implications which must be considered. Decisions concerning social consequences of research must be made in the public sector.

III. JUSTIFICATIONS AND RISKS

In attempting to justify a particular avenue of research, it is usually sufficient to show that the experiments will be scientifically productive. When the research appears hazardous, it becomes necessary to balance the possible benefits against the possible dangers. Those at risk must be involved in making the choice.

Risks are of two sorts: to the worker and to the public at large

Two sorts of risks are entailed in carrying out the research: those run by the researchers themselves, and those which affect a broader population. When the risks incurred will affect only laboratory personnel, it is sufficient to have safety procedures which satisfy the workers involved.[2] However, the major risks of research in Gene Implantation are run not simply by the participating investigators but by the public as a whole; therefore, the decision to proceed must be a public one. Benefits which will accrue to the general public are needed to justify the risks. Moreover, this research will, like animal virology, be very expensive to do, and should be shown to be a worthwhile investment (39, q9).

IV. PREDICTIONS OF BENEFITS

Proponents of this research claim that it will yield far-reaching benefits to humankind. Let me list some of the generally projected benefits of research in Gene Implantation (3,39, q18). Later I shall point out some of our objections.

1. Intellectual advances

We are told that invaluable knowledge will result from this research that cannot be gained any other way, and that this technology is the key to understanding the functions and control of DNA.

any organism which can exchange genetic material with E. coli is potentially as dangerous a host.

b. Drugs are not expensive to make

In any case, though such products as antibiotics are expensive to the consumer - that is, the patient - they are not really very expensive to make. The expense is due to excessive and unnecessary advertising and packaging costs, and to profit made by the manufacturers, distributors, and pharmacies.

c. Careless use may result in disaster

Furthermore, even with prices inflated in this manner, antibiotics have remained sufficiently cheap and abundant to lead to indiscriminate use which, in itself, constitutes a hazard. Some of you may have heard of the epidemic in the Yukon Territory in which many children have died or been brain-damaged by bacterial meningitis (13). Bacterial meningitis is caused by Haemophillus influenzae, many strains of which are now resistant to ampicillin. The infection is now being treated with chloramphenicol. The side effects of chloramphenicol are of two kinds: direct effects to the patient in the form of anemia and depression of the bone marrow; and epidemiological. Indiscriminate use of chloramphenicol in Viet Nam and in Mexico has resulted in chloramphenicol-resistant Salmonella typhi, which cause typhoid fever (29, q15). A chloramphenicol-resistant strain of Haemophillus influenzae has been isolated in Paris (2, 4, 42). It is likely that there will soon be strains of Haemophillus influenzae selected for which are resistant to both ampicillin and to chloramphenicol, which will make treatment of spinal meningitis very difficult. This is a flagrant example of how the selection of multiple drug resistance in pathogenic bacteria by indiscriminate use of antibiotics may result in unpardonable disaster.

d. Insulin

Proponents of Gene Implantation technologies often suggest that it would be

to be altruistic where our own work and self-interest are concerned. This is why it is so important to have people other than animal virologists and molecular geneticists on committees for the review of these questions. It is much too easy to argue that one's own work is the most important kind.

So far my discussion of the applications of Gene Implantation research to the cancer problem has focused on the pursuit of a more productive avenue - that is, prevention rather than cure. There are also serious drawbacks to be considered. We would worry that new cancers might be produced in the effort to cure the known ones.

Recently a group from Belgium reported their work with Agrobacterium tumafaciens, which infects plants thereby causing the disease called Crown Gall. A few days after infection a cancerous tumor is seen and the presence of the bacterium is no longer necessary to maintain the tumorous state (27, 28). Montague and co-workers have shown that the cancer is due to an oncogenic plasmid which is transferred from the bacterium into the plant cell (24,39,q22). This is a remarkable discovery and, though the detailed mechanism of action has not yet been elucidated, the parallel is striking: If in fact oncogenes are present in animal virus genomes and, therefore, in mammalian cells, there is obvious danger in combining mammalian DNA with coliform plasmids.

2. Critique of possible treatments of genetic diseases

a. We have not examined the political ramifications of the control of genetic disease

As regards curing or preventing genetic diseases, the cure appears worse than the malady. That a host of scientists are willing to plunge into this form of research without first carefully examining the political and social repercussions may be an example of the compartmentalization of our universities and of our minds. Even though one person can act in the various capacities of biologist, philosopher, parent, and artist, very rarely are these disciplines

4. Intellectual gains?

a. Freedom of inquiry has always been restricted

We have been accused of wishing to restrict freedom of inquiry. In a sense, this is true. But we do not deny the right to ask questions and to seek the answers; we deny only the right to create hazards in the process. We question the style, methodology, and timing of the technology of Gene Implantation. This sort of restriction is not new. It is already commonly accepted that human experimentation which endangers the subject either physically or psychologically is abhorrent; remember the Tuskegee study (36). Gene Implantation research is another example of a field of investigation in which there must be constraints because of the hazards to public health.

b. Problem-solving: There is always more than one solution

One of the most valuable lessons to be learned from the exercise of problem-solving, and one of the most exciting things about the human mind, is that there is never only one way to solve an intellectual problem. Of course, there may be a most elegant way, an easiest way, or a best way, but never just one way. What the best way may be is always open to debate and must be judged by the circumstances in which the problem is found. Questions of safety and ethics are among the relevant circumstances. In the case of the problem of genetic controls, there are sure to be alternative techniques which may be more cumbersome but less dangerous and controversial than recombinant DNA. In fact, alternative approaches to all of these questions were being pursued before the potential of Gene Implantation was realized; there is no reason to ignore these possibilities now.

VI. EVOLUTION

Individuals of two separate species can rarely cross-breed

As yet I have not discussed the ecological implications of the transfer of

many urged the government to refrain from using it. They were too late (23, 45).

We think that there is a direct parallel between the story of the Bomb and the problem of Gene Implantation. In both cases there were some scientists who warned of the danger, though not as forcefully as they could have. In both cases there was pressure to continue the work. There are, however, some clear differences. The Bomb was created during a war, and we are not under such pressure. Furthermore, there was only one atomic pile, and one bomb, while research in recombinant DNA is taking place all over the world, increasing the hazards to all of us. We think that the hazards of Gene Implantation may be equally great.

It is most important to realize that, while we can choose to stop utilizing nuclear technology, should any of the organisms which are created by DNA recombination escape, they will propagate themselves. We will have no way to monitor for them, or to stop their proliferation.

2. We do not support the status quo

In the history of science many great discoveries - that is, changes in the status quo - were greeted by opposition from traditionalists. Our specific criticism should not be viewed as a support of a traditionalist-status quo criticism of science. We agree with many of the supporters of this technique in believing that it is qualitatively different from anything which has ever been investigated until now. We oppose not its newness per se, but rather the intangible and incalculable hazards inherent in the technique of Gene Implantation, and therefore the haste with which the technique is being pressed into service.

FOOTNOTES, PART I

1. A survey taken in 1974 reports that 75% of the American public believes that science has changed things for the better (a point of view we do not dispute). Furthermore, 29% feel that science and technology have caused few of our current problems. In addition, 23% believe that science will solve most of our problems, while 53% believe that only some of our problems will be solved by science. It is important to note that responses reflecting positive attitudes toward science correlate strongly with education and income (26).

2. Unfortunately, in many cases, the workers are neither informed of the dangers, nor allowed to make modifications of safety proceedures. This is why we urge the organization of safety committees including students, technicians, custodians, dishwashers, and secretaries. At present, the principal investigator, who is responsible for maintaining safety, often does not enforce or even teach safety regulations (1).

3. There is another interesting case of possible damage to the biosphere resulting from technological manipulations. Some years ago there was talk of melting chunks of the polar ice cap so as to bring cold, fresh water to Southern California. The idea was abandoned because it was predicted that the effects on global weather conditions might be disastrous. Just recently, the Saudi Arabians confirmed that they had commisioned a study on the feasibility of bringing icebergs from Antarctica to melt for irrigation and drinking water. Clearly, there should be some means for international discussion before such drastic actions are taken. It was puzzling that there was no mention of ecological dangers in the New York Times article (34).

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ACKNOWLEDGMENT

I wrote this paper with support and generous criticism from the Group on Genetics and Social Policy of the Boston Area Science for the People, of which I am a member. This paper is meant to represent the view of this group.

Three philosopher friends of mine, J. Long, W. Berkson, and S. Richmond were good enough to remind me to keep in mind the History of Science.

Special help came from R. Goldstein, C. Orrego, P. Ward, J. King, E.S. Allen, D.J. Kayman, and S. Kayman.

F.R.W.

quickly. In 1934 the Race Hygiene Act was passed, and from what I've read there were 54,000 sterilizations in the first years.

Mr. DORNAN. How many years would you say this had been in ferment then?

Dr. KING. It had been in ferment from 1920 to World War II.

Mr. DORNAN. And were there lots of books that talk about the desirability of eliminating unwanted people in the 1920's?

Dr. KING. Yes. Now, the ones I've read were written in this country, because I don't read German, but there were plenty written in this country.

Mr. DORNAN. I would like to find some research. I recall hearing a German Ph. D., who had been in this country for about 30 or 40 years, who spoke of some books during that period that were written about unwanted people, but referring to older people, deformed people, and it was, I believe, in the early 1920's, which had nothing to do with Adolf Hitler.

Dr. KING. I must say there is some contention in the genetic community—and I note the presence here of Dr. Lewis, who is one of the distinguished policymakers in the National Science Foundation, over those issues. I would describe the split as follows: Some geneticists say, "The more we learn about genes, the more we understand the need to protect our genes, from carcinogens, from radiation, from mutagens." The opposing tendency, is to be interested in altering genes, to want to manipulate them. It leads to developing the technology to manipulate genes, rather than developing the skills to protect them.

It's not that we want to inhibit scientific research. We just want to make it easier to do the kind we need and maybe a little hard to do the kind that hurts us. It's not a question of repression or closing off research. It's just having people educated so they understand what can go wrong.

Mr. DORNAN. Mrs. Taft, did you have a comment on what Dr. King was saying?

Mrs. TAFT. I can wait.

Mr. THORNTON. We are running quite short of time, and I would like to recognize some of the other members of the committee.

But first I would like to place in the record at this point of the discussion the following observation. The terrible, barbaric activities of Hitler's Germany were the result of Government action and not of scientists in a laboratory. The actions were based upon ignorance, rather than upon knowledge, and stemmed from a society which decided to repress knowledge and information, which engaged in burning books, in distorting history, in adopting dogmas rather than the true scientific curiosity upon which the Western civilization have made great headway since the Galilean time, when dogma was finally put to the test and scientific inquiry opened.

For that reason I am always reluctant to see a discussion center upon the horrible events of Nazi Germany. I think the thing we can learn from that terrible experience in humanity is that man is capable of making horrendous mistakes, that those mistakes are usually made when we are ignorant of the consequences of our actions, and it does mean that all of us—and I think all of us would agree—have an obligation to try and educate ourselves on the risks and benefits of the choices that we are called upon to make.

Mrs. TAFT. These public members would be in more immediate contact. They would sit on the institutional review board and might be able to have greater contact with both local government and could serve a very essential role to Federal authorities, if necessary.

Mr. WHEELER. Could I just add one thing, Mr. Chairman?

Mr. THORNTON. Please go right ahead, Mayor Wheeler.

Mr. WHEELER. At the university we've gone through the process, and we have a committee with the faculty, we have a laboratory technician who works in the biochemistry lab, and we have a public representative.

Now, that's all well and good. But I think, to go back to what Dr. King said earlier, that there needs to be a body that is not necessarily responsible to that institution, but responsible to the community.

Mr. FUQUA. To the public interest.

Mr. WHEELER. Yes. I would hope, and I made the suggestion, and I think that the university has in its mind somewhere to create a broader general overview committee, in which there might be more public participation. But I think the community has that right—and I made a suggestion, I think, in my own paper that I will attempt to set up some sort of a policy level committee.

Mr. FUQUA. Mr. Chairman, I realize we're running past.

I want to thank the witnesses. Thank you.

Mr. THORNTON. Thank you very much for your good questioning.

I would like to recognize Mr. Pursell, who is here in preparation for our markup session, to make such statements or ask such questions as he may wish.

Mr. PURSELL. Thank you, Mr. Chairman.

I appreciate the excellent testimony we've heard here today.

Mayor, we're very pleased that you have taken the time to join us here.

We have a full committee meeting here in a few minutes.

I appreciate, Mr. Chairman, the opportunity to hear some of the testimony, and we appreciate those efforts in this program this morning.

Thank you very much.

Mr. THORNTON. I would like to ask each of our witnesses this morning if you would be willing to respond to such questions in writing as may be submitted to you by other members of the committee or the staff.

Mr. Flippo, do you have any statement, or comments?

Mr. FLIPPO. No. Thank you, Mr. Chairman.

The testimony of this committee and the questions to the panel have not nearly exhausted my curiosity, but in the interest of time I would not have any questions.

Mr. THORNTON. Thank you very much, Mr. Flippo.

Mr. Hollenbeck, do you have any questions?

Mr. HOLLENBECK. No, Mr. Chairman.

Mr. THORNTON. Mr. Dornan?

Mr. DORNAN. I would just add my association also with your eloquent closing statement.

All of the serious discussions we've had about mistakes in the past can sometimes come down to the focus of one broken-hearted set of

SCIENCE POLICY IMPLICATIONS OF DNA RECOMBINANT MOLECULE RESEARCH

WEDNESDAY, MAY 4, 1977

HOUSE OF REPRESENTATIVES,
COMMITTEE ON SCIENCE AND TECHNOLOGY,
SUBCOMMITTEE ON SCIENCE, RESEARCH AND TECHNOLOGY,
Washington, D.C.

The subcommittee met pursuant to notice at 9:38 a.m., in room 2325, Rayburn House Office Building, Hon. Ray Thornton, chairman, presiding.

Mr. THORNTON. The hearing will come to order. Good morning. It is a pleasure to have this distinguished group of scientist panelists before our continuation of hearings on the science policy implications of recombinant DNA molecule research.

Today we will be amplifying on yesterday's discussion concerning the subject of public participation in scientific and technical decisionmaking. We will be considering such questions as what actions should or could the Government take to encourage scientists to alert society to potential impacts of new developments in research, define terms such as what is meant by the public or the public interest, freedom of scientific inquiry, studying who among members of the public should be involved in scientific and technical decisionmaking processes such as the one we are focusing upon, and what ways might be useful in resolving value conflicts among various groups which are involved in these issues.

I hope to continue the panel format which has been so successful in previous hearings. We are very pleased to have each of our panelists with us this morning. We are hoping that Ms. Nelkin will be here in a little while. Dr. Stone of the Federation of American Scientists will be our first witness.

Mr. Alan McGowan of the Scientists Institute for Public Information will be next. Dr. Norman Wengert of the department of political science, Colorado State University is next. Dr. Richard Trumbull, the executive director of the American Institute of Biological Sciences is fourth, in that order.

I want to express my appreciation to each of the witnesses for their fine prepared testimony. I think it might be appropriate to insert those prepared remarks in full in the record.

[The documents referred to follow:]

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TESTIMONY OF
DR. JEREMY J. STONE
DIRECTOR, FEDERATION OF AMERICAN SCIENTISTS
BEFORE THE SUBCOMMITTEE ON SCIENCE, RESEARCH, AND TECHNOLOGY, OF THE COMMITTEE ON SCIENCE AND TECHNOLOGY
U.S. HOUSE OF REPRESENTATIVES, May 4, 1977

Thank you for inviting me to appear as a witness in these important hearings. I have the honor of representing the oldest American society devoted to public interest science activities, the Federation of American Scientists, founded in 1946. Indeed, we are not only the oldest but, to this day, the only scientific society organized as a civic organization--rather than as a tax-deductible 501(c)3 organization--so that we may pursue the public interest, as we see it, wherever it leads us, even into legislative activity. We represent approximately 7,000 dues-paying scientists and 50% of the American holders of the Nobel Prize.

I propose to state some lessons of the recombinant DNA experience and then to focus on the first question posed in my invitation.

Lessons of the Recombinant DNA Experience

First, the recombinant DNA chronology confirms that there are enough public-spirited biomedical researchers in the community to assure the society that new and potentially hazardous lines of biological research will be brought to public attention. The biologists have followed in the footsteps of the nuclear physicists who founded FAS--then the Federation of Atomic Scientists--in showing concern for the social implications of their work.

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example of the kind of scientific results that normally come to public attention; it is simultaneously more obscure and, at the same time its perils are especially easy to exaggerate. Therefore, all things considered, I believe the public reaction has been restrained and has reflected the high regard in which scientists and science are held.

I turn now to the first question:

1) What actions could the Government take to encourage scientists to alert society to the potential impact of new developments in research?

The Committee should understand that the reason most scientists need this encouragement lies not only in the pressure of their work, but also in a fundamental uncertainty they feel whether or not this function is one of their scientific responsibilities. American scientists are not sure to whom they are responsible and for what. To most American scientists, "scientific responsibility" on public policy issues means a responsibility to the scientific community to avoid actions that may be thought by their colleagues to demean science (i.e. to avoid irresponsible conduct). This point of view induces them to a caution and a precision that makes it highly difficult for them to function effectively in public debates (that inevitably have unsatisfactory ground rules) and to do so on matters of public policy (that inevitably merge science, public health standards, and values).

A minority of scientists do believe, as the Committee seems to believe, that "scientific responsibility" means a responsibility to the scientific community only on matters wholly contained within that community (e.g. plagiarism) and mandates a responsibility to society (social responsibility) on public policy matters that transcend the scientific community. It is this latter school of thought that permeates our organization.

These two points of view are contrasted in our F.A.S. Public Interest Report of December, 1976 along with an analysis of the corresponding problems of social responsibility in the Soviet Union and China. With the Committee's permission, I would ask to submit this publication for the record.

While a minority of socially concerned scientists is, in principle, enough to provide society with an early-warning network, in practice, it would be wise to enhance that capability.

With or without contracted studies to review, the Committee could hold hearings every two or three years on this subject so as to induce scientists to step forward by providing a suitable platform for their pronouncements.

LISTEN BETTER:

The formulation of the initial question assumes that, if only a scientist would speak out, society would immediately respond; nothing could be more misleading. Normally, some scientist is both willing and able to describe any given future potential hazard. But without encouragement, if then, he may not be willing to shout about it, to lobby concerning it, in short, to make a career of calling public attention to it. People in authority have to be willing to pursue the issue.

In doing so, they must pay less attention to status. For the most part, prestige is never having been wrong or been thought wrong (as in having been right "too soon"). Such reputability is too often earned and maintained by excessive caution. As a result, the first warnings of danger ehead virtually never come from self-consciously "prestigious" institutions as the National Academy of Sciences but usually from less official groups or selected individuals. Thus, where early warning is desired, the more established the group, the less useful it may be. Also, in scientific affairs especially, where truth rather than a consensus is desired, committees should be taken much less seriously than gifted, knowledgeable, and perceptive individuals. In short, societal government organs must be prepared to entertain and examine--if not decide--the merits of various expressions of concern, without waiting for them to be validated by the more ponderous mechanisms of bureaucratized institutions.

But no matter how ready society is to hear, some amplification of the voice of individual scientists is necessary. How can this be done?

Science For Citizens Program

The great democratic innovation of the 1970's has been the proliferation, and institutionalization of the public interest group. These organizations are formed around some perception or predisposition about where the public interest might be found (e.g., that the environment should be protected, the arms race controlled, or the laws enforced). Their use of the word

But the decision to subsidize such activities has already been taken. In the first place, the business community is permitted to use the equivalent of public monies for its legislative and legal actions when it deducts those expenses from its taxable income, thereby shifting the tax-burden for activities that are not only controversial but designed to provide profit to private individuals.

Second, as of last year, the tax-deductible groups have, as noted, been permitted to engage in legislative action thereby using tax-deductible monies for legislative work. The financial implications of this decision are equivalent to authorizing funds from the Treasury, and the Government has no control whatsoever on the projects undertaken as it does in the case of Science for Citizens. Indeed, the National Science Foundation is invariably sensitive--terribly sensitive--to the concern of Congress and indeed to every individual rank and file Congressman. No matter how well funded is the program, NSF is patently not about to fund researchers who are all interested in the same subject, or who share the same point of view, or who will ally themselves with the same or similar groups or who will all work on matters of legislative interest. You can depend upon NSF to be cautious and you can watch the program in action.

Third, the groups involved are going to engage in legislative and legal action whether or not Congress assists them to gain scientific expertise. The only question is: Will their positions be more or less responsible--better or less well grounded in what the scientific community knows or suspects?

Finally, the Science for Citizens program does not give funds to the public interest groups but to the scientists who work with the groups involved, so that the government subsidizes socially concerned scientists, rather than action organizations, in order to make it possible for these scientists to get their message across.

Obviously, the Science for Citizens program is intended to do many much less controversial activities--which I support, a fortiori. And it assists scientists whose message goes far beyond the implications of future research--the issue before us now--but reaches those

see fit. Participating in the public debate is an abrasive process for the individual scientists and, for most scientific organizations, a divisive process. Some praise would help keep them at it. For this reason, F.A.S. gives annual public service awards to scientists for science and society activities. The Forum of the American Physics Society has begun to do the same. If Congress and the Executive Branch would offer some kind of recognition, this would presumably help. And there is nothing wrong with calling in representatives of the scientific societies and asking them why they are not doing more in this area. Prod them. We do.

Continued from page 1

Standards

The central issue is how standards of responsibility in public communication should be maintained.

There should be standards. We do believe that scientists should hew to a higher ethical standard than that which need be obeyed, for example, by politicians. Scientists should: avoid dogmaticism; make their assumptions as overt as they can; qualify their remarks as well as conditions permit; be willing to surface, recognize, and admit weaknesses in their own argument; be ready to reason with those who disagree; and, in general, behave in a civilized fashion.

What we doubt, however, is the ability of professional scientific organizations to monitor and maintain these standards. Scientists involved in the public debate confront problems totally unfamiliar to these traditional organizations: unusual media conditions; the necessity to work from inadequate sources; enormous uncertainties about facts; pressures of time; tactical decisions concerning allies; controversies mixing values and facts; and many others. As a result, the traditional professional society really has no consensus, and hence no standing, with which to determine whether a scientist met his obligations to the public in a praiseworthy or censurable fashion. These are not questions of referees, of publication disputes, of methods of scientific argumentation. These are problems far more unruly.

Another method for maintaining standards is no better. This is the model known to lawyers, doctors, and engineers; these disciplines have codes of professional responsibility designed to monitor interactions between their professionals and the public. But codes of this kind have not worked well, often degenerating into self-serving efforts to protect the marketability of the scientific technology at issue. And, in any case, no formal code can resolve the multidimensional aspects of dealing with real problems in a real political world.

Marketplace of Ideas

What is left? In the first place, in the public arena, for the most part, the solution to poor analysis and scientific distortion is better analysis, and critiques of that distortion. In this sense, the solution to the involvement of scientists whose views or behavior one regrets is one's own involvement. We believe that, in the clash of scientific interpretations and opinions, those who apply the scientific ethos tend to prevail because those who apply that ethos most steadfastly enhance their credibility over time both with other involved scientists and with the public.

Moreover, in America, we have some faith that the societal methods of monitoring the public debate will be generally adequate to control scientific contributions just as they absorb the specialized contributions of many other kinds of experts. A competitive market place of ideas — including, of course, criticism by fellow scientists — will keep the discussion relatively honest.

To the extent to which the free market of ideas fails, it will be necessary for those scientists who are themselves involved in the public debate to evolve their own standards. Public interest scientists should have the right to be judged by their peers — by others who have run the societal gauntlets involved; by others who have appraised the options available. Through their own peer-group pressures — and their public service awards — scientists involved in public debate will provide role models for each other.

In sum, the solution to the interminable dispute over scientific behavior in the public arena is not to be found in merely repeating what scientists have preached as responsible conduct inside science but in what they come to practice collectively as social responsibility outside science. □

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the Soviet Pugwash participants to explain arms control to their government in the period between 1955 and 1970 before serious and sustained official talks began. No doubt there is much more that transpires within the permitted limits of discussion, struggles to clear up Lake Baikal and the like. But it is significant that real manifestos explaining science and social responsibility, such as Sakharov's *Progress, Coexistence and Intellectual Freedom* had to be smuggled to the West.

For the most part, the Soviet scientific community fights not for social responsibility but for unfettered foreign contacts, for free exchanges with other scientists.

To what extent are these problems arising from the nature of communism and to what extent from the distinctive cultural and historical conditions existing in the Soviet Union? Obviously views differ. One who traced the problem simply to economic planning was Friedrich A. Hayek, Nobel Laureate in economics. In *The Road to Serfdom* (1944) he argued that facism and communism were merely:

> "variants of the same totalitarianism which central control of all economic activity tends to produce."

He believed that an unforeseen but inevitable consequence of socialist planning was to create a state of affairs in which totalitarian forces would get the upper hand.

Maoism and Social Responsibility: Theory and Practice

The Chinese go much further than Bernal. The responsibility for the use of science is not a "joint responsibility" of scientific workers and the people at large — instead, the ideology gives much more weight to the public. Indeed, the scientists do not even have the primary responsibility that Bernal advocated for science itself. Instead — to summarize a friendly review by SESPA *(China: Science Walks on Two Legs* Avon books, 1974) — the literature shows constant emphasis on cases where "the peasants were ahead of the theoreticians." Efforts are made to demystify science, to deny that science is "too deep" for ordinary people, to combine the efforts of specialists and non-specialists alike and, above all, to combine "theory and practice." It seeks, in short, to reverse the saying of Mencius:

> "Those who work with the heart shall rule. Those who work with hands shall be ruled."

The cultural revolution instructed researchers to avoid the three divorces: "between politics, practice and laboring people." It led to debates over whether scientific papers should be signed individually or collectively and how collectively. It sent scientists out to the farms.

The net effect of these doctrines in practice is not now known. In the first place it is not very well understood why modern science did not develop in China for the past few hundred years, and this undoubtedly reflects casts of mind and cultural traditions to which this ideological approach is directed. Furthermore, when Joseph Needham began his celebrated investigations into this first question, he uncovered still another related conundrum: why was Chinese science ahead of the West in the period before the West's industrial revolution? There is obviously much in the notion of science and society in China that we do not understand.

Americans tend to think of ideology as a superfluous contaminant of law, regulations and tradition. In fact, these ideological injunctions — as with all ideology in China — are playing an active coordinating role instructing 800,000,000 citizens how to conduct their business. What do you do when you don't have law? For example, under the notion of the social reponsibility of science, enterprises are encouraged to allocate a certain portion of their funds for anti-pollution measures where formerly they might have made all efforts to increase production and exceed quotas. Similarly enterprises would have to inform workers that excessive sound might impair hearing. The desirability of an ideology that presses for this kind of activity is in accord with thinking in the Western democracies of socially concerned scientists.

On the other hand, most FAS scientists would look with horror at the likely disruptions of the Chinese scientific community, in practice, when forced to confront interference in the workings of the scientific community itself. No doubt scientific careers have been destroyed from "wrong thinking." And certainly, Chinese scientists have fewer rights of expression and communication than even those Soviet scientists about whom FAS is concerned. No doubt, the Lysenko affair is being repeated many times over in China. On the other hand, again, do Western scientists of developed countries have the perspective on the needs of an underdeveloped country to chide it for insisting that science be developed with applications first and foremost in everyone's mind?

In short, China exemplifies the most thoroughgoing destruction of barriers between the scientific community and the public. The destruction in one direction looks somewhat better than the destruction in the other, but we lack, at present, a sense of having standing to judge.

Scientific Responsibility in the Western Democracies

The basic theoretical issue in discussions of "scientific responsibility" in democratic states is "responsibility to whom." The progressive view largely agrees with Bernal's formulation; indeed, our Constitution carried these sentiments before he drafted them for WFSW. Here the responsibility is to the *public*. But the traditional view believes that the responsibility of scientists is a responsibility to the *scientific community* to act in ways consistent with the scientific ethos.

The traditional view is worried abont the effect on the public image of science of scientific involvement in public debate.

Thus in an October 11, 1976 speech, Dr. Philip Handler wrote:

> "We have learned that the scientist-advocate, on both sides of such a debate, is likely to be more advocate than scientist and *this has unfavorably altered the public view of both the nature of the scientific endeavor and the personal attributes of scientists."* (Emphasis added).

He went on to urge such scientists to be as "honest, objective, and dispassionate" in describing technological risks to the non-scientific public, as they would have to be in the self-policing scientific endeavor. (However, Dr. Handler was far from fulfilling his own charge in this speech; see page 7 of this Report for a number of examples.*)

*In fact, this individual's tendency to rhetorical exaggeration is notorious. For example, when the House of Representatives voted to require the National Science Foundation to let it review NSF grants before their final NSF approval, the Academy President charged the Congress with an action that was "tantamount to book burning" and to having adopted a procedure "appropriate only to authoritarian regimes." This can hardly influence Congressmen favorably in their assessment of "the personal attributes of scientists."

particular, disfranchise hundreds of thousands of scientists from political rights accorded their fellow citizens).

This view of what scientists do and should do in a Western democracy is the scientific analogue of a civics book discussion of how democracy works.

In the first place, the policymakers need inferences since facts seldom go far enough. This was put well in a *Nature* editorial of October 14 entitled "More than Facts, Judgments":

> "The scientist is most unlikely to be able to deliver to the decision-maker any useful sort of factual statement, because he is hardly going to be allowed to perform the appropriately large experiment or observation. All he can generally supply in the way of facts is some results from pilot projects, some calculations which may be relevant and so on. What the good scientist should also be competent to provide, however, is inference, and this albeit tentative and hedged-about, is what the decision-maker needs and what the science court seems to avoid.
>
> "Factual statements of the highest presumptive validity would merely be about rats, about rocket samples, about tensile strengths. Those involved in public policy need to know whether, in the scientist's best judgment, such statements can be generalized. Intelligent customers for these sorts of judgments know full well that scientific 'truth,' being a whole level higher than facts, is often every bit as elusive and changeable as political and economic 'truth.' But they still expect the scientist to go beyond the solid ground of his facts."

In the second place, the policymakers need policy analysis. Kenneth E. Boulding put it this way in a *Science* editorial (October 31, 1975): "The decision maker wants to know what are the choices from which he may choose" and "had agendas make it difficult to make good decisions."

Finally there is the all-important issue of political action by scientists. A *Science* editorial of November 28, 1975, observed:

> "If it is to be effective, the scientific community must learn to deal with Congress as it is, not as the scientist thinks it ought to be."

Branscomb Committee Takes Modern Approach

A more modern approach to scientific responsibility than that expressed in the *Wall Street Journal* was announced at the same conference at which Dr. Handler's speech was given, by a NAS Committee on "Science Technology and Society." It urged scientists not to view themselves "only as the custodians of knowledge, aloof from world affairs . . ." It said their role was:

> "not only to contribute new knowledge, but also to participate in the creation, evaluation, and application of the right technologies for societal use"

It urged scientists to "rethink their roles and the roles of scientific institutions."

The report said that the:

> "values by which scientists judge one another must undergo an evolution which elevates the incentives for responsible professional performance and high-quality research applied to problems of public importance . . ."

These were tasks that must be undertaken by professional societies, international unions and scholarly institutions and could not be left to legal or political institutions. (The 17 person Conference issuing this document was chaired by Lewis M. Branscomb of IBM and contained such American representatives as: Harvey Brooks, Roger Revelle, Stephen Schneider and Herbert York).

The traditional point of view in the scientific community has always feared too much emphasis upon the social ends of science. In 1945, Michael Polanyi called such emphasis "misguided generosity" that weakened the "autonomy of science." In 1949, he wrote that:

> "We scientists are pledged to a higher obligation, to values more precious than material welfare; to a service far more urgent than that of material welfare."

This point of view still exists, but in a muted form. Dr. Handler's October speech said that it was a challenge for the scientific community to "be seen as honestly responsive" to the needs of society. But he strongly urged scientists not to justify their research on social grounds except on the "historically valid argument" that science's benefits have come from permitting science what he earlier called its "own internal sense of direction."

He felt that scientists who emphasize the social utility of science:

> "force themselves to take a responsibility for technology which they should not have to take, because science is not technology and should not be held to account for those negative consequences which, rightly or wrongly, are being laid at the door of technology."

Responsibility For What?

But if scientists are not responsible for the technology that arises from science, what would they be responsible for? It is rare that science causes problems without an intervening technology. It is striking that this speech explained Pugwash not in terms of the social responsibility of scientists who built the bomb but simply because scientists were good at talking to one another:

> "Nor is it a problem in science that there is now in the hands of the military several hundred times more explosive power than was used in the totality of World War II. But because members of the scientific community, regardless of nationality, understand each other easily, the scientific arena offers a special platform for discussing the problems of arms control and disarmament, as the founders of the Pugwash movement recognized."

In fact, what they "recognized" was a sense of responsibility.

The NAS Committee is right. There is no safety today in a restraint that keeps scientists out of the debate. The scientific community that ignores the direct and side effects of its work on society is going to be blamed for them, all the more for its insensitivity. Conversely, the scientific community from which scientists emerge to take responsibility for, and to assist in managing, the implications of its work is going to be regarded with sympathy even when things go wrong.

To take a concrete example: What if the Federation of American Scientists (*nee* the Federation of *Atomic* Scientists) and the Bulletin of the Atomic Scientists, had not been created? What if atomic scientists had shown no interest in controlling the bomb or in the political and educational action required? What if the scientific community had provided "only the facts" and "only when asked" and had avoided being "shrill" and shown "restraint?" Would science and scientists be better thought of in Congress, among the press, in the media and in the public? Who can think so? □

overlook clippings and, when we noted their absence, agreed to send them.)

The only complaint that comes immediately to mind involves the FBI summary conclusion about some FAS officials having been "*described* as communist and pro-communist." (Emphasis added). While this was at least literally true in the tense forties and early fifties when people were freer in offering descriptions of others in those terms (and when, before Khruschev's denunciation of Stalin and the suppression of Hungarian, and Czechoslovakian uprisings more people might have fairly been described in that way) it is certainly not an accurate observation today. Evidently the cost of not having the FBI investigate one's organization continuously is an outdated investigatory report.

Items in the file included:

Item: In 1960, the Director of FBI's L.A. office reported on our (now divested) L.A. Chapter and noted that he "feels certain that the degree of CP (Communist Party) membership" in the chapter was "negligible." He termed the 75 members mostly "liberal in their thinking and mainly interested in peace and prosperity."

A summary memorandum reviewed a substantial number of chapters briefly concluding, in each case, that none were communist dominated but remarking variously that "visionary liberals" did take part or that "some members were communist sympathizers" and so on.

Item: 1952, FAS was complaining that the security requirements for alien scientists was so high that they could not visit the United States. The visa division wrote FBI at some length saying:

> "if the scientists really made an issue of it, it was a matter which should be handled by the Interdepartmental Committee on Internal Security rather than unilaterally by the Department of State."

Item: 1950, a report to FBI details the demise of the New York FAS Chapter; its decline is said to have begun during 1948 at which time three scientists, whose names are given, were defeated for re-election to the Executive Council. These three, termed a "pro-communist minority" then dropped out. (Two of the three subsequently became officers of the World Federation of Scientific Workers described in our November Report). □

FAS PUBLIC INTEREST REPORT (202) 546-3300
307 Mass. Ave., N.E., Washington, D.C. 20002
December 1976, Vol. 29, No. 10

☐ I wish to renew membership for calendar year 1976.
☐ I wish to join FAS and receive the newsletter as a full member.
Enclosed is my check for 1976 calendar year dues. (☐ I am not a natural or social scientist, lawyer, doctor or engineer, but wish to become a non-voting associate member.)
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NUCLEAR WEAPONS: CHINA & U.S.S.R.

On October 17, the People's Republic of China detonated its 20th nuclear explosion; the fallout from this atmospheric test was detected in America.

FAS wrote the Administration urging it to offer to sell the Chinese such (excavation and instrumentation) equipment as might make it possible for the Chinese to move these tests underground. Such a decision would put the Chinese in effective compliance with the Partial Test Ban Treaty. Our proposal was received as an ingenious and constructive suggestion by a number of high officials.*

Research revealed that only three of the 20 Chinese tests have been underground. Two were less than 20 kilotons and one in the "low-intermediate yield range." Other tests have ranged up to 3 megatons.

Threshold Test Ban In Difficulties

Meanwhile, Soviet underground testing has become the source of controversy. The United States and the Soviet Union have signed but not yet ratified a ban on underground tests above 150 kilotons and have agreed to stay below the limit pending ratification. However, it now appears that the United States cannot estimate the size of the Soviet tests with sufficient accuracy to monitor the agreement by national means. At the moment, the size of the tests can be gauged only up to about a factor of "two". This means that a test which the Soviet Union knew to be 100 kilotons — well below the limit of 150 — might appear to some U.S. estimators as 200 kilotons or well over the limit. It was believed that further experience would lower the range of uncertainty somewhat. But the agreement — which FAS opposed on a wide number of grounds beside this one — obviously lends itself to nasty interagency disputes about compliance. □

*However, in a letter that Parkinson would admire, the Department of State eventually responded with two contradictory assertions:

> "The Chinese atmospheric testing cannot be attributed to technological deficiencies since they have already conducted three underground tests, the latest on October 17 this year. We will, none-the-less, bear your suggestions in mind in formulating our future policies in this field."

The first of these sentences is obviously false — that small tests have been underground does not establish that the Chinese do not have technological problems. The letter was signed at a low level. FAS wrote back expressing our bemusement.

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Alan McGowan

PUBLICATIONS (partial listing)

Funk & Wagnalls New Encyclopedia, 1977 Yearbook. "Energy"
Think. "Science Court: Good or Bad?", March, 1977
The Sciences. "In the Matter of the Science Court", August, 1976
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Journal of Research in Science Teaching. "More on Guidelines for Research", December 1963; "Perception of Children", March 1964

Much biomedical research is funded through public monies, and agency research budgets are decided by the Congress. In a very real sense, therefore, the public is already involved in the determination of research priorities. However, the public has had a rather incomplete understanding of the nature of scientific research thus far. In order to have the continued support and confidence of the public at large the scientific community must help to broaden and increase public understanding of science. Failure to promote wider understanding will encourage the public to expect miracle "cures" -- when in fact science highlights the difficult nature of such cures, and the necessity for complex solutions to complex problems.

A key issue is the value of conflict. Conflict is so important in science that comprehensive rules, strictly adhered to, have been developed to handle disagreement and controversy. Scientific conflict is "resolved" by the addition of new information, most frequently information that would not have been obtained had the conflict not arisen and the differences been explored.

So it is with controversies which occur in the relationship between science and public policy. Starting with the sometimes bitter controversy over radioactive fallout -- "resolved" only when it became a matter for discussion involving the lay public as well as the scientific community -- issues have been clarified and subject to rational decisions once the topic was debated openly in public forums. Such public debate is the most effective means of resolving conflict.

On the other hand, acrimonious debate which disrupts the delicate fabric of interaction among scientists is to be deplored. Name calling and the politicization of an issue are not ways to achieve clarity on such complex issues as the ones we are exploring now and will increasingly be called upon to explore in the future.

This Commission should be composed of responsible members of not only the scientific community, but of the business, labor and public sectors as well. Such a Commission should be required to report its activities to the Congress and to the President annually. Only by taking such steps will public confidence in the scientific enterprise continue.

CONGRESSIONAL SEMINAR
on
RECOMBINANT DNA RESEARCH

Dirksen Senate Office Building
Washington, D.C.

