the federal government set aside land for preservation with national parks, they were also concerned with the management of natural resources, such as timber, water, and minerals, on federal lands. The Executive Branch created agencies such as the Bureau of Reclamation (1902), the U.S. Forest Service (1905), and the National Park Service (1916) to manage the resources for use by its citizens.[80] Although the government and the conservation organizations conflicted over particular projects,[81]

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[77] Congress passed legislation in 1958, which attempted to regulate billboards with little effect. Lady Bird Johnson, when she became First Lady, was a formidable advocate of urban and rural beautification projects.

[78] In 1892, two years after the creation of Yosemite National Park, the Sierra Club was founded in San Francisco, CA for the protection and promotion of the new park. John Muir served as its first president.

[79] Established as a national organization in 1901, the National Committee of State Audubon Societies changed its name in 1940 to the National Audubon Society.

[80] These agencies were not centrally controlled, though most of the early agencies were within the Department of the Interior, such as the Bureau of Reclamation, the US Geological Survey, the National Park Service, and the forerunner of the Fish and Wildlife Service. The Bureaus of Fisheries and Biological Survey transferred from the Department to Agriculture to the Department of the Interior in 1939, and in 1940, they were combined and renamed the Fish and Wildlife Service. A notable exception this trend was the Forest Service, which remains in the Department of Agriculture.

[81] The most public of these conflicts was the Sierra Club's attempt to stop the construction of the Hetch Hetchy dam in 1913. Though they lost this battle, the Sierra Club spent the next five plus decades building a larger and more diverse organization. It became the preeminent organization for stopping dam construction in the West. For a

their joint efforts increased the number of national parks and the protection of wildlife throughout the United States. During this time hunters, anglers, and outdoorsmen established grassroots organizations, such as the Izaak Walton League of America (IWLA)."[82]

During the Great Depression, the Roosevelt administration placed an emphasis on economic stimulus through large-scale projects that created jobs by controlling resources, notably soil conservation, which helped ease the Dust Bowl, and mitigated flooding. That conservation also harnessed and developed the nation's natural resources. The most famous examples of the latter are the Tennessee Valley Authority and the Hoover Dam, both of which aided in electrification and furthered rural development of the country. When the United States entered the Second World War, the emphasis shifted to the nation's defense and the production of weapons, vehicles, and materiel for the war. Natural resources were extracted and factories poured voluminous amounts of industrial waste into the land, water, and air without much thought. During the war years, the conservation organizations remained relatively dormant, only to reemerge in the late 1940s and early 1950s as the nation returned to a prosperous peacetime footing.[83]

The Depression era infrastructure improvements along with the new affluence of the post-war years brought with it more recreation time for a new segment of the

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history of the construction of dams in the American West and the success, conflicts, and failures they created see, Marc Reisner, *Cadillac Desert: The American West and Its Disappearing Water*, Rev. and updated ed. (New York, NY: Penguin Books, 1993).

[82] Christopher J. Bosso, *Environment, Inc.: From Grassroots to Beltway*, Studies in Government and Public Policy (Lawrence, KS: University Press of Kansas, 2005), 29.

[83] Riley E. Dunlap and Angela G. Mertig, "The Evolution of the U.S. Environmental Movement from 1970 to 1990: An Overview," in *American Environmentalism: The U.S. Environmental Movement, 1970-1990*, ed. Riley E. Dunlap and Angela G. Mertig (Philadelphia: Taylor & Francis, 1992), 1-2.; Bosso, *Environment, Inc.: From Grassroots to Beltway*, 33.

population who could enjoy the natural beauty of the country. What they found, however, were not the unspoiled wilderness they might have imagined, but rather the growing effects of industrial pollution. The conservation organizations tapped into this feeling and promoted the ideas of sustaining natural beauty and natural resources. The organizations' memberships increased dramatically over the decade. The Sierra Club, which allowed the development of chapters outside of California for the first time after the war, increased their average membership almost two hundred and forty percent between 1950-51 and 1960-61, and another one hundred and ninety percent by 1965-66. Over the same periods of time, the National Audubon Society grew nearly one hundred and ninety percent and eighty percent, respectively. In fact, all of the national conservation movements increased in membership during these years.[84]

The Influence of Science

By the 1960s, scientists became instrumental in influencing the environmental movement. The most notable works were Rachel Caron's *Silent Spring*, Paul Ehrlich's *The Population Bomb*, and Garrett Hardin's "Tragedy of the Commons."[85] The most influential and enduring of these works was Carson's *Silent Spring*, published in September 1962.[86] From an early age, Carson, born outside of Pittsburgh, showed an interest in the natural world. She turned this interest into her career by graduating college

[84] The actual membership averages for the years 1950-51, 1960-61, and 1965-66, respectively, are: Sierra Club, 7,000, 16,500, 31,000; National Audubon Society: 17,000, 32,000, 40,500. Bosso, *Environment, Inc.: From Grassroots to Beltway*, 35.
[85] Rothman, *The Greening of a Nation?: Environmentalism in the United States since 1945*, 84-86.
[86] Preceding the publication of the book, Carson published a serialization of *Silent Spring* in *The New Yorker* earlier in 1962.

with distinction in 1929 with a degree in biology. That year, she was awarded a fellowship to the prestigious Woods Hole Marine Biological Laboratory. She continued her education at Johns Hopkins University where she earned her M.A. in zoology in 1932. Following the death of her father, Carson first worked for the Bureau of Fisheries, followed by thirteen years in the Fish and Wildlife Service. She then became the editor-in-chief of all publications from the Department of the Interior. While at the Department of the Interior, Carson published her work in popular magazines and eventually in books. Though the amount of money she earned writing allowed her to retire early from the government, her research and interest in the effects of synthetic pesticides while at the Department of the Interior continued after she left.[87]

Synthetic pesticides became more potent and ubiquitous in the 1940s, especially inorganic compounds such as phosphorous-based insecticides, as well as dichloro-diphenyl-trichloroethane (DDT). Though first synthesized in the 1870s, the insecticidal properties of DDT were not discovered until the 1930s. Due to this discovery, DDT, the US military, used it extensively during the Second World War, notably in the Pacific Theatre to control the mosquito population.[88] After the war, the US government made DDT available as an agricultural insecticide, and production and use of it and other inorganic pesticides skyrocketed in the late 1940 and through the 1950s. Chemical manufacturers and the government both carried out little testing as to the safety of theses

[87] For biographies of Rachel Carson see: Mark H. Lytle, *The Gentle Subversive: Rachel Carson, Silent Spring, and the Rise of the Environmental Movement*, New Narratives in American History (New York, NY: Oxford University Press, 2007).; and Linda J. Lear, *Rachel Carson: Witness for Nature* (New York, NY: Henry Holt, 1997).

[88] "For his discovery of the high efficiency of DDT as a contact poison against several arthropods," Swiss chemist Herman Müller was awarded the Nobel Prize in Physiology or Medicine in 1948.

pesticides to humans or their effects on the environment. Though their initial use in agriculture was highly effective in reducing the numbers of problematic pests and increasing yields, after receiving a letter from a friend concerning the death of songbirds around her after aerial spraying of DDT to reduce the mosquito population, Carson examined the effects of DDT on the natural world. After carrying out her research, Carson discovered that DDT led to the death of animals, especially birds, and humans. In *Silent Spring*, Carson's examination of the toxicity of DDT on the environment tapped into the nascent, but growing concern over the consequences of unfettered economic and technological progress.[89]

Two additional works by scientists warned about the problems the world was facing due to the rate of population growth and were critical of the use of technology as a solution to the problems were published in 1968, Ehrlich's *The Population Bomb*, and Hardin's "Tragedy of the Commons." The first was a literary smash and sold more than three million copies. The second was the presidential address to the Pacific Division of the American Association for the Advancement of Science (AAAS), which was subsequently published in *Science*.[90] Ehrlich, a highly respected entomologist specializing in butterflies, wrote *The Population Bomb* at the suggestion of David Bower, the executive director of the Sierra Club, after an article he wrote the previous year in *New Scientist* predicating a great worldwide famine within the next two decades. In his

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[89] The year after the publication of Silent Spring, President John F. Kennedy in response to the intense public debate about the book ordered his Science Advisory Committee to examine the claims made by Carson. On May 15, 1963, the Committee issued their report entitled "The Use of Pesticides," which validated Silent Spring, and called for restricting the use of and more control over pesticides.

[90] Paul R. Ehrlich, *The Population Bomb* (New York, NY: Ballantine Books, 1968).; Ehrlich, *The Population Bomb*, Garrett Hardin, "The Tragedy of the Commons," *Science* 162, no. 3859 (1968).

book, Ehrlich took an even dimmer view of the future through Malthusian glasses, where he saw a world in which the population growth was going to outstrip natural resources, notably agricultural production, leading to massive famines and subsequent starvations, as well as, an intense competition for the remaining resources. Ehrlich challenged not only the Catholic Church, over reproductive control, but also the market based goals of the Green Revolution, whose earliest gains were a mirage as third world countries became net importers of agricultural stuffs again.[91]

Hardin, a professor of biology at University of California, Santa Cruz, also had a Malthusian slant on the world as when he viewed the population problem as a class of problems that had "no technical solution" – a null set in game theory – and he reached a similar conclusion to Ehrlich.[92] The analogy Hardin used is that of an open pasture, a commons, where herdsmen lead their flock to graze. How the herdsmen use the pasture and marshal their resources decided the fate of not only the pasture, but also the flock and the herdsmen. For Hardin, the only conclusion that reachable was that each herdsman would continually increasing his flock, since herdsman received all of the proceeds and did not spend money on the pasture, and because overgrazing is shared by all of the herdsmen – "Freedom in a commons brings ruin to all."[93] Though the United States worked out a solution for the tragedy of the commons within our National Parks, we have not worked it out for pollution, which has become a major problem as our population has increased density, has become greater and the commons are overloaded with chemical and biological waste. The corrective for the problem with pollution was an infringement

[91] Ehrlich, *The Population Bomb*.
[92] Hardin, "The Tragedy of the Commons," 1243.
[93] Ibid.: 1244.

on personal liberties, specifically a redefinition of property rights and restrictions on breeding, which would give a greater freedom for all and preserve the commons.

For all three scientist-authors, technology was not the solution for the environment, and for Carson it was a direct threat. Though not anti-science, the authors painted a picture of an environment overstretched and in need of societal solutions. They advocated that the environment was a shared responsibility. Environmental regulations, at a time of growing individual freedoms, were viewed as the solution to environmental problems.

Legislation, Regulation, and Lobbying

The first major piece of environmental legislation of the 1960s was the Clean Air Act (1963), which only set up funding to study air quality, to study the potential methods and costs for cleanup, and to provide technical assistance to states. The act, however, let the federal government intervene at the request of the state, which led to a dismal enforcement record. Individual states at that time also began passing local antipollution laws and ordinances. The Wilderness Act (1964) establishing the National Wilderness Preservation System to secure the wilderness for present and future generations. The Water Quality Act (1965) further expanded the federal role in regulating the nation's water by setting standards for water quality that were enforceable by the state and federal government. The bill, however, preserved the near immunity of offshore oil drilling as the law made it nearly impossible for the federal government to sue in the case of an oil spill. The final act signed into law in the 1960s was the Wild and Scenic Rivers Act (1968), which was aimed at protecting rivers from pollution, commercialization, and

development. It did not take long for some of these laws to face an environmental challenge. On January 28, 1969, a blowout at an oil-well platform off the coast of Santa Barbara, California, coated a thirty-mile stretch of shoreline with 235,000 gallons of crude oil. On June 22 of the same year, the Cuyahoga River at Cleveland caught fire again. Both of these events captured national headlines and heighten the public desire for more federal regulation of the environment. The public's pleas did not go unheard. In late 1969, Congress easily passed the National Environmental Policy Act (NEPA) – the most comprehensive piece of environmental legislation in US history.

On New Years Day morning from his "Western White House" in San Clemente, California, President Richard Nixon spoke to the press shortly after signing the National Environmental Policy Act into law. Although he did not support or fight the bill as it worked its way quickly through Congress in late 1969, Nixon took the opportunity at the bill's signing to take credit for it in a cleverly crafted statement that captured the spirit of the growing popular support for environmental issues as a common goal and as a commitment to environmental problems through a repetitive use of "we." "So unless we start moving on it now – there is a lead time – unless we move on it now, believe me, we will not have an opportunity to do it later…" The statement acknowledged the work of Congress and noted that the work before us would be "hard" and would include not "just air, water, and traffic, which are obvious ones, but it also involves open space, leisure time." Nixon's New Year's resolution for a better environment set into motion the greatest change in the US history of environmental regulation.[94]

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[94] Richard Nixon, *Public Papers of the Presidents of the United States. Richard Nixon: Contains the Public Messages, Speeches, and Statements of the President, 1970*, 6 vols., vol. 2 (Washington, DC: U.S. Government Printing Office, 1971), 1.

As a short and tightly worded law, NEPA's stated purpose was:

> To declare a national policy which will encourage productive and enjoyable harmony between man and his environment; to promote efforts which will prevent or eliminate damage to the environment and biosphere and stimulate the health and welfare of man; to enrich the understanding of the ecological systems and natural resources important to the Nation; and to establish a Council on Environmental Quality.[95]

Laid out in two Titles, the new law specified the new regulations and indicates how the new law governed federal agencies. The first section (101) of Title I of NEPA laid out six general reasons for the law including Congress' recognition "that each person should enjoy a healthful environment and that each person has a responsibility to contribute to the preservation of the environment."[96] The remainder of Title I (sections 102-105) detailed the implementation and the standing of the law in all federal agencies and how the new law should be carried out, specifically section 105, which mandated that it amended the statues of every federal agency, thus making it statutory law for the federal government.[97] Title II established the Council of Environmental Quality within the Executive Branch (CEQ).[98] The CEQ was a presidentially appointed three-member council that occupied the highest legal advisory board to the president, and whose actions

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[95] U.S. Congress, "The National Environmental Policy Act of 1969," in *Pub. L. 91-190* (1970).
[96] Ibid., 101 (c). The original wording of this section was "has a right to," but was replaced by "should enjoy" because of the legal issues that a right could entail. For a description of this change see Lynton Keith Caldwell, *Science and the National Environmental Policy Act: Redirecting Policy through Procedural Reform*, An Alabama Paperback in Public Administration (Tuscaloosa, AL: University of Alabama Press, 1982).
[97] Caldwell, *Science and the National Environmental Policy Act: Redirecting Policy through Procedural Reform*, 10.
[98] According to Flippen it was only Title II of the legislation that President Nixon objected to as he had set up an environmental council within his office in 1969.

were largely dictated by the chief executive.

The most enduring, and for some frustrating, part of the law was Section 102(2)(c) which created the Environmental Impact Study (EIS).[99] The EIS was a public document of a thorough scientific evaluation of the potential effects that the project may have on the environment an agency's or federally funded proposed project, which also required to provide detailed alternatives to the original proposed plan. This provision initiated a new level of accountability for each project, as well as, provided a way for the public to examine the rationale and decision making process on a project. The authors of the bill also displayed their understanding of the complexities of environmental problems by requiring a scientific study that directly influenced environmental policy and decision making beyond an individual EIS and even beyond the act.[100] The act, however, allowed exceptions for the military and other projects that would divulge state secrets.

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[99] 102(2)(c) Include in every recommendation or report on proposals for legislation and other major Federal actions significantly affecting the quality of the human environment, a detailed statement by the responsible official on –

(i) The environmental impact of the proposed action,
(ii) Any adverse environmental effects which cannot be avoided should the proposal be implemented,
(iii) Alternatives to the proposed action,
(iv) The relationship between local short-term uses of man's environment and the maintenance and enhancement of long-term productivity, and
(v) Any irreversible and irretrievable commitments of resources which would be involved in the proposed action should it be implemented.

Prior to making any detailed statement, the responsible Federal official shall consult with and obtain the comments of any Federal agency which has jurisdiction by law or special experts with respect to any environmental impact involved. Copies of such statement and the comments and views of the appropriate Federal, State, and local agencies, which are authorized to develop and enforce environmental standards, shall be made available to the President, the Council on Environmental Quality and to the public as provided by section 552 of title 5, United States Code, and shall accompany the proposal through the existing agency review processes;

[100] Caldwell, *Science and the National Environmental Policy Act: Redirecting Policy through Procedural Reform*, 14.

Immediately after its passage the federal agencies most effected were the Army Corps of Engineers, the Federal Highway Administration, the Department of Agriculture, and the Bureau of Reclamation, though within a few years most federal agencies had written their first impact statement.

Since the wording in the act for writing an EIS was non-specific and because no agency was required to write an impact statement previously, there was a learning curve to writing them. As political scientists Lynton Caldwell stated, "Misconstruction and misuse of the EIS was to be expected."[101] NEPA was amended in the late 1970s to correct some of the problems with impact statements.

This is not to say that some of the larger agencies that were directly affected by NEPA did not figure out ways to get their projects through the red tape. The Bureau of Reclamation "merely went through the motions of writing an environmental-impact statement; in the case of the Teton [Dam], it ran to fourteen pages and didn't say much of anything." The Teton Dam, although not a necessary water project, was of political importance in Idaho. Getting the impact statement through public examination was important to the Bureau. Shortly after construction of the Teton Dam, the Teton Dam collapsed becoming one of the largest disasters for the Bureau. Following the Teton, the Bureau decided on a different tact with their impact statements: "flood its critics with a tide of ink."[102]

Three weeks after signing NEPA into law, President Nixon delivered his first State of the Union Address. The president set out an ambitious agenda. He told the

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[101] Ibid., 22.
[102] Reisner, *Cadillac Desert: The American West and Its Disappearing Water*, 386. For a account of the Teton Dam see Reisner 383-433.

American people that protecting the environment would " require comprehensive new regulations," and proposed "the most comprehensive and costly program in this field in America's history" -- a five year, $10 billion dollar nationwide clean waters program. [103] For Nixon, despite the outward praise for environmental regulation in his address, the impact statements became a political problem for his administration.

The first problem arose when the impact statements became the chosen gateway for environmental lawsuits, especially after the courts' early ruling that NEPA should apply to all unfinished federally funded projects. The statements were used against Nixon's plans for an American built supersonic transport, and, more important, for the Trans Alaska Pipeline System (TAPS), which released its nine volume final EIS on March 20, 1972. By 1972, TAPS was a priority for the administration because of the looming energy shortage, and work projects were already delayed a couple years by environmental and non-environmental lawsuits.[104] Instead of continuing the battle in the courts, which were largely sympathetic to environmental concerns, the White House decided to take a different tact concerning energy. It put its weight behind legislation moving through Congress that would exempt nuclear power plants from writing an

[103] Nixon, *Public Papers of the Presidents of the United States. Richard Nixon: Contains the Public Messages, Speeches, and Statements of the President, 1970*, 12.

[104] The major set of non-environmental lawsuits was over Native consent and claims to both the land used by the proposed pipeline and to the oil. On August 15, 1972, the injunction prohibiting construction is dissolved when it is ruled that all requirements were met to allow TAPS to begin building. The U.S. District Court overturns the ruling in early February 1973. In a vote of 50-49 on July 17, 1973, the Senate passes the Gravel Amendment declaring that the Department of the Interior has met all of its requirements to begin construction. President Nixon signs the Trans Alaska Pipeline Authorization Act into law on November 16.

EIS.[105] During the hearing, complaints about NEPA and the EIS went beyond nuclear power plants and into highway construction and other development projects. Congress passed amendments to NEPA late in the spring exempting the Atomic Energy Commission, which Nixon quickly signed into law. The first amendment was only a precursor to events later in the decade and into the 1980s when conservatives with a deregulation agenda began gutting environmental regulations from hampering development and business.[106]

Environmental Organizations

The maturing of the environmental movement in the late 1960s also brought about a bevy of new environmental organizations. These new organizations differed greatly from those founded during the conservation movement earlier in the century. Their focus was no longer on a distinct geographic area or on a particular wildlife issues, rather their focus was on a broader ecological perspective of the world and the causes and effects of pollution. To succeed with their broader agendas, these new organizations had a more tactical and policy oriented focus on current environmental issues. To carry their missions these organizations focused their efforts on directly lobbying Congress,

[105] The hearings were a reaction to Calvert Cliff Coordinating Committee v. United State Atomic Energy Commission (AEC), where the plaintiffs sued the AEC using NEPA to stop the construction of a nuclear power plant on the Chesapeake Bay in Maryland because of the potential dangers of thermal pollution to the Bay. The U.S. District Court in Washington, DC, ruled on July 23, 1971 in favor of the plaintiffs because the defendants did not meet the requirements of NEPA.

[106] Flippen, *Nixon and the Environment*, 163-63. A good characterization of how conservatives perceived environmental regulation can be found in David Frum's discussion of the snail darter. David Frum, *How We Got Here: The 70's, the Decade That Brought You Modern Life (for Better or Worse)* (New York, NY: Basic Books, 2000), 217-19.

supporting candidates, initiating lawsuits, and on technology for direct mail and mass mobilization efforts. The majority also had a more centralized leadership structure in order to make quick tactical decisions for the organization.[107]

The first of these new environmental groups, Environmental Defense Fund (EDF) (1967) was followed quickly by Friends of the Earth (FOE) (1969), Natural Resources Defense Council (NRDC) (1970), Environmental Action (EA) (1970), and Environmental Policy Institute (EPI) (1972).[108] EDF and NRDC were founded by lawyers due to environmental court cases – EDF after a DDT spraying case on Long Island, and NRDC a case concerning a proposed hydroelectric power project on the Hudson River – and were financially supported by larger organizations.[109] The two organizations also shared a similar ethos to use science and the law approach to environmental advocacy. David Brower started Friends of the Earth (FOE) shortly after his resignation from the Sierra Club. FOE's goals were to combine scientific research with education outreach and direct lobbying with both a national and international focus. Environmental Action was the reincorporation of Environmental Teach-in, the student run group that organized Earth Day as an aggressive protest-oriented organization that

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[107] Robert Cameron Mitchell, Angela G. Mertig, and Riley E. Dunlap, "Twenty Years of Environmental Mobilization: Trends among National Environmental Organizations," in *American Environmentalism: The U.S. Environmental Movement, 1970-1990*, ed. Riley E. Dunlap and Angela G. Mertig (Philadelphia, PA: Taylor & Francis, 1992), 12-17, Bosso, *Environment, Inc.: From Grassroots to Beltway*, 39-47.

[108] Environmental Defense Fund dropped the "Fund" in 1998.

[109] EDF by the Audubon Society and the Conservation Foundation until they were able to gain support from the Ford Foundation, who funded NRDC. EPI origins lay in the founding of the Environmental Policy Center, which created EPI as its tax-exempt education and organizational. The two organizations merged in 1977 and kept the name EPI. EPI finally merged with FOE in 1989.

would focus less on education and more on electioneering and filing lawsuits.[110] EPI when the majority of FOE's Washington lobbying organization over leadership issues with David Brower. EPI remained in Washington as a broad environmental advocacy organization. By the mid-1970s the older conservation-based organizations were establishing Washington lobbying offices as well as legal defense funds.[111]

During this time some non-lobbying environmental organizations formed including Greenpeace (1971) and later in the decade Sea Shepherd Conservation Society (1977). Both of these groups espoused direct action as their method of environmental action. For Greenpeace it was non-violent action included putting members physically in the way of something they deem as wrong, as well as protesting and leading international environmental campaigns. In contrast to Greenpeace, the Sea Shepherd Society used violent direct action, including ramming boats, for the specific goal of protecting marine mammals.

Similar to the enthusiasm for the conservative-ear organizations in the early 1960s, the new environmental organizations grew quickly throughout the 1970s spurred on by the broad interest and participation in the environmental movement. EDF grew from an average membership in 1970-71 of 20,000 to 46,000 in 1980-81; FOE from 7,000 to 25,000; EA from 7,000 to 21,000; and NRDC 5,000-40,000.[112] The majority of the older organizations also underwent a growth spurt during these years though not

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[110] The announcement of this change came the day before the first Earth Day. Denis Hayes, the national coordinator, explained, "So far, the national office has been barred by the terms of its exemption from engaging in political activity," but after the change they will be "free to say and do whatever is necessary." As part of their reorientation the organization went from tax-exempt to non-tax-exempt status. "Earth Day Sponsors to Stay Together," 35.

[111] Bosso, *Environment, Inc.: From Grassroots to Beltway*, 42-44, 58.

[112] EPI was a none membership group.

approaching their growth rates during the 1960s. [113]

On the eve of Earth Day, Senator Gaylord Nelson voiced a concern heard throughout the decade when he stated that "Polluted countryside represents the antithesis of freedom."[114] The 1960s marked a time when the consequences of the post-war prosperity came into sharper contrast, especially for the environment where decades of polluting the air, water, and land had started to dramatically change the landscape and health of Americans. The modern environmental movement, however, was not without its paradoxes, and in the 1970s grappled with a major one: embracing science for environmental ends while criticize technology as solution to the very same problems on issues such as energy and biotechnology. In the 1970s the movement found it difficult to hold its broad coalition of members and its decade long political momentum. Indeed the decade saw an increase in major federal environmental laws and statutes in the first few years following NEPA: Clean Air Act (1970), ban of DDT (1972), Water Pollution Control Act (1972), Clean Water Act (1972), and the Endangered Species Act (1973). A few followed in the later part of the decade, but not at the concentrated level of the first three years.

Perhaps the first cracks of the movement were seen on Earth Day. In a speech on Earth Day in Philadelphia, frequent Nixon critic and potential Democratic presidential hopeful Senator Edmund Muskie said, "We are spending 20 times as much on Vietnam as we are to fight water pollution, and twice as much on supersonic transport as we are to

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[113] Bosso, *Environment, Inc.: From Grassroots to Beltway*, 54, Mitchell, Mertig, and Dunlap, "Twenty Years of Environmental Mobilization: Trends among National Environmental Organizations," 13, 18.
[114] Hill, "Earth Day Goals Backed by Hickel," 36.

fight air pollution."[115] The criticism of the supersonic transport was a legitimate critique, but the mention of Vietnam ran against the principle and purpose of the day. The most ominous sign of the change to come was an article just under the picture of the Earth Day celebration on April 23. The title of article was "Cambodia to Get Rifles from U.S." One week later in a televised address, President Nixon announced the American invasion of Cambodia. The public reaction to the invasion of Cambodia changed the environmental movement. Connecting such a wide range of the political spectrum became almost untenable. The lasting legacy of the movement, however, was in the laws that were passed, particularly NEPA, and the creation of organizations to support future legislation and use legal action and the courts to attempt to guarantee basic environmental standards.

The late 1960s through the early 1970s saw the rise of molecular biology and the environmental movement. And, though not dependant on each other, the two proved mutually beneficial to each other. The molecularization of biology provided biologists with new experimental methods to understand the inner workings of human genetics. Some of the biological research during this time provided the scientific legitimacy and evidence to the environmental movement. The legitimacy and evidence translated into boosts in members in environmental organizations as well as a popular momentum to pass federal environmental laws and regulations. The environmental movement, in turn, allowed molecular biologists, like Joshua Lederberg, a relatively non-controversial issue for public expression of social responsibility. What the two also shared was that they both worked within traditional organizational boundaries. The molecularization of biology did

[115] _____, "Activity Ranges from Oratory to Legislation."

not radically overturn the structure or nature of scientific inquiry of modern biology. The organizers environmental movement sought a broad popular bi-partisan constituency in order to pass environmental legislation and regulations, and in doing so intentionally distanced themselves from the radical movements of the late 1960s and early 1970s.

Chapter 3: "Why are we scientists?"

On a cold and clear late November Boston day in 1969, a group of three Harvard scientists, Jonathan Beckwith, James Shapiro, and Larry Eron, held a press conference just days after the publication of their ground breaking work in molecular biology – the first ever isolation of a gene from a chromosome in the prestigious journal *Nature*.[116] At the press conference they explained how they isolated the gene, but that was not the sole reason they held the press conference. Their real reason was to announce that their experiment should not be hailed solely as a scientific breakthrough, but rather it should also be considered a cause for concern. Citing the rapid progress of manned space flight as an example, the three argued that with the rapid advances in molecular biology that genetic engineering was a real possibility within a decade. They said, "We have to let people know that these things could be very, very dangerous – much more dangerous even than their worst imaginings – and that the problem is a political one. What uses are made of science are basically up to people to decide, not to scientists."[117] Despite the speculation of potential benefits of their work in one day curing genetic diseases such as Down's Syndrome or hemophilia, the press conference focused on the potential misuses of their technique, most notably, because of the government's active use of chemical weapons in Vietnam, the trio predicted its potential use in biological warfare as the next escalation in warfare. "The work we have done may have bad consequences over which we have no control. The use by the government or future governments is what frightens

[116] Jon Beckwith et al., "Isolation of Pure *Lac* Operon DNA," *Nature* 224 (1969).
[117] Herbert Black, "'Evil' Use Feared for Heredity Find," *The Boston Globe*, 24 November 1969, 8.

us." Whereas in previous years, Beckwith, Shapiro, and Eron's work may have sparked an internal debate within the field of microbiology, this press conference intentionally made the debate public. The press conference was a ripple on the seemingly placid pool of science. It altered the dynamics of the debate for the next great advancement in molecular biology only a few years away: recombinant DNA.

The Birth of Radical Science

There is no date of any formal founding of the radical scientists movement. It sprang up as an idea within the physics community in the late 1960s and quickly grew outwards into other scientific disciplines, notably molecular biology, at the same time the protest to the Vietnam War intensified. The vast majority of the radical scientists were younger faculty, newly minted doctorates, and graduate students who viewed science as insular and whose resulting technologies were either funded or exploited by corporations or the government, notably the Pentagon, with little to no consideration for or input from the public. In early 1969, one graduate student defined what it was to be a radical scientist as "seeing yourself not as a scientist but as radical scientists and a radical organizer. It means that your commitment is first to other people; your commitment is first to certain interests in the society and against other interests of society."[118] He characterized it this way:

[118] Eric Mann in Jonathan Allen, *March 4: Scientists, Students, and Society* (Cambridge, MA: MIT Press, 1970), 75-76.

> A group of people, some of whom are teachers, some of who are scientists, some of whom are lab technicians, some of whom are social workers, all go into those jobs understanding that every single institution that they're going into hurts other people. They work together by helping each other financially. They work together by being part of a political movement in a city. They do the best they can. They speak openly and honestly to as many fellow employees as they can, and they take the consequences. That seems to me a political model that threatens the government. The idea of working from within is precisely the argument that they hope you'll accept.[119]

Radical Science's priorities were the increased power of the military in society, and the exploitative nature of capitalism, notably among the poor, minorities, and women. In early 1969, the focus on the military was not especially surprising since in the prior two years open debates on university campuses broke out over the Pentagon's funding of university labs, the Cold War arms race heated up again over Anti Ballistic Missile (ABM) systems, multiple independently targeted reentry vehicles (MIRV), China's detonation of its first hydrogen bomb, and, most important, the increasingly unpopularity of the Vietnam war. The change in government funding of research and development between 1947 and 1969 was staggering. According to a study by sociologists Kelly Moore and Nicole Hala, in constant dollars the federal R&D budget grew from $6,190,000 in 1947 to $156,400,000 in 1969.[120] The vast majority of that R&D was carried out in college and university laboratories.

Three events transpired during 1969 in Cambridge, MA, New York City, and Boston that upset the traditional scientific establishment. The first of which grabbed

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[119] Ibid., 76-77.
[120] Kelly Moore and Nicole Hala, "Organizing Identity: The Creation of *Science for the People*," in *Social Structure and Organizations Revisited*, ed. Michael Lounsbury and Marc J. Ventresca (Boston, MA: JAI, 2002), 321.

national headlines. The final two, which merited little attention in science journals, created the first formal forum for radical scientists to freely express their opinions.

March 4

The first large scale organized expression of radical scientists occurred on March 4, 1969 at the Massachusetts Institute of Technology, where a one day voluntary work stoppage was substituted by panel discussions, lectures, and events open to the public. Planning for the day of work stoppage and activities originated from a discussion among some graduate students following the 1968 elections about what would hypothetically happen if scientists refused to work on military related research until the Vietnam war ended. The graduate students soon put forward the idea of a one-day work stoppage and a concerted effort between the students and faculty resulted in the March 4 event. When word spread during the planning phases, other campuses around the country held similar events.[121] Political activity on the MIT campus was not new as there faculty were actively involved in organizations supporting the Johnson-Humphrey ticket in 1964 and Senator Eugene McCarthy in the 1968 Democratic presidential primary, and large scale protests of the Vietnam war and on campus recruiting by Dow Chemical Company, supplier of napalm, began in 1967. [122]

The statement from the faculty planning March 4 began: “Misuse of scientific and technical knowledge presents a major threat to the existence of mankind. Through its

[121] Cornell and Yale Universities each held their own March 4 activities, and organizers claimed that 20 to 25 other colleges and universities also participated, including Bell Telephone Laboratories, which sent a busload of participants to MIT. "Science and Defense," *Science News* 95 (1969): 185.
[122] Allen, *March 4: Scientists, Students, and Society*, viii-xi, xxi-xxiii.

actions in Vietnam our government has shaken our confidence in its ability to make wise and humane decisions. There is also disquieting evidence of an intention to enlarge further our immense destructive capacity. The response of the scientific community to these developments has been hopelessly fragmented." The faculty called on scientists to "unite for concerted action and leadership: Action against dangers already unleashed and leadership toward a more responsible exploitation of scientific knowledge." The statement outlined a five-point plan including the examination of science policy, demilitarizing research and reorienting toward environmental and social problems, teaching students about research beneficial for society, opposing the arms race, and organizing scientists and engineers for effective political action.[123]

The March 4 panel discussions, which spanned three days, included speakers representing a broad spectrum of opinions from scientists, graduate students, policy makers, intellectuals, commercial interests, and the New Left. The focus of most of the panels was on the responsibilities of intellectuals, the role of military funding on campuses, the use of science for the benefit of society, and the arms race and disarmament. Concern among biologists at the conference, including Nobel laureate George Wald, focused almost exclusively on the Vietnam War, the intertwining of the science organizations with the military, and the arms race with the Soviet Union.

In his lecture, "A Generation in Search of a Future," Wald, focused on issues of concern to his students like the Vietnam war, the arms race, and the possibility of full-scale nuclear war. He did, however, wander away from issues directly affecting his students to ones more personal to him as a biologist. He excoriated scientific societies for

[123] "Union of Concerned Scientists Faculty Statement," in *March 4: Scientists, Students, and Society*, ed. Jonathan Allen (Cambridge, MA: MIT Press, 1970), xxii-xxiii.

establishing Washington, DC offices so that they could lobby "like everyone else," and "look for things to do, for more influence, for more money, and there's that Department of Defense money to burn." Wald directly called attention to the American Institute of Biological Sciences for holding a scientific symposium sponsored by Fort Detrick, which housed the U.S. Army Biological Warfare Laboratories between 1943-1956, entitled "Leaf Abscission" and not the more accurate term for Wald: "Defoliation." The "worst offender" for Wald, however, was the outgoing president of the National Academy of Sciences who was simultaneously the chairman of the Scientific Advisory Board of the Department of Defense before his resignation.[124]

Although no final conference statement was issued, March 4 demonstrated an active push to engage scientists, graduate students, and the public in discourse concerning the uses of technology. The fear and anxiety over the use of some technologies for military use, especially at MIT, demonstrated the need for scientists to be more socially conscious of who funded their research and the research's future applications. The creation of the non-profit science advocacy organization, the Union of Concerned Scientists (UCS), which still exists today, was the lasting legacy of the March 4 activities.

Towards a Science for the People

A month prior to March 4, the first formal organization created as a voice for radical scientists held its inaugural meeting during the American Physical Society's (APS) annual meeting at the New York Hilton Hotel. Created by four young theoretical physicists, the initial name of the group was Scientists for Social and Political Action

[124] George Wald, "A Generation in Search of a Future," in *March 4: Scientists, Students, and Society*, ed. Jonathan Allen (Cambridge, MA: MIT Press, 1970), 110-11.

(SSPA).[125] The first meeting of SSPA was due in large measure to the efforts of theoretical physicist Charles Schwartz (University of California, Berkeley) at the 1968 APS annual meeting. At that meeting Schwartz argued that the APS should take a stand against the Vietnam War, in particular he was concerned about the military funding research for the war at college and universities. He also urged the APS members to support an amendment to the organization's bylaws to permit members to have more direct input into the organization by permitting them to propose amendments.[126]

In the January edition of *Physics Today*, the official journal of the APS, Schwartz wrote the lead letter in the expanded "Letters" section. Schwartz argued that the "major question of society policy" was "the appropriateness of discussing public issues." He saw his amendment as moving the society away from the "absurdity of complete political innocence." Schwartz's distilled the current situation as "'We are concerned only with physics as physics.'" While acknowledging that "certainly one of the easiest ways to destroy the integrity of the society would be to turn it into a debating club open to every political issue of the day," he claimed his amendment was "carefully designed to protect against such excesses," while allowing for majority opinions of the members to emerge. To persuade readers for his amendment, Schwartz argued that the Vietnam War was "an external crisis of such magnitude that we fear a general catastrophe of a political, military, or cultural nature," and, as physicists, who have "a unique competence in this matter," they have "responsibility" to address social issues. He concluded by warning his

[125] On the initial leaflet the name of the organization was Scientists dedicated to vigorous Social and Political Action. The name in every other publication after the meeting is Scientist for Social and Political Action. Charles Schwartz, "Origin of SESPA," http://socrates.berkeley.edu/~schwartz/SftP/SSPA1.html, "Science and Defense," 185.
[126] Moore and Hala, "Organizing Identity: The Creation of *Science for the People*," 325-26.

colleagues that committing the society to making public statements was not without problems, but ignoring external realities then they will "continue to be judged according to the dictum, 'silence implies consent.'"[127]

Schwartz's proposal at the 1968 APS conference and his letter in *Physics Today*, touched off of debate within the society about the role of APS and its members in addressing social concerns. Ballots were sent out in May for members to vote on Schwartz's proposal. The ballots were counted that summer, and the Schwartz amendment failed. In the August *Physics Today*, the APS tried to put the matter to rest in with an editorial.

While the society did not "take a stand" against the Schwartz amendment, it did believe that *Physics Today* spoke "mainly *to* the physicists and not *for* them and only incidentally *from* them." Further, the editorial tried to dissuade physicists from taking "advantage" of the 'halo effect'" to make any public statements on social problems. (The "halo effect" was when a physicist gained an unusual amount of public attention for a discovery or breakthrough.) For the editors of *Physics Today*, there was nothing wrong with abandoning a career in physics to effect social change or to get involved as an individual citizen in social or political causes. The conservative approach of the organization and journal in addressing issues outside of its scientific purview, however, reigned supreme. There was no other way for Schwartz and his supporters to read the

[127] Charles Schwartz, "Should the APS Discuss Public Issues? For the Schwartz Amendment," *Physics Today* 21, no. 1 (1968).

editors conclusions that “we do not address ourselves to the outsiders” and “modesty is a useful virtue when it preserved the effectiveness of the modest man” any other way.[128]

Although the proposed Schwartz amendment touched off a year of letter writing to the editors of *Physics Today*, it did not change the fact that the amendment was dead. For his part, Schwartz spent a year working within the structure of a major professional scientific organization to enact change to make it more socially responsible. When that did not work, he formed SPPA as an organization that would work outside of the typical structure of a professional scientific organization. The fist SPPA meeting at the 1969 APS annual meeting attracted three hundred people, including many scientists for the APS meeting. The organizers lured participants by handing out leaflets, gathering signatures on a petition, and distributing buttons in the corridors of the hotel during the meeting. The petition sought to create a new division within APS focusing on social and political questions. The leaflet, however, spelled out the goals of the new organization.[129]

The leaflet opened with a statement about the complacency the scientific community in the face of a changing society and their dependence on the “government for research funds and for their very livelihood” making them “more and more cautious” in speaking out on public issues. Furthermore they called for scientists to “reject the old credo that ‘research means progress and progress is good.’” SSPA was an “antidote” because it provided an arena for socially responsible scientists to explore questions such as “Why are we scientists? For whose benefit do we work? What is the full measure of

[128] R. Hobart Ellis, Jr., "Editorial: What We Are Not Against," *Physics Today* 21, no. 8 (1968). Emphasis in original.
[129] "Science and Defense," 185, Moore and Hala, "Organizing Identity: The Creation of *Science for the People*," 325-27.

our moral and social responsibilities?" The statement closed with the goals of the organization:

> …We shall seek new and radical solutions for long range problems and immediate issues, and we shall press for effective political action. We shall work for change within our present affiliations (professional society, university, laboratory), but foremost we shall strive to present our opinions as an independent body of socially aware scientists free from the inhibitions which abound in the established institutions we now serve. We shall also seek to relate our activities to those of similar groups (radical causes) now forming in other professions.

Open to scientists, engineers, and students in related fields, SSPA was able to sign up 100 members following the first meeting.[130]

Members of SSPA like March 4, and later UCS, organized around their objection to the intentional and indirect acceptance of Department of Defense money for their research. Where the two differed, however, was in organizational structure. March 4 had multiple committees and an organizational hierarchy, which enabled it to easily form a sustainable organization. SSPA, however, was conceived as "an independent and loosely organized assembly… [Consisting] of local groups formed upon the initiative of their members; the national framework will act to coordinate projects begun at the local level rather than fix policy." This structure resulted in greater leadership and autonomy for individual chapters to initiate and carry out projects that reflected the desires of its membership. The only real means of national communication for SSPA was its newsletter.[131]

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[130] Schwartz, "Origin of SESPA.", "Science and Defense," 185.
[131] "Newsletter No. 1 [23 February 1969]," Scientists for Social and Political Action, http://socrates.berkeley.edu/~schwrtz/SftP/SSPA2.html.

The first newsletter, published on February 23, 1969, advanced the three major problems confronting SSPA: The first was the "overwhelming" domination of scientific research for military projects when current levels of weapons "already constitute the greatest threat to world peace and security. The AMB program must be stopped."; the second was the destructive nature of technological advances on nature and man; and the third was the inability to express scientific or moral opinions because scientific leadership had "fallen to a pocketful of government advisors who…have been absorbed into the Washington establishment."[132] Toward the end of the year SSPA once again changed its name to reflect the inclusive nature of the organization, which now included engineers and technical workers on its membership rolls. The new name for the organization was Scientists and Engineers for Social and Political Action (SESPA).

SESPA and the AAAS

During the last few days of 1969, the American Association for the Advancement of Scientists held its first-ever student panel during its annual meeting in Boston. Titled "The Sorry State of Science – A Student Critique," the papers focused on the Vietnam War, the American technocratic society, and the role the scientific community played in perpetuating these conditions. Organized by Allen Weinrub, a Harvard Applied Science graduate student, topics included: "Who Benefits from Technological Change?" "Secret Labs and Special Interests," and "Technology: Progress is Not its Most Important Product."[133]

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[132] Ibid. Emphasis in original.
[133] Matt Wisnioski, "Inside "The System": Engineers, Scientists, and the Boundaries of Social Protest in the Long 1960s," *History and Technology* 19, no. 4 (2003): 324-25,

Led by Weinrub, approximately 200 students and scientists, consisting of Boston chapter of SESPA and other activists, under the slogan of "Science for the People,"[134] handed out leaflets at the AAAS meeting on topics such as gender equality, "conversion," and criticism of panels exemplifying the scientific establishment. The group also convinced the organizers to open up the remainder of the proceedings to the public, which resulted in media coverage protests.[135]

In the early years, SESPA chapters participated in anti-war and anti-ABM marches. The Berkeley chapter circulated a pledge at various scientific meetings stating: "I will not participate in war research on weapons production. I further pledge to counsel my students and urge my colleagues to do the same." The most public function of SESPA, however, was disturbing the annual meetings of AAAS, which, as the largest and most diverse gathering of scientists, represented "a bastion of establishment ideology that seeks technological solutions to social and political problems," as well as the largest arena in which to make its voice heard and to attract new members.[136] The 1969 disruptions were only a mild sign to what awaited the AAAS at its annual meeting in late December 1970.

"The Sorry State of Science: AAAS Symposium, 28-29 December, Boston," *Science* 166, no. 3911 (1969).

[134] There are differing accounts as to when and where the slogan, and eventual organizational name, "Science for the People" originated. Wisnioski places the origin of the name at the 1969 AAAS annual meeting, while the article "Science for the People – A Ten Year Retrospective" places the name around the same period when other chapters were forming around the country. The article, however, does not place the origin at the Boston meeting. Kathy and Sue Tafler Greeley, "Science for the People: A Ten Year Retrospective," *Science for the People* 11, no. 1 (1979).

[135] Wisnioski, "Inside "The System": Engineers, Scientists, and the Boundaries of Social Protest in the Long 1960s," 325.

[136] Greeley, "Science for the People: A Ten Year Retrospective," 19.

At the 1970 meeting in Chicago, SESPA members continued to make use of the press coverage in an attempt to further its goals. The conference was open to the press, who ran four stories – three disruptions of speakers and one report – that garnered front-page headlines. The goals for SESPA at the 1970 meeting were two-fold with an overtly anti-elitist and communist tinge. The first was to point out to those in attendance at the convention that "scientific activity does not occur in a political vacuum. The use to which their work is put is inevitably a political decision, and the priorities are created by capitalist economy." The second was its desire for clarification on the definition of "science for the people" as scientists who want to work in science liberated from "capitalist control."[137]

The first event to grab headlines was a theatric interruption of an address by Edward Teller, which resulted in front-page coverage in *The New York Times*. Teller, an instrumental member of the Manhattan Project, associate director of the Lawrence Radiation Laboratory at Livermore, CA, and dubbed the "father of the hydrogen bomb" was at the conference to deliver an address on the abolishment of secrecy in scientific research at a panel on the scientific generational gap. Prior to his talk, Robert Lewontin, a biologist at the University of Chicago, called Teller a "flunk of power" for his work on the atom and hydrogen bombs.[138] According to *The New York Times,* just as Teller began his speech from the podium in front of the packed room dotted by SESPA posters – including its new icon: a fist grasping an Erlenmeyer flask in front of the iconic clenched left fist – protestors rose to their feet and held aloft signs reading "War Criminal." Teller

[137] John Noble Wilford, "Knitting-Needle Thrust Interrupts One Dissident," *The New York Times*, 30 December 1970.
[138] "Teller Says Oppenheimer Talked Him out of Opposing A-Bomb," *The Washington Post*, 28 December 1970.

stated that he would not continue with the signs displayed.[139] When Margaret Mead, the chairman of the session, finally restored order, Teller completed his speech and gave way to the next speaker, Richard Novick of the Public Health Research Institute of New York City. Novick implored the audience to reorganize science "along classical union lines."[140] After Novick spoke, he turned and presented Teller with SESPA's "Dr. Strangelove Award." The award was a wood and chrome solider which was inscribed with "I was just following orders."[141] Refusing to accept the award, Teller leaned over to Novick and said, "I didn't expect this from you. You should be ashamed at yourself."[142] When asked about the award after the session ended, Teller deadpanned that it was "the first award I have ever refused, although many of the others have also been undeserved."[143]

The following day the disruptions continued throughout the morning and afternoon once again bringing the media's spotlight to the conference. In a morning

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[139] The clenched fist, also known as the raised fist or closed fist, was a symbol used by social and political organizations with a leftist, anti-capitalist, and anti-establishment ideology. The clenched fist is generally regarded as a symbol of strength and unity. In the United States during the 1960s, it was used by the Students for a Democratic Society, the Black Panthers, and leftist organizations. It gained national and international notoriety in 1968 when Tommie Smith and John Carlos gave the clenched fist salute wearing black gloves on the medal podium at the Olympic Games in Mexico City. After winning the gold and bronze medals respectively, the two stood on the podium with their fist raised wearing black socks and no shoes in protest of the inequalities between the black and white communities in the US. For their political protest at the apolitical Olympics, the two were quickly suspended from the US Olympic team and banned from the Olympic village at the games.

[140] John Noble Wilford, "Teller Deplores Secret Research," *The New York Times*, 28 December 1970, 25.

[141] Wisnioski, "Inside "The System": Engineers, Scientists, and the Boundaries of Social Protest in the Long 1960s," 325. "AAAS: Conflict, Confrontation, Consideration," *Science News* 99 (1971): 21, ———, "Inside "The System": Engineers, Scientists, and the Boundaries of Social Protest in the Long 1960s."

[142] Wilford, "Teller Deplores Secret Research," 25.