Tuesday, December 14, 1976

SCIENTISTS' INSTITUTE FOR PUBLIC INFORMATION
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DNA experiments is relatively accessible: such experiments have been going on for several years in most major universities and in private industry.

The scientific proponents of recombinant DNA research, who have faith in the ultimate benefits of "pure research," project a wealth of possible technological advances, the benefits of which, they say, far outweigh the extremely low probability of any potential hazard. Praising the scientific community for its responsibility in voluntarily imposing, through the National Institutes of Health, stringent safety guidelines, they point to the possible manufacture of cheaper drugs using bacterial hosts as "factories" (insulin and antibiotics), possible agricultural benefits (the creation of nitrogen-fixing and high-protein crops), and a greater understanding of disease (the treatment of cancer and genetically-determined diseases). One possibility already being explored by General Electric is the creation of oil-eating bacteria to control oil spills.

Opponents of this research suggest that the workings of DNA are largely unknown to scientists, making such far-sighted proposals highly speculative, not at all immediately realizable, and research on them reckless, since scientists can't really predict what the results of their experiments are likely to be. This scientific ignorance, they say, coupled with the impossibility of any safety guidelines, no matter how stringent, being 100% effective in biology laboratories where mistakes are common, makes recombinant research unusually hazardous. Opponents claim that only one "accident" could unleash a disease pandemic.

Finally, opponents claim that the microbial world is in a delicate ecological balance of which little is known, the product of millions of years of evolution, and that recombinant DNA research may interfere with that balance.

Almost universally used in experiments is the human colon bacterium *E. coli.* This is highly controversial because, while well known to researchers and hence desirable, it is ubiquitous, found in all warm-blooded animals, in sea and air, in grass and vegetables. The strains used in high risk recombinant research, for this reason, are genetically weakened, following the NIH guidelines, to ensure they won't survive outside the laboratory. However, it is impossible to predict with certainty how these strains will behave after undergoing recombinant experimentation.

Thus, the fundamental issue is joined: What is to be the public's role with regard to basic scientific research programs? Obviously, the impact of the public is already felt in its traditional public policy role: the oversight role of technological funding priorities, i.e. guns vs. butter, medical research vs. space research, solar energy vs. nuclear energy. But in this case the public seems determined to have an even more active role.

The recombinant DNA debate in Cambridge propels the public dramatically into a new arena, where formerly only scientists walked. The new question becomes: Does the public have an obligation to determine the conduct of basic scientific research? Similarly, we must ask what the limits should be, if any, upon the public's right to know and to be informed of all relevant scientific knowledge.

With significant exceptions, the general public — in whose interest both sides claim to speak — has not been vocal. That public, for the most part, is ignorant of the fact that a debate is taking place — and of its grave import.

Public ignorance of scientific matters, the resentment of scientists who feel their freedom is in jeopardy, and even public indifference should not be used as arguments against full public access to balanced and accurate information about recombinant DNA programs. Important public policy decisions will be far better made when they fully reflect well-informed public participation.

The Scientists' Institute for Public Information is committed to keeping open the vital communication channel between scientists and the public. The case of recombinant DNA research signals with particular urgency the need for the public to enter the arena of debate.

No matter how this issue is ultimately resolved, one fact is absolutely clear: Well-informed public discussion must provide the marrow, sinew, and fiber — the animation of enlightened public policy. Without it, continued health of our social system is gravely imperiled.

March, 1977

Participants

OPENING REMARKS: Alan McGowan, President
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U.S. Representative, Maryland

MODERATOR: Judy Randal, Science Correspondent
Washington Bureau, New York Daily News

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Dr. Liebe Cavalieri, Professor of Biochemistry
Cornell University Graduate School of Medical Sciences
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about DNA recombinants, we are talking about gene transplants, and the recipients of those transplants are bacteria. To date most of the bacteria in question have been a species called *Escherichia* or *E. coli.* But there is no reason why other species of bacteria and other classes of plants and even animals can't be used as well.

The reason scientists haven chosen to experiment primarily with *E. coli* is simply that the species has been more thoroughly studied than other types of germs.

In any case, DNA recombinants are bacteria that in addition to possessing genes conferred on them by nature have been fitted with other genes selected by scientists. In principle at least, these could be genes from any plant or animal a scientist might choose—genes from viruses or even a synthetic gene compounded from materials in the laboratory. Once given these hereditary instructions, the germs and their descendants will presumably copy and translate their messages faithfully.

To be a little more precise about it, the foreign genes are not inserted directly into a bacteria. Instead viruses called phages or free-floating circlets of chromosomes called plasmids act as go-betweens. Thanks to chemicals known as restriction enzymes that behave much like scissors, scientists can snip open the DNA molecules of the phages or plasmids at predictable points and add new lengths of genetic material. These engineered viruses or plasmids then infect the target bacteria and cause what are, in effect, mutations in them.

It's very much as if you were to add a word or a phrase to a sentence, thus changing its meaning. As a startlingly simple example, consider inserting the single word "not" at an appropriate place in almost any sentence you can think of.

To abandon the grammatical analogy now and get back to cases about DNA recombinants, the remarkable part of this molecular engineering procedure is that the chemical incision made by the restriction enzymes seal almost immediately so that when the go-between viruses or plasmids find their way into bacteria, which is a quite natural thing for them to do, they change the nature of the bacteria and create an entirely new race of germs. A science writing colleague of mine has aptly referred to this as "instant evolution."

Obviously the discovery of restriction enzymes has given science an enormously powerful new tool. And many believe the consequent crossing of species barriers is as profound a development in biology as the splitting of the atom was in physics. Suppose, for example, you want to learn the molecular intricacies of how kidney cells are formed in the course of gestation so that they become capable of concentrating urine. If the only way you can study this is by studying materials from complex animals, the puzzle will be tremendously difficult to unscramble. But if instead you can give bacteria a short, well-defined sequence or sequences of genetic material from an animal and then follow how the bacteria express those sequences chemically, the task will be much simplified.

Said another way, it's the difference between looking at an omelette and trying to figure out exactly what has gone into it or doing the analysis before the eggs are broken, other ingredients are added or heat has been applied.

Obviously DNA recombinant technology has enormous commercial potential. There is every indication, for example, that bacteria given instructions, specified by man, can be programmed to become factories, as it were, for the inexpensive production of valuable chemicals and drugs. It is even possible, although it will be far more difficult, that food plants will be freed from their present dependence on nitrogen fertilizer by endowing them with the ability legumes already have to capture bacteria that fix nitrogen from the air. And these are only a few of the many possibilities. All this, however, is only one side of the picture. The other is that in crossing the species barrier man is bringing about a second genesis which could have the effect of opening a Pandora's box: A strain of germs, for instance, that no drugs could touch and which might threaten the public health should it escape into the environment.

In July 1973, in fact, biologists attending a conference in New Hampshire became so concerned about the possible hazards of gene-juggled bacteria that the two leaders of the meeting wrote an open letter to the scientific community warning that laboratory workers and the public might be endangered and urging that the matter not be swept under the rug. One of the authors of that letter was Dr. Maxine Singer of the National Cancer Institute, who is with us today. Then, some months later, 11 members of a National Academy of Sciences committee wrote a second letter, published in July, 1974, by *Science* and *Nature* magazines, calling for a moratorium on further such research, pending the establishment of guidelines by the National Institutes of Health that would permit the work to safely resume.

question period. The white cards in your folders are for questions and will be collected by the ushers as we go along so that you may ask questions of the panelists.

Dr. Singer is head of the Nucleic Acid Enzymology Section of the Laboratory of Biochemistry at the National Cancer Institute, but has asked us to announce that the views she will express are not the official views of the Institute but strictly her own. She will speak first. Dr. Singer.

DR. MAXINE SINGER: Thank you, Judy, for a really marvelous summary of the science and a lot of the history.

Three and a half years ago, as Judy explained, a colleague and I wrote a letter to the president of the National Academy of Sciences. We were not writing for ourselves alone but at the direction of 140 scientists, leaders in the field of nucleic acids and genetics. We had all been together at a scientific meeting, and we had heard some fascinating new experiments described, experiments which made it feasible to isolate fragments of DNA—that is, genes—from any living thing and to join these fragments together with DNA from a totally unrelated species and to insert this new DNA into single cells growing under laboratory conditions in order to study the properties of the genes.

Excited as we all were about the versatility and opportunities provided by the new techniques and although no hazards were actually known to exist, we voted to inform the Academy that cells or viruses carrying the recombined DNA might, in some instances, prove hazardous to man or to other components of the biosphere.

We also voted to publicize these concerns by submitting the letter for publication in *Science* magazine.

The Academy responded by establishing a committee of distinguished experts, some of whom were doing recombinant DNA research in their own laboratories. This committee took an unprecedented action. In July of 1974, they published a letter asking colleagues all over the world to join them in deferring certain recombinant experiments while a more thorough analysis of the potential for hazard could be made. They also requested various specific activities directed towards such an assessment. And again they made certain that their action was widely publicized in the popular and scientific press.

These precedents, that some experiments ought not be done, that the deliberations needed to be international and widely publicized, have been central to all considerations of the recombinant DNA issue since the summer of 1974. At the international conference at Asilomar in February of 1975, experts from relevant scientific fields and lawyers concerned with the impact of science on society made the first attempt at rigorous definition of the issues. And, as a result, by mid-1975, activities directed to providing assurance that potentially dangerous organisms would not inadvertently be released were proceeding in every country in the world where scientific capability might permit such experiments.

All of this occurred, and still proceeds, in the absence of any demonstration that hazardous organisms can indeed result from these experiments.

In the United States, the National Institutes of Health assumed responsibility for the problem. After extensive scientific consultation, and after opportunity for public comment — all of which took place in public — the NIH published guidelines for the conduct of research in June of 1976. A draft environmental impact statement was prepared and circulated. The comments on the statement are presently being considered and will help in the ongoing re-evaluation of the provisions of the guidelines.

Concurrent with all this activity, certain types of recombinant DNA experiments, not covered by the deferral or by the Asilomar recommendations, proceeded. The results that have been obtained confirm the initial enthusiasm for the method. By now most knowledgeable scientists and laymen recognize that this technology can be applied to many different problems in biology and medicine. The sweeping charge by some that the anticipated benefits of this research are dubious and speculative is misleading and simplistic. It ignores the human urge to understand both our own nature and the world that surrounds us, and it denies the need to acquire fresh insights if we are to ameliorate the individual and societal tragedies caused by disease and by hunger.

The voluntary deferral that started in the summer of 1974—it has been called a moratorium—did not, as some believe, call for a ban on all recombinant DNA research. Only two types of experiments were included: First, the construction of drug-resistant or toxigenic microorganisms that do not occur naturally and, second, the introduction into bacterial cells of all or part of the genomes or viruses known to cause cancer in animals. There are no viruses known to cause cancer in humans.

ful and safer bacteria exist. Predictions about the existence of rare and fastidious organisms, unable to exchange DNA with bacteria inhabiting man or other living things, are highly speculative.

One important problem demanding attention at present is the need to assure that all recombinant DNA research in the United States is carried out in a safe manner. The NIH developed its guidelines to govern the work of its grantees, contractors, and staff. The guidelines have since been adopted by the National Science Foundation, by the Energy Research and Development Administration, and by the Department of Defense. There are indications that the Department of Agriculture will soon join in. We may anticipate that all work conducted under the auspices of the United States government will be done according to the NIH guidelines.

At present no mechanisms, except voluntary ones, exist for extending the provisions of the guidelines to work supported by private funds either for research or commercial purposes. But an active search for appropriate mechanisms is under way.

A federal interagency committee, chaired by the director of NIH and formed at the request of President Ford is at work. Both research and regulatory agencies are involved. Their job is to determine whether existing powers within the agencies are sufficient to extend control to the private sector, to formulate recommendations as to how this may best be done, and to recommend legislation should that be deemed necessary.

In the meanwhile, several industrial organizations under the sponsorship of the Pharmaceutical Manufacturers Association have joined in a study of the NIH guidelines and their suitability to the special problems of industrial development.

There are several ways for us to deal with the problems engendered by scientific discovery. Historically, society has waited until some dreadful event occurred and then tried to stop repeated disaster. Manifold conflicting interests interfere with the prompt cessation of the hazardous activity.

One alternative is to try to think ahead and stop anything that might conceivably be hazardous before it gets started. That would result in stagnation. Not only that, it is not necessarily the safest course since it offers no hope for solutions to existing threats. Technical and cultural innovations will always be seen as fraught with danger by some component of society. Even the acquisition of knowledge is seen by some as dangerous, and it is risky. The outcomes are, by definition, not known in advance and applications of the resulting knowledge may indeed be undesirable.

The only sensible approach is to apply what knowledge we have, to debate openly so as to assure that many ideas, views and assessments will be available to inform us, and to proceed with prudence and caution. It is my belief that the history of the deliberations on recombinant DNA is by and large a history of good sense, of open and forthright debate. The guidelines are instructions for proceeding with prudence and caution. What we need now is continuing evaluation of the provisions of the guidelines by scientists and by the public and timely revisions responsive to the re-evaluations. We need to work at assuring diligent compliance with the guidelines. Most urgently, we need to find viable and effective mechanisms for extending the requirements to work not supported by the federal government.

Thus far I've talked about the immediate problem, the safety of presently feasible experiments. And I've carefully used the term "recombinant DNA." I reserve the term "genetic engineering" for the deliberate modification of the genetic constitution of higher organisms, especially of man, because most people have that in mind when genetic engineering is mentioned.

It may well be that the techniques of recombinant DNA and the understandings generated by the experiments will lead to a capability for genetic engineering. It is not too soon to begin a rational debate on the issues raised by genetic engineering. We need to prepare ourselves for the individual and societal decisions that we will need to make. It will be difficult, at best, and we will increase the difficulties and reduce the likelihood of wise decisions if we do not immediately and carefully distinguish recombinant DNA from genetic engineering, distinguish the acquisition of knowledge from the application of knowledge, and distinguish careful analysis of existing knowledge from vague uneasiness and distortion of fact. A concerned public, including knowledgeable scientists, together with federal and local governments, can debate rational policies — policies that offer both protection and opportunity, that encourage discovery and development of safe and desirable applications. The debate will prepare all of us for responsible consideration of the difficult problems to come.

MODERATOR RANDAL: Thank you, Maxine.

It is but a modest extrapolation to say that recombinant DNA technology makes available to us the gene pool of the planet, all the genes developed in the varied evolutionary lines throughout the history of life, to reorder and reassemble as we see fit. We have all seen drawings of the evolutionary tree, tracing the development of each of the extant living species—each of them, as are we, the product of billions of years of evolution. That tree is a representation of the fact that evolution proceeds in a linear manner, by small increments, to produce gradually diverging species. Nature has, by often complex means, carefully prevented genetic interaction between species. Genes, old and new, can only reassert within a species.

We can now transform that evolutionary tree into a network. We can merge genes of the most diverse origin, from plant or insect, from fungus or man, as we wish. The slow, almost measured pace of evolution permits the establishment at any time of quasi-equilibria among the various competing species. This balance is never a static one. It's dynamic. Some species continue to find a suitable ecological niche, others die off.

You all know that most species that have lived have perished and have been replaced. For example, the giant reptiles dominated the earth for 150 million years and then perished.

Now we come with our science and our ingenuity and we have now the power to introduce quantum jumps into this evolutionary process, with unpredictable consequences to the currently established equilibria on which quite literally our life support systems depend. As organisms evolve, they find an ecological niche which favors and permits their survival. They are where they are and what they are because of that evolution.

Man likes to think he is the exception, that he has made his own ecological niche. In part that's true. We build buildings and we wear clothes and so on. But, in large part, I would suggest that it is, as yet, a conceit. We literally rely on our fellow creatures. We obviously rely on the plant world for our food and our oxygen, and on the microbial world to degrade our wastes, to restore the planetary nitrogen, and so on.

Our resistance to disease, our susceptibility to disease, the severity of the symptoms caused by disease are all reflections of our evolutionary adaptation into an available niche.

For an instance, there are in the United States some 25 deaths a year from botulism poisoning. It's obviously fortunate that botulism is not a contagious disease. Of course, this is not just due to good fortune. If botulism were a contagious disease, the human species could simply not be what it now is. Our ancestors would have had to find another niche.

The NIH guidelines were conceived to cope with the perceived immediate medical hazards of recombinant DNA research. As such, I believe their authors did a commendable job. They rank ordered the hazards they envisioned and then, in a pattern of graded risk, imposed a graded set of containment provisions commensurate with the estimated risk. But it's clear that the authors of the guidelines did not consider the transfer of genes across species, the introduction of quantum jumps in the evolutionary process, to be of any hazard unless one could specifically pinpoint a gene of known toxicity.

Thus, any DNA fragment from any invertebrate can be inserted into the *E. coli* organism under the P-2 conditions that were described by Miss Randal and into the ordinary K-12 *coli*. Any DNA fragment from any embryonic form of a cold-blooded vertebrate can be inserted into the *coli* organism under the same conditions. Any DNA from any source that has been previously cloned and is not known to code for a toxic agent can subsequently be grown in the *coli* organism under the same mild conditions.

Consider for a minute what's implied here. The DNA from an insect or an echinoderm can be cut with a restriction enzyme into some twenty or thirty or fifty thousand fragments. Each fragment contains some generally unknown cluster of genes. With another restriction enzyme, one can produce a different set of twenty or thirty or fifty thousand fragments. Any or all of these fragments can be inserted into *coli* and grown up into a clone. Somehow it is presumed that we know a priori that not one of those clones will be harmful to man, or to our animals, or to our crops, or to other microbes on which we unthinkingly rely. I don't know that and what bothers me is I don't know how anyone else does.

Even more, this echinoderm DNA, for instance, may be prepared from organisms collected from nature, that live, perhaps, on a coastal shelf. Such organisms are surely not sterile preparations. They have their own, usually unknown, coterie of associated microbes and parasites, which can include those deposited on the coastal shelf by our waste disposal systems as well as more indigenous forms.

And, thirdly—the point to which you refer—at least some of the benefits which are proposed might be obtained by alternative technologies, not all of them at this time. That is, there are certain kinds of experiments, of which I cannot conceive at the moment, for obtaining the same information.

But, for instance, in terms of benefits of producing compounds in microorganisms (such as hormones) using them as factories, alternatives could exist in the way of straightforward chemical synthesis or even synthesis with subcellular systems, such as ribosomal systems which would not involve the hazard of incorporating the genes into free-living organisms.

MODERATOR RANDAL: Thank you. As a followup on that, I have wondered, for example, about one of the scenarios that is very often discussed in this whole area, the possibility of endowing food crops with the capability to work with nitrogen-fixing bacteria from the air. The other day I was doing some reading in agriculture, and I discovered that there are well over a thousand nitrogen-fixing plants, ranging from small food crops to large forest trees, that already exist in nature but that nobody has bothered to invest in, in an intensive way, to make them commercial and practical. I am wondering if there are a lot of scenarios like that around so that if one goes forward with the DNA recombinant thing in this area, one might also want to look at these alternatives.

DR. SINSHEIMER: It is primarily an economic kind of question as much as any, and one would want to work through it. There are a variety of scenarios that have been proposed. Still others are to develop existing strains of organisms which can fix nitrogen into sort of super-producers of nitrogen to the degree that each farm might have its own fermentation system for producing fertilizer rather than putting the capacity for N_2-fixation into plants. There are, in other words, a variety of ways one might think of going about the problem; which would be the most practical, either in technological or economic terms, probably couldn't be defined at the present time. But certainly one could pursue a variety of approaches.

MR. GUDE: I have a question.

MODERATOR RANDAL: Yes, Congressman Gude.

MR. GUDE: Dr. Singer, in regard to this strain of *E. coli,* which the guidelines provide should be used or could be used in experimentation, is it known, as far as the genetic structure of this strain is concerned, specifically where the ability of this organism is to maintain itself on its own or to become hardy and able to live in a hostile environment? Is that well known enough so that in experimenting with its genetic structure you couldn't possibly endow it with powers to survive under more hostile circumstances without realizing it? I mean, is that specifically known, where the hardiness lies in the organism?

DR. SINGER: I might say that the strain itself is not a hardy strain.

MR. GUDE: I am saying, can you not endow it with hardiness unknowingly?

DR. SINGER: Clearly, one of the things that one is worried about in this whole situation is that you would change the properties of that cell, and that is one of the things that you might do to it. That problem is very specifically recognized in the guidelines by the requirement that, when you have inserted a foreign piece of DNA, you must continually check the properties of the cells to be sure that they have not changed in such a manner. You must continuously check that those properties, which assured you to begin with that it wasn't likely to be viable outside of the laboratory, remain with it. That is a possibility. I think it's a remote one, but a real one. And I think the guidelines are responsive to that.

There is no other organism in the world that we know as much about as we know about this particular single cell. And it can be manipulated almost at will in terms of its properties, even without putting in foreign DNA. So one is comfortable with the fact that you can follow its properties and measure the kinds of parameters that you need to measure for this purpose.

MODERATOR RANDAL: Maxine, one of the things that I think troubles a lot of people is that, whereas one expects that a scientist working in a laboratory will be very cautious and so on, restriction enzymes are not terribly expensive, and it's quite possible that a relative amateur could avail himself or herself of this technology and simply pour the contents down the sink, or whatever. To what extent do you think that anything could be done to control that potential hazard?

DR. SINGER: It's true that restriction enzymes are inexpensive to buy, although most of the ones that you buy are not very good. It's also relatively simple to make them. People have also said that it is relatively simple to do a recombination experiment. That's really misleading in many ways. If you have

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I share the questioner's concern that we don't have guidelines governing work by private industry and by private funds, and I would hope that we are on the way to having that. I certainly am not going to defend the fact that we don't have it. It's not clear to me, and it's, in fact, interesting to me that, in spite of all the discussion, we haven't moved any further than we have and that only the NIH, within the whole government structure, undertook responsibility to develop anything.

MODERATOR RANDAL: Yes, Dr. Pollack.

DR. POLLACK: Just apropos the question of enforcement versus voluntary compliance, it seems to me that this meeting is valuable because, to the extent that enforcement will make any sense, it will have to make sense on as large as possible a governmental scale, hopefully international, but certainly national.

I work in the state of New York, and it's obvious that my colleagues cross state lines. Some are in Connecticut, some are in New Jersey, some are in drug companies, some are in state institutions, some are in private, and some are in governmental institutions of the good sort, I suppose. On the question of enforcement, I think that it's an ordered set which we have to consider, first information and then enforcement. If we consider it the other way around, we have a serious problem of a different sort, a political sort.

MODERATOR RANDAL: One of the things I've wondered about, for example, is a few weeks ago when Dr. Boyer and his colleagues announced some results with DNA recombinants, his institution at the same time applied for a patent on that technique. The guidelines apply to an academic institution and the club that is held over the institution is its funding, of course. And I had wondered if sufficient money were generated through the patent, if then academic institutions might be free to ignore the guidelines if they wished to do so. I'm not saying that it might, but it's something I've wondered about. Does anybody have any comments on that?

DR. POLLACK: It seems to me like a patent which some physicists obtained secretly in the late forties for work on the atomic bomb. The more important question is: What does the government do with the developed technology? That patent is worth no money, although it's a great honor, because the work is obviously restricted by government law. I think the same parameters of regulation apply here, but patents are really a separate and not scientific question at all.

DR. SINSHEIMER: In that regard, Judy, I don't know about some universities, but it really doesn't need that large a resource to undertake this kind of work. I would say a laboratory with—Maxine suggested several hundred thousand dollars; I'd be willing to do it with a hundred thousand—could set up to do this kind of work. And I could well envision small entrepreneurs doing this more or less on their own, and such enterprises obviously would not be bound by the existing guidelines.

DR. SINGER: I think it's probably quite clear that all of us really agree that we need ways to control this research wherever it is carried out. I don't think that is a matter that we disagree on at all. I tried to indicate what is going on in an attempt to find the proper way to provide controls on work that is funded by private money.

If you don't mind though, I would like now to take a bit of an opportunity to respond to some of the things that Dr. Sinsheimer said.

MODERATOR RANDAL: Please do, and then we'll get to our next two speakers.

DR. SINGER: I think it's important to recognize that implicit in what Dr. Sinsheimer said is the acceptance of the notion that experiments ought to proceed. Really what we're talking about is what the guidelines ought to look like. He mentioned that he thought that all experiments ought to proceed in maximum security conditions. That would mean essentially upgrading recommendations in the guidelines. But I think it's also clear that Dr. Sinsheimer, as well as many others who are critical of the guidelines, make distinctions between experiments. And there are some experiments which we all agree are not hazardous. So, it's important not to make sweeping statements about all the experiments. Experiments which involve DNA from organisms that are known to exchange genetic information in nature are widely agreed not to present any specially unique hazard when done in the laboratory.

However, it is true that Dr. Sinsheimer makes a unique argument when he is concerned about evolutionary problems, and he has been the chief spokesman for that particular concern within the scientific community and in public as well. But I think, Bob, that I'd like you to clarify a few things with that argument. You talk about the evolution of complex organisms. And, while you didn't specifically say

I hold a Ph.D. in biology, and obviously I'm not a clinician. I'm a member of the Human Cell Study Section of the National Science Foundation, and an associate editor of the *Journal of Virology* and of the *Journal of Cell Biology*. I received a bachelor's degree in physics. I left physics in the fifties because of my sense that there was an air of freer inquiry in biology than physics, and I'm not really prepared to give up that sense of freer inquiry for the sake of any regulation beyond what I think is sensible.

I've done no work on recombiant DNA at all, nor do I plan to do any, nor do I plan to have any done in my laboratory. I hold no vested interests or patents in this work. My interest in it arose at a very early point in the development of this technology. In 1972, while I was a staff member of Cold Spring Harbor Laboratory, I was asked by Paul Berg, David Baltimore and James Watson to help organize a meeting, which Maxine Singer alluded to, in Asilomar, to discuss possible hazards of some new techniques for the study of tumor viruses, including restriction enzyme ligation and plasmid amplification of DNA sequences. The proceedings of this meeting were published by Cold Spring Harbor as the book *Biohazards in Biological Research*, which I helped edit.

Now, although I was not involved in this research, they asked me to organize this meeting as a response to my personal apprehension in the summer of 1971 that unrestricted research on recombinant DNA of tumor viruses might be dangerous. And rather than argue we all agreed that it was necessary to have a meeting to get these fears out in the open. I believe that was the first meeting of a sort, of which this is the latest one.

The moratorium on certain aspects of this research, the NIH guidelines, and the current consideration of the degree to which they should have the force of law all followed in time. I thiuk it should be clear to you all that there is a basis of a conflict of interest in all of these hearings. I'm here now more as a member of the audience than as a purveyor of this technology. (Those of us who can read the research papers must be suspected of a conflict of interest: Otherwise, why would we have taken the time to learn that tedious terminology?) Nevertheless, you'll have to have a minimal amount of trust that we can differ honestly on this question and that we are not merely arguing from our pocketbooks. At least in my case I'm not.

Now, my first opinion. Recombinant DNA research is worth doing. I think that has to be said first, and one has to ask that of everybody on such a panel. Our current lack of knowledge about fundamental life processes is indisputable, and this ignorance centers about our inability to study the way in which genes are activated and inactivated as part of the normal processes of embryonic development and normal differentiation. Gene amplification has no substitute as a probe for understanding these processes. And here it differs from the use of this technology for the production of hormones, for instance, which, I agree with Dr. Sinsheimer, is a substitutable technology.

But there is an underlying reason why gene amplification has no substitute as a probe for understanding differentiation. That is that the vast majority of the DNA of any higher organism is silent and unexpressed and therefore unavailable for classic experimental genetic manipulation.

To understand the regulation of gene expression, we require a knowledge not only of the DNA of the genes themselves, which we might obtain by alternate technologies, but also of the DNA between the genes. These sequences exist. They carry information. They are sequences coding not for products which we can assay biochemically, but for addresses, information needed for regulation. We cannot study this regulatory DNA by classic genetic techniques. We can only clone it out directly through recombinant DNA plasmid amplification.

My second opinion: We will remain ignorant of the mechanism of action of certain diseases so long as we remain ignorant of the mechanism of regulation of gene expression. To give two examples of our current ignorance, consider; we have to rely upon injection of vaccines into a person in order to stimulate the immune response against a disease-causing agent. We do this because we have absolutely no idea how to directly stimulate the gene or genes coding for the immunoglobulin molecules that could directly interact with the offending agent and eliminate it.

We are ignorant of gene control processes in higher organisms in general. For instance, a tumor and a normal tissue shared their common origin from a single fertilized egg cell. So, it must be a failure in normal regulation of gene expression, no matter what the initial cause, viral or chemical, which yields the tumor. This approach to cancer research critically depends on being able to analyze the regulation of gene expression in mammalian cells. Indeed, I cannot think of a biomedical problem for which information would not be forthcoming from the technology of gene amplification.

DNA. I feel that this group will be interested in the broad issues of recombinant DNA technology. Therefore, my statement will not concern itself with the immediate details of the NIH guidelines.

The issue of recombinant DNA and its inevitable consequences for genetic engineering can be brought into proper perspective if we consider that research on recombinant DNA molecules has brought about a scientific revolution which will eventually have profound societal effects when the results of the research become technological realities. We have had a number of scientific revolutions in the past. An example is the work of Fermi, who showed in 1933 that nuclear fission was possible. We know only too well the consequences of this knowledge. The laws of heredity which were forumulated by Mendel in the last century revolutionized biological research in this century. In the last two decades Mendelian genetics have been formulated in molecular terms. This formulation provides the basis for recombinant DNA technology. Recombinant DNA represents the infancy of a revolution. It provides us with the tools for future genetic manipulation. I would emphasize that there is hardly any aspect of biology which is more fundamental than this.

The power in our hands now is unquestionably awesome. I will not deal with the immediate potential hazards, but they do exist as you've heard stated here several times. Uncertainties exist—fundamental uncertainties such as what happens to the enfeebled *E. coli* bacteria now in use when a new plasmid is introduced into it. Will it survive? Will it become less enfeebled and therefore potentially more dangerous?

Recently Dr. Stanley Falkow, Professor of Microbiology at the University of Washington School of Medicine, an expert in medical microbiology and a major contributor to the NIH guidelines, expressed the fear that someday a recombinant *E. coli* might be made in the laboratory, perhaps inadvertently, which could multiply in our drinking water. Stop and think about the implications of that for a moment. *E. coli* is a human pathogen. It can cause and spread disease. At present *E. coli* can live in water but it cannot multiply there. Imagine giving it the ability to multiply in the water supply. This would amplify enormously its disease potential. New organisms such as this can and very likely will be made.

I think it's important to recognize that recombinant DNA techniques are not likely to remain in the laboratory for long, but are already on the way to becoming one of our many technologies. What is the basic character of any technology? First, it is always proposed as an "advance" in the short run But we know all too well what happens in the long run. Witness insecticides, artifical food colors, hormones fed to cattle to fatten them, and so forth.

By the time the ill effects of a technology become apparent to all, the technology is usually so entrenched in our economic life that it cannot be reversed, and it becomes necessary to create additional technologies to alleviate the damages of previous ones. For example, we are now trying desperately to cure cancer, which in the main is caused by man-made chemicals introduced into the environment, as the NCI has shown. That is, instead of preventing cancer by eliminating the carcinogens, we are trying to find a way of co-existing with them to keep our disease and still survive. If the problem weren't so serious, we could laugh at these activities which have the aspect of a circus.

Technology tends to have its own momentum, to become increasingly divorced from human needs, as it is in many cases where recognized deleterious products continue to be manufactured for economic reasons. Thus, technologies represent ever-widening and endless circles of human endeavor, leading eventually and inevitably to a complete separation of man from his natural environment. As Rene Dubos has pointed out so well, we will one day soon be able to stay locked up in our houses and by interactive television bring the outside world inside, including ersatz food, exotic odors and scenery, et cetera.

In the case of recombinant DNA, the already visible connection between the present research and future technology means that the scientist can no longer really assert that he is responsible for his science only, that he is not responsible for what happens after his results are in the public domain. On the contrary, he must recognize that we live in a technological society where decisions are made many times on the basis of sheer momentum with very little thought or planning.

It has been argued that DNA technology will yield social benefits. The only immediate benefit is the advancement of knowledge concerning how genes function. Is this knowledge useful? The simplistic answer is yes. But it is not at all clear to me that this answer is yes in the broad context of the other implications of the research. For the moment, I suggest that we learn more about gene function in a

Singer would call a sweeping statement. It would sweep out more than I think most people would like to see swept out. The second is that as I understand the nature of research, I am reminded of a statement I believe of Winston Churchill's with regard to democracy, that it is the worst form of government except for all other forms.

Basic research—that is to say, undirected research—that is to say, free inquiry— is the worst possible way to protect yourself from the dangers of the unknown aspects of nature, but there is no other way that I know of to get answers about how nature works. And fundamentally, this is an existential problem. There is no way you can do this in a risk-free way. The question is whether you will share the risk with society at large or whether you will be asked to be given a privileged position and risk the population at large without accountability. But I think it's a will-o'-the-wisp to say that there is an alternate risk-free way to get any answers out of nature.

DR. CAVALIERI: Can I respond to that?

DR. POLLACK: Go ahead.

DR. CAVALIERI: I'd like to sharpen that up a little bit. Most scientists are also puzzle solvers. That's what we do all the time really. And most of our work is neutral. Problem solving is quite another thing. Specifically, if you set about to construct a gene to study this or that, you know what you are doing. You know, for example, that you are going to cross a genetic barrier. You know all of the questions. It's that you have set about to solve. And it's here where I think we should use some intelligence in making a decision. I say that we don't have to curtail freedom in scientific inquiry. Most of us, as I said, are puzzle solvers in the first place, and we can find out all we want about nature. And so I am not making the sweeping statement that we will all be out of business and that our intellects will disappear. But when it comes to solving problems, then I think we had better be very smart about it and decide whether we want to attempt to solve the problem.

You can bring up all the benefits you want, but I think the overriding concern is that, as Bob Sinsheimer has said, when you run the risk of messing up future evolution, you've got the biggest problem you can think of. And to be able to make insulin, for example, pales into insignificance.

MODERATOR RANDAL: We have a large backlog of questions from the audience. So, I am going to concentrate on those for a little while. This questioner says that he isn't necessarily interested in specifics but is interested in the philosophical aspects of the following. He or she says: "As you may know, a uniform patent policy may be introdueed in the 95th Congress to deal with patents coming from federally funded research and development. There may be studies of the Freedom of Information Act in terms of promoting international R&D involving government and industry. Therefore, what is the level of foreign work in recombinant DNA? What is the level of multinational work? And is the US involved through government or industry? Furthermore, assuming that US industry becomes involved in multinational DNA work, how could the Freedom of Information Act, et cetera, be applied? In other words, should industry be required to report its level of work to the governmeut or an international agency?" And this is for anyone on the panel who wishes to tackle it.

Maxine.

DR. SINGER: I'll take the last part first. One of things that is certainly being discussed in the interagency committee that is meeting in order to find ways to govern research funded by private money, and development as well, is the question of a registry that would include work that goes on both in research laboratories and in industry itself. And, from sitting in on some of those meetings, I think it's very clear that the question of the registry is one that most people agree on. So, I think that it won't be long before we do have a good recording of everything that is going on, at least in this country.

Now, with regard to activities abroad, there are various laboratories all over the world that are involved in recombinant DNA research, and there are rules of various sorts that are either in place or are being developed to govern that work. Because of the structures of specific national governments, many of those rules will immediately be applicable, not only to research situations but to industrial situations as well. For example, the rules that were promulgated this summer or early this fall in Great Britain govern work of any kind that proceeds in Great Britain. The same will probably be true, for example, in West Germany. In other countries there are still discussions going on, and it's less clear as to the precise mechanism that will be used.

In addition, there are several international organizations that have specifically concerned themselves with the problem of recombinant DNA research, with the problem of training, and with the problem of

MODERATOR RANDAL: Our next questioner wants to know if recombinant DNA has successfully made protein in the cells of another species. Who wants to tackle that?

DR. SINSHEIMER: Apparently is the only answer you can give. That is, functionally it does appear that functionally effective protein has been made in the yeast bacterial recombinants.

DR. SINGER: It appears that a trait which was missing in the bacteria has been supplied by the insertion of yeast DNA. The assumption is that the protein is being made. But in spite of the fact that the particular protein that needed to be made is well known and in spite of over a year's work in a very good laboratory where people are technically very competent, there is, to my knowledge, no evidence that in fact the protein has been made. There are other ways that one might explain the success of that experiment. So, I think, that's really a question that's still up in the air.

MODERATOR RANDAL: Are both of you alluding to the work at California—

DR. SINGER: Yes.

MODERATOR RANDAL: —because when I wrote this story several weeks ago, I asked this question specifically and was told yes, indeed, this had transpired. But I don't know what "yes" meant.

Herc's another question. "There was no real public participation in the decisions going into the formulation of the guidelines. To what extent did the panel members feel that the 'informed consent' of the public is desirable or necessary in formulating federal policy on recombinant DNA research, or should such decisions remain in the domain of scientists?"

DR. SINGER: I'd like to answer that because I think it's very clear from the history of the last three and a half years that there was never the implication that this was a problem that ought to be dealt with by scientists alone. It was clear in June of 1973 that the scientists involved in this recognized that this was a matter which had to be put before the public. It was recognized that it had to be put before the public in 1974. Every meeting that has ever been held has been open to the public. Every meeting that has ever been held has been covered by the press, much to the astonishment of many of us.

I don't think it's accurate to say that the gnidelines were devised without pnblic input. First of all, through the entire development of the guidelines those who were involved in it were receptive to comment by anybody. Anyone who ever wrote a letter had that letter considered. Everyone who ever made a phone call had the phone call considered.

Now, it may be that some people who had things to contribute didn't realize that they could do that. But why they would not have realized that is not at all clear. Finally after the draft of the guidelines had been submitted by the advisory committee to the director of NIH, the director held a public meeting to which anyone could come and make a statement or give a written statement. Those statements were considered at great length by the director, and certain of the comments were presented to the advisory committee with the notion of perhaps revising the guidelines in response to them.

The question of how much public participation is enough depends on who you are. I don't think that all of us will ever be satisfied that there was enough opportunity on any particular issue—and some of us will be right. But I think there was an enormous effort to collect public opinion. There was an enormous effort to give that opinion a forum in which to be heard. And there was a very serious intent on the part of the NIH to listen carefully to that opinion.

As I mentioned before, one of the really funny things that has gone on in this whole story is why there hasn't been action in other forums. Why is it, for example, that this number of years later we're all sitting here and talking? Why weren't we talking a year and a half or two years ago? Why is it that the Congress did not pick up on this issue earlier and look at it? I don't know the answer to that, but I think it's worth thinking about. And I might say at this point, because it would fit in really, that I think that Dr. Cavalieri's suggestion about a commission is a very useful and a very good one. And I think that it could serve to inform the public and the Congress in very important and useful ways about the very difficult problems that will arise when some of these technologies become useful for specific application.

MODERATOR RANDAL: Thank you.

DR. SINSHEIMER: Could I comment on that because I was a partial participant in some of the steps, particularly the ad hoc committee that was convened to review the guidelines once they had been drawn up.

that: 'We'll register, but, to protect industry's secrets, we don't want to be required to disclose to you the nature of our work' —and that is in fact what happened. To your knowledge, was that article in the *Post* correct? What is your view on the government having knowledge of the nature of the work, not just the parties involved?"

DR. SINGER: The story in the *Post* was certainly accurate, and this is a problem which needs to be worked on. Clearly just saying that you're doing research is not what any of us think we need to have in the registry. Nevertheless, as we're all aware, industrial concerns have certain notions about what they can and cannot reveal about the specific work that they're doing. I think it's a very knotty problem. It is currently being discussed. I think that those people in the interagency committee recognize that simply saying you're doing work is not going to be sufficient, and they are attempting to devise ways so that an appropriate and a useful register can be constructed that will be tolerable to industry and in some way protect their interests as they see it, and yet give the public the information that it may need to have.

I think there are ways to do this. They may be more or less satisfactory. But it is a problem which is being actively dealt with and one hopes that it will come out in a reasonable way.

MODERATOR RANDAL: Dr. Pollack, you wanted to add something further. I am going to ask all of you to be brief so we can get to some of the rest of these questions.

DR. POLLACK: If we are writing the legislation now, let me say that the previous time I sat on a panel like this I was preceded by the representative of the New York State Pharmaceutical Manufacturers Association, who said that his organization accepted the guidelines without quibble, except for one small reservation. That is, they would not accept any restriction on the volumes of material grown.

I hope I made myself clear. But if I didn't, let me just reiterate in one sentence. One breakthrough of this technology is that bacteria double every 20 minutes whereas higher organisms take much longer to grow. So it's intrinsically cheaper to get a large amount of DNA if you can grow it in a bacterium than in anything else. By this argument, if one were to make a profit-making chemical through recombinant technology, one would want large volumes of that bacteria. And, as I said before, it seems to me the underlying assumptions of the guidelines are that volume is your one measure of risk. So, I would say it is essential that any deliberations on possible enforcement of these guidelines insist on across-the-board enforcement of the volume restriction which, as I remember it, is ten litres for any microorganism under study.

MODERATOR RANDAL: That's not going to be very acceptable to industry anyway. Here they have already eliminated that.

DR. POLLACK: I understand that. But in answer to Maxine, just an extension of the idea of merely giving a list of names is not a sufficient registration. I would say it is insufficient to not list the volumes.

DR. CAVALIERI: Can I ask Maxine a question?

MODERATOR RANDAL: Certainly.

DR. CAVALIERI: Does this registry include day-to-day things like spills, accidents, and all that?

DR. SINGER: The registry and the nature that the registry will take in regard to profit-making organizations has not been established.

DR. CAVALIERI: No, no, I mean anybody.

DR. SINGER: The registry that NIH is forming is a registry that will describe the type of experiment, tell where it's being carried out, and indicate the assessment of risk and whether the proper facilities are there and so forth. The question of reporting accidents and spills, whether that can be plugged into the same computer or a different computer, I don't know. But there are plans also being made for collecting that information, because, as you know, the guidelines require the reporting of information of that type.

MODERATOR RANDAL: Here's another brief comment, and then we'll go on to a question. This is in the department of clarification. The commenter says that "The commission Mondale proposed some years ago now exists as part of Public Law 93-348, which is the Commission for the Protection of Human Subjects of Biomedical and Behavioral Research. However, it's Mondale's proposal"—I think it is—"in the broad sense, not only with relevance to DNA."

from the decisions of propagation for biomedical research in general. And the idea of some separate commission beyond the NIH to determine regulatory restrictions is a good one.

I think, however, that the strategy of bringing everything to P-4 is a poisoned pawn. I would say it's a dangerous rather than a safe move in the sense that in my experience, while advising on or observing the construction of restrictive facilities at Harvard, Cold Spring Harbor, Albert Einstein Medical School, Stony Brook, and in hearing about ones at other places, including Cal Tech—Dr. Sinsheimer's department—is that a P-4 facility costs on the order of hundreds of thousands of dollars to build new, and probably costs less to build new than to put into a pre-existing building because the airflow requirements are so stringent. That is, ventilation is in the order of hundreds of thousands of dollars by itself.

I think, therefore, that to say everything should be done in P-4 is essentially to eliminate the possibility that a young person can do this kind of work and to oblige this work to be done in established laboratories which have a lot of money. To my mind, that tilts the direction of this research in the direction of industrial and programmed research, away from the direction of free inquiry and thereby spoils, for me, the point of it all.

So, I think that we must proceed with great caution lest we raise physical containment to the point where economically we exclude all but the people we are most worried about.

DR. SINGER: As I indicated before, I think the idea of the national commission has proven itself to be a useful way to look at very difficult problems. And therefore I think that it is a useful way to begin to structure debate about the long-term applications of this technology and of genetic engineering, should that become a reality.

With regard to legislation for the safe conduct of recombinant DNA experiments, it's not clear to me that such legislation is in fact required. There are existing regulatory mechanisms which might be able to take this under their wings. For example, we have the Occupational Safety and Health Administration whose responsibility is the safety and health of people in places of work, which includes laboratories. It might well be that they have sufficient power in existing legislation to undertake the regulation of this work. It may well be that the Center for Disease Control, which also has certain legislative responsibilities, has sufficient power under existing legislation to add recombinant DNA to their control of known pathogenic agents.

If existing powers are there and can be used, then it's not clear to me that we need to burden the country with yet another regulatory agency, nor is it clear that a special regulatory agency would do a better job at assuring a certain amount of safety than the existing mechanisms would. So, I think that a very careful look at existing powers is required before anyone can say that we do or do not need specific legislation.

With regard to the licensing, that's an idea which I thought a good bit about over a year ago or so. The only component in the whole system which is even suggestive of being suitable for licensing are the restriction enzymes. But in fact it's so easy to make restriction enzymes by yourself and the fact is that most laboratories do make their own because the commercial ones are so terrible, that the licensing of that doesn't seem a very practical approach to control. And I haven't been able to think of any other thing that might be licensed in order to do something effective.

MODERATOR RANDAL: I just want to make one very quick comment, Maxine. I also think it sounds like a good idea, but I know of a Nobel Prize winner who shall be nameless, who constantly petitioned one of the senators, if not both of them, of the state from which he comes because he is angered that OSHA applies to his laboratory.

DR. SINGER: If we were all worried about what made people angry, we wouldn't get involved in all of this.

MODERATOR RANDAL: There is considerable intervention, I would think.

DR. POLLACK: I have the complementary tale to tell. That is, I work at a state university which, because it's funded by the state, built by the state, it's not under OSHA regulations. And I have had a devil of a time trying to get a sense of whether the laboratories and our universities biohazard guidelines fit within OSHA regulation, just in general, out of curiosity. So, I can't put great stock in this pre-existing regulatory agency (OSHA) with regard to at least this set of laboratories. I believe that by

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Prepared Statement of Norman Wengert, Professor of Political Science, Colorado State University, Fort Collins, Colorado 80523, and Member of the Wisconsin Bar, presented to the Subcommittee on Science, Research and Technology of Committee on Science and Technology, U.S. House of Representatives, May 4, 1977.

PUBLIC PARTICIPATION IN SCIENTIFIC AND TECHNICAL DECISION MAKING

Introduction

My name is Norman Wengert. I am a professor of Political Science at Colorado State University in Fort Collins, Colorado. I am also a member of the Wisconsin Bar. My entire professional life has been devoted to the study and participation in policy formulation and assessment, focusing particularly on resources, environmental, and science policy and administration. While two-thirds of my adult life has been devoted to University teaching and research, I have had ten years of federal government service with agencies concerned with resources development and environmental policy. Almost all of my research and writing has been in these fields. I am not a specialist in recombinant DNA nor in biological research, but I have long been concerned with issues of science policy. For example, in 1960 I edited an issue of The Annals of the American Academy of Political and Social Science entitled "Perspectives on Government and Science," which included among its distinguished

summarize parts of that article and if the Chairman of this Committee permits, I would like to introduce the entire article as an appendix to my statement. (The University of New Mexico, School of Law, which publishes the Natural Resource Journal has granted permission to reproduce the article in this manner.)

In the article I recognize that participation and citizen involvement have become important dimensions of governmental processes in the United States, but I also stress that no viable political theory formulating principles or establishing the methods and bounds for such activity has yet been articulated. Instead there is much rhetoric on the subject, backed up by wide-ranging and disparate perceptions, attitudes, and assumptions to justify advocacy of greater participation. It is popular to refer to the New England Town Meeting admired by Thomas Jefferson as a model for participation, but a little reflection indicates that such meetings provide no more than a superficial analogy for today. It is somehow difficult to hold town meetings in communities of five thousand, much less of two hundred twenty million!

The article points out that those urging increased citizen participation in governmental decision making perceive its function in different ways:

1. Participation as policy (It's good Democracy);
2. Participation as strategy (It furthers public support, increases political power, and permits control of or by the bureaucracy);

If these are indeed accurate assertions, then the focus of attention of those designing public institutions should be on control, on responsibility, on accountability, on how action agendas are determined, on how the public interest is defined and by whom, and not on simple processes of participation and involvement.

Basic Conflicts and Tensions

In its broadest terms, therefore, the issues which this Committee is considering in these hearings (as I understand them) are how the research agendas and the research programs of scientists, clearly an elite group in our society, might most effectively be guided in the public interest and how processes of public involvement and citizen participation might be used constructively to provide such guidance. Or stated in reverse, how should the public (citizens) relate to the scientific endeavor and what can and should be the public contribution to that endeavor.

Oversimplified, the issue appears to be simply control versus freedom. Scientists have long been concerned about freedom of enquiry (academic freedom to those of us in the Universities), and in this context public involvement in setting goals for research or in reviewing research procedures is looked on with hostility and suspicion. As science has become more and more specialized, any kind of review or other control raise issues of irrelevance, arbitrariness, and repression. Who determines the agenda for research and research priorities is not idle speculation. It can be of vital importance to society, to scientific

In designing institutions for dealing with such situations two points need emphasis. First, is the overwhelming quantity of both government and private sector decisions which may knowingly or unknowingly affect the lives of hundreds, thousands or even millions of people. In many situations, therefore, it is important to contemplate mass meetings or referenda to review the myriad of decisions that may affect and influenco our lives. More feasible and effective means for identifying and implementing the public interest must be developed. The protest groups, the watch dog organizations, the Ralph Naders are important but hardly sufficient to deal with the demands of the situation.

Second, to be effective, therefore, proposals for citizen participation vis a vis the scientific enterprise must be more discriminating, dealing in specific terms with procedural and policy issues, distinguishing between what participation is NOT and what it cannot be on one hand and what participation is and how it can be used constructively on the other hand. Questions as to what citizen inputs should be, when they should be made, and how they should be structured require answers. Similarly, attention needs to be directed to when scientists should seek citizen review, what information should be provided, what the scope of review should be, and the status of and weight given to citizen attitudes and expressions. The objective would be to permit and encourage two-way flows of views and information.

But valid and desirable as such procedures would be, they tend to side-step the issue of control to which perhaps there is no single or simple answer beyond stating that the problems must be dealt with

the basis for or the terms of participation. Even the generic statutory phrase "maximum feasible participation" included the word "feasible"--and feasibility is at the heart of the issue.

Freedom of Enquiry

From the point of view of individual freedom, including the freedom of intellectual and research enquiry, I would suggest that the ringing words of John Stuart Mill from his essay On Liberty provide a counter weight to the rhetoric on participation far more consistent with our traditions. Mill wrote (in Chapter II):

> "If all mankind minus one were of one opinion, and only one person were of the contrary opinion, mankind would be no more justified in silencing that one person, than he, if he had the power, would be justified in silencing mankind."

But particularly important to his tolerance of free speech was NOT the concern of some present-day psychologists that repression of that one person's speech would damage his ego because to achieve his full development he must be allowed to speak, but rather Mill stressed that society loses when speech is curtailed. Thus he wrote:

> "But the peculiar evil of silencing the expression of an opinion is that it is robbing the human race: posterity as well as the existing generation."

He presses his position no matter whether the opinion expressed is "right" or "wrong," suggesting that in most cases opinions are probably partly right and partly wrong and that it is through the clash of opinions, "the free trade of ideas" (quoting Justice Holmes), that society moves nearer the truth.

tested to assure protection of citizen interests. It must be recognized, too, that citizens probably have reason to fear that high specialization and single-minded pursuit of narrowly defined research missions may result in neglect of other social values, e.g., concerns for the environment and even for life itself. From their perspectives, citizens may perceive risks and benefits differently.

In part, citizen views may rest on lack of information and understanding; in part on oversimplification of issues and consequences. But perhaps the most difficulty arises from divergent moral and other values which when applied to research may (as viewed by the scientist) distort procedure, alter priorities, and frustrate objectives. The classic statement by Justice Oliver Wendell Holmes (in his dissent in Abrams v. United States) comes to mind in this context:

> ". . . If you have no doubt of your premises or your power and want a certain result with all your heart you naturally express your wishes in law and sweep away all opposition."

In these terms, the issue may become one of who has the power and in the struggle, the public interest may be forgotten.

The administrator and the politician, on the other hand, while often sharing the citizen point of view, also have other concerns. They want to know and respond to the degrees of public acceptance of and support for specific scientific programs and program goals. The politician in particular is highly sensitive to the need for public support (or at least for avoiding intense public opposition) since he must face the voters in order to be re-elected. And concern for public acceptance and support may obscure issues of merit or need.

What Participation is Not

Citizen participation in science policy and practice cannot be thought of as a mass meeting or a public referendum. Mass meetings and protests, such as those which have occurred from time-to-time in objections to atomic energy facilities provide important signals to policy makers, but it would be unfortunate if either participants or the targets of such activities confused such events as reflecting the "voice of the people." Hopefully we do not react like the French politician who observing a mob go by his window exclaimed "Those are my people; I am their leader; I must follow them!"

Mass meetings, protests, and demonstrations, while negative, are an essential part of larger social processes. "A function of free speech under our system of government," wrote Justice Douglas for the majority in Terminiello v. City of Chicago (337 U.S. 1, 1949), is to invite dispute. It may indeed best serve its high purpose when it induces a condition of unrest, creates dissatisfaction with conditions as they are, or even stirs people to anger. Speech is often provocative and challenging. It may strike at prejudices and preconceptions and have profound unsettling effects as it presses for acceptance of an idea. That is why freedom of speech, though not absolute, . . .is nevertheless protected against censorship or punishment, unless shown likely to produce a clear and present danger . . . ." At the same time, it is a serious mistake to regard mass meetings, protests, and demonstrations as democracy in full bloom. Such activities can never be a

on occasion less formal groups of scientists take on review responsibilities. It would be misleading to suggest, however, that peer review has no flaws. First, peer review tends to be conservative and it may dampen innovation. The hostile rejection by the earth sciences community of the continental drift theories of Dr. Maurice Ewing is disturbing evidence of professional resistance to new ideas. Only his persistence and the more-or-less fortuitous support of the Office of Naval Research led to what we now know as plate tectonics and major revisions in theories of geologic processes. Second, peer review has been known to be influenced by bargains, trade-offs, and doctrinal biases. Third, peer review may be limited by incompetence of various kinds, e.g., the reviewers may not have the requisite knowledge and experience, a prohlem of particular significance where research is interdisciplinary.

Sunshine and Openess. Sunshine laws and freedom of information acts are becoming commonplace. Unquestionably if the intent of such enactments is realized with respect to research, higher levels of responsibility to the public will result. The concept of replication, so important to the scientific method, rests upon shared information. But sunshine and openess also create problems. One of these involves the lag between theoretical formulations and experimental proof. Another reflects the psychological set of many researchers as well as their bitter experiences. Research into a problem, particularily in early stages, is highly personal, even in team research. Experience, real or

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be furthered by government agencies. But recent experience in many fields has shaken confidence in government capacity and competence to fulfill its role in this regard. In discussing this problem as related to environmental protection Garrett Hardin raises the age-old Quis custodet problem of how to keep bureaucracy responsible. Hostility among research and program agencies to the Office of Management and Budget seems to suggest a special bureaucratic problem deserving careful analysis.

The Ombudsman. Although several states have been experimenting with an ombudsmen to give focus to citizen concerns, these positions have only indirectly dealt with research and the relationship of the scientific endeavor to public participation. And in any case, results seem to have been mixed. Perhaps this too is a subject needing careful appraisal, since as an idea the ombudsman approach has much to commend it.

Conclusion

Problems of control of science and of relating science to the public are today considerably different from those of 200 years ago when the doctrines of academic freedom and freedom of enquiry were first being formulated. Today control to a large extent involves the allocation of public funds. It involves questions of priority; it involves relating work of many individual scientists effectively so that the end product has social value and significance. In addition, because of the substantial developments in science itself, control involves new kinds of questions of responsibility to the public in general and to policy

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CITIZEN PARTICIPATION: PRACTICE IN SEARCH OF A THEORY*

NORMAN WENGERT**

"If there is a political revolution going on throughout the world, it is what might be called the participation explosion."[1]

Although the participation phenomenon may be worldwide, its meaning, role, function, and importance vary from culture to culture and political system to political system. It also seems evident that the drive or reasons for seeking more participation vary, depending on the perspectives from which the subject is approached, the institutional, political, economic context, and the personal interests and points of view of those opposing as well as of those supporting participation. Similarly, the phrases "public participation" and "citizen involvement" have many meanings and connotations, depending on the situation to which applied and the ideology, motivations, and practical orientations of the users.

The terms are used in the context of fundamental political decisions with respect to government structure and the content of public programs, referring to the importance of "consent of the governed" as a prerequisite of the social compact. But the terms are also applied to routine processes of political activity, such as political parties and elections, administrative program planning, and day-to-day management of public agencies. Demands for more public participation may be motivated by a desire to alter the power structure and thus weaken "the establishment," or they may simply seek better information inputs and more responsive public service.

Given this variation in usage and the many meanings and connotations of the terms citizen involvement and participation, it is probably not surprising that neither normative nor empirical theories applicable to the topic have been formulated. Little research on the subject has been undertaken, and even as speculative philosophy the ideology of participation has not been systematically organized or neatly structured. Yet in the last decade the literature on citizen involvement and public participation has grown, so much that it has

*This article is based in part on a study prepared for the Economic Commission for Asia and the Far East of the United Nations.

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1. G. Almond & S. Verba, The Civic Culture: Political Attitudes and Democracy in Five Nations 2, as quoted in Participatory Democracy 1 (T. Cook & P. Morgan eds. 1971).

problems, both implicit and explicit, in the current emphasis on public participation, to suggest some of the previous thought on the subject, and to indicate points at which both normative and empirical social theory may have something to contribute toward putting citizen involvement and public participation into a philosophic perspective. Perhaps this effort may suggest lines for subsequent philosophic inquiry and empirical research.

PERCEPTIONS OF PARTICIPATION

As indicated, those urging citizen participation (as well as those resisting it) perceive it in different ways, depending on such factors as position and status, whether they are in power or out of power, their responsibilities, their constituencies, their overt and covert goals, and many others. In part, perceptions are tied to motivations—an impenetrable morass for policy analysis, for while types of motivations can be described, it is often impossible to know which motivation or combination of motivations determined particular behavior. This has been the dilemma faced by the legal realists[6] in seeking to explain judicial behavior, and it continues to plague attempts at explaining any social behavior, whether of individuals or groups. In most situations, the best explanations must rely on the weakest component of scientific method—inferences and circumstantial evidence. And to an unavoidable degree, this deficiency limits the following general exploration of perceptions of participation.

Participation as Policy

To some, increasing citizen participation is simply a matter of sound and desirable policy to be implemented in as many ways as possible. Like most policy choices, this is a normative conclusion—a goal to be sought. Thus a high official in the Department of Commerce can state, commenting on artificial rainmaking, that the person on whose land manmade rain falls has a right to be consulted. And the idea of a "right" to be involved in decisions affecting one is frequently voiced in the literature. What the nature of that involvement should be, how it relates to decisionmaking responsibility, and whether the normal representative system and the constitutional protection of individual rights are insufficient (topics to be considered below) are seldom discussed.

6. Jerome Frank, seeking to apply Freudian psychological concepts to the judicial process (J. Frank, Law and the Modern Mind (1936)), developed intriguing theories of behavior, but they were largely untestable short of psychoanalysis.

as conflicts rest upon misinformation, participation and involvement in town meeting situations provides opportunities for exchange of information and may induce modifications of values and opinions and increase confidence and trust. While intimacy may breed contempt, group discussions and exchanges of ideas are said to minimize hostility and may permit constructive collaboration. Certainly experiences in the field of labor-management relations would seem to support this proposition. At the same time, the proposition that participation leads to consensus would in most situations be of dubious validity. There is reason to believe that in a nonhomogeneous community increased participation will highlight differences and increase conflict. Probably the proper question is whether a condition for consensus already exists—in which case participation may further its realization. But where a condition of diversity exists, participation can contribute little to conflict resolution and may even increase conflict by creating confrontations and inducing polarization. Where a diversity of interests is clearly established, participation can contribute to conflict resolution only in highly structured situations with institutionalized procedures and a willingness to accept unacceptable decisions (as in litigation).

Participation as Therapy

In recent years the emphasis on participation as social therapy has been frequently articulated in connection with the so-called War on Poverty.[7] On the premise that particularly the urban poor are alienated from society, opportunities for them to be involved in decisions with respect to programs which affected them were provided to cure this "social disease." Variants of this approach have appeared on college campuses, leading to varieties of student involvement in academic decisions. Proposals for increased participation have also been directed to overcoming the adverse effects of racial prejudice and other forms of discrimination.

STIMULI FOR INCREASING PARTICIPATION

One of the major stimuli to current interest in participation is rapid change in the patterns of life which pose a threat to traditional existence and require a host of adjustments in ways of solving prob-

7. The "War on Poverty" has generated a tremendous literature. Numerous publications deal with the concept in the statute urging that "maximum feasible participation" be secured from the poor. How this concept got into the law without much deliberation is detailed in D. Moynihan, Maximum Feasible Misunderstanding (1970). *See also* Advisory Comm'n on Intergovernmental Relations, Intergovernmental Relations in the Poverty Program (1966).

has obviously not been against industrial production; its dominant concern has been with control of that production.

The Communist Manifesto sought to rally workers by the slogan "Workers of the world unite; you have nothing to lose but your chains." For Marx and his followers these chains were not only lack of economic benefits from labor inputs, but also psychological alienation resulting from not having a role in the productive process. It was consistent with these views for Lenin to emphasize in 1917 worker participation in the organization of factories, using the slogan, "All power to the Soviets," the Soviet being the local council of workers. But Communist practice has not dealt any more effectively with the problems of alienation stemming from size and depersonalization of the productive process and patterns of modern life in a scientific and technological era than has the capitalist world. That the present clamor for participation has roots in this situation seems evident.

HISTORICAL INTERPRETATIONS

It would be a mistake to suggest that citizen alienation alone is the cause of the present interest in participation. Although the conditions which induce modern alienation probably did not exist in the New England town—the classic image of true American democracy—other social forces undoubtedly affected individual behavior so as to prevent full and free expression of opinions and unfettered participation in community life. We know, for example, that theocratic dominance was an important constraint in New England governing processes. But in any case, the town meeting ideal admired by Jefferson and other democrats was incorporated into the American local political structure by converting the survey townships into governmental and school district units, even though the six mile square pieces of geography did not always coincide with sociologically defined communities. Thus town and school district meetings did provide opportunities for extensive citizen participation in local government. At the same time, reflecting both population numbers and spatial distance, a complex representative system at state and federal levels, reinforced by political and electoral systems, provided for the form of popular control, if not always the substance. Implicitly, the present emphasis on community involvement and citizen participation raises doubts as to the validity and adequacy of the American representative system, which has substantially taken the place of an earlier system which provided for citizen inputs at the township base of the governmental pyramid. At issue is the question of where participation fits in a nation of 220 million people.

At the same time, normative theory as well as pragmatic experience continue to influence how Americans regard government and the governmental process. In the following paragraphs reference is made to a wide spectrum of theory, with suggestions that ideas on citizen involvement and public participation might benefit from specific attempts to relate them to these theories. Implicit is the belief that, rhetoric aside, public participation as a theory of governance has not been effectively dealt with and that its formulation and critical analysis is badly needed.[10]

American government rests on pragmatic experience, rather than on grand formulations of political theory. Our great documents enunciating political principles, such as the Declaration of Independence and the Federalist Papers, are polemical rationalizations of political action. Americans, in politics as in other aspects of their culture, are not philosophers or great theoreticians. Pragmatic responses to particular problems have dominated political action —and the major characteristic of pragmatic philosophy is that it is no philosophy. Thus it is not surprising that such political theory as we have been able to articulate has been retrospective, inferred from action, behavior, and political statements and writings rich in normative content.

It has frequently been pointed out that the Founding Fathers held to no fully articulated philosophy of government. We are left to infer their values and perceptions from the polemical Federalist Papers, written to persuade New Yorkers to vote for the proposed Constitution. Although the Federalist Papers are conceded to be great works of advocacy and reflective of the pragmatic mood which still dominates American political thought, they hardly provide a coherent and integrated statement of political doctrine. Being dominantly instrumental in character, they express concern over rule by the masses and the influence of interest groups (factions—including political parties). At the same time, they voice support for a checks and balances system which reflects fear of a too powerful government.

From the beginning of the U.S. government conceptions of the

10. Perhaps the lack of attempts to deal with participation is overstated. The criticism is really directed at the more ardent advocates of participatory systems, many of them Federal bureaucrats, who have not faced up to the conceptual problems with which this article deals. The following works, largely by political scientists, indicate some efforts in the analysis of participation: G. Amond & S. Verba, *supra* note 1; R. Dahl, A Preface to Democratic Theory (1956); T. Dye & H. Zeigler, The Irony of Democracy (2d ed. 1972); T. Lowi, The End of Liberalism (1969); A. McFarland, Power and Leadership in Pluralist Systems (1969); D. Thompson, The Democratic Citizen (1970); S. Verba & N. Nie, Participation in America (1972); H. Zeigler & T. Dye, *Elite-Mass Behavior and Interaction*, 13 Am. Behavioral Scientist (1969).

not tremendously successful. Others sought to experiment with proportional representation, seeking to correlate representation to voting strength. This remains a characteristic of the German Parliament. In any case, those who urge greater public participation, and certainly those who seek to formulate a political theory on participative democracy, must confront the question of how participation is to be related to representation. Whatever system may be proposed, representation is a stark necessity which must reflect population size and geographic area. And while one may join Rousseau in concluding that a representative system is not democracy, one must nevertheless confront the question of designing a system, in which there is a degree of responsiveness and citizen control. The alternative is to opt for dictatorship.

THEORIES OF POWER

Through the ages political philosophers have been fascinated by issues of social and political power—the influence by some over the behavior of others. Concepts of public participation could benefit from efforts to relate them to theories of political and social power. Three aspects of power theory would seem of particular relevance: the *first* is the revolutionary concept of the *seizure of power;* the *second* are the concepts of *community power,* as developed in a variety of social research in recent decades; and the *third* are *elite theories,* ranging from rather modest research in leadership to Hobbesian criticisms of democracy to C. Wright Mills' analysis of the Power Elite.[12]

Seizure of Power

Seizure of power, at least since the French Revolution of 1789, is the other side of the coin on which is engraved "Power to the People." It serves to remind those concerned about formulating a political theory of participation that citizen involvement, especially when not structured, can become a revolutionary force seeking the redistribution of power. It raises the question of whether, and to what extent, an existing system ("The Establishment") can accommodate change.

Community Power

Community studies became well-established, if not popular, during the 1920's and 1930's, e.g., *Middletown* by Robert S. and Helen M.

12. C. Mills, The Power Elite (1956).

mental fact that a few citizens are always called upon to govern the remainder.[16]

This statement must be dealt with in a viable theory of participation. In more moderate terms, the problem is one of authority and responsibility, of leadership and capacity, in the context of which the nature and scope of participation are to be spelled out.

The issue of the importance of a controlled and responsible elite is more sharply drawn by Professors Thomas R. Dye and L. Harmon Zeigler in their *The Irony of Democracy*. In a trenchant and challenging *Postscript* to the Second Edition, Professor Dye asserts:[17]

> Mass governance is neither feasible nor desirable. Widespread popular participation in national political decisions is not only impossible to achieve in a modern industrial society, it is incompatible with the liberal values of individual dignity, personal liberty, and social justice. Efforts to encourage mass participation in American politics are completely misdirected. To believe that making American government more accessible to mass influence will make it any more humane is to go directly against the historical and social science evidence. It is the irony of democracy that masses, not elites, pose the greatest threat to the survival of democratic values. More than anything else, America needs an enlightened elite capable of acting decisively to preserve individual freedom, human dignity, and the values of life, liberty, and property. Our efforts must be directed toward ensuring that the established order is humane, decent, tolerant, and benign.
>
> Elitism is a necessary characteristic of all societies. The elitism we have ascribed to American society is not a unique corruption of democratic ideas attributable to capitalism, war, the "military-industrial complex," or any other events or people in this nation. There is no "solution" to elitism, for it is not the problem in a democracy. There have been many mass movements, both "left" and "right" in their political ideology, which have promised to bring power to the people. Indeed, the world has witnessed many "successful" mass movements which have overthrown social and political systems, often at great cost to human life, promising to empower the masses. But invariably they have created new elite systems which are at least as "evil," and certainly no more democratic, than the older systems which they replaced. Revolutions come and go--but the masses remain powerless. The question, then, is not how to combat elitism or empower the masses or achieve revolution, but rather how to build an orderly, humane, and just society.

16. H. Zeigler & T. Dye, *supra* note 10.
17. T. Dye & H. Zeigler, *supra* note 10

brevity of treatment does not reflect their lack of importance, but rather space limitations and the competence of the author.

Behavioral Analysis

Discussions of participation tend to reveal an egalitarian one man-one vote bias. As normative policy this is consistent with dominant American values. As psychological reality it falls considerably short. Theories of public participation have not yet begun to utilize the results of social, psychological, and behavioral research. Theories of public participation need to take such findings into account. Only in this way, for example, can what is known about the "silent majority" be dealt with adequately. To concepts of alienation need be added concepts of span of attention, so that the limitations of hortatory admonitions to "get involved" are qualified by hard reality.

The Boundary Problem

Recommending participation on the lowest level or on a face-to-face basis does not automatically identify the geographic unit which provides the focus for attention. In fact, one of the most difficult and complex decisions is determining appropriate boundaries. Simple geography, i.e., where people live or work, is not enough. Problem boundaries must be related to reflect interest boundaries—and depending on the problem these could be the entire nation. Who, for instance, has an interest in a National Forest? Clearly, those living close to it, but not they alone. Those in the watershed of the forest, those using timber and timber products, those seeking recreation in the forest and many more have an interest. Who has an interest in the public domain, in atomic energy research and production, in coal and oil production, in the development of a river? Paraphrasing the Supreme Court in a 19th century case, "We are, after all, one nation." The locale is important, but it is not the sole dimension. The gerrymander must be recognized as a factor in drawing social and economic boundaries as well as political boundaries. Boundaries determine problems and participation. If one's goal is to raise average income levels in Appalachia, one can achieve this goal by redefining Appalachia to include Philadelphia and St. Louis.

Functional Approaches

Structural-functional analysis continues to be a valid and useful social science technique. A traditional and still important approach to American government has been separation of functions into legis-

concept of majority support for any program or policy is difficult to prove. Even in a town meeting situation majority views of the community and certainly majority interests are difficult to identify. On a few limited issues polling may give a static picture of attitudes, but it cannot capture the dynamics of change, particularly in highly volatile situations.

CONCLUSION

A classic statement of elite theory is the "iron rule of oligarchy" formulated by the French sociologist Robert Michels. As theory, his conclusions would clearly be opposed to most concepts of participation—even though, as this article has suggested, there is not yet a coherent body of ideas which might be labeled participation theory. But if Michel's conclusions approximate reality, the role of participation is narrowly constrained and must be approached on a much more limited basis. Perhaps the issues are, as they have been from 1789 on, issues of controlling government, assuring sound and wise decisions, providing for due process, protecting minority views, establishing responsibility and responsiveness, seeking equity, and striving for the public interest. It is a sobering thought that, in the context of one man-one vote—the simple statement of majoritarian decisionmaking—most of those shouting loudest for participation have generally been minorities. The poor, the Blacks, the environmentalists—all are clearly and obviously minority groups. Only a sense of equity and public responsibility (contrary to the economic model resting largely on greed and self-seeking), together with a good portion of concern and even fear, make a war on poverty possible. Social reform, environmental protection or other new thrusts in public policy have not been and cannot be majoritarian, participation rhetoric to the contrary not withstanding. There is no substitute for a policy which seeks the public interest.

For some time after World War II it was fashionable among social scientists to assert that the public interest was a myth—like religion, an opiate of the masses. What was confused in this view were the difficulties in defining the public interest and the ease of equating personal aggrandizement as the simple definition of that interest, with the much more important fact that it was the *search* for the public interest, the *requirement* to *rationalize* decisions as being in the public interest, that was the significant aspect of the concept. The preacher says "Seek ye first the kingdom of God;" the responsible democrat says "Seek ye first the public interest." Neither

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Green Mountain Junior College 1935
Union College–A.B., M.S. 1939
Syracuse University–Ph.D. 1951

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Green Mountain Junior College–1939-41, 1946-49
Chairman of Department, Director of Student Personnel

Syracuse University–1941-43, 1949-51
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Has been lecturer for Maryland and Tulane Universities

Military Service:

Served in U.S. Navy during World War II and Korean conflict as an Aviation Psychologist. Achieved the rank of Captain which he now holds in the U.S. Naval Reserves. Retired 26 July 1970.

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Over 40 Research Reports
Over 13 Addresses
Co-author of Two books:
"Sensory Deprivation"–Harvard
"Psychological Stress"–Appleton, Century, Croft
Director and Editor of NATO Exercise and publication on "Environment Modification for Human Performance."

Family:

Four daughters and four grandchildren.

Special Awards and Honors:

Navy Superior Civilian Service Award, 1960
Navy Distinguished Civilian Service Award, 1961
Raymond F. Longacre Award of Aerospace Medical Association, April 1966 (outstanding accomplishments in the psychological and psychiatric aspects of aerospace medicine)
Nominated for Rockefeller Public Service Award, 1966
Sustained Superior Accomplishment Award, June 1966 (based on "0" performance rating)

Participation in Societies and Organizations:

Committees:

Interagency Committee on Cross-Cultural Researc
Behavioral Sciences Advisory Group for ARPA
NASA-DOD Life Sciences Sub-Panel
Advisory Panel for Navy Surgeon General
Committee on Scientific Communication in Bi Astronautics and Space Medicine
American Institute of Aeronautics and Astronauti (AIAA), Committee on Instrumentation
Advisory Group on Aeronautical Research an Development (AGARD)–NATO
NASA Research Advisory Committee on Bic technology and Human Research
Past President, Military Psychology Division, Amer can Psychological Association
Chairman, NATO Advisory Group on Huma Factors
Steering Committee, Career Management Plan fo Scientists and Engineers
Distinguished Civilian Service Awards Panel
American Nurses' Foundation, Research Advisor Committee
Advisory Committee to the Surgeon General, Fed eral Aviation Administration
AF-NRC Committee on Vision, Executive Counc
AF-NRC Committee on Hearing and Bioacoustics
AF-NRC Committee on Vision

Society Membership:

American Psychological Association
Aero-Space Medical Association
American Association for the Advancement o Science
Maryland Psychological Association
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Office of Naval Research history:

1953-1954 Assistant Head, Physiological Psychology Branch
1954-1961 Head, Physiological Psychology Branch
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1967-1968 Research Director
1968-1970 Director of Research

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TESTIMONY PROVIDED BY
DR. RICHARD TRUMBULL, EXECUTIVE DIRECTOR
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MAY 4, 1977

It is a pleasure to have this opportunity to share some thoughts on public participation in scientific and technical decision making. Responsible participation in an area to which they come with little background and much apprehension poses special problems. There can be little doubt but that the public is asking, even demanding, to be in the game. As has been true in other areas, we find ourselves paying a price for concentration upon advancement in one cultrual entity (science), in this instance sparked by WWII and Sputnik, while neglecting others. Thus, it is that we now, rather belatedly, recognize our increasing requirement for concomitant advancement in education, social, ethical and moral values. There is much catching up to be done to bring it to pass even at some future date. Therefore, it is vital to set planning of an orderly sequence of achievable levels at the present time.

Today, we are considering the publics which must now be recognized, informed and acknowledged as legitimate partners in the scientific enterprise. The plural, publics, is appropriate because everything which I have read to date distinguishes many separate "publics" as governmental agencies attempt to venture into this new world utilizing the public in the decision-making process. This distinction avoids assumption that our title refers solely to the great American public with resultant over-simplification of the problems as well as solutions. Thus, we must analyze the elements, the levels and the other participants in scientific and technical decision making before we attempt to model appropriate and meaningful public participation. As indicated above, we also must ascertain just where our culture is in its educational, social and other relevant developments. Only the most optimistic

It is regretted that we must indulge in this definition of "publics" at this late date. There have been many statements by officials about public participation in decisions in their agencies with the public left to decide just who was to be involved in the new venture and to what purpose. Undoubtedly, these statements have raised hopes. They certainly already have resulted in costly and unproductive forums, town meetings and other media where all of the forces of confrontation, the adversary process and a little knowledge being a dangerous thing have intermingled to serve no purpose but the opportunity to be heard or, more accurately described by modern idiom "to sound off." If provision of the "feeling" of participation is the actual objective of our current undertakings on public participation, it is far better to state this frankly and clear the air. The public(s) do not deserve to be deluded or lulled into a false sense of involvement in an issue of such consequence. There are the skeptics and those who will watch every step along the way to be the first to declare "they never meant it." Thus, it is especially satisfying to appear before this Committee which is indeed very serious about the matter of public participation in science and technical decision making.

Among the many reasons why we find ourselves discussing the topic before us today is our growing awareness of the ultimate, complex nature of things. We have learned that there are few individual variables. Actions taken to solve one problem often bring others to the fore, in other systems, in other decades and, even, in other generations. We have come by this intelligence the hard way, and its pervasiveness will mark this generation's decision making in many ways. Public participation is but one way to assure that nothing is overlooked, whether it be a determining factor or potential impact. We need all of the help we can get to assure that full consideration is afforded all variables.

informing umpires, referees and others, nominally in charge of the game, when we differ from their decisions - a difference arising from our biases as much as our vantage point.

I would rather not see decision-making in science reduced to this type of sport based upon some contention that we brought our ticket to the arena and that, alone, qualifies or justifies our participation as kibitzers, getting onto the field to delay the same or destroying the goal posts. Someone else is always left with the responsibility for replacing the goal posts and assuring that next week's game is played while the "fan" returns to his work-a-day world in search of some other diversion. Thus, I continually return to our responsibility for an informed public and the need for that informed public to be responsible as the present decision maker opens his process to public participation.

The process begins by the department or agency designating specific decision-making levels for public participation. It is noted that the Department of Health, Education and Welfare Task Force report has suggested that "The HEW staff should systematically conduct meetings and seminars with a wide variety of citizens prior to to the Secretary's final decision on all major policy matters." (Underscoring mine.) Further, "there are approximately 90 major and diverse programs in the Department (HEW) which call for some form of citizen participation." Once the decision has been made as to where participation from outside the agency can be helpful, one is faced with the identification and selection of the appropriate "public." In the past, we have used the expressions "interested" or "concerned" parties. It is unfortunate that this more realistic phrasing was not retained because subsequent definition of "publics," how one contacts and selects them, and expected roles would have been easier to achieve. The mechanisms HEW anticipates and employs reflect some

There are other needs for public participation in agencies which influence priorities in science and related funding. The National Academy of Sciences and the National Science Foundation have roles to play and charters which require procedures to assure inputs from widely diverse sectors of interest. They must not be perceived or treated as "captive" organizations by either the scientists or the public. This is not so much a matter of maintaining public support as it is public understanding and appreciation of their roles. The issue is how they translate their vital role in behalf of science into the vital role of science in behalf of the public. For solution of that issue, they require responsible dialogue with representatives of that public. As odd as it might seem, on occasion, their own members and their own grantees do not play this second role so well.