[143] "Teller Says Oppenheimer Talked Him out of Opposing A-Bomb."

session panel on violence and the police, a few SESPA members rushed the podium, deposed the chair of the session, Joseph Coates of the National Science Foundation, called him "one of the super criminals in this nation," and asked for him to explain his prior work for the Institute for Defense Analysis. Coates explained that he would only deviate from the schedule if they audience agreed, and he would give the protestors the floor from the last ten minutes of the session "in the spirit of Chairman Mao we are going to let one of our thousand flowers flourish in our way."[144] The disturbances continued through the next presenter. Mrs. Garrett Hardin, wife of a biology professor, fed up with what she saw and heard stabbed one of the protestors in the arm with a knitting needle while another audience member was threatening to hit the same protestor with her shoes. The irony of a stabbing occurring during a session whose theme was violence was no doubt lost on Mrs. Hardin, who said of the incident "I don't have as loud a voice as he has…I just had to use the only weapon I had available."[145] No vote was taken to cede any remaining time to Coates or the protestors, but they did agree to answer questions with the panelists after the final two speakers were permitted to finish their talks.[146] One afternoon session was completely over taken by SESPA. It disrupted speakers, showed movies, and passed around a hat for donations for the Black Panther Health Clinic and Legal Defense Fund.[147] During another afternoon session, however, the protest went from a simmer to a full boil.

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[144] Stuart Auerbach, "Irate Knitter Needles Heckler," *The Washington Post*, 30 December 1970, A6, Wilford, "Knitting-Needle Thrust Interrupts One Dissident."
[145] Auerbach, "Irate Knitter Needles Heckler."
[146] Stuart Auerbach, "Young Radicals Drive Seaborg Off Platform," *The Washington Post*, 31 December 1970.; "A.E.C. Head Walks out as Dissidents Crowd In," *The New York Times*, 31 December 1970.
[147] Auerbach, "Irate Knitter Needles Heckler."

The second afternoon session included a much-anticipated talk by Glenn T. Seaborg, the president-elect of AAAS, chairman of the Atomic Energy Commission, and chemist who discovered many of the heavy elements used for nuclear enrichment. The packed room included scientists, the press, and a contingent of around forty SESPA members. The first two speakers – Rep. Charles Mosher (R-OH) and Homer Newell, associate administer of the National Aeronautics and Space Administration – gave their talks without protest. The third speaker, Robert Marston, director of the NIH, just completed his speech when the first interruption occurred as a member of SESPA in the third row began shouting questions. At that moment Seaborg stood up from his seat in the front row and walked out the side door of the room, instantly enraging the SESPA members in attendance. They immediately rushed the podium with bullhorns as hotel security rushed to turn off the microphones. Undeterred, the group composed of mostly graduate students and young university scientists "in their late twenties, but a few of their leaders…were older," took their positions in front of the room where Dr. Herbert Fox, a 40 year-old physicist from Boston, began reading the "indictment" against Dr. Seaborg over the din of the confused audience and press.[148] The "indictment against Glenn T. Seaborg for the crime of science against the people"[149] accused Seaborg of a "conscious, major, self-serving and ruthless role in establishing, organizing, maintaining and developing institutions of science and government for effective use by the ruling class."[150] Continuing the theme of the scientific elite the indictment expanded and targeted the AAAS directly because its "undemocratic structure immunizes it against the

[148] "A.E.C. Head Walks out as Dissidents Crowd In."
[149] Ibid.
[150] ———, "Young Radicals Drive Seaborg Off Platform."

criticism of the disenfranchised scientific worker membership."[151] While Fox delivered the indictment members of SESPA held signs that read, "We are a death-oriented culture" and "Reverse priorities – all life and power to the people."[152]

Chemical and Biological Weapons

The final major headline from the 1970 conference was the issuing of the AAAS Herbicide Assessment Commission. The commission, headed by respected Harvard University molecular biologist Matthew Meselson, was founded in 1966 to examine effects of defoliants, and concluded three years later that the spraying was "ineffective." The report was unequivocal in some of its findings, which included: The Army knowingly sprayed populated areas; crop spraying was destructive to the population, but had little to no affect on the North Vietnamese Army; and that at least one-fifth of the 1.2 million acres of mangrove forests were destroyed. The commission was unable to definitively determine the effects on the food chain or on birth defects and it recommended further study in both these areas. The White House made no announcement in the days following the release of the report. Meselson's near decade long work with the Arms Control and Disarmament Agency, however, paid dividends two months earlier when it helped convince President Nixon to issue an official statement on chemical and biological weapons. [153]

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[151] "A.E.C. Head Walks out as Dissidents Crowd In."
[152] Ibid.
[153] Walter Sullivan, "Spray in Vietnam Said to Level Fifth of Mangrove Area," *The New York Times*, 30 December 1970, Victor Cohn, "Vietnam Defoliation Called Failure," *The Washington Post*, 31 December 1970.; ———, "Army Knew of Defoliation Failure in '67 but Crop Spraying Continued," *The Washington Post*, 31 December 1970. David E.

On November 25, 1969, President Nixon pledged that the US would dramatically limit the research, manufacture, and use of chemical and biological weapons. The statement renounced the use of offensive chemical weapons and submitted the Geneva Protocol of 1925 prohibiting the use of incapacitating chemicals. The statement called for the complete destruction of the current biological weapon stockpiles and a halt to current research into both offensive and defensive biological weapons. Further, Nixon associated the United States with a United Kingdom draft convention to ban the use of all biological weapons. The statement also separated chemical and biological weapons, so that the government could regulate the two as separate entities domestically and in international treaties.[154]

Nixon's announcement made national headlines and was widely lauded as a significant step in arms reduction and possibly towards peace. (The press did not miss the near contradiction between the official statement and continued heavy use of chemical defoliants in Vietnam by the US military.) The biological community praised Nixon's decision. Meselson called it "one of the better examples of government-wide decision making." Joshua Lederberg, Nobel laureate and Stanford Professor of genetics, was "quite elated," and Paul Doty, Harvard Professor of biochemistry, thought it was "the

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Rosenbaum, "Activist Germ War Foe: Matthew Stanley Meselson," *The New York Times*, 26 November 1969.

[154] James M. Naughton, "Nixon Renounces Germ Weapons, Orders Destruction of Stocks; Restricts Use of Chemical Arms," *The New York Times*, 26 November 1969, "Text of Nixon Statement on Chemical and Biological War, of the Geneva Protocol and of British Plan," *The New York Times*, 26 November 1969, "Nixon Reported Set to Ban Gases," *The New York Times* 1969.

most anyone could hope for, given the attitude of the Joint Chiefs of Staff." On April 10, the UK draft convention on banning biological weapons was realized. [155]

On that day, over seventy nations, including the US, the USSR, and the UK, signed the *Convention on the Prohibition of the Development, Production and Stockpiling of Bacteriological (Biological) and Toxic Weapons and Their Destruction* (BWTC). The convention was the first multi-lateral disarmament agreement since the Second World War to provide for the elimination of an entire category of weapons. The convention required the signatory state to "never in any circumstance to develop, stockpile or otherwise acquire or retain" biological agents or toxins in quantities for "protective or other peaceful purposes" or as "weapons."[156] The convention also set guidelines for limiting the spread of biological agents and toxins. The BWTC differed significantly from chemical weapon treaties because it did not allow for research or development of non-offensive weapons. With its signing, Nixon was once again hailed on front-page headlines around the nation for limiting the scope of the arms race and setting the stage for future strategic arms talks. [157] Despite the 1969 official statement, the signing of the BWTC, and the work of Meselson and other scientists, the distrust of the

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[155] Naughton, "Nixon Renounces Germ Weapons, Orders Destruction of Stocks; Restricts Use of Chemical Arms." Quotes of scientists from "Ban on Germ Weapons Lauded by Scientists," *Washington Post*, 26 November 1969.

[156]US Department of State, "Convention on the Prohibition of the Development, Production and Stockpiling of Bacteriological (Biological) and Toxin Weapons and on Their Destruction," (10 April 1972). The treaty was jointly signed in Washington, DC, London, and Moscow on April 10, 1972. The US Senate ratified it on December 16, 1974; President Gerald Ford ratified it on January 22, 1975. The US ratification was deposited at Washington, DC, London, and Moscow on March 26, 1975, proclaimed by President Ford on March 26, 1975, and entered into force on the same day.

[157] Hedrick Smith, "70 States Reject Biological Arms," *The New York Times*, 11 April 1972.

federal government to use new technologies for military purposes, including recombinant DNA was always present even among non-radical scientists.

Contradictions and Change

Perhaps the contradictions of protesting the 1970 AAAS annual meeting were lost on SESPA. AAAS was an association that did not live up to the elite label placed on it by SESPA. The association had no selection process equally welcomed members from any field of science, both hard and soft. And instead of remaining a society steeped in traditions, the AAAS made structural changes in the 1960s and throughout the 1970s to create an organization that could meet and quickly more accurately reflect the societal changes in the country. AAAS designed new science education outreach programs, created new committees for young scientists, women, and minorities, and expanded its emphasis on issues central to science policy and the role of the federal government. Other initiatives included the creation of the AAAS Herbicide Assessment Commission in 1966, which authored the 1970 report on the use of defoliants in Vietnam, and the committee on Scientific Freedom and Responsibility, which authored *Scientific Freedom and Responsibility* in 1975 that reflected the new pressures faced by post-war scientists and how to find resolutions to conflicts. [158] Instead of remaining inflexible to the increasingly diverse number of scientists created in the post-war boom, AAAS tried to change to meet the needs of its membership.

[158] Sally Gregory Kohlstedt, Michael M. Sokal, and Bruce V. Lewenstein, *The Establishment of Science in America: 150 Years of the American Association for the Advancement of Science* (New Brunswick, NJ: Rutgers University Press, 1999), 5, 136-47.

Following the 1969 SESPA protests, AAAS instead of ignoring or actively removing SESPA from the 1970 meeting did the opposite. AAAS provided SESPA with a room at the convention site hotel in an effort to block their "criticism that they were not being excluded from the conference proceedings."[159] The meeting was described by reporter John Noble Wilford as one not about the "wonders and elegance of science," but instead one at which the scientists were "agonizing over the decline in public support for science."[160] The focus of Teller's address was not on any justification for his work on nuclear weapons, but on the dangers of scientific secrecy in a democratic society. He argued, "Secrecy in science should be legally abolished, so that the democratic process can be better able to work in making decision on how science is applied." Though not advocating SESPA's utopian vision of formulating science policy, Teller stressed the need for public interaction in decisions affecting science policy. Had SESPA allowed Seaborg to deliver his address they would have heard similar refrain. Seaborg, who released his prepared remarks to the press the morning before he was scheduled to speak, articulated a new role for scientists in the public sphere. He argued for scientists to address "human problems" that scientists and the public shared such as "environmental, educational, social or political" because they have "ramifications that somehow involve scientific and technological judgment or require more and better knowledge that can only be achieved through scientific investigation." He continued that the role of scientists "must be more aggressive in bringing the issues, deliberations and thinking of science to the public."

[159] Wilford, "Knitting-Needle Thrust Interrupts One Dissident."
[160] Ibid.

The radical actions by SESPA distracted the impact of the detailed AAAS Herbicide Assessment Commission report on the ineffectiveness and effects of chemical defoliation in Vietnam. It certainly banished the blistering speech by former Secretary of the Interior Stewart Udall off the front page. In his speech, which sounded sympatric in many ways to SESPA's goals, he called the National Academy of Sciences a "virtual puppet government," eviscerated the elderly scientific establishment as "political eunuchs" who remained politically detached, and deplored the "special status" given to the scientific community[161] The 1970 AAAS meeting, however, was not the last time that SESPA disrupted an annual meeting.

At the 1971 AAAS annual meeting in Philadelphia, SESPA continued to hand out fliers, hang posters, and disrupt talks. Members threw paper airplanes at Senator Hubert Humphrey and hit him with a rotten tomato. They forced Senator Daniel Moynihan to cancel his talk to avoid additional controversy. By 1972, when some SESPA members tried to set up a literature table at the meeting, the police were called and the members were arrested. [162]

The Boston Chapter

In these early years, the Boston chapter of SESPA was quickly recognized as both the most organized and most militant chapter. The chapter took responsibility for the

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[161] "Udall Deplores State of Science," *The Washington Post*, 31 December 1970.; John Noble Wilford, "Ex-Interior Chief Calls for Moral Leadership," *The New York Times*, 31 December 1970.

[162] Greeley, "Science for the People: A Ten Year Retrospective," 19, Robin Marantz Henig, "Radical Group 10 Years Later...Science for the People: Revolution's Evolution," *BioScience* 29, no. 6 (1979). 341-342, "Confusion and Confrontation," *Science News* 101 (1972).

newsletter in 1970 and immediately turned it into a bi-monthly magazine that was available to members and sold at a few local newsstands. Two key members of the Boston chapter were responsible for the dramatic change in tone of the magazine. Herb Fox and Britta Fisher were "deeply committed to class-based political organizing," and had associations with the British Society for Social Responsibility in Science, the October League, as well as notable British Marxists.[163] The magazine lost its focus for broad national scientific concerns and quickly became the organ reflecting the activist nature of the Boston chapter. It now included slogans such as "End Science mind-fuck," "Science is not neutral!" and the intentionally Maoist-sounding "Scientists are workers," and "Join with other workers to make science serve the people."[164] A review of table of contents of the first year of the magazine demonstrates the editorial shift in content Articles focused on supporting radical causes, such as the Black Panther Party, the North Vietnamese government, and other revolutionary movements, women in science, and anti-capitalistic causes. In addition to articles in the magazine, the Boston chapter began a long-term relationship with the Black Panthers beginning project called the Technical Assistance Project (TAP). The purpose of TAP was to "teach people basic technical skills, like working on automobiles, sounds systems, chemical analyses and self-defense mechanisms."[165] The Boston Chapter also collaborated with other chapters in the 1971 Science for Vietnam project that sent textbooks and technical information to North Vietnam, and the 1973 effort to send the first SESPA delegation to the People's Republic

[163] Moore and Hala, "Organizing Identity: The Creation of *Science for the People*," 328-29. Founded in 1971, the October League was a Maoist political party in the United States and predecessor to The Communist Party (Marxist-Leninist)
[164] Herb Fox, "Five Years of Science for the People: A Political Analysis," *Science for the People* 7, no. 4 (1975): 21.
[165] Greeley, "Science for the People: A Ten Year Retrospective," 20.

of China to study their "commitment to developing non-elitist science that would serve the interests of the people."[166]

The organization, central issues, and early activities of SESPA collectively showed how much influence the New Left had on the evolution of the radical science movement. The basic organization began and continued to be a "non-organization" of loose local chapters with no permanent hierarchal structures.[167] According to historian Doug Rossinow, this structure was an identifying structure of New Left organizations of the 1960s. "The New Left was a messy agglomeration of national and local groups and initiatives…Many radical initiatives did not cohere as organizations of any kind, and most that did…paid little attention to the niceties of membership. The New Left gleefully ignored organizational structure."[168] The central social issues, especially for the Boston chapter and with the constant use of the SESPA icon are best understood as the merging of "struggle against the [Vietnam] war and the struggle for black liberation" by the late 1960s.[169] Its "psychological alliance" with the communist and revolutionary forces of the Third World was also a trait of the New Left, notably SNCC and SDS, in the 1960s.[170] And finally its efforts to provoke change through "media-oriented" confrontations were also a common strategy of the New Left.[171]

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[166] Ibid., 20-21.
[167] Fox, "Five Years of Science for the People: A Political Analysis," 21.
[168] Doug Rossinow, *Letting Go: Revisiting the New Left's Demise*, ed. John and Paul Buhle McMillian, The New Left Revisited (Philadelphia, PA: Temple University Press, 2003), 241-42.
[169] Maurice Isserman and Michael Kazin, *America Divided: The Civil War of the 1960s* (New York, NY: Oxford University Press, 2000).
[170] Ibid., 183.
[171] Ibid., 188.

Beckwith the Activist

Jonathan Beckwith was one of the early members and leaders of the Boston chapter of SESPA. His response to the reaction from other biologists to the press conference he held following the publication of the first isolation of a gene in *Nature* makes it clear why he held the press conference and what he saw as the problem with science.

> What we are advocating is that scientists, together with other people should actively work for radical political change in this country. If we do not, we will one day be a group of very regretful Oppenheimers. Scientists have no right to claim a special position of intellectual leadership in this political effort…As we see it scientists are obligated to inform the public what is happening in their secluded fields of research so that people can demand control over decisions which profoundly affect their lives. If our arguments mean that "the progress of science itself may be interrupted", that is an unfortunate consequence we will have to accept. It certainly should not inhibit us from speaking out on crucial issues.[172]

Beckwith was not done with using the pulpit that his breakthrough earned him. For his discovery he was awarded the prestigious Eli Lilly Award by the American Society for Microbiology (ASM) in May 1970. In his acceptance speech, entitled "Gene Expression in Bacteria and Some Concerns About the Misuse of Science," Beckwith explained the science behind his gene isolation experiment, "spoke of how basic science as well as applied science can be used to destructive ends," and then told his audience that he was not keeping his award money, but rather donating it to a worthy cause.[173]

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[172] Jon Beckwith, Larry Eron, and Jim Shapiro, "Correspondence: "More Alarums and Excursions"," *Nature* 224 (1969).
[173] Jonathan R. Beckwith, *Making Genes, Making Waves: A Social Activist in Science* (Cambridge, MA: Harvard University Press, 2002), 62.

The worthy cause Beckwith told his suddenly shocked audience was the Black Panthers – half to the Boston Panther Free Health Movement and the other half to the Defense fund for Panther 21 in New York – because "in a just society, those who received the awards should be those who are contributing in a meaningful way to the welfare of all people."[174] For Beckwith, the donation was both personal and an extension of the tradition begun with his press conference.[175] When reading an article detailing the arrest of twenty-one Black Panthers in New York City on April 1, 1969, he recognized one of the members, Curtiss Powell, a biochemist that he met during a year spent working at the Pasteur Institute in the early 1960s. The Panther 21, later the Panther 13, were charged with 12 crimes, including conspiracy to murder police officers, arson, possession of weapons and explosives, and plotting to bomb department stores in New York City, or what District Attorney Frank Hogan boiled down to as a plot to destroy part of "the power structure."[176] Following the arrest of the Panther 21, Beckwith became more engaged with the Black Panthers in the Boston area notably through nutrition and other health outreach programs.[177]

His sermon to the unsuspecting ASM congregation continued as he ran through a short list of the misuses of science including the nuclear bomb, the Vietnam War, and

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[174] Jon Beckwith, "Gene Expression in Bacteria and Some Concerns About the Misuse of Science," *Bacteriological Reviews* 34, no. 3 (1970): 225.
[175] Beckwith, *Making Genes, Making Waves: A Social Activist in Science*, 60.
[176] Morris Kaplan, "Bomb Plot Is Laid to 21 Panthers," *The New York Times*, 3 April 1969, 1, 36.
[177] Beckwith, *Making Genes, Making Waves: A Social Activist in Science*, 46-47, 60-63. Beckwith reconnected with Powell shortly after his ASM speech, and after the charges against the Panther 13 were dismissed two years later Beckwith helped Powell secure a job as a biochemist. The accusations of a federal government conspiracy against black and other radical/militant groups during this time period were proven true years later when the operations of the FBI's Counter Intelligence Program (COINTELPRO) became public.

then he focused directly on drug companies, including the sponsor of the award. The drug companies were a mixture of capitalism, public relations, and science that resulted in over prescription, over use, and "constant pressure on doctors" for the benefit of only the drug companies.[178] Beckwith concluded his speech with a list of suggestions for how scientists could change policy because, as he stated, "I am not going to offer a blueprint for change because I believe that I and all of us have a lot to learn about the world going on around us. I think the form of that change will emerge as we continue what I believe is a long struggle." His four suggestions were: to organize scientists to recognize their responsibilities; to not do research that directly or indirectly helps to make war; to contribute knowledge toward groups that are helping working and poor people; and finally, to operate within their own institutions and organizations to help working people and the surrounding community.[179]

Before the year was over, Beckwith attended an international conference devoted to the social responsibility of biologists in an age of rapid advances in the field. The British Society for Social Responsibility in Science organized the conference, which was composed presentations and discussions by scientists, including Nobel laureates, such as James Watson, that was open to the public and the press. The majority of the presentations and discussions inevitably focused on controls on scientific research and how the public should be involved in the decision making process. For Beckwith the conference offered him the opportunity to present his ideas on the role a radical scientist should play within the scientific community, as well as warning about the potential for

[178] Ibid., 62.; ———, "Gene Expression in Bacteria and Some Concerns About the Misuse of Science," 226.
[179] Beckwith, "Gene Expression in Bacteria and Some Concerns About the Misuse of Science," 226-27.

misuse of science and the greater "likelihood of greater ties of biologists with drug companies who are rapidly beginning to see the potential of research in cancer, molecular genetics, immunology, etc." again. Repeating his disdain for elite science, Beckwith concluded his talk by attacking the very idea of the conference as reflecting the "elitist concept" of control over science. He ended his talk with the hope that "in the future, meetings of this sort will bring together more than just the elite of a particular scientific field."[180]

For Beckwith and the other radical biologists, the early 1970s proved to be a fertile ground as the discoveries as molecular biology brought into question the role of scientists, the government, and the public in deciding the future course for science policy in the United States. Their methodology of aggressive public confrontation was a novel approach to seeking change within the field, though because of their organizational structure it is difficult to tell the rates of growth of groups (nationally and locally) like SESPA. The goal of the group was to bring science to the people, and in that regard they were successful in pushing the envelope in attracting press attention to its activities. In the following years the Boston chapter of SESPA would be instrumental in forcing the recombinant DNA debates out of the scientific community and into a broader public debate by using similar methods it employed in its first years of existence.

[180] Jon Beckwith, "The Scientist in Opposition in the United States," in *The Biological Revolution: Social Good or Social Evil?,* ed. Watson Fuller (Garden City, NY: Doubleday, 1971), 302-03.

Chapter 4: "What do you think a scalpel?"

On a warm and partly sunny Sunday morning in mid-July 1970, regular readers of *The Washington Post* were skimming through the above-the-fold front page articles on congressional spending, the recap of the visit by England's twenty-one-year-old Prince Charles, and the near-daily Vietnam War update. And perhaps readers may have moved onto the Outlook section where they might have come across one of Joshua Lederberg's last science columns, tucked neatly under a larger piece entitled "W.H. Auden: The Poet as Punctual Man." The title of Lederberg's column, "Government Is Most Dangerous of Genetic Engineers," was more than a little jarring when juxtaposed to an homage to the influential poet.[181]

Lederberg used his column to expand upon and clarify the published reports of his *New York Times* headline-making testimony in June before the Senate Subcommittee on Government Research hearing entitled "Departments of Labor and Health, Education, and Welfare Appropriations for 1971."[182] At this closed hearing, Lederberg provided testimony at the request of the National Cystic Fibrosis Foundation, and he made it to the front page of many national newspapers by requesting ten million dollars for the 1971 fiscal year for the study of genetic diseases and the creation of a National Task Force on Genetics, which would include a panel coordinated with the National Institutes of Health

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[181] Joshua Lederberg, "Government Is Most Dangerous of Genetic Engineers," *The Washington Post*, July 19 1970, 39. Lederberg might not have had control over the title of his column because newspaper editors (and not the authors themselves) usually write the titles.

[182] US Congress. House. Subcommittee of the Committee on Appropriations, *Departments of Labor and Health, Education, and Welfare Appropriations for 1971*, 91st Cong., 2nd sess., 1970.

and which would serve as a clearing house for on-going genetics research as well as help direct funding more efficiently to future research in the United States.[183] At the hearing, Lederberg acknowledged the already decreasing budgets for scientific research, but stressed to the legislators that investment in genetic research was needed because of the "skyrocketing medical costs" of patients suffering wholly from genetic diseases, which he estimated to be at least 25 percent of all patients, a percentage that did not include the number of major diseases in which genetics may only play some part.[184]

Lederberg viewed his request in three distinct and interrelated ways. His first and central point was that "there are a great many diseases that we do not ordinarily think of as 'genetic diseases' but where there is an important genetic component."[185] The second was that the rapid progress of molecular biology and the understanding of human genetics had direct applicability to medicine. As he made clear in his testimony, "We are witnessing today the beginning of a third major stage in the evolution of medicine." The first stage was the understanding of the structure of the human body (anatomy), and the second stage was the understanding of the complexities of the body's organs (physiology). Molecular biology was the third and highest stage.[186] His third distinction was that medicine was evolving now into treatment and prevention not based solely on any physical structures of our bodies, but on our genes. His request for funding was to

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[183] $10 Million dollars, Ibid., 942. Harold M. Schmeck, Jr., "National Genetic Study Urged as Step to New Era in Medicine," *The New York Times*, June 10 1970. Victor Cohn, "Task Force Asked on Genetic Illness," *The Washington Post*, June 11 1970.. *The New York Times* was the only major newspaper to mention that the hearings were closed, albeit near the bottom of the column.
[184] *Departments of Labor and Health, Education, and Welfare Appropriations for 1971*, 915, 41, 42.
[185] Ibid., 937.
[186] Ibid., 915.

make sure that the money was being spent in the most efficient way to make "genetic medicine"[187] a reality in a cost-effective and broad manner.

His July column, however, was written for a more general audience than his testimony and therefore carried his argument out into the daily lives of the citizens of the country. The tautly written piece made genetic mutation personal and the arrival of genetic-based medicine imminent. From the potential inborn errors to the dangers posed by pollution in the environment by radiation and chemicals, to better genetic screening methods to the potential of genetic vaccines, to the financial burden placed on the country by rising medical costs, Lederberg argued that genetic-based medicine should be supported by the government.

Lederberg was uniquely aware of the repercussions of sudden changes in molecular biology, such as Jonathan Beckwith's isolation of a gene seven months earlier or the discovery and application of restriction enzymes. Lederberg's Nobel Prize-winning experimental work as a graduate student in the 1940s laid the foundation for much of modern molecular biology leading to what we understand today as genetic engineering. His testimony and his July column tied together his extensive scientific background with an understanding of social and cultural concerns that presaged the recombinant DNA debates that would rage through the mid-1970s.

[187] Lederberg used the somewhat technical term "biochemical-genetic stage," which I have simplified to simply "genetic stage" or "genetic medicine."

Joshua Lederberg and the Foundation of Genetic Engineering

The trajectory of Joshua Lederberg from Brooklyn to Stockholm to accept his Nobel Prize, to the halls of Congress to provide testimony about genetic research is a one of dedication, social concern, and the occasional bit of luck. His early scientific work forever changed molecular biology and genetic research and laid the groundwork for genetic engineering.

Born in Brooklyn, New York, in 1925, Lederberg graduated high school at the age of fifteen and enrolled at Columbia University in the fall of 1941. During his sophomore year, he met the biochemist Francis J. Ryan, who would quickly become young Lederberg's mentor and set him on his scientific course. Ryan had returned to Columbia for the 1942-1943 academic year from a one-year postdoctoral fellowship at Stanford University working in the laboratory of George Beadle and Edward Tatum at the time they were completing their biggest discovery.

Beadle and Tatum were working with the red bread mold *Neurospora* to prove the hypothesis that genes controlled the synthesis of enzymes and that different genes produced different enzymes. Through the use of radiation, they were able to create mutants that were unable to produce either vitamin B1 or B6 yet required those vitamins to be present in their growth medium to survive. (Biologists use the term auxotrophic to describe nutritionally dependant mutants. The term prototrophic is used described nutritionally self-sufficient organisms.) After crossing the two mutants, the scientists discovered that their mutant progeny were unable to synthesize the amino acid tryptophan, and thus required the amino acid to be added to their medium to survive. They were then able to trace the tryptophan pathway (the specific gene responsible for

tryptophan production), and the slogan (and one of the early foundations of molecular biology) "one gene, one enzyme" was born.[188]

In 1943, Lederberg enrolled in the United States Navy under their V-12 program for premedical and medical training. As a technician at St. Albans Naval Hospital in the borough of Queens, his main duty was to "diagnosis malaria in the blood smears from the First Division Marines returned from Guadalcanal."[189] The training lasted a year and provided him with some of the necessary skills for a career in biomedical research and also discharged him from being drafted. Upon graduation from Columbia with a bachelor's degree in zoology, Lederberg entered Columbia University's College of Physicians and Surgeons in the fall of 1944 at the age of nineteen.

It was also in 1944 that a scientific paper was published that would open Lederberg's mind and motivate his research: "Studies on the Chemical Nature of the Substance Inducing Transformation of Pneumococcal Types" by Oswald Avery, Colin MacLeod, and Maclyn McCarty of the prestigious Hospital of the Rockefeller Institute for Medical Research.[190] Avery and his colleagues were trying to determine how nonvirulent strains of pneumococci (the bacteria that causes pneumonia) were

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[188] Michel Morange, *A History of Molecular Biology* (Cambridge, MA: Harvard University Press, 1998), 21-29, Horace Freeland Judson, *The Eighth Day of Creation: Makers of the Revolution in Biology*, Expanded ed. (Plainview, NY: CSHL Press, 1996), 189, George W. Beadle and Edward L. Tatum, "Genetic Control of Biochemical Reactions in Neurospora," *Proceedings of the National Academy of Sciences of the United States of America* 27 (1941). Beadle and Tatum first presented their findings in the summer of 1940, and their first publication was in 1941 (Judson, *The Eighth Day of Creation: Makers of the Revolution in Biology*, 189.).

[189] Joshua Lederberg, "Genetic Recombination in *Escherichia coli*: Disputation at Cold Spring Harbor, 1946-1996," *Genetics* 144 (1996): 440.

[190] Oswald T. Avery, Colin M MacLeod, and Maclyn McCarty, "Studies on the Chemical Nature of the Substance Inducing Transformation of Pneumococcal Types," *Journal of Experimental Medicine* 79, no. 2 (1944).

transforming to virulent strains. Their work focused on the protein coat of the bacteria and eventually led to an historic conclusion: genetic material was contained in nucleic acids. At the time of its publication, the most widely accepted model for genes held that they were protein, but through their carefully crafted research with pneumococci and a cautiously worded paper, Avery and his colleagues provided the first significant evidence that genetic information was stored on nucleic acids, which they described as the "substance inducing transformation" in their bacteria. They were also able to prove the first means of genetic exchange in bacteria when they concluded that bacteria took up a fragment of DNA and incorporated it into its own chromosome.[191]

Lederberg found reading Avery's paper "excruciating" and its implications "unlimited,"[192] and the paper left him with the idea that it "had all the earmarks of being the foundation of a new molecular genetics."[193] One problem still remained, and that was the common belief that bacteria reproduced asexually, thus creating identical daughter clones. Since, as the theory held, bacteria did not exchange genetic information with each other, there was no way to localize and map where or how the mutated genes segregated and resorted on the chromosome. In 1945, Rene Dubos of the Rockefeller Institute published *The Bacterial Gene*, which Lederberg later described as fulfilling "the expectations of its title in offering a very broad biological perspective on bacteria as organisms, not merely as malicious agents of putrefaction and disease."[194] Perhaps even

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[191] For an extensive history of Avery et al's experiment and its impact see: Judson, *The Eighth Day of Creation: Makers of the Revolution in Biology*, 17-24. Also see Morange, *A History of Molecular Biology*, 30-39.
[192] Judson, *The Eighth Day of Creation: Makers of the Revolution in Biology*, 23.
[193] Lederberg, "Genetic Recombination in *Escherichia coli*: Disputation at Cold Spring Harbor, 1946-1996," 439.
[194] Ibid.

more helpful to the young scientist was that Dubos did not try to conclusively answer the question of whether or not bacteria sexually reproduced, and even suggested that experiments should be carried out to determine if they did. The state of bacterial genetics was nearly virgin soil for Lederberg, all he had to do was prove bacteria sexually mated or carried out sexual recombination.[195]

Lederberg quickly set out on a research program to search for the sexual process in the summer of 1945 at Francis J. Ryan's Columbia laboratory. The bacterium that Lederberg chose for his research was a strain of *Escherichia coli*, which is common to the flora of the human gut. The design of Lederberg's *E. coli* experiment was similar to that of *Neurospora* experiment Beadle and Tatum mating prototrophic with auxotrophic mutants. Unable to create double auxotrophic mutants on his own,[196] Lederberg contacted Tatum, who successfully created *E. coli* double mutants for experiments.

Ryan wrote a recommendation letter to Tatum, who was just about to move to his new lab at Yale. Ryan and Tatum had known each other from the formers postdoctoral year at Stanford, and Tatum wanted to take on Lederberg for a year. Lederberg was taking a year's leave from medical school to carry out his experiments. The arrangement was agreed upon, and Lederberg joined Tatum's lab on March 18, 1946. Within three months of arriving in New Haven, Lederberg made the discovery that would forever alter genetics: bacteria mated and displayed genetic recombination. That is, the genes from a bacterium are exchanged with genes from another bacterium, and both bacteria

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[195] Rowland H. Davis, *The Microbial Models of Molecular Biology: From Genes to Genomes* (New York, NY: Oxford University Press, 2003), 82-85, Morange, *A History of Molecular Biology*, 55-56.

[196] "Double auxotrophic" means that the mutant is dependent on the addition of two nutritional elements from an external source for its survival.

incorporate the exchanged genes in their genomes. This second form of genetic exchange required two bacteria to be in contact with each other and was called conjugation.

The speed at which Lederberg reached his findings was extremely fortunate because it allowed him to travel with Tatum to the influential Cold Spring Harbor Symposium on Quantitative Biology in early July 1946. This meeting was the first one since the three-year hiatus during the Second World War, and the list of attendees reads likes a *Who's Who* of the founding fathers of molecular biology.[197] Lederberg announced his discovery here, unexpectedly. As Lederberg recalled, Tatum delivered his talk entitled "Induced Biochemical Mutations in Bacteria," where he mentioned,

> "The main attribute lacking in bacteria which would make them ideal material for combed genetic and biochemical investigation is their apparent lack of a sexual phase…." We were just not sure whether the time was ripe for the announcement of my recent findings. Carl Lindegren did pick up on the cautious wording about "apparent lack," saying "[Tatum] was somewhat more cautious than Dr. Dubos, Dr. Lwoff, and Dr. Luria all of who deplored the fact that 'there is no sexual mechanism in bacteria'."[198] He then voiced the parable that the sexual phase of a red bread mold (namely *Neurospora*) was unknown for a hundred years. Tatum's discussion did then refer to my experiments, and he negotiated with Demerec for an exception from the published program to permit me to present them.[199]

A few days later the twenty-one-year-old Lederberg, along with Tatum, presented his findings at a talk entitled "Novel Genotypes in Mixed Cultures of Biochemical Mutants of Bacteria."

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[197] Those in attendance included Max Delbrück, Salvador Luria, Rene Dubos, Alfred Hershey, Boris Ephrussi, Barbara McClintock, Ernst Mayr, Oswald Aver, Maclyn McCarty, Jacques Monod, Milislav Demerec, and André Lwoff. (Cold Spring Harbor Library, "CSHL: Symposia on Quantitative Biology, Xi: Heredity and Variation in Microorganisms, 1946," http://library.cshl.edu/symposia/1946/contents.html.)
[198] Rene Dubos, Andrew Lwoff, and Salvador Luria.
[199] Lederberg, "Genetic Recombination in *Escherichia coli*: Disputation at Cold Spring Harbor, 1946-1996," 441.

The talk was well received with the notable exception of André Lwoff,[200] who believed that it was an exchange of metabolic material between the bacteria and not sexual recombination that accounted for Lederberg's results.[201] The composition of audience for the presentation and the debate meant that Lederberg's discovery would be accepted much more quickly than if it had simply appeared as a journal article.[202]

The ramifications of his findings were immediate and enormous for the fields of genetics and molecular biology. *E. coli* reproduced exponentially faster than any of the other multicellular model organisms, such as fruit flies and *Neurospora*, and the single chromosome of a bacterium is vastly more simple than those of higher organisms. Mutations over successive generations could be studied in days, not weeks or months. And, using the proper techniques, the bacteria would reproduce asexually, creating identical clones of the initial bacterium. Within just months of dedicated research Lederberg, by proving that genetic recombination occurred, opened the very real

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[200] André Lwoff (1902-1994) joined the prestigious Institut Pasteur at the age of 19, became Head of the Department in 1938, and Professor of Microbiology at the Science Faculty in Paris in 1959. His early work was on the development of growth factors for flagellates – cells with a whip-like (or hair-like) organelle used as a means of locomotion. In the 1930s he research focused on the nutritional requirements, such as vitamins, that were essential to metabolism and growth of bacteria. By the late 1930s he began his pioneering work on the study of bacteriophages that helped create the foundational discoveries for molecular biology. His later work included the study of poliovirus in 1954 and the development of viral infections. For his work he shared the Nobel Prize in Physiology or Medicine in 1965 with Institut of Pasteur colleagues Francois Jacob and Jacques Monod for "their discoveries concerning the genetic control of enzyme and virus synthesis." See Judson, *The Eighth Day of Creation: Makers of the Revolution in Biology*, 345-47, Morange, *A History of Molecular Biology*, 52, The Nobel Foundation, "Andre Lwoff -- Biography," http://nobelprize.org/nobel_prizes/medicine/laureates/1965/lwoff-bio.html.

[201] With the help of Max Zelle, Lederberg was able to provide Lwoff with microscopic evidence of the crossing over of genetic material. (See Morange, *A History of Molecular Biology*, 57.)

[202] Lederberg, "Genetic Recombination in *Escherichia coli*: Disputation at Cold Spring Harbor, 1946-1996," 441, Morange, *A History of Molecular Biology*, 57.

potential that a genetic map for bacteria could be created and demonstrated that the genetics of bacteria might be analogous to higher organisms.

Lederberg and Tatum published two initial papers based on their results. The first was a taut eight-paragraph paper in the prestigious and widely read journal *Nature* plainly stating their results and concluding, "These experiments imply the occurrence of sexual process in the bacterium *Escherichia coli*; they will be reported in more detail elsewhere."[203] And, following protocol of publishing their work in the official journal of the symposium where it was first presented, the authors published their first descriptive account of their experiments and results in the *Cold Spring Harbor Symposia on Quantitative Biology*.[204] After the initial rush to publish following the announcement of their results, the two penned an extensive article the following year entitled simply, "Gene Recombination in the Bacterium *Escherichia Coli*." This clearly written paper included a new piece of much-needed information: that the strain of *E. coli* was important to a successful recombinant experiment.[205]

The Role of Luck

Lederberg later described his selection of the K-12 strain of *E. coli* as "an especially lucky selection." Not realizing that the particular strain of *E. coli* was consequential when he was performing his experiments, Lederberg soon found out that

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[203] Joshua Lederberg and Edward L. Tatum, "Gene Recombination in *Escherichia coli*," *Nature* 158 (1946).
[204] _____, "Novel Genotypes in Mixed Cultures of Biochemical Mutants of Bacteria," *Cold Spring Harbor Symposia on Quantitative Biology* 11 (1946).
[205] _____, "Gene Recombination in the Bacterium *Escherichia coli*," *Journal of Bacteriology* 53, no. 6 (1947).

the use of the K-12 strain might have been one of the most serendipitous events in the history of molecular biology. Soon after returning to the California Institute of Technology after the symposium, Salvador Luria tried to carry out Lederberg's experiments using the same protocol, except the *E. coli* commonly used in Luria's lab was the B strain. Luria was unable to reproduce any of Lederberg's results, causing a small uproar at Caltech, until other researchers noticed that they were successful using the K-12 strain.[206]

The K-12 strain came to Columbia and Yale from Stanford, where it was initially isolated in 1922 from a person recovering from diphtheria and has been part of the bacterial strain collection at Stanford since 1925. Tatum regularly used this strain in his laboratory at Stanford and made it a regular part of his laboratory at Yale.[207] The K-12 strain is unique that would later be designated F^+ (for fertility factors), and approximately 5% of all strains of *E. coli* are F^+. It was later determined that the mutations to the *E. coli* used in Lederberg's experiments created a mating between F^+ and F^- bacteria, which is optimal of detecting any gene transfer between the bacteria.[208]

Following his initial discovery in 1946, Lederberg continued experimenting with *Salmonella*, a bacteria related to E. *coli* but more virulent and, thus, of greater interest for more general medical studies. Lederberg also isolated β-galactosidase, a group of enzymes that enabled the bacteria to metabolize glucose. His laboratory also pioneered

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[206] Lederberg, "Genetic Recombination in *Escherichia coli*: Disputation at Cold Spring Harbor, 1946-1996," 441.
[207] Although I could not find any definitive proof, the use of K-12 by Lederberg at Columbia may have been very well at the suggestion of Ryan after his postdoctoral work with Tatum at Stanford.
[208] Davis, *The Microbial Models of Molecular Biology: From Genes to Genomes*, 91.

the use of antibiotics, notably penicillin and streptomycin, as a means to more quickly identify auxotrophic mutants. The most important discovery during this time was a third way that genetic information could be exchanged between bacteria. In 1951, Lederberg and his graduate student Norton Zinder were working on transformation experiments, when they noticed that genetic information was being exchanged without the mutant bacteria strains coming into contact with each other. The two determined that bacteriophages (commonly called "phages")—viruses that infect only bacteria—were present in the solution that contained the mutants.[209] It was quickly determined that, when the phages infected the bacteria, the phages were incorporating some of their DNA into the chromosome of the bacteria. The term "transduction" was coined to describe this method of genetic exchange.[210]

Also at this time, a circular piece of naked DNA,[211] replicated independently from the bacterial chromosome, became an object of study for Lederberg. In 1952 he coined the term "plasmid"—"a generic term for any extrachromosomal hereditary determinant. The plasmid itself may be genetically simple or complex."[212] Plasmids were found to carry out nonessential cellular functions (i.e., the bacterium could survive without the

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[209] The term bacteriophage is the combination of the word "bacteria" and the Greek word *phagein*, "to eat."
[210] Norton Zinder and Joshua Lederberg, "Genetic Exchange in Salmonella," *Journal of Bacteriology* 64 (1952). For a personal history on the use bacteriophages as a foundational tool of molecular biology see, John Cairns, Gunther S. Stent, and James D. Watson, *Phage and the Origins of Molecular Biology*, Expanded ed. (Plainview, NY: Cold Spring Harbor Laboratory Press, 1992).
[211] Naked DNA means that there are no genes necessary to create any type structure (membrane, protein coat, etc.) to encase the genetic material.
[212] Joshua Lederberg, "Cell Genetics and Hereditary Symbiosis," *Physiological Reviews* 32 (1952): 403.

plasmid) and to contain the F factors that enabled conjugation to occur. It was later determined that plasmids carry the genes that provide bacteria with antibiotic resistance.

The legacy of Lederberg's discoveries during the decade from 1945-1955 helped drive the field of molecular biology and created the foundation for recombinant DNA. For decades, *E. coli* was the workhorse model organism in molecular biology because it could provide insight into the basic functions of living organisms that mammalian cells, which proved too large and inaccessible to manipulate on the molecular level, could not. An example of the long-term influence of Lederberg's early research was his work isolating β-galactosidase. This was not an experimental avenue that Lederberg himself traveled down, but it led to the Nobel Prize-winning work by Francois Jacob and Jacques Monod on enzyme regulation and the discovery of the *lac operon*—the gene sequence that controls how lactose is metabolized. This discovery attracted Jonathan Beckwith to work with Jacob and Monod after completing his doctorate, and later Beckwith became the first person to physically isolate a gene from a chromosome.[213]

Professional Success and New Pursuits

Professionally Lederberg advanced quickly during these years of discoveries in bacterial genetics. He was awarded his PhD from Yale (1948), appointed Assistant

[213] Francois Jacob, Jacques Monod, and Andrew Lwoff equally shared the Nobel Prize in Physiology or Medicine in 1965. For an explanation of their work see Morange, *A History of Molecular Biology*, 150-63, Judson, *The Eighth Day of Creation: Makers of the Revolution in Biology*, 390-91, 401-11. For Beckwith's work with Jacob and Monod see Jonathan R. Beckwith, *Making Genes, Making Waves: A Social Activist in Science* (Cambridge, MA: Harvard University Press, 2002), 18-27. For the first physical isolation of a gene from a chromosome see Chapter 3.

Professor at the University of Wisconsin (1947), where he was promoted to Associate Professor (1950) and full Professor (1954). He organized the Department of Medical Genetics at Wisconsin in 1957 and was soon hired by the Stanford University Medical School to organize the Department of Genetics. That same year he was also elected a member of the National Academy of Sciences.

Perhaps the most famous and, perhaps, most widely read description of Lederberg's work following his discovery of bacterial mating and genetic recombination is by James Watson in his bestseller, *The Double Helix*—his autobiographical and heroic sketch of his and Francis Crick's discovery of the structure of DNA. The very competitive Watson described Lederberg's early research as "a prodigious number of pretty experiments that virtually no one except [Luigi Luca Cavalli-Sforza] dared to work in the same field." Watson continued, "Hearing Joshua give Rabelaisian nonstop talks of three to five hours made it all to clear that he was an *enfant terrible*. Moreover, there was his godlike quality of each year expanding in size, perhaps eventually to fill the universe." Watson concluded that Lederberg only "took any enjoyment from the rabbinical complexity shrouding his recent papers [on bacterial genetics]."[214]

In 1958, thirty-three-year-old Joshua Lederberg obtained the most prestigious honor in science when was awarded the Nobel Prize in Physiology and Medicine "for his discoveries concerning genetic recombination and the organization of the genetic material of bacteria."[215] He shared the award that year with George Beadle and Edward Tatum for

[214] James D. Watson and Gunther S. Stent, *The Double Helix: A Personal Account of the Discovery of the Structure of DNA* (New York, NY: Norton, 1980), 83.
[215] Werner Wiskari, "Three U.S. Geneticists Win Nobel Medicine Award," *The New York Times*, 31 October 1958.

their work with *Neurospora*.[216] The 1958 prize proved to be the first awarded to molecular biologists, who would regularly win the award for the next two decades as their discoveries propelled the field into the forefront of science and into the public eye.[217]

During the late 1950s and 1960s, Lederberg's scientific interests began to stray from bacterial genetics into other fields. While a Fulbright Visiting Professor of Bacteriology at Melbourne University in 1957, the Soviet Union launched Sputnik into low earth orbit. Upon returning to the University of Wisconsin, Lederberg immersed himself in astronomy and aeronautics to understand the possibilities of life in outer space and the possibility of discovering the origins of life. Lederberg's excitement over potential discoveries was tempered by an anxiety about the potential of spacecrafts (manned or unmanned) that returned to Earth carrying unknown pathogens with the potential to cause mass casualties. Lederberg coined the term "exobiology" for the branch of science that studies life beyond earth's atmosphere.[218] Lederberg served on national panels that made recommendations that were accepted by the National Aeronautics and Space Administration (NASA) about sterilization, containment, and quarantine protocols for the entire spacecraft before take off and again upon reentry, especially if any samples

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[216] The actual breakdown of the 1958 Nobel Prize in Physiology or Medicine: Lederberg was awarded half of the prize and Beadle and Tatum each received a quarter of the prize.
[217] Notable molecular biologists awarded the Nobel Prize in Physiology or Medicine include Arthur Kornberg (1959); Francis Crick and James Watson (1962); Francios Jacob, Andrew Lwoff, and Jacques Monod (1965); Max Dulbruck, Alfred Hershey, and Salvador Luria (1969); David Baltimore, Renato Dulbecco, and Howard Temin (1975); Werner Arber, Daniel Nathans, and Hamilton Smith (1978). Max Perutz and John Kendrew were awarded the Nobel Prize in Chemistry in 1962.
[218] More recently, exobiology has fallen under the more encompassing term "astrobiology," which also now includes exopaleotology and bioastronomy. NASA, however, still retains an Exobiology Program as part of its Solar System Exploration Division in the Ames Research Center.

were gathered while in space. This concern about potential interstellar pathogens contaminating the earth (and killing humans) gained broad cultural and scientific significance after the 1969 publication of Michael Crichton's bestseller *The Andromeda Strain.*

By the mid-1960s Lederberg became more recognizable inside the Beltway when he began to write a science column for *The Washington Post* initially titled "Science and Man" that began in the summer of 1966 and appeared weekly until late 1970. It continued as a monthly until the column ended in September 1971. Lederberg might have seemed like an odd selection to write a regular scientific column for *The Post*, but the selection proved prescient as the breakthroughs and technological advancements in biology and medicine occurred during these years. Though a geneticist by training, his columns addressed broader scientific issues over the six years it was published. Some of the topics that Lederberg returned to over the years included space exploration, science education, food policy/hunger, reproduction issues, science and war and peace, environmentalism, and, especially, breakthroughs in medicine and genetics.