It is apparent that our term "public" passes through many transitions in this sequence. The deliberate selection of experts and peers for advisory boards, review panels and program planning represent the use of individuals from outside the department or agency and hardly matches any usual definition of "the public." In many cases, these are individuals who, hopefully, will be perceived as surrogates by the greatest number of "interested" parties. One is reminded here of the widely divergent employments of "peer review panels" recently under consideration.

In addition to the requirements for managerial, scientific, technical, predictive or other skills, there will be a need for further education or orientation of the participants to be fully effective for any given particular agency or department or

Let us assume that this process has gone smoothly, and we are about to have "public participation.' It should be clear from the beginning that seldom if ever are we expecting the great American public to be participants in such decision making. There are other means for their involvement than those afforded through the process considered here. Experience has shown that any hearing declared to be "public" will attract individuals with many needs to be heard and seen quite unrelated to the provision of information or contribution to decision making. There also will develop a cadre of "hearing attenders" whose consistent attendance will shame others of more competence and somewhat greater desirability. Equally certain is the tapering off of these attendees as the newness disappears and the very fact of a public hearing has been established. The latter point, notwithstanding, no hearing should become perfunctory.

The preparation for outside participation in decision making does not come easily even in the prescribed peer review context. The process is even more difficult as we move farther and farther toward holding a truly public forum. When assembling a group of scientists to review proposals in a restricted disciplinary area, one assumes that all participants are current on research in related areas. Too frequently, this is not the situation. We must concern ourselves, then, with the universality of information base represented by a truly public forum. If we are primarily providing an opportunity "to be heard" or "to feel as if they are participating," that is one thing. If we are merely interested in how the proposed program/policy affects or appears to them, that is another. In these instances, we are

It might be well for decision makers to afford such opportunities for discussion between groups of different persuasions before "going public." This is a maturing process which tests the objectivity of both sides and exposes primary motives. As the informed American public we envision evolves, this process should become more widespread because their interests will be represented by the participants. The real public thus is participating because its surrogates are more accountable as the former review their actions.

This consideration of the real public as participants and the manner in which we accept its present understanding of the issues or policies under consideration reemphasize our responsibility for an "informed public." As a scientist who has devoted many years to acquainting engineers with their need for human requirements, technologists with their need for basic research, academicians for the need for research in the military and, finally, scientists with the need to communicate with that public which supports them, I fully appreciate the pitfalls and problems on the path to that day when the American Public truly can play a responsible role in decision making. I have taken some pains to define our many publics in this presentation to alert some administrators not to expect too much from public participation and, likewise, to temper the public participants' expectations of influencing things beyond their comprehension. While much of the impetus for public participation today is aimed at preventing their abuse by the government, their education is equally important to prevent their misuse by adversary and special interest groups.

Let us now consider the operation of this participation, at any level. To reiterate supplemental, germane information is vital regardless of the composition of our "public" and the task before it. This is where the first inroads are made on staff time. Anticipation, collection and dissemination of appropriate information

is not too early to establish this concept and inform departments and agencies that such records and later analysis will be expected. Such a procedure also provides some incentive to truly try something new and not just retitle previous modes of operation to establish that "we have really been doing that all along." As I stated in the beginning, this is a major undertaking. Its complexity must be appreciated before we go much further lest the American public has one more experience vis a vis science and technology which is negative. We can ill afford such an experience in the years ahead.

One inevitable consequence of greater public participation in our decision making might well be an increasing demand for goal-oriented research. No matter how optimistic we might be about achieving an "informed" public in the future, the understanding of basic research's role in the scheme of things will be out of their reach for some time. The public will want results and its desire for accountability will impact upon all levels of decision making. The experiences of program managers who have had to fight constantly for support of their basic programs with informed supervisors, agency directors and, even, special committees of Congress underscore this point. This comment might elicit shudders from the basic research community but it does not necessarily follow that their freedom of scientific inquiry is in any greater jeopardy.

Program managers and upper echelons of management must become more capable of interpreting research results for public understanding and satisfaction. Accumulating experience with second and third order effects, as well as those in second and third generations, should assure us that program managers will become more sophisticated re the many variables in their areas of concern. They must dig deeper into basic processes and, typically, the information on basic processes will be of value across a

in scientific and technical matters as our educational standards should imply. Were we to start at the beginning in dealing with our topic of today, we would begin with the education of the greater public regardless of how we might extract certain groups for specific roles in decision making later.

Long before reaching this point, the reader has become aware of a conviction that the public will seldom be involved in the decision making under consideration in many ways other than the traditional. However, we have spoken to the increased need for a better basis for that public's understanding of science and its processes and means for its achievement. We do believe that the decision-making process must be opened in many ways with the appropriate representation of the public involved. The requirement for further orientation even of those well qualified in science and/or management in order to perceive their decision-making task in the perspective of the agency or program has been noted. Many of the guidelines for peer review team selection, avoiding inhouse control, conflict of interest and preferential treatment per individuals, laboratory, or research facility pertain to all levels of decision making. We have acknowledged the plethora of variables which must be dealt with in the context of improving the probabilities indicated earlier, including a benefit to the greatest number of the public. However, any decision will be of greater concern and/or interest to certain segments of the public and we must recognize that "the public interest" will pertain to them quite specifically. The recent saccharin/dieter situation was a case in point.

The declared intention of many scientific societies over the past decade to improve the public understanding of science has been implemented very slowly. There has been some opening of annual meetings for the public to hear major addresses in plenary

This is not to imply that change cannot be accomplished. Many scientific societies have cooperated with the National Science Foundation and other agencies to broaden the educational base of future generations, our primary requirement for public participation. We often overlook the tremendous growth in information and the continued pressures to lower the grade levels at which subject matter is presented. Thus, the volume of information believed to be relevant to living in today's world comes into conflict with the special knowledge believed to be required to qualify for specific degrees and occupational preparation. Although we might be witnessing some return to the graduation of an "educated" individual vs a trained specialist, any ultimate goal of truly "informed" public participating in decision making remains distant. In the meantime, our "publics" will continue to be various aggregates of peers, the interested and the concerned who will assume guidance roles to assure progress of a science, success of an agency mission and, even, protection of the public interest. Our hope lies in the concomitant advance of education, ethics and social values which will bring more objectivity and less self-interest into the arena wherever it is found.

DOROTHY NELKIN

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Nelkin's research has focused on controversial areas of science and technology, and she is the author of several books; on nuclear power plant siting, airport siting, military research in universities, methadone maintenance programs and most recently on science textbook controversies. She is presently completing a monograph on controversies over technology and participatory experiments in Europe.

Nelkin is a member of the International Council for Science Policy Studies, the National Academy of Sciences Committee on Nuclear and Alternate Energy Sources, and the AAAS Committee on Science, Technology and Public Policy. She is on the advisory Panel on Public Participation of the OTA, and on the executive councils of the Society for the History of Technology and the Society for the Social Studies of Science. She is a Fellow of the Hastings Institute of Society, Ethics and the Life Sciences.

PUBLIC PARTICIPATION
SOME EUROPEAN EXPERIMENTS

Dorothy Nelkin

I am pleased to participate in hearings on the question of public involvement in science and technology. The tension between the ideal of participation and its pragmatic implementation is a persistent problem for democratic governments, but in complex technological societies this tension poses special dilemmas that I feel must be faced directly and not merely by minor procedural adaptations in response to public concern about technology. My own research has focused on controversies over science and technology, on public demands for participation, and on ways that governments seek to extend participation to areas of policy often considered in the realm of technical expertise.

It sometimes lends insight to examine such issues in a comparative context. What I would like to do is to describe some recent efforts to broaden participation in policy making in several European countries--in Sweden, the Netherlands and Austria--for they suggest some tentative generalizations about participation in technology policy that might be useful in the American context.*

These three governments have initiated deliberate experiments in participation in response both to environmental protest and

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* A monograph with complete documentation of these experiments in their political context is forthcoming. See Dorothy Nelkin, Protest and Participation in the Technological State, SAGE Publications, Fall 1977.

matter, should be discussed from the diverse ideological viewpoints of political and social interest groups. Seven organizations participated; each ran several thousand study circles with 10 to 15 members who met together for at least 10 hours. About 80,000 people participated in all. The Ministries of Education and Industry gave the organizations funds to hire experts, to train leaders, and to develop material that would reflect the interests of concern to their participants. The cost of the program was about $650,000.

Government officials expected that public involvement would create more favorable attitudes towards nuclear power, but reports from the study groups suggested that prior commitments persisted with some increase in uncertainty and confusion. Moreover, surveys suggested that overall public opinion remained ambivalent. An inquiry into the direct effect of the study circles on attitudes towards government energy policy suggested only slight differences in the opinions of those who participated and those who did not. In fact, one evaluation suggested that increased knowledge contributed to uncertainty and indecision; the number of persons who could decide neither for nor against nuclear power increased form 63% to 73%.

The subsequent government policy essentially continued the existing program but also initiated an active conservation plan. Then in an upheaval partly attributed to anti-nuclear attitudes, the 1976 election replaced the Social Democratic Government with a coalition government headed by the Center Party leader. Despite campaign vows to kill the nuclear program, he too eventually approved its continuation.

hearings, analyzes the public comments, and makes recommendations. The Council report is re-circulated to guard against the risk of manipulating the public response in the course of summarizing it. The material then goes to the appropriate Minister who must respond to criticism, either defending or adjusting the proposed plan. Ultimately, it is the Minister who must resolve conflicts between local and national needs by publicly justifying his intentions. The Ministerial recommendations and all public documents go to parliament where citizens once again have a right to lobby.

These procedures, used originally for regional planning, were extended to include the plans for major technological projects in the transportation, communication, and energy sectors.

Austria

In Austria, after an active protest over the siting of a nuclear plant, the Chancellor directed the Ministry of Industry to set up procedures for a public debate on nuclear energy. The Ministry sought to organize a debate that would fairly reflect opposing points of view and adequately distinguish the technical from the political arguments. Those scientists who had most strongly expressed their opposition to nuclear power were asked to prepare a list of all questions they felt must be considered before a decision could be made on a nuclear program. The list was divided into ten themes. Teams of experts, equally divided between supporters and opponents of the nuclear program prepared information on the controversial

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industrial firms and efforts to broaden representation in decisions about scientific research. In 1975, a commission in the Swedish Ministry of Education, concerned with lack of public representation in the councils that established research priorities, proposed that these councils be re-organized. The proposed structure has two administrative levels that differentiate between research of "social relevance" and of "scientific relevance." A Research Councils' Coordinating Board will be responsible for initiating, coordinating, and supporting research in the category labelled "socially relevant." On this Board, representatives of public interests will have a "dominating influence." Seven of its twelve members will be appointed by government (mostly from the parliament). The other five will be appointed by the research councils. The Research Councils will be responsible for research of "scientific relevance." Seven of the ten members will be elected representatives from higher educational establishments representing the research community; the other three will be appointed by the government to represent research-dependent sectoral organizations. The commission also recommended that these councils, while dominated by researchers, should draw upon outside evaluation groups (from the political parties, labor unions, and industrial organizations).

In the Netherlands the Minister for Research and Development also proposed greater public involvement as a way to clarify the needs of society and to develop research priorities. The government, according to the Minister, cannot claim to fully interpret social needs; there must be scope for direct public intervention through the inclusion of consumers or users in the

power in parliaments at a time when proliferation of administrative bureaucracies seemed to widen the gap between the citizenry and its political representatives. Thus, public involvement was perceived as a practical means to implement technological policies and also to reinforce political stability by meeting criticism about centralization of authority and the declining influence of the citizen.

The experiments in each country differ, however, in the extent to which they actually allow for greater public influence on government policies. The structure schemes in the Netherlands for example, have greater possibilities for influence at an earlier stage in decision making than do the Swedish study circles which are mostly educational. In Sweden, there was assumed to be an underlying consensus about national goals, and the hostility over nuclear power was assumed to be an anomaly. Thus, the government expected that more favorable public attitudes would emerge given public understanding of the government position. In contrast, the most striking characteristic of the Dutch experiment is its effort to incorporate dissenting public opinion at the stage of policy making when objectives are first articulated as "policy intentions." Criticism and the expression of diverse opinions was encouraged; conflict is expected and accepted as a political reality. Austrian officials see public involvement as a way to create the conditions to implement government policies, but also as a means to clarify conflicting points of view. Note that it was the opponents of nuclear power who were asked to formulate the questions raised in the public debate.

of implementing specific policies.

The participatory experiments described above proceed with cautious enthusiasm mixed with fear about their implications for technical decision making and for existing representative institutions. But despite reservations, the participatory ideology has been "contagious." Demands for increased public involvement have spread from one sector to another; even the question of basic research is no longer immune as the recent events concerning recombinant DNA suggest. Reforms tend to reinforce each other creating expectations about the role of the citizenry. In the long run, the implementation of policies for science and technology, and the very legitimacy of the responsible authorities may depend on the politics of participation.

For us, scientific responsibility means "social responsibility". While we think there is a minority of scientists that is prepared to take this view, and they can indeed provide early warning, we think it would be wise to bolster this group. So we propose four possibilities that the committee might consider: (1) Asking the scientists more often what is going on, (2) listening better, (3) making it financially feasible for scientists to speak up and (4) commending them when they do the right thing.

One proposal has to do with the National Academy of Sciences. I discovered to my surprise, in preparing a footnote in the testimony about the National Academy of Sciences' procedures that President Carter had recently sent them a letter expressing the same concern that I wanted to express, that NAS studies take too long to be conducted. I think one of the reasons they take too long is that the academy has always worked on the basis of voluntary scientific help. This is an anachronism—the notion that scientists should not get any benefit from the advice they are giving the Government.

I think it would be a simple matter for this committee to urge the academy to ask the Government agencies that fund its contracts to put in additional sums for some kind of reimbursement of the time that the scientists provide.

I think you would then find it must easier to round up the scientists necessary to do the studies. But, in return for this, I would hope you would ask the academy to agree to accept the requirements of the Federal Advisory Committee Act, which thus far it has not accepted, it has argued that it is only a quasiofficial body and has persuaded the court that the act is not applicable.

Your questions suggest that you are concerned that scientists should speak up. But there are always some scientists prepared to speak up. The problem is that the inertia of government is such that it is a full-time job for more than one person to get the Government's ear.

I know the chairman understands this quite well. We felt when we saw the science for citizens program that this was an opportunity to forge an alliance between socially responsible scientists and public interest groups who had a predisposition to kinds of the conclusions that the scientists might be raising (that the arms race should be controlled or the environment protected, etc.). We felt that these public interest groups provided a platform which would amplify the view of the scientists in question.

We feel that these public interest groups are going to raise these questions in any case. They are going to sue, and are going to be involved in legislative action, with or without scientific help. The question at issue is: Are they going to behave in a more or less scientifically responsible fashion?

We understand very well that it can be controversial among the committee members, and in the Government at large, to assist a program that is engaged in helping groups intervene, in some sense, in Government processes. But we would like to make the following arguments about this: In the first place, these organizations are very responsive to the public.

They are in constant communication with the public and do not get funds, memberships, and contributions unless they are taking up an issue which is deeply felt by the public. With 7,000 scientists we,

to help the poor that that group should also be permitted to influence legislation and certainly to sue.

So the question of permitting advocacy with public moneys has, I think, already been resolved.

Mr. THORNTON. I think the point is well made that the nonprofit groups are permitted to do this and to utilize tax benefits.

Dr. STONE. Thank you, Mr. Chairman. Most of the groups that are at issue here are, in fact, tax deductible groups. We are the only scientific group in the country that is organized in a different manner, as a civic organization, that is nonprofit but not tax deductible.

This is something on which reasonable men can differ but another argument for science for citizens occurs to me. In my opinion a major problem in the scientific community is that there is not enough concern infusing the different scientific organizations about the public implications of scientific research.

This is of course the committee's concern today so I know I am speaking about something to which you are sympathetic. But it seems to me healthy for the scientific community, and for the NSF, to understand that it must study and assist in the promulgation of the implications of the scientific work in which it is engaged.

As for your question about scientific education, I am not familiar with that program. If it is education for scientists, I know there is a lot of education for scientists. If it is education for citizens——

Mr. THORNTON. It is undergraduate programs for developing scientific knowledge among undergraduate students.

Dr. STONE. I am for such programs, but I consider them redundant and ephemeral compared to the things we are talking about.

Turning to your committee report, you said:

> Nonetheless, both the committee and the NSF are concerned about the possible use of NSF funds to encourage the promulgation by special interest groups of their already determined positions. Both recognize the difficulty of establishing criteria for grants and awards which will distinguish such activities from those that will desirably enhance the public understanding of policy issues involving science and technology or contribute to the effective resolution of such issues.

That is well stated. But it seems to me that supporting advocacy groups does desirably enhance the public understanding of policy issues and does contribute to the effective resolution of these issues.

As a Congressman, you understand, Mr. Chairman, that you find the truth and right in the conflict between views. Without a balance——

Mr. THORNTON. Not always.

Dr. STONE. Not always. But without a balanced conflict, you would not get the balance of advice and political pressure, even, that the issues deserve. So if, on the one hand, we let private groups who are not supervised by the NSF, and not supervised by this committee, engage in any kind of presentations they want, at public expense—to 50 percent—and then if, on the other hand, we suppress the public spending to the level where it is teaching undergraduates rather than supporting the champions of the public citizens movement, it seems to me there can only be an imbalance which is undesirable for the country.

I want to emphasize that we know the science for citizens program could do many other good things that are much less controversial. But I want to say we do not feel that any form of it is a ripoff by public-interest groups.

This issue of recombinant DNA has thrust a scientific controversy into the public domain as never before and although the participants in that controversy have not always found it easy to live with, I think the net result is that the public is far better informed about the potential and the dangers of biomedical research than they ever have been before.

Some people don't think that is a good thing. I, however, think it is an extraordinarily beneficial thing. The point also should be made that the public is already involved in the determination of research priorities since much biomedical research is funded through public moneys and budgets are decided by Congress and it is my understanding that at least some Members of Congress hear from their constituents about how moneys should be spent.

Mr. THORNTON. Or should not be spent.

Mr. MCGOWAN. Or should not be spent. A key issue here is the value of conflict and an informed debate. It is so important in the scientific community that comprehensive rules strictly adhered to have been developed to handle this disagreement and controversy.

A conflict is resolved by the addition of new information, most frequently information that would not have been obtained had the conflict not arisen and the differences been explored. So it is I think with controversies that occur in relationship between science and public policy and open debate is, I think, the closest thing to resolution of conflict that we need.

On the other hand acrimonious debate which disrupts the delicate fabric, name calling, making a political issue out of a disagreement, is not the way to clarify the issue and lead to informed public policy.

If the scientific community is to retain the confidence which it so far has been rather successful in retaining, I think that public awareness of scientific controversies and public awareness of science has to increase.

Therefore, as a proposal perhaps to focus discussion around, I would urge the creation of a national commission on biomedical research which would encourage discussion, which would develop an independent information service, which would convene public hearings around the country to encourage lay and professional citizens to question and comment, the development of educational materials which consider the implications of biomedical research and the initiation of international discussions for efforts to deal with these scientific problems cannot be limited by national boundaries.

This commission should be composed of responsible members of not only the scientific community but of the business, labor, and public sectors as well. Only by taking such bold steps, I think, will public confidence in the scientific enterprise continue.

Thank you, Mr. Chairman.

Mr. THORNTON. Thank you very much, Mr. McGowan, for a very splendid summary of a thoroughly prepared and documented paper. Our next witness is Mr. Norman Wengert, professor of political science, Colorado State University, and member of the Wisconsin bar.

I want to welcome you, sir. It is a real pleasure to have a scientist-lawyer appearing before our subcommittee.

we on the campuses have really not resolved the question of scientific education for the nonscientifically trained person.

At Colorado State University, we require one course in science for most students not majoring in a science. This is an introductory course. As a scientific colleague of mine once said, we never get a chance to tell the students about the misinformation we gave them in the introductory course.

In a scientific age, I think we need to find some way of exposing students more rigorously to the policy dimensions of science, as against the processes and techniques of science in a particular field, such as physics, or chemistry.

I have a bias here because of a course I teach in environmental policy in which one of the thrusts is to make undergraduates skeptical of the information which they mave have received in their scientific courses. The students are from many different disciplines. It is amazing to me how at first some are very hostile to suggestions that they be critical of what they have been told in chemistry, in biology and in other courses. Perhaps we all have an inner need to regard knowledge as truth rather than as part of a continuing process of growth.

To develop an analytical attitude in students is a very real challenge to all of us who are teachers.

Mr. THORNTON. I thank you for that observation because I think that whenever knowledge is equated with faith or dogma, you run into difficulties. It seems to me it is always necessary to have a tolerance of viewpoints. Scientific fact itself is not determining with precision that a particular event happens but rather than that the events cluster within an area which you can describe as a range of high probability.

Some things are much more probable than others.

Dr. WENGERT. Let me elaborate with an experience I had a few years ago. I used as a book of readings a set of essays taken from the "Bulletin of Atomic Scientists." One of these included a statement that mankind is best suited to the nomadic way of life. I proceeded to take that as a theme for analysis, and pointed out the life expectancy of most nomads and some of the things we would have to give up if we went back to a nomadic way of life.

The students bristled because of my attack on this article, written by distinguished scientists. Fortunately, the next article was by a Russian scientist who, consistent with Marxist dogma, said that the problems of the environment were not problems of production or of science, but of who controlled science. In the Soviet system he asserted, "the people" and not capitalists controlled scientific endeavor and hence there was no abuse of the environment.

I was able to show through the juxtaposition of these two articles that even the most detached scientist approaches his job with certain biases, certain value commitments. Thus I would urge that citizens need to develop a degree of skepticism. To encourage such attitudes is a real challenge.

As specialization increases, the problem becomes that much more complex. A former dean of mine, a physicist, commented that if 100 physicists were assembled in a room, only groups of 70 could talk to each other because of high specialization.

So citizen education at some point becomes a kind of cliche unless we really deal with particular policy issues in that education. Money

dom of Information Act is in that discretion, represent an important part of how we deal with science and scientists.

The concept of replication, so important to the scientific method, really rests upon shared information. But again we have to recognize the problems of whether and when to expose an idea to "sunshine."

Timing is important. Premature release of ideas, theories or experimental data can be embarrassing and even dysfunctional, and could result in a particular idea being idea being prematurely rejected.

Sunshine and openness rest on assumptions with respect to the capacity of both the public and the professionals to understand and interpret. The frequent failure of the news media to communicate research theories or findings effectively, often because of oversimplification, can be the cause of public misunderstanding.

Research findings with respect to a possible cancer breakthrough may be headlined as being an accomplished fact.

This kind of problem is very difficult, especially for the smaller newspapers. Papers like the Denver *Post* or the Washington *Post* or the New York *Times* can afford to have specialists dealing with scientific subjects. But smaller papers may not have the competence to make proper judgments.

This is a very real problem, leading into the next topic: press releases, public meetings and hearings. Here, too, there are problems that we can't avoid, since press releases, meetings, and hearings can also be manipulated.

Many years ago, the topic of litigation would not have been considered in this kind of hearing. The possibility for litigation, which has been referred to several times this morning, intrigues me as a lawyer because it reflects the fact that the courts have enlarged the doctrine of "standing to sue" so public interest groups can get into court.

When I was in law school the primary basis for getting into court was an economic interest. That is no longer the case, and it is encouraging that lawyers, who are probably among the most conservative groups in our society, have opened up that route for a kind of participation.

But litigation is expensive. An outstanding environmental lawyer has said that an environmental suit can cost $50,000 to $100,000.

The Colorado Open Space Council, an environmental public interest group, has had to resort to garage sales to try to raise money. This means they hardly can afford to fight very many battles in the courts.

I think the committee's concern for how these groups get financing is an important concern. I would suggest that the problem of dealing with it will not be any different than the problem Congress has already dealt with in financing political parties.

What do you do about the minority parties? What do you do about the people who want to run for the fun of running? How do you handle them? This is not, as you well know, a simple problem.

Finally I want to refer briefly to the omsbudsman concept, a concept which has been experimented with by several States. I think the record needs some analysis as to why experiences of States have been unimpressive—Oregon abolished its omsbudsman perhaps for economic reasons.

I had the privilege last summer of participating in a 6 months study of powerplant siting sponsored by the Western Interstate Nuclear

There are townhall meetings. There are lecturers going around trying into being, "public" then being defined any number of ways. I tried committees. As we find the different Government agencies trying to respond to the pressure of the moment, we find all of these things coming into being "public" then being defined any number of ways. I tried to put this together in a schematic presentation which you have before you.

I needed that type of guidance myself as to who were the actors, what decisionmakers we are talking about, what types of public participation are available, what are the media the public might use and so on. I think it is very important to recognize that what we are undertaking here is not something from which we can depart, believing that we are going to supply an answer.

We can merely set something in motion, something that should be appraised over the coming months, a point which Jerry made very well. In a way, this is a major sociological experiment. I would hope somebody would see the opportunities to understand the pressures now upon social science to go after this total concept of public participation.

What has been happening here and how it is going to evolve there is a need for questions about this. It is a very important thing.

To what end has research been supported? That is a new facet to research in this country. To mention some of the ways that we find ourselves in some difficulty: Two recent actions by parts of the Government should concern this committee. One is a statement that the Government has 25,000 Government workers who now have $9 million worth of projection and movie-making equipment and a budget of $500 million for making movies.

We are going to clean this up because the Government does not need this amount of public relations. Somewhere buried in that public relations are some extremely fine efforts to educate the public.

True, some of this footage is for selling an agency but many of the agencies have played a fairly responsible role in developing good material for high schools and colleges and for other types of exposure. The space ageney has been a phenomenal thing in bringing the average citizen to the point where he can understand any of that.

That is quite an achievement.

Second. There is a requirement for review to better evaluate and weed out useless Federal advisory committees. This poses some problems because you do have people, responsible scientists and others, playing roles on committees, many times at some sacrifice in their personal lives. To suddenly have the committee labeled "useless" does not help further public participation. I feel quite happy about other things.

There is evidence of the awareness of the problem you have here. There is evidence of Government agencies doing a positive thing. ERDA now has a simulator that simulates an energy/environment relationship that is available at 72 different cities. If you have not seen it, I would recommend it. It is something in which the citizen can participate in making decisions about how you change certain types of energies.

As he sits there and watches, it rapidly dispels some of his misconceptions about energy. And it establishes in his mind the complexity of this problem with which others are trying to deal.

objectives. We must work toward that end as the limited resources decrease while the individual appetites for the many benefits increase.

Finally sooner or later there must be a public interest that is over and above the interest of individuals and served by the full recognition of technology through participation of that informed public in our decisionmaking processes.

Thank you, sir.

Mr. THORNTON. Thank you very much, Dr. Trumbull. We appreciate that excellent statement.

Our next witness will tell us about public participation and some European experiments. We are very pleased to have with us Ms. Dorothy Nelkin, who is in the program on science, technology, and society at Cornell University.

STATEMENT OF DOROTHY NELKIN, PROGRAM ON SCIENCE, TECHNOLOGY AND SOCIETY, CORNELL UNIVERSITY

Ms. NELKIN. I am pleased to take part on this discussion. I strongly feel that part of the tension over recombitant DNA research reflects a much more general concern about authority and expertise in this country and also about how to employ science and technology on an ever-increasing scale without departing from democratic ideals.

I agree with Mr. Trumbull that we need social innovation in this area. In particular, we must seek ways to channel demands for accountability into participatory mechanisms. Thus, I thought it might lend some insight to look at these issues in a comparative context.

I would like to share some research material on recent efforts to broaden public involvement in science and technology policy in several European countries: Sweden, the Netherlands, and Austria. These three governments initiated experiments in several areas but especially in response to the nuclear protest as nuclear power became a symbol for public concerns about bureaucratic centralization and the declining role of the citizen. I will briefly describe these experiments and suggest some implications that might be useful in the U.S. context. A more detailed analysis is available in a book to be published by Sage Publications this fall.

In the summer of 1974, the Swedish Government sponsored some experiments in public education concerning energy policy. The mechanism exists in Sweden in the "study circles," a system of study groups normally used for adult education. Several thousand groups with about 15 members each were convened to discuss Government energy policies. About 80,000 people participated in groups each meeting for at least 10 hours.

The Government gave the sponsoring organizations funds to hire their own experts and to develop material that would reflect the social and political interests of their members. The Swedish officials expected the public involvement would create more favorable attitudes toward nuclear power but surveys suggested that prior commitments persisted with even some increase in uncertainty and confusion. Yet there was some abatement of the more hostile antinuclear activity.

The Dutch Government responded in a somewhat different way to the antinuclear protest. In September 1972 the Minister of Physical Planning set up a system in which all plans for physical planning are

Furthermore the three governments were especially sensitive to criticism because of the delicate balance of power in their parliaments. Thus public involvement was perceived both as a means to implement technology policies and to reinforce political stability. The experiments differed in the extent to which they actually allowed for public influence and a voice for the opposition to existing policies. The Netherlands plan clearly has more opportunities for influence at an earlier stage than the Swedish system.

It is too early to evaluate these experiments, but the comparisons suggest several points. First, regardless of their technical nature, policies concerning science and technology are intrinsically controversial and more technical information in itself is not sufficient to change public attitudes or reduce conflict. A participatory process that realistically confronts difficult choices will not avoid conflict but will bring better focus to the issues of concern to the public and perhaps reduce hostility and polarization.

Second, expertise is a crucial political resource. If an open decisionmaking process is to be effective, and participation more than a symbolic exercise, there must be means to improve public access to technical expertise.

Finally, the response to participatory demands must vary according to the values one wishes to maximize. A major concern is that greater public involvement may further encumber efficient decisionmaking. The importance of an enlightened public and the articulation of diverse values that may emerge from a participatory process must be weighed against the urgency of implementing specific programs. The participatory experiments proceeded with cautious enthusiasm mixed with fear about their implications for decisionmaking and for existing representative institutions. But the participatory impulse has been contagious, spreading from one sector to another. As we know today, even the question of basic research is no longer immune as recent events in the recombinant DNA dispute suggest.

Thank you.

Mr. THORNTON. I want to thank you, Ms. Nelkin, for a very excellent summary of your statement. As you related the experiences of other countries, the closest parallel that I could think of in this country was the great decisions courses and seminars and policy groups which are organized in some areas of the country.

I am not sure if those are organized in each of your areas. But these are attempts to involve citizen participation in discussion of our foreign policy issues. I don't know exactly how that works or if it is a parallel to the Swedish system.

Dr. WENGERT. I was going to suggest two other examples which you might find of interest. The term "policy education" has developed since the end of World War II in association with the Cooperative Extension Service.

Performance in various states has varied, but two examples (not science policy), were remarkable in their success. The Iowa Extension Service, right after World War II, decided to carry out policy education for citizens of Iowa on two issues.

One was school consolidation, which in the Midwest was a very hot issue; the other was on international relations. This, too, was a hot issue because the Midwest had been a center of isolationism. The Iowa

Mr. McGowan. I would like to make a comment on both what Ms. Nelkin said and on what Dr. Wengert said because there is a danger first of all in thinking that there is one public and that when you say involve the public you are talking about a single thing. In fact, there are many publics and the fact that there are many publics and most of them are not and cannot be part of the scientific elite to which you referred, Mr. Chairman, I agree.

There is something a tendency in the scientific community to say well, this group X cannot understand how to make decisions unless they are brought to a given level of education. When you ask them what that level of education is, it more often turns out to be such as to include them in the scientific elite.

Mr. Thornton. So that they agree.

Mr. McGowan. That is right. There is also a tendency to think that education only works if it resolves conflicts and people end up the educational process by agreeing. We have to remember always that these are political and moral decisions that we are asking people to make and there is always, I hope, going to be a wide diversity in the decisions people are going to come to in this country.

If we ever try to get unanimity on any issue that has a political or moral basis, we are in serious trouble. There is a tendency in the scientific community to shy away from the press because we think they always oversimplify.

They will write headlines. Well, that is what newspapers do. You cannot write a headline on a scientific story that any two scientists will agree as accurate. If you insist on that, that means not dealing with the press.

That is a serious mistake because it is the press that provides the people with most of their information. I think that the tendency within the scientific community is to regard a scientist who deals with the press as guilty ego gratification and of trying to promote his or her own cause I think has to be combatted. I think it should be required of scientists to at least to a certain extent deal substantively with the press and learn the rules of the press.

We expect the press to learn the rules of science. But we don't as scientists understand the rules of the press which means for example, that with rare exceptions, they will never check with a story with you. They are writing a news story, not a research article. I think just as there should be courses for the press on how science works, I think there ought to be courses for scientists on how the press works.

Mr. Thornton. Thank you very much for that very fine statement. Dr. Trumbull?

Dr. Trumbull. I would like to return to a comment made by Ms. Nelkin and Dr. Wengert. There was a foundering when there was going to be technical terminology. The question is how do you come back to information? What is your medium to help the public understand the situation better?

Dr. Wengert gave part of an answer when he indicated the use of extension services for implementing these problems.

There you are taking advantage of a relationship already established between some educators and the public. The little example I used of the ERDA energy environmental simulator, actually that is what they have done.

Unlike a totalitarian regime or a dogmatic viewpoint, it has some flexibility and does not break apart when its structure is challenged severely.

Dr. Stone?

Dr. STONE. It seems to me that all three branches of Government, and the fourth estate, all work on what is really an advocacy system. The most respected branch of Government, the judicial branch, has two advocates which the judge decides between.

The executive branch is dominated by public choice between two political parties. In Congress, the debates on the floor go forward between champions of the different points of view.

Also, as was mentioned earlier, the press will not report things unless there is some kind of conflict. Even that system works on advocacy. As a result the public will not pay attention to anything unless it involves a conflict that it feels is of sufficient proportions to merit attention.

Thus, it seems to me the committee would have to decide that the important thing is to strengthen an advocacy system already deeply embedded in our whole way of doing business in the Government.

Therefore the phrase "advocacy groups" should not be used as a pejorative term but these groups should be looked upon as a structural element to be strengthened in what is inevitably going to be an advocacy process anyway.

Mr. THORNTON. That is certainly acceptable as a means of focusing attention on issues provided that we do not err in thinking that the outcome of such a procedure is going to disclose scientific truth.

Dr. STONE. I think that is quite right. We would be wrong to say that all we should have is advocacy.

On nuclear power and recombinant DNA, FAS found itself the only group putting out information that tried to be unbiased to both sides. Because our 7,000 scientists were not agreed on this issue, we were forced to keep our statements especially balanced and to try to explain the different points of view on both sides.

These statements were received with unexpected enthusiasm because so many people had already chosen sides on the issues and because we write on a 30-day basis and so we prepared our statements with a short leadtime and they appeared while the issue was ripe.

More authoritative groups like the Ford study, and others, took a year and a half to address some of these questions. By the time they were done the issue was largely decided. President Carter received their report on the eve of his announcement of what it was he wanted to do.

My conclusion is that the more "authoritative" groups who want to put forward more dispassionate statements, and want to do something better than advocacy, have got to get with it and move more readily.

Too often, they will find the conclusions come out after the political process has concluded on the issue. Therefore besides strengthening advocacy, Mr. Chairman, you should be prodding these groups that want to take a nonadvocacy role into putting forward their conclusions while there is still controversy.

Mr. THORNTON. We have a very interesting combination here of this view which you have articulated and that of Dr. Wengert who relied upon Mill's definitions of science and made the point that when two

Controversy and conflict within the scientific community does give rise to additional questions for which experiments can be done, or at least some calculations can be made. Thus, conflict is very important for the pursuit of what we would like to call the truth. I would also like to make a point that we tend to think that the education of scientists stops after graduate school and that it in any case should be limited to the technical field in which they are experienced.

I think that is a faulty notion and one of the benefits to me of the science for citizens program is the education of scientists, bringing them into contact with real public policy issues with all of the acrimony that sometimes surrounds that. That is a very important part of the education of the scientists.

Mr. THORNTON. Thank you, Mr. McGowan. Dr. Trumbull?

Dr. TRUMBULL. I would like to follow-up on separating the scientific content from other decisionmaking. We are going through a process, I believe, today and I guess most of my hours are devoted to doing the educating that Alan is talking about.

Jeremy talks to about 7,000 scientists who have this concern about how their science is used in the public. How we get them to this point is one of our concerns.

We have gone through this rather extensively with programs, with our journal, our magazine. We have tried to present both sides of issues and to educate them about this growing world as most of us well know. AAAS went into this public understanding of science 5 or 6 years ago.

They have problems because people write in and say if that is what you are going to do, I don't want to be a member any more. If you are going to put out a journal that contains scientific data I can use in my research, fine. If you are going to go down this sociopolitical road, here is your membership.

This is so in the scientific world today. There are many things in the training of a scientist which are counter to the involvement in public decisionmaking or recognizing the potential impact of their research upon the economy or the citizenry.

Mr. THORNTON. I would like to turn this discussion now toward the specific which pulls us all together here at this time, namely how this impacts on the question of DNA recombinant research. We are told that the results of DNA research, what can and can't be learned, what can and can't be achieved, what may be done and may not be done are questions for which there is as yet no answer. There is a great deal of speculation about what may result from different research procedures.

And yet we in Congress are being called upon right now to make some decisions with regard to what research will be allowed, or more basically, shall we make a decision allowing and disallowing certain types of research.

And further, we may be asked to make decisions on what type of commercial application of that research will be allowed and what will not be allowed?

We are in recess for 1 minute.

[Brief recess.]

Mr. THORNTON. Back on the record.

So, the real issue is what is the long-term benefit and what is the addition to basic knowledge that comes about as a result of recombinant techniques. That is the question, not the short-term benefits. I do not think they are there. I think as we increasingly look at it, we are going to find that they are not there.

Therefore, rushing into discussions as to how this research is going to be regulated, if at all, I think could yield unsatisfactory results which could be harmful to the scientific community as well as to the public. Hastily deciding only to regulate certain kinds of research is a mistake.

Hastily deciding to overregulate all kinds of research is a serious mistake. I think a lot more discussion and a lot more public education has to go on before we can adequately make a decision.

I do think that in the interim, the strictest caution has to be exerted in the conduct of the research.

Dr. STONE. Mr. Chairman?

Mr. THORNTON. Dr. Stone.

Dr. STONE. I disagree with a number of points there. In the first place, while I think it is true that the basic results of recombinant DNA experience are contributions to basic knowledge, which may only at some future time provide benefits, I do not think that people have tried to sell it in any other way.

I think there has been talk about possible near-term benefits, but I think for the most part, scientists have been fairly candid about the fact that the promise was very great, but not a short-run promise.

But over and above that, I would disagree with any certainty that there are not some short-range benefits because it is in the nature of science that one does not know how one is going to make use of the tools that come up. I would not be as dogmatic as Alan may seem to be about the short-run benefits.

We do not really know. I would not agree that we have time to wait before passing some legislation. Rather I would argue that certain legislation goals are forced upon us. For example, I do not think it is right to regulate scientists in academic laboratories and leave industrial firms uncontrolled by law simply because they do not take the grants from the agency that is handing down the regulations to the individual scientists. This is something that has to be repaired at the outset.