The rationale for writing the column was mentioned in Lederberg's first appearance before Congress in 1967. Lederberg described his frustration that the academic scientist's "message has, seemingly, not been understood" by the public or government because "perhaps we have been too busy in the laboratory and not given enough attention to this essential political communication about our purposes." The column was his attempt to make the "substance and purposes of science" available to the

voting public without couching it in "specialized language."[219] Despite discovering a "lively reaction" to his column, Lederberg, however, admitted that he has "been chastened that another metropolitan newspaper editor found the same material to be too 'lofty' for his own comprehension."[220]

The following year, Lederberg, once again on Capitol Hill for testimony, addressed the problems of "communication between [the scientific] community and the other world, of politics and the man in the street. Some [scientists] spend more than others trying to accomplish fulfillment of that gap."[221] The gap exists not out of the concern of scientists, but rather because either they viewed themselves as "not especially well-equipped to probe the human and social implications of their work" or because they cannot divert time and energy away from their labs and work. Lederberg viewed his role in filling the communication gap as important by accepting that his own "productivity as a scientist as greatly decreased."[222]

The Headline

The day after his June 10, 1970, testimony, Lederberg made the front page of *The New York Times* by himself for the first time on an extremely busy news day.[223] Politics

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[219] US Congress. Senate. Subcommittee on Government Research, *Research in the Service of Man*, 90st Cong., 1st sess., 1967, 8.
[220] Ibid.
[221] US Congress. Senate. Subcommittee on Government Research, *National Commission on Health and Science Policy*, 90st Cong., 2nd sess., 1968, 56.
[222] Ibid., 55-56.
[223] Lederberg appeared on the front page of *The New York Times* several times. The first was on October 31, 1958, with George Beadle and Edward Tatum when all three were awarded the Nobel Prize in Physiology and Medicine. The second time was as a signatory in support of a few scientific proposals. And multiple times in passing stories

and the Vietnam War drove the news of the day and occupied the above-the-fold headlines. The Nixon administration occupied the majority of those headlines as President Nixon was moving people around the executive branch,[224] a panel backed Nixon's incursion into Cambodia, and, in a move to mollify Congress, Nixon presented a revised welfare reform program. The other major political headline was the passage of the Clean Air Act in the House by an overwhelming vote of 374-1. And yet, placed near the longitudinal meridian of the page, two-thirds from the top, a picture of Lederberg appeared next to a headline that read "National Genetics Study Urged As Step to New Era in Medicine."[225] Inside the Beltway, in *The Washington Post*, Lederberg's testimony did not get front page coverage—it was buried on page B4.[226]

Both the *Times* and the *Post* covered the testimony with the ten million dollar appropriations request, the creation of the Task Force, and the headline-grabbing figure that at least 25 percent of hospital patients are suffering wholly from genetic disease. Both, however, failed to explore the second half of Lederberg's testimony that focused on

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on space exploration in the 1960s. Wiskari, "Three U.S. Geneticists Win Nobel Medicine Award." Walter Sullivan, "Manned Moon Flight Supported in 8 Scientists' Retort to Critics," *The New York Times*, 27 May 1963. ———, "Panel Urges Landing on Mars to Seek Signs of Life," *The New York Times*, 27 April 1965, John Cogley, "Nobel Laureates Petition the Pope," *The New York Times*, 22 January 1965, Walter Sullivan, "Scientists Find Thin Air on Mars Ideal for Low-Orbit Inspection," *The New York Times*, 4 September 1965.

[224] Notable moves were Nixon naming George P. Shultz, who was the Secretary of Labor, as the director of the Office of Management and Budget (OMB); James Hodgson, the Under Secretary of Labor, was nominated to succeed Shultz; John D. Ehrlichman, Assistant to the President for Domestic Affairs, as the executive director of the newly created Domestic Council; Caspar W. Weinberger, chair of the Federal Trade Commission, as the deputy director at OMB; and Robert P. Mayo, director of the Budget Bureau, as Counselor to the President.

[225] Schmeck, "National Genetic Study Urged as Step to New Era in Medicine."

[226] Cohn, "Task Force Asked on Genetic Illness."

what constituted genetic medicine and why it was a pressing topic that needed to addressed sooner rather than later.

The Third Wave of Medicine: Genetic Medicine

In both his testimony and his column of July 20, Lederberg avoided the use of the term "genetic engineering"—a topic he had given testimony on in previous years—going so far as to referring to genetic engineering as a "subject that has seriously loaded overtones to many people" and which had produced the impression that "the State might decide what the characteristics of an individual ought to be, that it might program what a human being out to be like and so forth."[227] In his column a month later, he took it a step further stating, "Most people's fears about genetic engineering are probably more mythological than political, based on unfamiliarity with the role of genes and other factors in development, and on the sense that the DNA amounts to the inner personality."[228]

He couched his language of genetics as an evolution of medicine, including creating new terminology, to avoid using the term "genetic engineering," which, for him, was a loaded and pejorative term too easily employed to detract from the scientific merits of genetic research. The clearest expression of his thoughts on the use of the term appeared in his written testimony on June 10 where Lederberg stated, "A great deal of recent discussion about 'genetic engineering' should be mentioned only to be deplored, for it gives a distorted view of the present status and ultimate purposes of research on

[227] *Departments of Labor and Health, Education, and Welfare Appropriations for 1971*, 943.
[228] Lederberg, "Government Is Most Dangerous of Genetic Engineers."

molecular biology." Proving that he understood the proverbial other side of the coin, he continued, "Nevertheless, it would be obtuse to deny the ultimate revolutionary importance of developments that bear on the further course of human evolution. By replacing blind fate with human reason, they may place a crushing burden of responsibility for the assignment of goals for man. Many people react with dismay that knowledge is coupled with such a responsibility. Failing to accept it is also a decision, and one that has its own consequences."[229]

In his column and his testimony, Lederberg employed a subtle rhetorical style to lower his readers' concerns about the potential dangers of human genetic manipulation, but also making it clear that the potential to therapeutically alter human DNA was not without moral or ethical concerns. These concerns, however, were ameliorated because, as Lederberg put forward, it was not the information in our DNA that was of the utmost importance, but rather that "the impact of culture, the progressive component of history, is, after all, what distinguishes man from every other living species."

DNA, for Lederberg, was not a static set of instructions that predetermined a person's outcome. He wrote what might be one of the simplest and most powerful explanations of this by using a string of tangible analogies aimed at an American audience:

[229] *Departments of Labor and Health, Education, and Welfare Appropriations for 1971*, 943. Quotation marks in original.

> The DNA of a human cell is not a man, any more than a written constitution is a living democracy or than the architectural plans for a dwelling are a happy home. An ideal plan can be wrecked by clumsy implementation of by unpredictable accidents. Many imperfections in engineering designs may give no trouble; an oversized auto piston may still work most of the time. Yet when we have a catastrophic failure, like the explosion of the oxygen tank of Apollo 13, we may discover that the fail was a weakness in design. This view depends, however, on what we expect the engineer to allow for and how much we will invest to cover every contingency. Many run-of-the-mill breakdowns in automobiles could be called design failures; they are also faults in a market mechanism that makes it difficult for the consumer to buy reliability from the auto dealer. [230]

These engineering flaws and unpredictable accidents, whether they were inborn or caused by environmental factors, however, do not pose the greatest risk to the survival of humans.

Lederberg also expressed two concerns that had gained large public attention by 1970 and tied them into his larger argument on the need for genetic medicine. Those two concerns were the exploding growth rate of the world's population and the United States' growing environmental problems. Of these two, Lederberg downplayed the role of a biological or technical solution to overpopulation, concluding that, like war, it required political solution. For Lederberg, it was "pure fantasy" to believe in purely biological solution such as controlled breeding or forced sterilizations—the latter by traditional or genetic means. However, biological advances in contraception and their safety and availability were creating an atmosphere for a voluntary means of slowing population growth.[231]

The other major concern in the late 1960s and early 1970s was the impact of pollution on humans. The increased damage to human DNA by environmental factors in

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[230] ———, "Government Is Most Dangerous of Genetic Engineers."
[231] *Departments of Labor and Health, Education, and Welfare Appropriations for 1971*, 943. ———, "Government Is Most Dangerous of Genetic Engineers."

modern America was a major point of concern for Lederberg because it was increasing rate of genetic mutations—he cited the Atomic Energy Commission, which reported that the natural mutation rate had increased by 10 percent. The damage to human DNA was being done not only by chemical pollutants in our air and water, but also by radiation exposure in our medical tests, namely X-rays, and in the environment through atomic energy production. The result of the increased mutations to human genes was an increase in the rate of genetic diseases, and thus an increase in medical costs.

The real threat to human existence for Lederberg ("our glaring fault") was war, "our failure as a species to institutionalize a global framework for peace." War did not have a genetic solution. War was a "political problem" that required political solutions. He also disregarded eugenics as a potential hazard. Knowledge of genetics had increased exponentially in the last two decades, and it was known by the early 1970s that parents usually are not afflicted by the genetic diseases that they can pass on to their children. Therefore "it is almost totally futile to try to block the spread of genetic diseases to future generations by sterilizing those who are affected in this one."[232]

The solution to the problems outlined by Lederberg in his testimony and column was genetic medicine. This new stage of medicine was made up of three basic parts: genetic screening, genetic counseling, and biological engineering. Each of these three parts saw rapid progression in the years preceding Lederberg's testimony in what he

[232] Lederberg, "Government Is Most Dangerous of Genetic Engineers." In his column, Lederberg avoided using the term eugenics, though he makes implicit reference to the failures of eugenics in his use of the term "controlled breeding" and in his explanation of why it was successful with animals and plants and not humans. For Lederberg's understanding of eugenics see, *Departments of Labor and Health, Education, and Welfare Appropriations for 1971*, 930-31.

referred to as the “swift pace” of molecular biology and its “unexpectedly rapid translation of the basic research findings into practical medical therapies.”[233]

Genetic screening of humans had been carried out on adults for decades, and by 1970 there were an estimated 2,000 distinct genetic disorders that had been catalogued. The techniques over these decades had become more refined and identification more accurate, but for Lederberg discovering these diseases at adulthood did not help babies with genetic diseases. A solution was amniocentesis, which had made rapid progress during the previous fifteen years.[234]

Amniocentesis is the technique of removing fetal cells from the amniotic fluid inside the womb—typically through the use of a large syringe—and then testing them in a laboratory for potential genetic defects. The first major breakthrough in amniocentesis occurred in 1956 with the publication of the seminal paper by Fritz Fuchs and Povl Riis in the journal *Nature*, where they determined fetal sex from cells from amniotic fluid.[235] Following their paper, discoveries were made at an incremental pace until 1966 when it was demonstrated that fetal cells drawn from amniotic fluid and cultured were suitable for karyotyping—a process where the chromosomes are stained and compared to the standard chromosomes of an organism to examine any chromosomal aberrations.[236] After this discovery, amniocentesis was used to identify many inborn metabolic errors,

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[233] *Departments of Labor and Health, Education, and Welfare Appropriations for 1971*, 916.
[234] Ibid.
[235] Fritz Fuchs and Povl Riis, "Antenatal Sex Determination," *Nature* 177, no. 4503 (1956).
[236] MW Steele and WR Jr. Berg, "Chromosome Analysis of Human Amniotic-Fluid Cells," *The Lancet* 1, no. 7434 (1966).

neurological disorders, and sex-linked disorders that doctors regularly test for today, including Down's syndrome (also known as trisomy 21).

The prenatal diagnosis for Lederberg was the crucial because it allowed for therapy to begin before birth or allowed for a medically justified abortion—the latter of which he referred to as a way to "prevent a genetic tragedy" while allowing the woman to "try again to have the healthy baby which she has some odds of producing."[237] Lederberg acknowledged that abortion was not an "ideal solution" for inborn genetic errors, allowing for personal and religious reasons for bringing a child to term, but because genetic errors accounted for the high rates of infant mortality in hospitals (40 percent by his calculations), amniocentesis must be further studied to reduce these deaths.[238]

Genetic medicine solved the problems of genetic diseases in two ways after genetic analysis: genetic counseling and biological engineering. Genetic counseling, which had been used with limited effectiveness since the early twentieth century for simple Mendelian genetic disorders such as hemophilia and Tay-Sachs, began to gain considerably more emphasis in the late 1960s and early 1970s with advances in screening. The notion that all or the great majority of children will have a specific genetic disease was largely discounted by Lederberg because genetics counselors could only predict the probability that a child would have a genetic disease. The more likely use of

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[237] "prevent a genetic tragedy" *Departments of Labor and Health, Education, and Welfare Appropriations for 1971*, 917, "try again..." Lederberg, "Government Is Most Dangerous of Genetic Engineers." Lederberg provides an example of how amniocentesis would be used and weighed in ethical terms if a medical abortion was recommended, *Departments of Labor and Health, Education, and Welfare Appropriations for 1971*, 940-41.The *Roe v. Wade* decision legalizing abortion in the United States was still over two years away, occurring on January 22, 1973.

[238] "ideal solution" Lederberg, "Government Is Most Dangerous of Genetic Engineers.", mortality statistics *Departments of Labor and Health, Education, and Welfare Appropriations for 1971*, 938.

genetic counseling would be to provide information about the problems patients might confront with any given genetic disease, as well as advice on the potential outcomes. Managing these outcomes was biological engineering—the third and final piece of Lederberg's genetic medicine.

For Lederberg, biological engineering included a wide array of practices from reproductive technologies to organ transplants to intervention at the molecular level. Reproductive technologies included medical abortions because of genetic defects and artificial insemination, which was a relatively new technology.[239] The mid-1960s was a decade of spectacular advances in organ transplantation. The first successful pancreas transplant was in 1966, followed by the first liver and heart/lung transplants the next year. Organ transplants were a kind of biological engineering because they could prolong the lives of patients diagnosed with genetic diseases (or diseases with a genetic component) that afflicted a certain organ of the body.[240]

The most advanced technique that Lederberg proposed was genetic vaccination—a term Lederberg coined. Lederberg did not envision inserting or directly altering the DNA of a germ (sex) cell to prevent transmission of a genetic disease, but rather a process analogous to vaccines already used, such as the polio and smallpox vaccines. Though lacking specific details, Lederberg proposed that a specially developed virus could theoretically be used to repair genetic damage in somatic (body) cells rather than provoke an immunological response. The goal of this treatment would be alleviate the

[239] In the late 1960s and early 1970s consideration was given to couples where the male was sterile. In vitro fertilization (IVF) was not an option until later in the decade when the first IVF baby was born on July 25, 1978. *Departments of Labor and Health, Education, and Welfare Appropriations for 1971,* 948-49.
[240] Ibid., 922, 37.

suffering caused by a disease and not to eradicate genes. As Lederberg pointed out in his testimony, the eradication of some genes that are detrimental in particular combinations may prevent researchers from discovering biological advantages in other genetic combinations.[241]

Although advances such as genetic vaccines were only theoretical in 1970, Lederberg's consistent concern about the rights of individuals over state control of genetic technologies dominated portions of his testimony and writings. Before Congress, he expressed concern, not just about the creation of new technologies, but "of [the] massive intrusion of the power of the State with respect to biological engineering."[242] And though, "there may be a time when some form of social control of genetic technology may be desirable," it was not in 1970.[243] To avoid state control in the future, Lederberg stressed to the congressmen that great care and caution must be used to preserve the balance and tension between the individual and the community in field of biological technology, as much as in free speech and education.[244]

In his column, Lederberg painted a similar picture of the dangers of the state, especially when applied to reproductive technologies and the poor. He, however, used a common analogy that a lay readership would understand when he concluded: "What do you think of a scalpel? It can cut out a tumor or repair a heart. It can also turn a stallion into a gelding. Genetic engineering will need much the same kind of legal control that

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[241] Ibid., 926, 40, 42-43, Lederberg, "Government Is Most Dangerous of Genetic Engineers.", Joshua Lederberg, "Biological Innovation and Genetic Intervention," in *Challenging Biological Problems: Directions toward Their Solution*, ed. John A. Behnke (New York, NY: Oxford University Press, 1972), 21-23.
[242] *Departments of Labor and Health, Education, and Welfare Appropriations for 1971*, 943.
[243] Ibid., 945.
[244] Ibid., 943, Lederberg, "Government Is Most Dangerous of Genetic Engineers."

surgery needs and has." Lederberg probably had no idea that the debate over genetic engineering would begin and be so contentious only a couple years after the publication of this column.[245]

One Last Step Toward Recombinant DNA: Restriction Enzymes

There is no straight, direct, or Lamarckian direction to molecular biology. Rather, discoveries happened organic intermixing among several new scientific fields. Lederberg's discovery of conjugation in bacteria serves as such an example. His interest in bacteria was spurred by Oswald Avery's groundbreaking (albeit tempered) discovery; his experimental program was based on Beadle and Tatum's experiments on the *Neurospora* fungus; his luck grew by using the K12 strain of *E. coli*; and his dogged intellectual determination and laboratory skill to prove that bacteria do indeed have sex yielded a foundational model organism that would drive many biological fields, including molecular biology and genetics.

There were many important discoveries leading up to the creation of recombinant DNA technology since the discovery of bacterial recombination, notably James Watson and Francis Crick's discovery of the structure of DNA in 1953.[246] But arguably the most important was the discovery of restriction enzymes in the late 1960s and early 1970s. Salvador Luria and Mary Human first observed the action of these enzymes in 1952-

[245] Lederberg, "Government Is Most Dangerous of Genetic Engineers."
[246] James D. Watson and Francis Crick, "Molecular Structure of Nucleic Acids," *Nature* 171, no. 4356 (1953).

1953.[247] The two researches were attempting to explain why the ability of bacteriophages (phages) to grow in different bacteria, including *E. coli,* varied. In some bacteria, the phages reproduced normally, while in others their reproduction was restricted. What Luria and Human concluded was that, once reproduction was achieved by a phage in a particular strain of bacteria, it would continue, but if it was then moved to another bacteria (or different strain of the same bacteria), its reproduction was restricted. The terminology they created to describe their observations were "host-induced" and "host-controlled" because Luria and Human were not able to identify exactly what in the bacteria was restricting the reproduction of phages.[248] Their paper, however, opened a new avenue for further research on the interaction between bacteria and phages.[249]

In 1960, Werner Arber, a young Swiss biochemist, became interested in the restriction of phages in bacteria. Arber chose the *E. coli* K12 strain and a derivative of the K12 strain to carry out his experiments because of the work on bacteria Lederberg in the 1950s.[250] After a year of experimentation Arber concluded that host-controlled modifications in the bacteria were a defense mechanism against foreign DNA and acted by destroying the foreign DNA by cutting up at a specific DNA sequence. Arber's experiments throughout 1960s allowed him discover the first restriction enzyme and create a theoretical framework for understanding the restriction process in bacteria where

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[247] Salvador E. Luria and Mary Human, "A Nonhereditary, Host-Induced Variation of Bacterial Viruses," *Journal of Bacteriology* 64, no. 4 (1952).
[248] Ibid.: 557.
[249] Richard J. Roberts, "How Restriction Enzymes Became the Workhorses of Molecular Biology," *Proceedings of the National Academy of Sciences of the United States of America* 102, no. 17 (2005): 5905.
[250] Werner Arber, "Promotion and Limitation of Genetic Exchange," in *Les Prix Nobel*, ed. Jan Lindsten (Singapore: World Scientific Publishing Co., 1992), 486.The other strain was K(P1), the P1 lysogenic derivative of K12.

foreign DNA was restricted, but the bacteria's own DNA was protected. The process is known today as the restriction/methylation system, where an enzyme adds a methyl group ($-CH_3$) to the specific sites that the restriction enzyme recognizes, thus preventing the restriction enzyme from cutting the bacteria's own DNA. Foreign DNA not containing the methyl group, however, are recognized and cut by the restriction enzymes.[251]

In the late 1960s this breakthrough was made when Matthew Meselson and Bob Yuan isolated a restriction enzyme from a strain of *E. coli*.[252] The early restriction enzymes that were discovered, however, had the "unfortunate property of cleaving DNA randomly, thus rendering the enzymes unsuitable for use as cloning and mapping reagents."[253] What was needed to make restriction enzymes more useful was cut (or cleave) a piece of DNA into specific fragments.

In 1970, while studying *Hemophilus influenzae*, Hamilton Smith of Johns Hopkins University discovered a new class of restriction enzymes that was both site-specific and that cut DNA into specific fragments.[254] After publishing his journal article, Smith moved on from restriction enzymes toward other scientific pursuits. As he later explained, "I did my nice piece of biochemistry and, for some reason, I wasn't interested in the actual application of the enzyme…. I just more or less published my work and put

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[251] For the state of restriction enzyme experiments as understood by Arber in the mid-1960s see, ———, "Host-Controlled Modification of Bacteriophage," *Annual Review of Microbiology* 19 (1965).
[252] Matthew Meselson and Robert Yuan, "DNA Restriction Enzyme from *E. coli*," *Nature* 271, no. 5134 (1968).
[253] Roberts, "How Restriction Enzymes Became the Workhorses of Molecular Biology," 5905.
[254] Thomas Kelly and Hamilton O. Smith, "A Restriction Enzyme from Hemophilus Influenzae Ii Base Sequence of the Recognition Site," *Journal of Molecular Biology* 51 (1970).

it on the shelf."[255] Interest in the application of this new class of restriction enzymes, however, was quickly picked up by Daniel Nathans, his colleague at Johns Hopkins.[256]

Nathans spent 1969 on a sabbatical studying simian virus 40 (SV40), and upon returning to Johns Hopkins, he began studying the effects of Smith's restriction enzyme on SV40.[257] First identified in 1960, SV40 is a small, circular, and relatively simple virus containing only five genes that cause tumors in some animals, notably mice, but is usually found in a latent form in primate and human cells. Another important characteristic of SV40 is that it integrates itself into the DNA of the organism it infects. Although it replicates itself as part of the host's DNA, most of the time it remains latent and does the host no harm. Because of its sheer simplicity it quickly became a widely used experimental organism. Nathans, having read the paper by Meselson and Yuan in 1969, as well as the work of his colleague Smith, concluded, "it seemed likely (as first suggested by Arber) that restriction enzymes could be used to digest DNA molecules into specific fragments, just as specific proteolytic enzymes are used to fragment proteins. If the genomes of DNA tumor viruses could be dissected in this way, and if individually fragments of viral DNA could be isolated, one might be able to determine by chemical

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[255] Christen Brownlee, "Danna and Nathans: Restriction Enzymes and the Boon to Modern Molecular Biology," *Proceedings of the National Academy of Sciences of the United States of America* 102, no. 17 (2005): 5909.

[256] For an account of Smith's discovery and the subsequent outcomes see Hamilton O. Smith, "Nucleotide Sequence Specificity of Restriction Endonucleases," in *Les Prix Nobel*, ed. Jan Lindsten (Singapore: World Scientific Publishing Co., 1992).

[257] SV40 has a unique history. It was first discovered in 1960 in the cultures of rhesus monkey kidney cells that were being used to create the injected (Salk) and oral (Sabine) polio vaccines. It was found to have contaminated both vaccines produced between 1955 and early 1961. The lack of detection was in large part due to its relatively small size. After extensive study it was determined that the virus has the potential to cause tumors, but the virus typically causes a latent infections. It is called simian virus 40 because it was the fortieth simian virus identified.

mapping which segments of the genome were responsible for the various biological activities of the virus."[258]

Nathans, along with his colleague Kathleen Danna, applied Smith's restriction enzyme to map the genome of SV40 by breaking the virus into specific fragments where the sites of the cuts were known, and then separating those fragments in an electrophoresis gel. The resulting picture was similar, albeit much smaller, to those regularly seen on television police programs. Their work was first published the year after they began this ambitious research and quickly became a classic paper in molecular biology because it clearly laid out the experimental power of restriction enzymes as well as a potential method for mapping genomes.[259]

The race to discover more restriction enzymes and put them to experimental use commenced in the early 1970s, and many were quickly found and identified. Laboratories that specialized in find restriction enzymes, such as Richard Roberts' at Cold Spring Harbor, "gladly shared these enzymes with the academic community."[260] The free sharing of ideas, results, or specimens was a regular part of the molecular biology community. With the advent of recombinant DNA and the biotechnology field, however, this ideal quickly changed. Scientists like Roberts quickly converted his laboratory into a business as he helped found New England Biolabs in 1975, which he "started to make the

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[258] Daniel Nathans, "Restriction Endonucleases, Simian Virus 40, and the New Genetics," in *Les Prix Nobel*, ed. Jan Lindsten (Singapore: World Scientific Publishing Co., 1992).
[259] Kathleen Danna and Daniel Nathans, "Specific Cleavage of Simian Virus 40 DNA by Restriction Endonuclease of Hemophilus Influenzae," *Proceedings of the National Academy of Sciences of the United States of America* 68, no. 12 (1971). For an account of Nathans' work see: Nathans, "Restriction Endonucleases, Simian Virus 40, and the New Genetics." John Richards, *Recombinant DNA: Science, Ethics, and Politics* (New York, NY: Academic Press, 1978). Brownlee, "Danna and Nathans: Restriction Enzymes and the Boon to Modern Molecular Biology."
[260] Richards, *Recombinant DNA: Science, Ethics, and Politics*, 5907.

sale of [restriction] enzymes its major product line, and many companies soon followed suit."[261]

For their groundbreaking work in restriction enzymes, Werner Arber, Hamilton Smith, and Daniel Nathans were awarded equal shares of the Nobel Prize in Physiology or Medicine in 1978. Restriction enzymes served almost instantly as a turning point for the creation of recombinant DNA technology. The ability to cut larger strands of DNA into smaller fragments made experiments that were only theoretical a handful of years before into reality. As much as bacteria were analogous to higher forms of life, they were not identical. Thoughts quickly turned toward studying the inner workings of mammalian cells. In 1971, a highly respected biochemist at Stanford University leapt into the breach and designed the first recombinant DNA experiment.

The First Proposed Recombinant DNA Experiment

In 1971, Paul Berg was starting his third year as chairman of the esteemed Biochemistry Department in the Stanford University School of Medicine. It was a department that Berg helped found in 1959 with his mentor Arthur Kornberg.[262] Berg's early research was characterized by microbiological research into enzymes and the use of phages for experimentation on bacteria. This phase of his research was notable for his

[261] Ibid. New England Biolabs is still in business today and supplies a wide array of products, still specialized in restriction enzymes and other enzymes used in biotechnology research. New England Biolabs, "New England Biolabs," http://www.neb.com.

[262] Arthur Kornberg (1918-2007) was a biochemist primarily interested in enzymes and was awarded the Nobel Prize in Physiology or Medicine in 1959 for his 1956 discovery of DNA polymerase in *E. coli*. DNA polymerase is an enzyme that facilitates DNA replication by adding additional nucleotides to a growing strand of DNA.

collaboration with Kornberg on the synthesis of protein by *E. coli*, as well as his 1965 work creating chemical models to produce RNA and proteins *in vitro*.[263] For his research, Berg earned the title of California Scientist of the Year (1963) and election in the National Academy of Sciences (1966) and American Academy of Arts and Sciences (1967).[264] While at Stanford, however, his research interests began to follow the natural drift of the field away from microbiology and to more complicated mammalian cells.[265]

To make the conversion over to mammalian cell research, Berg spent a year on sabbatical studying SV40 at the Salk Institute in California with Renato Dulbecco who specialized in the polyoma mouse virus—a small, five-gene virus that causes cancer in mice.[266] It quickly occurred to Berg that tumor viruses might act on mammalian cells like bacteriophages did on bacteria and this could be a better way to understand the form and function of mammalian cells. Upon his return to Stanford in 1968, Berg began an experimental program to see if his theory was defensible. Berg would test if SV40 could act as a vector—a genetic vehicle that can transport and insert specific known genetic information into the genome of another cell. He used SV40 due to its size but more importantly because of its ability to insert itself into mammalian DNA.[267]

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[263] *In vitro* means literally "within the glass," and used to describe experiments that are performed in a controlled laboratory setting outside of the living organism or cell. The opposite of *in vitro* is *in vivo*, which means "within the living."
[264] Michael Rogers, *Biohazard*, 1st ed. (New York, NY: Knopf, 1977), 33.
[265] Paul Berg, "Autobiography," The Nobel Foundation, http://nobelprize.org/nobel_prizes/chemistry/laureates/1980/berg-autobio.html.
[266] He spent a year at the Salk Institute during 1967-68.
[267] Alan P. Lightman, *The Discoveries*, 1st ed. (New York, NY: Pantheon Books, 2005), 484-85.

After some early experiments convinced Berg that SV40 was too small to act as a vector,[268] the discovery and application of restriction enzymes by Nathans and others provided Berg with the means to potentially create his own viral vector using SV40. Berg's experimental solution was elegant, but extremely complicated and sophisticated. In 1971, he proposed to first cut open the circular SV40 bacterium using a restriction enzyme then use another enzyme (an exonuclease) to create new ends on the opened virus. Then, another enzyme would be used to place a new nucleotide sequence on each end of the virus. A similarly treated segment *lambda* bacteriophage would be attached to the two terminal ends of the SV40 fragment.[269] Finally, two enzymes essential and common to DNA replication would be added: DNA polymerase to fill in any missing nucleotides and DNA ligase (discovered only five years earlier) to permanently join DNA together into a large hybrid DNA ring that was one-half SV40 and one-half bacteriophage. This new hybrid molecule had the potential to be used as a dual vector by exploiting both the ability of the SV40 virus to insert itself into animal DNA and the *lambda* phage's ability to insert itself into the DNA of *E. coli*. Using SV40 as the vector, the DNA of the lambda phage DNA could be inserted into an animal cell. The corollary was also true. If *lambda* was used as the vector then the oncogenic SV40 DNA could be inserted into the DNA of *E. coli.* What Berg was proposing was a vector to create the first cross-species recombinant DNA molecule.[270]

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[268] A vector is a genetic vehicle that can transport and insert specific known genetic information into the genome of another cell.

[269] The lambda bacteriophage, discovered by Esther Lederberg in 1951, is a small virus that lyses the K12 strain of *E. coli*, and because of that it quickly became the most widely used bacteriophage in early molecular biology.

[270] Lightman, *The Discoveries*, 484-89, Clifford Grobstein, *A Double Image of the Double Helix: The Recombinant-DNA Debate* (San Francisco, CA: W. H. Freeman,

If successful, Berg's proposed experiment would use the mechanisms of SV40 to insert the recombinant DNA molecule into an *E. coli* so that it replicated every time the *E. coli* replicated. This proposed experiment was only known a few people within his lab. One of those who knew was Janet Mertz, a graduate student. Berg would soon send Mertz to an early summer three-week tumor virus workshop at Cold Spring Harbor. This was one year after Joshua Lederberg's testimony before the House Subcommittee of the Committee on Appropriations. The workshop at Cold Spring Harbor was where the first tremors of the coming recombinant DNA debates were felt.[271]

1979), 15-16, John Lear, *Recombinant DNA: The Untold Story* (New York, NY: Crown Publishers, 1978), 21-25, Nicholas Wade, *The Ultimate Experiment: Man-Made Evolution* (New York, NY: Walker, 1977), 32-34, Susan Wright, *Molecular Politics: Developing American and British Regulatory Policy for Genetic Engineering, 1972-1982* (Chicago, IL: University of Chicago Press, 1994), 72-75, Rogers, *Biohazard*, 35-38.

[271] Lederberg, "Biological Innovation and Genetic Intervention.", Joshua Lederberg, "Politics Nullifies Science in Environmental Studies," *The Washington Post*, 13 June 1970, John Osmudsen, "Scientists Find Genetic Root in a Hose of Diseases," *The Washington Post*, June 14 1970.

Chapter 5: "Well, now we can put together any DNAs we want to."

Over the course of three days in late January 1971, the US House of Representatives Committee on Science and Astronautics held its twelfth meeting of the Panel on Science and Technology. The theme of the Panel was "International Science Policy," and the topics for some of the sessions included "International Cooperation in the Environment," "New Mechanisms for Scientific Cooperation in the Future," "Legislative Role in Science Policy," and "Science, Technology and the Developing Countries." The Panel was not a legislative hearing, but a forum for free discussions "intended primarily to encourage the exchange of ideas and information between the world scientific community and Congress." As Chairman George P. Miller (D-CA) stated in the opening remarks, the Panel served as "most useful in defining how science and technology can be utilized in the solution of national and international problems."[272]

The keynote address, and first speaker of the Panel, was Secretary of State William P. Rogers, who addressed the Committee, its staff, and the twelve guest panelists that covered a wide range of scientific pursuits. In his short speech, Rogers provided an overview of the need for international cooperation in science and highlighted efforts of the Nixon administration to further this goal. Placing special emphasis on new environmental controls put forward by the administration, including the creation of the Environmental Protection Agency, his talk covered major advances in space, nuclear

[272] US Congress. House. Committee on Science and Astronautics, *Panel on Science and Technology, Twelfth Meeting: International Science Policy*, 92nd Cong., 1st sess., 1971, 1. Paul Rogers (1891-1982), was an elected Democratic Representative from Northern California from 1945-1972, and served as chairman of the Committee of Astronautics from the eighty-seventh through the ninety-second Congress.

power, non-proliferation treaties, protection of the oceans, and the successes of satellite and space technology. With the exception of the environment, Roger's talk focused on the traditional science programs that fell largely under the auspices of federal government control during at least the prior two decades. The omission of rapid advances in biomedical sciences and molecular biology, in particular, went completely unrecognized by the Secretary. Fortunately, the organizers of the Panel invited a biochemist with instant name recognition to the final untitled session. James D. Watson, Professor of Microbiology at Harvard University and Director of Cold Spring Harbor Laboratory, was awarded the Nobel Prize in 1962 for discovering the structure of DNA,[273] and was perhaps the most well known molecular biologist in the country.[274]

Watson, who followed world-renowned Jacques Cousteau, decided not to discuss the cutting edge breakthroughs in molecular biology, which had occurred in the previous two years including restriction enzymes, genetic medicine, and even the potential for the first recombinant DNA experiment. Watson instead chose to talk about recent work in human reproductive technology by way of "some experiments on the frog."[275] In particular, Watson talked about the first successful frog clone experiment by John Gurdon in 1958, which created a perfect clone by using cells from an adult donor.[276]

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[273] The discovery of the double helical structure of DNA was made by Watson and Francis Crick in 1952 [James D. Watson and Francis Crick, "Molecular Structure of Nucleic Acids," *Nature* 171, no. 4356 (1953).], and they, along with Maurice Wilkins, were awarded the Nobel Prize in Physiology or Medicine in 1962.
[274] *Panel on Science and Technology, Twelfth Meeting: International Science Policy*, 2-8.
[275] Ibid., 336. Watson abridged his testimony for an article for May edition of *The Atlantic Monthly* in 1971. James D. Watson, "Moving toward the Clonal Man: Is This What We Want?," *The Atlantic Monthly*, May 1971.
[276] M Fischberg, JB Gurdon, and TR Elsdale, "Nuclear Transplantation in *Xenopus Laevis*," *Nature* 181, no. 4606 (1958). Robert Briggs and Thomas King created the first successful frog clone in 1952, though they used cells from a frog embryo at the blastocyst

In his experiment, Gurdon removed the nucleus from an ovum of a *Xenopus laevis* frog[277] and then implanted the nucleus removed from an intestinal cell of an adult from of the same species.[278] The experiment settled a long running argument within the field of developmental biology about "whether the process of cell differentiation in vertebrates was primarily an event which occurred in the cytoplasm or the nucleus."[279] Gurdon's experiment confirmed that the nucleus from a differentiated cell was capable of directing the complete development of a frog. Gurdon's cloned frog, however, was not the primary subject of Watson's talk.

Watson used the cloned frog as a means to discuss potential human reproductive technologies and potential safeguards to protect both research and the public. He predicted that the cloning technology could radically change human reproduction, including *in vitro* fertilization, surrogate mothers, and, though discounting it likelihood in the very near future, human cloning.[280] In order to properly weigh the potential good versus bad experimental outcomes, Watson recommended safeguards, such as legislation, that should fit both national value systems and should be cognizant of the policies created in other countries. He reminded his audience that policies concerning cutting-edge biological advances, such as *in vitro* human embryo experimentation, should be

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phase and not adult cells. (Robert Briggs and Thomas King, "Transplantation of Living Nuclei from Blastula Cells into Enucleated Frogs' Eggs," *Proceedings of the National Academy of Sciences of the United States of America* 38 (1952).)

[277] The African clawed frog.

[278] This type of experiment is referred to as "clonal reproduction." The process of removing the nucleus from a cell is called "enucleation," and the process of moving the nucleus of one cell into the enucleated ovum is called "genetic transfer."

[279] *Panel on Science and Technology, Twelfth Meeting: International Science Policy*, 336-37.

[280] Watson first referred to human cloning as "science fiction" and then later referred to it as "most likely [to] appear on the earth with in the next 20 to 50 years, and conceivably even sooner, if some national actively promotes the venture." Ibid., 338, 43.

essentially international because "even if one or more countries stop such research, their action could effectively be neutralized by the response of a neighboring country." Watson concluded his testimony by proposing the formation of committees in the United States very similar to the Task Force for Genetics proposed the previous year by Joshua Lederberg.[281] For Watson, the organizing idea of these committees was to "know where [the science] is going, where experiments are being done, what experiments might be happening, and inform the public as a whole." Recognizing the need for greater social responsibility in science, Watson put forward a proposal where scientific decisions would not be just in the hands of the scientists or legislators, but also in the hands of the public. The main question that the public should consider was "do you want this [research] or not?" It was "a decision which the people as a whole must make."[282] Although this was a commonly held position for Watson and many scientists in the early 1970s, recombinant DNA would soon test both their willingness to actively advance their research while including the public in the decisions concerning technological advances. Recombinant DNA would also force scientists to test their own ability to judge the safety and acceptability of their own research.

The Debate Begins

In early June 1971, Janet Mertz came east from Stanford University to attend the annual three-week tumor virus workshop at Cold Spring Harbor Laboratory. The workshop on the north central coast of Long Island was usually a quiet affair filled with

[281] See Chapter 4 for more information on the proposed Task Force on Genetics.
[282] *Panel on Science and Technology, Twelfth Meeting: International Science Policy*, 344.

lectures, symposia, and workshops. The only controversy during the first two weeks of the conference occurred outside the confines of the white colonial buildings dotting the campus of Cold Spring Harbor.

On June 13, *The New York Times* published its first article about a leaked classified Defense Department report. The story ran within inches of the picture of a smiling and proud President Nixon walking Tricia, his eldest daughter, down the aisle at her wedding in the White House Rose Garden. The headline-grabbing article entitled "Vietnam Archive: Pentagon Study Traces 3 Decades of Growing U.S. Involvement" exposed a "massive study" completed by the Pentagon in 1968 analyzing American involvement in Southeast Asia from the Second World War until mid-1968. The study – officially entitled *United States-Vietnam Relations, 1945-1967: A Study Prepared by the Department of the Defense* and comprising 7,000 pages in 47 volumes – was leaked by its author, Daniel Ellsberg.[283] The initial publication of a small portion of the study, which would quickly become known as the Pentagon Papers, had public, legal, and political consequences. The Pentagon Papers provoked a negative public reaction to a war that had expanded to Cambodia the prior year and was actively referred to as a quagmire. The Nixon Administration immediately took the *Times* to court in an attempt to block any additional publication of the classified study. The lawsuit worked its way quickly to the Supreme Court of the United States, where the Nixon administration lost in a 6-3 decision. Politically, the leak of the Pentagon Papers marked the impetus behind the first use of the infamous plumbers when they broke into the office of the psychologist of

[283] Neil Sheehan, "Vietnam Archive: Pentagon Study Traces 3 Decades of Growing U.S. Involvement," *The New York Times*, 13 June 1971.

Ellsberg. At Cold Spring Harbor, a seemingly insignificant interruption by Mertz during a presentation acted as the spark that ignited the recombinant DNA debates. [284]

In the final week of the workshop, Janet Mertz attended a session on techniques of mammalian cell cultures taught by Robert Pollack, a young cancer researcher and instructor at Cold Spring Harbor. The session was Pollack's last of the workshop, and he decided to focus on a subject of great personal concern: safety and ethics in working with mammalian cancer cultures. As a cancer researcher the previous two years at the New York University of Medicine, Pollack became aware of current research in the field as well as all of the typical oncogenic model organisms, especially SV40. Shortly after arriving at Cold Spring Harbor, Pollack was concerned about the safety of ongoing experiments to replicate large quantities of the SV40 DNA, so Mertz's explanation of the proposed recombinant DNA experiment only exacerbated Pollack's anxieties about safety and ethics in the laboratory.[285]

For the thirty-year-old Pollack, safety and ethics were interrelated. He recalled that the session focused on "how to keep yourself from getting contaminated from doing this work – yes, literally, how to keep from getting sick. From the fear of that, I moved to the other question, whether there's a danger of doing something that might screw things up in a much larger way. I got a lot of arguments from some students, one girl in

[284] For a larger context of the publication of the Pentagon Papers see: Peter N. Carroll, *It Seemed Like Nothing Happened: America in the 1970s* (New Brunswick, NJ: Rutgers University Press, 1990), 99-100, Edward D. Berkowitz, *Something Happened: A Political and Cultural Overview of the Seventies* (New York, NY: Columbia University Press, 2006), 20-21, David Frum, *How We Got Here: The 70's, the Decade That Brought You Modern Life (for Better or Worse)* (New York, NY: Basic Books, 2000), 42-46.
[285] John Lear, *Recombinant DNA: The Untold Story* (New York, NY: Crown Publishers, 1978), 24.

particular, from Paul Berg's laboratory."[286] That girl was Mertz. She regularly interrupted Pollack's lecture in an attempt to discuss the proposed recombinant DNA experiment that she was part of in Paul Berg's Stanford laboratory. In his own words, Pollack "had a fit."

Pollack's reasoning for his "fit" was straightforward and simple: SV40 causes cancer in some animals, and SV40 could also infect humans. Inserting SV40 into *E. coli*, a bacterium that naturally lives in the human gut, provided the potential for the SV40 DNA to replicate every time the *E. coli* reproduced and potentially exposed the gut directly to SV40, which was a situation that never occurs in nature.[287] The reaction of directly exposing an oncogenic virus to the cells in the gut to was completely unknown and unpredictable. Further angering Pollack was the fact that Mertz viewed her experiment as simply an "academic exercise" without taking into account what might happen if the recombinant DNA experiment was successful and the newly created organism escaped from the lab. The discussion on the proposed Stanford experiment engulfed the session as others also criticized the experiment for its potential hazards.[288]

After the session ended, Pollack was on the horns of a dilemma: he could chose to stop pursing his concerns about Berg's proposed recombinant DNA experiment with his comments to Mertz, or he could act. Pollack chose the latter, and decided to call Berg directly and sidestep the social norms of the scientific community – junior scientists did

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[286] Horace Freeland Judson, "Fearful of Science," *Harper's Magazine*, June 1975, 72. This account by Pollack as it appears in Judson's Harper's article is reproduced, including verbatim quotes, in Nicholas Wade, *The Ultimate Experiment: Man-Made Evolution* (New York, NY: Walker, 1977), 33-34.
[287] Judson, "Fearful of Science," 72.
[288] Ibid.

not directly confront senior scientists about the work being were doing in their lab. When Pollack called Berg on June 28, the call was not a surprise to Berg.[289]

While Pollack deliberated, Mertz called and told her advisor that she had discussed the proposed experiments and had "received a rather severe criticism from a number of people." She called Berg "to relay this criticism." [290] Berg's initial reaction to what he heard from Mertz was "surprise," and then after he thought it over for a bit he found it "even more outrageous than my initial reaction."[291] Though there are slightly conflicting recollections of the exact conversation that took place during the phone call between Pollack and Berg,[292] Pollack convinced Berg to postpone the experiment until he considered the potential hazardous outcomes.

Soon thereafter, Berg began meeting and talking with scientists and colleagues about the safety of his proposed experiment. The people Berg consulted included David Baltimore, a young MIT specialist in ribonucleic acid (RNA) and future Nobel Prize winner; Maxine Singer, a contemporary of Berg's and a researcher at the National Institutes of Health; and her husband Daniel Singer, a lawyer specializing in scientific ethics.[293] Over the next six months, Berg considered the evidence and opinions from

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[289] Lear, *Recombinant DNA: The Untold Story*, 22-26.
[290] Interview with Paul Berg, Graham Chedd, Paula S. Apsell, and Time Life Video, *The Gene Engineers* (New York, NY: Time Life Video, 1977).
[291] Ibid.
[292] See: Lear, *Recombinant DNA: The Untold Story*, 25-26, Judson, "Fearful of Science," 72, Michael Rogers, *Biohazard*, 1st ed. (New York, NY: Knopf, 1977), 36-37, Wade, *The Ultimate Experiment: Man-Made Evolution*, 33-34, Clifford Grobstein, *A Double Image of the Double Helix: The Recombinant-DNA Debate* (San Francisco, CA: W. H. Freeman, 1979), 17, Films for the Humanities (Firm), *DNA. Playing God* (Princeton, NJ: Films for the Humanities & Sciences, 2004).
[293] Rogers, *Biohazard*, 37-38. See also Wade, *The Ultimate Experiment: Man-Made Evolution*, 34, Lear, *Recombinant DNA: The Untold Story*, 36-38.

colleagues and decided to cancel his experiment. In a 1977 interview, Berg explained his rationale for canceling the experiment.

> And although it was clear there was no hard data, which could establish that it would be risky, nevertheless, I couldn't convince myself that it was totally without any risk. And, since I felt that while I would be willing to take a risk of doing such an experiment if I was the only one to be exposed, I began to think in terms of whether it was my prerogative to, in fact, make that decision for other people who worked with me and around me. And, in fact, I finally decided that even if the probability was very low, it was still indeterminate, and we decided not to carry on this experiment any further.[294]

Although Berg canceled his experiment, which quelled the initial debates, the incident demonstrated the limitations on scientific debates concerning future recombinant DNA experiments. These early debates remained primarily within the molecular biology community because the complexity of the experiment that Berg was proposing was too difficult for many biologists let alone non-scientists to fully understand or carry out. The eventual decision by Berg to cancel the experiment was based almost entirely on the issue of experimental safety, a purely scientific reason. The initial round of debates also exemplified two characteristics that would continue to plague recombinant DNA: the debate rarely shifted away from the issue of laboratory safety, and because of its isolated nature, it could be easily perceived as only including a small cadre of academic elites who excluded the public and dissenting scientific views.

Even though Berg postponed his experiment, the scientists who advised and criticized the experiment largely remained involved in the early recombinant DNA debates, which provided a measure consistency to the debates through 1975. The

[294] Chedd, Apsell, and Video, *The Gene Engineers*.

scientists whom he contacted about his experiment formed the beginnings of a group that could provide their opinions on recombinant DNA if the need arose. Berg contacted many of them the following year. These scientists eventually became organizers, chaired committees, provided testimony, and helped steer the recombinant DNA debates towards a consensus in the scientific community. The most immediate result of the Pollack-Berg phone call, however, was the Biohazards in Biological Research Conference that Berg organized for January 1973.

Biohazards in Biological Research Conference, 22-24 January 1973

While planning the Biohazards in Biological Research Conference, Berg reached out to Pollack for assistance in organizing and running the conference. The two were joined by Alfred Hellman and Michael Oxman – scientists from the National Cancer Institute and Children's Hospital Medical Center, Harvard Medical School, respectively – as organizers of the conference.[295] The impetus for the conference was the "widespread interest and growing participation of many laboratories in the problems of animal cell biology and tumor viruses" causing a "growing need for consideration of potential health hazards." The goal of the conference, which would take place at the Asilomar Conference Center in Pacific Grove, California, was to more widely publicize within the biological community the "experience and knowledge" of potential hazards, both "imaginary and real." Though not explicitly oriented to address recombinant DNA, a topic not directly discussed, the problems addressed at the conference were directly

[295] The National Science Foundation, the National Cancer Institute, and the American Cancer Society funded the conference.

relevant to the new technology. For the organizers, this conference was to be the first in a series on biohazards on biological research.[296]

This exclusive gathering of approximately one hundred experts in cancer and virus research included James Watson and David Baltimore, but was closed to the press. (The official proceedings of the conference were published as a book later in the year.[297]) Sessions at the conference included papers on potential biohazards posed by biological agents in the lab, methods to eliminate risk, biohazards and modern research methodology, and physical containment practices for viruses, bacteria, and laboratory animals. The conference papers ran the gambit from oncogenic viruses that could be transmitted from animal to humans, such as SV40, to potentially dangerous non-cancer causing viruses, such as tuberculosis. The thrust of the presentations and discussions focused on safety issues of the primary experimenter, which was succinctly stated by Emmett Barkley of the National Cancer Institute in his presentation. “In descending order of importance, the success of biological safety and environmental control program is dependent on (1) the safety awareness and techniques of the investigator, (2) the availability and correct use of safety equipment, and (3) the design and operation” of the research facility.”[298] On a few occasions, the focus of the conference shifted away from the pure science and physical containment of viruses and bacteria to two topics that were tied to the growing sense of social responsibility within science and internal unease

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[296] Alfred Hellman, Michael. N. Oxman, and Robert Pollack, *Biohazards in Biological Research* (Cold Spring Harbor, NY: Cold Spring Harbor Laboratory, 1973), v.
[297] Ibid.
[298] W. Emmett Barkley, "Facilities and Equipment Available for Virus Containment," in *Biohazards in Biological Research*, ed. Alfred Hellman, Michael. N. Oxman, and Robert Pollack (Cold Spring Harbor, NY: Cold Spring Harbor Laboratory, 1973), 327.

within the molecular biology community: the hazardous nature posed by experiments to the laboratory workers, and the unsafe practices of younger scientists.[299]

The definition of laboratory workers was broad and included just about everyone except for the primary researcher. Graduate students, laboratory technicians, and even the janitorial staff were all considered laboratory workers with potential exposure to the viruses and bacteria that were being researched. As one scientist stated during a discussion on laboratory safety, "Technicians maybe more than the professional people no doubt have the most intimate exposures to [hazardous] agents. They're the ones who most often cut or inject themselves accidentally and have other exposure to accident that distribute the viruses into the environment."[300] Common concerns at the time ranged from the dangers of mouth pipetting to methods used by the janitorial staff to clean the laboratory.[301] Also, younger scientists and their laboratory workers were at an increased risk if they had not received the safety training in medical microbiological or epidemiological safety procedures that was a central part of the training senior scientists received in graduate school.[302]

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[299] Michael. N. Oxman, "Comments," in *Biohazards in Biological Research*, ed. Alfred Hellman, Michael. N. Oxman, and Robert Pollack (Cold Spring Harbor, NY: Cold Spring Harbor Laboratory, 1973), 347.

[300] Robert Huebner, National Cancer Institute, Hellman, Oxman, and Pollack, *Biohazards in Biological Research*, 215.

[301] Mouth pipetting. A pipette is a slender tapered glass tube used for measuring or transporting liquids. In a laboratory setting it is commonly graduated and used for accurately measuring small amounts of liquids. Mouth pipetting is using a pipette like a straw to draw up liquid into the tube. It was a method used until the 1970s because it regarded by some as a faster and more accurate way of drawing liquids into the pipette than using a rubber bulb. Beyond the danger of the liquid into the mouth of the researcher, mouth pipetting had reached its nadir due to advances in manual pipette technology, including the adjustable pipette and mechanical pipettes in the 1970s.

[302] For a proposed epidemiological study of laboratory worker safety see Robert W. Miller, "Comments," in *Biohazards in Biological Research*, ed. Alfred Hellman, Michael.

During a discussion on virus containment, Barkley pointed out, "Safety is not dependent alone on equipment and facilities but is primarily dependent on the attitude of the scientist. It is our responsibility to reduce laboratory exposures, to train our staff in relevant safety methods, and to assess the risk of potentially hazardous research materials."[303] A few scientists did not shy away from what they viewed as an obvious and troubling problem of modern microbiology: improper training of newly minted molecular biologists. As the field expanded quickly throughout the late 1960s and early 1970s, increasingly it appeared that students were not properly trained in standard laboratory safety in handling hazardous biological agents.

Some scientists, such as Michael Oxman, gently addressed this problem in his closing remarks at the conference, when he stated:

> Although the intelligent application of available knowledge should often permit and investigator to select the least hazardous biological system for study, or at least to maximally protect his personnel, this will only be possible if he is aware that his experiments many involved some biohazard. Unfortunately workers trained in areas other than microbiology often do not think in these terms. Thus a laboratory engaged in serological studies[304] of cancer patients may not be concerned with biohazards until someone develops serum hepatitis…[And reiterate] that all individuals planning research which may involve biohazards receive training in microbiology—not simply to learn techniques for handling potentially infectious materials, but also to learn to think in microbiological terms. I suspect, for example, that such training, together with plain old common sense, would cause many investigators to think twice before initiating experiments that might broad the host range of a virus.[305]

N. Oxman, and Robert Pollack (Cold Spring Harbor, NY: Cold Spring Harbor Laboratory, 1973), 211-13.

[303] Emmett Barkley, National Cancer Institute, Hellman, Oxman, and Pollack, *Biohazards in Biological Research*, 342.

[304] Serological studies are studies of the blood serum of patients, usually to identify antibodies to a viral or bacterial infection.

[305] Oxman, "Comments," 348-49.

Oxman did not identify one group responsible for the more reckless laboratory practices, though one of his colleagues did during the closing discussion of the conference.

Edwin Lennette of the California State Department of Public Health spoke directly about the on the safety practices of his younger colleagues. “Speaking from my own experience, we find that individuals graduating from college microbiology departments, may have an excellent foundation in biophysics, biochemistry and the genetics of microorganisms, but little or no perception of the hazards involved in handling pathogenic agents or how to protect themselves and innocent bystanders against such pathogens.” His remedy for this distressing situation was to train and retrain them “to the point where aseptic techniques and safety precautions become virtually reflect and are carried out without conscious attention being paid to them.” What these new microbiologists lacked most of all was the “exposure to medical microbiology” that used to be a required part of the training during the 1950s and early 1960s.[306]

Drawing from personal observation, Wallace Rowe of the National Institute of Allergy and Infectious Diseases put the entire issue of laboratory safety more bluntly in his final statement of the conference. “I think a very important concept is that much of microbiological safety consists simply of having good habits. For example, it would never cross the mind of a trained microbiologist to touch anything that a drop of virus had fallen on, even if it was the most harmless virus around. Lab workers should have the operating room mentality, that there are clean and dirty areas with clearly defined but constantly changing boundaries. There is no reason why this mental approach should be restricted to microbiology, but it should be the training and lifelong habits of every lab

[306] Hellman, Oxman, and Pollack, *Biohazards in Biological Research*, 353.

worker; it is just as easy to work using good sterile techniques as it is to use bad techniques." These concerns voiced by older scientists about the unsafe practices of their younger colleagues were at times indirect, but nevertheless a constant refrain in the majority of closing remarks of the conference.[307]

Paul Berg, as the organizer of the conference, had the final word, and largely offered three suggestions about what should happen after attendees returned to their laboratories. His first proposal was that epidemiological studies of laboratory personnel should be carried out in order to ascertain the magnitude of potential biological hazards. He also suggested that the National Cancer Institute publish a newsletter and that other information be regularly disseminated to keep scientists up to date on the most current containment procedures. Berg's final suggestion was that federal and private funding agencies that sponsor research involving potential biohazards should provide necessary funds to bring laboratories up to minimum standards of safety. Noticeably absent from Berg's final remarks were any direct mention of proper training for younger scientists or for scientists of any experience level to carefully review their experiments for potential risks, as he had with his proposed recombinant DNA experiment. During 1973, a new technique for creating recombinant DNA was announced, and concerns about laboratory safety only intensified.[308]

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[307] Ibid., 352.

[308] Paul Berg, "Closing Remarks," in *Biohazards in Biological Research*, ed. Alfred Hellman, Michael. N. Oxman, and Robert Pollack (Cold Spring Harbor, NY: Cold Spring Harbor Laboratory, 1973), 353-55.

A New Discovery…

One of the restriction enzymes Berg used in his laboratory and proposed using in his canceled recombinant DNA experiment was EcoR1 – the first restriction enzyme isolated from R strain of *E. coli* (EcoR1).[309] He received EcoR1 from Herbert Boyer, a professor of biochemistry and biophysics just up the coast at the University of California, San Francisco. Since the study of restrictions enzymes was still in its nascent phase in 1971, Berg asked Janet Mertz to examine EcoR1 and see if she could identify any new properties which might prove useful in his experiments with SV40. What Mertz discovered would soon change the way recombinant DNA experiments were conceived and carried out.

While experimenting with EcoR1 on SV40, Mertz noticed something completely unexpected. She found that the ends of the SV40 DNA cut by EcoR1 appeared to spontaneously rejoin to reform the original circular virus. Mertz wanted a closer look at what was going on, so she took her work to Roland Davis, an electron microscopist at Stanford. The process of preparing the cut DNA for electron microscopy involved cooling the DNA to the point where activity ceased. When Davis examined the different states of the cut DNA, he confirmed that the linear SV40 DNA was indeed restoring itself to its natural circular shape.

[309] The nomenclature for restriction enzymes is obtained from the first letter of the genus and the first two letters of the bacterium that the enzyme is isolated from. The fourth letter identifies the particular strain of the bacteria. The Roman number is used to identify the order in which the enzyme was discovered. Thus EcoR1 is the first restriction enzyme identified from the R strain of *E. coli*.

Davis also discovered that EcoR1 did not cut DNA leaving blunt ends[310] of DNA, but instead cut in a pattern resembling a "Z" with the horizontal line straightened.[311] The restriction enzyme cuts between two bases on the first strand of the DNA then cuts the bonds between the two strands, before cutting between two base pairs on the second strand. The straightened Z pattern created perfectly matched complementary terminal ends of the cut SV40 DNA. The creation of complementary ends allowed the ends to rejoin and allowed the virus to return to its original shape. Mertz and Davis then began experimenting with EcoR1 on DNA from other sources, and to their amazement they found that, regardless of the source of the DNA, EcoR1 created matching straightened Z pattern ends. These straightened Z pattern ends would quickly become know as "sticky" (or "cohesive") ends because the ends of any DNA segment when cut by the same restriction enzyme were always complementary to each other, and thus were attracted to spontaneously recombine. All it took to make these cohesive ends reattach permanently was the addition of a common enzyme (DNA ligase) to cement the bonds between the two strands of DNA. Mertz and Davis submitted their results to the *Proceedings of the National Academy of Sciences*. The results were published November 1972. The November issue also included Berg's article outlining the experimental procedure for the first proposed (and canceled) recombinant DNA experiment.[312]

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[310] Blunt ends are what remain on the terminal ends of DNA when a restriction enzyme cuts straight through both strands of DNA leaving no unbinded nucleotides. Think of the ends of a piece of string made up of two wound threads and cutting it with a pair of scissors. The straight clean cut ends are blunt ends.

[311] Another way to imagine the cut made by the EcoR1 is that it resembles the shape of a dual ended hex wrench that comes with IKEA furniture.

[312] Michel Morange, *A History of Molecular Biology* (Cambridge, MA: Harvard University Press, 1998), 188-89, Lear, *Recombinant DNA: The Untold Story*, 52-53, Janet E. Mertz and Ronald W. Davis, "Cleavage of DNA by R1 Restriction Endonuclease

…and a Novel Approach

In November 1972, in Honolulu, Hawaii, a chance encounter occurred between Herbert Boyer and Stanley Cohen, the latter a professor of medicine at Stanford University. Cohen's work focused on the growing number of antibiotic resistant bacteria, specifically on the genes on plasmids that conferred antibiotic resistance among bacteria. Cohen was at the Japanese-American Conference on Bacterial Plasmids to present his work on isolating the fragments of plasmids that he was researching to identify their genetic functions in antibiotic resistance. To create his DNA fragments, Cohen was still using a decades old standard of cutting his plasmids into fragments by using a blender.[313] Boyer was at the conference to discuss his work on restriction enzymes, and the two started to discuss how they might collaborate.[314]

Cohen and Boyer's work together started shortly after they returned to the continental US. They quickly yielded results. Instead of using the traditional blender method to mechanically cleave the plasmids, Cohen and Boyer decided to use EcoR1. The problem, however, was selecting a practical *E. coli* plasmid that was small enough to work with and had only one site where EcoR1 cleaved it. Cohen found such as plasmid,

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Generates Cohesive Ends," *Proceedings of the National Academy of Sciences of the United States of America* 69, no. 11 (1972), John F. Morrow and Paul Berg, "Cleavage of Simian Virus 40 DNA at a Unique Site by a Bacterial Restriction Enzyme," *Proceedings of the National Academy of Sciences of the United States of America* 69, no. 11 (1972).

[313] The most famous blender experiment was the Hershey-Chase experiment in 1952 where Alfred Hershey and Martha Chase used a Warring blender to break apart radioactively labeled bacteriophages from the *E. coli* they were infecting to prove that DNA, and not protein, was where genetic information was stored. For more detail on the Hershey-Chase experiment see,
Horace Freeland Judson, *The Eighth Day of Creation: Makers of the Revolution in Biology*, Expanded ed. (Plainview, NY: CSHL Press, 1996), 108-09, Morange, *A History of Molecular Biology*, 47-50.

[314] Lear, *Recombinant DNA: The Untold Story*, 59-66.

which he named pSC101 – p for plasmid and SC for Stanley Cohen). The plasmid contained only three basic functions making it perfect for the insertion of foreign genes. The first was that it contained a replication site, so that the plasmid was able to reproduce. The second was that the gene that conferred resistance to the antibiotic tetracycline was present. And finally, it had only one recognition site for EcoR1. As a potential cloning vehicle in a recombinant DNA experiment pSC101 was just what Boyer and Cohen were searching for.[315]

Fortunately, the next step of the recombinant DNA experiment was already solved. Three years prior, Cohen, Annie Chang, and Leslie Hsu discovered the method to extract and insert plasmids. The methodology was simple and took only four easy steps: take a broth filled with a standard strain of *E. coli* like K12 on a bed of ice, treat the broth with plasmids and a salt (calcium chloride), mix, then remove the mixture from the bed of ice and place it into a warm bath of water. The shock of the salt and change in temperature caused the bacterial cell membranes to become porous enough for the plasmids to slip into the cell. By using this technique, the small size of the pSC101 plasmid and its resistance to tetracycline made it the perfect vehicle to attempt recombinant DNA experiments. Its size made it more likely to slip through the membrane openings and the antibiotic resistance made it easy to test which cells absorbed the pSC101 plasmid – those bacteria that survived a treatment of tetracycline contained the pSC101 plasmid.

During the spring and early summer of 1973, Cohen and Boyer used all of the techniques at their disposal to create the first recombinant DNA molecules. They

[315] Wade, *The Ultimate Experiment: Man-Made Evolution*, 22-23.

carefully chose what genes they wanted to insert and decided initially not to cross the species boundary that is, they chose not to insert genes from any forms of life higher than viruses and bacteria. For their first three experiments, the pair chose genes from bacterial plasmids unrelated to the host bacteria. By cutting both the bacteria plasmid with EcoR1 and the pSC101, Boyer and Cohen were easily able to insert a gene into the opened pSC101 plasmid, and then insert the recombinant plasmid into *E. coli*. The plasmid then successfully replicated every time the host bacteria replicated. The most famous of the three experiments was the insertion of the gene from the bacterium *Staphylococcocus aureus* that conferred the resistance to penicillin. Boyer and Cohen knew they had successfully inserted the gene from *Staphylococcocus aureus* properly into the pSC101 plasmid when they found that *E. coli* successfully survived a treatment of both penicillin and tetracycline.

What differentiated the pSC101 plasmid method of creating recombinant DNA molecules from Berg's method was its simplicity. Although Berg's was scientifically elegant, it was also impractical for the majority of researchers to attempt because of the laboratory skill and sheer complexity required. In contrast, the pSC101 plasmid method was simple. The majority of biological and non-biological agents were easy to obtain and relatively simple to use, it could be carried out in a very rudimentary laboratory, and it required minimal scientific training. Like the proposed Berg experiment, however, the experimental work of Boyer and Cohen was done relatively quietly. Cohen attempted to quickly publish their results in their journal of choice, but the fastest turnaround for the

Proceedings of the National Academy of Sciences was November.[316] In mid-June, with three successful experiments under his belt, Boyer headed off to the prestigious, secluded, and by-invitation-only Gordon Research Conference on Nucleic Acids at the New Hampton School in New Hampshire.

Gordon Research Conference on Nucleic Acids, 11-16 June 1973

Founded in 1931, the Gordon Research Conference on Nucleic Acids was an influential annual conference dedicated to allowing scientists to openly discuss their current work, and that was no different in 1973 when Herbert Boyer attended. The Gordon Conference, however, differed from other conferences in that it historically attempted to remain purely scientific in nature, and actively encouraged attendees to refrain from discussing social or political issues. It was also traditionally a closed conference, where participants could not be quoted without permission from the organizers, and where no records were kept. The idea was to create an environment that would allow the freest flow of scientific information between the invited scientists. Prior to leaving for the conference, Boyer and Cohen agreed that Boyer was not going to discuss their work. [317]

Chaired by Maxine Singer, whom Paul Berg had consulted about his recombinant experiment two years prior, the 1973 conference was typical of previous years until the final morning. It was on the last morning that Boyer reversed course and chose to discuss the successful recombinant DNA experiments that he and Cohen had carried out at a

[316] Stanley N. Cohen et al., "Construction of Biologically Functional Bacterial Plasmids in Vitro," *Proceedings of the National Academy of Sciences of the United States of America* 70, no. 11 (1973).
[317] Lear, *Recombinant DNA: The Untold Story*, 68-69.

session chaired by Daniel Nathans,[318] entitled "Bacterial Restriction Enzymes and the Analysis of DNA."[319] The initial reaction to what Boyer said was oddly silent until another scientist, William Sugden, stated matter-of-factly, "Well, now we can put together any DNAs we want to."[320] Upon Sugden's statement, the real consequences of Boyer's experiment came quickly into focus for the other scientists in the room. As Singer later recalled, "It was that remark that everybody heard."[321] For the remainder of the day, the conference was abuzz with talk about the prospects for new avenues of experimentation and concerns about the safety of recombinant DNA experiments.[322]

Before the conference adjourned, Singer took an unprecedented step of calling for the remaining members of the conference to discuss the issues raised by recombinant DNA. Three proposals were put forward and a vote on them was announced for the remaining attendees. The first proposal was for the conference to send a letter voicing their concern to the National Academy of Sciences (NAS), as well as the Institute of Medicine (IOM). The second allowed for the letter to be signed by as many of the participants as wished to do so. The final allowed for participants to write individual letters to NAS and IOM.[323]

The first ballot was taken and the vote was overwhelming with 78-17 voting in favor of sending a letter to the NAS and IOM on behalf of the conference. After the first proposal passed, a second ballot was quickly taken to determine if the letter on behalf of

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[318] See Chapter 4 for a discussion of Nathans research on restriction enzymes.
[319] Lear, *Recombinant DNA: The Untold Story*, 70.
[320] Sugden statement has been quoted in multiple sources including Rogers, *Biohazard*, 42, Lear, *Recombinant DNA: The Untold Story*, 70.
[321] Rogers, *Biohazard*, 42.
[322] Lear, *Recombinant DNA: The Untold Story*, 70-71.
[323] Ibid., 72.

the conference should be published in *Science*. The vote again was in the affirmative, though it was much closer than the prior vote, 48-42. Since the vote was close and almost a third of the attendees had left the conference before the final day, Singer decided to send every participant a letter and ballot in order to ensure the conference participants were in general agreement before proceeding with sending the letter to the NAS and IOM or for potential publication. Only sixty-one of the possible one hundred and forty two ballots were returned, but of those forty voted to send the letter to the Academy and Institute, as well as to *Science*. [324]

The mailing sent to attendees with the ballots and the recommendation to make suggestions to the draft conference letter. Exactly a month and day after the end of the Gordon Conference, Maxine Singer with her co-chair Dieter Söll (Yale University) sent the official conference letter to Philip Hander, president of NAS, and John Hogness, president of the IOM.[325] The letter was written to "communicate a matter of deep concern" on behalf of the Gordon Conference scientists, and would become known as the Singer- Söll letter.[326]

The single page Singer-Söll letter briefly outlined the advancements that have created the situation where it was possible "to combine DNA from animal viruses with bacterial DNA or DNAs of different viral origins might be so joined…with biological activity of unpredictable nature." It stressed that experiments with these new hybrids held

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[324] Ibid., 72-73.
[325] For a discussion of the suggestions to the draft letter see Sheldon Krimsky, *Genetic Alchemy: The Social History of the Recombinant DNA Controversy*, Third Printing, 1985 ed. (Cambridge, MA: MIT Press, 1982), 75-78.
[326] "Letter from Maxine Singer and Dieter Soll to Philip Handler, 17 July 1973," (ASSEM: Life Sciences: Com on Synthetic Nucleic Acids: Ad hoc Proposed, 1973, Central Policy Files, NAA).

"exciting and interesting potential both for advancing knowledge of fundamental biological processes and for alleviation of human problems." But it also warned because the products of such experiments "may prove hazardous to laboratory workers and to the public…prudence suggests that the potential hazard be seriously considered." The lone recommendation of the letter was that the "Academies establish a study committee to consider this problem and to recommend specific actions or guidelines should that seem appropriate." Tacked on to the recommendation, and recognizing safety concerns like those voiced by Robert Pollack, was the statement: "Related problems such as the risks involved in current large-scale preparations of animal viruses might also be considered." The short letter contained an attachment listing the participants at the Gordon Conference.[327]

The attendees' vote for the Singer-Söll letter at the Gordon Conference demonstrated a new degree of social responsibility that ran counter to the insular nature of the decades old conference. The social responsibility of the scientists at the Gordon Conference, however, was limited as they kept their concerns within the traditional institutions of mainstream science.[328] They gave no serious consideration at the conference about recombinant DNA becoming part of a larger dialogue with the public. The letter on behalf of the conference was sent to the presidents of the NAS and IOM in mid-July, where the initial debate about the safety of recombinant DNA could remain an

[327] Ibid.
[328] For a discussion of institutional structures in science see Kelly Moore and Nicole Hala, "Organizing Identity: The Creation of *Science for the People*," in *Social Structure and Organizations Revisited*, ed. Michael Lounsbury and Marc J. Ventresca (Boston, MA: JAI, 2002).

internal debate among scientists. The only access the public would have was the publication of the letter in the "Letters" section of *Science* three months later.[329]

[329] Maxine Singer and Dieter Soll, "Guidelines for DNA Hybrid Molecules," *Science* 181, no. 4105 (1973).

Chapter 6: "A green, yellow or red light on these experiments"

Boyer's enthusiasm about plasmid recombinant DNA experiments and the Gordon Conference letter had put his partner Stanley Cohen in an awkward position. Soon after the Conference, requests began to flood into Cohen's laboratory for the pSC101 plasmid. Some of the experiments proposed using pSC101 to combine genes from viruses and bacteria more dangerous than Paul Berg's initial recombinant DNA experiment. In the early 1970s, sharing of biological material was the norm, whether it was for a particular strain of bacteria, bacteriophage, or restriction enzyme, like EcoR1. These requests for pSC101 put Cohen in a difficult position: he wanted recombinant DNA experiments to go forward, but he was also conscious of the potential dangers inherent in of some of the experiments for which pSC101 was requested. Cohen initially chose a middle ground strategy. He filled a few early requests and included a warning note about potential careless experimentation using pSC101. Because there were no guidelines for the safe use of his plasmid and the very real potential for dangerous recombinant DNA experiments, he soon decided to completely shut off access to pSC101 and refused all other requests, including one from Berg. With his action, Cohen essentially shut down the majority of recombinant DNA experiments until the NAS completed their review of the issues outlined in the Singer-Söll letter.[330]

[330] Nicholas Wade, *The Ultimate Experiment: Man-Made Evolution* (New York, NY: Walker, 1977), 36, John Lear, *Recombinant DNA: The Untold Story* (New York, NY: Crown Publishers, 1978), 83, Michael Rogers, *Biohazard*, 1st ed. (New York, NY: Knopf, 1977), 43-44.

The Singer-Söll Letter and the National Academy of Sciences' Response

The publication of the Singer-Söll letter in *Science* in late September 1973 marked the first public acknowledgement of the recombinant DNA debates. The letter, however, arrived on the desks of Philip Handler and John Hogness at the National Academy of Sciences in Washington, DC over two months earlier on July 19. This letter is traditionally referred to as the first document of the debates, and it marks the beginning of the first organizational attempt by the NAS to resolve concern about the safety of recombinant DNA. The structure of the future debates both within the scientific community and in the public was framed by how the NAS handled the concern expressed by the Gordon Conference about recombinant DNA.

It was a safe choice for co-chairs of the Gordon Conference to send the letter to the NAS. The NAS was a century-old institution based in Washington, DC dedicated to "investigate, examine, experiment, and report upon any subject of science" when called on by any department of the federal government.[331] To maintain the highest scientific standards, membership of the Academy was highly selective with current members electing new members. By the early twentieth-century, the NAS could no longer provide for all the needs of the federal government, especially at a time of war. In 1916, President Woodrow Wilson created the National Research Council (NRC) to increase scientific and technical output for First World War. Due to its success during the war, President Wilson issued and executive order that made the NRC a permanent entity of the NAS on May 11,

[331] "National Academy of Sciences: Act of Incorporation," (1863).

1918.[332] The NRC quickly became the locus for carrying out studies requested by the NAS and later the Academy of Engineering (1964) and Institute of Medicine (1970).

Thus the NAS seemed like the appropriate institution to examine the issues associated with, and provide recommendations for a problem as complex as recombinant DNA. In 1969, Philip Handler was elected as president of the Academy, and shortly after taking office he started moving the organization toward public needs and away from an insular science-centric direction. Handler, a Duke University biochemistry professor, created public-oriented programs such as the Academy Forum, where scientists and the public could openly debate an interesting or controversial scientific issue. Recombinant DNA, however, was not initially chosen as a Forum topic, and instead began moving through the bureaucracy of the Academy.[333]

On July 20, 1973, Handler responded to both Singer and Söll in a three-sentence letter. Handler deemed the "problem" posed by the Singer-Söll letter as having "scientifically interesting and socially serious implications." However, the issue would not be dealt with immediately, but would be "raised with our [National Research] Council next month." Hogness did not reply to the letter until August 8, and also questioned the choice of the National Research Council (NRC) because "it is the group within the Academy that deals with technical scientific issues, whereas the Institute of Medicine deals with broader issues of national health policy." Although for Hogness

[332] President Wilson's Executive Order No. 2859
[333] Lear, *Recombinant DNA: The Untold Story*, 77-78.

recombinant DNA was "a very important [issue] and deserves careful monitoring," it was solely a technical and safety issue to be handled by scientists in the NRC.[334]

Despite the arrival of the Singer-Söll letter, recombinant DNA proved not to be a top priority at any level of the NAS. At the NRC meeting in late August, Handler recommended that the recombinant DNA issue be referred to the Assembly of Life Sciences (ALS) – a newly formed division of the NRC that was created on July 1, 1973.[335] After the Council meeting, Handler sent a memo with the Singer-Söll letter attached to the members of the Executive Committee of the ALS requesting "your thoughts on [Singer and Söll's] suggestions."[336] Paul Marks responded quickly on behalf of the ALS Executive Committee. Writing on August 30, he agreed with the Singer-Söll letter that the ALS "should consider establishing a study committee."[337] Marks concluded his letter by stating that he "was as concerned with the potential hazards of certain of the hybrid molecules now being studied as I am with the potential of unreasonably gloomy predictions as to these hazards."[338]

Before the Executive Committee could meet again, *Science* published the Singer-Söll letter in "Letters" section of the September 21 issue. The editorial of the issue was

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[334] Philip Handler quote from: "Philip Hander to Maxine Singer and Dieter Soll, 20 July 1973," (ASSEM: Life Sciences: Com on Synthetic Nucleic Acids: Ad hoc Proposed, 1973; Central Policy Files; NAA). John Hogness quotes from: "John R. Hogness to Maxine Singer and Dieter Soll, 8 August 1973," (ASSEM: Life Sciences: Com on Synthetic Nucleic Acids: Ad hoc Proposed, 1973; Central Policy Files; NAA).
[335] Assembly of Life Sciences, "Report - Assembly of Life Sciences, National Research Council, 1973-1975," (Washington, DC: National Academy of Sciences), 6.
[336] "Philip Handler to Dr. James Ebert, Dr. Leonard Laster, and Dr. Paul Marks, 27 August 1973," (ASSEM: Life Sciences: Com on Synthetic Nucleic Acids: Ad hoc Proposed, 1973; Central Policy Files; NAA).
[337] "Paul A. Marks to Philip Handler, 30 August 1973," (ASSEM: Life Sciences: Com on Synthetic Nucleic Acids: Ad hoc Proposed, 1973; Central Policy Files; NAA). Paul Marks was the Vice President for Health Sciences at Columbia University.
[338] Ibid.

written by Amitai Etzioni, and highlighted a recent Louis Harris poll that showed a modest upswing in the confidence in science among the American public. Science, in fact, finished third in the survey, behind only medicine and finance in the sixteen institutional areas. Those older than fifty and those in the rural South had the lowest confidence in science. Politically, those who intended to vote for Democratic presidential nominee George McGovern in 1972 were less confident in science than those who supported President Nixon by a margin of 33 to 41 percent. Etzioni concluded optimistically that "While obviously the work and values of science must be carried to many Americans, a job to which the [Association for the Advancement of Science] has been devoting increasingly more effort, there is certainly no reason to despair of public support or to believe that a greater recognition of the merits of science cannot be regained."[339]

The first letter to the editor in *Science* that week, however, was not as sanguine about science. The text of the Singer-Söll letter was identical to the letters sent to the NAS in July, but also included an introductory statement from the authors. The statement read that the letter had been sent to Handler and Hogness and that "a majority [of Gordon Conference attendees] also desired to publish the letter more widely." Despite Philip Abelson, editor of *Science*, remarking, "Do you really want to do this?" when Singer submitted the letter for publication, the letter did not cause much a public stir. [340] No major newspapers reported on the publication of the letter, and *Science* published one

[339] Amitai Etzioni and Clyde Z. Nunn, "Public Views of Scientists," *Science* 181, no. 4105 (1973). Amitai Etzioni was a professor of Sociology at Columbia University and Director of the Center for Policy Research, Inc. *Science* is weekly peer-reviewed science journal published by the Association for the Advancement of Science.
[340] Abelson's quote appears in by both Rogers and Lear. [Rogers, *Biohazard*, 43, Lear, *Recombinant DNA: The Untold Story*, 74.]

article that wrapped up the year in microbiology including the Berg proposed experiment.[341] How to proceed with recombinant DNA remained safely within the machinations of the NAS.

Formation of a study committee

Four days after the publication of the Singer-Söll letter in *Science*, Maxine Singer appeared before the ALS Executive Committee to discuss the recommendations made in the letter. At that meeting, Singer recommended Paul Berg to head a study committee to consider recombinant DNA technology and produce recommendations for the ALS. Singer's recommendation of Berg made sense on three levels. First, he was intimately involved in science of recombinant DNA technology. Second, he had already weighed the potential benefits and risks of the technology when deciding whether or not to carry out his proposed recombinant DNA experiment in 1971. And third, he had just organized and chaired a conference on safety and biohazards in 1973.[342] Singer, in a letter to Leonard Laster of the Executive Committee following her meeting, thanked them for "prompt and serious response" and was "satisfied with their decision to look further into the matter."[343] The summary of the September 25 ALS meeting stated that they "will begin an effort to establish whether it is appropriate to initiate a project to study the possible risks associated with experimental combinations of DNA's [sic], and related

[341] Nicholas Wade, "Microbiology: Hazardous Profession Faces New Uncertainties," *Science* 182, no. 4112 (1973).
[342] The official title of the Conference was Biohazards in Biological Research, which was held 22-24 January 1973. For more information on the conference see Chapter 5.
[343] "Maxine Singer to Leonard Laster, 28 September 1973," (ASSEM: Life Sciences: Com on Synthetic Nucleic Acids: Ad hoc Proposed, 1973; Central Policy Files; NAA).

more general problems." Laster was chosen to contact Berg about "an ad hoc group to consider this subject."[344]

Between the late September ALS meeting and their next meeting in mid-November, the NAS took very little action following through on the recommendations from their September meeting. In late September, Laster contacted Berg about heading up the proposed committee on recombinant DNA and got a lukewarm answer from the Stanford professor.[345] Still a bit bruised over his own experience with his proposed experiment and having just finished organizing a biohazards conference, Berg agreed to talk to some of his colleagues and learn more about the issues surrounding recombinant DNA.[346]

In November, the Cohen-Boyer journal article on their successful recombinant DNA experiment using pSC101 was finally published in the *Proceedings of the National Academy of Sciences*. The ALS also had a mid-month meeting where recombinant DNA was a topic, and for the first time details and goals of the study committee were discussed. It was concluded that a "group will be formed to assess hazards [concerning recombinant DNA experiments], recommend Assembly of Life Sciences action and develop alternatives for appropriate publicizing of the issues that arise."[347] Also during

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[344] "Albert Vosburg to James Ebert and Leonard Laster, Re: Highlights of Decisions and Assignment of the September 24-25 Executive Committee Meeting, 19 October 1973," (ASSEM: Life Sciences: Com on Synthetic Nucleic Acids: Ad hoc Proposed, 1973; Central Policy Files; NAA).
[345] Ibid.
[346] Lear, *Recombinant DNA: The Untold Story*, 79.
[347] "Summary of Minutes of the Meeting of the Executive Committee of the Assembly of Life Sciences Held 12-13 November 1973, 13 November 1973," (ASSEM: Life Sciences: Com on Synthetic Nucleic Acids: Ad hoc Proposed, 1973; Central Policy Files; NAA).

the month, Berg flew east to begin to discuss recombinant DNA concerns with scientists, including James Watson.[348]

The holiday season of 1973 was a quiet time for NAS action on recombinant DNA. Once the new year rolled around, however, the ALS again stirred into action. On January 2, 1974, Paul Berg contacted Laster to update him on his decision about chairing the study committee, first by phone and then in writing. The letter Berg wrote to Laster demonstrated just how difficult a situation it was for scientists to pass judgments on the safety on their own work, in this case recombinant DNA.[349]

In his letter, Berg agreed to organize a single-day meeting in late March or early April comprised of a group of less than ten individuals on the east coast "for reviewing any potential dangers (as well as benefits) stemming from our ability to generate a wide variety of hybrid DNA molecules." He laid out a simple agenda for the study committee of examining the potential risks of the experiment in order to reach a judgment if they posed a "serious health risk." Berg's judgment exemplified the problems and contradictions of recombinant DNA.[350]

For Berg, if the committee concluded that serious risks were "imminent," then they would "recommend to the [National Research] Council several courses of action." If, however, there was a non-imminent risk, Berg proposed a course for discussions by scientists working with recombinant DNA sponsored by the NAS. As for what the study committee could actually accomplish, Berg was not optimistic. "Frankly, I, myself, cannot be sure of what can come of all this except to alert people to the problem and its

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[348] Rogers, *Biohazard*, 43, Lear, *Recombinant DNA: The Untold Story*, 78.
[349] "Paul Berg to Leonard Laster, 2 January 1974," (ASSEM: Life Sciences: 1974: Com on rDNA Molecules: Ad hoc; Central Policy Files; NAA).
[350] Ibid.

consequences." He saw the issue of "control and supervision" of recombinant DNA as "fraught with its own dangers," and, echoing Watson's January 1971 testimony, acknowledged that international agreement might be needed, but worried that might not be feasible. His letter to Laster ended by wondering if they were making a mountain out of a molehill. "[I]t might be best to have some serious thought and discussion as to whether there really is a problem."[351]

Berg spent the winter of 1974 and early spring in his Stanford laboratory and communicated occasionally with some scientists about the proposed NAS recombinant DNA committee study committee. Richard Roblin, professor of microbiology and molecular genetics at Harvard Medical School, contacted Berg at Maxine Singer's suggestion because he was preparing for a summer Symposium on Genetic Manipulation.[352] Berg had originally met Roblin in Renato Deulbecco's laboratory at the Salk Institute in the late 1960s, so when Roblin wrote to him in March for information on recombinant DNA Berg quickly responded. Berg replied that he had been in contact with Laster, but no definite plans had been agreed upon to meet. On March 20, slightly nine months after the Singer-Söll letter arrived at the NAS, Roblin wrote to Laster directly asked, "Can you tell me whether the committee has been formally constituted yet, which questions it plans to consider, and who is on the committee?"[353]

The directions from ALS for the study committee were vague at best. In early April, the only clear instructions Berg had were to determine whether the study committee "would take the lead in deciding whether the NRC could contribute

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[351] Ibid.
[352] Lear, *Recombinant DNA: The Untold Story*, 79.
[353] "Richard Roblin to Leonard Laster, 20 March 1974," (ASSEM: Life Sciences: 1974: Com on rDNA Molecules: Ad hoc; Central Policy Files; NAA).

meaningfully to the problems" posed by the Singer-Söll letter and then to "file a report to the Executive Committee of the Assembly."[354] As Berg recalled,

> "Remember, we were not on a mission to do what we did. I had been asked…to advise the academy on how to respond to the concerns that had been voiced at the Gordon conference. It was a very limited request. I just took it on myself to say that I wasn't going to advise the president of the academy on my own. And what I would do is try to bring some people together that I thought would be thoughtful and so something about it."[355]

The ALS never officially recognized the study committee, and therefore if the committee wished to make any of its recommendations public, it could only do so as a group of individuals.[356] The NAS hoped that a non-sanctioned voluntary small group of scientists selected by Paul Berg would consider whether the NAS should take action on the issues raised by the Singer-Söll letter, and if so what possible actions could they take.[357]

After discussing the logistics of the first committee meeting with Roblin, Berg set the date of April 17 at the Massachusetts Institute of Technology (MIT). The conversation with Roblin also included a discussion about whom to invite. Roblin suggested two of his non-traditional science colleagues, who he had regular interactions with, to provide a broader range of opinions. He recommended Leon Kass, a bioethicist

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[354] "Leonard Laster to Philip Hander, 5 April 1974," (ASSEM: Life Sciences: 1974: Com on rDNA Molecules: Ad hoc; Central Policy Files; NAA).
[355] Paul Berg, *A Stanford Professor's Career in Biochemistry, Science Politics, and the Biotechnology Industry*, An Oral History Conducted by Sally Smith Hughes (Regional Oral History Office, The Bancroft Library, University of California, Berkeley: 1997), 120-21.
[356] "Paul Berg to Leonard Laster, 2 January 1974.", "Richard Roblin to Leonard Laster, 20 March 1974.", "F. Herbert Bormann to James Ebert, Henry Kaplan, and Ray Owen, 28 May 1974.," (ASSEM: Life Sciences: 1974: Com on rDNA Molecules: Ad hoc; Central Policy Files; NAA).
[357] "Minutes: National Research Council Governing Board, 8 June 1974," (ASSEM: Life Sciences: 1974: Com on rDNA Molecules: Ad hoc; Central Policy Files; NAA).

and founder of Institute of Society, Ethics, and Life Sciences at Hastings-on-the-Hudson, and Jonathan Beckwith of Harvard Medical School and Science of the People.[358] Despite the recommendations of Roblin, Berg invited scientists with similar academic backgrounds and affiliations, as well as familiarity with recombinant DNA research and current debates on biohazards. The final study committee consisted of David Baltimore (MIT), Daniel Nathans (Johns Hopkins University), James Watson (Cold Spring Harbor Laboratory), Norton Zinder (Rockefeller University), Herman Lewis (National Science Foundation), Sherman Weissman (Yale University), and Roblin.[359] Berg invited Maxine Singer because of their discussions surrounding his initial recombinant DNA experiment, but she was unable to attend the MIT meeting. Six of the eight members of the group attended the Biohazards in Biological Research Conference in 1973. In a recent interview, Berg explained the reason for his selections. "I had invited primarily people who had been involved in [the Biohazards in Biological Research Conference], who had some experience in thinking about the risks of this kind of experimentation."[360]

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[358] Lear, *Recombinant DNA: The Untold Story*, 80. Roblin knew Kass through his work at the Institute of Society, Ethics, and Life Sciences at Hastings-on-the-Hudson. Roblin knew Beckwith through their work at Harvard Medical School and their work together with Science for the People. Roblin sympathized with and was active with SftP, but not to the extent of Beckwith or some of the other leaders in Cambridge. The Institute of Society, Ethics, and Life Sciences was later renamed the Hastings Center.

[359] Three members of the study committee were already members of the NAS (Berg, Watson, and Zinder), and two would be elected within the decade (Baltimore (1974) and Nathans (1979).

[360] Berg, *A Stanford Professor's Career in Biochemistry, Science Politics, and the Biotechnology Industry*, 120.

The Study Committee, Cambridge, MA, 17 April 1974

The study committee met at MIT on April 17, 1974, and within the first hour, they agreed on their first recommendation to the Assembly of Life Sciences. The group members soon recognized the enormity of the problem in contrast to their limited size and amount of time of their meeting. The group quickly agreed to Watson's suggestion for an international conference to be held in 1975. They found this necessary in order bring together scientists from around to world "to consider these problems from many many different points of view and to decide what should be done in the future."[361] The group's conversation followed a predictable trajectory. They began their discussion by considering the safety of experiments similar to what Berg proposed and that Cohen and Boyer carried out. It quickly changed, however, when Berg mentioned an experiment that John Morrow, one of his students, had recently carried out with Cohen and Boyer. The new experiment was the successful insertion and replication of functioning genes from an African clawed frog (*Xenopus laevis*) into *E. coli* using the pSC101 plasmid method. The experiment was shocking to the other scientists in two ways. The first was that the experiment was successful. The frog genes reproduced perfectly along with the plasmid. And second, the experiment proved that it was possible to cross the species divide with recombinant DNA technology, in this case between animals and bacteria. As one scientist would later recall, this breakthrough enabled researchers to "put duck and orange DNA together – with a probability of *one*."[362]

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[361] David Baltimore quoted from The National Academy of Sciences, *International Conference on Recombinant DNA Molecules, February 1975, Asilomar Conference, Reel 1 Side 1* (NAA, 1975), Reel-to-Reel.
[362] Sydney Brenner quoted in Horace Freeland Judson, "Fearful of Science," *Harper's Magazine*, June 1975, 71.

The committee was most concerned that potentially more hazardous recombinant DNA experiments "were going to be carried out in a very short period of time," such as the proposals sent to Cohen or the experiment carried out by Morrow.[363] These experiments would be carried out without any general guidelines or special safeguards. The committee "therefore felt it necessary to design some sort of strategy to defer certain types of experiments until consideration could be given to their safety." These deferred experiments were the most surprising aspect of the recommendations of the study committee to the ALS.

The study committee reported back to ALS with three recommendations: (1) announce an international conference; (2) urge the director of the National Cancer Institute to undertake an experimental research plan to study the effects of naked oncogenic DNA to determine its risk; and (3) that the signatories of the group not undertake any recombinant DNA experimentation that had the potential to spread cancer or increase antibiotic resistance. [364] The first point was important because recombinant DNA experiments were potentially going to go forward in other countries, and there was a clear need for an international understanding about how to safely carry out recombinant DNA research. The second and third points were the same concerns that Berg encountered with his proposed experiment, that were address at the 1973 biohazard conference, and criticisms that Cohen and Boyer heard following the public announcement of their successful experiments in 1973.

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[363] David Baltimore quoted from The National Academy of Sciences, *International Conference on Recombinant DNA Molecules, February 1975, Asilomar Conference, Reel 1 Side 1*.
[364] Lear, *Recombinant DNA: The Untold Story*, 82-85.

The third recommendation, however, was essentially declaring that a large number of recombinant DNA experiments should be halted until their risks were assessed, and that members of the study committee were declaring a self-imposed moratorium on most of their own recombinant DNA research. The committee broke the experiments that they were deferring until the experiments were deemed safe to perform into two types. The first type (Type I) were experiments that created autonomously replicating plasmid that had the potential to increase antibiotic resistance or introduce toxin formation into bacteria. The second type (Type II) were introduction of DNA from oncogenic or other animal viruses into bacteria. The committee was also concerned about the recombinant DNA experiments that were crossing the species barrier. Without halting such experiments, the committee concluded that they "should not be undertaken lightly in the sense that one should understand that one is reaching into the unknown without a very good grasp on what might happen."[365] The initial draft of the committee's letter went through some revisions between the original members of the study committee. Additional members were added to the committee to provide expertise in areas that were absent from the April 17 meeting, such as Cohen providing information on plasmid based recombination experiments.

May 1974

May 1974 marked a turning point in how the NAS dealt with recombinant DNA. That month's issue of the *Proceedings of the National Academy of Sciences* included the

[365] David Baltimore quoted from The National Academy of Sciences, *International Conference on Recombinant DNA Molecules, February 1975, Asilomar Conference, Reel 1 Side 1*.

article detailing Morrow, Cohen, and Boyer's successful insertion of frog DNA into *E.coli*. It also marked the first time that the ALS Executive Committee demonstrated that they understood the importance of dealing with the issues surrounding recombinant DNA. On May 24, David Baltimore arrived in Washington, DC to deliver the letter of recommendations of the study committee to the ALS Executive Committee. During the meeting with the Committee, Baltimore said that the study committee wanted to submit their letter for publication. The meeting bore immediate results. The following Monday, Herbert Bormann, a new member of the Executive Committee, wrote a memorandum to the other members of the committee summarizing his impressions about recombinant DNA after the meet with Baltimore. Bormann acknowledged that study committee's recommendations were "ground breaking," and recognized that the issues were far too complex for the study committee to act alone. If their recommendations were going to be meaningful, the study committee would need official NAS support. He further urged the ALS to "not fritter away" the "lead time to develop policy."[366]

Ten months after the Singer-Söll letter arrived at the NAS, Bormann proposed three actions in order not to expedite the process. The first was for Handler to issue an official NAS statement simultaneously with the publication of the study committee letter. The second was for the ALS to help organize an international conference. The third was for the NAS to recognize the need to communicate this issue with the public. The final action was to avoid recombinant DNA becoming an issue that "could result in still further diminishment of the scientific establishment" in the eyes of the public.[367]

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[366] "F. Herbert Bormann to James Ebert, Henry Kaplan, and Ray Owen, 28 May 1974.." Herbert Bormann was a professor of forest ecology at Yale University.
[367] Ibid.

The next day Leonard Laster wrote to Baltimore with news of massive changes to the study committee. What had been a volunteer group with little instruction from any branch of the NAS was about to become an official NAS committee responsible for recombinant DNA. Handler decided that "it would be a strange" for the NRC to respond to the Singer-Söll letter by stating that "several of their peers had been consulted and that they were writing a private letter for publication in journals." The simple solution was to make the study committee into an official ad hoc committee under the aegis of the ALS. Given the official name the Ad hoc Committee on Synthetic Nucleic Acids, the committee was now officially writing a response to the Singer-Söll letter on behalf of the National Research Council. The first step in providing the official response was to convert the letter submitted by the study committee into a report. Handler then proposed that the report, when completed, would be submitted for simultaneous publication in journals *Science*, *Nature*, and the *Proceedings of the National Academy of Sciences*. He also provided additions and suggestions to help with the conversion of the letter into a report. The biggest initial change made by Handler was the addition of language to include potential plant recombinants – a major oversight by the study committee, which never included a specialist in plant ecology.[368]

On May 21, the first major revision of the report was sent around to the potential signatories. The ad hoc committee now included Stanley Cohen, Herbert Boyer, Ronald Davis (Stanford University), and David Hogness (Stanford University), though Herman

[368] "Leonard Laster to David Baltimore, 29 May 1974," (ASSEM: Life Sciences: 1974: Com on rDNA Molecules: Ad hoc; Central Policy Files; NAA).

Lewis was subtracted.[369] Hogness and Davis were added because the two were performing recombinant DNA experiments that inserted *Drosophila* genes into *E. coli* using both the Berg and plasmid methods.[370]

Due to the serious concern that "artificial recombinant DNA molecules could prove harmful to man," the draft report made four recommendations. "First, and most important, that all scientists join with us in voluntarily deferring the following types of experiments until the potential human hazards of such recombinant DNA molecules have been better evaluated or until adequate methods for preventing their spread." The authors called on all scientists to voluntarily defer the same two types of experiments outlined in the study committee letter.[371] Second, again from the letter, experiments involving inserting animal genes into bacterial DNA could be carried out, but "should not be undertaken lightly" due to the unknown risks. Third, a recommendation that the Director of the National Institutes of Health (NIH) create an advisory committee to oversee programs designed to evaluate experimental risk, and to devise "guidelines to be followed by investigators working with potentially hazardous recombinant DNA molecules." The final suggestion was for an international conference to convene "no later that the Spring of 1975" to review the recombinant DNA "progress" and discuss "appropriate ways to deal with the potential biohazards." The recommendations of the

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[369] All of the added scientists, with the exception of Boyer, became members of the NAS: Davis (1983), Cohen (1980), and Hogness (1976). David Hogness was the younger brother of John Hogness, president of the Institute of Medicine.

[370] "Proposed Final Draft of Biohazard Letter, Sent to Potential Signatories, 21 May 1974 [Privileged Document]," (ASSEM: Life Sciences: 1974: Com on rDNA Molecules: Ad hoc; Central Policy Files; NAA).

[371] Type I experiments created autonomously replicating plasmid that had the potential to increase antibiotic resistance or introduce toxin formation into bacteria. Type II introduction of DNA from oncogenic or other animal viruses into bacteria.

report were based on judgments about the potential risk posed by experiments. The authors acknowledged that this would impact recombinant DNA research in the short term and possibly longer. They wrote that "adherence to our major recommendations" would lead to "the postponement or possibly abandonment of certain types of worthwhile experiments." The report concluded by urging scientists to agree to not "initiate" any of the defined experiments "until attempts to evaluate the hazards have been made and some resolution of the outstanding questions have been achieved."[372]

The change in scope from the letter to the report was nothing short of stunning. It not only called for the authors to postpone the majority of their recombinant DNA research, but for all scientists to do the same. It also called on the NIH to being assessing the risks for experiments, and to create a policy that all research should follow. The goals of the international conference were still unclear, but there was an urgency that it should be held as soon as feasible.