It seems that if there is any problem in recombinant DNA, it has to be addressed rather soon so we do not have high school students and teachers doing experiments that may be risky, without having some web of regulations and restraints that govern this process.

I do not think you have to worry about overly hasty regulations because the legislative process is, after all, a sequential one. Laws that do not work out can be changed. Indeed, I think the scientists have been concerned that the field would change so rapidly that the regulations and the laws could not keep up with it.

Therefore it may be wise not to be overly detailed. But if there are problems, I think they are problems that have to be handled promptly.

Mr. THORNTON. That is also an argument for not putting in place mistaken provisions of law, the fact that it does take time to correct them.

Dr. STONE. Of course, it is true that we should not do mistaken or "hasty" things. But we have to handle them as best as we can.

Dr. TRUMBULL. No; I do not think so. This is a very important point you raise. If you decide that you are not going to take advantage of their willingness to play this role and say "No, we do not trust you," you could do quite a disfavor to further development of science, not only in terms of what people will be undertaking, but their willingness to face these problems openly.

I think you have an issue there that people have not recognized. This is not the time to tell the scientists you do not trust them, but to take advantage of those who are trying to right the situation.

Mr. THORNTON. I would like to underscore what you just said. That is very appropriate.

Dr. STONE. Mr. Chairman, were you not asking the opposite question—whether scientists were going to conclude that having raised the issue, they no longer trusted society to deal with the issue?

Dr. TRUMBULL. Every now and then, I think we are advancing very rapidly in science and we are having a lot of our problems arising primarily because of our greater sensitivity to things. We are going to get very concerned now about mothers' milk.

We can do this ad nauseam if we do not remember that this has been going on for some time. Mother Nature has been doing this DNA research and making products on her own over time. You have had drastic changes in species and so on without catastrophic eventualities.

Mr. THORNTON. An interesting theory was presented to us by a scientist from Rutgers, Dr. Pieczenik, to the effect that the same scientific systemology which applies to heredity on a gross scale during selection of genetic information in crossbreeding of plants and so forth might also apply within the DNA molecule and that certain combinations would be rejected that might not be possible to recombine or to effectively make certain things happen because of rules that are not yet understood.

Dr. TRUMBULL. I think that is a point well taken. I do not want us to get overly scared. You are going to find many things because our techniques for measurement are getting better and better. You cannot keep getting the American people upset over each one of the findings.

Ms. NELKIN. Let me try to respond to part of your earlier question to Dr. Trumbull. The scientists' effort to act responsibly by calling the Asilomay conference was based on the assumption that this would help to establish guidelines monitored and sponsored by the scientific community.

This is related to the question of legislation and public participation of concern at this hearing. If scientists had predicted that Congress, or the Cambridge City Council, or the Citizens Board in Michigan would get involved in recombinant DNA research as a public issue and that the question of safety would have gone beyond the scientific community and the problem of educating the scientific community about ways to deal with pathogenic materials, I guess they might have thought twice before they wrote their letter to Science magazine. Isn't that what you were trying to get at, Mr. Chairman?

Mr. MCGOWAN. As a matter of fact, Dr. Berg is on record as saying he is not sure whether he would do it again, given the controversy that has erupted over it. It is definitely a point that if scientists do it it has to be regarded as a legitimate activity within the scientific community.

This thing did not start in 1973. It started in 1971 and it was a phone call from a scientist not doing recombinant DNA techniques who called the attention of the scientist who was doing it to the fact that this was a serious issue.

I don't think that we can rest assured as Jeremy stated rather blithely and without awareness of some of the potential difficulties, that there are going to be enough responsible scientists.

I think that will be true if there is some mechanism for the public or at least the larger scientific community to be aware of what is going on in each lab. That must include industrial laboratories.

Dr. STONE. It is in the nature of the scientific endeavor that there is a great deal of communication between scientists about their results. A scientist gets the credit for his result only by making it public.

There is therefore a great temptation to spread this knowledge around. Second, increasingly we do find science going forward on a broad front in many different countries at the same time. It is rare that one researcher is as much as 6 months to 2 years ahead of the rest. People know pretty much about what is going on.

Further the higher the scientists rise, the more they know about what is going on in their field and, also the more socially responsible they seem to become. For example, we have 1 percent of the scientific community. We are about as big as Common Cause is for citizens in general. But we have half the Nobel Prize winners in the country sponsoring our organization.

So at the top, where they may not be personally engaged in the relevant research they are still aware of who is making the important advances, and they can be expected to blow the whistle on these important dangers, should they come up.

True, an experiment today might destroy us all tomorrow, there would be real problems. But we have not seen things get quite that tense. Within reasonable time limits, I believe, we can depend upon scientists reporting, in one vehicle or another, that questionable important things are going on.

I also think that, to the extent one tries to get the public to help fulfill this function, they would not in any case, understand sufficiently well what is going on in the laboratory to provide an early warning. So I am not sure that Alan's suggestion for improving the situation is actually appropriate.

Mr. THORNTON. Looking down the road from a policy standpoint, let's suppose the Congress puts in place an agency which can either be a new one or an arm of an existing agency whose job it is to regulate to some degree DNA research at least as far as industry is concerned and the commercialization of products.

Then would it be your thought, any member of the panel, that the next issue of science research, which does cause some people to think that this might be a dangerous area of research should also be assigned to that agency and provide that it will be regulated by similar rules?

Is there a danger in developing an institution to regulate DNA recombinant research that the next time an issue comes along which raises some of the same questions which are raised here, that it just automatically will be put into the mechanism that is devised for this DNA issue?

Dr. STONE. When you say put into——

Mr. THORNTON. I mean regulated. The next time a public policy debate emerges on a question of scientific research, the availability

That is one of the things that I am concerned about. If we rush into regulation of recombinant DNA and do not include other equally hazardous research of a biological nature, then are we by implication saying that that other research is not deserving of regulation, but recombinant DNA, just because of—it has received public attention, is?

That is a serious issue and has to be considered. There are many scientists who feel very strongly that the research ought to go on, who want the regulations put into the force of law to avoid the problem of one scientist looking over the shoulder of another and telling that scientist you should abide by the guidelines. The point that Dorothy mentioned a minute ago, that is of the tremendously difficult issues that surround human genetic engineering for example, where we don't have a history, I think if the recombinant DNA has taught us anything, it is that we need to consider the issues of genetic engineering now before we have the potential to do it, not as we are about to accomplish that experiment or act.

Then it is going to result in serious acrimony and not careful attention paid to all of the ethical questions concerned.

Mr. THORNTON. In an effort to keep up with an announced schedule, I would like to allow each member of the panel a few seconds to make any comments which you may feel are appropriate at this time and further to invite each of you to express your willingness to answer questions which may be submitted to you in writing as we go forward with these hearings.

Are each of you willing to respond to such questions?

Dr. STONE. Yes.

Dr. WENGERT. Absolutely.

Dr. TRUMBULL. Yes.

Ms. NELKIN. Certainly.

Mr. THORNTON. Very good. I will take a reverse rundown. Ms. Nelkin?

Ms. NELKIN. I would simply like to reinforce my statement that this is an area where we need social innovation to create better forms of public understanding and appropriate means of accountability. As in any innovation we must expect to flounder, but the issue of public participation deserves direct attention. It is not just a means to "sell" nuclear power, recombinant DNA, or other technologies, but an important part of maintaining democracy in a technological society.

Mr. THORNTON. Dr. Trumbull?

Dr. TRUMBULL. As a followup of your last topic as well as an expression of a total picture, I believe much more can be done within the present agencies we have supporting research to assure that they pay more attention to what the eventualities are of the research they are supporting.

We have a tendency in Government agencies to have a budget to spend and to spend our time spending it. I think we could devise a system for bringing that under control in present agencies in a productive way. It would preclude the overreacting to individual emergencies and then setting up individual agencies to control DNA and whatever else happens.

Mr. THORNTON. Thank you, sir.

Dr. Wengert?

SCIENCE POLICY IMPLICATIONS OF DNA RECOMBINANT MOLECULE RESEARCH

THURSDAY, MAY 5, 1977

HOUSE OF REPRESENTATIVES,
COMMITTEE ON SCIENCE AND TECHNOLOGY,
SUBCOMMITTEE ON SCIENCE, RESEARCH AND TECHNOLOGY,
Washington, D.C.

The subcommittee met, pursuant to adjournment, at 10 a.m., in room 2325, Rayburn House Office Building, Hon. Ray Thornton, chairman of the subcommittee, presiding.

Mr. THORNTON. The hearing will come to order.

Good morning.

In our hearings this morning we continue our examination of the science policy implications of DNA recombinant molecule research. As we have gone through these hearings we have found both our witnesses and ourselves frequently referring to the potential risks and benefits of this research. Consequently one of the questions uppermost in our minds is, "How can you weigh these risks and benefits, particularly when many of the risks and benefits are highly speculative?"

We have with us today four witnesses who are expert in the quantification of risks and benefits.

Each of us has studied this question. We will discuss with you whether it is possible to apply some kind of risk-benefit analysis to the recombinant DNA debate, and what use such analysis could be to us as we seek to resolve the other issues which involve science policy, science and technology.

I will begin by repeating a question to our panelists. This is the same question which was posed when you were invited to appear before us:

What are the utility and limitations of risk benefit analysis techniques in decisions involving science and technology?

We will ask each of you to reflect on that question.

And to begin our hearing this morning, and help us answer this question, I first would like to recognize Dr. Richard Wilson, Professor of Physics at Harvard University.

Dr. Wilson, I have had an opportunity to read your excellent prepared statement. Without objection that statement can be made part of the record verbatim, and then I will ask you to highlight and summarize it as you choose.

[Biographical sketch and complete statement of Dr. Richard Wilson follows:]

Proposed Testimony for House Subcommittee

Chairman Representative Thornton

10:00 a.m.

Thursday, May 5, 1977

Rayburn House Office Building, Room 2325

My name is Richard Wilson. I am a professor of physics at Harvard University. I have recently concerned myself with comparative risk analyses.

I would like to touch upon three topics. Firstly, I will describe issues where risk benefit analysis should give answers of direct use to decision makers; secondly, I will explain how bad presentation of these analyses can make them lose credibility and thirdly, I want to describe cases where risk benefit analysis can illuminate a complex issue without suggesting a complete solution.

In cases where the risk is based upon experience, the reliability of calculating the risk is high. Over 50,000 Americans lose their lives on the road each year; the risk of death because one gets into a car and drives can be estimated and well. Each event is objective and definite.

The benefit to society of driving is obvious but hard to quantify. Given these it is easy to consider whether it is worth paying the cost of installing seat belts for example. It transpires that installing seat belts saves lives--at the rate of one life for every $5,000 in cost--surely a worthwhile figure.

low concentration we actually use. Once these assumptions are made, we can estimate the risk in each of these cases.

An easy way to do a risk benefit study is to compare risks of different ways of obtaining the same benefit. For the above cases, we can compare the risk of air pollution from sulphur oxides from burning coal, with the risk of a little more radiation from a nuclear plant. It is well known that on this count a nuclear plant is safer.

We can go further and ask how much we should be willing to pay to reduce the radiation or the sulphur oxides. Here we run again into the imponderable we met before. Clearly we can't pay more than the gross national product, and we'd prefer to have some money left for other things.

This is clearly a matter for the decision maker rather than for the analyst. But a good example of how a risk benefit analysis can be used was apparent in Connecticut a couple of years ago. Northeast Utilities proposed to burn oil with a 2% sulphur content instead of .5% sulphur content. They estimated the saving that would result--$100 million in reduced fuel adjustment charges. On the other hand, scientists from Brookhaven National Laboratory estimated that this would lead to about 30 extra deaths--mostly from bronchial ailments. The hearing board had a clear decision to make and chose to maintain the tough air quality standard.

All too often, the case is not presented to the decision maker so well; we all like playing God, but it is important to do so only on the last line of the report.

of the benefits. Hopefully the analysis will be written to highlight the imponderable which others will have decided according to the rules you make. Their decisions will be a little easier if the irrelevant factors are put out of the way by a responsible risk benefit analysis.

Challenge

Examples in Risk-Benefit Analysis

I count myself an environmentalist. Of course we are all environmentalists. But if you work for industry, the Army Corps of Engineers, or the Atomic Energy Commission, no one believes you. I don't work for any of these.

As civilization has progressed, man has congregated in large cities; at the same time economies of scale have demanded that industrial facilities become ever larger. Therefore, there is now potential for man-made catastrophes of unprecedented magnitude. Moreover, as civilization progresses we live longer, and want to live longer still. We want to make our lives ever safer. Accidents, and even natural disasters which were once dismissed as acts of God, are now considered to be under control. It's not that we deny the existence of God; it's just that there is no need to blame Him for our incompetence. However, only recently has any serious attempt been made to express risks in quantitative terms; and these attempts have not yet been understood by our decision-makers and politicians. I maintain that some of our decisions are crippled by this lack of understanding.

The largest potential catastrophe is probably the unrestrained use of nuclear power—and the worst hazard here is an outbreak of nuclear war. For this reason, among others, scientists in the U.S. and elsewhere who participated in development of the atomic bomb insisted on civilian control of the atom's development for peaceful purposes; nationally through the AEC, internationally through the IAEA.

Richard Wilson is a product of England's St. Paul's School and Christ Church, Oxford, which awarded his three college degrees. His academic appointments have included Oxford, Rochester, Stanford, and most recently Harvard, where he was named Professor in 1961. A Fellow of the American Physical Society and the New York and American Academies he serves as consultant to ERDA and the Nuclear Regulatory Agency. He's also served as consultant on nuclear power to Maine's Attorney General. Professor Wilson has been an editor of *Annals of Physics* since 1956.

From the outset, the AEC endeavored to have frank and open discussion of the risks to society—although like all human institutions it occasionally allowed its staff to slip from these ideals (though not so often as Ralph Nader is suggesting).

Many of the problems with the nuclear industry follow from this aim at open discussion of risks. First, the "public" does not understand risk analysis and consequently demands an elusive certainty. Recent disclosures from Washington suggest that even taxes are not as certain as I had been taught, so maybe only death is certain: Death *is* certain for the individual, but we hope it is not certain for society.

Risk

Man often takes risks, and always has. The purpose of risk-benefit analysis is not to stop man's taking risks; it is to help man to choose the risks he wishes to take.

The nuclear industry is not unique; risks are everywhere. I would like to illustrate the problems of risk benefit analysis by several simple examples. No complex calculations are needed at our present stage of knowledge. If you can count up to ten, multiply, and divide, that is enough. What I am doing, therefore, is presenting you with a do-it-yourself kit for risk-benefit calculations.

One problem is comparing diverse risks and diverse benefits—such as comparing apples and oranges. In energy matters, we can simplify by expressing all benefit in the common energy unit—kilowatt hour of electricity produced. This is only valid, of course, if all methods of producing electricity cost about the same. I did a very rough cut at this from available statistical tables two years ago; this was published as a letter (*1*) in *Physics Today* (I show an updated form in Table 1).

The preparation of such a table is, in principle, simple. We look up the statistics for the various causes of death, and divide by the energy produced. The *Statistical Abstract of the United States* (*2*) has tables for industrial accidents and energy use by type of resource. For air pollution, I take the paper by Lave and Seskin (*3*), and apply their correlation to a rough estimate of pollutants given by EPA; for coal mining black lung disease I could take the numbers of sufferers compiled by the Social Security Administration and assume that the sufferers, who are never cured, will suffer a premature death, but it is widely believed that these data *underestimate* incidence of this disease. Accordingly I took numbers outlined in an article in *The Sciences* (*4*). Although the numbers are no better than a factor of 10, no one has yet done much better; serious studies are under way in several places, however.

Reducing risk

We see clearly that some sources of energy are worse than others. Hydropower, which superficially seems benign, is quite bad. Now while we can use these data to guide us in selecting between fossil fuel and nuclear, for example, they don't tell us how much to spend on reducing risk. Maybe some of the entries in the table can be reduced almost to zero by a modest expense. Therefore, we usually make a risk-cost analysis whereby we compare costs of reducing a risk. It is important to realize that it is not worth calculating these numbers to better than one significant figure.

There are over 50,000 deaths due to automobile accidents in the U.S. every year. In an attempt to reduce the death toll, we now insist that all cars have seat belts and shoulder harnesses. Probably only one third of all travelers buckle their belts, but it is estimated that one third of the deaths could be avoided if everyone buckled his belt (*5*). We can make this estimate by comparing the death rate in all accidents to the death rate in those accidents where seat belts were used. Of course this omits consideration of many issues; are people who use seat belts safer drivers because they think about safety as they buckle, or are they more dangerous because they feel secure and more reckless?

In the U.S. we spend about $40 for factory-installed seat belts in nearly 10,000,000 automobiles per year for a total cost of $400,000,000. (From Sears-Roebuck I can buy belts for $29 and install them myself, but my time is worth something, too.) About 5000 lives are saved—at a cost of $80,000 per life. Injuries are also reduced, making the value

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Table 1. U.S. Deaths due to electricity generation
(1965 figures unless stated otherwise)

	Deaths/kWhr$_f$	Deaths/10^9 MWhr	Adjusted to allow for no. in accident
Fossil fuels			
Air pollution (undifferentiated)	3×10^{-9}	3000	
Coal mining			
Black-lung disease	1×10^{-9}	1000	
Accidents	6×10^{-11}	60	6000
Total	$\sim 4 \times 10^{-9}$	~4000	
Petroleum refining and oil-well accidents	7×10^{-12}	7	
Total	3×10^{-9}	3000	
Gas			
Main explosions (1971)	1×10^{-11}	10	40
Poisoning	3×10^{-11}	30	
Explosions and fires caused in homes	1×10^{-11}	10	
Total	5×10^{-11}	50	
LNG tank failures	10^{-12}	1	100
Hydroelectric dam failures			
One failure per 50 years as at Vaiont, Italy			
Direct deaths	3×10^{-10}	300	1,000,000
Indirect deaths	3×10^{-10}	300	
Miscellaneous small failures	2×10^{-11}	20	
Drownings (estimate)	1×10^{-11}	10	
Total (actual electricity)	$\sim 6 \times 10^{-10}$	~600	
Nuclear fuels (fission)			
Uranium mining cancers, if fuel used for:			
Breeder reactors	7×10^{-14}	0.07	
Light-water reactors	1.5×10^{-11}	15	
Uranium processing and fuel-fabrication accidents, if fuel used for:			
Breeder reactors	2×10^{-12}	2	
Light-water reactors	2×10^{-11}	20	
Radiation cancers from normal operation of 500 reactors and processing plants (0.1 mrem/yr.)	3×10^{-13}	0.03	
Potential reactor accidents: 1/30 yr. of WASH 740 severity by yr. 2000. (very pessimistic)			
Direct deaths	1×10^{-11}	10	
Extra cancers	1×10^{-12}	1	
Other indirect deaths	6×10^{-12}	6	
Total	$\sim 5 \times 10^{-11}$	~50	
Potential reactor accidents: Rasmussen study 1000 deaths in 10^5 reactor years, 1000 reactors			
Direct	3×10^{-13}	0.003	{3
Extra cancers	3×10^{-14}	0.0003	{0.3
Other indirect	2×10^{-13}	0.002	

per year, so long as no more than one person is involved (at risk) at a time. This would include black lung disease, radiation cancers, or asphyxiation by gas in a domestic kitchen. In each of these hazards, only one person dies at a time, and sometimes we add a pollutant and, by chance no one dies.

However, when an accident involves more than one person—say N persons—then such a comparison may no longer be valid, and I assume, as a guess, that a risk involving N people simultaneously is N^2 (not N) times as important as an accident involving one person. Thus a bus or airplane accident involving 100 people is as serious as 10,000, not merely 100, automobile accidents killing one person.

In calculating the cost per life of reducing a risk, I will multiply the deaths per kilowatt hour, and divide the cost per life to reduce the risk, an *extra* factor of N to obtain a cost figure "adjusted" to that for a risk involving only one person at a time.

Let me illustrate this by Figure 1; I here assume we can afford $1,000,000 to save a life; but to save 100 lives at once, we should pay $100 billion. In the graph, therefore, if a risk lies above the line, we should pay to avoid it; and if below the line, we might accept it. An example can make this clear.

Nuclear risk

In present U.S. nuclear reactors, the most serious potential accident is a loss of coolant accident. I do not want to go into detail about this, which is a lecture in itself, I will, however, point out that this is an accident which cannot be prevented by a basic physical principle, but needs reliable engineering, plus engineered safeguards that operate in the case of pipe failure. This fact makes the estimation of safety very difficult, and it is at this point where public criticism rightly has been focused. A study was being carried out for the AEC by Rasmussen (*11*) at the time this paper was first drafted. I took a peek at the preliminary output, and assumed that the results were known *before* administrators made their decisions, which was unfortunately not the case. I also assumed that the study was "correct" in spite of the reservations above. Of course it is in making such studies that the real work of risk-benefit analysis is done.

Rasmussen estimates that a loss of coolant accidents (LOCA) can occur, and the emergency core cooling system (ECCS) fails to operate, such that the whole core may melt down—once in every 17,000 reactor operating years. If and when this occurs, there will be no consequences to public health most of the time; the wind may blow radioactivity out to sea, and so forth. Once in 100 million reactor years 1000 people may die in an accident. Let me say that 1000 people will die every 100 million reactor years. This is small. The AEC has recently tightened its criteria for these emergency core cooling systems from the interim acceptance criteria of June 1971. How bad were the interim criteria? I do not know, but I can make a pessimistic guess that they resulted in a probability for a LOCA out of control 100 times worse. This corresponds, roughly, to the ECCS working none of the time. I don't think any serious critic was more pessimistic than that.

Then we can say that meeting the new criteria saves 1000/1,000,000 lives per reactor year. How much do the new criteria cost? Apart from an expensive hiatus caused by insisting on retrofitting, we meet them by making more fuel rods; roughly 25% more. The fuel fabrication cost is $6,000,000 per year for a 1000 MWe reactor, and it increases roughly proportional to the number of rods—$1,500,000 per year. In the letter of Reference 7, a slightly smaller number, $900,000 is suggested. The Boiling Water Reactor of General Electric Co. (BWR) was going to increase the number of rods for other reasons anyway, and the high-temperature gas-cooled reactor (HTGR) of General Atomic does not have this problem. Only half the reactors need be changed at an average cost of $750,000 per year. These crude figures, confirmed by testimony of industry personnel to the Joint Committee on Atomic Energy (*12*), suggest we will pay $750,000 per year to save 1/1000 lives; $750,000,000 per life

Discussion Paper

Comments Welcome

The Status of Risk-Benefit Analysis

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December 1976

Prepared under Contract No. 33-542-9018-2 for Brookhaven National Laboratory, Upton, NY, as part of the Biomedical and Environmental Assessment Division program, sponsored by the Division of Biomedical and Environmental Research, U.S. Energy Research and Development Administration under Contract No. E(30-1)-16.

Description and Limitations

Risk-benefit analysis is a generic term for techniques encompassing risk assessment and the inclusive evaluation of risks, costs, and benefits of alternative projects or policies. The risk-benefit analyst attempts to measure risks and benefits, to identify uncertainties and potential tradeoffs, and to present this information coherently to decision makers. Like other forms of policy analysis the steps in risk-benefit analysis include specifying objectives and goals for the project options, identifying constraints, defining the scope and limits for the analysis itself, and developing measures of the effectiveness of feasible alternatives. Ideally, these steps should be completed in conjunction with an accountable decision-maker, but in many cases the decision-maker is unknown to the analyst. In such cases poorly defined decision options or the selection of alternatives which are too limited to meet proposed objectives may result. These faults are shared by all forms of policy analysis, but because risk-benefit analyses are frequently controversial, the risk-benefit analyst must be particularly careful to state the assumptions and limitations of each assessment.

The principal task of the risk-benefit analyst is to express numerically, insofar as possible, the risks and benefits which are likely to result from project outcomes. Calculating these outcomes may require scientific procedures or simulation models to estimate the likelihood of an accident and its probable consequences. These consequences are first

a concept which cannot be avoided. Many of the difficult issues related to society's willingness to pay to prolong life have been discussed in References 3-6.

Recognizing that subjective value judgments are required in order to assign monetary values to costs and benefits, the risk-benefit analyst will not always attempt to arrive at a calculation of "net" benefits, but may choose to present risks and benefits in their respective units or categories. This leaves the decision-maker free to impose his own values or a range of values in aggregating risks, costs, and benefits. Thus risk-benefit analysis, in contrast to risk-cost-benefit analysis, will not necessarily arrive at a single number to represent the value of a project. Instead, a matrix of effects may be given including such disparate costs and benefits as lives lost, property damage, kilowatt-hours of electricity, and aesthetic losses. A "meticulous accounting" of like effects may avoid some of the obfuscation inherent in dealing with issues such as the identifiability of the life at risk or the voluntary/involuntary nature of a risk.[7]

Most of the disagreement over the usefulness of risk-benefit analyses derives from disputes over the methods used to aggregate risks and benefits. The most widely used measure for aggregating cost and benefit streams is the net present value:

$$NPV = \sum_{T=0}^{T} \frac{(B_t - C_t)}{(1 + r)^t}$$

where B_t and C_t are the benefit and cost in year t, respectively,

rate. It is for these cases that a negative discount rate has been suggested, but an explicit accounting in C_t is to be preferred. Uncertainties in these costs should also be handled in the numerator of the NPV formula, not in the discount rate itself. Economists inevitably dispute the choice of the specific discount rate to be used, e.g. the social rate of time preference or the prevailing interest rate. Except when a particular discount rate is specified by the decision maker, the NPV calculation should be repeated using several discount rates to ascertain the sensitivity of results.

The difficulty in agreeing on a discount rate is usually secondary to the problem of determining future cost and benefit streams. Uncertainties in long term costs and benefits may be large for time horizons up to T years, although frequently all alternatives will suffer from similar uncertainties. Because of uncertainty it has been suggested that we should not discount potentially large effects more than a generation in the future.[9] We believe these uncertainties should be reflected in the benefit and cost streams and not masked in the discount rate. Investigating questions of intertemporal equity and methods for dealing with uncertain outcomes are central problems of research, and their logic must be relentlessly pursued. Moreover, all forms of decision making must resolve these questions whether or not they are explicitly dealt with.

Risk-benefit analysis has been slow to develop, partly because of its multi-disciplinary nature and partly because

related issues in fields like architecture, decision analysis, economics, physics, engineering, chemistry, law, government, and medicine. Few questions were answered, but the hope was engendered that interdisciplinary approaches would lead to improvements in risk-benefit decision making.

Four years later a second conference was held at Asilomar, California to examine the state of the art. In the intervening years considerable work had been performed in diverse areas such as the reliability analysis of engineering systems, health effects assessment, economic approaches to life-saving, insurance protection for natural hazards, and the psychological perception of risks. From the 1975 conference[11] and from a survey of literature[12] it is evident that no coherent definition of risk-benefit analysis has emerged, owing to the breadth of subjects under study. Most recent effort has been in the area of risk assessment, less attention has been given to benefit assessment, and even less attention has been devoted to how decision makers should integrate this information into the political process.

Risk assessment can require expertise in several disciplines, since risks may originate from causes such as disease or natural hazards, from human errors or sabotage, or from hardware or equipment failures. For frequent risks the expected rate of occurrence may be calculated statistically from similar experience or predicted from models. Failure and reliability analyses for engineered systems may employ sophisticated event tree and fault tree methods such as those

folded in to estimate the overall magnitude of the risk. Although vast amounts of information are required and there are uncertainties in our current knowledge, consequence models can roughly estimate these risks. Refinements of our scientific understanding and of our ability to estimate such risks are needed to ensure that decisions and regulations are indeed reducing the most severe risks.

While decision makers readily appreciate the significance of mortality or morbidity estimates, it can be difficult to develop good measures for environmental losses such as damage to vegetation, recreational losses, and ecological or biosphere contamination. Indeed, it is not always necessary to assign dollar values to aesthetic or environmental losses, so long as the losses can be identified in appropriate categories. (The National Environmental Policy Act requires the consideration of alternatives in a cost-benefit framework, but Environmental Impact Statements usually only categorize like effects. Their major failing is that differences between the proposed alternatives are usually so small that the decision maker has no real choice. In addition, the voluminous amounts of information are often not adequately summarized so that meaningful comparisons can be made.)

Latent effects, which may not appear until 20 years after exposure in the case of some cancers or until the next generation in the case of mutations, pose severe problems. For example, if the depletion of atmospheric ozone continues, how should we assess the risk to succeeding generations? How

but it is not necessarily so for substantial additions to a generating system. Further, highly aggregated data is needed in many instances to measure health and other risks reliably. There can be difficulties in using these average costs in choosing among alternative technologies, especially when the geographic locations can be different. Economic theory makes a distinction between average and marginal costs, and analyses should properly utilize marginal costs. In studies evaluating energy technologies with common benefits the separation of the risk-benefit analysis into two separate parts, one national in scope and another regional or local, might well be appropriate. Otherwise it is hard to see where to bring in such important factors as the advantages of diversifying methods of electricity generation or advantages to the nation of energy independence. Ideally the benefit of an action should exceed the risk both for the nation as whole and for each significant region or political jurisdiction. Transfer payments, including taxes and the like, may be necessary to ensure that this is true. In the case of energy supply the separation of risk-benefit analyses into national considerations of the level of supply and regional considerations of particular sources might clarify present debates.

The literature on risk-benefit analysis is largely dominated by articles on how to perform aspects of an analysis or determine acceptable levels of risk, largely without reference to the benefits. Apparently it is easier to suggest how one might proceed in theory than it is to carry out practical analyses. In 1973 C.O. Muehlhause of the National Bureau of Standards was asked whether he could cite some

aid decision makers by making explicit the relationships between lives saved and dollars spent.

In general risk-benefit analyses which succeed are those which have been constructed to provide information on well-defined decisions with specific options. The analysis of Acton[21] uses surveys and decision analysis methods to rank several programs for treating heart attacks, including mobile coronary care units, for a town of 100,000 people. Terrill[22] compares two major sources of radiation, nuclear power plants and medical x-ray machines, and estimates the costs and benefits of reducing radiation doses from each. Kitabatake et al.[23] estimate the number of lives saved from a program of mass chest x-rays in Japan and compare this to the induced cancers. In each case it is clear which questions the analyst is attempting to answer and the tradeoffs in each are of like risks.

In contrast a very comprehensive analysis by Klarman[24] which measured many potential economic benefits of syphillis control programs was not examining well-defined decision options and thus would have been difficult to apply to a particular decision. Typically, in situations where projects invest in the well-being of people rather than purchasing capital goods, it is difficult to define the benefits or develop comparable alternatives. The analysis by Klarman offered considerable insight into the ramifications of a syphilis control program but was not directed to guiding choices among possible program objectives.

analysis is most often used to supplement deterministic calculations, but new means of incorporating probability distributions for uncertain outcomes and for assessing relative preferences among multi-attributed choices are beginning to be applied to decisions involving hazards. The analytical methods of decision analysis are providing useful tools for exploring the effects of uncertainty on project outcomes.[28,29] While these are techniques with great promise, they too can deal successfully only with well-defined questions. For example, a decision analysis comparing coal and nuclear fuels for an additional power plant in New York can not be readily extended to a choice between energy systems on a larger scale.[30] (We have mentioned earlier that choices of policy can depend significantly on the geographic scale considered for the particular decision.) Important "costs" may lie outside the defined scope of a risk-benefit analysis; the potential costs of legal liability were excluded explicitly in an analysis of a hypothetical decision to seed hurricanes.[31] Decision analysis methods can be used to incorporate probability distributions and expert judgments, to develop hierarchies among attributes, to discriminate between alternate strategies, and to point out significant information gaps. These methods may also be utilized for performing sensitivity analyses on parameters subject to variation or uncertainty.

As a rule, all costs which might affect the balance between risks and benefits should be identified and included. Implementation costs should not be overlooked. Analyses of

hearths, we would elect the latter. However, if the choice is between higher prices for energy and reduced risks, how do we choose? How do uncertainty and other factors affect our perceptions of risk situations? Lowrance has dealt admirably with many risk-benefit issues in his book, "Of Acceptable Risk: Science and the Determination of Safety."[34] There are no hard rules for equating risk and benefit tradeoffs, and when the numerous risk situations in society are considered the situation becomes most complex. Retrospective studies of the previously accepted levels of risk in our society may be a guide to understanding our past behavior,[35-37] but comparing predicted future risks to statistically determined past risks can be misleading, especially if the predicted risks are presented without corresponding information on their uncertainties.

Risk-benefit analyses usually calculate the probability of death per person exposed to a hazard. This omits from consideration one important feature of public concern: Whether an accident involving the potential death of 10,000 people at once is to be considered worse than 10,000 accidents involving one person.[37-39] In an extreme case society could not recover from 4 billion simultaneous deaths, even if such an accident occurred only once in 10,000 years. Such an event is clearly worse than the preventable deaths from cigarette smoking, which occur at the same average rate. Both the uncertainty of a risk and its magnitude increase the perceived risk, thus focusing public concern on low probability, high

formulae is very limited and their applicability has not been widely demonstrated. Empirical formulae may be useful for engineering design and as a basis for risk analyses,[44] but at present it is doubtful that rigorous formulae can be applied to public acceptability decisions.

Public perceptions of risks and benefits do not always coincide with the actual level of risk or benefit. People may choose to live on flood plains either because they misperceive the real risk of floods or because other constraints (job availability, family ties, etc.) make flood plains an acceptable place to live. Psychologists have suggested that people in groups are more willing to take uncertain and larger risks than individuals and that delayed or latent risks are more acceptable than immediate risks. Smoking is one good example. Studies of the many factors involved in risk taking may aid in understanding the implementation problems of risk-related programs.[45]

In many cases a risk may be acceptable if it is borne by the persons receiving the benefits and be unacceptable if those bearing most of the risk are not those receiving most of the benefits. We must emphasize that risk-benefit analysis is not equipped to judge the equity of the distribution of risks and benefits, but it can identify impacted groups. Many present risk-benefit analyses fail to clearly identify the groups who are to be impacted. Often in aggregating net costs and benefits this information is lost. Because some impacts are more certain and more important to the decision

has to discover ways of going beyond them. Solutions need to be found, especially if two persons' gut feelings differ. Surely, it is incumbent on someone whose gut feeling differs from a careful analysis to try to understand and explain the reason for that difference, so that the analysis may be improved.

From the point of view of public policy it would be desirable to know if standards should be designed to minimize the probable level of risk (minimizing the expected value) or to minimize the maximum harm (protecting against the catastrophe). Depending on the risk spectrum (probability vs. level of damage), these two possible criteria will lead to different choices, which can be distinguished by risk-benefit analysis. It is likely that other criteria for choices among alternatives should be applied for decisions involving more uncertainty or greater potential risks. Differences in costs, including benefits foregone, which will result from applying different decision rules need to be more clearly presented. Increased attention must also be devoted to finding methods for developing feasible alternatives and for identifying ways in which proposed alternatives may be modified to achieve better outcomes.

The concepts embodied in the phrases "as low as practicable," "best available technology," and "factor of safety" require baselines for judgment. Improvements in risk assessment should suggest how well these concepts work in practice and enable us to judge whether other regulatory schemes may reduce cumulative damages.[46] These expectations will not

or opponents of particular alternatives are not capable of pointing out the limitations of an analysis. Surely, if decision makers are capable of comprehending the complex scientific and technological decisions to be made, they are capable of recognizing the limitations of analytical methods. Holistic decision making is not precluded by using risk-benefit analysis. Careful risk-benefit studies subjected to open criticism are more likely to rationalize and clarify the decision process than they are to hinder or obscure it.

Conclusions

This has necessarily been a superficial survey of the developing field of risk-benefit analysis. In the past risk and benefit have usually been evaluated separately, and relatively few analyses have been presented in a format where risks have formally been balanced against benefits.

As we become aware of more and more sources of risk and of society's limited resources, the need for setting priorities, identifying constraints, and for preserving future options will increase. Inevitably decisions must be made, and therefore, refined tools for measuring and evaluating risks and benefits are needed. Thus far the techniques of risk-benefit analysis have had limited application and limited success, but the art is improving with experience. Further research is especially needed to improve our assessments of risks and benefits, to develop means for dealing with uncertainty, to identify feasible alternative options, and to

Acknowledgments[+]

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STATEMENT OF RICHARD WILSON, PHYSICS DEPARTMENT, HARVARD UNIVERSITY

Dr. WILSON. Thank you, Mr. Chairman.

For about 5 years, I have been engaged with comparative risk analysis, largely because undergraduates start asking questions and then professors in search of answers find themselves in new fields.

It seems to me there are some places where risk analysis works well, some places it probably doesn't.

I always try to give examples of places where one has tried in the past to apply risk analysis and, hopefully learn from that where to apply it in the future.

Clearly, when you try to establish the risk of automobile accidents we can very clearly say that 60,000 people a year die on the roads, and we can be pretty sure however hard we try we will not get it much below 40,000 next year.

We can calculate how many lives it would save if you installed seatbelts, because we can subdivide the risks into risks to people who have worn seatbelts and those who have not. A rough calculation shows installing a seatbelt saves lives, with a rough cost of about $5,000 per life saved. Clearly this is a simple calculation and it is clearly worthwhile for society to pay.

Next we come to a lot of different cases which are very similar: radiation from all sorts of causes, chemical carcinogens, and including sulfur oxide pollution. In all those cases the real similarity is that we know what happens at high doses of the insults we are giving. We are not sure what happens at low doses. We know on small populations the risk is quite small. But exposing all these 200 million Americans to the risk, we have to have some method from what happens at high doses, to calculate what happens at low doses. We want to be conservative in that, and get the best decisional knowledge we can.

Recently, work in cancer, in the last 20 years has led us to believe that to draw a direct straight line between the high dose point and zero is probably the best procedure to establish the low risk. First it was done for radiation, by the International Committee on Radiological Protection, started in 1927, and it is now applied to chemical carcinogens, believing that the same general theory applies there. It is a conservative, slightly pessimistic way to look at it, but it is a useful grounding study, because you are being safe.

There is an argument whether this would apply to sulfur dioxide pollution. But if one looks at the data of, for example, the Eastern United States, whether or not the straight line goes through zero, or gives a threshold with no risk at low doses is not very important to us here in Washington or in Boston, or all the way over to Chicago, because the sulfur dioxide concentration is so large that it merely adds something to what is already there. So we can calculate the hazard from evidence on rats and other animals, at large doses. If we do this, there is one point which most of the people setting regulations have not yet realized, is that inherent in taking a linear curve of this sort is that we also take a theory which says it is a long-term average effect.

The average dose is what counts at low doses; it is the accumulation over, say, 10 years. This automatically leaves you only having to monitor a long-term average, although you have to monitor very low

metic so we can leave out the unimportant and you can concentrate on all that matters.

The uncertainties of extrapolation have to be highlighted. They are coming out in the public mind, of course, because of saccharin, which is one of the clearest cases of proper application of the present laws, because by using the straight line I calculated—by a conservative estimate—200 deaths due to saccharin per year in this country, based on our known consumption of saccharin.

I think Dr. Marvin Schneiderman said 500, and I would not disagree.

Mr. THORNTON. That is a straight line linear projection.

Dr. WILSON. Exactly.

Mr. THORNTON. I hesitate to digress, but your prepared testimony also raised a question in my mind whether it might not be useful to enhance this country's capability of performing low dosage level tests.

It happens that in my own congressional district the National Center for Toxicological Research (NCTR) is probably the only institute in this country capable of performing low-dosage tests involving thousands of test animals over a long period of time. And by means of such tests, a check could be made as to the reliability of extrapolating from high-dosage test data and the linearity of the risk-benefit assessment.

Dr. Morris Cranmer, director of the Center, told me that he had some concern about the Canadian tests on 200 test animals, because of the possibility, which he supports by his work with test animals, that the metabolism of the rat is upset by the extremely large doses, causing formation of microcrystals in the bladder. These crystals might produce a constant irritation to the bladder wall and this constant irritation may cause the development of tumors. He speculates that such tumors might be reversed by the addition of ammonium chloride to the feed solution.

I think it is very likely that NCTR will be called upon to perform some low-level tests with regard to this particular matter.

Dr. WILSON. I think that would be very good, sir.

There was another institution which has done large numbers of tests, the Oak Ridge National Laboratory, irradiating over 2 million mice by now to low levels of radiation; and this is one of the reasons we know a lot about radiation.

There was some indication with those low-dose experiments on radiation at Oak Ridge that when radiation that comes at a slow, steady rate, like background radiation, it is a factor of 4 less severe than the linear curve predicts.

I point out of course that in most of the cases where we have information on high doses that it comes from one of society's mistakes. It is important to realize we should learn from mistakes. In the chemical industry there was the vinyl chloride cancers, where people were given doses with concentrations up to 10 percent, where as we now know industrially that we can reduce concentrations to one part in a million. It was ridiculous to have such high concentrations.

We dropped bombs on Hiroshima and Nagasaki—which was from some points of view a mistake; and the medical people made mistakes on radiation.

Our next witness, Dr. William Lowrance, is now, I believe, with the Department of State.

Dr. LOWRANCE. That is right.

Mr. THORNTON. Dr. Lowrance received his Ph. D. in biological and organic chemistry from Rockefeller University, and was a Research Fellow of Harvard University's Program for Science and International Affairs.

We are very pleased to have you with us, Dr. Lowrance. I thank you for your excellent prepared statement. It is relatively short. We would be pleased to ask you to summarize it or add such thoughts as you may wish.

Without objection your statement will be made part of the record.

[Biographical sketch and complete statement of William W. Lowrance follows:]

WILLIAM W. LOWRANCE, SPECIAL ASSISTANT FOR SCIENCE AND TECHNOLOGY, TO THE UNDER SECRETARY FOR SECURITY ASSISTANCE, SCIENCE AND TECHNOLOGY, U.S. STATE DEPARTMENT, WASHINGTON, D.C.

Age 33
Unmarried

January 1977–present: Special Assistant to the Under Secretary for Security Assistance, Science and Technology, U.S. State Department.

July 1975–January 1977: Research Fellow, Program for Science and International Affairs, Harvard University. Worked on problems of societal risks. Studied nuclear export policy. Wrote on the ethical responsibilities of scientists and other technical people. As a consultant to the Ford Foundation Nuclear Energy Policy Study, reviewed the worldwide development of nuclear power 1946–1976. Served on U.S. delegation to the Joint U.S.-U.S.S.R. Science Academies Study of Policy for Fundamental Research.

July 1973–July 1975: Resident Fellow, National Academy of Sciences, Washington, D.C. Under the sponsorship of the Alfred P. Sloan Foundation and the National Science Foundation, wrote the book, Of Acceptable Risk: Science and the Determination of Safety. Prepared part of the Joint U.S.–U.S.S.R. Science Academies Study of Policy for Fundamental Research.

Jnne 1972–July 1973: Assistant Executive Editor, The Journal of Cell Biology, New York City. Served as interim editor of the journal. Made recommendations on manuscript review and other editorial matters for this and the four other journals published by The Rockefeller University Press.

February–July 1972: Research Consultant, North Carolina Department of Education, Raleigh. Studied the educational institutions of the state with respect to institutionalizing the process of change.

1970–1971: Research Chemist, Tennessee Eastman division of Eastman Kodak Company, Kingsport, Tennessee. Discovered, developed, and patented a new method for synthesizing phenyl esters.

1965–1970: Graduate Fellow, The Rockefeller University, New York City. Ph.D. in organic chemistry and biochemistry. Carried out research on the biochemistry of cartilage and chitin with Dr. John D. Gregory in the laboratory of Professor Fritz Lipmann. Taught graduate biochemistry. Did theses research in synthetic organic chemistry and photochemistry under the direction of Dr. William C. Agosta in the laboratory of Professor Lyman C. Craig.

1961–1965: John Motley Morehead Scholar, University of North Carolina in Chapel Hill. A.B. in chemistry and biology. Order of the Grail, Order of the Old Well honorary societies.

1957–1961: Lee H. Edwards High School, Asheville, North Carolina. President of the Student Body. National Honor Society.

"Photochemical addition of ethylene to 3-carboxycyclohexenone and the derived ester and nitrile," William C. Agosta and WWL, Tetrahedron Letters, 3053 (1969).

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RISK--BENEFIT ANALYSIS AND RECOMBINANT DNA RESEARCH

Testimony of

William W. Lowrance

Before the

Subcommittee on Science, Research and Technology

of the

Committee on Science and Technology

U. S. House of Representatives

May 5, 1977

To introduce myself, I am William W. Lowrance. After receiving a Ph.D. degree in biological and organic chemistry from The Rockefeller University in 1970, I have moved full-time into issues of science and public policy, with a special interest in, among other topics, problems of social risk. I was a research fellow of Harvard University's Program for Science and International Affairs until January, when I moved to Washington to become special assistant for science and technology to Under Secretary of State Lucy Wilson Benson. Although I have watched the recombinant DNA research from a distance, I have not engaged in such experimentation myself and have no vested interest in it. Today I am not in any way representing my new employer, the Department of State, but am speaking for myself as a private citizen. I am grateful for this opportunity to meet with the Subcommittee and the panel.

For a few well-defined and well-understood technological problems, the several classical forms of risk--benefit analysis have proven useful in clarifying the issues, in making explicit the underlying assumptions, in anticipating the consequences, and in describing the available tradeoffs. The analyses probably did not of themselves decide those public issues, but they did inform and assist.

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Mr. THORNTON. We discussed the other day the question of the use of DES, which had been prohibited but is now being reevaluated as an additive to animal feeds under the provisions of law which prohibits any food additive known to be carcinogenic in man or animal. Even though only trace amounts remain in the animal's liver, DES is being reexamined to permit prohibition as an animal feed additive. Yet it is directly used for human consumption as a drug.

So, assessment of risk-benefits, in keeping our laws in shape, become a pretty important factor.

Dr. WILSON. Indeed, yes.

There is one other feature, about risk analysis which is particularly important for nuclear power. That is the hazard of large-scale accidents, and so forth.

As we get more people in one place in a big city, and a large concentration of energy in one place, the potentialities for large accidents increase. The question is: Should one consider large accidents with 1,000 people involved as worse than 1,000 small accidents, each with one person involved?

That is a difficult question, on which public perceptions differ. Someone whose wife was just killed in a car accident thinks small accidents are very important. But newspapers don't like them very much, because you cannot make front page news out of them. So it is not quite clear how one should assess those, at the moment.

Again, one must remember there always seem to be all sorts of features involved in large-scale accidents. For some reason there was little concern about the large hydroelectric accidents in Europe, where there were a couple big ones 15 years ago, killing 3,000 people—they hardly hit the newspapers here, and have been forgotten since. But they are some of the worst accident cases in the whole energy industry.

So it is not clear quite how one should take account of large accidents and why it is some have greater public visibility than others.

So I think that the main problem with risk analysis is that in almost every case where people blame risk analysis I think they are really blaming what is left out, or they are blaming the presentation.

I have personally gone out to talk to people who have been intervenors in important society cases, to find out why they intervened. It is almost always because they thought they were being lied to by the company, or not given the whole truth. If someone told them, yes, it is risky, but I have estimated the risk and I think it is this, they would have been much happier.

Mr. THORNTON. Thank you very much, Doctor, for a very excellent paper.

We also appreciate having a copy of the discussion paper and of the examples in risk-benefit analysis which you have appended to your statement. We will without objection consider adding parts of these documents to the record of this hearing. We would like to have that option.

Dr. WILSON. Thank you. I also sent a bibliography that I thought might be useful for your files.

Mr. THORNTON. We would be happy to have that for our files and for staff use in tracing source materials.

Thank you.

concentrations, which simplifies the problem. Neither industry nor government regulators seem to have realized that all the theories that give you linearity automatically have this in them.

Mr. THORNTON. Is it not true that while the assumption of linear projection does in all likelihood overstate the risk, it is a proper public policy to overstate? That is, you rationalize it on the basis it is better to overstate risk by using a linear projection than it would be to assume a curve?

Dr. WILSON. Yes. I believe that is correct. But then one no longer can have the concept of zero risk or zero pollution, because otherwise you are not allowed to breathe out and pollute the air.

Mr. THORNTON. Isn't it true that the problem with linear projection is, if you are plotting data from a high dosage test, it overstates the risk at any level up to that point. But if you are dealing with a curve rather than a straight line, the curve would be lower up to that point, but would accelerate more rapidly presumably after it passes the dosage point?

Dr. WILSON. But in most cases we have data at extremely high doses. We know of cases where people have been killed from what was probably sulfur dioxide air pollution, the same kind of incident that in London, in December 1952 killed 4,000 people. I was in that fog, and it was not the worst of London fogs, but it was quite bad.

So we have some data on very high doses, higher than we hope we will ever get to again.

Even if you can do this sort of calculation you must then ask yourself, to compare risk and benefit, and that is like comparing apples and oranges, or apples and steak, each of which is different.

But if you have the same benefit you can compare the two risks, which simplifies the problem: If you want to make electricity, you can use coal or nuclear power. If you make radiation and sulfur oxide calculations, it is well known coal is pretty bad compared to nuclear.

From this point of view, you make the calculation for the nuclear case, as people have in the past 20 years, and ask why aren't we 100 percent for nuclear, and why is the Carter administration changing the country's policy?

It is because the risk analysis left out the big thing about nuclear power, that is, is there or not a connection with nuclear war? Some people say the chance of nuclear war by the end of the century is even odds. If you work this out as a probability it is much more likely that you will be killed by nuclear war than that you would be killed by a radiation accident, by a factor of 100,000.

That means all the other calculations are almost irrelevant compared to nuclear war.

You ask a consulting organization to report on the probability, you pay them a lot of money and they have to give you value for money it terms of 490 pages and 1 page, the last page, says, "We have left out one important factor—nuclear war," which is the most important item.

That does not say the risk analysis is wrong. It does say it has been badly presented—and they often are.

A really good analyst will point out the things he left out and highlight them. The point of view of a proper analysis is to do the arith-

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select appropriate decision criteria.

Two points remain to be made. Even if accurate estimates of risks and benefits can be provided, the final problem is how to aggregate them. A decision maker should be free to weight the various risk and benefit categories and their uncertainties in order to explore questions of equity as well as efficiency. Most analysts currently fail to present their results in a fashion which will enable a decision maker to examine for himself the sensitivity of the results to the assumptions and the distributional effects of alternative policies.

Finally, if decision making involving risks and benefits is to improve, more attention must be paid to the clear presentation of the assumptions, values, and results. Reports need to present concise summaries which convey the uncertainties and limitations of the analysis in addition to the matrix of costs, risks, and benefits. As the field of risk-benefit analysis advances the estimation of risks and benefits will become more precise and implicit valuations will be made more explicit. Corresponding improvements must also be made to enhance communications between the risk-benefit analyst and the accountable decision maker.

be fulfilled immediately, but only over the course of time as our knowledge and experience increase.

Moral and Ethical Issues

Critics of risk-cost-benefit analysis have aptly and correctly pointed out that risk-benefit analysis cannot make equity or ethical judgments.[47] They further feel that benefit-cost analysis may act to obscure important issues,[48] presumably because such analyses can be used to justify difficult political decisions by persons avoiding their personal responsibilities. Risk-benefit analyses are not intended as substitutes for moral and political judgments or for holistic decision making which includes factors outside the scope of formal analysis. As we have already pointed out the quantitative assessment of risk may be objective, but choosing the scope and values of any analysis requires subjective judgments. These limitations should not dissuade us from analyzing as objectively as possible the consequences of possible courses of action. To fail to do so would be to deny the worth of better information and greater knowledge. Merely knowing the extent of our uncertainties may guide our choice of action more wisely than proceeding in ignorance of potential risks and benefits.

Moral, ethical, and political considerations may all properly take precedence in decisions in our democratic society. Nevertheless, in many situations where ethical or political arguments are not paramount, understanding risks and benefits may be crucial. Fears that risk-benefit analyses will obfuscate the issues seem to imply that decision makers

maker than others, the risks and benefits to each identifiable group should be distinguished. Ultimate decisions of equity rest with the political process, but comprehensive risk-benefit analysis should supply distributional data. If compensation to those bearing undue risk is politically desirable or feasible, risk-benefit analyses may have an additional role to play.

Assessing risk and judging the acceptability of a risk (i.e. determining safety) are independent processes. Much confusion has arisen in public policy disputes over the failure to separate the distinguishable questions:

1. What are the scientific and technological bases for assessing the expected risks and benefits?
2. What are the relative probabilities and uncertainties of particular consequences?
3. Can the risk be reduced and what will it cost?
4. Is the distribution of risks and benefits fair?
5. Is this risk acceptable?

Attempting to answer these questions simultaneously can often mean that none are adequately answered. The last two questions fall outside the domain of risk-benefit analysts and lie in the province of the decision maker.

Much of present day legislation, regulation, and standard setting is based on intuitive balancing of risks and benefits. One objector to risk-benefit analysis has said that my gut feeling is better than any of your analysis. Gut feelings will continue to serve us well in many instances, but society

consequence risks. One of us[38] has suggested that the perceived importance of a large accident with N fatalities is proportional to N^2, rather than N. Slesin and Ferreira have investigated frequencies of multiple death accidents in the United States between 1956 and 1970 and conclude that the social impact of large accidents varies as N^3, implying that one 100-death accident has the impact of one thousand 10-death accidents.[37] Society apparently acts to reduce the anxiety and impact of severe risks more than the absolute risk might suggest.

Although comparing risks and understanding risk perception are important for the decision maker, it is not always helpful to include information about other risks to influence the acceptability of a particular project. Risk-benefit analysts who do may all too easily overstep their role as risk assessors and appear to try to usurp the decision maker's function. A decision maker must be made aware of current levels of risks, but it is always possible to demonstrate that some other activity is worse. Directly comparable examples with similar benefits are relevant, but comparing automobile fatalities to accidents in chemical plants may not be particularly useful to a decision maker whose sole authority is to decide upon the acceptable levels of risk in a chemical factory.

Various formula or criteria have been suggested for defining levels of acceptable risk and allocating resources to reduce risks.[39-43] The empirical basis for most of these

the federal attempt to control automobile air pollution suggest that the development of long-term alternative engine technologies would have achieved greater overall reductions in air pollution from 1975 to 1989 at lower implementation costs than the strategy which was actually followed by Detroit. In one analysis the costs of various programs were plotted against an index for weighted reductions in air pollution to indicate the most desirable policy outcomes.[32]

Sensitivity analyses which investigate the effect of varying parameters can provide important information for the decision maker. Changes in the discount rate or in societal risk aversion may change the net benefits of a project. If possible a range of values should be studied. One example where results were given for a range of differing assumptions was in the analysis of automobile safety features by Lave and Weber.[33] In this study the worth to the consumer of seat belts, dual braking, and other safety systems was calculated for several discount rates and for different consumer aversions to injury and death, allowing an individual to determine the value of safety features for his own assumptions.

Acceptability of Risks

Even if the risk-benefit analyst is able to quantify risks and benefits, how are we to judge the acceptability of a risk? What criteria should apply to our choice among alternatives? This judgment is, or course, not the role of the analyst, but of the decision maker. If a choice were solely between freezing to death or burning unclean coal in our

The literature contains other analyses and reviews which examine the efficacy of various medical treatments and discuss cost-benefit applications.[25-27] When the alternatives and the tradeoffs are explicit, and where statistical data exist, these risk-benefit analyses are quite useful. It is interesting to note that those who claim that risk-benefit analyses should not quantify tradeoffs between lives and dollars often do not object to its use for the allocation of resources in the medical field, where lives and dollars are directly at stake.

Dealing With Uncertainty

We should distinguish between cases where the project outcomes are well-characterized and their probabilities reliably determined and those cases where the probabilities of individual consequences are not well-known. It is in the latter situation that the most vigorous objections to utilizing risk-benefit techniques have been made. Here new ground must be broken, although the risk-benefit framework can still illuminate these tradeoffs. Decision criteria which reflect our lesser degree of certainty and perhaps a greater risk aversion may need to be adopted in such circumstances.

Dealing with uncertainty is the central dilemma of all policy choice. Uncertainty occurs in predicting the consequences of actions as well as in valuing the particular outcomes of alternative policies. Reducing uncertainty, defining its bounds and its effects on policy preferences should be primary goals for risk-benefit analysts. Sensitivity

quantitative success at risk-benefit analysis, and he replied "I know of no instance where the nonpecuniary aspect of the problem has been included in a proper quantitative manner."[19] He did state that such analyses had proven useful in cases where the risks were already accepted by the public. Obviously the most difficult area for risk-benefit analysis is in treating those future risks with the greatest uncertainties. In this area improved risk assessments and a better framework for considering these problems are sorely needed.

The most apparently straightforward risk-benefit studies are those which evaluate the costs of saving lives through the application of known medical technologies or safety equipment. Here the tradeoffs can be direct: years of life saved vs. the risk of losing a life in an operation. But the situation is quickly complicated by questions of disability, quality of life, and choices involving whose life to save. Determining the real costs of a program and evaluating the efficacy of medical treatments have posed severe difficulties to the use of risk-benefit analysis techniques in the medical area. Analyses have usually presented the decision-maker with a cost/life-saved (cost-effectiveness) comparison of several possible options, but at some stage a decision-maker might have to choose between a large program or a small one and in these cases net benefits become important. However, progress is being made in performing risk-cost-benefit comparisons.[20] Because of the limited resources which can be allocated for all medical treatments, risk-benefit analyses can

do we measure low level chronic effects or account for risks which are not yet identified? These are unanswered questions which exacerbate the previously mentioned difficulties of specifying an appropriate discount rate and dealing with uncertainties.

Although risk assessment is improving, relatively little work has gone towards assessing the benefits of those technologies or activities which generate risks. Research on benefit assessment for earlier cost-benefit analyses is relevant, but in many cases these benefit calculations have been hotly disputed. (The Corps of Engineers has become adept at measuring benefits but not always successfully.) Cost-benefit analyses have been extensively applied to water resource problems.[15,16] In a number of cases these have been incomplete or wrong. Many lessons on the limitations of cost-benefit methods which were applied in the Delaware River Basin have been discussed in Reference 17.

In instances where the benefit is common to all alternatives under consideration, it may be possible to examine the cost-effectiveness of alternatives for producing a given unit of benefit. However, a principal limitation of analyses which distinguish among alternatives on the basis of cost-effectiveness is their inability to determine the overall scale or size for a program. One risk-benefit study of alternative methods for generating electricity compared only the risks, claiming the benefits of equivalent amounts of electricity are equal.[18] This might be true for one additional power plant

used on the widely publicized Rasmussen study of nuclear reactor risks.[13] However, for low probability risks it may be difficult to apply present knowledge to accurately predict the probabilities of accidents. There is always the lingering doubt that possible failure modes may have been overlooked, especially common mode or simultaneous failures. In estimating probabilities for particular events the influence of design failures and of deliberate actions like sabotage must also be considered. Scenarios are usually constructed in order to envision rare potential accident sequences. Each of the analysis methods now in use has limitations in its applicability to new circumstances, particularly in estimating absolute probabilities of very infrequent events. Despite their shortcomings, these methods have proven to be powerful techniques for finding the most prominent failure modes and for identifying potential weak spots in technological systems.[14]

The consequence of an accident determines the magnitude of the risk. For many risks models must be developed to predict the damage to humans or to the environment. For example, estimating the effects of air pollution can involve dispersion models for transport of the pollutants from the source to the individual, including atmospheric chemical conversions. Such models permit estimation of the dose received. Additional studies in experimental toxicology and epidemiology are then needed to characterize the dose-response relations. Here synergistic effects and the problems of competing risks must be sorted out. Population distributions must then be

its objective and subjective components can never be wholly separated. Although it has bases in scientific and economic techniques, it is an art with limitations. These limitations have arisen because the ultimate criteria for any decision must reside in exogenously determined values and goals specified by society or by an accountable decision maker. So long as the limitations are recognized, risk-benefit analysis can establish a basis for the explicit comparison of alternatives, indicate significant uncertainties, and point out aspects of the decision which are outside the scope of formal analysis.

Development and Usage

Many methods of risk assessment and cost-benefit analysis have been used. In an attempt to promote interdisciplinary communication and increase awareness of these methodologies, the Committee on Public Engineering Policy of the National Academy of Engineering and the Engineering Foundation have sponsored two conferences: "Benefit-Risk Decision Making"[10] and "Risk-Benefit Methodology and Application."[11] The first of these was held in 1971 in order: (1) to help make the issues of benefit-risk decision making explicit enough for public discussion; (2) to ascertain the current status of benefit-risk decision making as a field of study and in terms of current practice; and (3) to identify promising lines of inquiry that might lead to improvements in methodology and implementation."[10] The colloquium succeeded in asking a number of important questions and discussed risk-

r is the appropriate discount or interest rate, and T is the time horizon for the project. In most cases it is appropriate to discount equally costs and benefits if future opportunities (e.g. to prevent premature death) are likely to be the same or greater than today's. Questions of intertemporal equity become most important for evaluating long term effects like those resulting from persistent chemicals in the environment, increasing global CO_2 concentrations from fossil fuel combustion, or long-lived radioactive wastes from nuclear power generation, just to name a few examples. Relative net present values and the ranking of alternative projects with substantially different timing of the relative costs and benefits can be dependent upon the choice of a discount rate.

The idea of a different discount rate for risks and for economic costs has been widely mooted but is only beginning to be discussed logically.[7,8] If the cost of saving a life in the future is expected to be the same as the cost today, the discount rate for risks should be the same as for other costs. If, however, the cost of saving a life is expected to go down in the future, one might account for this by taking a higher discount rate. Arrow has shown that this is incorrect.[8] Instead one should explicitly take the expected cost change into account in the cost or benefit stream, C_t or B_t. For some cases of environmental and health hazards the costs of cleanup might increase with time. If, for example, toxic chemicals in the biosphere increase over time, costs attributed to their effect should rise more rapidly than the discount

measured in the most appropriate units (e.g. injuries, deaths, tons of emissions, dollars of damage) and their uncertainties indicated. Finally, an inclusive assessment is carried out which aggregates the disparate measures of the alternative outcomes. The conclusions should incorporate the results of a sensitivity analysis, which varies each significant assumption or parameter in turn to judge its effect on the aggregated risks, costs, and benefits.

The economic methods of cost-benefit analysis are most commonly used to assess the overall merits (net benefits) of proposed alternatives.[1,2] The extension to include risks is, however, not trivial. A principal problem is that risks and benefits may be measured in different units and therefore are not strictly additive. By definition risk-cost-benefit analysis will attempt to express all quantities in a common unit, usually dollars, so that tradeoffs are between comparable quantities and a net benefit can be calculated. This may require estimating a producer's or consumer's surplus where economic markets exist or determining a "willingness to pay" in cases where no markets exist (e.g. for goods like clean air, salt marshes, or human lives). If fatalities are potential consequences, we might wish to assign a cost by estimating the willingness to pay for reducing the probability of death or injury. This has somewhat misleadingly been described as determining "the value of human life." We would like to avoid this overly dramatic description. For actual decisions the cost of decreasing a risk is nonetheless

Introduction

Decision makers are faced to an ever increasing extent with evaluating uncertain risks and benefits to human health and to the environment. Without reliable knowledge of the implications and consequences of alternative projects or possible courses of action, their ability to make sound judgments is diminished. However, estimating the magnitude, probability, and distribution of risks and assessing the costs and benefits of projects are fraught with the difficulties of science, the uncertainties of technological and economic forecasting, and the pitfalls of public policy. How then can risks, costs, and benefits be explicitly compared? How should pertinent information be ordered and assimilated to assist in achieving acceptable balances between benefits and risks, both in the short term and in the long run?

The methodologies which are used in "risk-benefit analysis" attempt to make explicit the often hidden tradeoffs between lives lost and dollars spent, or between pollution and environmental quality. No magic formulae have been evolved for grappling with these seemingly incommensurable attributes. Nevertheless, the growing difficulties of regulation, standard setting, legislation, and technological choice have necessitated improved methods for answering risk-benefit questions. The purpose of this paper is to review the status and identify the common problems of this developing art which is beginning to be applied in numerous subject areas.

where lives are lost 1000 at a time. The "adjusted" cost becomes $750,000. This is still a large number, but we have not allowed for the "public confidence" factor I introduced earlier. It is well known that the AEC made the decision that society must spend this money, and I concur.

Assumptions

Unless the next step is very cheap we should probably not make it. *I must warn the reader, however, that my calculation considers only one of the nuclear accident risks; I ignore completely questions of sabotage, diversion of nuclear materials to military and clandestine uses,* and accidents associated with storage of plutonium. Since one of the effects of sabotage might be to induce a loss of coolant accident, improved safeguards presumably guard against sabotage somewhat also, and make the decision to improve emergency core coolant systems even more worthwhile.

For operations involving liquefied natural gas (LNG) we are a lot less careful (*13*). There is, to my knowledge, no detailed accident study (*14*). But we have had some bad experiences. In Cleveland in 1946 when a tank, built of steel with an incorrect composition, collapsed; there was no surrounding earth berm, and the liquid entered sewers; 133 people died. In *1973, 33 workmen repairing a leak in an* empty tank died in the fire. We now have about 75 such tanks, hopefully better built, but ten times larger. For the rough study I have here I will assume we can extrapolate from experience. These tanks are not in sites that are particularly remote from people, in spite of the fact that the only fundamental parameter in the dispersion of LNG vapor after a catastrophic spill is distance. To choose a remote site does not seem expensive. In one case of which I am cognizant, I believe it would cost about $200,000 more to build the new tank in a remote site; in many cases it might cost less (for 75 tanks—$15 million.) This might save the next 133 lives at a cost of 15,000,000/ 133 = $100,000 each, or with my equivalency rule, of $100,000/133 ≈ $1000 equivalent for accidents occurring one at a time. It seems to me that the gas industry is not yet prepared to spend enough. Maybe matching the nuclear case would be overdoing it, since there seems to be little question of public confidence involved, but to insist on a $750,000 expense (adjusted for the large number in accident) in the nuclear case and not to spend $1000 (adjusted) for LNG seems inappropriate.

Finally I want to illustrate some implications of discussing risks openly—even when few lives are involved. Twenty-four years ago, I came from England to Rochester, N.Y., at that time almost a one-industry town dominated by Eastman Kodak (Xerox has since been added). Tornadoes sometimes occur in New York state, and it is only a small exercise of a morbid imagination to calculate the probability of a tornado crossing Kodak Park and destroying most of the plant. I find this will happen once in 50,000 years. Such a catastrophe could cause a major economic depression in Rochester.

> Has this been discussed at the shareholders' meeting?
> I doubt it.
> Has this been brought up at a Board of Directors' meeting?
> Maybe, because I am not the only man who has a morbid curiosity.
> Has this been discussed with the insurance company, or the New York Stock Exchange?
> I doubt it, but it should be.

How are we going to discuss these matters of life, death, and probability? For we must, if we are to become masters of our fate.

Conclusion

Applications of risk-benefit analysis is complex, and I suggest that we need several iterative steps:

First, we must be sure that we understand the benefit and the risk and that the former outweighs the latter.

Second, we must be sure we have chosen the method of achieving the benefit with the least risk.

Third, we must be sure we are spending enough money to reduce the risk further—at a rough figure of $1,000,000 *N* per life saved, where *N* is the number of persons killed in one incident.

Fourth, we go back (iterate) and recheck our numbers with new perspective from the preliminary calculations.

Of course we must include in the benefit the advantage of continuing on a planned course without a change—this is easily expressed in terms of cost—and there is a clear financial benefit in the cheapest method of performing an action.

So far, as I have shown roughly, differences of a factor of 1000 are common; so nothing more than common sense and grade school arithmetic are needed to improve the situation in any given case.

Some people object to this type of "cold" analysis, and argue that decisions on safety are usually and necessarily emotional. Man is, I am glad to say, an emotional animal, and at the least, his instincts are not always wrong. But the National Environmental Policy Act of 1969 enjoins us all to make comparisons; to compare emotions is impossible, and an attempt can lead to argument and even war; to compare analyses of this sort is possible, and it seems to me necessary, even when emotional factors enter into a final decision.

Finally I wish to reiterate that this is a crude survey; the examples are merely examples (even Table 1 is not complete). In particular, I have not discussed the risk of sabotage, and any increase or decrease in the risk of nuclear war. These are the major imponderables, for example, in any national or international decision on whether to exploit nuclear power on a large scale. However, if we can put numbers on the easily assessed risks and get them out of the way, we can concentrate on these important issues.

References

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(3) Lave, L., Seskin, E., "Air pollution and human health," *Science*, 169, 723 (1970).

(4) "The Sciences," *N.Y. Acad. Sci.* Magazine, July 1971.

(5) Numbers are hard to find. These agree with guesses provided for me by the Department of Transportation and the Highway Safety Research Center, University of North Carolina, Chapel Hill, N.C. 27514.

(6) "The effects on populations of exposure to low levels of ionizing radiation," Report of National Academy of Sciences–National Research Council Committee on Biological Effects of Ionizing Radiation (BEIR Report), *November* 1972.

(7) According to Dr. C. F. Storrs, *Combustion Engineering*, the numbers include:
- extra equipment $8,000,000–$12,500,000/ year
- larger exclusion area
- increased direct operating cost $1,000,000 to $2,500,000 per year
- increased unscheduled shutdowns
- increased exposure to operators

(8) Sagan, L., "Human costs of Nuclear Power," *Science*, 177, 487 (1972). Sagan finds a larger number for the dose. In *Nucl. Saf.* January–February 1974, a larger number is also found. I believe that is more nearly a worst case, whereas I am trying to find an average figure.

(9) Farmer, F., "Siting Criteria—a New Approach," Conference on Containment and Siting of Nuclear Power Reactors, Vienna, IAEA, 1967.

(10) Starr, Chauncey, "Risk-Benefit Analysis in Socio-Ecoic Systems," in Conference on Environmental Effects of Nuclear Power Stations, IAEA, 1971.

(11) AEC reactor safety study: preliminary indications of report presented by N. C. Rasmussen (WASH 1400).

(12) Testimony by J. C. Moore, Westinghouse Electronics Corp., and by C. Storrs, *Combustion Engineering*, Joint Committee on Atomic Energy, U.S. Congress, January 1974.

(13) "Natural Gas is a Beautiful Thing," Richard Wilson, *Bull. Atomic Scientists*, p 35, September 1973.

(14) Since the above was written, some accident probability estimates have appeared for accidents involving LNG ships—"Final Environmental Impact Statement for the Construction and Operation of an LNG Import Terminal at Staten Island, New York," Federal Power Commission, Bureau of Natural Gas: Docket nos. CP 73- 47, 58, 132, 148, 230, and CP 74- 122, July 1974.

(15) "Perspectives in Risk-Benefit Analysis," Conference held by National Academy of Engineering, 1972.

(16) "Risk-Benefit Analysis, Solution or Dream?" Conference at Los Alamos Scientific Laboratory, November 1971; and particularly the paper "Quantification of Social Values" by H. Otway.

(17) Chapter 9 of "Energy, Ecology and the Environment," Richard Wilson and W. J. Jones, Academic Press, 1974.

From a talk presented at the Conference on Advanced Energy Systems, Denver, Colo., 1974.

of seat belts much greater, is the expense of seat belts worthwhile? Society says yes, and demands the payment.

Should we then say that a human life is "worth" $100,000? That would be too naive a view of risk benefit analysis, and would get us into trouble. If we can save a life immediately, we often do so at a cost far greater than $100,000, and even risk our own lives; out of such matters are heroes made. Religious men may argue that a human life is priceless, so how do we express the results of such an analysis? Should we just increase the "value" of a human life indefinitely? Should we spend $100,000,000 to save a life? Even in the U.S. we can't spend $100,000,000 more than 1000 times a year, and more than 1000 occasions arise. We must be able to spend our money to save as many lives as possible.

We sometimes distinguish between risks taken voluntarily and those taken involuntarily. Man will take large risks voluntarily, and at the same time rebel at being asked to take risks involuntarily. Society therefore must be more cautious than an individual. Society, moreover, may spend money to make even voluntary risks less. Most people would not pay for seat belts if they did not have to; some don't use them when they exist. Riding without a seat belt is clearly a voluntary risk; spending money on one today is involuntary.

Benefit

One way of expressing the result of a risk benefit analysis might be to say that society should spend X dollars in the expectation of saving or prolonging life *when the particular life is neither known nor knowable;* X should presumably be a number gradually increasing with society's affluence, and roughly constant from activity to activity.

Let us proceed to an example of the cost to reduce the radioactivity from normal operations of nuclear power. Three years ago the AEC promulgated new rules (10CFR50 Appendix I) for design of power stations, to reduce the maximum allowable dose at the site boundary to 10 mrem per year (in the open).

What is the reduction of risk, and what is the cost? First, the risk under previous operation was the effects of the radiation on people. The recent National Academy of Science's BEIR report (*6*) (Committee on Biological Effects of Ionizing Radiation) suggests that, under a reasonably pessimistic assumption of effect proportion to dose, that 3000 extra people per year might die of all forms of cancer if everyone in the U.S. were exposed to 170 mrem per year extra dose. (Here I leave out genetic effects for simplicity.) The previous rules would have led to about $\frac{1}{5}$ mrem per year, or four cases per year.

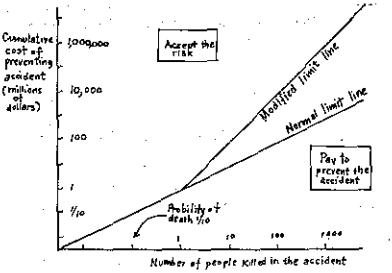

Figure 1. Cost of avoiding accidents

People voluntarily and unhesitatingly take much bigger doses than this, but this is an involuntary risk. This is because most people are not at the site boundary; many are in shielded buildings, and utility companies must plan to be well below the limit to avoid being shut down (as the Palisades plant of Consumers Power Co. of Michigan was shut down). The new rules will lead to about $\frac{1}{20}$ mrem per year average.

Cost

What does it cost? This is a little hard to tell because several changes were made at once. We are here only talking about the power station *itself;* not the chemical processing plant, where all the krypton 85 is released. There are several estimates. Among the higher is $5,000,000 capital cost per 1000 MWe power station and $1,000,000 extra operating cost per power station (*7*). A lower estimate would be to argue that the only really necessary change was in the off-gas holdup time—almost nothing for PWR and $2,000,000 capital with $50,000 per year operating for a BWR, and therefore averaging to $700,000 capital for an average *overall* nuclear power station. The total cost for 1000 power stations becomes $2,000,000,000 per year with the higher figure, and $700,000,000 per year with the lower.

This comes to $500,000,000 (or $140,000,000 with the lower figure) per life saved—6000 times what we pay to avoid automobile deaths. Of course we also reduce genetic risk as well as the cancer risk here considered. This reduces to $125,000,000 if we take the dose figures in Reference 8. If we are very pessimistic and say that *all* cancers (300,000 per year in the U.S.) are radiation induced, and apportion them by dose, we still get $2,000,000 per life saved.

Sagan (*8*) goes over some of these numbers. But his estimate of the reduction of dose to the population by this change is overstated. Even under the old rules, no power station emitted 170 mrem per year at the boundary. If all equipment worked well and there were no fuel rod failures, much lower figures were achieved. A utility company *had* to aim at a lower figure to prevent being shut down by the AEC if a failure occurred. My figure of $\frac{1}{5}$ mrem is taken from old power stations operating in 1971 before the new regulations took effect; adjusted to allow for the expectation that in the U.S. there will be 1000 power stations with 1000 MWe capacity by the year 2000.

We can see at once that this is out of balance. Why then do we do it? I submit that we do it because it is necessary to ensure public confidence. For decision-makers to have this before them clearly, I suggest that we should include an item which I call *the cost of public confidence* in our risk-benefit and cost-benefit analysis. Without this item decision-makers can, as they did some years ago in the nuclear case, make a wrong decision and reduce public confidence, which will take much more money to restore.

My studies lead me to believe we should spend 100 times more on nuclear operations than on others—to ensure public confidence. If we spend less, we will later have to spend 1000 times more to recover lost confidence. Part of this factor is necessary, of course, because it is a new technology, and any new source of death is of special concern.

The question of catastrophic accidents involving many people is more serious. Mankind has tolerated a billion accidental deaths spread roughly uniformly over a thousand years, and heals the wounds steadily. Yet a billion deaths, occurring *together* once every thousand years, would cause a wound to society which might destroy the human race.

This aspect of catastrophe was recognized by Farmer (*9*) and by Starr (*10*) among others. I will outline here, and attempt to justify, a modification of Farmer's approach. We can correctly compare risks by comparing them in my form, deaths per kilowatt hour, or deaths

Difficulties increase as we go to less calculable processes. The risk of reactor accidents, for example, is harder to evaluate than for hydroelectric plant accidents largely because they haven't happened. Yet in many cases the risk can still be estimated even though there is now more uncertainty. We have a simple procedure--a straight line--which is conservative.

Why, in the nuclear case, aren't the calculations accepted? Largely, in my view, because there are risks left out--the risk of sabotage, and the risk of war. A typical risk study can occupy 400 pages. Only a sentence says what is left out! The analyst is proud of his hard work and is often unwilling to emphasize what he hasn't done. This is bad presentation but often the whole risk analysis is blamed.

In cases like this, the risk analysis can illuminate the issues and can show what matters need not be considered. The recent Ford-Mitre study of nuclear energy is an example of this logic. The study group went through the risks of nuclear power and showed they were all comparable or less than other energy sources except one--nuclear proliferation. They then highlighted this issue. In my view they suggested the wrong answer on this issue, but at least they isolated the most important problem.

The trouble with risk analysis for DNA research is similar to that for nuclear power. The benefits are unknown, but may be huge. The hazards are unknown, they are probably small, but may be huge. If risk benefit analysis is applied it may help to isolate some components of the risk, or isolate some components

Radiation from x-rays or normal operation of nuclear power stations, sulphur oxides from coal burning, and the effect of chemical carcinogens all present another concept in risk analysis. We only know the risk for high levels of the environmental insult. For low levels the risk is expected to be small, but we expose 200 million Americans to the risk. Moreover, we do not want to carry out experiments with people--but instead use other mammals: pigs, rats, and mice--and assume they are like people. Typically a test for a chemical carcinogen may use 200 rats exposed for a lifetime. If the "true" cancer incidence is one in this sample of 200, we have an appreciable probability of finding none.

Therefore, we cannot prove in any ordinary experiment that any lifetime risk less than 1/200; 2,000,000 Americans die a year so this would still give 10,000 cancers per year when applied to all Americans! This death rate is too much. We have to find a procedure for deriving the risk at low doses from that at high doses.