By the end of May 1974, the complexion of the NAS involvement changed completely. The unaffiliated study committee had become an official ad hoc committee, and, according to Handler, the first draft of their report had "a very special quality" that would make a "good opportunity for the highest level policy review" by the NAS Governing Board in June.[373]

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[372] "Proposed Final Draft of Biohazard Letter, Sent to Potential Signatories, 21 May 1974 [Privileged Document]."
[373] "Philip Hander to Leonard Laster, 30 May 1974," (ASSEM: Life Sciences: 1974: Com on rDNA Molecules: Ad hoc; Central Policy Files; NAA).

NRC Governing Board Meeting, 8 June 1974

On June 8, the NRC Governing Board held its monthly meeting, and, near the top of the list, recombinant DNA was discussed under the heading "Biological Hazards of Synthetic Nucleic Acids." Prior to the meeting, the members received the latest draft of the ad hoc committee's report, as recombinant DNA was a major topic of the meeting. Handler admitted early in the discussion that the newly minted ad hoc committee was itself a novel NAS organism. "The procedures which had been followed were not in conformity with established Academy policy—although this group could be reconstituted as an Academy committee, the present letter was signed by individuals—and that it was not responsive to the original request for an NAS action." The Board's assignment of blame and vocal criticism of the initial recommendations in the report was new, and the timing may have seem odd if was not for the sudden publicity surrounding the recombinant DNA and role of the NAS. The pressure for an official NAS report was now coming from two sources: the scientists and the press.

The scientists emphasized their "pressing concern" about safe recombinant DNA experiments, and, perhaps more important, to the Board was that "a reporter on the New York Times" had knowledge of the study committee draft letter. After having received the Singer-Söll letter almost a year prior, there was a sudden "urgency" for a public statement due not only to the safety recombinant DNA experiments, which had continued relatively unabated since the Singer-Söll letter, but because of the mounting publicity surrounding the issue.[374]

[374] "Minutes: National Research Council Governing Board, 8 June 1974."

The Board agreed that the Academy should "promptly" produce a response, and the remainder of the discussion on recombinant DNA focused on what parts of the draft report should be included in the final report. The Board criticized the drafts they had seen as "negative" and not addressing "how one goes about the process of learning while avoiding the dangers which might be involved." The Board was also uneasy with the "self-denying" nature of the original letter and draft report, though Laster reminded them that the ad hoc committee felt strongly about this aspect because it provided "a needed impact." The Board agreed to leave the issue open by deciding that if the self-denying statement was taken out, the scientists could publish a separate private letter expressing their opinion.[375]

The Board meeting itself resolved little, but the Minutes of the meeting provide insight into how the NAS handled the issues raised in the Singer-Söll letter. For nine months the Board member's engagement with the issue was to request that a small and self-selected group of scientists to address a letter sent to the NAS by some of their most esteemed colleagues. A study committee was eventually formed and proceeded with vague instructions to provide guidance to the ALS on recombinant DNA, however, it was not officially affiliated with the NAS, and so the impact of its recommendations initially carried little weight. When the committee reported back after its one day meeting, the reaction in NAS was to break standard procedures and quickly recognized it as an official ad hoc committee. The change in the committee's designation was due to mounting scientific pressure and growing publicity, but it also allowed the redrafted letter to become the official NAS report.

[375] Ibid.

At the NAS Board meeting almost a year after the arrival of the Singer-Söll letter, resolution of issues surrounding recombinant DNA were anything but certain. The president noted that the recommendations of the study committee did not address the issues it was assigned to undertake. There was general concern over recombinant DNA becoming another issue that damaged the public's confidence in science, especially with the *Times* having knowledge of the study committee's letter. And agreement on the content the final NAS report was still muddled at best. The only item where there was complete agreement from the beginning was the need for an international meeting. From the outside, it may have appeared that the NAS was slow, cautious, and or thorough with determining the how to answer the issues set out in the Singer-Söll letter. The reality, however, was that it was only in May 1974, nearly ten moths after Singer-Söll letter's arrival, that recombinant DNA was given a high priority, and only gained a sense of urgency with the extraordinary nature of the study committee letter and the growing publicity surrounding recombinant DNA.

By mid-June, avoiding the growing sense of how recombinant DNA was radically altering research in molecular biology was difficult. The two articles that laid the foundation for experiments that by detailing the procedures for two unique types of recombinant DNA experiments had been circulating for almost a year.[376] An article documenting the first recombinant DNA experiment to cross the species divide was in the

[376] John F. Morrow and Paul Berg, "Cleavage of Simian Virus 40 DNA at a Unique Site by a Bacterial Restriction Enzyme," *Proceedings of the National Academy of Sciences of the United States of America* 69, no. 11 (1972), Stanley N. Cohen et al., "Construction of Biologically Functional Bacterial Plasmids in Vitro," *Proceedings of the National Academy of Sciences of the United States of America* 70, no. 11 (1973).

prior issue of *Proceedings of the National Academy of Sciences*.[377] And recombinant DNA experiments were continuing without any boundaries, despite the Singer-Söll letter and personal efforts like Cohen withholding pSC101. Molecular biologists were still waiting for the NAS to provide any guidance on recombinant DNA experiments.

The Draft Report, June 1974

Although the minutes of the June NRC Governing Board meeting lacked a breakdown of the Board members' votes to leave open the issue of self-denying nature of the letter, it was not an issue that concerned Handler. As the first person to provide suggestions for turning the letter into a report, he was personally engaged with drafting the letter and proposed ways to publish it. The week prior to the Board meeting, he held a positive review of the current draft of the report.[378] Following the meeting, he stayed in touch with Laster, asking that he be "alerted as soon as some word is received concerning the disposition" of the report.[379]

Drafts were disseminated to the members of the ad hoc committee and members of the ALS, and reviewed during June. On June 20, Handler wrote a simple memorandum to editors of scientific journals and the presidents of scientific societies and academies of science alerting them to the enclosed NAS report, which was "of such urgency that I trust you will bring it to the attention" or readers and members. On June 26, Handler read over his and Albert Bormann's final edits to the current draft. These last edits were only

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[377] John F. Morrow et al., "Replication and Transcription of Eukaryotic DNA in *Escherichia coli*," *Proceedings of the National Academy of Sciences of the United States of America* 71, no. 5 (1974).
[378] "Philip Hander to Leonard Laster, 30 May 1974."
[379] "Philip Hander to Leonard Laster, 17 June 1974," (ASSEM: Life Sciences: 1974: Com on rDNA Molecules: Ad hoc; Central Policy Files; NAA).

cosmetic in nature, leaving a final report that was more official in writing and structure that contained the core concerns of the initial study committee letter. There was, however, one exception. Handler altered one critical sentence to add greater impact. He changed "and to a request from the National Academy of Sciences for advice, the undersigned members of the Committee have agreed to abide by, and put forward for others to consider the following recommendations:" To "who requested that the National Academy of Sciences give consideration to these matters. This Committee on behalf of the Academy, offers the following recommendations[.]"[380] With the stroke of his pen, Handler had given the recommendations of the study committee the full support of the NAS. After completing a review of the edits, he then picked up the phone and called Paul Berg to discuss the final draft of the report.[381] The two were in agreement about the final round of edits, and Handler offered to contact Robert Stone, the director of NIH, so that Stone could get started on the report's recommendations for NIH.[382]

Following their phone conversation, Berg wrote a letter to Handler concerning the report and the international conference. Berg was "glad we were able to resolve the content and wording of this 'document,'" and that it clearly expresses the views of the study committee "with the flavor we intended when we conceived and drafted it." He ended his comments on the final version of the report succinctly: "Needless to say, all of

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[380] "Proposed Final Draft of Biohazard Letter, Sent to Potential Signatories, June 1974 [Philip Handler and Herbert Bormann Draft]," (ASSEM: Life Sciences: 1974: Com on rDNA Molecules: Ad hoc; Central Policy Files; NAA).

[381] A handwritten note by Philip Hander appears on an undated edited draft. The note states: "Checked [with] P Berg on 26/6/74[.] Await New Draft[.] PH" From the records and date it appears to be the last edited draft before the final draft was distributed for approval. [Ibid.]

[382] "Paul Berg to Philip Handler, 27 June 1974," (ASSEM: Life Sciences: 1974: Com on rDNA Molecules: Ad hoc; Central Policy Files; NAA).

us hope it has its intended effect." Berg was in agreement with Handler's plans for the distribution of the report, and that he would attend the press conference on the day the report was published. He also suggested that David Baltimore and James Watson attend.[383]

Berg's letter also demonstrated that he was actively working on plans for the international conference. He contacted the National Science Foundation about providing funds to help support the conference "now planned for [international conference at] Asilomar in February," and proposed a schedule for when working groups for the conference should meet and have proposals prepared for discussion at the conference. Berg concluded his letter by thanking Handler for contacting the NIH because "some movement is imperative if for no other reason than to let people know that they don't have to wait forever before there is a green, yellow or red light on these experiments." In early July, the ALS Executive Committee unanimously endorsed the final version of the report, which noticeably provided a greater endorsement of the report in two important ways. First, it dropped the "ad hoc" from the Committee on Recombinant DNA Molecules, and second the wording of one important sentence changed again.[384] The final wording of the sentence, which Handler had changed to provide more impact, now read: "The undersigned members of a committee, *acting on behalf of and with the endorsement*

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[383] Ibid.

[384] "Philip Hander to Robert Stone, 15 July 1974," (ASSEM: Life Sciences: 1974: Com on rDNA Molecules: Ad hoc; Central Policy Files; NAA). The exact date for when the "ad hoc" was dropped is a bit of a mystery since there are no official records of the ALS or any board or committee within the NAS/NRC voting for an official change of name. According to the records, the change occurred between the last edited draft on 26 June 1974 and when an advanced copy was sent from Philip Handler to Robert Stone on 15 July 1974. ["Philip Hander to Robert Stone, 15 July 1974."]

of the Assembly of Life Sciences of the National Research Council on this matter, propose the following recommendations[.]"[385]

The Final Report and the NIH, 15-17 July 1974

On July 15, Handler sent the report to Stone before it was published, so that the NIH could begin working on the report's recommendation addressed directly to the NIH ("Recommendation Three"). Stone was aware of the report for some time, since members of the ad hoc committee were in contact with Stone and other NIH scientists during the drafting of the report. The advanced copy of the final version, however, allowed Stone to focus on the exact wording of Recommendation Three. While Handler expressed hope that the NIH would accept the recommendation, and even offered NAS assistance, he permitted the NIH latitude for its rejection by Stone.[386]

The NIH had already started working on the recommendations prior to Stone receiving the final report. On July 16, the NIH began work on negotiating a contract with the NAS to establish an advisory committee on recombinant DNA that would support the proposed international conference. The advisory committee was described as "recommending an experimental program to evaluate the potential biological and ecological hazards of various types of recombinant DNA molecules and other autonomously replicating laboratory-produced agents, developing procedures to

[385] Paul Berg et al., "Potential Biohazards of Recombinant DNA Molecules," *Proceedings of the National Academy of Sciences of the United States of America* 71, no. 7 (1974). Emphasis added.
[386] "Philip Hander to Robert Stone, 15 July 1974."

minimize spread of such molecules with human and other populations, and devising guidelines to be followed by investigators working with such agents."[387]

Stone replied to Handler two days later, and agreed to proceed with Recommendation Three, and judged the authors of the report to be "highly responsible thinking and action." He also assured Handler that the NIH was actively monitoring recombinant DNA experiments, and had been "developing guidelines, soon to be published, for assuring containment of such agents while at the same time encouraging the remarkably able work of the molecular biologists, virologists, and geneticists who are pushing forward at this leading edge of science." The growth of recombinant DNA work over the past year, however, expanded the NIH's concerns beyond human pathogens to potential experiments in agriculture and industry. Stone concluded his letter by approving the social responsibility demonstrated by of scientists in the initial recombinant DNA debates. "It is apparent that such individuals are not only out to do 'science for science's sake,' but are prepared to postpone further work until they are sure that there will be no risk to the public as a result of their investigation." The next day the report was made public at a press conference held at the NAS, and for the first time the public would have a chance to judge the initial work of the NAS and weigh in on the recombinant DNA debate.[388]

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[387] "Leon Jacobs to Acting Chief, Research Contracts Branch and Office of Contracts and Grants, ADA/NIH, 16 July 1974," (Res 15-1-D, NIH/NAS Asilomar Conference Feb. 23-27, 1975, 1974-1975; NACP-NIH).
[388] "Robert Stone to Philip Handler, 17 July 1974," (ASSEM: Life Sciences: 1974: Com on rDNA Molecules: Ad hoc; Central Policy Files; NAA).

The Announcement of Deferral

On July 18, the NAS report on recombinant DNA entitled "Potential Biohazards of Recombinant DNA Molecules" was released to the public.[389] The NAS held a press conference in Washington, DC to announce the publication of the report. David Baltimore wrote the opening statement for press conference, which was approved by committee members Paul Berg and Richard Roblin. The statement was essentially a condensed version of the published report with the science drastically abridged and provided some tangible examples that the public could understand. The most remarkable part about Baltimore's statement was how it framed recombinant DNA.

The statement opened with an oversimplified explanation of recombinant DNA. "Pieces from any source can now be caused to multiply inside a bacterium…[that can] produce enormous amounts of the specific DNA of interest." The overly positive nature of the technique quickly followed in the next paragraph. "This technique is being exploited in a number of laboratories to answer important outstanding questions in biology." The technique "holds the promise" for the public as well. It could produce "therapeutic compounds" such as insulin as well as turning "nitrogen from the atmosphere into plant food."[390] In the third paragraph, the authors of the report finally

[389] The report would be published in next issues of *Nature* [———, "NAS Ban on Plasmid Engineering," *Nature* 250, no. 5463 (1974).], *Science* [———, "Potential Biohazards of Recombinant DNA Molecules," *Science* 185, no. 4148 (1974).], and the *Proceedings of the National Academy of Sciences* [Berg et al., "Potential Biohazards of Recombinant DNA Molecules."].

[390] Nitrogen fixation was the first proposed application for plants because, if successful, it would dramatically reduce the amount of fertilizer need for crop production, and well as alter the methods of crop rotation.

addressed the concerns about recombinant DNA that led report and that dominated the press conference.[391]

While the technique "is a crucial advance in the methodology of molecular biology," "if" it were used in certain experiments it "could represent a danger." The statement then briefly defined the "two types of DNA which could prove hazardous" (Type I and Type II experiments), while expressing a "special worry" about the use of viruses that caused "cancer in animals." In a classic case of burying the lead, the reason for the press conference was not reached until the fifth paragraph that Baltimore got to.[392]

The issuance of the report was "in response to the concern of many members of the scientific community about the hazards implicit in this type of research." Unlike the benefits of the technique, there were no tangible examples of any of the potential hazards. Rather it referred people to the report, which "outlines the potential hazards" in less accessible language to the general public. [393] The concluding paragraph began with the statement that the committee was bound "not to utilize" the technique in any of the stated two types of potentially hazardous experiments. The paragraph ended with a sentence on

[391] "Statement Prepared by Dr. David Baltimore and Approved by Dr. Paul Berg and Dr. Richard Roblin for Presentation at a Press Conference July 18, 1974 at the National Academy of Sciences," (ALS: D Med: Recombinant DNA Molecules, Organizing Com for Int'l Conf on, Report Inception 1973-1974 [ARMN-91 P77-004]; Central Policy Files; NAA).
[392] Ibid.
[393] The statement references the report twice, the first time referring to it as "a statement" and later as "the report." I am going to refer to it as the report because before it was published all references to it were as a report and not a statement.

the committee's request that the worldwide scientific community in "voluntarily deferring" from these experiments as well.[394]

The statement briefly mentioned experiments with the "remote possibility of hazard" that should only be undertaken "with the appropriate concern for the hazard."[395] It did not explain that the experiments were defined in the report that created recombinant DNA molecules using animal DNAs. The report had a more explicit warning for these experiments that held the potential risk of creating recombinant molecules with animal tumor viruses "whose biological properties cannot be predicted with certainty." "[S]uch experiments should not be undertaken lightly."[396]

The statement then covered the recommendations for the NIH and international conference. The NIH was to create a committee to evaluate potential hazards, in order to produce the outcome that the NAS committee hoped. The outcome was "that it will be possible to proceed with the experimentation in the near future when the potential for hazard has been overcome." Likewise the international conference would act as a gathering where the scientific community "will try to access the pace at which this type of research should proceed." The emphasis of the statement on the recombinant DNA experiment continuing in the very near future is unmistakable.[397]

Due to "simple" nature of and "short" time required to carry out the experiments, the committee was publishing the report in *Science*, *Nature*, and the *Proceedings of the*

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[394] "Statement Prepared by Dr. David Baltimore and Approved by Dr. Paul Berg and Dr. Richard Roblin for Presentation at a Press Conference July 18, 1974 at the National Academy of Sciences."
[395] Ibid.
[396] Berg et al., "Potential Biohazards of Recombinant DNA Molecules."
[397] "Statement Prepared by Dr. David Baltimore and Approved by Dr. Paul Berg and Dr. Richard Roblin for Presentation at a Press Conference July 18, 1974 at the National Academy of Sciences."

National Academy of Sciences "in order to reach the scientific community rapidly with its appeal."[398] The urgency of the appeal, however, was quickly undermined by the final paragraph, which returns to the theme of continuing recombinant DNA research in the very near future.

> The committee wishes to emphasize that nothing which as been done thus far by scientists has produced any known hazard and that the risk of hazard is not enormous. Rather than accept the risk of hazard, however, the committee felt it more prudent to evaluate the hazard before carrying out the experiments.[399]

The statement as a whole, and the final paragraph in particular, may have easily led someone in the audience to believe that scientists were halting recombinant DNA research as soon as they recognized the potential hazards as opposed to more than two years after Berg canceled the first recombinant DNA experiment. Further they might also believe that if the proper safety mechanisms were available that they would be quickly installed allowing recombinant DNA research to begin again.

A comparison of the opening statement of the press conference and the published report is striking. The statement was rather nebulous about when the technology was invented, any of the prior debates, or even the Singer-Söll letter, which was the impetus for the NAS report. In contrast, the report did mention the Gordon Conference and references the Singer-Söll letter. The two also differed significantly in tone. The statement contained a much more positive outlook for a quick resumption of recombinant DNA research and downplays the potential hazards, except for calling attention to viruses that caused cancer in animals. The report was more detailed in explaining the potential

[398] Ibid.
[399] Ibid.

hazards and the reasons for the recommendations. It also was more tentative about the future of recombinant DNA experiments with no explicit mention of when "postponement or possibly abandonment" of certain experiments might be reversed.[400]

The statement and report also differed extensively in word choice. The two shared only one phrase of consequence, that the committee was "voluntarily deferring" Type I and Type II experiments. Although it is understandable that Baltimore did not want to delve into too much actual science in the statement knowing that the report was provided at the press conference, he used the word "recombinant" only once in the statement, which was a reference to the title of the report. The report, however, used the word "recombinant" twelve times in reference to the both recombinant molecules and recombinant plasmids. Notably absent from the statement were the terms "plasmid" and "virus," although both were plentiful in the report. Perhaps the most interesting word choice was "hazard."[401] Despite the generally optimistic outlook of the statement, the word hazard was used twelve times, with "worry" and "danger" each used once. In the report, which was more cautious about the research proceeding, hazard was only used five times. The juxtaposition of the excessive use of hazard – twelve times in an eight-paragraph statement – against its overall positive outlook on recombinant DNA pointed more to the oversimplification or generalization of the potential hazards of recombinant DNA experiments than to the actual concern of Baltimore about the safety of the technology. Echoing the Singer-Söll letter, the NAS report and statement both made a concerted effort to channel discussions about recombinant DNA into a debate exclusively focused on mitigating potential hazards surrounding the technology. Both documents

[400] Ibid, ———, "Potential Biohazards of Recombinant DNA Molecules."
[401] The word hazard in all of its forms was counted, including hazards and hazardous.

intentionally excluded issues relating potential military uses, genetic engineering, gene therapy, or the politics, ethical, or legal ramifications of recombinant DNA thus constraining initial debates focused primarily on the issue of biohazards.[402]

Into the Fold

The day of the press conference, recombinant DNA appeared on front pages across the country. *The New York Times'* mundane headline "Genetic Tests Renounced Over Possible Hazards" sat amongst the London Tower bombing, the war in Cyprus, and news of Nixon's potential impeachment.[403] The *Washington Post* headline "NAS Panel Warns of 'Hazards': Halt in Genetic Work Urged" was mirrored by headline of its in town rival, "Halt Genetic Experiments, Science Panel Urges."[404] On the west coast the *Los Angeles Times* ran a more sensational headline, "Hybrid Molecule Test Threat Seen: Group Asks Moratorium on Certain Experiments."[405] The following morning, the headline of the *Oakland Tribune* "Scientists Fear Release of Bacteria" was perhaps the most extreme headline for an article covering the press conference and release of the report.[406]

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[402] "Statement Prepared by Dr. David Baltimore and Approved by Dr. Paul Berg and Dr. Richard Roblin for Presentation at a Press Conference July 18, 1974 at the National Academy of Sciences.", Berg et al., "Potential Biohazards of Recombinant DNA Molecules."
[403] Victor K. McElheny, "Genetic Tests Renounced over Possible Hazards," *The New York Times*, 18 July 1974.
[404] Stuart Auerbach, "Halt in Genetic Work Urged," *Washington Post*, 18 July 1974, Judith Randal, "Halt Genetic Experiments, Science Panel Urges," *The Washington Star*, 18 July 1974.
[405] Harry Nelson, "Hybrid Molecule Test Threat Seen: Group Asks Moratorium on Certain Experiments," *Los Angeles Times*, 18 July 1974.
[406] Juan Vergara Hovey, "Scientists Fear Release of Bacteria," *Oakland Tribune*, 19 July 1974.

All of these articles were similar in that they all condensed the report and recommendations into a language that could be understood by their lay readers. Most also explored the idea of scientists voluntarily stopping their own work, and concluded that it was the first time that it had been done in modern science. *The New York Times* compared the voluntary moratorium to the non-German nuclear physicists refusing to publish their work during the Second World War to deny Nazi Germany the information.[407] A final similarity is the general sense of fear of recombinant DNA, which seemed to be caused more by the over reliance on "hazard" in the press conference than from the very tentative optimism of the report. The abundance of the word "hazard" in statement did have its intended effect of focusing the immediate press coverage on issues of potential experimental risks. It also led to the unintended consequence of stoking fear and misunderstandings about what recombinant DNA was and why it was of such great importance to the scientists.

Over the next few weeks and months more thoughtful analysis from the general press and reaction from scientists took shape, and the terms like "moratorium" quickly replaced the report and statement terms, "voluntary deferral" and "postponement or possibly abandonment." Without data to support the potential for risk of biohazard, the majority of the reports were informed speculation from both scientists and non-scientists. While the debates continued to simmer in the popular and scientific press, the NAS and NIH worked diligently on a couple of the recommendations from the report, most important of which was the international conference scheduled for early the following year.

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[407] McElheny, "Genetic Tests Renounced over Possible Hazards."

Chapter 7: Filling the Spaces in Between

In the month following the initial news reports regarding the Committee on Recombinant DNA Molecules' (CRDM) press conference on July 18, 1974, reaction to the press conference occurred simultaneously among four groups: the press, scientists, the National Institutes of Health (NIH), and the National Academy of Sciences (NAS). The NAS and the NIH assumed two distinct tasks. The NAS was to organize an international conference, and the NIH was to create a national advisory committee on recombinant DNA. The debates during the moratorium took place in the publicly in lay and scientific press, as well as, privately between scientists. The press and the scientists filled the space between the CRDM report and the international conference. An examination of press reports following in the wake of after the CRDM press conference reveals how the public first was introduced to and came to understand recombinant DNA through an analogy that their readers might understand. The analogies ranged from the bestselling *Andromeda Strain* to Greek mythology. The molecular biologists themselves were not immune from analogizing their situation to that of atomic physicists after the first successful controlled fission chain reaction. This analogy began in the late 1960s with the dramatic advances in molecular biology and reached a fevered pitch among scientists during the early years of recombinant DNA. A few molecular biologists went so far as to refer to their current situation in 1974 as a "pre-Hiroshima condition."[408] The private debates between the scientists reveal what initial concerns the moratorium raised

[408] Nicholas Wade, *The Ultimate Experiment: Man-Made Evolution* (New York, NY: Walker, 1977), 30.

and forecasted the first disagreements to be argued at the upcoming international conference.

The Press and Analogy

After making national front-page headlines on July 18, 1974 the recombinant DNA issue was short-lived as national press coverage of the issue died within a month. The coverage was, however, the first sustained national reporting and editorializing specifically on recombinant DNA. As Paul Berg admitted in late 1974, his months-long decision to halt the first proposed recombinant DNA experiment "was all done without newspapers."[409] Indeed recombinant DNA was a difficult topic to understand and the science itself was in such a nascent stage that the benefits and risks were almost completely unknown. The scientists called for a moratorium due to these unknowns. The press searched for ways to examine the problem of the unknowns in ways its readers could understand.

Following the public release of "Potential Biohazards of Recombinant DNA Molecules,"[410] the initial newspaper articles largely deconstructed the scientific language of the report into terms its readers could understand and primarily focused any analysis on the potential risks of the outlined recombinant DNA experiments – the "hazards" repeatedly mentioned at the press conference by David Baltimore. While most of the initial reports in the days following the moratorium becoming public were balanced, others dramatically overstated the potential risk beyond those stated by the scientists,

[409] Cristine Russell, "Weighing the Hazards of Genetic Research: A Pioneering Case Study," *BioScience* 24, no. 12 (1974).
[410] Paul Berg et al., "Potential Biohazards of Recombinant DNA Molecules," *Science* 185, no. 4148 (1974).

including one with a subheading entitled “Massive Hazard.”[411] Within weeks, the press quickly adopted a new strategy of using analogies to convey the known and unknown potential benefits and risks of recombinant DNA and significance of the moratorium. The choice of analogies, however, reinforced the language and framing of the recombinant DNA debates by the CRDM as one of scientific risk. An examination of the analogies used in the month following the release of the CRDM report demonstrates the public’s first exposure to recombinant DNA and how the public first came to understand recombinant DNA.

Once of the first analogies employed to connect readers with recombinant DNA was human genetic engineering. The definition of genetic engineering was broad and there was already a degree of public understanding to make the analogy work. The use of human genetic engineering as an analogy was best exemplified by an editorial published in *The Washington Star News* two days after the release of the CRDM report. “Caution in Genetics” opened by strangely defining genetics as “a very prickly subject, when applied to the human race, and one that still is full of mysteries.” Human genetics was well over a half-century old and not itself a “prickly” scientific field. More important, the members of the CRDM were careful not to make any reference to applying recombinant DNA to humans in their internal decisions and public statements. That did not prevent the author of the editorial from introducing the prospect of direct application of recombinant DNA on human. “Apart from the issue [the moratorium] at hand, of course, is the question of whether genetic science should be used to change human characterizes ‘for the better.’” The answer for the author was, of course, no. To reinforce the point, the

[411] Warren Froelich, "Genetic Scientists Seek Ban," *The Philadelphia Bulletin*, 23 July 1974.

author raised the specter of the discredited notion of human "eugenic redesign." The tenuous analogy created between recombinant DNA, genetic engineering, and eugenics was completed by the editorial's conclusion that all three were linked by the necessary "limits beyond which science can never go." The editorial argued for a limit to recombinant DNA research not on the risk of biohazards, but based on the potential of research in areas that were morally and ethically problematic. Although genetic engineering was a topic of Congressional hearings in the late 1960s and early 1970s and a regular feature of modern fiction, it was a contemporaneous science-fiction scenario that became common in the weeks that followed.[412]

A more timely analogy played on a fictional disease outbreak that was part of the popular culture of the early 1970s. *The Andromeda Strain* by Michael Crichton was published in the early summer of 1969 and was soon on the same *The New York Times* Bestseller List as *The Godfather* and *Slaughterhouse-Five*.[413] It remained on the Bestseller List for thirty weeks, and was quickly turned into a feature length movie.[414] The story took place over a four-day period when a virus enters earth's atmosphere for the first time aboard a satellite that crashed to Earth in a small southwestern town. The virus quickly began killing humans, and no scientist could slow its transmission or design

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[412] "Caution in Genetics," *The Washington Star News*, 20 July 1974.
[413] "The New York Times Book Review: Best Seller List," *The New York Times*, 29 June 1969.
[414] "The New York Times Book Review: Best Seller List," *The New York Times*, 18 January 1970. The paperback version of The Andromeda Strain debuted in the summer of 1970 and soon became a paperback Best Seller. The movie, directed by Oscar winner Robert Wise, had the tagline "The picture runs 130 minutes!... The story covers 96 of the most critical hours in man's history!... The suspense will last through your lifetime!" and debuted on 12 March 1971 to mixed reviews. [See: Roger Ebert, "The Andromeda Strain," *Chicago Sun-Times*, 9 April 1971, Roger Greenspun, "Screen: Wise's 'Andromeda Strain'," *The New York Times*, 22 March 1971.]

a cure. Just before it was able to arrive in a large city, however, the virus mutated and, due to the greater percentage of oxygen in earth's atmosphere, returned to space. *The Andromeda Strain* was an analogy that could easily be understood and automatically carried with it the idea of biohazard, fatal disease, and the helplessness of scientists to stop an outbreak.[415]

In an article entitled "A New Fear: Building Vicious Germs," Judith Randal began by creating an Andromeda Strain-like scenario of a "potentially very dangerous bug" imported from a space mission. Her article, she assured her readers, had nothing to do with the space program, but rather "to an unprecedented appeal last week by a group of scientists." After stating *bona fides* of the scientists who called for the delay, Randal decided to mix in more analogies to make her point. She explained that recombinant DNA "may unleash on the world micro-organisms with Frankenstein prosperities," and that the moratorium was called because the "potential hidden dangers are so great – the Trojan Horse possibilities so frightening." Randal's final analogy was also out of Greek mythology. She suggested that disease constructed by recombinant DNA would be akin to opening Pandora's box.[416] In her attempt to explain the complexities, benefits, risks, and unknowns, Randal evoked a dizzying array of analogies that may have future obscured the realities of recombinant DNA and the rationale for the moratorium.

Though it provided a more balanced analysis between the potential benefits and risks or recombinant DNA, the *Chemical and Engineering News* also could not resist the Andromeda Strain analogy. It began its coverage of the CRDN report, "With images of

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[415] Michael Crichton, *The Andromeda Strain* (New York, NY: The Ballentine Publishing Group, 1969).
[416] Judith Randal, "A New Fear: Building Vicious Germs," *The Washington Star News*, 22 July 1974.

an ‘Andromeda Strain’ lurking in the background.” The article, however, quickly deviate from a potential Andromeda Strain outcome, and instead focused on the general issue of hazards, scientific agreement on the moratorium, and the work underway by the NIH. As a respected science news organization extending the analogy beyond a headline deviated from the importance of the moratorium itself.[417]

As one of the most widely read weekly news magazines, *Time* magazine also found the Andromeda Strain analogy irresistible in its brief coverage of the moratorium. The article, entitled “The Andromeda Fear,” made no reference to an alien outer space virus, though it played heavily on the fears of an uncontrollable disease by quoting perhaps the most famous biologist, James D. Watson who “declared” that the reason to halt recombinant DNA experiments was “fear that if they do not stop, they may inadvertently loose upon the world new forms of life—semisynthetic organisms that could cause epidemic, or resist control by antibiotics, or increase incidence of cancer.” According to the article, the CRDM was also “haunted” by the “possibility that their work might be subverted for inhuman ends.” Though the *Time* article did overindulge in the potential hazards of recombinant DNA, it did not create potential recombinant DNA scenarios from whole cloth. That accomplishment rested in an editorial in a late July 1974 editorial in the *Salt Lake City Tribune*.[418]

In a July 21 editorial in the *Salt Lake City Tribune* aptly titled editorial, “Shades of Science Fiction,” the most imaginative use of a science fiction analogy appeared when

[417] "Halt to Genetic Manipulation Urged," *Chemical and Engineering News*, 29 July 1974, 4.
[418] "Andromeda Fear," *Time*, 29 July 1974.

the author actually wrote a thumbnail sketch of a dangerous recombinant DNA molecule getting out of the laboratory.

> We can see it all now. The laboratory is dark and deserted but inside a large bottle there is restless stirring. Suddenly the stopper pops out and slim blobs of deadly new germs slither to the floor, out the door and down the street to the apartment of a beautiful young lab technician. She gives the alarm. After a harrowing week or so during which humanity is threatened with extinction, a handsome young technologist neutralizes the all-powerful bugs by the simple device of spraying them with mountain spring water. (He then marries the lab technician almost as an afterthought.)[419]

The author concluded that if the CDRM report was correct, "there is no fiction involved here. The threat is real and, unless experiments are abandoned, immediate."[420] Though the science fiction analogy was certainly stretched beyond anything imagined by the scientists of the CRDM, the author of the editorial used it as near scientific truth to explain the potential dangers posed by recombinant DNA to its readers. Across the country, a more understated editorial entitled "Dangerous Knowledge," in *The Times of Trenton* described the "request" of Paul Berg and the other members of CDRM as reading "almost like something from a science fiction thriller."[421]

An analogy that both the *Salt Lake City Tribune* and *The Times of Trenton* mentioned in passing near the end of their editorials regarded atomic scientists. The *Tribune* referred not to the Manhattan Project era, but to the "early days of nuclear energy" when "a number of scientists objected to continued nuclear research out of fear

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[419] "Shades of Science Fiction," *Salt Lake City Tribune*, 21 July 1974.
[420] Ibid.
[421] "Dangerous Knowledge," *The Times of Trenton*, 25 July 1974.

that nuclear power, misapplied, could destroy or greatly endanger mankind."[422] The *Times* editorial delved back to the late 1940s and the debate over the hydrogen bomb. "The [CRDM]'s request isn't likely to spark the sort of public debate that preceded the development of the hydrogen bomb." These two passing references were only the beginning of an analogy that would endure throughout the recombinant DNA debates both in public and between scientists.[423]

An early August 1974 editorial in the *Washington Post* opened haltingly with a clear analogy referencing the legacy of the use of atomic energy as a weapon. "Ever since Hiroshima, scientists have been concerned that probing the secrets of nature without caution and moral restrain might open a Pandora box of ills, if not disasters, that could do terrible damage."[424] The editorial also asserted that Berg and the other members of the CRDM did not act with a "collective conscience…to weigh the risks" to prevent another Hiroshima disaster.[425] The CRDM report and public statements by its members focused almost exclusively on the potential scientific hazards. The voluntary moratorium focused tightly on only two types of experiments that the committee deemed as potentially too hazardous with little expressed consideration of potential moral or ethical problems that may result from recombinant DNA research. Further, the length of the voluntary postponement was to be decided by a select international group of scientists at a private conference early in the new year, and not contingent on any public or legislative input.

The CRDM actions were a marked contrast to Jonathan Beckwith, James Shapiro, and Larry Eron, in November 1969 when the three scientists held a press conference in

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[422] "Shades of Science Fiction."
[423] "Dangerous Knowledge."
[424] "The Scientific Conscience," *Washington Post*, 3 August 1974.
[425] Ibid.

Cambridge, MA and announced the isolation of the first gene and also call attention to the potential public risk of their work and warn against the potential for government misuse of science.[426] Despite their groundbreaking discovery, all of these scientists abandoned this line of research because of the moral and ethical problems they thought it might create.

Gerald Leach wrote an article for the *Washington Post* titled "A Nuclear Hangover Makes Us Panic Over Genetic Advances" about the Beckwith press conference. Leach explained that the scenario of a "biological bomb, with white-coated genetic engineers leering from the mushroom cloud read to create races of supermen and docile naves" was "completely unfounded."[427] He concluded that our "very deep-seated" rationale for biological advances caused panic. [428] In the midst of the Cold War, one of the easiest ways to express panic was to invoke the specter of the potential of a weapon as deadly as a nuclear bomb. A few years later, the claims of new and deadly biological warfare agents created through genetic manipulation became a reality with the advent of recombinant DNA. The reaction of some scientists was predictable as they began describing the situation as a "pre-Hiroshima condition."[429] The use of the atomic physics analogy was an often heard warning to the advances of molecular biology in the late 1960s due in largely to the discoveries in the late 1960s that made genetic engineering a very real possibility.

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[426] For more on Beckwith, Shapiro, and Eron, and their press conference see Chapter 3.
[427] Gerald Leach, "A Nuclear Hangover Makes Us Panic over Genetic Advances," *The Washington Post*, 7 December 1969.
[428] Ibid.
[429] Wade, *The Ultimate Experiment: Man-Made Evolution*, 30.

Molecular Biologists and the Atomic Bomb Analogy

In the October, 20 1969 issue of *The Nation*, Salvador Luria – pillar of molecular biology community, liberal political activist, and soon to be Nobel laureate – wrote the article "Modern Biology: A Terrifying Power."[430] In the article, Luria argued that biology, with all of the recent discoveries, was not undergoing a "revolution." Biology attained a "scientific fulfillment" that permitted molecular biologist to "soon be able to influence [human] heredity directly" by making the "genetic experimentation available to chemical experimentation." Although Luria found the idea of any scientific moratorium as the "least rational and least effective approach," his concern focused on the potential for social and military misapplication of molecular biology. He explained that "genetic surgery" had advantages, but that it was also possible for a government to misuse the technology to create biological weapons or a master race akin to Aldous Huxley's "nightmarish utopia" of a *Brave New World*. Luria concluded that society needed to direct technology "towards socially chosen goals," and scientists must engage in dialogue about their science with the public and governments. In his four-page article, Luria did

[430] Salvador E. Luria, "Modern Biology: A Terrifying Power," *The Nation*, 20 October 1969. Luria shared the Nobel Prize in Physiology or Medicine in 1969 with Max Delbrück and Alfred Hershey for "their discoveries concerning the replication mechanism and the genetic structure of viruses." Luria, along with Delbrück, was a founding member of the "phage group," which was initially a group of scientists around the United States in the early 1940s. Their work with bacteriophages was instrumental in advancing molecular biology. Luria's political activities included protesting nuclear weapons testing, protesting the re-election of President Nixon, and protesting the Vietnam War. For more information on Luria and the phage group see Horace Freeland Judson, *The Eighth Day of Creation: Makers of the Revolution in Biology*, Expanded ed. (Plainview, NY: CSHL Press, 1996), John Cairns, Gunther S. Stent, and James D. Watson, *Phage and the Origins of Molecular Biology*, Expanded ed. (Plainview, NY: Cold Spring Harbor Laboratory Press, 1992), Michel Morange, *A History of Molecular Biology* (Cambridge, MA: Harvard University Press, 1998).

not use the word “genetic engineering” once. Perhaps he did this because of the broad definition of the term as well as its potentially negative implications. [431]

Prior to recombinant DNA, because there was no way to cut and paste a gene or gene sequence of a chromosome with surgical accuracy, “genetic engineering” was term used to describe any general experiment that altered the genetic make up of an organism, including animal husbandry and plant breeding. Accordingly, by the late 1960s the term had become an umbrella term for a broad range of real and proposed experiments on an equally broad range of organisms. As a 1970 Congressional Research Service (CRS) report on genetic engineering made clear, “it is obvious from the many questions asked about genetic engineering that a number of subjects such as ‘test-tube’ babies, clonal propagation and others are being ‘lumped’ under the general term genetic engineering.” The report later attempted to provide a definition of the genetic engineering that spanned the better part of a page. The essence of the definition was that genetic engineering was the modification of “biochemical instructions monitored by genes so that the development of the organism from the modified cell will be affected by the change.”[432] Genetic engineering could also be applied to both the somatic (body) and germ (sex) cells. The result of modifying germ cells was to insert “favorable” traits that would replace the deficient traits in the current and future generations.[433] A report by the same

[431] Luria, "Modern Biology: A Terrifying Power," 406-09.
[432] James M. McCullough, "Genetic Engineering: A Brief Review and a Bibliography," (Washington, DC: Congressional Research Service, 1970), 1-2.
[433] Ibid., 11.

author on genetic engineering two years later to the US House Subcommittee on Science, Research, and Development show little to no change in his "broad field" definition.[434]

That the term genetic engineering was hard to define with any certainty in the late 1960s did not mean it was without the ability to fascinate or frighten the public.[435] Senator Walter Mondale in his opening statement to the 1968 hearing of the National Commission on Health Science and Society compared genetic manipulation to the "dawning of the nuclear age." Mondale addressed a wide array of scientific technologies related to human health, and employed two specific analogies for genetic engineering. The first analogy, which he used multiple times, was atomic physics. For Mondale, atomic physics produced a template in how to proceed with advancing technologies because "our experience with the atom teaches us that we must look closely at the what we do." Later in his opening, Mondale returned to the analogy and concluded that "[s]urely when a scientist unravels a technique for engineering future generations, that is not a matter solely for the scientist's interest. It fundamentally affects our society; it fundamentally affects mankind—perhaps in a more searching sense than the atomic bomb." The second analogy was that human genetic engineering would result in a "brave new world." He viewed the idea of creating a master race as the most terrifying of potential outcomes. [436]

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[434] James M. McCullough and Congressional Research Service Science Policy Research Division, Library of Congress, "Genetic Engineering: Evolution of a Technological Issue," ed. Research Subcommittee on Science, and Development, Committee on Science and Astronautics, and US House of Representatives (Washington, DC: US Government Printing Office, 1972), 1-2.
[435] US Congress. Senate. Subcommittee on Government Research, *National Commission on Health and Science Policy*, 90st Cong., 2nd sess., 1968, 42.
[436] Ibid., 5, 6.

On March 8, 1968, two renowned molecular biologists went to Capitol Hill to provide testimony on advancements in genetics and while there, they directly confronted the atomic science analogy. Providing testimony for the Commission were Arthur Kornberg and Joshua Lederberg, both from Stanford University School of Medicine and Nobel laureates.[437] The Commission questioned Kornberg directly before Lederberg, and genetic engineering was the issue that held the senators' focus. The Commission briefly questioned Kornberg about the idea of creating a master race (even invoking Adolf Hitler). Kornberg's responded that the outcome was "remote" and that there are "more worthwhile" studies to be done. [438] When Senator Abraham Ribicoff did not receive a satisfactory answer to a question about how scientists dealt with legal and ethical questions about genetic engineering, he decided to use atomic science to make a "point." "The scientists—and I am sure you must know many of them—who were involved in the creation of the atomic bomb and the hydrogen bomb over the years since their work, they have undergone great soul-searching, from the public platform, in philosophical writings, in protests. And I wonder what they would have been thinking then if they knew what the development of atomic energy would lead to in the present time."[439]

Ribicoff wanted to know if Kornberg and other scientists working in genetic engineering adequately considered the potential negative outcomes of their work. Kornberg responded that geneticists do not focus primarily on potential consequences of their work because those potentials may never be realized. As a concerned citizen,

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[437] Both were awarded the Nobel Prize in Physiology or Medicine, Kornberg in 1959 and Lederberg in 1958. Kornberg shared the prize with Severo Ochoa for their discovery of biological synthesis of RNA and DNA. See Chapter 4 for Lederberg's scientific work and Nobel Prize.
[438] *National Commission on Health and Science Policy*, 45, 47.
[439] Ibid., 46.

Kornberg recommended two ways that the United States could be prepared before genetic engineering was a reality. The first was to educate the public in biology and chemistry, so they would be able to make informed judgments on science. And second, the science must be properly funded to enable advancement of the science in order that enough information would be available to allow the geneticists to answer legal and ethical questions. Kornberg provided concrete solutions to the problems from the perspective of an active scientist. He also revealed that molecular biologists generally did not take into account potential moral or ethical problems of their results when designing and carrying out experiments.[440]

Lederberg's testimony also focused on genetic engineering. Senator Ribicoff asked him a similar question concerning the consequences of his work. Ribicoff saw genetic engineering as a field that had "grave consequences—when I say grave, I mean positive and negative." And again Ribicoff referenced atomic scientists. "I have been personally struck by the traumatic effect that the consequences of atomic research and atomic bombs, and the hydrogen bombs, have had upon so many pure scientists, brilliant scientists, who worked in this field. And you get the feeling that many of them felt that when they see the consequences of their discoveries maybe the wish they were a plumber of a truckdriver." The senator wanted to know how "brilliant scientists" dealt with the potential consequences of their work. [441]

Lederberg responded to this question more thoughtfully than Kornberg. He told the senator that some scientists were not much different from legislators. Some "go through a period of deep soul-searching," while others "find some kind of practical

[440] Ibid., 46-47.
[441] Ibid., 55.

accommodation" or deal with "matters at very different kinds of levels." The solution Lederberg proposed was to have better communication between scientists and the public because scientific information "is the freest that exists in any part of our culture." He also stressed the need for a scientifically educated public because they had a right to understand what is happening in science.[442]

In the spring of 1968, the two Nobel laureates acknowledged that the field of molecular biology was not yet capable of accomplishing even some of the most minor potential outcomes of genetic engineering. As for the atomic physics analogy, the two suggested that more research and funding were needed before the field was at the stage where atomic physicists found themselves before the dropping of the first atomic bomb. They also emphasized the need for the public to become educated and engaged in biological issues to prevent any potentially dangerous outcomes from human genetic research.

Two years later, Joshua Lederberg once again provided testimony on genetic research, and again he was asked to discuss the prospect of genetic engineering. This time, however, Lederberg brought up the atomic science analogy in a different context. His concern was not genetic manipulation of humans, but of the genetic engineering used to produce biological weapons. "However, whatever pride I might wish to take in the eventual human benefits that may arise from my own research is turned into ashes by the application of this kid of scientific insight for the engineering of biological warfare agents. In this respect we are in somewhat the same position as the nuclear physicists

[442] Ibid., 55-56.

who foresaw the development of atomic weapons."[443] Lederberg was pessimistic because he saw nuclear power as "ironically" a "stabilizing factor" that "established levels of economic and industrial development," whereas "germ power" could only have detrimental effects.[444] During his congressional testimony, he asserted that the potential applications made possible by genetic engineering should be considered by scientists, the public, and policy makers, but cautioned that they were all still in the future because the science was just not yet advanced enough. More basic genetics, such as understanding of developmental pathways, had to be funded and researched before genetic engineering at the molecular level as even remotely possible.[445]

By 1973, the "pre-Hiroshima condition" analogy had real resonance because molecular biologists successfully altered the genetic structure of an organism for the first time with surgical precision. It was akin to the breakthrough when atomic physicists were first able to split the atom. The differences, however, did not go far beyond the manipulation of the structures at their most basic level.

Though the theories of nuclear fission were discussed openly among scientists in the 1930s, the basic science leading to the first nuclear chain reaction and the creation of the first atomic bomb was top secret, overseen by the U.S. Army, and under the full control of the federal government. The government control of the Manhattan Project and

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[443] US Congress. House. Subcommittee of the Committee on Appropriations, *Departments of Labor and Health, Education, and Welfare Appropriations for 1971*, 91st Cong., 2nd sess., 1970, 924.
[444] Ibid.
[445] Ibid., 943. An example of what information was still unknown in a developmental pathway was understanding what genes are involved in diabetes, "factoring them out one by one, being able to anticipate what the heritage of a particular child is, and learning what are the therapeutic measures or the prophylactic measures, that will prevent the manifestation of the disease in later like." [*Departments of Labor and Health, Education, and Welfare Appropriations for 1971*, 943-44.]

future control of nuclear energy also provided defined leadership structure, bureaucracy, and (for better or worse) publicly identifiable scientists and government personnel, such as Robert Oppenheimer, Leslie Groves, and Edward Teller. The majority of recombinant DNA research was federally funded, though not top secret and with little national control, and there was little to no concerted effort for leadership hierarchy. And though the molecular biologist working with recombinant DNA expressed real concern that "a real disaster" might occur "if one of the agents now being handled in research should in fact be a real human cancer agent."[446] This, however, paled in comparison to the private actions and soul searching of the early atomic physicists.

In the year before the first atomic bomb was dropped, Leo Szilard, who worked out the science for the chain reaction at the Metallurgical Laboratory in Chicago and left physics for molecular biology in the late 1940s, began actively discussing the consequences of using an atomic bomb with other atomic scientists.[447] He was a signer of the Frank Report,[448] sought personal meetings with Presidents Franklin D. Roosevelt and

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[446] Wade, *The Ultimate Experiment: Man-Made Evolution*, 30.

[447] For more on the life and science of Leo Szilard see: William Lanouette and Bela A. Szilard, *Genius in the Shadows: A Biography of Leo Szilard: The Man Behind the Bomb*, University of Chicago Press ed. (Chicago, IL: University of Chicago Press, 1994), Leo Szilard, Spencer R. Weart, and Gertrud Weiss Szilard, *Leo Szilard, His Version of the Facts: Selected Recollections and Correspondence* (Cambridge, MA: MIT Press, 1978). For more on the creation of the atomic bomb see also: Richard Rhodes, *The Making of the Atomic Bomb* (New York, NY: Simon & Schuster, 1986).