It is usual to take a conservative procedure and to assume that the risk is strictly proportional to dose. This was first done for radiation by the International Committee on Radiological Protection; various bodies now recommend that linearity be assumed for chemical carcinogens and I and others have suggested that there is no evidence of non-linearity for sulphur oxides. It is widely accepted that in all these cases linearity probably somewhat overstates the risk, and it is therefore a suitable basis for a prudent public policy. Then we feed the rats at a high concentration and using a straight line estimate the risk at a

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CURRICULUM VITAE

Richard Wilson

EDUCATION

St. Paul's School, London, England
Christ Church, Oxford University, Oxford England
Open Mathematical Scholar 1943
B.A. (physics) 1946
M.A., Ph.D. 1950

POSITIONS

Research Lecturer, Oxford	1949-50
Research Associate, University of Rochester	1950-51
Research Associate, Stanford University	1951-52
Research Officer, Clarendon Laboratory, Oxford	1952-55
Assistant Professor, Harvard University	1955-57
Associate Professor, Harvard University	1957-61
Professor, Harvard University	1961-present
Associate, Adams House, Harvard University	1971-present

SUMMER AND VISITING POSITIONS

Stanford University	1958
John Simon Guggenheim Fellow, University of Paris-Sud (Orsay)	1961
Fulbright Fellow, Laboratori Nazionali di Frascati, Rome	1969
Lecturer on Energy and the Environment, Summer Institute Lawrence Radiation Laboratories, Berkeley, California	1973
Lecturer, Second Energy Symposium, Boulder, Colorado	1974
Lecturer, Summer School on Aspects of Energy Conversion, Oxford, England	1975
Lecturer, International School on Energetics, Erice, Sicily	1975

COMMITTEE, CONSULTANT, OTHER POSITIONS

Assistant Editor, Annals of Physics	1956-present
National Science Foundation, Physics Advisory Panel	1967-70
Trustee, Universities Research Association	1968-73
Consultant to Attorney General's Office, State of Maine (on Nuclear Power)	1971-72
Transportation Advisory Committee, City of Newton	1973
Consultant to Energy Research and Development Administration	1974-present
Consultant to Nuclear Regulatory Commission	1974-present

SOCIETIES

Fellow--American Physical Society
Member--American Academy of Arts and Sciences
Fellow--New York Academy of Arts and Sciences
Life Member--Society for Psychical Research (London)
Newton Conservators--Member of Board of Directors

Dr. WENGERT. I don't think I have any substantive comments to make. I would like to emphasize that I think it is the scientific community generally and the public generally that ought to be aware of the approach this committee is taking.

We are told that the confidence of the American people in government is at a low ebb. I think it is important for the public to know that committees of Congress are going at problems of this sort in a highly intellectual way. As an academic, I am much in favor of such approaches. I think the public ought to be aware of the fact that this committee is taking the time to go into a very difficult issue in this way.

I think you deserve commendation for this.

Mr. THORNTON. Thank you very much.

Mr. McGowan?

Mr. MCGOWAN. I would like to add my commendation to that and thank you, Mr. Chairman, for spending the time out of what must be a busy legislative schedule to listen and ask pertinent questions. The only substantive thing I want to say is that we have got to develop, somehow, feedback looks to change decisions that we—social decisions—that we make now that may be correct now but may be outdated or incorrect or off the mark 2 years from now.

We tend to think of social decisions and institutions that we put into place as being there in perpetuity, which is a mistake.

If there is one thing we have learned out of this controversy is that it is that things change a lot faster than we think they are going to.

Mr. THORNTON. Mr. Stone?

Dr. STONE. I would conclude by warning that we should not be guilty of what generals are accused of, namely of fighting the last war all over again. Recombinant DNA has alerted the public to the future problems of biomedical science. But, in itself, it does get right down into the laboratory, and deals also with a highly obscure aspect of science.

Most of the time, in future, you seem likely to be dealing with the problem of digesting potential scientific advance in society. You will be wondering, Shall we make these nitrogen fixing plants? You will wonder whether we really want to experiment on people in a certain fashion, whether we want to use certain kinds of applications which may have side effects and so on. These are different kinds of problems.

So I think that what one has to do is not worry quite so much about whether the scientist will "speak up" to sound the initial alarm, but to worry about the process beyond the initial alarm. In the late debate, how will society go about solving the problem.

Then I would reemphasize the strengthening of the advocacy process and also those parts of the society that want to go forward in a self-appointed, or otherwise appointed, "objective" role.

It seems to me that that is where the future action will be. What recombinant DNA has done is to alert society to a whole field of problems. But otherwise it will remain, as a problem, somewhat anomalous.

Mr. THORNTON. Thank you very much.

I want to thank each of the panelists today for a fine discussion. We will be adjourned until 10 o'clock tomorrow morning in this room.

[Whereupon, at 12:03 p.m. the hearing adjoured, to reconvene at 10 a.m., Thursday, May 5, 1977.]

of a Federal institution to regulate and determine what DNA recombinant research can be conducted and where and what commercialization of it can be obtained may suggest that an emerging scientific issue should also be put into this same agency which has expertise then on settling this kind of problem.

Dr. STONE. I think the history of regulation in this country is not a hopeful one. By and large, the regulators, when set up in a formal fashion have been taken over by the regulated. This is the conclusion that all students of the regulatory process make, for example, in courses on one subject.

Second, the fast moving rate of biomedical research is such that it is a very difficult thing to regulate and would confound the regulators even more than is the case with the railroads and the airplanes and the coal. I am not sure whether it would be better, depending on your point of view, to have the new issues diffused by flowing into this institution or not.

But I think it is a very open question. I am not sure whether by SIPI's own goals it would be desirable.

Dr. WENGERT. I wonder if the term regulation is appropriate? Would it not be better to think in terms simply of review and public analysis, and not emphasize the regulatory functions immediately. Scientists would not be comfortable with regulation. I think a review function may be what is needed.

I don't have quite the optimism about communication just expressed by Jeremy. As I think of the Watson and Crick experience, there are parts of it that are quite amusing in terms of the lack of communication between and among scientists.

There was high secrecy about what they were doing, and competition as to who was going to get there first. This can be beneficial especially if you are not dealing with risks. That is where I think review would be appropriate—to identify possible risks.

After the risks are identified, then maybe regulatory activity becomes important, but the first step is to make sure whether or not there may be risks.

Ms. NELKIN. In dealing with regulatory institutions, I think it is useful to separate two issues which are being merged in the discussion right now. One is the question of immediate safety. The other, in a way of greater public concern, is the question of future potential applications of the research. The problems regulating for immediate safety, are much easier to resolve. We have some experience in congressional legislation that dates back to the 1946 Atomic Energy Act with its provisions, concerning safety procedures for research on fissionable materials.

However, the issue of genetic manipulation and other future potential applications, like the questions raised by the IQ controversy and the XYY controversy, are more difficult. I am much less sure about how to develop institutions to regulate future potential impacts.

Mr. MCGOWAN. I would like to point out that first of all that SIPI is not proposing a regulatory agency as was I think implied by Dr. Stone. What I suggested is a commission to allow for the discussion of some of these issues and to provide for public education and input.

I think the issue of regulation of recombinant DNA goes beyond just recombinant DNA. There are other areas of biohazardous research which have been going on in this country for some time.

There was a great division within the scientific community as to whether it should be done. The people who disagreed that it should be done said you are going to get involved in a public brouhah. Now, they are saying, you see, we told you.

It is not easy. It is not a motherhood issue. I think that it has to be made a legitimate activity within the scientific community and those people who do it have to be protected in some way.

Mr. THORNTON. It was stated by Dr. Stone that this history confirms that there are enough public-spirited biomedical researchers in the community to assure the society that new and potentially hazardous lines of biological research will be brought to public attention.

It is certainly true that in this instance there were sufficient. Do you have any comment with regard to the exchange that has just occurred as to whether there might be any tendency on the part of scientists to not focus attention upon their problem areas as a result?

Dr. STONE. I agree with the statements just made on this. I see clearly, in consulting with my colleagues many of whom were involved with that original letter, that some thought the danger greater when they started than they think it is now.

I still assert that there are enough scientists to bring such issues to public attention. I think there will aways continue to be. But I also would stand by a statement that I made in my testimony that some way has to be established to amplify the voices of those who do speak up. You may not always have a large core of scientists who are determined to force an issue to the public attention.

Mr. THORNTON. How about protecting an employee or technologist or scientist, employee who blows the whistle on a project that he feels is dangerous?

Dr. STONE. I think this is not the main problem. We are not talking about the level of the lab technician who says "they are not following P-3 procedures here so I am going to complain to the university safety committee."

We are referring to the scientist who says I have just made a very important breakthrough and this could have far reaching effects and implications. He may ask himself the question, not am I going to be censured by my peers for raising it; am I going to be thrown out of my job for raising it? The question that is facing the scientists in my organization is do I want to spend 2 or 3 years away from my research at my most productive time arguing my position.

If I become the champion of the whistle blowing operation, will I then be committed to be on every TV show and to end research in my laboratory for a very long period?

Mr. MCGOWAN. I would like to point out that the history of this is very interesting. I refer you to an article in Atlantic on "the science that frightens scientists." It points out that the first awareness of these issues was by a scientist who accidentally learned what was happening in another scientist's laboratory as a result of a graduate student coming from one scientist's laboratory to another and telling him what was going on. That second scientist made a phone call to the first scientist and said you can't do that, that being putting SB40 virus into E. coli and the response from the scientist who was about to do it says you are crazy.

It took 6 months of discussion—that was in 1971—for the first scientist to realize that this was in fact a dangerous procedure.

For example, the preemption issue is one that cannot be avoided. But it seems to me in the long run, I think the real dangers are going to be ones that we may have given little thought to, and perhaps cannot influence at all.

They will not, perhaps, be problems of accidents, but of deliberate misuse. In the nuclear reactor case, it is very interesting that the debate has moved on from concern about accidents in reactors to worrying about terrorism and proliferation. This is analogous to saying, in recombinant DNA, that perhaps the problems are deliberate misuse by malevolent scientist or by military establishments.

But these problems could not be resolved, I would say, short of stopping DNA research, which nobody has proposed and which I am not proposing here. As often occurs in these cases, there are comedies of misapplication of concern.

One must, therefore, deal with present problems which confront the Congress, and then be alert to the possibility that as fashions change, completely new problems will come up.

Dr. WENGERT. It is important to recognize that in the research process, certain activity must go on at a level of secrecy, partly because the researcher is not quite sure of where he is going, and also, because some scientists have experienced the theft of ideas.

For that reason, there may be more secrecy than needed. The point at which an issue is brought to a review board or presented for some open discussion becomes an important policy issue.

A second point relates more to the time question than to timing. As a result of both atomic energy research and space research American science has become crisis oriented.

Perhaps we need to try—I am not sure we can—to go back to a more leisurely approach to some of these research problems so that the adequate discussion can occur, so that interaction can take place.

I get a feeling as I read hearings involving a variety of research that all are presented as crises. It is encouraged by—with apologies to you, Mr. Chairman—the Congress, and by the Office of Management and Budget.

The Federal agencies tend to make grants for only 2 years. But in some fields research funding should be assured for 5, even 10 years, so that the intellectual processes and interactions can go on.

The 2-year limitations of NSF are a mistake, even when the expectation is that a grant will be subject to renewal. The pressure, therefore, is to be dramatic, to take shortcuts.

I think this needs to be looked at. It seems to me that this time pressure on the researcher should be relieved.

Mr. THORNTON. Thank you, Dr. Wengert. Dr. Trumbull?

Dr. TRUMBULL. I think in this instance, the scientists have been very responsible. They were the ones who raised the question. They tried to the best of their knowledge to place physical and biological restraints upon research.

Mr. THORNTON. May I ask a question there? Do you think that the response which has flowed from that activity—and I agree it was very responsible activity on the part of the scientists—will be encouraging to other scientists to raise similar issues later on, or has it gone beyond what they expected?

What advice would you give to those people who must make decisions now as to whether we should await additional scientific facts? What degree of caution is appropriate for us to exercise in this circumstance?

Ms. Nelkin?

Ms. NELKIN. I think the issue has to be defined in terms of what kind of controls should be exercised and not what kind of definitive judgments can be made at this point about safety or about future benefits. For we really are not in a position to judge hypothetical risks or benefits with any kind of certainty, and if we were, I think the questions of authority and regulation would be obvious.

One of the interesting things that struck me in the recombinant DNA dispute is the comparison between the science court model and the citizens court model that evolved with the Cambridge Review Board. I think this poses a very interesting contrast in quite different modes of approaching the question of decisionmaking authority.

Mr. THORNTON. I agree. There is a contrast there. I wish you would describe that to us.

Ms. NELKIN. The science court—I think most everybody is familiar with this proposal—uses adversary procedures in which scientists air their disagreements. It then seeks to separate facts from values, looking to "scientific judgment" about what is the state of fact at a given time. This is to be the major input into policymaking.

The citizens court procedure in Cambridge involved evaluation by citizens. They took the time to educate themselves about the issue and to make informed judgments as citizens, not experts. This also would enter the decisionmaking process.

There are a number of different conflict resolution models that could be experimented with, but these often involve external controls. Often scientists operate on a set of assumptions based on the situation after World War II. This gave extraordinary autonomy and powers of self-regulation to the scientific community. This is clearly being challenged today, and we need to rethink the question of self-regulation and autonomy within the scientific community. I would hesitate at this point to draw any conclusions, but I think this is an issue at stake in the recombinant DNA area, and it hears on the role of Congress.

Mr. THORNTON. Do the other members of the panel wish to address the question of what shall the Congress do about recombinant DNA?

Mr. McGowan?

Mr. McGOWAN. Well, I think that the first realization is that it is extraordinarily complex and that it is going to take a fair amount of discussion and controversy in order to ultimately come out with something that protects the public.

I am not talking about the dangerous organisms that some people feel could be created. But, I am talking about some of the long-term impacts of this kind of research. Here, I would like to point out that there has been a tendency to think that recombinant DNA research is important because of the short-term benefits it is going to yield; that is, nitrogen-fixing bacteria, cheap production of insulin, and so forth.

The more one investigates this—and I refer you to an article in the most recent issue of Science magazine, May 6, which talks about the possibility of developing nitrogen-fixing bacteria—it is a lot longer off than we think.

people disagree, it is not necessarily that one is right and the other is wrong.

It may be that both are partly right and partly wrong. I don't want to oversimplify, but I regard advocacy as being useful in terms of focusing interest and attention on problems, but if we ever allow the results of the advocacy proceeding to determine questions of scientific fact, then we have moved the determination of scientific fact from the laboratories and the experimenters into the hands of the courts.

I don't think that is where it can properly be explored. I think it is useful as a tool for developing interest but I don't think you can determine a scientific fact on the basis of an adversary procedure.

Dr. STONE. None of the issues on which our federation has worked in the last 6 years—and we work on about 10 a year—turn on narrow scientific facts. After all, where the scientific facts are well known, there is no political controversy about them.

All the issues in controversy therefore fit on top of a stipulation of human facts. Where the scientific facts are not fully known, they will, it is true, not become known through advocacy. But neither will they suddenly become known in the laboratory. The real controversy, we find, involve the kind of issues about which Congressmen make judgments, and have to make judgments.

Scientists provoke these controversies by their discoveries but do not have the answers for them. The search for scientific truth and the search for political solutions are quite separate.

Mr. THORNTON. Mr. McGowan?

Mr. MCGOWAN. I agree with almost everything that Jeremy said, but I do think there are other organizations which have tried to put forward information in a dispassionate manner. [Laughter.]

Mr. MCGOWAN. Jeremy is well known as a promoter of his organization and I give him full credit for that. I do think, however, that conflict and controversy are important and I totally agree with you, Mr. Chairman, that you cannot decide questions of fact in an advocacy procedure.

I take it that you and I would agree, therefore, on our disagreement with the science court idea that has been proposed?

Mr. THORNTON. I think the science court has potential for a useful function limited to exploring issues, developing them, getting issues and ideas presented but not in making final determinations.

Mr. MCGOWAN. I would agree with you absolutely because if I can refer to the seminar on recombinant DNA which we held on December 14, one of the reasons it was interesting to get that group of people together is that they really wanted a chance to ask each other questions in a way that would be useful to people other than themselves.

Dr. Maxine Singer, one of the participants pointed out that Dr. Sinshimer makes a unique argument about evolutionary problems. Then she went on to discuss some of those evolutionary problems from her perspective.

The point was that unless the question had been asked, there would not have been an answer. Unless the controversy had existed in general, there would not have been the set of experiments, the guidelines which are now being developed by the National Institutes of Health.

They put this simulator in the hands of extension service people. We seem to have lost for some reason the capability of the individual scientist, to get up and tell the public something.

I am not convinced that we have found the best answer to this. But you don't find an automotive engineer selling an automobile. You find somebody else a singer or actor. We might start looking for those translators of science, between scientists and the public.

Mr. THORNTON. You find someone with whom the public can identify.

Dr. TRUMBULL. They have an image from some other point which has been established as with the extension service people.

Ms. NELKIN. I am not sure that the problem is one of translating science to the public. There is something that is missing which I think Mr. Thornton tried to get with his original question. I think we need to think about reformulating some of the questions we ask about technology into political terms because there are real political choices that cannot be dictated by technical experts.

Mr. THORNTON. I want to thank you for that observation. That is indeed what I was trying to reach toward.

Ms. NELKIN. One of the things I think we must try to do in this respect is to establish mechanisms that will enhance a sense of trust, or at least avoid the mistrust and hostility that I often sense during my research on controversies in this country.

We need public trust in institutions so that even though there can never be unanimity in these issues, people will accept decisions, even if they disagree with them, because they will know that in other cases they will have an opportunity to at least express their own concerns.

Mr. THORNTON. Dr. Wengert?

Dr. WENGERT. On the issue of conflict and disagreement, I want to tell a little experience I had. I was invited to address the 7th annual conference in New Mexico on land use planning and control. As you know, this is a hot issue in many States. I was the last speaker of a 2-day conference. It was obvious that the ranchers were not about to accept planning or land use control. There was great hostility, much conflict. But they were a little uncomfortable with this. I think many Americans tend to be uncomfortable with conflict. But we have to recognize that conflict is necessary and important. So I told the conference not to be upset by the fact that there was a great deal of conflict because I could think of only two situations in which there would be no conflict.

One is if you were dead, or second if you were living in a totalitarian regime. Conflict is really what makes our system operate. While we hopefully don't get nasty, but even in some situations that may not be inappropriate. In my statement I quote Justice Douglas to the effect that speech can sometimes be irritating and nasty and yet it may still be very valuable—even irritating free speech has a valuable function.

So I agree fully with what you have said in this regard.

Mr. THORNTON. An early American patriot said democracy is like a raft, it never sinks but you always have your feet wet. I think there is a lot of truth to that, a system which is flexible, which rides with changes and accommodates different views, yet which forms a pretty solid basis.

Extension Service developed a citizen education program which contributed to attitudes of the public on these issues being significantly changed.

School consolidation became a reality in Iowa long before it did in Wisconsin or in Michigan, Minnesota. The other experience to which I would call your attention is the Corps of Engineers planning for the Susquehanna River. It was decided to formulate not a single plan, but three plans which would then be presented to the people in the Susquehanna Basin.

But it was apparent that the people did not know enough about the alternatives and thus could not make intelligent choices.

So the Corps contracted with the University of Michigan to organize a series of workshops with the objectives of making the public aware of what the choices were. Funds for workshops in all the counties of the Susquehanna were not sufficient, so five counties in Pennsylvania were chosen, and later on New York State made money available to replicate the experience in several New York counties. The Federal and State agencies involved in the development of these three alternate plans participated in the workshops which met in local communities.

One plan was to accomplish the most contribution to the gross national product. A second plan was to accomplish the most for the regional economic development, and the third was to accomplish most in terms of environmental protection.

These were the three basic alternatives. I think we could have told the Corps ahead of time which alternative the people would tend to favor. But that is not the significant point. In a democracy, it is important that the people feel that they participate in decisions. These workshops, then, provided for a kind of public opinion crystallization based upon the people having information about alternatives.

Mr. THORNTON. May I put a question to you at this point?

Dr. WENGERT. Of course.

Mr. THORNTON. Is there a danger that what we are seeking to express here is that in a concern about decisions of science being made by a scientific elite, that we need to involve the people in that decision but before we do, we must make sure that they become members of that scientific elite?

Dr. WENGERT. This is a problem. In the Susquehanna case labor was not represented although invited. Obviously, though, labor had a stake in Susquehanna development.

How is one to deal with such a situation? My answer is that you can't use participation as a substitute for professional responsibility. My suggestion was that the Corps had to attempt to articulate what the interests of labor might be since labor was not willing or able to speak for itself.

There is the silent majority—which is partly a question of span of attention. None of us can attend all of the meetings that affect us. There is just no way. When our children are in school, we go to the PTA meetings. As soon as they are finished with school, we no longer attend. And yet public education is as vital to me at the age of 60 as it was when I had three kids in school. What psychologists call span of attention is a part of the problem.

Mr. THORNTON. Mr. McGowan?

to be preceded by the publication of so-called policy intentions. These deal with broad issues such as the objectives of economic growth, the rationale for specific projects, various alternatives, likely impacts. These statements are distributed very widely for public criticism.

The public response goes directly to a representative council that includes workers and members of voluntary organizations. They conduct hearings, analyze the public comments, and make recommendations which then go back to the public to make sure that they were not manipulated in the course of summarization. The response then goes to the Minister who has to respond to the criticism or else adjust his plans. Eventually Parliament makes the decisions.

In Austria, the Government's response to the nuclear debate resembles the science court procedure proposed in the United States, but with interesting variations. After an active nuclear protest, the Minister of Industry organized a procedure for public debate among scientists, intended to create an informed public opinion. Seeking to reflect opposing points of view, the Minister asked the scientists who had most strongly expressed their opposition to nuclear power to prepare a list of the questions that they felt must be considered prior to developing a nuclear program. Then he appointed teams of experts equally divided between supporters and opponents of nuclear power to prepare information on the most controversial issues. These are discussed in televised debates. To prepare the public to follow the discussion, the Ministry circulated a brochure defining technical terms. There is an opportunity for public response. The final report is intended to clarify which issues in the scientific debate are resolved and which remain controversial.

As in Sweden, increased information tended to increase conflict. However, Austrian officials feel that these debates will reconcile conflict by demonstrating publicly that experts can state the limits of their competence.

These experiments take place in the context of participatory efforts in other sectors, particularly in science policy. For example, a Swedish Commission concerned with lack of representation in the councils that establish research priorities proposed a new structure. This has two administrative levels that differentiate between research of "social relevance" and of "scientific relevance." The former is run by a coordinating board dominated by public representatives; it initiates and supports research in the category labeled social relevance. The council responsible for research of "scientific relevance" will be dominated by research community representatives. However, they must also draw upon outside evaluation groups.

In the Netherlands, the Minister of Research has argued that in establishing research priorities, the Government cannot fully interpret social needs and he has tried to create a tripartite system in which research workers, Government representatives, and consumer groups participate in an open planning process within sectoral research councils. These councils outline plans that are disseminated for public reaction as in the case of the policy intentions described above.

Several conditions in these countries converged to provoke efforts to expand public involvement. It was strongly felt that future technological development would require greater public confidence which had to be restored through greater public participation.

Also I believe that one gains confidence from the fact that we find much more arbitration and reconciling of differences at the present time. There was a conflict between timber cutting and the protection of the southern warbler in South Carolina.

It was finally decided to let a panel deal with this and a group of dispassionate scientists, biologists, ornithologists, and foresters became involved and there was a reconciliation.

Each side understood the other's concern and needs. I was going to quote, too, a statement by a scientist teacher who had attended a program under the National Science Foundation Summer Institute. Having been exposed to decisionmakers in the Government he had gone away with an entirely new feeling about how the sciences he was teaching in the school system had to be explained in a better way for the students' understanding.

We must seriously consider what means we have to keep this teacher and others like him informed and enthusiastic about that role in behalf of science. There are not many of those. Dr. Wengert said he was putting these theories to a test. We need much more of that.

We need a picture of the total role of science and how it plays a role in our daily life. This is again a stress upon public education where funds could be better spent in the objective of educating the American public to understand science, to be sure that the science courses on the campuses are aimed toward that citizen much, much more than they are today.

For every scientist and research man coming off that campus, there are 98 citizens who some day have to understand what this is all about. I think we owe them a debt that is being very slowly paid.

I would indicate, too, that this is not simple because the American public has got to be convinced, has to understand the role of probabilities, of tradeoffs. These are not simple concepts to understand at all. There are those who would mislead them into the belief that these major problems are going to be solved by simplistic answers. The only way you are going to beat that approach and the appeal of these people is by educating the American public to a better level than it is today.

We have developed a number of fine information retrieval systems under government expense which we have not begun to use at all in the ways we could to establish a better information base for advisory committees, for review and other groups about which we would talk this morning.

The same is true with the American public. We could do much more by way of retrieving some of this information for their benefit. I believe that there is need for an understanding of the social aspects of this undertaking.

This is reinforcing. Those who would abuse, misuse the public for their own self-interest have developed their skills for molding public opinion to a carefully orchestrated scenario.

The achievement of an informed public to decrease this full vulnerability can only be done through a much improved process of knowledge involvement. This committee can help in bringing about this process. DNA might be a major reason for asking the question but DNA is only the beginning.

It is going to go on and on. There remains some conviction that a public fully informed will accept compromises and forgo some of its

Board. The study dealt with nuclear and conventional powerplant siting. In that study I learned that the State of Washington has a procedure whereby when a permit is applied for for a new powerplant an attorney is appointed as a public defender from the bar at large.

He provides advice—he gets paid for this—and guidance to the groups who want to oppose the powerplant proposal. I think this gives a great deal of order to the proceedings. I would say it does not necessarily have to be an attorney to fill such a role; it would depend on what the nature of the problem is. The case for opposing views is organized on a much more equal basis than is the case, for instance, in Colorado where there are now two very bitter fights going on with respect to powerplant sitings, and the opposition is quite disorganized.

Public interest groups are not coordinated. They don't have the means. So I would suggest you may want to look at this Washington experience. I think it is good that the man is appointed from the bar generally, that he is not a public employee.

He is a kind of a public defender if you will. I think such an approach could be related to, say, licensing or guideline procedures involved with major research proposals in the recombinant DNA field as well as in other fields.

Thank you, Mr. Chairman.

Mr. THORNTON. Thank you very much, Dr. Wengert, for a very fine statement. I appreciate the prepared statement and also the additional material which you have supplied.

Our next witness is Dr. Trumbull, executive director of the American Institute of Biological Science.

Dr. Trumbull, you bear a very distinguished name. We are very pleased to have you in attendance at our hearings.

Dr. TRUMBULL. Thank you. Might I avail myself of your offer to have the statement placed in the record?

Mr. THORNTON. Your statement in its entirety will be made a part of the record.

STATEMENT OF DR. RICHARD TRUMBULL, EXECUTIVE DIRECTOR, AMERICAN INSTITUTE OF BIOLOGICAL SCIENCES

Dr. TRUMBULL. After hearing the presentations that have been preceding me, I have some tendency to say amen. However, let me try to pick up some things mentioned in my statement and maybe elaborate on things not touched upon by the previous testimony.

It is only fair as has been said before that we recognize that we are not starting from scratch. The public has been involved in decisionmaking. There are many ways in which it has become involved. But we cannot become complacent about what we have been doing.

It has not been fair to the public. My major concern as expressed through my paper has been bringing the public into the system much more thoroughly than we have with an emphasis primarily upon educating them and making them an integral part of the decisionmaking process.

It is difficult in dealing with a problem as complex as this when you have an overall objective of public participation and you recognize how many ways the public does get into the system.

made available for the purpose without some indication of the kind of education we expect to get from it will not alone do the job.

Mr. THORNTON. Science education ultimately is of value only as it educates individuals.

Dr. WENGERT. Right. Related to this whole question of citizen participation is a quotation from John Stuart Mill in his "Essay On Liberty." Mill wrote:

> If all mankind minus one were of one opinion and only one person were of the contrary opinion, mankind would be no more justified in silencing that one person than he, if he had the power, would be justified in silencing mankind.

In explaining his position Mill pointed out not that it is important for individuals to "let it all hang out," but rather that it is society that suffers if we do not permit freedom of expression.

Our scientific effort is based on the kind of values that John Stuart Mill expressed over 100 years ago.

If we start with that premise, we do have to recognize the "tyranny of the majority" evident in behavior of pressure groups, even public interest pressure groups. Sometimes the individual has difficulty in being heard, even when he is a scientist, and here I refer to the experience of Dr. Morris Ewing in the development of plate tektonics. When he first proposed the idea of continental drift he was deliberately not invited to read papers on his ideas at scientific meetings. The scientific establishment, too, can be repressive. So for this reason, too, I think it is important to stress the right of the individual dissenter.

Ultimately, because Ewing had good relationships with the Office of Naval Research, he was able to develop the data which persuaded his scientific colleagues.

Today, of course, plate tectonics and continental drift is accepted and taught. But it was really the struggle of a very few distinguished people who made that possible.

In dealing with public participation we have to recognize some of its very serious limitations. Citizen participation in science policy and practice cannot be thought of as a mass meeting or a public referendum. Mass meetings and protests such as those that have occurred from time to time in objection to atomic energy facilities provided important signals to policymakers.

There is one of those going on right now in Vermont or New Hampshire. It would be unfortunate if the participants or the targets viewed such events as necessarily reflecting the voice of the people.

Hopefully we do not react like the French politician who observing a mob go by his window exclaimed "Those are my people, I am their leader. I must follow them." We do have to recoguize the need for professional responsibility in our complex world today.

Finally, I discuss in my paper several techniques related to participation in scientific policy development. First of all peer review, which has already been referred to, is essential. But again there are tremendous pressures within the peer-review system which may work against scientific progress.

There is a tendency to play it safe. Peer groups are conservative. They are dominated by those of us who have arrived rather than by innovators, the developers of new ideas.

Next, "sunshine" and openness "Sunshine laws," which are coming to characterize many State activities, and of course the Federal Free-

STATEMENT OF DR. NORMAN WENGERT, DEPARTMENT OF POLITICAL SCIENCE, COLORADO STATE UNIVERSITY

Dr. WENGERT. Thank you, Mr. Chairman. It is a pleasure for me to be here. All my professional life has been devoted to issues of policy development and policy control. My presence here is based on recent experience and research which I have had in the area of citizen participation, particularly related to environmental policy.

My function, I suspect, has been to raise questions and to analyze what often are very superficial statements about the role of the public. Perhaps I am somewhat negative with respect to participation, but if my presentation suggests the importance of careful analysis, as against simply resorting to rhetoric that people ought to participate, perhaps it will be useful.

In my prepared statement I ask permission to introduce into the record as an appendix an article I wrote about a year ago for the Mexico) entitled "Citizen Participation: Practice in Search of a Theory."

Mr. THORNTON. Without objection, that document will be included in the record along with your statement.

Dr. WENGERT. It is clear that participation is used in many ways and for many different purposes. To some, it is a matter of good policy because it represents democracy. For others, it is a strategy, a way of organizing public support, often increasing political power, permitting control of or by the bureaucracy depending on which side one stands.

For some participation is looked upon as a means of communication through which people, specialists, public servants will understand each other better. In the literature participation is also discussed as a means of conflict resolution.

Conflicting points of view will disappear as a result of shared interaction, it is suggested. And for some participation is therapy.

You probably recall the first major thrust for participation was in the Poverty Act of 1964 when the phrase "maximum feasible participation" was introduced into law.

Senator Moynihan has written that as one of the three authors of that phrase he could assert that they had nothing very specific in mind. It sounded like a good phrase when introduced into a draft of the law.

But participation became an important political tool in the hands of minorities and the poor in the administration of the poverty program. But despite authorization in the act, it became very clear that full citizen participation is difficult, if not impossible to achieve.

In one sense, where science is concerned, the issue is one of control versus freedom. This is a very delicate line. On the one hand, you hear advocates of participation making statements to the effect that the public should participate because scientific research policy involves the public and the public should not be deprived of the right to sit in judgment of its own fate.

Statements of that sort, it seems to me, tend to avoid the very complex issues of citizen education which have already been discussed and referred to this morning by other witnesses.

I would like to stress in that connection on the basis of having spent two-thirds of my life as a university professor I must conclude that

I hasten to say that we recognize these groups don't always know what the public interest is and do not claim to. By "public interest" is meant only that they do not have vested financial interest, but that seems to us to be quite a substantial point.

I have one proposal which I would like to put some emphasis on, Mr. Chairman. Perhaps the way to get more study on the implications of science is to require the grant making agencies to devote 1 percent of the funds that they are allocating to scientific studies to the social implications of the grants that they are engaged in putting forward.

If agencies are going to spend large funds to advance scientific research, it seems that related small funds would be desirable to establish what the implications of the research success would be.

Finally it seem to me that the committee could well consider ways of commending the scientists that it feels are doing the right thing and condemning those that it feels are doing the wrong thing, and thus prodding the scientific community.

Thank you, Mr. Chairman.

Mr. THORNTON. Thank you very much for your very fine written testimony and for your very good summary of it, Dr. Stone. I am looking forward to further exchange of questions and answers with you during the process of the panel this morning.

Dr. STONE. Thank you, sir.

Mr. THORNTON. Our next witness is Mr. McGowan. Mr. McGowan, we would like to ask if you would also want to introduce your prepared statement into the record in full and then to summarize that statement?

Mr. MCGOWAN. Thank you very much. I would like to do that. I would also like to introduce into the record a transcript of a seminar on recombinant DNA research which the Scientists Institute in cooperation with the Environmental Study Conference held on December 14.

Mr. THORNTON. Mr. McGowan, I appreciate that. It might be appropriate if—I have just been handed a note that we ought to have a recess for about 5 minutes. So I am going to interrupt at this point before you get into your statement to have such a recess.

Thank you.

[Voting recess.]

Mr. THORNTON. The hearing will come to order. We are very pleased that the weather has abated sufficiently to allow Ms. Nelkin's airplane to make it all the way in to Washington. We are pleased to have you join our panel as we continue to receive Mr. McGowan's testimony.

STATEMENT OF ALAN McGOWAN, SCIENTISTS INSTITUTE FOR PUBLIC INFORMATION

Mr. MCGOWAN. Thank you very much, Mr. Chairman. The thrust of what I have to say is that information is the key to this as well as I think all of the future issues in which the scientific community and the public policy bodies are going to be involved.

President Carter has called for an independent information system to resolve the difficulty and the thorny issues surrounding the development of the national energy program. Surely such a system is also needed for the equally important area of biomedical research.

for example, are very much limited by the number of scientists that will join us.

If we don't take on issues and express views consonant with general concern, we get nowhere in maintaining their support. These public interest groups are related to their constituencies much as Congressmen are.

Second, I think you cannot support citizen education in science without supporting the citizen movement because the citizens work through the citizens movement.

Otherwise it is like saying we want to support the laboring man but we don't want to be associated with the labor unions. But the labor unions, whatever problems they may have, are nevertheless the spokesmen for the laboring man.

Further, it seems that the new tax laws that Congress has passed have confirmed the view that Congress believes that tax deductible moneys can be used for controversial matters.

Mr. THORNTON. If I may interrupt at this point, because I think it might be appropriate to have additional amplification on the record at this point in the discussion as it does relate to a particular subject matter, I have two questions which I would like to ask you to address which are supplementary to the material which you have presented and which you have summarized and are in the process of summarizing.

First, if we assume that Federal support of advocacy groups is a proper function, as in the area of law enforcement where the idea of providing legal aid is an appropriate Federal function, then the question is whether this support is within the mission of the National Science Foundation for supporting this activity or whether it should be accomplished through some more citizen-oriented organization.

The second question relates to your statement about expressing a concern for the proper education of scientists. It does seem to me that that is by far the larger and more important question. As you know, in the program for providing educational opportunities for undergraduate schools, the NSF requested an authorization of $14½ million.

Our committee and the House authorized $17½ million for education of people in science. The other body reduced that to $100,000 for education of scientists. I wondered if, indeed, the education of scientists is not perhaps more important than providing funding for organized groups.

Do you have a comment?

Dr. STONE. Yes, sir. With regard to the first question, about supporting advocacy groups, at the moment, through the tax laws, the public pays 50 percent of all the lobby and litigation expenditures of private groups. Thus, if a Government corporation comes to speak to you about legislative activity, to try to influence your vote on this or that, which is their right to do, or if they sue the Government, since they are in a 50-percent tax bracket, 50 cents out of every dollar they spend on their efforts is defrayed by the Government.

Mr. THORNTON. Assuming they make a profit.

Dr. STONE. Assuming they make a profit. In the case of the advocacy groups, the Congress has already decided that if we give a group a tax deductible certificate to perform some good work, for example,

Mr. THORNTON. Then we will request you to summarize your remarks so that we can get to an interaction of questions on the issues which are raised.

Dr. Stone, we are very pleased to have you with us and we would like to ask you to begin.

STATEMENT OF DR. JEREMY STONE, FEDERATION OF AMERICAN SCIENTISTS

Dr. STONE. Briefly, five lessons of this recombinant DNA experience. First, there are enough researchers to alert the public to new dangers. The biomedical scientists fulfilled their obligations in raising this issue much as the atomic scientists that founded my organization in 1946 fulfilled their responsibilities.

Second, in cases like these it is not enough to ask scientists to cry alarm about the dangers. Instead it is very difficult to figure out exactly what the dangers are and so courage has to be matched by wisdom. Public opinion shifts in emphasizing one danger is another as it has shifted on this question of nuclear reactors.

Third, this history shows that there is great difficulty in controlling scientific developments. Even if it had been felt that recombinant DNA was so dangerous it had to be stopped, it could not have been stopped because it goes forward in other countries.

Fourth, recombinant is unusual in that it is a case in which the scientific research itself could be claimed to be dangerous. In most cases it is the technological developments that are the problem, the problem of society absorbing the scientific advances.

I think that it would be misleading to think that we are going to have a stream of dangerous research projects, but I think it likely that we will have, over the next hundred years, a long series of new developments springing from biomedical research which are going to be difficult to integrate into the life of our society.

Finally the scientific community—which is edgy at the moment about how it is going to be treated by the body politic—should be reassured by this reception. The public reaction to recombinant DNA has been restrained. For example, so far, I think, the actions taken by State and local groups have not indicated that it is desperately important to have Federal preemption, a matter on which our group is not decided.

I should interject that this statement has not been circulated to our Executive Committee or our Council. There is a consensus on most of one major points it contains in our group.

The main question I wanted to address is what actions the Government might take to encourage scientists to alert society to impeding dangers. This is a theme that has run through our Federation's activities over the last 30 years—trying to get more scientists to get into the action.

The basic problem is that scientists in the large think that scientific responsibility means avoiding "irresponsible conduct". This suggests to them that they have to be very cautious and precise in their public statements which really inhibits their activities in the pubic domain where traditional scientific caution would often lead to statements being made only after their usefulness has vanished.

It is too early to evaluate these participatory experiments, but the comparison suggests several points. First, regardless of their technical nature, policies concerning science and technology are increasingly a source of conflict, raising basic questions of value. More technical information is not itself sufficient to change public attitudes or reduce conflict. The usual procedures of policy making about technology, in which fully-formed plans are thrust upon the public as if they are non-controversial technical decisions are inappropriate. A participatory process that realistically confronts the difficult choices involved in technology policy would not avoid conflict, but might bring better focus to the issues of concern to the public, and thereby reduce hostility and polarization.

Second, expertise is a crucial political resource. If an open decision-making process is to be effective, and if participation is to be more than a symbolic exercise, there must be means to improve public access to technical information and expertise.