[448] The Frank Report was named for James Frank, who the head of the committee that wrote the report in early June 1945 that concluded that the atomic bomb should not be used, and either it should be publicly tested as a demonstration or it should be kept its existence secret for as long as possible. It also predicted a nuclear arms race. The signers of the report included Frank, Donald Hughes, J. J. Kickson, Eugene Rabinowitch, and Leo Szilard. The report was delivered on June 12, and a separate committee concluded that the atomic bomb should be used against Japan. Bombs were subsequently dropped on Hiroshima and Nagasaki on August 6 and 9, 1945, respectively. Daniel J. Kevles, *The Physicists: The History of a Scientific Community in Modern America* (Cambridge, MA:

Harry S. Truman, traveled all night to meet with Secretary of State designate James Byrnes, and gathered the signatures of sixty-seven other Manhattan Project all scientists in an attempt to prevent the use of the atomic bomb. The petition, written and sent in July 1945, was his last attempt to appeal to the president on moral grounds to describe and demonstrate the power of the atomic bomb to the public before it is used.[449] Although Edward Teller did not sign the petition, he sympathetically wrote to Szilard:

> "I do not feel that there is any chance to outlaw any one weapon. If we have a slim chance of survival, it lies in the possibility to get rid of wars…Our only hope is in getting the facts of our results before the people…The accident that we worked out this dreadful thing should not give us the responsibility of having a voice in how it is to be used. This responsibility must in the end be shifted to the people as a whole and that can be done only by making the facts known…But I feel that I should do the wrong thing if I tried to say how to tie the little toe of the ghost to the bottle from we just helped it to escape."[450]

Despite the occasionally vast difference in the situation between the scientists concerned with recombinant DNA and the pre-bomb atomic physicists, the metaphor endured. The lack of government control, however, provided the molecular biologists with an opportunity that atomic physicists never had. They had the opportunity to decide the how recombinant DNA experimentation would progress and offered the opportunity to permit the public have input. Though free to have open and frank discussions about recombinant DNA, the scientists also needed to limit discussion in order to determine if they could safely proceed with their research. There was no invisible hand of the government

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Harvard University Press, 1995), 335-36, Brian VanDeMark, *Pandora's Keepers: Nine Men and the Atomic Bomb*, 1st ed. (Boston, MA: Little, Brown, 2003), 154-59.

[449] Truman never saw the petition. VanDeMark, *Pandora's Keepers: Nine Men and the Atomic Bomb*, 159-66.

[450] Szilard, Weart, and Szilard, *Leo Szilard, His Version of the Facts: Selected Recollections and Correspondence*, 208-09.

prodding or protecting their research. The scientists were left to debate how to proceed. Though the press became actively engaged in the issue for the first time during the late summer of 1974, the debates that began in 1971 continued within the scientific community throughout the moratorium.

The Reactions from the Lab

In the *Science* issue that carried the CRDM report, Nicholas Wade wrote the article explaining the report, which included a few initial reactions from scientists. While there was general agreement in deterring Type I and Type II experiments, the scientists that were interviewed did have some outstanding questions about the report. The majority of the critiques centered on the second recommendation of the report, which was soon referred to as Type III experiments. Type III was the linking of animal DNA to plasmids or bacteriophage "should be carefully weighed" because they may contain sequences similar to those of the RNA of tumor viruses.[451] Some scientists wondered why the moratorium was not extended to Type III or why it did not propose a safe bacterial vector for carrying out the experiment. Other scientists, who thought that Type III experiments posed no risk, questioned why it was even included on the report. Donald Brown was of this latter group. Of this latter group was Donald Brown, a scientist at the Carnegie Institution of Washington and member of the NAS, who planned to carry out an experiment where the silk producing genes from a silk moth into bacteria. Brown dismissed claims that his experiment might result in a "gut-full of silk" by explaining that

[451] Berg et al., "Potential Biohazards of Recombinant DNA Molecules."

the silk genes would reproduce in the bacteria, but the protein coded for by the gene would not be produced.[452]

Joshua Lederberg voiced a final criticism of the report that would become a continual refrain at the Asilomar Conference. Though agreeing with the substance of the report, Lederberg worried that "there is already such momentum toward the regulation of research that the proponents should carefully consider the consequences of such a recommendation." Lederberg was essentially concerned about the potential conversion of the report into an official regulatory policy or law unduly burdensome to scientists working with recombinant DNA. On the opposite side of the issue was Jonathan Beckwith who stated that he was "happy to see this precedent set because it will raise a debate about academic freedom to pursue whatever research one wishes." Though Wade did not further explore Beckwith's statement beyond offering it as a counter to Lederberg, it did provide an example of an early critique of the limited boundaries of the recombinant DNA debates. For radical scientists,[453] experiments should not be carried out for the sake of science and scientific knowledge, but should be directed for the betterment of society. Beckwith was pleased because the less control the scientists had on the debate the more the public could become engaged.[454]

In his article, Wade provided a balanced view of the CRDM report and the initial reaction from scientists, though he did link recombinant DNA to genetic engineering – a topic not addressed by either the CRDM committee or any scientists he interviewed. Wade stated that recombinant DNA was "a major step towards genetic engineering, since

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[452] Nicholas Wade, "Genetic Manipulation: Temporary Embargo Proposed on Research," *Science* 185, no. 4148 (1974): 332-33.
[453] For the origins and early organizations of radical scientists see Chapter 3.
[454] Wade, "Genetic Manipulation: Temporary Embargo Proposed on Research," 333.

it renders genes accessible and manipulable in a way that has been impossible hitherto." Wade stayed clear of a brave new world hypothesis and dealt only with the "practical" application of creating insulin. He did acknowledge that genetic engineering was not the reason for the embargo because the report was "quite narrowly focused on the specific health hazards potentially raised by genetically altered bacteria." He concluded that the CRDM created a report that was "framed so as to command the maximum possible agreement among the scientific community." The general agreement to uphold the moratorium was successful. After the first few weeks passed, however, more questions were raised about the moratorium.[455]

On August 6, Roy Curtiss, a microbiologist at the Medical Center of the University of Alabama, Birmingham, sent a memorandum to all the members of the CRDM. As a specialist in bacteria -- how bacteria colonize, invade, and infect organisms -- Curtiss wrote to "argue for the broadening of the definitions of both Type 1 experiments (to include construction of all hybrid bacterial plasmids regardless of the known or unknown phenotypic traits specified) and Type 2 experiments (to include construction of all viral-viral and viral-plasmid hybrids regardless of whether the viruses come from bacteria, plants or animals and whether the plasmids come from prokaryotic or eukaryotic organisms) and strongly urge a temporary ban on Type 3 experiments." He also suggested the creation and "cessation" of Type IV experiments, which were the creation of recombinant DNA molecules containing DNA "one species to another that is not a normal host for such DNA" that might result in a "new pathogenic organism" that could transmit the trait to future generations. Curtiss's sixteen-page single-spaced memo

[455] Ibid.: 333-34.

proceeded to provide the scientific rationale for his proposed changes to the report, suggested future "genetic engineering" research provided a restatement of his recommendations and new definitions, and included an extensive section on ways "for dealing with biohazards associated with genetic engineering research."[456]

The scientific rationale Curtiss provided came from his expertise in infectious bacteria, and included his own research with plasmid in different bacteria including a *streptococucus* mutant and *E. coli*. He also provided an overview of the virulence of *E. coli*, though he never specifically addressed the pathogenicity of laboratory strains, such as K12. Curtiss also defined Type IV experiments and redefined Types I, II, and III experiments and by doing so created a hierarchy of experiments according to potential hazard. The definitions of the experiments provided more specificity than those of the report, which also enabled them to remain "unaltered" by the collection of more data on potential hazards. His rationale for doing this was "based on a desire to see that important areas of genetic research continue when biohazards are minimal or nonexistent." Not surprisingly, the next section of the memo contained his recommendations for biohazard safety, which sounded strikingly similar to those of made by microbiologists who worked with infectious agents at the 1973 biohazards conference – the proper training of lab personnel, disposal methods of recombinant molecules, and use of physical containment

[456] "Roy Curtiss III to Paul Berg, David Baltimore, Herbert W. Boyer, Stanley N. Cohen, Ronald W. Davis, David S. Hogness, Daniel Nathans, Richard Roblin, James D. Watson, Sherman Weissman, and Norton Zinder, 6 August 1974," (ALS: D Med: Recombinant DNA Molecules, Organizing Com for int'l Conf on, Report, Synthetic Nucleic Acids, 1974-1975; Central Policy Files; NAA.).

equipment. Perhaps the most interesting section of the memo was what Curtiss saw as potentially beneficial uses of recombinant DNA.[457]

Despite not providing a definition of genetic engineering, Curtiss stated that he was an "advocate and not opponent" of the research. He provided a clear delineation of what genetic engineering research he would endorse, which included research in medical, agriculture, and industrial fields. Medically, research leading to "better understanding of genetic regulation" of eukaryotes might provide insight into cancer, and the use of microbes to produce insulin and other hormones. In agriculture, the ability to genetically alter plants could increase their yield and nutrition. Production of biological compounds by industry could be used to reduce water pollution and the creation of "effective vaccines." It is important to note that Curtiss's genetic research proposals avoided any mention of a direct application to humans, such as genetic surgery or Lederberg's "genetic medicine." The use of the term genetic engineering without definition in a personal communication between scientists was interesting because it demonstrated scientists used the term freely in private communications, but did not use it in either the CRDM report (or any of its drafts), or the Singer-Söll letter. Both the report and letter used technical scientific language with terms that could not be potentially misconstrued.[458]

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[457] Ibid, Alfred Hellman, Michael. N. Oxman, and Robert Pollack, *Biohazards in Biological Research* (Cold Spring Harbor, NY: Cold Spring Harbor Laboratory, 1973).
[458] "Roy Curtiss III to Paul Berg, David Baltimore, Herbert W. Boyer, Stanley N. Cohen, Ronald W. Davis, David S. Hogness, Daniel Nathans, Richard Roblin, James D. Watson, Sherman Weissman, and Norton Zinder, 6 August 1974.", Berg et al., "Potential Biohazards of Recombinant DNA Molecules.", Maxine Singer and Dieter Soll, "Guidelines for DNA Hybrid Molecules," *Science* 181, no. 4105 (1973). For Lederberg see Chapter 4 and Joshua Lederberg, "Government Is Most Dangerous of Genetic

In his concluding remarks, Curtiss was cautiously optimistic about the "safe resumption" of recombinant DNA research moving forward. Specifically, he was pleased that the CRDM was starting to formulate its own regulations before the government could establish "restrictive regulations" as they had on radiation hazards and human or animal research. He concluded by praising the CRDM for creating the "highly responsible and unselfish actions" that permitted the opportunity for scientists to "take a responsible stand that will establish guidelines to permit this research to be conducted in a safe and beneficial way in the absence of unwarranted restrictions that would make this research impossible."[459]

The August 6 Curtiss memo received wide distribution as Berg sent it to other scientists, including Donald Brown, who would be interested in Curtiss's thoughts on the matter.[460] Curtiss also sent the memo to the Philip Hander, president of NAS, and Robert Stone, Director of NIH, along with carbon copies of selected responses from other scientists to keep them abreast of private discussions between scientists.[461] There was general agreement with Curtiss' proposals, albeit by some scientists requested for more information and other scientists requested clarification on experiments that should be postponed. The issue of regulation was not brought up by any of the respondents to Curtiss, but Lederberg addressed it in a series of correspondence with Stone.

Engineers," *The Washington Post*, July 19 1970, 914-50, *Departments of Labor and Health, Education, and Welfare Appropriations for 1971*.

[459] "Roy Curtiss III to Paul Berg, David Baltimore, Herbert W. Boyer, Stanley N. Cohen, Ronald W. Davis, David S. Hogness, Daniel Nathans, Richard Roblin, James D. Watson, Sherman Weissman, and Norton Zinder, 6 August 1974."

[460] Ibid.

[461] "Roy Curtiss III to Philip Hander, 5 September 1974," (ASSEM: Life Sciences: 1974: Com on rDNA Molecules: Ad hoc; Central Policy Files; NAA), "Roy Curtiss III to Robert Stone, 25 September 1974," (Res 15-2, Recombinant DNA Research & Regulation, 1970-1975; NACP-NIH).

Lederberg correspondence in late August 1974opened by commending the "major thrust" of the CRDM and then proceeded to criticize the assertion made by the press that "self-regulation in science has little or no precedent."[462] The assertion upset Lederberg because it made a false implication about the social responsibility of scientists and therefore created the potential for some "undesirable side-effects." He fervently believed that it was a fact that science, like every other activity, was "sharply constrained by social sanctions of man, many kinds," and that it was a fallacy to believe that scientists "pursue every experiment" regardless of the outcome. To believe this would imply that scientists had been "inherently irresponsible in the past and that unless some regulatory framework is laid on they will act like mischievous little boys flying off in all directions throwing firecrackers into gasoline tanks." The potential for regulatory framework really worried Lederberg.[463]

Although he saw the current situation as little different than the institutional safeguards adopted by early bacteriologists handling dangerous microorganisms, Lederberg warned that "very much more stringent [safeguards] that are necessary will be forced upon us partly in consequence of careless language."[464] His solution had two steps. The first was to examine and learn from successful precedents where hazards posed potential risks, in particular the interplanetary contamination and the quarantine safeguards established by NASA. This, of course, was something that Lederberg in the

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[462] The phrase "self-regulation in science has little or no precedent" does not appear in the CRDM draft of final reports, though it was a common assertion made by and about the CDRM. "Joshua Lederberg to Robert Stone, 23 August 1974," (Res 15-2, Recombinant DNA Research & Regulation, 1970-1975; NACP-NIH). Emphasis in original.
[463] Ibid.
[464] Ibid.

1950s played a large role in creating.[465] The second step was to solve the "careless" "enforcement and compliance" of the moratorium. Lederberg was concerned that a recombinant DNA experiment would create a real biohazard that would cause a reaction beginning with public "over-reaction" and ending with a "smothering blanket" of policies that went "far beyond any needs that are reasonable for the problem intended to be addressed." Lederberg offered a simple solution to the second step, "we must look systematically for means to police such nefarious activities in an ever widening area." Just like Curtiss, Lederberg permitted input from multiple sources. Lederberg sent carbon copies of the letter to CRDM members Paul Berg, David Hogness, and Stanley Cohen, and Arthur Kornberg. Although he worried about a crippling policy enforced upon scientists, Lederberg did not provide any realistic solutions beyond more careful wording of published documents concerning recombinant DNA.[466]

A month later Stone sent a rather muted reply. While agreeing that scientists do take the risks of society into consideration, Stone provided an example where scientists had ignored a memorandum of understanding (MUA), and thus negated Lederberg's assertion that external pressures are not needed to constrain them. The problem with the

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[465] In particular Lederberg was proud of his work in helping to influence Article 9 of the 1967 United Nations Treaty on the Principles Governing the Activities of States in the Exploration and Usage of Outer Space, including the Moon and Celestial Bodies. The section of Article 9 on potential contamination states: "States Parties to the Treaty shall pursue studies of outer space, including the moon and other celestial bodies, and conduct exploration of them so as to avoid their harmful contamination and also adverse changes in the environment of the Earth resulting from the introduction of extraterrestrial matter and, where necessary, shall adopt appropriate measures for this purpose." The initial signatories of the treaty were the United States, United Kingdom, and the Soviet Union. The treaty still serves as a basis for international space law, and is today referred to as the "Outer Space Treaty." [United Nations Office for Outer Space Affairs, "Outer Space Treaty," United Nations, http://www.unoosa.org/oosa/SpaceLaw/outerspt.html.]

[466] "Joshua Lederberg to Robert Stone, 23 August 1974."

creation of recombinant DNA molecules was one of proper safety training. “The techniques for producing them are becoming so well worked out that they can be used in laboratories where the expertise and experience in related disciplines is not available to handle them properly.” Echoing sentiments expressed since the first debates of recombinant DNA, Stone wrote that some scientists seemed to not “understand the biology of infectious organisms or the principles of epidemiology of infectious disease.” Stone’s solution to this problem was the creation “institutional safeguards,” such as voluntary compliance with newly created MUAs for “specific agents” rather than “sanctions or enforcement.” Stone also hoped that the NIH committee and international meeting would develop guidelines for any “potentially hazardous agents.” The response by the director of NIH was consistent with the framework of previous recombinant DNA debates: it was a purely scientific issue of hazards that could be corrected by proper training and laboratory equipment.[467]

Less than a week after receiving Stone’s response, Lederberg responded with a short letter. He again expressed concern over the use of the word “unprecedented” in headlines to describe the actions of the CRDM. He worried on “hyperbole” provoking public reaction and restrictions on recombinant DNA. With no more communication from Stone, Lederberg, like other scientists waited until the Asilomar Conference to address issues relating to recombinant DNA research.[468]

The scientists tabled their debates about recombinant DNA until the Asilomar Conference in late February 1975. The press also remained largely dormant during the

[467] "Robert Stone to Joshua Lederberg, 5 September 1974," (Res 15-2, Recombinant DNA Research & Regulation, 1970-1975; NACP-NIH).
[468] "Joshua Lederberg to Robert Stone, 10 September 1974," (Res 15-2, Recombinant DNA Research & Regulation, 1970-1975; NACP-NIH).

fall and early winter of 1975. Despite this fact, some scientists privately worried about the potentially negative tone the press and public took about recombinant DNA. Bernard David of the Harvard Medical School expressed this sentiment most clearly in a letter to Philip Hander. “But I am concerned --- and I’m not sure you are too --- that the expected public outcry stirred up by others may prove to be more than we can comfortably accept.”[469] The two knew, however, that the biggest problem for recombinant DNA research would be in trouble if either the NAS in preparing the conference or the NIH in creating an advisory committee failed.

[469] "Bernard Davis to Philip Hander, 27 September 1974," (Public Relations, General 1974; Central Policy Files; NAA).

Chapter 8: Organized Chaos

In the month following the initial news reports on the Committee on Recombinant DNA Molecules (CRDM) July 18, 1974 press conference, four groups reacted simultaneously: the press, scientists, the National Institutes of Health, and the National Academy of Sciences. As discussed in Chapter 7, the press worked to explain recombinant DNA to their readers. The scientists continued their research with the exception of the experiments identified in the report and they began publicly and privately their concerns about recombinant DNA and the moratorium. While this was occurring with the press and scientists, the National Academy of Sciences (NAS) began their critical work on Recommendation 4 of the CRDM report, the proposed international conference scheduled for early 1975. And the National Institutes of Health (NIH) began formalizing Recommendation 3, the recombinant DNA committee to evaluate the "potential biological and ecological hazards."[470]

While prior histories of recombinant DNA have downplayed the fulfillment of Recommendations 3 and 4 as inevitable, in truth, their success at times during the moratorium was far from certain. While the journey towards successful fruition of the Recommendations occurred simultaneously one was undeniably more important. The international conference was the most critical because the NIH advisory committee could not hold their first meeting until after the conference. The NIH committee would rely on the recommendations of the conference to help formulate national policy. The success of both of the Recommendations formed the intellectual and regulatory foundation that

[470] Paul Berg et al., "Potential Biohazards of Recombinant DNA Molecules," *Science* 185, no. 4148 (1974).

enabled creation of the first recombinant DNA guidelines, which were followed with universal agreement by the scientific community.

Recommendation 4: The NAS and the organizing of the Asilomar Conference

Recommendation 4 of the CRDM report suggested the convening of an international meeting early in 1975 to "review scientific progress" and "further discuss appropriate ways to deal with the potential biohazards of recombinant DNA molecules."[471] Immediately after the July 1974 CRDM press conference, the NAS and the NIH negotiated a contract for the international conference, which included a "review of scientific progress and a discussion of appropriate ways to deal with the potential biohazards resulting from the techniques of molecular biology and microbial genetics." Initial cost estimates were $65,000 for pre-conference meetings, administrative costs, and travel and per diems for about fifty scientists, of which fifteen to twenty were from foreign countries. Funding came from the NAS, the NIH, the National Cancer Institute, and the National Science Foundation (NSF). The initial contract specified a very small conference with the same central focus of biohazards.[472]

By late July 1974, the NIH and the NAS agreed on a division of labor for carrying out the CRDM Recommendations. The NAS handled the details of planning the Asilomar Conference (the recommended international conference) and the NIH concentrated on

[471] Ibid.
[472] "Leon Jacobs to [Acting Chief, Research and Contracts Branch], 22 July 1974," (Res 15-1-D, NIH/NAS Asilomar Conference Feb. 23-27, 1975, 1974-1975; NACP-NIH).

creating the committee recommended in the CRDM report. With the NAS in charge of the Asilomar Conference, planning for the conference began in earnest.[473]

On August 23, S.D. Cornell, the Acting Executive Director of NAS, sent a cover memo for the formalized budget for the conference to Bernard Kropp, the Business Manager, requesting his signature for the detailed budget proposal. As the memo made clear, this was not a traditional request. “Because of the urgent nature of the subject matter, authorization by the Governing Board has not been received. Dr. Handler is well aware of the matter and urged that the conference be held as soon as feasible.” Once again, as with their delayed official recognition of the CRDM and their initial recommendations, matters of recombinant DNA did not follow usual NAS formalities.[474]

The budget for the Asilomar or CRDM requested conference, after input from Paul Berg, David Baltimore, and James Ebert, was submitted to President Philip Handler in late August. The identically worded agenda adopting language from the NAS-NIH contract in July focused on biohazards. The exact date was no more specific than February 1975, and a potential location and prospective number of participants were absent. The form of the conference, however, was taking shape. The three and a half day conference would include four sessions – viral DNAs, animal cell DNAs, plasmids and bacteriophages – and one final session to consider adoption of a statement. It was “hoped” that the statement would “indicate mechanisms for overcoming potential hazards so that all types of experiments could be considered safe in the future.” The budget

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[473] "James Ebert to Albert Vosburg, 30 July 1974," (ALS: D Med: Recombinant DNA Molecules, Organizing Com for Int'l Conf on, Report Inception 1973-1974 [ARMN-91 P77-004]; Central Policy Files; NAA).

[474] "S. D. Cornell to Bernard L. Kropp, 23 August 1974," (International Relations: Internatl Conferences: Recombinant DNA Molecules: Organizing Com, 1975; Central Policy Files; NAA.).

estimate for the conference grew to $74, 300, and included pre-planning, administrative costs, and travel expenses.[475]

Planning Asilomar

On August 27, 1974 David Baltimore sent out a memo to eleven scientists inviting them to a September 10 meeting at MIT to "flesh out the format and program for the February Asilomar meeting on hazards involving recombinant DNA molecules."[476] (The exact date of the selection of Asilomar as a location of the conference is unknown, though Berg was familiar with the location because of its proximity and use by Stanford University as a site for conferences, as well as personally when he organized and chaired the 1973 biohazard conference.) Of the eleven scientists invited to the September 1974 meeting, five were members of CRDM (Baltimore, Paul Berg, Richard Roblin, Sherman Weissman, and Norton Zinder), one was a member of the initial NAS study group (Herman Lewis), one previously discussed Berg's initial recombinant DNA experiment (Maxine Singer), one was a member of the NAS Assembly of Life Sciences (ALS) (Albert Vosburg), one was abstaining from a Type III experiment (Donald Brown), one was involved in public health (Richard Novick, Public Health Research Institute (NY)), and the other was from industry (Aaron Shatkin, Roche Laboratories). On September 9,

[475] "Proposal for International Conference on DNA Recombinants, 21 August 1974," (International Relations: Internatl Conferences: Recombinant DNA Molecules: Organizing Com, 1975; Central Policy Files; NAA.).
[476] "David Baltimore to Paul Berg, Don Brown, Herman Lewis, Richard Novick, Dick Roblin, Aaron Shatkin, Maxine Singer, Albert Vosburg, Sherman Weissman, and Norton Zinder, 27 August 1974 ", (ALS: D Med: Recombinant DNA Molecules, Organizing Com for Int'l Conf on, Report Inception 1973-1974 [ARMN-91 P77-004]; Central Policy Files; NAA).

the ALS signed off on the official contract for the Asilomar Conference.[477] The following day the planning committee for the Asilomar Conference met for the first time with one substitution – William Gartland (NIH) replaced Vosburg,[478] The final make-up of the planning committee consisted of four representatives from universities, four representatives from governmental institutions, two representatives from private research institutions, and one representative from industry.[479]

The daylong meeting of the planning committee came to eight significant conclusions that proved remarkably accurate for the upcoming conference.[480] The first conclusion was that the committee should be expanded to include two international highly respected molecular biologists, Sydney Brenner (Medical Research Council (MRC), UK) and Niels Jerne (Director, Basel Institute for Immunology, Switzerland and chairman of the European Molecular Biology Organization Council (EMBO)).[481] The next two conclusions concerned the post-conference report. A statement or "meeting report" should be published summarizing the meeting, and the NIH advisory group would need to get approval from the Department of Health, Education, and Welfare and the

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[477] "S. D. Cornell to Philip Handler, 10 September 1974," (ALS: D Med: Recombinant DNA Molecules, Organizing Com for Int'l Conf on, Report Inception 1973-1974 [ARMN-91 P77-004]; Central Policy Files; NAA).

[478] "Notes on Planning Meeting for Asilomar Conference (MIT), 10 September 1974," (ALS: D Med: Recombinant DNA Molecules, Organizing Com for Int'l Conf on, Report Inception 1973-1974 [ARMN-91 P77-004]; Central Policy Files; NAA). There is no record why Vosburg as the official designate of the ALS did not attend. Gartland, however, provided the planning committee with an additional NIH member as they were creating a recombinant DNA advisory committee per CRDM report.

[479] Though Public Health Research Institute is a private non-profit it was founded and funded by New York City to provide basic research on infectious diseases. For this reason I classified the Institute as a governmental agency.

[480] The most noticeable, though small, error was the planners initially thought the conference would be held February 2-5. The conference occurred February 24-27.

[481] The Nobel Prize in Physiology or Medicine was awarded to both men, Brenner in 2002 and Jerne in 1984.

Office of Management and Budget. This meant there would be an "indeterminate (but hopefully short) wait" before experiments could resume.

The international dimension of the conference was also the focus of two other conclusions. The first was the "speed" and "importance" that the British had responded to the CRDM report. By late July, the Advisory Committee to the Medical Research Council (UK) had formed a committee under the leadership of Lord Ashby to examine the British position on recombinant DNA safety with a report expected that winter.[482] The other international component was suggestions about which international scientists should be invited and what organizations should be represented.[483]

The final three conclusions all dealt with the organization of the conference. The first was the governing idea of the conference – an issue already partially decided from the goals set forth in the CRDM report. "It will be important to consider conditions under which experiments can safely go forward (containment) and the design of tests which can assess safety or hazard." The remaining two conclusions were the initial plans for the conference. The plan for the Asilomar Conference closely resembled almost any

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[482] Though commonly known as the Ashby Committee, their proper name was the Working Party on the Experimental Manipulation of the Genetic Composition of Micro-Organisms. For more and the origins and composition of the Ashby Committee and its predecessor, the Godber Committee, see Susan Wright, *Molecular Politics: Developing American and British Regulatory Policy for Genetic Engineering, 1972-1982* (Chicago, IL: University of Chicago Press, 1994), 140-44. For the Ashby Committee see also Donald S. Fredrickson, *The Recombinant DNA Controversy: A Memoir: Science, Politics, and the Public Interest 1974-1981* (Washington, D.C.: ASM Press, 2001), 30-31.

[483] "Notes on Planning Meeting for Asilomar Conference (MIT), 10 September 1974." David Baltimore's initials appear on the center top of the first page of the typed notes. The records do not indicate if he served as the secretary of the meeting or if he was tasked with organizing and then distributing summary notes for the meeting. The latter seems more plausible as Baltimore, and not Berg, sent out the invitation memo and the meeting was held at MIT – where Baltimore was a professor.

scientific conference. Centered around the governing idea of biohazard and risk assessment, the plans were for four sessions containing few "(1-3)" formal scientific talks with panel discussion, with an open session for adoption of a statement on the final morning. The sessions were arranged with one following breakfast (with the mid-morning coffee break), the conclusion of the first or start of the next session after lunch (with the mid-afternoon break), dinner, and then an evening session.[484]

As the Committee foresaw it, the conference would begin with opening remarks and then an overview of recombinant DNA technology, followed by a break and a session on bacteria and plasmid that would extend through the remainder of the day. The second day would complete plasmids discussion in the morning and would be followed by the complete session on "joining viral genes to plasmids." The final full day would have a morning session on the "joining of animal or plant genes to plasmids," and an afternoon session on "involvement of non-scientists in decision-making." Though lacking any detail beyond the prospective title, the planning committee from the initial stages of planning Asilomar recognized the need for the scientists at the conference to hear the views of those outside the scientific sphere. The committee reserved the final night for open discussion, followed the next morning by the adoption of the conference statement.[485]

The planning committee also agreed that there was a need for an examination of scientific and technical information, as well as, potential biohazard risks before the conference, so that that the information could be presented in a manner that helped guide the conference towards a consensus opinion. The solution for this pre-conference

[484] Ibid.
[485] Ibid.

examination was the formation of three working groups. The working groups would meet prior to the conference to discuss issues, decide on speakers and panel members for their session, and create a position paper to distribute at the conference. The three working groups were entitled the Plasmid Working Group (PGW), the Eukaryote Working Group (EWG), and the Viral Working Group (VWG).[486]

The proposed international conference's shape was dramatically different from the 1973 Biohazard conference. The proposal for Asilomar called for it to be dominated by technical sessions on recombinant DNA experimentation, but it was going to include non-scientists and have a public consensus statement. The non-scientists were not invited to be neutral observers. They were deemed from the initial planning to have an important role in shaping the conference statement.

Embarrassment and Coordination at NAS

Despite the CRDM report, the resulting press conference, and month-long press coverage on recombinant DNA, the importance of the Asilomar conference remained relatively isolated within the ALS branch of the National Research Council (NRC). In late September, George Hammon, the Foreign Secretary of NAS and Chairman of the Commission on International Relations, was questioned about the upcoming recombinant DNA conference at the International Council of Scientific Unions. Unable to provide much information, he was "embarrassed" to find himself so "uninformed about our plans for an international conference on such an important subject." The state of knowledge of Hammon and other NAS representatives at the Council was "probably at the state at

[486] Sheldon Krimsky, *Genetic Alchemy: The Social History of the Recombinant DNA Controversy*, Third Printing, 1985 ed. (Cambridge, MA: MIT Press, 1982), 106-08.

which the problem was reported to the Governing Board" at their June 8 meeting where any discussion of an international conference or potential NAS role for such a conference was "pretty vague."[487]

In an October 2 memo, an "apologetic" Acting Executive Director Cornell noted that "nothing" about the conference had been sent from the ALS to the NRC since the June 8 Board meeting. The reason for the problems at the Council was the lack of coordination within the NRC. The lack of coordination was addressed and was improving. Cornell also noted without irony that "because of the unusual origins" of the CRDM that there was "no formal approval of the planning committee by the ALS." The Singer-Söll letter arrived on July 19, 1973 and, despite the growing importance of the issue, recombinant DNA was treated within NAS in an ad hoc manner from the initial MIT meeting, through the process of the creation of the CRDM and their report, to the planning of the international conference. The only constant at the NAS was the involvement of the ALS and that President Handler took such a personal interest in the issue.[488]

The Final Stages of Planning for Asilomar

On November 13, 1974 James Ebert had a long phone conversation about the Asilomar conference. The conversation ranged from the members of the Organizing Committee to the size of the conference to the budget. After the conversation, Ebert sent a memo to Thomas J. Kennedy, Jr., the new Executive Director of ALS, with some

[487] "S. D. Cornell to Thomas Kennedy, 2 October 1974," (ASSEM: Life Sciences: 1974: Com on rDNA Molecules: Ad hoc; Central Policy Files; NAA.). For analysis of the June 8 Governing Board meeting see Chapter 6.
[488] Ibid.

recommendations. The first was that "we should proceed promptly to 'formalize' the organizing committee." The committee would include Berg, Baltimore, Brenner, Roblin, Singer, and Jerne. Though not selected for the Organizing Committee, three members of the planning committee that met in September were selected as chairs of the conference's three working groups – Brown (EWG), Novick (PWG), and Shatkin (VWG).

Stanley Falkow, professor of microbiology and immunology at the University of Washington, Seattle, specialized in antibiotic resistance and enteric (intestinal) pathogens, including *E. coli*, would serve as a co-chair with Novick because of the large scope of plasmid experimentation. Falkow was one of the first to use the plasmid method to construct recombinant DNA after Herbert Boyer and Stanley Cohen worked it out. He also had a penchant for public pronouncements. During his tenure at Georgetown in the late 1960s, Falkow sampled the Potomac River for bacterial pollutants, and, having discovered the high levels of pollutions, warned the public: "'On a particularly bad day, one cup of the [river water] is the equivalent to a half gram of feces.'"[489] His co-chair, Novick, also had a history of social activism, as he was the one who presented Edward Teller with Scientists and Engineers for Social and Political Action's "Dr. Strangelove Award" at the 1970 American Association for the Advancement of Science annual meeting.[490]

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[489] Virginia L Miller, James B. Kaper, and Daniel A. Portnoy, eds., *Molecular Genetics of Bacterial Pathogenesis: A Tribute to Stanley Falkow* (Washington, DC: ASM Press, 1994), xvi.

[490] Scientists and Engineers for Social and Political Action (SESPA) was the name of the radical science organization that was soon renamed Science for the People. For more on SESPA, their protests at the AAAS annual meetings, and the Dr. Strangelove Award see Chapter 3.

Daniel Singer selected to organize "a small group which will center its attention on the legal aspects of the problem." The shape of presentations by the three working groups also was refined. The PWG was focusing on "plasmids and drug resistance and the natural history of enteric organisms." The VWG was now dealing with oncogenic (cancer causing) viruses. And the EWG was organized around "cloning eukaryote segments," specifically those of plant and animal cells. By mid-November, the organization of committees and working groups were set pending approval.[491]

Beyond the nominations for committees and working groups, Ebert's memo also explained that Berg wanted to limit the attendance of the conference to 135-150 people because his "main concern" was that it "will grow too large." Berg also intended to make the conference as inclusive as possible to nations with scientific capabilities to perform recombinant DNA experiments. The international reach included sending invitations to scientists and scientific societies in the communist countries of the People's Republic of China and the Soviet Union. As of mid-November, the committee did not anticipated problems with the budget. Two days later the Ebert memo was forwarded on to Albert Vosburg, the person responsible for recombinant DNA within the ALS, for any potential action on the recommendations.[492]

In early December 1974, the CRDM committee sent invitation letters to the Asilomar Conference out to international and domestic scientists. Berg signed he letters on behalf of the CRDM. The letters were printed on his Stanford University Medical

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[491] "James Ebert to Thomas J. Kennedy, Jr., 13 November 1974," (ALS: D Med: Recombinant DNA Molecules, Organizing Com for int'l Conf on, Report, Membership, 1974-1976; Central Policy Files; NAA.).

[492] Ibid. There is no official record of Berg or the NAS extending and official invitation to any scientist or scientific society in the People's Republic of China.

Center letterhead. The invitation included general information about the meeting (location, dates, etc.), as well as the purpose, which was "to review the progress, opportunities, potential dangers and possible remedies associated with the construction, and introduction of new recombinant DNA molecules into living cells." The letter also listed the topics for the program, which were being prepared by the working committees, as well as, the general topic of "approaches to assess, and to minimize or eliminate any serious biohazards stemming from this line of research." The invitation stated that because the conference was limited to a total of 150 people, "we must insist that this invitation not be transferred to someone without our knowledge."[493]

The Invitations are in the Mail

On December 18, George Hammond sent a short memo to Handler, Baltimore, Berg, and Kennedy that included the first official draft of a list of participants for the Asilomar Conference. The list was distinctly informal with common first names, incomplete information, and not addresses. The list included seventy-six American scientists and forty foreign scientists from the UK, Australia, Canada, Switzerland, The Netherlands, West Germany, France, Belgium, Denmark, Italy, Israel, Sweden, Japan, and the Soviet Union. This initial list was noticeable for who was included, as well as,

[493] The record is unclear why Berg sent out the invitation letters on his own letterhead as opposed to ALS, NRC, or NAS letterhead. The National Academies Archive contains two sample letters, one marked "international" and the other marked "U. S. Participants" which are identical in their body demonstrating that the domestic and international letters were the same and that the text of the letters remained constant. The quotes are from "Paul Berg to W. Gayewski, 12 December 1974," (ALS: D Med: Recombinant DNA Molecules, Organizing Com for int'l Conf on, Report, Invitation letters, 1974; Central Policy Files; NAA.), "Paul Berg to Rudolf G. Wanner, 6 January 1975," (ALS: D Med: Recombinant DNA Molecules, Organizing Com for int'l Conf on, Report, Invitation letters, 1974; Central Policy Files; NAA.).

who was absent. By including molecular biology luminaries such as James Watson, Joshua Lederberg, and Matthew Meselson (Harvard University) with strong dissenting voices such as Jonathan Beckwith (Harvard Medical School and Science for the People), Leon Kass (biochemist and professor of bioethics, Kennedy Institute of Ethics, Georgetown University), and Robert Pollack (Cold Spring Harbor Laboratory) on the list, the committed created the potential for diverse opinion. The list also included a few members from government institutions, private laboratories, and industry. Absent from this list were any members of the ALS and NRC, as well as, Herman Lewis and William Gartland – two members of the September planning committee.[494]

By January 10, the final official list totaled one hundred and forty-nine scientists, ninety-three domestic and fifty-six international. This list, which included proper names and addresses, still contained a wide spectrum of molecular biologists, and individuals from other related fields that were actively involved or could become involved in recombinant DNA research. Invitees were from fields that included specialists from genetics, microbiology, biochemistry, public health, epidemiology, pathology, ecology, and embryology. There was a broader range of government institutions invited, such as Los Alamos Scientific Laboratories, as well as more domestic and international industry representation. International invitations mirrored domestic invitations in scientific disciplines, as well as distribution of academic, governmental institutions, and industry.

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[494] "List of Participants for Asilomar, December 1974," (International Relations: Internatl Conferences: Recombinant DNA Molecules: Organizing Com, 1975; Central Policy Files; NAA.). List attached to "George Hammond to Philip Handler, David Baltimore, Paul Berg, and Thomas Kennedy, 18 December 1974," (International Relations: Internatl Conferences: Recombinant DNA Molecules: Organizing Com, 1975; Central Policy Files; NAA.).

A small amount of diversity of opinion was still important on the final invitation list as Beckwith, Kass, and Pollack remained on the list and each received an invitation.[495]

Confusion and approval at NAS

The end of 1974 and the first few weeks of 1975 were an active period within the NAS bureaucracy for the official formalization of the conference. There was also continued confusion within the NAS. On December 11, Handler wrote to Kennedy, wondering "Who – if anyone – is riding herd on the Berg conference re[:] synthetic DNA."[496] Artemis Simopoulos was quickly assigned the position of Staff Officer to the conference and began reviewing the official records. By January 6, Simopoulos, finished reviewing the records for the conference. He wrote a short memo to John Coleman, Executive Officer of NAS, alerting him to the fact that he had "found no written documentation that [the conference] has been approved by the Governing Board."[497] Perhaps unsurprisingly, on the same day a different search for any official paperwork on the Organizing Committee came up empty. The potential problem for the NAS was that without official recognitions of the committee, "it could not be officially disbanded" after

[495] The list is dated January 10, 1975 because that is the date on the list that William Gartland requested. The list is only a list of invitees and does not include the list of those who accepted an invitation to the conference. "Lyn Mortimer to William Gartland, 10 January 1975," (Res 15-1-D, NIH/NAS Asilomar Conference Feb. 23-27, 1975, 1974-1975; NACP-NIH). The total number and breakdown is from the attached list "List of Participants for the International Conference on Recombinant DNA Molecules, January 1975," (Res 15-1-D, NIH/NAS Asilomar Conference Feb. 23-27, 1975, 1974-1975; NACP-NIH).
[496] "Philip Handler to T. J. Kennedy, Jr., 11 December 1974," (ASSEM: Life Sciences: 1974: Com on rDNA Molecules: Ad hoc; Central Policy Files; NAA.).
[497] "Artemis P. Simopoulos to Philip Handler, 9 January 1975," (International Relations: International Conferences: Recombinant DNA Molecules, 1975; Central Policy Files; NAA.).

the conference.[498] During the same period, however, the ALS engaged in officially formalizing the Organizing Committee of the Asilomar Conference. On December 31, Vosburg followed up on Ebert's recommendations from November and sent a memo to Handler for his approval of the six nominations for the Organizing Committee of the Asilomar Conference.[499] Handler approved all of the nominations on January 7, 1975,[500] and the following day Kennedy, on behalf of the NRC and ALS, officially "recognized and formalized" the contributions to by the six approved members of the Organizing Committee by sending them a letter with an acceptance, insurance, and bias form.[501]

Final Preparations

On January 24, 1975, Ann Ferrell, Berg's secretary, sent out the official letter to all the participants of the Asilomar Conference on Stanford University letterhead.[502] The letter provided the basic information for the conference, including travel information,

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[498] "Lyn Mortimer Note, 6 January 1975," (ALS: D Med: Recombinant DNA Molecules, Organizing Com for int'l Conf on, Report, Synthetic Nucleic Acids, 1974-1975; Central Policy Files; NAA.).

[499] The name of the committee was officially the "Planning Committee" at this time, but was officially changed to the "Organizing Committee" in January because that is the term Berg used to describe the committee. See "Artemis P. Simopoulos to Philip Handler, 9 January 1975."

[500] "Albert Vosburg to Philip Handler, 31 December 1974," (International Relations: Internatl Conferences: Recombinant DNA Molecules: Organizing Com, 1975; Central Policy Files; NAA). The memo has an "APPROVED" stamp with Philip Handler's signature and the date.

[501] "Thomas J. Kennedy, Jr. To Paul Berg, 8 January 1975," (ALS: D Med: Recombinant DNA Molecules, Organizing Com for int'l Conf on, Report, Membership, 1974-1976; Central Policy Files; NAA.). The same letter was sent to all members of the Organizing Committee.

[502] Berg recommended and then used Ferrell to work out the logistics with the Asilomar Conference Grounds because of her experience with organizing the 1973 Biohazard Conference at the same location. ["Paul Berg to Albert Vosburg, 12 September 1974," (ALS: D Med: Recombinant DNA Molecules, Organizing Com for int'l Conf on, Report Inception 1973-1974 [ARMN-91 P77-004]; Central Policy Files; NAA.).]

check-in and checkout times, and reimbursement information. Attached to the letter was a tentative conference schedule, and flight schedules for the Monterey airport.[503]

The "outline of the tentative program" was just that.[504] It listed all of the rudimentary titles of sessions as well as their meeting times. There was no definitive listing of who was going to give talks and exactly on what topics for the sessions. The only session that changed its focus slightly was the VWG, which focused not specifically on oncogenic viruses, but on the more general topic of "opportunities and problems in constructing and cloning recombinant DNAs containing animal virus DNAs." Missing from the letter was any description of what was going to occur each day between the end of lunch (1:00pm) and beginning of the afternoon session at 4:30pm. Perhaps the greatest reason for the vagueness of the program was that the working groups had not had enough time to meet and formalized their own working papers and thus the central idea of their sessions.[505]

An early 1975 list of participants consisted of the one hundred and forty-seventy scientists in addition to the six members of the organizing committee.[506] There were a few substitutions with Berg's approval made by invitees who could not attend. Domestically the majority of invitees were members of the NIH recombinant DNA advisory committee. The committee was not finalized until the majority of the invitations

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[503] "Ann Ferrell to All Conference Participants, 24 January 1975," (Res 15-1-D, NIH/NAS Asilomar Conference Feb. 23-27, 1975, 1974-1975; NACP-NIH.).
[504] Ibid.
[505] "Tentative Program for Asilomar Meeting on Recombinant DNA Molecules, January 1975," (Res 15-1-D, NIH/NAS Asilomar Conference Feb. 23-27, 1975, 1974-1975; NACP-NIH.).
[506] "Artemis P. Simopoulos to John S. Coleman, 6 January 1975," (International Relations: International Conferences: Recombinant DNA Molecules, 1975; Central Policy Files; NAA.).

were sent. International substitutions were mainly among country representatives, including four Soviet scientists. Absent from the participant list were Kass, Beckwith, and Pollack. Beckwith did nominate a postdoctoral candidate who was a member of Science for the People. Berg rejected the nomination because the nominee would be there primarily as a spokesman who may hold an alternative view from the majority of the conference. The lack of strong opposing views seemed to leave the conference ripe for criticism if the conference appeared to reach a predetermined conclusion about progressing with recombinant DNA research without considering the social implications.[507]

There was one last problem that Berg ran into before the conference. In late January 1975 he realized that the indirect costs for the NAS were much greater than he had originally thought when the contract was drawn up in July. By early February he was scrambling for additional funds to cover the over commitment on travel expenses by $18,000. Berg contacted Frank Rausher, the director of the National Cancer Institute, first by phone and then by letter expressing "sincere appreciation" for his support in the request of $25,700 to cover the travel overrun and its indirect costs. The supplemental money was quickly granted, and with that the last hurdle was cleared before the conference.[508]

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[507] Krimsky, *Genetic Alchemy: The Social History of the Recombinant DNA Controversy*, 110-11.

[508] "Paul Berg to Frank J. Rausher, 7 February 1975," (Res 15-1-D, NIH/NAS Asilomar Conference Feb. 23-27, 1975, 1974-1975; NACP-NIH.).

Recommendation 3 and the NIH

In selecting the National Institutes of Health to "establish an advisory committee" for recombinant DNA,[509] the CRDM shifted the onus of creating guidelines away from the insular NAS and to a governmental entity within established record of creating guidelines. The NIH was also more responsive to the public. As a government agency, the NIH restricted how fast the bureaucratic machinery could move to establish an advisory committee. As mentioned in chapter six, Handler sent Robert Stone, Director of NIH, the CRDM report before its public released, and Stone assured Handler that the NIH was going to set up the recommended committee. The reason Stone decided so quickly to establish the committee was because the NIH was actively involved in the financial "support and conduct of research on DNA recombinants." Stone also saw a parallel between recombinant DNA and the guidelines that they were developing for hazardous microbiological agents that would assure "containment of such agents while at the same time encouraging the remarkably able work of molecular biologists, virologists, and geneticists who are pushing forward at the leading edge of science." The NIH immediately began formulating the Recombinant DNA Advisory Committee (RAC).[510]

Due to the fact that the NIH was a federal agency, the 1972 Federal Advisory Committee Act (FACA) bound any national advisory committee they attempted to establish. The main objective of the act was to provide public access to the committees, although it also formalized the process of creating, managing, and terminating the

[509] Berg et al., "Potential Biohazards of Recombinant DNA Molecules."
[510] "Robert Stone to Philip Handler, 17 July 1974," (ASSEM: Life Sciences: 1974: Com on rDNA Molecules: Ad hoc; Central Policy Files; NAA).

committees. The bureaucracy of the Executive Branch also placed an extra burdened on the NIH in creating a committee. Once out of the NIH, the committee would need to clear the desk of the surgeon general, and then through the Byzantine hierarchy and legal offices of the Department of Health, Education, and Welfare. The potential of White House involvement always loomed in further slowing down any request.[511]

In late July 1974, the NIH began outlining the goals of the advisory committee to address "broader issues of man-made recombinants which have potential for colonization of man."[512] The NIH believed that the eventual recommendations would intensify the drive for better biohazard controls in microbiology and genetics research. The NIH also contacted a large number of scientists for input and consultation on the science of recombinant DNA and the potential biohazards. The NAS understood the NIH to be organizing a single committee to advise several Institutes and "indirectly" the public on "appropriate protocols for experiments to eliminate biohazards."[513] By early August, it was decided that the committee "should be advisory to the Director, NIH" rather than any other entity on campus, NIH veterans Leon Jacobs and Stephan Schiaffino began soliciting recommendations for members for the committee, and a charter for the committee was started.[514]

In a late August, Stone wrote to Handler to update him on funding for the Asilomar Conference and that the formation of the advisory committee was proceeding in

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[511] Fredrickson, *The Recombinant DNA Controversy: A Memoir: Science, Politics, and the Public Interest 1974-1981*, 32-33.
[512] "John R. Seal to Dorland J. Davis [Director, NIAID], 22 July 1974," (Res 15-2, Recombinant DNA Research & Regulations, 1970-1975; NACP-NIH.).
[513] "James Ebert to Albert Vosburg, 30 July 1974."
[514] "Thomas E. Malone to Director, DRG [Division of Research Grants], 7 August 1974," (Res 15-1, Recombinant DNA Molecule Advisory Committee, 1974-1975; NACP-NIH.).

a timely fashion. He also gave Handler assurance that, despite the levels of the Executive Branch that had to be navigated, that it was "of such importance that we do not anticipate any difficulty" in its approval. Stone also mentioned that the NIH had been contacted with suggestions for the committee, and that would be contacting the NAS soon for advice. Throughout the early fall, the NIH received solicited and unsolicited suggestions, nominations, and advice for the advisory committee from scientists and scientific organizations across the country.[515]

Recombinant DNA Experiments within NIH Laboratories

On September 1, 1974, the NIH released "Control of Certain Hazardous Microbiological Agents in Intramural Research" as an addition to the NIH Manual. The purpose of the tightly worded four-page document was "to outline a procedure for control of hazardous and potentially hazardous microbiological agents resulting from intramural research involving genetic and other manipulations." Control was based on proper scientific and safety training of the scientists and technicians as well as proper physical containment facilities in order to "provide optimal safety conditions without unduly restricting bona fide research." The issuance was directed at three groups of hazardous or potentially hazardous agents. Group III was defined by rather standard issue agents of potential and known high pathogenicity to man, such as the Lassa virus. Groups I and II,

[515] "Robert Stone to Philip Handler, 28 August 1974," (ALS: D Med: Recombinant DNA Molecules, Organizing Com for int'l Conf on, Report Inception 1973-1974 [ARMN-91 P77-004]; Central Policy Files; NAA.).

however, were new classes of bacteria and viruses according to any NIH classification system.[516]

Group I included microbial agents "potentially hazardous to man which have been derived as hybrids or recombinants of microorganisms which are infectious to man." The examples for Group I included SV40-*Eschericia coli* hybrids, bacteria with manually inserted antibiotic resistant autonomously replicating plasmids, and "other bacteria agents containing eukaryotic genes" – a topic that the CRDM report only suggested "should be carefully weighed."[517] Group II was an extremely broad category defined as "laboratory-induced mutants, of agents infectious for man, of potential high virulence and pathogenicity for man or other animals." Groups I and II were clearly created to deal with potential recombinant DNA research at NIH run laboratories.[518]

Research involving any of the three groups required signing a Memorandum of Understanding and Agreement (MUA). The MUA outlined the safety procedures, the facilities, and the training to contain and prevent any accidental infections using any of the agents from the three groups. The signature of the lead investigator demonstrated understanding of the potential risks of the agent and the safety practices required. In addition to the signature of the scientist, the director of the lab had to sign, and if an agent was being sent (e.g. pSC101 or a particular strain of a bacteria or virus) then the director of the donor lab was required to sign as well. Though "Control of Certain Hazardous

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[516] "3031-1 Control of Certain Hazardous Microbiological Agents in Intramural Research, 1 September 1974," (Res 15-1-D, NIH/NAS Asilomar Conference Feb. 23-27, 1975, 1974-1975; NACP-NIH.).
[517] Paul Berg et al., "Potential Biohazards of Recombinant DNA Molecules," *Proceedings of the National Academy of Sciences of the United States of America* 71, no. 7 (1974).
[518] "3031-1 Control of Certain Hazardous Microbiological Agents in Intramural Research, 1 September 1974."

Microbiological Agents in Intramural Research" did not restrict recombinant DNA research any more than the CRDM report, it did demonstrate that the issue was concerning enough within the NIH to produce a set of general guidelines for its researchers.[519]

The RAC Charter

Throughout the fall, the charter for the advisory committee steadily worked its way through the DHEW bureaucratic channels, finally landing on Secretary Casper Weinberger's desk in early October. The charter for the Recombinant DNA Molecule Program Advisory Committee (RAC) was a simple three pages. The charter explained the purpose, responsibilities, and structure of the advisory committee. The purpose of the committee was "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 possible hazards to public health and to the environment; and to recommend guidelines on the basis of the research results." The committee was to consist of twelve members selected by the Secretary of DHEW (or his designate) from "authorities knowledgeable in the fields of molecular biology, virology, genetics and microbiology." Though careful not to specify that the members were scientists, the RAC Membership in its early years was predominantly filled with scientists from these fields.

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[519] Ibid. The National Institute for Allergy and Infectious Diseases defines intramural research as "research conducted in government laboratories headed by NIH-employed scientists." [National Institute for Allergy and Infectious Diseases, "NIAID Glossary of Funding and Policy Terms and Acronyms: I-M," http://www.niaid.nih.gov/ncn/glossary/default4.htm.]

The members could serve on the RAC for four years contingent on the continued activity of the committee.[520]

The committee had a few, but important, responsibilities. The first responsibility of the committee was to advise the Secretary and Assistant Secretary of DHEW and the Director of NIH concerning a program to evaluate potential hazards, develop procedures to minimize those hazards, and, most important, "devise guidelines to be followed by investigators working with potentially hazardous recombinants." The end result of the committee was a set of guidelines for all, not just DHEW or NIH, scientists. To accomplish this task, the committee held at least four public meetings a year, which were called by the Chairman with an agenda approved by a government official. Public announcements were required for the meetings, as were records of all meetings. The committee was also responsible for producing an annual report by August 15 each year.[521]

The RAC charter also had a sunset clause of two years from the date of approval. Two years made a good deal of sense because it provided enough time for the RAC to accurately determine the potential risks of recombinant DNA and the need for guidelines. If guidelines were not needed then the committee would have outlived it usefulness. If, however, guidelines were needed the committee could be renewed prior to its sunset date to carry out that part of its purpose. On October 7, 1974, Secretary Weinberger signed the charter and officially created RAC.[522]

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[520] "Charter of the Recombinant DNA Molecule Program Advisory Committee, [Approved] 7 October 1974," (Res 15-1, Recombinant DNA Molecule Advisory Committee, 1974-1975; NACP-NIH).
[521] Ibid.
[522] Ibid.

Confusion in the Ranks

Progress in filling the RAC membership continued within the NIH throughout the remainder of the fall and through the early winter. Stone appointed DeWitt (Hans) Stetten, Jr., NIH deputy director for science, as the first chairman of the committee. Stetten was a highly respected biochemist and, having arrived in Bethesda in 1954, a veteran of the NIH scientific community and bureaucracy. The work in creating the committee, however, was not well communicated outside of the NIH. On December 10, Berg wrote Stone an impassioned and sternly worded letter.

Berg was concerned that the NIH was not living up to its agreement on Recommendation 4 of the CRDM report because in the intervening five months he had not heard anything about committee's approval, membership, or potential meetings. Consequently, Berg wrote the letter "in the hope that the same spirit that moved you to move so quickly on the financing matter can be brought to bear on expediting the organizing and charging of the above advisory group." To "spur" Stone, Berg outlined the work of the MRC committee led by Lord Ashby in the United Kingdom.[523]

Berg concluded the letter with an appeal to Stone "to see to it that our virtual inaction or snail's pace progress is remedied" and a committee is quickly approved and convened. Berg worried that scientists by would interpret the lack of obvious action by the NIH in this matter as a diminution of the potential risks of recombinant DNA research. His greatest concern was borne out in the final sentence of the letter. "If there

[523] "Paul Berg to Robert Stone, 10 December 1974," (ALS: D Med: Recombinant DNA Molecules, Organizing Com for int'l Conf on, Report Inception 1973-1974 [ARMN-91 P77-004]; Central Policy Files; NAA.).

is nothing to show for our efforts by the time of the Asilomar Conference (February 24-27, 1975), I fear it will be difficult to persuade scientists to continue with the voluntary suspension of the particularly worrisome experiments." Berg's concern was palpable. He, however, never received a response from Stone. [524]

One day after Berg authored his letter to Stone, Stone was dismissed as Director of NIH. He served only eighteen months on the job. His firing was not a great surprise within the biomedical community, as *Science* had speculated about how long Stone was going to remain director in the November 15 issue. The *Science* article pointed out that the problems were not within NIH, but were communication and personality issues between Stone and his superiors at DHEW.[525] Less than a month later the speculation was reality, when Stone's superior, Charles Edwards, suggested that he "look for another opportunity."[526] The firing of Stone provoked some outrage within the scientific community because of the apparent political nature of the firing. On December 17, a rally led by some NIH scientists that also included three Nobel laureates, protested against the firing and the damage and hindrance of biomedical research by "unwarranted and counterproductive" political control by the Executive Branch.[527] The Federation of American Scientists, a non-profit scientific organization concerned with issues of science and society, held a press conference at their Capitol Hill office following Stone's firing to "deplore" the "politicization" of the NIH and especially its director of NIH, which was

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[524] Ibid.
[525] Barbara J. Culliton, "NIH: Robert Stone Is in Trouble with Hew," *Science* 186, no. 4164 (1974): 615.
[526] Stuart Auerbach, "NIH Director Fired; 2nd to Go in 2 Years," *Washington Post*, 11 December 1974.
[527] ———, "Dr. Edwards Leaves Hew; Two Top Health Posts Open," *Washington Post*, 18 December 1974.

considered to be nonpolitical.[528] The latter sentiment had broad support in the scientific and medical fields.

Making matters a bit worse for the NIH, on December 17, Edwards resigned leaving the Ford administration to simultaneously fill vacancies at two top interrelated health positions. The same day, Handler, having received a copy of Berg's December 10 letter to Stone, sent a letter to Berg concerning the progress of the NIH. He suggested that due to the "turbulence" at the NIH that it might be just as well "to stall things at the NIH until a proper message comes from the Asilomar meeting."[529] Ronald Lamont-Havers, the Acting Director of NIH, however, had different ideas. In a December 19 response to Berg's December 10 letter, Lamont-Havers brought Berg up to date on the organization of the RAC, informed him of new procedures of intramural research at the NIH, recognized the importance of the Asilomar Conference for RAC, and the reassured him that "there is no lack of interest in the subject by the NIH leadership and NIH scientists and scientist administrators."[530]

The final membership of RAC consisted of Edward Adelberg (Yale University geneticist), Ernest Chu (University of Michigan geneticist), Roy Curtiss, James Darnell (Rockefeller University cell biologist), Stanley Falkow, Donald Helinski (University of California, San Diego, biologist), David Hogness, John Littlefield (Children's Hospital,

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[528] Barbara J. Culliton, "FAS Attacks Politicization of NIH," *Science* 187, no. 4171 (1975). The federation had a membership of 6,500, including over 30 Nobel laureates. [Harold M. Schmeck, Jr., "Scientists Urge 3-Man Council to Improve Advice to President," *The New York Times*, 28 December 1974.]

[529] "Philip Handler to Paul Berg, 17 December 1974," (ALS: D Med: Recombinant DNA Molecules, Organizing Com for int'l Conf on, Report Inception 1973-1974 [ARMN-91 P77-004]; Central Policy Files; NAA.).

[530] "Ronald Lamont-Havers to Paul Berg, 19 December 1974," (ASSEM: Life Sciences: 1974: Com on rDNA Molecules: Ad hoc; Central Policy Files; NAA).

Johns Hopkins University, pediatrician), Jane Setlow (Brookhaven National Laboratories), Waclaw Szybalski (McArdle Laboratory), and Charles Thomas (Harvard Medical School, biochemist). The first members of RAC were all active scientists, though there was representation from academics, government institutions, and private laboratories, and industry. All of the RAC member were invited and attended the Asilomar Conference because their first official meeting was in at the Bellevue Hotel in San Francisco the day following the conference.[531]

Just weeks before the Asilomar Conference, everything was in place. All that was left was to determine if the potential hazards of recombinant DNA research were within acceptable levels to lift the moratorium. If the moratorium was lifted, then the scientists at the conference would have to agree how to carry out recombinant DNA research safely. The RAC would then begin considering guidelines for American scientists the following day. The last week of February 1975 would determine the future of recombinant DNA.

[531] "William Gartland and S. Stephen Schiaffino to Members of the Recombinant DNA Molecule Program Advisory Committee, 14 February 1975," (Res 15-1-A-1, Mailings for Meeting Review, 1975-1976; NACP-NIH).

Chapter 9: Under the Influence

Recombinant DNA did not gain much of a foothold in the public's imagination after the headline-grabbing announcement of the voluntary moratorium on experimentation in mid-July 1974. Experiments, within the boundaries of the CRDM Report continued without a major breakthrough or calamity. In the fall and early winter, the majority of the work concerning the future of recombinant DNA research was inside the National Academy of Science and National Institutes of Health where the details and planning of the international conference and creation of an NIH advisory board were proceeding. Three events, however, occurred outside of the NAS and NIH that would influence and help shape the Asilomar Conference. The first was an address by an eminent physicist. The second was the renewed interest in recombinant DNA at year's end by the scientific and lay press. And third, the Ashby Committee, which formed by the British government a few weeks after the publication of the CRDM Report, released their findings.

Even without any new public proclamations or scientific breakthroughs, recombinant DNA continued to pique the interests of scientists. It was a central example, in eminent physicist and intellectual Freeman Dyson's influential speech at the International Meeting on Scientific Research and Energy Problems in October in Spain. The topic of Dyson's talk was the cost financially and scientifically of deciding to stop research. Recombinant DNA was his major example of where the question of continuing or stopping a new technology or innovation had no definitive answer.

Though largely remaining in the shadows from late summer and throughout the fall, the scientific and lay press briefly brought recombinant DNA back into the spotlight again at the end of the year. They did this as part of year-end recounts of advances in science or as the final update on recombinant DNA before the international conference. These press accounts offered the first public statements in months by the scientists involved in the recombinant debate. The statements provide a window into scientists' current state of recombinant DNA research and expectations for the Asilomar Conference. The articles from this period also gave the scientists evidence about how they and recombinant DNA were being presented to the world.

In the first month of the new year, the Medical Research Council (MRC) in Britain published the Ashby Report, the first official governmental assessment of the potential benefits and potential hazards of recombinant DNA research. The report was the first official government study or recombinant DNA, and its positive findings helped reinforce the prospects of the Asilomar Conference and recombinant DNA research.

Although each was important, the combination of Dyson's speech, the renewed interest of the press in recombinant DNA, and the Ashby Report helped shape opinions and influences the discussions at the Asilomar Conference.

The Hidden Cost of Saying No

At the International Meeting on Scientific Research and Energy Problems in Madrid, Spain on October 16, 1974, Freeman Dyson delivered "The Hidden Cost of

Saying No."[532] In his talk, Dyson argued for the need to "count the hidden costs to saying *no*" to new technologies and technological innovation. For Dyson, the costs of saying yes could be "calculated and demonstrated" economically and politically, but the costs of saying no were mere "conjecture." With the possibility for the costs of saying no to be "extremely high" and "often uncertain and intangible," there should be "procedures and allow a more realistic weighing of uncertainty when knowledge is lacking." Dyson's concern was not the publicly discussed stoppage of projects, such as the American supersonic airliner, but that smaller projects and innovations that "die unseen." These projects died because their proponents were "discouraged by expense, delay and uncertainty which the procedures of public examination impose" or they were "privately discouraged" after a similar project was killed. To illustrate his argument, Dyson examined the costs of saying no with four examples: new drug research, the American supersonic airliners, commercial nuclear reactors, climate modification, and recombinant DNA – the latter of which he focused on with the most detail.[533]

Dyson was influential in the scientific community because of his vast theoretical and intellectual background. A mathematics prodigy who worked for the Royal Air Force Bomber Command during the Second World War, the dropping of the atomic bombs on Hiroshima and Nagasaki drew Dyson to atomic physics. Dyson later characterized his move to physics as the seduction of "glitter of nuclear weapons."[534] In 1945, he left

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[532] Freeman Dyson, "The Hidden Cost of Saying No!," *Bulletin of the Atomic Scientists* 31, no. 6 (1975). "The Hidden Cost of Saying No" was originally given as a talk to the International Meeting on Scientific Research and Energy Problems, Madrid, Spain, 16 October 1974, and first reprinted in the *Bulletin of the Atomic Scientists* in June 1975. Emphasis in original.
[533] Ibid.: 23-24.
[534] Dawidoff, "The Civil Heretic," *The New York Times*, 25 March 2009.

mathematics and began his new career in physics. Two years later Dyson immigrated to the United State and quickly became integrated into the atomic physics community. It was during his early years as a scholar in America that he worked with titans of the field including Michael Feynman, Hans Bethe, Robert Oppenheimer, Enrico Fermi, and Edward Teller. In 1953, he was awarded a position at the prestigious Institute for Advanced Studies in Princeton, NJ.[535] During the next few decades, Dyson's scientific work ranged from working on creating a safe nuclear reactor, to harnessing an atomic explosion, to launching a craft into outer space, to studying climates, and to working with optics. He also had a spirit for social activism, which included testifying for a nuclear test-ban treaty before Congress in 1963 on behalf of the Federation of American Scientists. He opposed the use of nuclear weapons on North Vietnam. In the early 1970s, Recombinant DNA piqued Dyson's interest. He first publically addressed recombinant DNA in his talk in Madrid.[536]

In "The Hidden Cost of Saying No," Dyson stated that regulation on the drug industry as a result of the "thalidomide tragedy…must be accepted as a permanent necessity."[537] The regulations acted to protect society and also acted to hinder drug

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[535] The Institute of Advanced Studies was founded in 1930 as a private institution in Princeton, NJ, and still exists as a leading center for "theoretical research and intellectual inquiry." Faculty at the Institute has included Albert Einstein, Kurt Gödel, and George Kennan, and J. Robert Oppenheimer was the director from 1947-1966. [Institute of Advanced Studies, "Institute of Advanced Studies: Mission and History," http://www.ias.edu/about/mission-and-history.]

[536] Dawidoff, "The Civil Heretic."

[537] Dyson, "The Hidden Cost of Saying No!," 24. Thalidomide was originally available between 1957 and 1961. Initially used to treat insomnia, coughs, colds, and headaches, it was soon prescribed to pregnant women to alleviate morning sickness. Although effective at lessening morning sickness, researchers discovered the drug caused severe birth defects. As a result it was it banned in the United States in 1962 for use by pregnant women until it could be shown to have no ill effects on the mother or fetus. Recently, the

development because of the time and possibility of governmental action halting the research at any stage of its development. By saying yes to safety regulations the public incurred the cost of paying for the enforcement of the regulations as well as increased funding of drug research. The costs of saying no in this example (i.e. regulation of the drug industry) were to a degree quantifiable by tallying the expenditures born by the drug companies to comply with the regulations versus the costs prior to the implementation of the regulations. What could not be quantified, however, were the experiments that were not carried out because of the potential burdens imposed by the regulations.[538]

The supersonic airliner and commercial nuclear reactors shared a similar role as opposing examples of the cost of saying no. Dyson argued that the cost of saying no the supersonic airplane was to discourage the "Darwinian process" inherent in the success of airplane engineering in the United States. The safety regulation on commercial nuclear power plants, however, discouraged "any attempt at radical innovation" and evolution of more than a few different models. For Dyson, the "wastage" of innovation by saying no led to incalculable inefficiencies in the development of both fields.[539]

Dyson then turned his attention to two fields where the question of yes or no had no definitive answer. The first was climate modification and the second was recombinant DNA. He argued that climate modification through human intervention coupled with "adequate understanding of the consequences" could have a positive effect

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drug has gained approval by the Food and Drug Administration for the treatment of a few diseases under tightly controlled conditions. See The Insight Team of the Sunday Times, *Suffer the Children: The Story of Thalidomide* (New York, NY: Viking Press, 1977), Trent Stephens and Rock Brynner, *Dark Remedy: The Impact of Thalidomide and Its Revival as a Vital Medicine* (Cambridge, MA: Perseus, 2001).

[538] Dyson, "The Hidden Cost of Saying No!," 24.

[539] Ibid.: 24-25.

on the climate of the earth, including increasing arable land. This argument was purely theoretical because, as Dyson admitted, there was no technology yet capable of completely controlling climate change. Recombinant DNA, however, was a technology already at the point where the costs of either yes or no were calculable.[540]

Freely admitting that he was "not a biologist," Dyson saw biology achieving a "mastery of the fundamental processes of living organisms" in the same way that chemists and physicists had gained the mastery of the fundamental processes of their sciences. Recombinant DNA was the technique that would allow this to happen because it would allow the enzymes tailored to carry out industrial processes "with much higher efficacy, higher specificity, and lower wastage of material." He proposed recombinant DNA as having the potential to revolutionize food production, create clean fuel from coal and oil, and recycle waste. Dyson saw recombinant DNA as the basis for "a second industrial revolution" that "may undo the evil effects of the first industrial revolution." These were the rewards for saying yes. The Committee on Recombinant DNA Molecules (CRDM) Report of July, however, demanded the need to calculate the hidden costs of saying no.[541]

For Dyson, the CRDM Report was the result of molecular biologists "thinking much more about dangers than rewards." And again he reiterated that he was "not a biologist," Dyson was "quite willing to accept the view of the experts that the dangers are real and serious." He, however, was "deeply" dissatisfied by the report's conclusion that the dangers outweighed the rewards. In Dyson's eyes, the authors of the CRDM report presented an unbalanced cost ratio in their report. "The cost of saying yes to [Type I and

[540] Ibid.: 25-26.
[541] Ibid.: 25.

Type II recombinant DNA] experiments is a risk of disastrous epidemic disease, the cost of saying no is a minor setback in the professional careers of a few scientists." The only conclusion that could be reached by examining the costs presented by the CRDM Report was "a negative one" by the public and scientific peers. Any scientists who performed those experiments would be "automatically judged to be selfish and irresponsible." The hidden cost of saying no to these experiments was "postponement of an industrial technology" that would hinder the "benefits to mankind" of a second industrial revolution.[542]

Dyson was hopeful about the prospects for the Asilomar Conference. He believed the Conference provided an opportunity for scientists to debate the costs of saying both yes and no. And to that end he proposed that the "most useful outcome" of the conference would be for a bifurcation of all recombinant DNA experiments. The first class would include the wildly broad definition of experiments "carrying danger to human populations." The second class would consist of all of the remaining experiments. With the discouragement of the first class and encouragement of the second class, Dyson's recombinant DNA policy would theoretically properly balance the costs of yes and no, and result in the innovation of the new technology to safely create the next industrial revolution.[543]

While the text of "The Hidden Cost of Saying No" was eventually published in the June issue of the *Bulletin of Atomic Scientist*, copies of it were circulated shortly after Madrid. Among those who read it by the early winter of 1975 were a few prominent people involved in the Asilomar conference. Those who read an early copy included Paul

[542] Ibid.: 26-27.
[543] Ibid.: 27.

Berg, Maxine and David Singer, and Harold Green (The George Washington University National Law Center and The Hastings Center). Berg, after he received the text of the talk from the Singers, decided to write Dyson after its publication.[544]

In his letter, Berg attempted to correct the portion of Dyson's argument about recombinant DNA that he felt was "based on inadequate and misinformation." Berg focused on the recommendations of the CRDM Report as a "temporary and voluntary deferral" or "pause" of two types of experiments and not as an outright ban of any recombinant DNA research. For Berg the CRDM issued its report due to safety considerations and not on "moral or ethical ground." The Asilomar Conference, therefore, was an arena for scientists who were carrying out or proposing recombinant DNA experiments "to meet, discuss the possible risks and devise guideline that would prevent the unwanted consequences." Berg suggested a historical scientific analogy he thought better explained the situation of recombinant DNA technology in 1974. The analogy he chose used the pioneers of radiology, who, had they known the dangers of excessive exposure to x-rays, would have taken measures to reduce the hazards of exposure to radiation. Berg assured Dyson that "none of the signers" of the report were "opposed to carrying on this research…but each of us felt a way to do it safely." Berg concluded that "a go slow policy now will provide the opportunity to consider appropriate measures to reduce or eliminate the risks; and, I suspect, we will then move further and more assuredly along the promising paths this research portends." In the conclusion of his letter, Berg suggested that Dyson's argument had some influence on his

[544] "Paul Berg to Dyson Freeman, 9 June 1975," (Recombinant DNA Technologies and Researchers' Responsibilities, 1973-1980: The Paul Berg Papers: Profiles in Science: NLM).

decisions concerning recombinant DNA research at Asilomar. Berg also enclosed a copy of the Summary Statement of the Asilomar Conference for Dyson to decide whether his "hopes were fulfilled."[545]

The text of Dyson's talk was not the only thing those involved in the Asilomar Conference read in the months preceding the conference. They also reviewed the end of year articles in the national and scientific presses, and the Ashby Report. In the two months leading up to Asilomar, some scientists saw their fears about the press realized, and others arrived at the conference with their first experience of any kind of constraint on their research.

The Press

The end of year provided an opportunity for science writers to author updates or print brief summaries to the major events of the year. In the finals weeks of 1974, recombinant DNA failed to generate much notice inside or outside of the scientific journals, despite the impending international conference (Asilomar) that could determine the fate of the technology only two months away. Prominent scientific journals were no exception. Recombinant DNA received extensive coverage in *Science* in late July,[546] and *Nature* in a September 6 editorial examining the question of publication policy of recombinant DNA research papers in scientific journals. That latter editorial was given the original title "Should we publicise those experiments?"[547]

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[545] Ibid.
[546] Nicholas Wade, "Genetic Manipulation: Temporary Embargo Proposed on Research," *Science* 185, no. 4148 (1974).
[547] "Should We Publicise Those Experiments?," *Nature* 251, no. 5470 (1974).

Smaller scientific publications and one prominent newspaper provided some insight into what occurred with recombinant DNA at year's end, and even included reference to the upcoming Asilomar Conference. The *Medical World News* ran an editorial "Genetic engineering alive – but is it well?" which questioned whether some of the ongoing experiments "lie"[d] within the acceptable and safe experiments outlined in the Committee on Recombinant DNA Molecules (CRDM) Report of July.[548] *BioScience* ran a feature news story in its December issue and made the recombinant DNA debates current with the article entitled "Weighing the Hazards of Genetic Research: A Pioneering Case Study."[549] Stuart Auerbach, the *Washington Post*'s science writer, penned an article titled "Cancer Risk May Curtail Genetic Study" that was sure to make a few scientists worry about the prospective public perception of the Asilomar Conference outcome.[550]

The editorial in the *Medical World News* focused on an experiment by Kenneth Murray at the University of Edinburgh that the author viewed as potentially breaking the voluntary moratorium. When questioned about the Murray's experiment, Berg concluded that it appeared to lie within the boundaries established by the CRDM Report. The editorial, though skeptical of Murray's adherence to the moratorium, concluded that the Asilomar Conference would review all of the current research "in detail to see if some are better – and less hazardous – than others."[551]

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[548] "Genetic Engineering Alive – but Is It Well?," *Medical World News* 15 (1974): 58C.
[549] Cristine Russell, "Weighing the Hazards of Genetic Research: A Pioneering Case Study," *BioScience* 24, no. 12 (1974).
[550] Stuart Auerbach, "Cancer Risk May Curtail Genetic Study," *Washington Post* 1974.
[551] "Genetic Engineering Alive – but Is It Well?."

The *BioScience* case study by Cristine Russell, briefly traced the origins of the recombinant DNA debate back to the 1973 Gordon Conference through the CRDM Report. The case study provided a view of where the debate stood just prior to the Asilomar Conference. Russell argued that despite the broad support for the moratorium, there were concerns about the future of recombinant DNA research. As a creator of a recombinant DNA technique, Herbert Boyer believed that greater adherence to standard biohazard safety procedures would lessen the already small possible dangers. He also shared his concern regarding the potential weaponization of the technology by a government. Boyer told Russell that "I'm not at all convinced that some government interested in biological warfare wouldn't try to do that. This is the most dangerous scenario I can think of, but it's a political not a scientific issue."[552] As Boyer's statement demonstrated, despite the President Nixon's 1969 "Statement on Chemical and Biological Defense Policies and Program" that ended the United State's offensive weapons programs, scientists remained skeptical of governments' use of technology for military applications.

Joshua Lederberg gave voice to another concern about the future of recombinant DNA. He worried about public's role in the debate, and had privately expressed this concern to NIH Director Robert Stone in the fall of 1974. In his interview with Russell, however, Lederberg ironically decided to make his private concerns public. Lederberg's concern ran counter to Paul Berg's effort to keep the public apprised of advances in recombinant DNA. Previously, Berg emphasized the importance of a public route "to reach as many people as possible in a short time," and to avoid "an 'ostrich' attitude that

[552] Russell, "Weighing the Hazards of Genetic Research: A Pioneering Case Study," 744.

could only lead to further mistrust among those who already fear 'Andromeda Strain' effects."[553] Lederberg on the other hand wanted the debate to remain within the scientific community to avoid the "continuing polemics of the press."[554]

Scientists also raised a concern about the potential regulation of recombinant DNA. David Baltimore expressed caution that if self-imposed regulation by the scientific community was not successful that "it could lead to future restrictive legislation." Nobel Laureate Sir Jon Kendrew furthered Baltimore's point when he told Russell that in the case of recombinant DNA "it is for scientists themselves to put their own house in order, or else others will step in and do it for them, perhaps in ways which will lead to quite undesirable restrictions on what they do." The scientists clearly understood the importance of the Asilomar Conference. What they agreed upon (or what they did not) would likely impact the amount of regulation placed upon recombinant DNA research.[555]

A Lump of Coal in the Stocking

On Christmas day, Bernard Davis's fear that "even the most respectable newspapers" preferred "sensational" rather than "authoritative" articles on science, and recombinant DNA was realized.[556] "Cancer Risk May Curtail Genetic Study" was Stuart Auerbach's last article on recombinant DNA prior to the Asilomar Conference. The sources he used for his short article were the CRDM Report and interviews with David Baltimore and Paul Berg. Despite his highly credible scientific sources, Auerbach's

[553] Ibid.: 693, 774.
[554] Ibid.: 744.
[555] Ibid.
[556] "Bernard Davis to Philip Hander, 27 September 1974," (ASSEM: Life Sciences: 1974: Com on rDNA Molecules: Ad hoc; Central Policy Files; NAA).

article contained facts countered by a good deal of sensationalism about recombinant DNA. Through his interviews with David Baltimore and Paul Berg, the article at least shed light on the goals of two of the organizers of the upcoming Asilomar Conference.[557]

Auerbach reintroduced his readers to recombinant DNA by stating that it held "great promise for mankind," but was "so potentially dangerous it could bring about incurable diseases and increases in cancer."[558] The CRDM Report and many scientists made the former claim, but the latter claims were sensational or a plain distortion of the "unpredictable effects" stated in the report.[559] Baltimore stressed to Auerbach that recombinant DNA held the promise the betterment of mankind through industrial processes, such as the manufacture of insulin and other "therapeutic compounds," as well as creating nitrogen-fixing bacteria to increase crop yields.[560] After listing the potential benefits of the technology, Auerbach shifted his writing to the potential dangers highlighted in the opening paragraph.

Auerbach cited the issuance of the CRDM Report as evidence of the potential dangers of recombinant DNA, and proceeded to sensationalize the concerns of the authors of the report. He made the suggestion that the report used tentative language to describe potential dangers ("unpredictable effects" and "unfortunate consequences") because the safety of the new experiments was still largely unknown.[561] In fact he committee only called for a voluntary moratorium for those experiments that they deemed most likely to cause potential hazards (Type I and Type II). Auerbach provided

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[557] Auerbach, "Cancer Risk May Curtail Genetic Study."
[558] Ibid.
[559] Paul Berg et al., "Potential Biohazards of Recombinant DNA Molecules," *Science* 185, no. 4148 (1974).
[560] Auerbach, "Cancer Risk May Curtail Genetic Study."
[561] Berg et al., "Potential Biohazards of Recombinant DNA Molecules."

examples of the dangers he claimed were posed by the report. These included antibiotic-resistant and “untreatable infections,” “news types of poisonous substances,” and “an increase in cancer.” The last of which he claimed the “committee feared.” The report of the committee was not that definitive, nor did it ever use the word fear. The report stated that Type II experiments “might be more easily disseminated” and could “possibly increase the incidence of cancer or other diseases.” Auerbach then turned to the Asilomar Conference.[562]

According to Auerbach, the Asilomar Conference was “the first time scientists have come together voluntarily to see if they should continue their work.” Berg, as chairman of the conference, viewed the conference as a “fact finding meeting” to “evaluate the extent of the risk and what to do about it.” And according to Berg, it would also provide an opportunity for scientists to find out “where we are going, should we go there, and can we get there safely.” Largely agreeing with Berg, Baltimore hoped that a general agreement about safely advancing recombinant DNA research could be reached at the conference.[563]

Auerbach then broke up the idea of general agreement between the two organizers concerning the outcome of the conference by inserting a curious quote from Baltimore. “We are not talking about a technique that can be used in a malevolent way. We are talking about a technique which is in itself potentially harmful and therefore needs to be controlled.”[564] Though Baltimore had made public comments about the potential dangers or recombinant DNA research, especially at the press conference on the release of the

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[562] Auerbach, "Cancer Risk May Curtail Genetic Study.", Berg et al., "Potential Biohazards of Recombinant DNA Molecules."
[563] Auerbach, "Cancer Risk May Curtail Genetic Study."
[564] Ibid.

CRDM Report, he had never previously publicly expressed a need for any controls to be placed on recombinant DNA. Baltimore's quote was even more interesting because the quotes from Berg in the article were largely consistent with comments he had made both to the press and in private correspondence. Perhaps Auerbach selectively selected a quote from Baltimore to shadow the conference with some conflict or sensationalism. Auerbach closed his article by listing the myriad of biological fields, the pharmaceutical companies, and government agencies who were invited to the "four-day closed" conference.[565]

The Ashby Report

At the July 26, 1974 meeting of the Advisory Board for the British Research Councils meeting, which occurred one week after the publication of the CRDM Report in *Nature*, the British government took its first organized step towards dealing with recombinant DNA research.[566] The Board, whose primary role was to advise the Medical Research Council on non-military science policy, set up an expert Working Party under the chairmanship of Lord Eric Ashby[567] to "assess the potential benefits and potential hazards of techniques which allow the experimental manipulation of the genetic composition of micro-organisms; and to report to the Advisory Board for the Research Councils." Additionally, the Working Party "should, as far as possible, not include those

[565] Ibid.
[566] Paul Berg et al., "NAS Ban on Plasmid Engineering," *Nature* 250, no. 5463 (1974).
[567] Lord Eric Ashby (1902-1994) was a botanist and best know for his work as a university administrator. He is most well known for opening Clare College Cambridge to women while Master there. [Sir Brian Pippard, "Obituary: Lord Ashby," *The Independent (UK)*, 28 October 1992.]

who were using the [recombinant DNA] techniques and who might therefore be directly affected by its conclusions."

The final Ashby Committee consisted of thirteen members. This included Lord Ashby, Nobel laureate Maurice Wilkins, and biologists from a broad range of fields including biochemistry, biophysics, virology, genetics, epidemiology, and botany. The committee met on five occasions between August and October to hear testimony from a broad cross section of eminent British-based biologists ("Expert Witnesses"). A total number of twenty-eight biologists presented testimony to the committee: twenty-five in person, and another three in writing from.[568]

The committee began writing their report in late October, and submitted it the Advisory Board on December 13. With its publication in January of the new year, the Ashby Report became the first thorough governmental assessment from any nation focused exclusively on recombinant DNA research. The committee wrote a twenty-two page report accessible to anyone because it did not assume any "specialised knowledge on the part of the reader." This was important to the committee because they wanted the "social values of the community at large [to] be incorporated into decisions on science policy,"[569] including recombinant DNA. The report followed a simple format with an introduction and summary, an explanation of recombinant DNA, a discussion of the potential benefits, a review of the potential hazards, safety recommendations, a

[568] Lord Ashby et al., "Report of the Working Party on the Experimental Manipulation of the Genetic Composition of Micro-Organisms," (London, UK: Medical Research Council, 1975), 2, iv, 19-21. For the politics of the formation of the Ashby Committee see Susan Wright, *Molecular Politics: Developing American and British Regulatory Policy for Genetic Engineering, 1972-1982* (Chicago, IL: University of Chicago Press, 1994), 140-42.

[569] Lord Ashby et al., "Report of the Working Party on the Experimental Manipulation of the Genetic Composition of Micro-Organisms," 3.

conclusion, and recommendations. The Ashby Report was nearly as elegantly simple as the Cohen and Boyer technique for creating recombinant DNA molecules.

The "Introduction and Summary" section outlined the origins and purpose of the committee, as well as a general summary of the report. The authors of the report took special care to note what they "were *not* set up to do," which was create a "code" for those working with recombinant DNA or "pass ethic judgments" about the technique. They also clearly avoided sensational claims for the potential benefits and especially the potential risks. The report conceded that laboratory mistakes are made with biohazards, but "talk of other hazards, such as 'cancer-producing microbes' is speculation." The committee was also convinced after reviewing all the testimony that "the hazards are less serious than some of us first thought," and they could be reduced to "levels far lower than other hazards which the public currently accepts without question." The committee believed that the potential benefits that might arise from this research were "very great," and included advances in basic science, medicine, agriculture, and the pharmaceutical industry.[570]

The section concluded with a general "verdict" about recombinant DNA research. "[W]e can expect many benefits form using these techniques and, provided precautions are taken…the potential hazards need not cause public concern. So while we urge that possible hazards must always be borne in the mind of those engaged in research, we also believe it very important to stimulate lively enquiry into this field of research, its possible applications, and their social consequences." The Ashby Report assessed the potential benefits and risks and concluded that recombinant DNA research within the boundaries

[570] Ibid., 3-4.

of the CRDM Report should be given a cautious go ahead as long as researches followed safety recommendations.[571]

The science section was written in "non-technical" language with the notion that it should be "intelligible to people unfamiliar with modern genetics."[572] The section provided the necessary background in basic genetics to understand the reason behind the creation of recombinant DNA technology was and explained what types of experiments can be carried out. It also focused on the role of *E. coli* and the two methods for creating recombinant DNA molecules. The section served to give enough information so that the reader could properly understand the potential risks and benefits of the technology.[573]

The third section of the report dealt with the potential benefits. While the report acknowledged that all of the potential benefits were "an act of speculation, resting on nothing more than informed judgment," the authors provided four values that the technology could provide for science and society. The four values were: "deepening the understanding" of basic science; using bacteria as a "factory" to produce hormones, antibodies, or vaccines; advancing understanding of antibiotic resistance; and helping to ease the "world food shortage" through the creation of artificial manures and genetically modifying plants. The evidence presented by the witnesses persuaded the committee that "the potential benefits are likely to be impressive."

The "Potential Hazards" section avoided focusing on "unpredictable hazards" that were in the "realm of conjecture." Instead the section honed in on potential methods that a recombinant DNA molecule might escape a laboratory. The section read more like a

[571] Ibid., 4.
[572] Ibid., 3.
[573] Ibid., 4-5.

prelude to the following section on containment principles because of that focus. The report explained laboratory safety precautions such as "epidemiological monitoring" and using animals as test subjects. The report covered safety of the K12 strain of *E. coli* and included a test that the committee requested Ephraim Anderson, one of their witnesses and expert on *E. coli*, enteric organisms and antibiotic resistance, to carry out. After he dosed milk with a high concentration K12 containing harmless recombinant plasmids, Anderson had some volunteers drink the milk, and then they sampled their excretions for the presence of the modified K12 strains. Anderson concluded that

> orally-ingested K12 can survive quite well for a few days in the human large intestine, and may multiply, but that even at high dosage…transfer of the plasmid to the resident flora is meagre, if it takes place at all. Accidental ingestion of more than a few hundred organisms is unlikely. It is obviously impossible to generalise with absolute confidence, but…our studies with K12 (F-T) suggest that the likelihood of accidental transfer of experimental plasmids to the resident intestinal flora is low. This indicates that no more than 'medical' microbiological care is needed in the handling of plasmid-carrying *E. coli* K12.[574]

Though Anderson's study proved that it was unlikely that the K12 strain could transfer its plasmids while in the human gut, it was not impossible. Therefore the committee could not declare K12 "safe" because it might "create a false sense of security." Since the scientists and the public discussed the topic of the potential use of recombinant DNA in the creation of biological weapons, the committee concluded that, with their limited knowledge, they could "conjecture possible malicious uses for these techniques."[575]

The "Defence Against Potential Hazards" section read like a primer on basic medical microbiological and epidemiological safety procedures, with one exception

[574] Ibid., 8-9.
[575] Ibid.

unique for recombinant DNA. The goal was to provide protection primarily for all the workers in the laboratory, including scientists, technicians, janitorial secretarial staff, as well as, for the public. The committee determined that the first line of defense against potential hazards was adherence to proper laboratory safety protocols. The report covered the proper handling and disposal of biohazards, the proper use of the appropriate physical containment equipment complimentary to the assessed risk of the experiment, as well as, the proper employment of epidemical monitoring for all laboratory workers. The authors also proposed a unique form of containment: biological containment.

All of the previous debates about the safety of recombinant DNA focused on the use of physical containment, both procedure and hardware. The Ashby Committee, however, suggested that both the plasmid and the bacteria should be equipped with "genes that would disqualify them from surviving in the human gut at all." Ways of "disarming the bug," as they termed it, included creating mutant bacteria that could only survive or reproduce in the presence of a rare chemical or vitamin. They also suggested that experiments should be carried out in well-understood bacteria that are not endemic to humans, such as *Bacillus subtilis*, which is commonly found in soil.[576]

In the "Conclusion" section, the committee reiterated its "unanimous view that potential benefits are likely to be great," and that the public had the "right to seek assurances that they are not at risk" by ensuring proper safety protocols for laboratories performing recombinant DNA research.[577] Further the committee agreed that the "pause" called for by the CRDM Report should be "no more than a pause because the techniques open up exciting prospects both for science and for its application to society" provided

[576] Ibid., 10-12.
[577] Ibid., 12.

the testimony of witnesses indicated that "the potential hazards can be kept under control." The committee also relied on the continuation of the "general concern among scientists themselves over this matter" to dissuade their peers from performing "irresponsible and unnecessarily hazardous experiments."[578]

The report further concluded that it would be "excessive constraint upon important work to insist on conducting all [recombinant DNA] experiments with elaborate and expensive procedures used for the most dangerous pathogens." Therefore it was important to create a system to "match the scale of precaution to the estimated risk." The reason for this was because it was believed that many bacterial geneticists and molecular biologists, especially the recent graduates, were "unfamiliar with these hazards" and that they were "unable to assess the levels of precaution needed." Though they were not charged with creating a code, the Ashby Committee did so anyway because they believed it would be "welcomed" by scientists and institutions.[579]

The Ashby Committee created a set of eight basic recommendations for a code that would provide the flexibility needed for safe recombinant DNA experiments as research progressed. Three recommendations focused on safety procedures and containment of pathogens. The first of which stated that all researchers "should have some experience in the technique of handling pathogens."[580] Two recommendations dealt with the proper role of oversight at the funding and institutional bodies. In the remaining three recommendations, the committee decided that compulsory registration was unnecessary, large-scale experiments should be carried out on a small-scale first, and

[578] Ibid., 15.
[579] Ibid., 12-13.
[580] Ibid., 13.

epidemiological tests should be carried out in laboratories to safeguard against carrying a disease out of the laboratory.

Just as the case with creating a code, the committee went beyond their original set of instructions and provided eight recommendations to the Advisory Board. They did this because they felt that their "assessment would not be valid without certain recommendations." The recommendations were to create a code, train all laboratory workers in the safe handling of pathogens, designate a biological safety officer, make epidemiological monitoring standard practice, prohibit large-scale experiments except in specially equipped laboratories, create and use of biological containment, and conduct further research into the safety of E. coli, especially the K12 strain, in recombinant DNA experiments.[581]

The Ashby Report attempted to find balance between potential benefits against potential risks as well as balance the needs of science versus the values of society. It was not a report written by scientists for scientists. Rather it was a document produced with the intention of engaging the public in the creation of a recombinant DNA policy. Indeed this was only the initial assessment and not meant as an attempt to formulate policy. The report, however, belied its founding principle as the authors felt it necessary to create the broad outline of a code as well as provide recommendations to the Advisory Board. The report stressed the importance of proper biohazard training for researchers, which was a problem that dogged recombinant DNA research almost from the beginning.

Due to the fact that the Ashby Report was published in January 1975, it was available to all of the participants in the Asilomar Conference prior to their arrival.

[581] Ibid., 15-16.

The Tie that Binds – The Ashby Committee and Asilomar Conference

The organizers of the Asilomar Conference noticed the work of the Ashby Committee. Chairman Paul Berg had intermittent contact with Lord Ashby throughout the fall of 1974, and had personal knowledge of the work of the Ashby Committee through fellow Organizing Committee member Sydney Brenner, who was an Expert Witness. On November 1, Berg wrote to Ashby concerning the status of the committee's report, and, more important, inquired whom he should invite to the Asilomar Conference. As the primary person responsible for the selection of scientists invited to participate at Asilomar, Berg wanted not only prominent scientists whose work would be impacted by recombinant DNA, but also scientists who "can influence policy decision making in their own country." In his initial thoughts on which British scientists to invite at Asilomar, Berg wanted included Brenner, two members of the committee, and "an appreciable number" of Expert Witnesses, as well as, other prominent British scientists. In truth, "an appreciable number" was understating matters a bit.[582]

The first list of invitees to the Asilomar Conference from the United Kingdom appeared a month and a half after Berg's letter to Ashby, and contained fourteen biologists, twelve of whom were members of or witnesses before the Ashby Committee.[583] About a month later, Berg created the second invitee list, and it contained fifteen biologists from the UK including the same twelve from the first list involved in

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[582] "Paul Berg to Lord Eric Ashby, 1 November 1974," (Recombinant DNA Technologies and Researchers' Responsibilities, 1973-1980: The Paul Berg Papers: Profiles in Science: NLM).

[583] "List of Participants for Asilomar, December 1974," (International Relations: Internatl Conferences: Recombinant DNA Molecules: Organizing Com, 1975; Central Policy Files; NAA.).

the Ashby Report.[584] The official National Academy of Sciences list of confirmed participants at the end of January included eleven scientists from the UK, of which nine participated in the Ashby Report (one member and eight witnesses). The final attendance record of the Asilomar Conference showed a total of ten total participants from the UK, of which eight participated in the Ashby Report (one member and six witnesses).[585] And of those eight, five gave talks at Asilomar – four presenting papers on the first day and one leading a session on the second day.[586]

Berg's inclusion of eight biologists who participated in the Ashby Report was a shrewd maneuver because he wanted experts in the field who could address the concerns about the potential risks of recombinant DNA research and who could influence policy after the conference. Those who participated in preparing the Ashby Report met both of those qualifications by their research and by their standing within the biology

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[584] International, "National Academy of Sciences International Conference on Recombinant DNA Molecules, Participants, 1975," (International Relations: International Conferences: Recombinant DNA Molecules, 1975; Central Policy Files; NAA.).

[585] Paul Berg et al., "Report of the Organizing Committee of the Asilomar Conference on Recombinant DNA Molecules," (International Relations: International Conferences: Recombinant DNA Molecules, 1975; Central Policy Files; NAA., 1975). The eight involved with the Ashby Report were Ephraim Anderson (Enteric Reference Laboratory, Public Health Laboratory Service), Sydney Brenner (Medical Research Council Laboratory for Molecular Biology), Walter Bodmer (Genetics Laboratory, Department of Biochemistry, Oxford University), Kenneth Murray (Department of Molecular Biology, University of Edinburgh), Mark Richmond (Department of Bacteriology, University of Bristol), and H. Williams Smith (Houghton Poultry Research Station), Robert Williamson (Bateson Hospital, Glasgow). All of the Expert Witnesses, except Williamson, gave talks at Asilomar, and only Bodmer, who was the only member of the Ashby Committee in attendance, did not. The two UK attendees not part of the Ashby Report were N. H. Carey (G.D. Searle and Company, Ltd. Research Division) and Ray Dixon (ARC Unit of Nitrogen Fixation, University of Sussex)

[586] Anderson, Williams Smith, Richmond, and Murray all presented papers during the Ecology of Plasmids and Enteric Organisms session on the first day. Brenner led a discussion titled "Design of Safe Vectors" following lunch on the second day.

community. Anderson (Enteric Reference Laboratory, Public Health Laboratory Service) and H. Williams Smith (Houghton Poultry Research Station) were both widely respected for the years of research on *E coli* and other enteric bacteria with a focus on antibiotic resistance. Mark Richmond (Department of Bacteriology, University of Bristol) and Kenneth Murray (Department of Molecular Biology, University of Edinburgh), and Robert Williamson (Bateson Hospital, Glasgow) were all actively carrying out innovative recombinant DNA experiments. Murray's research garnered him the editorial discussed earlier in the *Medical World News*. The contingent of seven participants in the Ashby Report, including Stuart Glover (Department of Genetics, University of Newcastle-Upon-Tyne) and committee member William Bodmer (Professor of Genetics, University of Oxford), could also provide the Asilomar Conference attendees with experience and insight into a recent governmental assessment of recombinant DNA as well as shine insight onto new or unresolved issues from the committee and report. The combination of the Ashby Report and the seven participants provided Berg with an influential group of scientists that could help inform and influence opinion at the Asilomar Conference, and then return to the UK and report back to governmental and scientific organizations about decisions agreed to by the conference.

Chapter 10: Phase One Complete

On the evening of Sunday, February 23, 1975, one hundred and fifty-three participants were shuttled from area airports to the Monterey peninsula for the beginning of the Asilomar Conference. One hundred and thirty-four of the participants were from universities, governmental institutions, private laboratories, and industry. Of these, eighty-three were from the United States and fifty-one were from foreign countries.[587] The conference participants also included four lawyers, sixteen members of the lay and scientific press, and three National Academy of Science staff members. Over the next three and a half days, these participants were going to make a decision about how to proceed with recombinant DNA research. They could decide that their concerns about the hazards posed by Type I and Type II experiments were not realistic and lift the voluntary moratorium.[588] They could decide that there were more risks posed than previously thought and recommend deferrals of more types of experiments. The meeting could be divisive with no decision reached regarding any standards for the prospective research. This last option posed the greatest threat to the future of recombinant DNA research because a lack of agreement could lead to the likely intervention by governments and a potential balkanization of policies between cities, states, and countries.

Most of the attendees did not know what to expect as they assembled at the Asilomar Conference. Their general understanding came from either what they read in

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[587] The fifty-one foreign attendees came from seventeen different countries: United Kingdom (10); West Germany (6); Switzerland (5); Soviet Union (5); France (5); Canada (4); Australia (3); The Netherlands (3); Japan (2); Italy (2); Belgium (1); Poland (1); Denmark (1); Mexico (1); Sweden (1); and Israel (1).

[588] Paul Berg et al., "Potential Biohazards of Recombinant DNA Molecules," *Science* 185, no. 4148 (1974).

the press, information gleaned by talking with their colleagues, or their invitation letter to the Conference. The invitation was the most accurate source of information for the vast majority of the attendees. Only members of the Organizing Committee or any of the three small Working Groups knew more.

The invitation to the Asilomar Conference stated that the "purpose of the meeting is to review the progress, opportunities, potential dangers and possible remedies associated with the construction, and introduction of new recombinant DNA molecules into living cells." The letter declared that, "the purpose of this meeting is to obtain as much information and expert opinion as exists on the question at issue."[589] The program was outlined in five points that made the Asilomar Conference sound like a scientific conference centered on a single topic.

> The program will include discussions of a) the present and possible future methodologies for constructing, propagating and amplifying recombinant nucleic acid molecules; b) the molecular biology and natural history of autonomously replicating plasmids and their microbial, plant and animal hosts; c) the molecular biology of free and cryptic oncogenic virus genomes including an examination of the rationale and potential risks of introducing such genetic infection into microbial plasmids; d) the methodology, scientific and practical benefits, and the risks of linking segments of eukaryote genomes to autonomously replicating microbial plasmids; e) approaches to assess, and to minimize or eliminate any serious biohazards stemming from this line of research.[590]

The invitation also stated that the size of the conference was limited "to permit frank and effective exchange of information and discussion" between attendees.[591]

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[589] "Asilomar Conference Invitation Letter, 18 December 1974," (ALS: D Med: Recombinant DNA Molecules, Organizing Com for int'l Conf on, Report, Invitation letters, 1974; Central Policy Files; NAA.).
[590] Ibid.
[591] Ibid.

On a sunny Monday morning in late February the attendees began arriving at the Asilomar Conference in Pacific Grove, California. Nestled neatly on the Pacific Ocean end of a point between Monterey Bay and Carmel Bay, the Asilomar Grounds were established in 1913 as YWCA campgrounds and used as conference center until 1956 when it became part of the California State Park System. Over the next three and a half days, the attendees found out that it was not the science that was the most important issue at the Conference, but rather it was the need to reach for a consensus on how to safely continue recombinant DNA research. The Conference tested the translation of the word "Asilomar" and whether it was fitting or a cruel joke for the fate of recombinant DNA research. Asilomar meant, "refuge by the sea."[592]

Day One

When the participants arrived on that Sunday evening, they received a complete and very full program. "The Introduction: History, Aim of Meeting, Outline of Task" by David Baltimore was to start the conference promptly at 8:30am on Monday morning. Baltimore wasted little time getting down to the business of the conference by defining its organization and boundaries. In a firm and authoritative voice he stated that this conference was unrelated to the 1973 biohazard conference also held at Asilomar,[593] and, unlike that conference, the committee decided not to publish the current proceedings. He then began a recitation of the "events" that led to the conference. He began with the

[592] "Asilomar Conference Grounds Pamphlet, C.1973," (ALS: D Med: Recombinant DNA Molecules, Organizing Com for int'l Conf on, Report, Invitation letters, 1974; Central Policy Files; NAA.).
[593] Alfred Hellman, Michael. N. Oxman, and Robert Pollack, *Biohazards in Biological Research* (Cold Spring Harbor, NY: Cold Spring Harbor Laboratory, 1973).

publication of the Singer-Söll letter in *Science*,[594] and included the Committee on Recombinant DNA Molecules (CRDM) and the publication of their report,[595] and then discussed reasons for the organization of the Asilomar Conference.[596]

Having completed the background for the reasons for the Conference, Baltimore told the audience, "this meeting is being taped," which came as a surprise to many of the scientists. He then stated, to quickly ease the participants' nerves that "the tape is not to be released publicly. It is for the use of the Organizing Committee… Anybody who feels nervous about being taped, or doesn't wish their discussions or talk to be taped merely has to tell the person who is running the tape machine that they would like it turned off. And he will turn it off during the time of your presentation." Baltimore then called attention to the sixteen members of the press, sitting in the front with their tape recorders out. Again, he assured the scientists that the reporters were prohibited from publishing any story until the conclusion of the Conference.[597]

The invited press derived mainly from large domestic newspapers and journal, including Stuart Auerbach (*Washington Post*), Victory McElheny (*The New York Times*), Judy Randal (*Washington Star-News*), George Alexander (*Los Angeles Times*), Cristine Russell (*BioScience*), Nicholas Wade (*Science*), and Michael Rogers (*Rolling Stone*).[598]

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[594] Maxine Singer and Dieter Soll, "Guidelines for DNA Hybrid Molecules," *Science* 181, no. 4105 (1973).
[595] Berg et al., "Potential Biohazards of Recombinant DNA Molecules."
[596] National Academy of Science, *Asilomar Tapes, Reel 1 Side 1a* (Recombinant DNA Molecules; Central Policy Files; NAA: 1975), Reel-to-Reel.
[597] Ibid.
[598] The others were, Jerry Bishop (*Wall Street Journal*), Graham Chedd (*New Scientist* and *Nova*), Robert Cooke (*Boston Globe*), Rainer Flohl (*Frankfurter Allgemeine*), Gail McBride (*JAMA*), Colin Norman (*Nature*), Dave Perlman (*San Francisco Chronicle*), Janel Weinberg (*Science News*), and Dermot A. O'Sullivan (*Chemical and Engineering News*). Paul Berg et al., "Report of the Organizing Committee of the Asilomar

Rogers, who wrote a long article for Rolling Stone and later published a book on the recombinant DNA debates, described the atmosphere created by the scientists to the perceived interlopers as "hardly one of welcome."[599] The attitude of the press toward the scientists was only slightly better. George Alexander told Rogers the "scientists loved the press when we got to Nixon, but when we start hanging around their own backyard, they get very nervous."[600] When getting a press invitation to the conference proved difficult for Stuart Auerbach, he claimed to have told members of the Organizing Committee "A secret international meeting of molecular biologists? If the press isn't allowed, I'll guarantee you nightmare stories."[601] Although the press was not permitted to interrupt the proceedings, they were welcomed to dine and ask questions of willing scientists after the session.[602]

After trying to smooth over the presence of the press, Baltimore moved onto the "principles" of the conference. In fact, it was one principle:

> [The Organizing Committee] felt that issues, which involved safety or morality, for that matter, can only be intelligently considered in the context of existing knowledge and by discussions, which are grounded in the facts of the situation. So this meeting was conceived to layout the existing technology, to consider what has been done, what might be done in the future, what benefits can come from the technology, both in terms of scientific knowledge and in more practical terms.[603]

Conference on Recombinant DNA Molecules," (International Relations: International Conferences: Recombinant DNA Molecules, 1975; Central Policy Files; NAA., 1975), 10.

[599] Michael Rogers, *Biohazard*, 1st ed. (New York, NY: Knopf, 1977), 55.

[600] Ibid.

[601] Ibid.

[602] Rogers provides the best, albeit short, account of how the press were invited to the conference, Ibid., 50.

[603] National Academy of Science, *Asilomar Tapes, Reel 1 Side 1a*.

He then posed a series of broad questions that the attendees needed to consider during the conference, including "What should be done? What should we know before doing a certain thing? And basically when do procedures in molecular biology become more of a hazard than a benefit?" What Baltimore said next surely may have surprised some of the audience.[604]

Baltimore emphasized the importance of "hard science" at the conference, and instructed that the speakers not to "talk down" to any members of the audience whether they are Nobel laureates or members of the press. Next he delineated the boundaries of the discussions, by identifying two topics off-limits for discussion. The first of these "peripheral" issues was "the utilization of this technology in what's been called gene therapy or genetic engineering." The reason for eliminating this issue from discussion was because of its potential to lead to political disagreements and "complicated" philosophical questions, such as "what's right and what's wrong." The second issue was biological warfare, which, according to Baltimore, "this meeting is not designed to deal with."[605]

Having eliminated two relevant issues that pertained to social responsibility and public policy, Baltimore then defined what was going to be discussed. Recombinant DNA – a technique Baltimore defined as being able "to outdo the standard events of evolution" – posed "special potential hazards" while offering "enormous benefits." The strategy of the conference, according to Baltimore, was to "balance the benefits and hazards right now…which will maximize the benefits and minimizes the hazards for the future." Baltimore anticipated a reliance on a strategy based on hard science to balance

[604] Ibid.
[605] Ibid.

benefits versus risks. By defining the conference based on hard science, Baltimore was forcing the debate to disregard any degree of social responsibility of the scientists, and it also undermined the rationale for the presentations by the four lawyers.[606]

After quickly reviewing the conference agenda through Wednesday night, Baltimore illuminated the audience with what was going to occur on Thursday morning. The program only listed the nondescript session as "Discussion and adoption of conference statement."[607] The final morning consisted of the Organizing Committee issuing general guidelines based on a general consensus of the participants. The plan was to send the guidelines to "whatever bodies exist at national or international levels, which are attempting any sort of regulation or advice." Baltimore then warned the audience about the potential price of not reaching a consensus. "I would just point out that there is no one else to appeal to. And if we come out of here split an unhappy, then we have really failed the mission that I think is in front of us."

When summing up his remarks, Baltimore took a shot at the press seated in front of him when he reminded the scientists to keep in mind the possible benefits of recombinant DNA even though some have become "almost folklore from their repetition in the newspapers and elsewhere." He once again impressed the need to focus on issues of "physical containment" and "hazards" while acknowledging the possibility of policy ramifications stemming from the Conference's recommendations. The worst of which

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[606] Ibid.
[607] Berg et al., "Report of the Organizing Committee of the Asilomar Conference on Recombinant DNA Molecules."

would be the development of an "enormous" bureaucracy that is responsible for checking off every step of their research. "And that's certainly the situation that we're in."[608]

Before turning the podium over to Paul Berg, Baltimore took one important question on the procedure the Organizing Committee would use to determine the consensus of the conference. Baltimore responded, "The procedure by which the consensus will be determined are largely determined by the extent of the consensus." After laughter from the audience temporarily halted his response, Baltimore pointed out that unless there was an even split that the organizing committee planned "to see that a specific statement and a meeting report are prepared and are available in the scientific literature." With that Baltimore turned the microphone over to Paul Berg, and thus began the long day of scientific papers.[609]

The remainder of the morning consisted of Berg providing a ten minute refresher on the methods for creating recombinant DNA molecules. He then gave way to the members of the Plasmid Working Group (PGW), who had handed out their thirty-five page working paper earlier that morning. The PGW had eleven presenters, which made it the longest session of the conference. The majority of the PGW presenters followed Baltimore's dictate to "not attempt to talk down to laymen who may be in the audience or to people who don't have a specialize knowledge of the subject." The morning session focused on the potential for *E. coli* to transfer genes in the human gut. Stanley Falkow gave a primer on plasmid ecology and the introduction of flora into the human bowel. The three speakers following Falkow were all Expert Witnesses from the Ashby Committee. It was a shrewd decision to have Ephram Anderson, H. Williams Smith, and

[608] National Academy of Science, *Asilomar Tapes, Reel 1 Side 1a*.
[609] Ibid.

Mark Richmond finish out the morning. All three were experts in their respective specialties – Anderson in antibiotic resistance and *E. coli* K12, Williams Smith in enteric organisms, and Richmond in gene transfer of enteric organisms – and all three had presented their findings recently. The three talks together yielded no precise consensus, though they did agree that there was a very small, though not insignificant, chance of a genetically modified *E. coli* K12 transferring some of its genes in the human intestine.[610]

Following lunch, there was a discussion session on public policy, which Baltimore described as "what to do in the face of uncertainty as a policy question. And that's certainly the situation that we're in." Harold Green, a well-known Washington lawyer, presented at Asilomar because of his work on genetics as a fellow at the Hastings Center – one of the foundational bioethics organizations in the United States.[611] Green, in a slow and deliberate style, addressed the assembled scientists about uncertainty in the public policy. In Green's theoretical discussion of how to deal with a new technology that has uncertainties, like recombinant DNA, he posited that the most rational approach for a technology that showed early benefits would be to continue even when the risks appear "remote, speculative, [and] hypothetical." This approach, however, he doubted would be "acceptable as a matter of social responsibility and public policy" because society usually does not take the "chance of finding out whether or not there will be an injury." Before

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[610] _____, *Asilomar Tapes, Reel 1 Side 2a* (Recombinant DNA Molecules; Central Policy Files; NAA: 1975), Reel-to-Reel.

[611] Harold Green became famous as the staff attorney for the Atomic Energy Commission who drafted disloyalty papers against J. Robert Oppenheimer in 1954. Patricia Sullivan, "Harold Green: Drafted Oppenheimer Disloyalty Charges," *Washington Post*, 1 August 2008. Albert R. Jonsen, *The Birth of Bioethics* (New York, NY: Oxford University Press, 1998), 179. M. L. Tina Stevens, *Bioethics in America: Origins and Cultural Politics* (Baltimore, MD: Johns Hopkins University Press, 2000), 60, 65. The Hastings Center (originally named the Institute of Society, Ethics and the Life Sciences) was founded in 1969.

moving onto his next topic, Green warned that there "may still be sound reasons why the Pandora's Box should not be opened."[612]

Beyond uncertainties, Green postured that scientists must be aware of the momentum of technologies. The danger with momentum was that it led to "commitments in technology we have regarded as involving unacceptable injury." He warned, especially with new technologies, that "enthusiasm" and "optimism" must be tempered at the early stages until it is "free" from unacceptable injuries.[613]

Green then proceeded to summarize Freeman Dyson's "The Hidden Cost of Saying No."[614] While agreeing with the majority of Dyson's argument, Green made an addition that "society can more easily tolerate a postponed or even a lost benefit than it can actual injury." The voluntary moratorium on the two types of recombinant DNA research called for in July 1974, was "an admirable approach to the problem of protecting society against hazards inherent in scientific and technological advance." Further it was "particularly admirable" that the moratorium was taken by the "scientific community itself rather than having the situation await a more formal action by government."[615]

Green, unlike Dyson, did not propose a potential solution. He believed that the Conference was the correct approach and that he 'would not feel uncomfortable with any decision that results." In his conclusion, however, Green left the scientists with a suggestion. "[F]ind a mechanism whereby these risks can be forcefully brought to public attention and given time and dignity more commensurate with the time and dignity which

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[612] National Academy of Science, *Asilomar Tapes, Reel 1 Side 2b* (Recombinant DNA Molecules; Central Policy Files; NAA: 1975), Reel-to-Reel.
[613] Ibid.
[614] See Chapter 9.
[615] National Academy of Science, *Asilomar Tapes, Reel 1 Side 2b*.

is invariably given to benefits. Only with such mechanism can fundamental public values be brought to bear on important issues of public policy." Green's message to the scientists was to go slowly.[616]

After Green's discussion, six more talks by PWG members transpired ranging in topic from "Molecular biology of bacteria conjugation and conjugative mobilization of plasmid and other DNAs" to "Genetics and biochemistry of DNA restrictions." The last few talks were rushed in a failed attempt to make sure everyone on the schedule finished. Kenneth Murray waited until the next morning. After the last speaker, Richard Novick, a co-chairman of the PWG, quickly summarized the day and the feeling in the room. "I guess with that very brilliant presentation we have some inkling what kind of excitement there is in store with respect to some of these kinds of experiments and I hope we can work out a way of dealing with some of the possible hazards, so we can figure out ways they can go on. Let's have some beer."[617]

Day Two

On Tuesday morning, Kenneth Murray rushed through his talk on bacteriophage systems in order to get to the main topic of the morning: the PWG panel discussion. Though no one at the conference knew it at the time, the PWG panel discussion changed the tone for the remainder of the conference. The arguments made during the PWG panel discussion extended through the remaining two days of the conference, and participants

[616] Ibid.
[617] ———, *Asilomar Tapes, Reel 2 Side 2b* (Recombinant DNA Molecules; Central Policy Files; NAA: 1975), Reel-to-Reel.

and presenters staked out surprising positions about the future of the voluntary moratorium.

As the panel discussion got underway co-chairman Stanley Falkow, intoned, “Thus far, we have really, most of the speakers, have carefully avoided the question of biohazard, but that's what this meeting is about, and that's what we have to discuss this morning. The document is 35 pages long. It's single-spaced. It contains the philosophy of the committee. The philosophy in many cases was a compromise in feelings.” The document was the PWG working paper, and it contained a classification system for experiments from Class 1 to Class 6. Class 1 experiments had their biohazard assessed as “insignificant.” Class 6 experiments were judged too dangerous to be performed by anyone given current containment technologies. Classes 2 through 5 experiments fell into increasing levels of necessary physical containment. Novick acknowledged that the working paper was “by no means a final version,” but it was hoped that it formed a “basis for discussion.”[618]

After Novick summarized the PWG working paper and the rest of the working group moved to the dais to form the “firing line.” The committee opened the floor for questions. Paul Berg, as the conference organizer stepped forward to moderate the discussion and stated, “I think I'd like to encourage you and try to eliminate any shyness or uneasiness about speaking out. So, who'll be the first?” Silence. Then like a schoolteacher, Berg asked the provocative question “Shall I move adoption as it stands?” The question was greeted with laughter, and then the discussion began.[619]

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[618] _____, *Asilomar Tapes, Reel 3 Side 1a* (Recombinant DNA Molecules; Central Policy Files; NAA: 1975), Reel-to-Reel.
[619] Ibid.

The initial questions raised concerned where different experiments ranked in the scale presented by the PWG. Mark Richmond then addressed the biohazard concerns that lay dormant on the first day and echoed a refrain that was constant during the biohazard conference at Asilomar in 1973.[620] Richmond stated flatly "you just don't dump them down the sink. I'd like to see them, as a routine procedure, autoclaved." Falkow agreed with Richmond and emphasized "a specific point…is that we hope people will simply stop pouring their cultures down the sink. And that is a very important point of [the working paper]." Despite two years of discussion, the only agreement in the molecular biology community about safety was that the older scientists and the microbiologists thought that the rest of the field was ill trained to deal with anything pathogenic. Before the debate about proper training progressed, Joshua Lederberg stood up to address the panel.

Rising from his seat, too far away from the microphone for everyone to hear, the Nobel laureate expressed apprehension about the document. Though "sympathetic" with their goals and conclusions, Lederberg worried about the detail of the working paper and what that might mean for the final consensus. "My concern about the establishment of the guidelines is what happens to them when they are translated into the bureaucratic process? We have not had an opportunity to go over this in a way that it will exactly into a legislative document, and I'm a little concerned that the language that is adopted as a consensus here will" become law. What Lederberg wanted was a set of very general guidelines. He did not want any guidelines quickly turned into potentially restrictive law. Novick tried to assuage Lederberg's fears by responding that he could not imagine "no

[620] Hellman, Oxman, and Pollack, *Biohazards in Biological Research.*

matter how hard we work, of producing number one a fully workable document, a fair document and one that is free of any loopholes. So that's our implicit intention or hope in our minds." Lederberg sat back down, but he continually voiced this opinion over the next two days.[621]

Ephraim Anderson then rose and returned to the matter of laboratory safety. He asked a very pointed question to the panel. "Which of you have had practical experience of the handling, the isolation, the handling and the disposal of pathogenic microorganisms of epidemic potentiality?" The response of laughter only irritated Anderson, who then broke through the dying laughter with "I ask this. This is no joke." Stanley Falkow almost got his wish as the majority of the remaining time of the panel discussion focused on biohazard and laboratory safety. That was until James Watson stood up.[622]

Watson was nothing if not direct. The Nobel laureate who was one of the authors of the CRDM report calmly stated, "It's out of place, but I think the moratorium should end because when we originally met I thought that it might be a good time to have six months to decide whether we could hear anything that would frighten us." His reasoning was "we'd like to improve what we are doing and get safer things," but he actually liked not having any regulations. In fact, he viewed them as "almost impossible to set them up." He admitted that he was agreeable to vote for a prohibition against biological warfare, but "beyond that, I think it's outside the fact of being educated as to why we might be dangerous or why we might be doing something for which a technician could then sue us for vast sums of money if they got sick." For Watson, the solution and future

[621] National Academy of Science, *Asilomar Tapes, Reel 3 Side 1a*.
[622] Ibid.

for recombinant DNA research was that “we should use common sense that the dangers that are involved are probably no greater than that faced in any hospital all the time. And where you just sort of get to live with it.” If there were a lawsuit for an experiment that went badly, he believed it was the cost for being “stupid.” Watson finished and took his seat.[623]

Maxine Singer, who was part of the Organizing Committee and was involved since Berg asked for her opinion about his proposed experiment, stood up for the first time at the conference. She wanted to know “between last July and now which makes it possible to say at this point that we should undo or lift whatever was done in July because you're particular summary of that would be useful to all of us in thinking about it.” Before Watson answered Singer, the conversation drifted briefly back to issues of physical containment procedures. Berg soon steered the discussion back to Watson. Watson once again made his case for removing the moratorium.” I think sure you should realize if you put SV40 into a little plasmid and grew tons of it and passed it around without trying to kill it that's gross stupidity. But I am not sure how you can legislate it.” As for guidelines, Watson believed the vaguer the better. Watson indicated that “I think if we get precise it will be such the many of us in our labs will be faced with situation where we know someone is slightly cheating. And slightly cheating on regulations is impossible to defend. And then do you go along with it, and I can just see circumstances which would lead to massive dishonesty without any real benefit.”[624]

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[623] _____, *Asilomar Tapes, Reel 3 Side 1b* (Recombinant DNA Molecules; Central Policy Files; NAA: 1975), Reel-to-Reel.
[624] Ibid.

The discussion then returned to proper laboratory safety and trying to match experiments with the categories outlined in the working paper. Before the panel discussion finished, Berg made sure that DeWitt Stetten, chairman of the National Institutes of Health recombinant DNA Advisory Committee, had a chance to provide his input to the discussion.[625]

Stetten admitted to arriving "naive scientifically, but bested with the uncomfortable responsibility of chairing" the advisory committee's first meeting on Friday." After describing his strategy for factoring hazards, Stetten turned to federal regulations. "My own experience with regulation is that the pure regulations we have to live by the better off we are, and also that specific regulations are the ones that always cause the most trouble." He closed with his recommendation about the ideal form of the consensus. "I would therefore urge that the advice that be given be couched in as general terms as are deemed useful, and that specifics should be avoided if at all possible."[626]

At the end of the session, Sydney Brenner attempted to refocus the participants for the next panel discussion, if not for the remainder of the meeting. Brenner clearly stated, "the question we have to decide scientifically is whether our ability to put these things together causes any added enhancement of the dangers. That is the real question we have to decide." With the focus back on a question of science, the session broke for lunch.

After lunch the science continued. First was a session led by Brenner titled "Designing Safe Vectors," which quickly became "disarming the bug." The session was

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[625] ———, *Asilomar Tapes, Reel 3 Side 2a* (Recombinant DNA Molecules; Central Policy Files; NAA: 1975), Reel-to-Reel.
[626] Ibid.

based on a recommendation from the Ashby Report to create a biological containment system. The session focused around creating a bacterium, likely a modified strain of *E. coli* that only survived on a very strict chemical diet. The idea was that if the bacteria ever contaminated anything that it would be unable to live or reproduce.

The Viral Working Group (VWG) ran their two sessions in the same manner as the PWG. They presented seven papers based on hard science before opening up the panel discussion. Unlike the PWG, the VWG's working paper was one single-spaced page, and was as broad as the prior working paper was specific. Eschewing numbered categories, the VWG agreed on the standards set by the "National Cancer Safety Standards for Research Involving Oncogenic Viruses." The only edition that the group made to the standards was to handle "segments from highly pathogenic viruses" according to the standards for "high risk oncogenic viruses." The group also concluded that proper containment facilities existed, and as such, recombinant DNA research should move forward.[627]

The VWG's working paper only added to the confusion during the panel discussion that night as it only exacerbated issues left unresolved from that morning's panel discussion. Questions regarding the classification of experiments from one to six or the more general low, medium, high were asked in multiple different ways. Brenner's "disarming the bug" session opened up the discussion about the new possibilities of combining differing levels of physical and biological containment to create safer containment facilities. Participants' frustration levels were running high, as more questions were raised than clear and agreeable answers formed. When the discussion

[627] Berg et al., "Report of the Organizing Committee of the Asilomar Conference on Recombinant DNA Molecules."

about biologically safe vectors seemed to assume that it was inevitable, it provoked Stanley Cohen to flatly comment that the discussion had a "sort of a tone of 'Let's wait for the Messiah because he is just around the corner waiting to come.'"[628]

Like Brenner after the PWG panel discussion, Berg tried to bring the conference back to some general agreement at the end of the VWG panel discussion. He attempted to do this by testing the consensus in support of the VWG working paper. Though there seemed to be general agreement, there was pushback about taking any type of vote at that time. Berg aptly closed the day by stating "I think going round and round would be useless."[629]

Day Three

The final full day of the conference began with an odd letter in everyone's mailbox. The top of the letter read "OPEN LETTER TO THE ASILOMAR CONFERENCE ON HAZARDS OF RECOMBINANT DNA" and was from the Genetic Engineering Group of the Science for the People. Jonathan Beckwith of the Harvard Medical School headed the group.[630] Complete with corrected typos, the contents of the letter probably came as little surprise to those who were aware of the group or radical scientists. The three-page letter called upon the members of the conference to "not contribute to the inventory of tragic results already caused by, for example, radium, asbestos, thalidomide, vinyl chloride and dieldrin." The letter also put forward multiple

[628] National Academy of Science, *Asilomar Tapes, Reel 4 Side 2b* (Recombinant DNA Molecules; Central Policy Files; NAA: 1975), Reel-to-Reel.
[629] Ibid.
[630] See Chapter 3 for Jonathan Beckwith and the origin and philosophy of Science for the People.

times that the scientists should not make decisions "without public participation" because the risks are also borne by the public as well. The letter contained a list of five proposals.[631]

1. Involve those most immediately at risk – technicians, students, custodial staff, etc. in collective decision making on safety policy for the laboratory.
2. Integrate into the curriculum of biology and medical courses the social implications of present and future biomedical research.
3. Require social and environmental impact statements on the means and goals of biological research projects.
4. Continue examinations of these matters at public sessions of scientific meetings.
5. Expand participation in the advisory committee of the National Institutes of Health requested by the moratorium. The N.I.H. could be the structure through which the involvement of non-scientists in decision making could be implanted.[632]

The letter ended with a minimal request that the "moratorium should be continued until the above proposals are put into meaningful effect." Although not as stridently radical as it was five years prior, Science for the People's letter was unfortunately ignored by the conference. The letter could have been used as a catalyst to discuss the effects that the decisions coming out of the conference would impact society. Instead, the conference kept to its continued rigorous hard science dictate.[633]

The Eukaryote Working Group (EWG) started the third day, and little had changed from the discussion the previous night. The five presentations occurred in quick succession, and then the Conference once again arrived at a panel discussion. The EWG working paper was similar to that of the PWG in that it categorized experiments from 1 to

[631] "Open Letter to the Asilomar Conference on Hazards of Recombinant DNA, C. February 1975," (ALS: D Med: Recombinant DNA Molecules, Organizing Com for int'l Conf on, Report, Synthetic Nucleic Acids, 1974-1975; Central Policy Files; NAA.).
[632] Ibid.
[633] Ibid.

5. The discussion, unsurprisingly, quickly became bogged down in discussions about the need for vague guidelines versus specific guidelines as the Conference neared its close. The working paper also concerned some of the scientists who feared that consensus might lead to legislation. Midway through the discussion, an exasperated Berg asked "What's the alternative to trying to increase the…to match the level or risk?" To which Watson blurted out "You can't measure the risks." The discussion continued to lose focus as the scientists debated the possibility of measuring risk and the prospect of creating a classification system. Toward the end of the session, David Baltimore tried to get the discussion back on track "We are supposed to be the people who know about this stuff and can't go home and say nothing. It seems to me that's not a feasible result unless we can't agree."[634] Berg soon followed and warned the participants in a sober tone that "if we set standards that look like they are self-serving" we will have regulations imposed upon us. However, "if we error on the side of being too safe. We'll be fine" because any regulations can be lowered as research becomes safer. Though he did not achieve general agreement, he did not start another round of the now well-worn debates.[635]

Daniel Singer of the Hasting Institute opened the final session of lawyers at the conference. He complimented the scientists for their "unusual and important undertaking…it is an exercise on your part of an usually high level of public responsibility, of concern for the welfare of those who are around you, and concern for the development of science itself with a continuing vigor and with a continuing appreciation for the growing role science plays in the general life in all of us." The

[634] National Academy of Science, *Asilomar Tapes, Reel 5 Side 2b* (Recombinant DNA Molecules; Central Policy Files; NAA: 1975), Reel-to-Reel.
[635] Ibid.

lawyers, however, were not present to praise the scientists for their handling of recombinant DNA, but to provide an outsiders view. The first speaker after Daniel Singer's brief remarks on risk-benefit analysis was Alex Capron, a University of Pennsylvania Professor of Law and director at The Hastings Center.[636]

Capron's purpose was "to show how the law bears on the work of the conference." After two and half days at the conference, his view as an "outsider" was

> We were called here to discuss the biohazards of recombinant DNA, and by what means they could be minimized. Perhaps the tone of the meeting was set, however, when David Baltimore declared at the outset that this was to be a scientific meeting. And, indeed, to an outsider at least, and a number with whom I have spoken, it has often seemed like a technical meeting at Cold Spring Harbor and the like. I don't want to suggest that the technical aspects are unimportant for us…but the question remains why was so much time spent in sessions that seemed far removed from the immediate task at hand. The task that was forever being "to be dealt with on Thursday morning." I have a few answers, which I will offer to you to ponder for whatever it's worth. First as scientists, you were most accustomed to delivering and listening to papers of this type. Second, as complex as the scientific matters are, they are easier to handle than difficult task of thrashing out together the real issues, particularly when these can involve unpleasant disagreement. And third, it is exhilarating to hear about the delightfully clever things your colleagues have just discovered, so that you may emerge from a session not filled with the concerns about the hazards, but inspired instead by the desire to pursue the research to a next tantalizing goal.

He discussed that ignoring the law or legal institution was impossible. In fact, it was the public's purview to make "erroneous mistakes" by acting through the legislative process. He concluded "legal institutions are an inescapable part of your work whether you like it or not." He believed those institutions "can promote as well as restrict research. And given the nature of the research that you are doing, it is entirely appropriate that such

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[636] ———, *Asilomar Tapes, Reel 6 Side 1a* (Recombinant DNA Molecules; Central Policy Files; NAA: 1975), Reel-to-Reel.

bodies now have a say." Capron received a loud round of applause upon finishing, though there was little time to incorporate his recommendations into the final discussion.[637]

Following Capron, Roger Dworkin, Professor of Biomedical History at the University of Washington, discussed the "legal liabilities of investigators." He addressed the potential legal ramification if a recombinant DNA experiment had a hazardous outcome. While Dworkin admitted there was no law explicitly on point regarding recombinant DNA, he made it clear that a lead investigator or his institution had potential legal exposure. Dworkin walked the scientists through how tort law, workmen's compensation law, and even how Occupational Safety and Health Administration (and therefore the Department of Labor) law all had potential uses in the courts. After Dworkin finished the questions, the panel of lawyers, including Harold Green, took questions. The question and answer session lasted so long that someone asked Berg what time they were supposed to leave. When that matter of time was posed to Berg, the session had already overrun its allotted time, but the lawyers agreed to stay on and continue to answer questions. Berg and the Organizing Committee quickly retired to begin work on the writing the consensus.[638]

Day Four

The final day of the Conference was dedicated to the conference consensus. The Organization Committee completed the six page statement by 4:30am, and handed it out to the participants over breakfast. Just as Baltimore did in the first session of the Conference, Paul Berg defined the parameters of the final session. Listed as "Discussion

[637] Ibid.
[638] Ibid.

and adoption of conference statement," Berg defined the role of the Organizing Committee as an attempt "to try to determine if there was a consensus, the nature of that consensus, and to try to embody that in a statement." He believed that the committee's statement succeeded in representing "the consensus of the opinions of most of the people who are here."

While the consensus statement was non-specific, it had some real teeth. The Organization Committee decided to go with the "low-medium-high" classification from the VWG's working paper. These classifications were more restrictive than the National Cancer Institute standards. Low risk would have to have minimal physical containment recommended by the Institute standards, but medium and high risk experiments had to await the development of appropriate biological containment. In essence, the Organizing Committee extended the voluntary moratorium until such time that biological containment was feasible.

Berg then defined the participants' role. The participants were to help "clarify" the "undoubtedly inelegant phrases, omission, and perhaps even some confusions" of the late-night writing that went into the document. Berg then broke his chain of thought and used his "chairman's prerogative" to remark on the events that transpired over the last few months. He was especially proud that many of the scientists showed what they "can and will do when their conscious and the opportunity is at hand, when their conscience brings them to this kind of a challenge." Berg then quickly shifted gears back to the matter at hand: the consensus statement.[639]

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[639] ———, *Asilomar Tapes, Reel 6 Side 2a* (Recombinant DNA Molecules; Central Policy Files; NAA: 1975), Reel-to-Reel.

Berg further clarified the role of the conference participants. They were to "offer their suggestions one section at a time…bearing in mind that we are not debating the language, the exception where it bears on the substantive issue." Their comments would not change the statement in front of them, but would be used in the final report required by the National Academy of Science. When someone questioned Berg about this lack of input, Berg stated that the statement represented the Organizing Committee's "assessment of what went on at the meeting and our assessment of the consensus as it exists." The committee clearly did not intend for the statement to be amended by the floor. Reluctantly, however, the Organizing Committee acceded to the desires of the participants to discuss each section of the document and vote on agreement. If the majority favored a section then the section stayed in the consensus statement as written. If the majority voted against a section then it was altered or deleted.[640]

The Organizing Committee only lost one vote. One experiment was downgraded from the high to medium classification.[641] The Organizing Committee's sense of the consensus was correct. The consensus also served to diminish some of the sensationalistic press accounts of recombinant DNA, especially among those members of the press who attended the conference. The title of the Asilomar article on front page of *The New York Times* the day following the conference was "World Biologists Tighten Rules on 'Genetic Engineering' Work."[642] Science journals followed suit with their next issues: *Science News* ("Asilomar Decision: Unprecedented Guidelines for Gene-

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[640] Ibid.
[641] _____, *Asilomar Tapes*, *Reel 6 Side 2b* (Recombinant DNA Molecules; Central Policy Files; NAA: 1975), Reel-to-Reel.
[642] Victor K. McElheny, "World Biologists Tighten Rules on 'Genetic Engineering' Work," *The New York Times*, 28 February 1975.

Transplant Research),[643] *Science* ("Genetics: Conference Sets Strict Controls to Replace Moratorium"),[644] *Nature* ("Berg Conference Favours Weak Strains"), and *BioScience* ("Recombinant DNA Molecules: Biologists Draft Genetic Research Guidelines").[645] Not all of the press reports following the Conference were as positive. The *Washington Post* ran the disjointed headline "Genetic Research Ban Lifted: Strict Safety Standards Imposed" above and article that was generally supportive of the consensus.[646] The same was true with Michael Roger's account in Rolling Stone ("Pandora's Box Congress").[647] And the *Washington Star* ran a long piece titled "New Biology: Second Genesis of Pandora's Box?"[648] The Conference's end was the calm before the storm for most recombinant DNA researchers. The consensus would hold until the NIH published Guidelines in late June 1976.

The Ramifications

In his closing statement, Berg mentioned that "the situation is changing from the time the letter was published in July, which we might have referred to as phase one to now entering a second phase, and it's our estimate in the statement there which suggests that we're moving into the second phase."[649] The Asilomar Conference was a major event, and Berg could have easily guessed that debates over recombinant DNA would

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[643] Janet Weinberg, "Decision at Asilomar," *Science News* 107, no. 12 (1975)
[644] Nicholas Wade, "Genetics: Conference Sets Strict Controls to Replace Moratorium," *Science* 187, no. 4180 (1975)
[645] Cristine Russell, "Recombinant DNA Molecules: Biologists Draft Genetic Research Guidelines," *BioScience* 25, no. 4 (1975)
[646] Stuart Auerbach, "Genetic Research Ban Lifted," *Washington Post*, 28 February 1975
[647] Michael Rogers, "The Pandora's Box Congress," *Rolling Stone*, 19 June 1975.
[648] Judith Randal, "New Biology: Second Genesis of Pandora's Box?," *Washington Star*, 11 March 1975
[649] National Academy of Science, *Asilomar Tapes, Reel 6 Side 2a*.

enter a new phase. However, at the time of the Asilomar Conference, the participants were still in the first phase of the debates when nothing seemed inevitable, momentum was fleeting, and molecular biologists questioned their own advances. The questioning led to a voluntary self-imposed moratorium on research, which was faithfully abided. The first phase of the recombinant DNA debates demonstrated the limits of social responsibility in molecular biology. Scientists were willing to impose a moratorium, but they strictly limited their debates about it to internal questions of hard science, proper laboratory training, and containment policies. The first phase of the recombinant DNA debates was when the boundaries of the debates were set, a consensus was forged, and relationships were bound within the scientific community that would prove important as the debates soon went public.

Not surprisingly, one of the first places where recombinant DNA erupted as a public issue was in was Cambridge, MA spurred largely by Science for the People.

Bibliographic Essay

The recombinant DNA debates have been undertaken by journalists, scientists, philosophers, ethicists, social scientists, legal scholars, and, most recently, by historians. The body of literature was narrowed to include books exclusively on recombinant DNA or the 1970s. What emerged was a cross section of major works to deal with recombinant DNA and issues that directly affected it. The review is broken down into loose subject categories for simplicity and clarity. Within each category the books are presented in chronological order. An attempt was also made to provide competing viewpoints.

Journalists

By the 1970s, the authority of science and scientist began being questioned by the public and the first to fill this arena were journalists. The first of these was by Michael Rogers in his book *Biohazard*. The first quarter of the book is a personal account of his experience at the Asilomar Conference as a reporter for *Rolling Stone*, and the rest of the book tells the story of recombinant DNA as the story nearly simultaneously unfolds before the author with the final printing in 1977. Rogers understand the technical language of the scientists in a way that he is then able to make it accessible to the lay reader. The recombinant DNA story as told by Rogers attempts to balance the pro-recombinant DNA research with those opposed to it in a way that does not leave either side devoid of scientific authority.[650]

[650] Michael Rogers, *Biohazard*, 1st ed. (New York, NY: Knopf, 1977).

The second journalistic account was *The Ultimate Experiment: Man-made Evolution* by Nicholas Wade. Wade was the science writer from *Science* assignment to the recombinant DNA controversy from almost the first article with concerns about recombinant DNA to appear in *Science*. Though Wade's book is a bit more technical than *Biohazard*, it is still written in a style for the lay reader to understand. Wade, like Rogers, also attended the Asilomar conference, but does not spend a great deal of time on that issue in *The Ultimate Experiment*. The majority of the book is one focused on the issue of policy as control over a technology that could, if left unchecked, create great cures and diseases as well as a host of other benefits and problems for humans. Wade consistently, though implicitly, questions the authority of those pro-recombinant DNA researchers, who want little government, local or national, interference in their labs, because so little is known about the positive and negative implications of recombinant DNA.[651]

The third journalistic account was *Recombinant DNA: The Untold Story* by John Lear, a reporter for the *Christian Science Monitor*. As opposed to Rogers and Wade, Lear tries to frame the controversy through the prism of one of the major scientists, Robert Sinsheimer, who tried to find a middle road between those calling for little to no regulation to those calling for sever restrictions on research. Lear likens Sinsheimer to Old Testament prophet Jeremiah, and provides a much richest journalistic account of the many views scientists held as well as examined the public groups who pressed for tighter

[651] Nicholas Wade, *The Ultimate Experiment: Man-Made Evolution* (New York, NY: Walker, 1977).

regulation. Despite the large number of interviews, Lear's book falls short of telling an "untold story."[652]

The final journalist treatment was June Goodfield's *Playing God: Genetic engineering and the Manipulation of Life*. Goodfield, like Lear, did not attend the Asilomar Conference, though she did later listen to the recording of the proceeding, and as such *Playing God* is not as detailed account as *Biohazard* or *The Ultimate Experiment*. Playing God's focus is more on the arguments that are made by each side on the debate, which makes her argument a more philosophical treatment of recombinant DNA.[653]

Anthologies

Due to the vast number of debates concerning the recombinant DNA research in the late 1970s, two detailed anthologies were published in an attempt to provide an accurate representation of the all sides of the debate. In *Recombinant DNA: Science, Ethics, and Politics*, John Richards assembled a large number of authors to debate the different aspects. The book includes six attendees of the Asilomar Conference. Despite this Recombinant DNA focuses almost exclusively from the Asilomar Conference until the publication of the book in 1978.[654]

In *The Recombinant DNA Debate*, David A. Jackson and Stephen P. Stich included essays from scientists, doctors, philosophers, ethicists, and lawyers to include the majority of the debate topics: historical background, debates within science, and

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[652] John Lear, *Recombinant DNA: The Untold Story* (New York, NY: Crown Publishers, 1978).
[653] June Goodfield, *Playing God: Genetic Engineering and the Manipulation of Life* (London: Hutchinson, 1977).
[654] John Richards, *Recombinant DNA: Science, Ethics, and Politics* (New York, NY: Academic Press, 1978).

philosophical, legal, and social issues raised by recombinant DNA. In each section the editors provide the same number of opposition as defense articles. Jackson and Stich provide a forum for the reader to judge where the authority in each debate should lie.[655]

Histories

A Double Image of the Double Helix: The Recombinant-DNA Debate is a tight and terse book written by Clifford Grobstein, a developmental biologists, in the summer of 1978. Written a year after the height of the recombinant DNA debates, Grobstein, who was researching with recombinant DNA, presented a basic argument for the consensus reached at the Asilomar Conference. Since the book is supported by very little research, it serves more as a memoir of a scientist during the recombinant DNA debates.[656]

Sheldon Krimsky's *Genetic Alchemy: The Social History of the Recombinant DNA Controversy* was the first comprehensive history to deal with the public aspect of the recombinant DNA debates and how that impacted creation public policy. In *Genetic Alchemy*, Krimsky, a professor of Urban and Environmental policy at Tufts University, dealt with the policy aspect of the controversy. Krimsky makes use of the recombinant DNA archive at MIT to attack the rational of the pro-recombinant DNA research scientists. The authority for Krimsky should not be with the pro-recombinant DNA research scientists since they had shown that they were only out for their own personal

[655] David Archer Jackson and Stephen P. Stich, *The Recombinant DNA Debate* (Englewood Cliffs, NJ: Prentice-Hall, 1979).
[656] Clifford Grobstein, *A Double Image of the Double Helix: The Recombinant-DNA Debate* (San Francisco, CA: W. H. Freeman, 1979).

and scientific gain. The scientists were more concerned with their science than with the public safety and welfare.[657]

In the 1990s recombinant DNA was once again address, this time by an historian. *Molecular politics: Developing American and British regulatory policy for genetic engineering, 1972-1982* by Susan Wright is the best researched book to deal with the topic. Again, like Krimsky, Wright argues that the pro-recombinant DNA research scientists were only looking out for their interests as scientists when the policies pertaining to the use of recombinant DNA techniques, such as genetic engineering, were used. Wright further argues, though more subtly than Krimsky, that too much authority was given to the scientists and that a large measure of authority should have been taken from them to create a recombinant DNA policy that addressed the needs of public safety and welfare.[658]

The most recent history is *The Recombinant DNA Controversy: A Memoir, Science Politics, and the Public Interest 1974-1981* by Donald S. Frederickson, former director of the National Institutes of Health beginning shortly after the Asilomar Conference. Despite its occasional poor writing, the book provides an insiders account from the perspective of a policy maker, and more important provides insight into the personalities and politics within and between the House and Senate, and what that effect that had on the passage of recombinant DNA bills. Fredrickson is also explicit in laying blame at the feet of some scientists and policy makers, providing a one dimensional view

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[657] Sheldon Krimsky, *Genetic Alchemy: The Social History of the Recombinant DNA Controversy*, Third Printing, 1985 ed. (Cambridge, MA: MIT Press, 1982).
[658] Susan Wright, *Molecular Politics: Developing American and British Regulatory Policy for Genetic Engineering, 1972-1982* (Chicago, IL: University of Chicago Press, 1994).

of the opposition the NIH Guidelines, and giving himself more praise than perhaps he deserves.[659]

Document Reader

When *The DNA story: A Documentary History of Gene Cloning* was published in 1981, the controversy over the use of recombinant DNA for NIH approved laboratory use was all but over. The book consists of major documents in the history of the recombinant DNA controversy from the early 1970s up until 1980. The collection of documents attempt to provide a balanced picture of each side of the debates in the call for recombinant DNA policy to the creation of a recombinant DNA policy to the arguments that ensued over the recombinant DNA policy. Although providing items from each side of the debate, the book places the majority of the authority with the scientists, especially those pro-recombinant DNA research, and not the policy makers and laymen. This should come as little surprise as the editors were James Watson, a pro-recombinant DNA research leader, and James Tooze, executive secretary of the European Molecular Biology Organization, both of whom attended the Asilomar Conference. [660]

The 1970s

The existing histories also do not place the scientists within the larger world where they lived, nor do these other histories examine how the scientists' changing world effected the decisions that they made concerning recombinant DNA. It was in the early

[659] Donald S. Fredrickson, *The Recombinant DNA Controversy: A Memoir: Science, Politics, and the Public Interest 1974-1981* (Washington, D.C.: ASM Press, 2001).
[660] James D. Watson and John Tooze, *The DNA Story: A Documentary History of Gene Cloning* (San Francisco, CA: W.H. Freeman and Co., 1981).

years of the 1970s that these questions and decisions were made which would lead to an enduring consensus among recombinant DNA researchers in February 1975. After the consensus was reached among the scientists, they could present a unified front to deal effectively with questions, criticisms, and concerns of the public, legislators, politicians, as well as other scientists about recombinant DNA.

The essential first years of the recombinant DNA debate occurred at an unsettled time in the history of the United States. Socially and culturally the idealism of the 1960s was giving way to movements that were avowedly radical in their ideology and in their methods of protest. The decade included larger cultural, social, economic, and political events. The rights revolution, Vietnam War, the environmental movement, the rise and fall of Richard Nixon, Watergate, Jimmy Carter, the conservative shift, stagflation, and the oil shocks were all much more public and national events that shaped the decade. The major scientific advances that occurred during the decade seem lost in the major histories of the 1970s, which mainly focus on the major social, cultural, and political shifts of the decade. Of the three major histories of the 1970s,[661] only *It Seemed Like Nothing Happened* mentions recombinant DNA, and then only briefly. Peter Carroll introduces the recombinant DNA debates by way of the remake of the film *Invasion of the Body Snatchers* and the novel *The Boys from Brazil*. The one page devoted to the recombinant DNA debates oversimplifies it to the point of irrelevance. Perhaps it is justified that Carroll transitions quickly into the 1976 Legionaries Disease outbreak in Philadelphia

[661] Peter N. Carroll, *It Seemed Like Nothing Happened: America in the 1970s* (New Brunswick, NJ: Rutgers University Press, 1990), David Frum, *How We Got Here: The 70's, the Decade That Brought You Modern Life (for Better or Worse)* (New York, NY: Basic Books, 2000), Edward D. Berkowitz, *Something Happened: A Political and Cultural Overview of the Seventies* (New York, NY: Columbia University Press, 2006).

with: “The terror of an uncontrolled biological holocaust, lurking close to the kin of the culture, erupted with unexpected violence during the summer of the Bicentennial.”

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