Finally, the response to participatory demands must vary according to the values one wishes to maximize. A major concern is that greater public involvement may further encumber efficient implementation of public policies. Participation is indeed cumbersome. The Austrian campaign will take three years. In the Netherlands, it takes about a year to approve a policy intention. The importance of an enlightened public and the greater articulation of diverse values that may emerge in a participatory process must be weighed against the urgency

consultative process in which Research and Development decisions are made. Thus, he proposed a tripartite system in which research workers, government representatives, and future consumers of research (i.e., producer organizations, professional organizations, consumer groups, and citizen environmental groups) would participate in an open planning process within sectoral research councils. These councils would advise the Ministry on research policy and outline multi-year plans which would then be disseminated for public reaction.

Concerned that the Councils represent the interests of environmental groups as well as industrial consumers, the Minister also proposed ways to subsidize these groups in order to encourage informed and critical scrutiny of government policy. Two proposals have been considered; to develop a scientific bureau within the government to do research requested by such groups, or to provide the major groups with their own research capacity. Finally, the Ministry has set up projects to teach scientists to communicate their research findings to the public, to train science journalists, and to work with public television.

Analysis

Several conditions converged in Sweden, the Netherlands, and Austria to provoke efforts to expand public involvement in technical decisions. First, it was felt that technological development would require greater public confidence, which must be restored through increased involvement in technical policy decisions. But also, the governments were especially sensitive to criticism by citizen groups because of the delicate balance of

aspects of each theme and these are topics of discussion in public, televised debates now taking place throughout Austria. The Ministry also tried to prepare the public to follow the technical discussion by publishing a free brochure defining technical terms at a level that corresponds to the minimum requirements of public school education.

The Austrian experiment stresses education and the creation of an informed public opinion. A final report on opinions expressed during the discussion will be written by participating experts and submitted to parliament by 1978. The report is supposed to clarify which problems in the scientific debate are resolved and which remain open and controversial. Parliament will ultimately make the decision.

The public debates began in October 1976. As in Sweden, the first discussions suggested that increased information tended to increase conflict. In one of the early debates the audience (mostly anti-nuclear) objected to its orientation, and called for introducing new questions for discussion. The media reported the sharpness of the conflict. Austrian officials, however, remain enthusiastic, intending their campaign to reconcile contradictions between expertise and democracy by demonstrating that experts can publicly state the limits of their competence. This they feel will provide a better basis for political decisions in technical areas.

Participation in Science Policy

Participatory reforms are also underway in other sectors. These include the organizing of local councils with authority over planning and industrial development, democratization of

The Netherlands

As in Sweden, the Dutch Government responded to environmental protest by efforts to increase public involvement in technological planning. In September 1972, the Minister of Physical Planning had presented a White Paper to parliament stating that decisions affecting the environment involve conflicts between economic and ecological interests, and between individuals and the collectivity that are "not so much technical, but of a political nature." The White Paper recommended greater involvement by all affected interests in developing any plan that might alter the environment. The Ministry thereupon set up a system in which all government plans are to be preceded by the publication of "policy intentions." These deal with political and philosophical questions: the objectives of economic and industrial growth, the rationale for proposed projects, and their likely impacts. The statements are widely distributed for public criticism in a process that takes about a year.

First, the Ministry circulates a provisional plan to schools, libraries, town halls, and local newspapers. Information evenings (approximately 40 evenings for each proposal), photo exhibits, expert lectures and television programs are organized to explain the ministerial preference and to present alternative plans. Local governments organize discussion groups to interest "the man in the street." People are invited to send written comments directly to the Minister. All responses go to a representative council that includes workers, employers, and members of voluntary organizations. This Council conducts public

pressures to reevaluate research priorities. It was the anti-nuclear movement that actually triggered the response, for the nuclear power program in each of these countries became a symbol for concerns about bureaucratic centralization, the increasing authority of expertise and the declining role of the citizen. Occurring at a time when the parliamentary majority of the governing parties faced political challenge, the nuclear issue became a key factor in the struggle for parliamentary control. Thus, participatory mechanisms were initiated both to promote public acceptability of government programs and more generally to meet criticisms about governmental authority.

Sweden

In the summer of 1974 the Swedish government decided to finance a major experiment in public education and consultation in the area of energy. The mechanism for such an experiment existed in the "study circles," a system of small study groups managed by political parties and the major popular organizations (trade unions, temperance groups and religious groups). The government provides factual information on the subjects requested by the various organizations, and also funds them to develop their own material reflecting local concerns.

Until 1974, study circles were a vehicle for adult education. The decision to sponsor a large scale study circle program on energy reflected recognition that this area, previously considered within the ministries as a technical

PUBLIC PARTICIPATION: SOME EUROPEAN EXPERIMENTS

Testimony by
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before the
United States House of Representatives
Committee on Science and Technology
Subcommittee on Science, Research
and Technology

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VARIABLES IN PUBLIC PARTICIPATION IN SCIENTIFIC AND TECHNICAL DECISION MAKING

DECISION MAKERS

President
Members of Congress

Executive Agency Directors
Public Response Officers
Division/Branch Directors
Program Directors
  Research Personnel

State Governors
State Legislators
State Agency Heads

City/Town Councils
Mayors/Managers

PUBLICS

General Public
  (a) Interested
  (b) Concerned
  (c) Impacted

Organizations
  (a) Interested
  (b) Concerned
  (c) Impacted

Advisory Committees
  (a) Appointed
  (b) Selected

Peers
  (a) Volunteers
  (b) Nominated
  (c) Selected

MODES *

Letters
Letters to Editor
Editorials
Page Ads
Petitions

Remonstrations
Radio Spots
Radio Rebuttals
TV Spots
TV Rebuttals

Organization
  (a) Publications
  (b) Releases
  (c) Meetings

Provision of Testimony
Participation in Hearings

Discussion Groups

Lectures

NATURE OF PARTICIPATION

Reaction to Item/Action
Anticipation of Results
Recommendation for Action
Expert Opinion
Evaluation, Appraisal Rating
Expert Prediction
Priority Setting
Impact Assessment
Feasibility Determination

* May be self-initiated or stimulated

Richard Trumbull
May 4, 1977

sessions. There have been some efforts of attracting local radio and/or TV coverage along with that from the press. There have not been major concessions to the requirements of these media, however. Publications of the societies have brought the outside world into their pages with limited acknowledgment of legislative actions impacting upon "their world." The same publications, however, have made few if any concessions to the public in the nature of materials covered or the manner in which it is presented. Scientists continue to write for scientists and unhappy is the lot of the editor who would change the code in deference to public understanding of science. This leaves us with the impact of the societies upon public media and the educational system. Few societies have individuals charged with preparing materials or interpreting the scientific products of the discipline for mass media. There have been limited efforts supporting such writing by some foundations. The governmental agencies could and should have been much more supportive of this approach but they, too, have tended to support the rigorous, the traditional world of the journals.

The same defense of that rigor, the disciplinary separatism of science begins and prevails in the academic world. Our present bastion in the battle to prepare the American Public for decision making in science and technology is in the courses of General Science. This is where the sciences must interrelate with each other and relate to the problems of society. Coincidently, this also represents the years of growing awareness on the part of students (our future public) of those problems and their complexity. Individual teachers and individual courses in secondary school and college might continue the understanding of science which begins here, but there are many forces working against them. Individual problems such as that associated with recombinant DNA might perturb the traditional course sequence for awhile. The problem lies in the inertia of the system and the many reasons for its strength and ability to return to its earlier form even when pulled upon for some limited time.

broader spectrum of research than originally perceived. Throughout this process, it is the responsibility of program managers to see the relevance of basic research to their program objectives. The freedom of the investigator to pursue his initiatives does not have to be influenced by those objectives. Like any artist who uses his medium for self-expression, however, finding the market for his product will pose problems.

From a fairly long participation in research and its management, I believe that this element is overemphasized. There are few investigators to whom knowledge of potential values of their work is not a stimulus. There are few who do not keep fairly well posted on programs of agencies as well as progress in their fields. These are vital determinants of so-called "initiatives" for most research. Indeed, I would suggest deliberate program exposure to those individuals funded by an agency. Periodic meetings and discussions for those who are supported under a program to explore the progress and potentials would be far more productive than competitive isolation and reporting solely to their own discipline. This is especially true where the program is interdisciplinary. If the program manager has been successful in assembling the best researchers available for work on the relevant aspects of the problem, he has the nucleus for a "team" approach to problem solution. It could be furthered by personal interactions and communications.

Finally, let me return to the most pressing issue of all - the intellectual capability of the American public to become involved as we envision. This means a delicate balance between domination of advances in science and technology by fear and emotion on the one hand and a return to scientific arrogance on the other. The deciding factor will be the extent to which we raise the level of the public's understanding and appreciation of science. The American public is not so well informed

requires skills not always available. They do develop over time through familiarity with agency programs and policies as well as past experience with the public. One-shot efforts and those involving a major public require additional preparation and skills. There is no magic by which secretaries or other personnel in a department suddenly achieve competence in determining the proper sequence for speakers managing a large audience, fielding or referring questions and, generally, assuring all participants of a satisfying and productive discussion or decision-making event. This will require additional personnel or, possibly, contracting out for such skills where personnel ceilings deny the former. Depending upon the nature and extent of our present commitment of public participation, there can be long-range benefits from development of internal personnel competence. Cumulative experience can contribute to a sensitivity of real value to an agency. Introduction of rating sheets, opinion polls or other mass responses will bring requirements for printing, computer programming and analyses. Again, our earlier determination of just what type and level of participation is desired in the agency will establish the frequency and need for such procedures and the parallel requirement for an inhouse competence vs. a contracting arrangement. Depending upon the level of participation, there are many more bits of information and guidance found in a good procedure than were originally anticipated in the design and competent staff will be the deciding factor.

The recording, summarizing and culling of and from a hearing, meeting, review or other process are basic to implementation, feedback to public and establishment of the history of the action for office records. As we move farther into this public participation, those involved should recognize that they are in an experimental exercise of some significance. Proper recognizing of events, people, and actions will be laying the groundwork for continued improvement in this system and assure a more significant and productive relationship between those in science and the public. It

accepting their level of information and the many influences of others who already have tried to form their opinions through mass media, canvases, polls, and personal contact. Many of this "public" have come with minds made up, and there is little that can be accomplished by handout, introductory remarks, the appearance of experts, structured debate or discussion that will influence either the comments that will follow or their feeling of satisfaction with the results of the meeting. Finally, there is the increasingly employed "going to the public" by departments and agencies through mass media prior to arriving for a town meeting or forum. There are many questions about the use of this technique arising. They pertain to the size of the issue, the competition for the public's time, the optimum use of the official's time, the effect of any excessive repetition, and the specter of media mechanisms submerging the original objectives and becoming objectives in themselves.

We must recognize that today we often find ourselves where the decision maker is not in the rather enviable position of seeking the best research data or the best informed management. Too often, the issue has already gone public and positions have been taken by pros and cons who then proceed to recruit support. How does one achieve public participation in decision making in this situation? Here, our "public" is rather well defined and establishing an environment in which objectivity prevails is most difficult. Getting these interested parties together for a review of the variables and the consequences of optional responses requires the greatest of management skills. It can be done, however, although the greater the prior public display and the stronger the position taking, the less likely is any perceived "retreat" from a position to be accomplished. The Natural Resources Council of America in recent years has become such a forum where people with conscientious concerns over conflicting interests have learned to hear the other side and reach "areas of agreement" prior to appearing before committees or other decision-making bodies in Congress.

task. Even with small, peer groups, we have learned that the scope of programs/policies and their impacts often leave some information or viewpoint unrepresented. The staff work required to update participants or compensate for oversights can be extensive. In this matter, relatively little use is being made of many data and information systems which have been developed over several decades for such purposes under government funding. Among these resources are the Biosciences Information Service, MEDLARS, TOXLINE, Smithsonian Information Exchange and the Chemical Abstracts Service.

Let us return for a moment to discuss how we get our "public" assembled for participation. What is the nature of the announcement of the meeting, review, forum or seminar and where has it appeared? This is most critical because we do have a target "public." What are its usual information media and means of communication? How does the effort to honor the commitment to public participation appear when the department involved employs the Department of Commerce Bulletin, the Federal Register, or the Congressional Record for communicating its needs? Does the government intend to sell subscriptions to its publications and change the reading habits of its publics or will it evidence its sincerity in utilizing other resources? The same question will be raised about the lead time provided in advance notices.

of these roles: advisory boards and councils, public forums, meetings, surveys and program studies, paraprofessionals and volunteers.

The public also becomes involved at levels outside of agencies and departments. This hearing and the willingness to receive testimony from four quite different orientations represents one. Attempts at reorganization in Congress reflect a growing awareness of the limitations of time and expertise available to any one member of Congress. However, not many days can be devoted to any one issue as the multitude of requests to testify are honored and/or experts are called in to assure coverage of all sides. Weighing the validity of arguments by opposing and equally qualified experts is difficult enough in a scientific meeting of peers. It becomes a matter of concern, then, as to how members of Congress can assure the most productive use of their staff and their own time for getting the facts. I believe that one very effective method was demonstrated in the earlier AAAS-Brookings Congressional Seminars where indepth exchange was possible with individuals selected for their expertise and objectivity. The AIBS has tried to maintain some of this provision of testimony by producing "A Guide for Providing Scientific Testimony" and encouraging its 53 member societies to assume a responsibility for informing congressional decision makers of relevant progress and problems in their disciplines. We now have a network of 50 state representatives who translate the needs for such testimony on federal, state and local issues into action. This has been our acknowledgment of the immensity of the problem now facing legislative decision makers in an increasingly complex world. It is our approach to providing Congress with the best information our science has to offer, without bias and without position taking. We share the testimony and the ultimate actions among our representatives so that the cumulative experience assures improvement in the process.

However, let us recognize that there is a cost to this process. Let us also recognize that there is no way, including such public participation, to assure us of no ill effects, no risk or no danger. The recent accelerated pace at which we have discovered long-term effects underscores the futility of seeking a decision-making process that covers all of the bets. Nor is there any process which absolves decision makers from the responsibility for decision making. We are talking about increasing probabilities: the probability that the greatest number of people will benefit, the probability that science or technology will advance faster, the probability that there will be a cost benefit, et cetera. Certainly a major determinant of the quality of public participation in decision making will be the public's acceptance of this fact of probabilities. Their participation in decision making is hampered just as much by expectations of a "fail-safe" life as it is by retention of the old ways which are "tried and true." We are here today because the old ways are being tried more and more and are proving to be fallible.

There are additional constraints which must be recognized and understood by our publics. These include ultimate responsibility and, even, liability for decisions made. There must be no question about the advisory nature of their role and the fact that someone else has the position of secretary, program manager or department head with attendant authority. Unless this working relationship is established clearly, the public's role can degenerate from cooperative problem solution to contentious, protection of self interest. I believe that it is important to recognize our potential here for setting up a new derivative of an all-American past time - kibitzing. Part of the fun in sports events for the non-participant, the non-professional, is telling the quarterback when to run on fourth down, the hitter in baseball when to swing away, the basketball player when to shoot. We also enjoy

or the least informed would suggest that we are ready for public participation at all levels of decision making. A primary concern must be our all-to-evident failure in the past to provide the information base which the public requires for such participation at any but the lowest levels or in development of the most general concepts. We shall discuss the resources for establishing that base later.

There are those who contend that this thrust for public participation represents a distrust of science or a latent anti-intellectualism. There has been an increased sensitivity to the potential of science which some factions would eventually exploit if we do not provide a better educational base for understanding of science and how it operates. Fears about the potential and products of science have been evident throughout much of the history of man. Their expression usually took the form of deterrence or prohibition although it often resulted in loss of position and, occasionally, a life. Much of this history reflected the relative ignorance of the "public," a fear of the unknown and/or resentment of the intellectual discrepancy. We need our social sciences to tell us just how far we have progressed from those days. I suspect that they would find continued attraction revulsion/ambivalence pertaining to the unknown and supernatural today. They would find demographic and ethnic foci of distrust of science. In essence, they would find that we have not come as far as is necessary to enter this discussion without all due caution. In other words, we must wend our way slowly and deliberately into the world we <u>desire</u> to achieve - to foster and ensure increasingly responsible roles for the public in scientific and technical decision making. With that as our ultimate goal, we must determine promptly the levels at which certain parts of the public now can participate predicated either upon present knowledge or that which can be provided within the time frame allowed. Thus, we always shall be returning to that major requirement for democracy, itself - an informed public.

TESTIMONY PROVIDED BY

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FOR

SCIENCE AND TECHNOLOGY COMMITTEE

is easy; with respect to both it is the *seeking* that makes the difference, even when it is recognized that we often fall short.

Citizen involvement and public participation must also meet the test of public interest. This is why this article has stressed the need for a theory of participation which can be related both to normative and empirical conceptions of our democratic system and integrated with American pragmatic experience.

lative, executive, and judicial, functional distinctions coinciding with allocation of authority to the three branches of government established in the U.S. and state constitutions. To these three functions Almond and Powell have added three more: "interest orientation, interest aggregation, and communication."[20] This sixfold classification of functions becomes the basis for analyzing the conversion processes of the political system which transform the inputs of demands and supports into program and policy outputs representing extraction from the system, distribution and redistribution within the system, negotiation and the like. Such a systems model is far from simple, but it may be useful in deciding the nature and role of participation and in distinguishing types of participation needed and desirable at different process stages. It seems clear, for example, that participation in the formation of new government structures, new programs, and new policies will vary from participation in the execution of generally established programs and policies. Although the distinction between "policy" and "administration" has been discredited in the literature of public administration, since administrators make policy through exercise of delegated authority and by accretion through day-to-day administration, in a polar sense the functional distinction would seem to be useful. One can identify different types of participation in relation to different functions—ranging from mass meetings, political assemblies, strikes and demonstrations (and even revolutionary mobs) to community meetings and formal hearings, where seeking information is a primary objective.

RELATIONSHIPS TO THE EXISTING SYSTEM

It has already been pointed out that public participation, depending on where and how it occurs, implies change and often is a deliberate threat to existing decisional (power) arrangements. No theory or procedure for participation can be adequate if it does not deal explicitly with how participatory processes relate to the formal structures of government, including the regular representative system, political parties, etc. Essential to this problem is the question of majority rule and minority rights. In fact, except in the election of officials (and not always then), it is usually impossible to find majority support for most governmental decisions. Not only is the silent majority a reality—barriers of understanding and interest in this age of specialization are equally limiting. In the absence of general referendum procedures which would be of doubtful utility and with political parties that are not issue-oriented or programmatic, the

20. G. Almond & G. Powell, Comparative Politics (1966).

Participation theory must confront the challenges formulated by Professor Dye.

GROUP THEORIES OF POLITICS

Any theory of politics is a theory of power, its management and use. In separately discussing the three subsets of power theory in the preceding paragraphs it was intended simply to suggest the explicitness with which the concepts of power were dealt with. Group theories also concern power, but, as dealt with by many political scientists, power is the result, rather than the purpose of group behavior; it is the object, rather than the subject.

American political science is pluralist in orientation, and this fits in nicely with group theories of politics and political behavior. Essentially, group theory states that for a variety of reasons, including the desire to be effective, political man in America organizes himself into groups. Political activity therefore involves conflict, bargaining, and negotiations among groups. It is through alliances and alignments of groups that political action occurs. Groups, in turn, are kept from overreaching themselves by overlapping memberships and because new groups can always be organized. Thus, a system of countervailing power serves to check excesses.[18]

Critics of group theory have pointed to the fact that there is a silent majority not represented by the myriads of groups interacting in the political process—and potential groups do not necessarily emerge to balance the situation. Others have pointed to the establishment bias of group theory, suggesting its failure to accommodate change. Still others have challenged the motivational logic of group behavior.[19] Yet the effect of these criticisms has not been to depreciate the descriptive validity of group analysis, but to suggest that group theory is not the "general theory of political behavior" which some had hoped it would be. In any case, theories of citizen involvement and public participation cannot ignore group theory and the research on which it rests because the latter explains a great deal about how the American political system functions.

RESIDUAL PROBLEMS

This section identifies a number of conceptual problems which impinge upon citizen involvement and public participation. The

18. The classic explication of group theory remains D. Truman, The Governmental Process (1958).

19. A frequently overlooked criticism of group theory, using the concepts of economic utility analysis is M. Olson, The Logic of Collective Action (1965).

Lynd. But the emphasis in these early studies was less on political power than on a portrayal of a cross-section of local culture.[13]

Following publication of Floyd Hunter's *Community Power Structure*[14] after World War II, attention was directed to decisionmaking processes within a community and to the role of those who were designated "The Influentials." From the point of view of citizen participation, the importance of Hunter's study is perhaps that those who ruled "Regional City" were not politically accountable. The power structure described by Hunter was hierarchical with the social, economic, and political life of the community being dominated by a relatively small and homogeneous group of influentials.

In the early 1960's a number of political science studies of community power challenged the Hunter thesis and suggested that power in American communities was shared by a variety of elites with varying interests and that their power was effective only in certain areas of community policy. This pluralistic view of community processes was formulated in Robert Dahl's *Who Governs*.[15] From the debate between class-oriented sociologists and pluralist political scientists arose efforts to synthesize results of many studies and to develop a comprehensive theory of community power. But these efforts have not been entirely successful, and some significant gaps in the theories of community power remain. One of these, particularly relevant to this essay, is the failure generally to deal explicitly with the question of citizen participation as it relates to community power structure. This remains a challenge to anyone seeking to formulate a theory of participation.

The Governing Elite

As indicated in the discussion of community power, elite control may be inferred from certain formulations of how community decisions are made. But in addition, the annals of political thought contain a wide range of material dealing more directly and explicitly with the role of governing elites. Thus, an issue of the *American Behavioral Scientist* devoted to the topic of "Elite-Mass Behavior and Interaction" began with the editors' axiomatic declaration:

> In all societies, and under all forms of government, the few govern the many. This is true in democracies as well as in dictatorships. . . . Because the symbols and concepts of American politics are drawn from democratic political thought, we seldom confront the ele-

13. For a review of the community power studies *see* W. Hawley & J. Svara, The Study of Community Power: A Bibliographic Review (1972).

14. F. Hunter, Community Power Structure (1953).

15. R. Dahl, Who Governs? Democracy and Power in an American City (1961).

political process have oscillated from a view regarding the government as "they" to the alternate view of regarding the government as "we." The Declaration of Independence, at least insofar as government of the Colonies was concerned, moved in the direction of "we" —suggesting the linkage between free and independent men and self governance. The Bill of Rights in the first ten constitutional amendments was premised on a "they" concept of government—one that had to be controlled by laws, one that could not be completely trusted to guard individual liberties.

This ambivalence continues to be an important aspect of American political behavior, just as the absence of a fully developed theory of American government continues to be unavailable. The best that has been done has been done to analyze processes of politics, administration, and government as a basis for formulating from such observation political theories that attempt to characterize actual political behavior.

At the same time, as the scientific method has come to dominate the study of politics (and of society), a different kind of theory seeking to order and explain processes and phenomena has begun to develop. In some areas such theory has also been subjected to empirical tests. But it is clear that we are far from any general theory of politics. And even at the middle range level a great variety of unintegrated political theory is available for scholarly application.

This brief characterization of American political theory has been introduced to provide a backdrop for a review of the status and development of political theories relevant to citizen involvement and public participation in governmental processes.

THEORIES OF REPRESENTATION

Problems of the relationships of government to the governed are not new to political philosophy. Two aspects of these relationships were well-developed over the preceding two centuries: one concerns systems of representation, the other questions of control. Both were recognized in the Declaration of Independence; both were important issues at the Constitutional Convention. One of the most thorough examinations of the subject was John Stuart Mill's essay *Representative Government.*[11] Early in the present century, Guild Socialists in England and Syndicalists in France, searching for an alternative to geographic representation, concluded that functional representation would more adequately reflect popular interests. A few attempts at functional assemblies were made in Italy and France but were clearly

11. J. Mill, Representative Government (1949).

For Jean Jacques Rousseau[8] the answer was simple: democracy could only exist on a face-to-face basis, such as he found in the Swiss Cantons and as existed in New England towns. Representative government to him was not democracy. And this view is implicit in the position of those arguing for increased community control—of schools, of police, of planning. But such advocates, like Rousseau, usually neglect the issues of intercommunity coordination and of resolving policy conflicts in the larger communities—cities, counties, states, regions, and the nation.

Professor Herbert Kaufman, reviewing American political and administrative history,[9] has suggested that the current concern for greater participation illustrates a theory he advanced some years ago that the nation oscillates from one to another of three dominating concepts with respect to public service: 1) a search for representativeness; 2) attempts to secure politically neutral competence; and 3) desire for executive leadership. In Kaufman's analysis, the current period is not unlike the Jacksonian era (1828-36) when the search was for greater representativeness. Not unlike today, the idea of career service was challenged, and a dominant view was that every man could handle the tasks of government administration. Frequent rotation in office was considered desirable, with the result that a wide list of officials was required to stand for election. Some 80 years later a similar search for representativeness and popular participation led to the initiative and referendum, the recall, local home rule, and women's suffrage. Kaufman's structuring of history does not take into account social forces which may have caused or contributed to the oscillation from one set of attitudes and demands to another. This is not the place to analyze the validity of his analysis nor to expand on it to suggest some elements of social causation. But one might note that the times in which the demand has been for greater representativeness in the governing process would appear to have been periods of substantial social change with accompanying turmoil. Times in which the demand has been for executive leadership has been characterized by acute social problems, e.g., war, depression. And times where the clamor has been for neutral competence have been periods of consolidation.

PARTICIPATION AND SOCIAL THEORY

Recent decades have seen the flowering of empirical social theory.

8. Rousseau, *The Social Contract*, in Political Writings 102-106 (F. Watkins ed. & transl. 1953).

9. Kaufman, *Administrative Decentralization and Political Power*, Public Administration Review (1969).

lems. A prime factor in this change situation has been the increase in technology and the scientific basis for decisions, so that the individual has less and less been able to do as he chooses but has instead had to follow the advice of scientists and technicians remote from him psychologically, if not geographically.

To illustrate, 100 years ago the location of streets and roads was largely a matter for local community decision in the framework of local political processes, reflecting the interaction of community interests and local interpersonal relationships. Decision processes and the inputs to them were generally known and understood by the people in the community, even when they did not participate in or were not happy about them. Today, in contrast, the location of roads is generally the result of economic and technical studies and engineering surveys far removed from the ken of ordinary people, with the decision process only dimly perceived and understood by even the most highly educated. As a result, citizens feel excluded from the process as decisions are made *for* rather than *with* them. And where the location of roads and highways is used to accomplish hidden objectives and realize ulterior motives, confidence in the process is truly shaken.

Scientific and technological developments with respect to communications and transportation have contributed to obscuring community boundaries, making it possible to substitute centralized decisions for what once were local decisions. The expansion of government in the past 75 years has probably intensified the feeling of alienation with respect to what government is doing and how it affects particular people. While some technological and scientific developments may contribute to strengthening community ties, on balance it seems reasonable to generalize that today's citizen, no matter where he lives, has lost control of many aspects of his life. In addition, whatever the specific facts, many people *feel* that they have lost such control, even though the actions of government agencies, scientists, and bureaucrats are justified as being for the public good. Whatever program objectives may be, it is often uncomfortable and disconcerting to have others make decisions which the individual only barely understands and which he may prefer to make for himself.

The concept of worker alienation was an important element in class-struggle doctrines formulated by Karl Marx to characterize the psychological state of workers who, he argued, were being exploited by capitalist managers. It was clear to him that workers were not emotionally involved in the productive process and gained inadequately and disproportionately from their inputs. Communist theory

Participation as Strategy

Some advocates of participation approach the subject as a matter of strategy—a maneuver to accomplish other unstated or stated objectives. How participation and the arguments for it are used depends on, among other things, whether one is working from within or from outside the system. For those outside the system "Power to the People" signals major changes in power relationships, if not revolution. For those within the system, such as government agencies and interest groups, participation may serve as a major technique for gaining legislative and political support and legitimation. It is not uncommon to try to interpret the support of large numbers of citizens as equal to the public interest. The use of survey research may serve similar strategic purposes. The agency head who can report that 53 percent of individuals surveyed in scientifically conducted interviews agreed with his position is generally regarded as more credible than his colleague who has conducted no survey. Where the public interest may lie and what should be done about the 47 percent who held other views are questions often overlooked. The situation is not unlike that of the French leader who viewed a mob passing under his window and exclaimed "Those are my people—I am their leader—I must follow them." American politicians and bureaucrats similarly prefer to act from positions in which they feel they have public support. Thus planning for public participation to gain such support is a natural strategy.

Participation as Communication

Some argue for more participation in order to improve information inputs into administrative decisions. Since government is designed to serve people, the views and preferences of people are necessary inputs to responsive decisions. Often, it is argued, the technician or bureaucratic specialist will make "bad" decisions when he decides *for* people instead of *with* them. In this view, questions of how to deal with dissent or with minority groups are usually minimized, and the importance of making choices and of determining how costs as well as benefits should be allocated is overlooked.

Participation as Conflict Resolution

In some situations participation is urged as a way to reduce tensions and resolve conflicts. Underlying this emphasis are assumptions that sharing points of view increases understanding and tolerance and that the very process of involvement weakens a tendency toward dogmatic assertions and reduces personal biases and mistrust. Insofar

been possible to prepare several useful bibliographies on the subject.[2] But much of the literature, especially that related to particular governmental programs,[3] has tended to be prescriptive and hortatory, abounding with rhetoric and polemics and resting on unanalyzed premises and assumptions. Much of the literature, too, has dealt with the subject of participation as though it had never before been the subject of intellectual attention and as though it bore little relationship to earlier streams of political thought and analysis, as well as to empirical social research.

Among the reasons why the recent emphasis on public participation in the United States has received minimal analytic or theoretical attention is that criticism of participation grates on the ears of many Americans. To suggest that the process, role, and function of public participation may require specification and may even be subject to limitations is regarded as a denial that all men are created equal and construed as a challenge to the very foundations of American democracy. Like secret caucuses, racism, or socialism, expression of doubts as to the general appropriateness and applicability of participatory systems are labeled unAmerican—even by intellectuals and academics.[4] Political leaders, bureaucrats, and others who must face the public and need its support are especially reluctant to criticize public participation or to examine its premises or applications for fear of being accused of undermining cherished traditions.[5]

It is the objective of this essay to review some of the conceptual

2. Three of these bibliographies are: J. May, Citizen Participation: A Review of the Literature (Council of Planning Librarians, Exchange Bibliography 1971); U.S. Dep't of Housing and Urban Affairs, Citizen and Business Participation in Urban Affairs: A Bibliography 3 (1970); Marshall, *Who Participates in What?*, 4 Urban Affairs Q. 206n.2 (1968).

3. Many titles related to particular programs might be listed (see bibliographies cited in note 2); the following are illustrative, as are those in other notes: R. Apter, Environmental Planning and Citizen Participation in Colorado Water Resource Development (1971); A. Bishop, Socio-Economic and Community Factors in Planning Urban Freeways (1969); T. Borton & K. Warner, Techniques for Improving Communications and Public Participation in Water Resources Planning (1971); Institute for Water Resources, U.S. Army Corps of Engineers, The Susquehanna Communication–Participation Study (IWR Rep. 70-6, 1970); Institute for Water Resources, U.S. Army Corps of Engineers, Public Participation in Water Resources Planning (IWR Rep. 70-7, 1970); J. Kintel, Organization of Community Groups in Support of the Planning Process and Code Enforcement Administration (1970); K. Warner, Public Participation in Water Resources Planning (Nat'l Water Comm'n, NWC-SBS-71-013, 1971); J. Zimmerman, The Federated City: Community Control in Large Cities (1972); Landstrom, *Citizen Participation in Public Land Decisions*, 9 St. Louis U. Law J. 372 (1965); Wengert, *Public Participation in Water Planning: A Critique*, 7 Water Resources Bulletin 26-32 (1971).

4. The author himself was criticized at a professional conference by a distinguished economist for suggesting the kinds of analysis proposed in this article.

5. A sensitivity to this kind of criticism is indicated by K. Prewitt & A. Stone in The Ruling Elites (1973), in which they suggest an elite theory of government which is clearly opposed to participatory conceptions.

makers including government legislators and executives. Most difficult is the question of how research programs and research results should be related to the public, both specialists and ordinary citizens especially since results of research can harm as well as benefit the public. To state the problem differently, the question is one of how should the public participate or how should the public be involved in determing the research agenda, in reviewing research procedures, in deciding on research applications. Since the public through its government is financing much of today's research the scientist can hardly claim that he is independent of any responsibility to the public. And it is not enough to say that the results of his research are in the public interest and will benefit the public. It is not always clear that this will be the result. The risks can he great and those exposed should be informed. These are the crucial areas of research policy; these are the as yet unresolved questions of scientist-citizen relationships.

imagined, with stolen ideas has increased the desire for secrecy. And sunshine and openess involve complex issues of timing; premature release of ideas, theories, or experimental data could be embarrassing. Finally, sunshine and openess rests on certain assumptions with respect to the capacity of the public to understand and interpret. The frequent failure of the news media to communicate research theories or findings effectively because of oversimplification is but one example of the problems of public understanding.

Press Releases, Public Meetings, Hearings. In some cases these are important to scientific responsibility which can be defined as including the responsibility to keep the public informed, to educate the public. But these techniques have been abused by providing a basis for personal advancement, ego satisfaction, or by being timed in relation to budget or appropriation hearings.

Litigation. Twenty years ago this topic would not have been listed. But recent experience with respect to environmental litigation, based in part or relaxed judicially formulated rules with respect to standing to sue, suggests that litigation can be important in establishing professional responsibility and protecting public interests. Professor Joseph Sax of the University of Michigan Law School, with perhaps typical lawyer's bias, urges litigation as a major technique for keeping public servants responsible.

Bureaucracy. Since government funds a major part of American research, professional integrity and scientific responsiblity might

substitute for careful analysis and responsible professional decision making.

Stimulating support and cooptation of interest groups are well established techniques for influencing public decisions. It is too easy to mobilize phony crowds and to manipulate masses of people to accept such activities uncritically. Thus while recognizing the importance of mass meetings, protests and demonstrations, it is necessary to devise institutionalized means by which views and interests of various "publics" may more systematically be brought to the attention of scientists. Modes of communication and procedures for control must be developed. In its root sense "due process" is as important in the relationships of citizen to scientists (and vice a versa) as in any other field of human endeavor.

Control Techniques

In the following paragraphs some of the principal control techniques will be reviewed. Each of them deserves more detailed analysis, but perhaps this brief review will provide a kind of agenda for further study.

Peer Review. Among the most common controls of scientific endeavor is peer review. It is common to National Science Foundation grant award procedures and is used by other public and private research supporting organizations. Peer review may be anonymous, or it may involve formal committees often of prestigous scientists. The National Academy of Sciences or its committees may perform this review role, and

Administrators and politicians are also concerned with priorities and fund allocations, particularly since they are perpetually confronted with more demands for public financing than can possibly be met. Spending decisions in a nation as large as the United States inevitably involve concerns for where (geographically) spending will occur, as well as for other so-called "secondary benefits." There was joy in Colorado when the decision to locate a solar research facility outside Denver was recently announced. And since we tend to over-emphasize local benefits and minimize local costs, concerns for the adverse effects of nuclear processing and other military chemical activities at the Rocky Flats facility emerged long after the facility itself had been welcomed as a desired addition to Colorado's economy.

Administrators and politicians also often share a concern for special interests and special pleaders, although perhaps for different reasons. Ever since James Madison wrote the Tenth Federalist Paper, it has been a commonplace of American politics to recognize the many groups which make up our society. And while at one time, political scientists hoped that group interaction represented a kind of checks and balances system (what Galbraith called countervailing power), we are today less sanguine about the group basis of politics, recognizing the difficult problems associated with the silent majority and its manipulation. Too often only squeaking wheels get the grease while the work-horses get the shaft--the public interest being obscured in the clamor for public funds!

Modern science and scientific research has in general been based on views like those expressed by Mill. And if one subscribes to these values, as well as to the democratic process as we know it, one must in urging fuller citizen participation deal explicitly with the following questions:

1. The tyranny of the majority;
2. The fact of the "silent majority,"
3. The problems of interest groups and what has been called bureaucratic pathology, and
4. The specific nature, content and timing of public inputs and involvement in scientific research decisions.

To this point the argument has reflect the biases of scientists and researchers, stressing the importance of safeguarding academic freedom and the freedom of enquiry. The issues can and should also be stated from the viewpoint of the citizen, the administrator, and the politician.

An Alternative Point of View

The citizens (or at least articulate members of the citizenry) are concerned that what scientists and the technological establishment do may have spillover or externality effects which will damage individuals, groups, communities, or even all of humanity. Citizens also often recognize that Common Law nuisance doctrines, and Constitutional principles may be inadequate to protect them, and that the newer statutes such as the National Environmental Policy Act, the air and water pollution control laws, and OSHA, are still not sufficiently

constantly in each area of research and perhaps with respect to each research project. The issue thus becomes one of mutual responsibility--the scientist to the public and the public to the scientist.

Obviously, no one would seriously propose a referendum on whether a patient had small pox and should be isolated. The apparent successful elimination of this plague of humanity was not based on citizen participation neither with respect to research on the disease and its control, nor with respect to the desirability of world-wide programs for mass vaccination.

> When I was in fourth grade, Milwaukee experienced a small pox epidemic and one of my classmates died of the disease. The only choice offered was whether to be vaccinated by the Public Health Department or by one's family physician. There was an extensive information campaign in the press, and I suppose we had to have our parents complete some kind of consent form. Whether certain religions objected to vaccination I cannot recall, hut I do know that the law in most states is quite clear that in epidemic situations the police power may override parental desires and beliefs.

The point simply is that the role of the public and of the process of citizen participation must be spelled out in more discriminating terms than are evident in much of the rhetoric on the subject. It is next to meaningless to say that "humanity must sit in judgment on its own fate;" nor does the simple fact that the welfare of people is at stake establish

progress, and to individual researchers--as the debate in the scientific journals over the National Science Foundation's R.A.N.N. program (Research Applied to National Needs) suggests.

In many respects, it is clear, the concepts, the values, and the practice of science and scientific research are logically and fundamentally in conflict with the concepts, the values, and the practice of citizen participation. The challenge, thus, is to establish guidelines and to develop institutions which will reduce the natural tensions between the two processes--research on the one hand and participation on the other--and which will encourage constructive and creative interrelationships and interaction among scientists and citizens.

It has been asserted that NOT to have citizen participation in formulating and controlling scientific research policy deprives humanity of the right to sit in judgment on its own fate. In a more concrete situation a high government official some years ago in discussing cloud seeding experiments stated that the property owner on whose land artificially induced rain may fall has a "right" to participate in the decision on whether such research should go forward. The U.S. Constitution and our principles of law provide simply that if the property owner is unreasonably damaged by government action to the extent that a taking of property results ("inverse condemnation"), he has a claim for just compensation.

But the concern is generally not with rain but with the likes of radio-active fall-out; not with water but with materials or organisms which might harm innocent persons or future generations.

3. Participation as communication (People, specialists, public servants will understand each other better from participatory interaction);
4. Participation as conflict resolution (Conflicting points of view will disappear as a result of shared interaction);
5. Participation as therapy (By expressing their views, "letting off steam," citizens will feel less frustrated).

The article also suggests a basic conflict between a) traditional theories of representative government and the responsibilities of elected representatives and b) extensive unstructured citizen involvement and mass public participation. The basis for conflict, of course, is that in few participatory situations all those affected or with apparent interests do in fact participate and it is often impossible to determine who speaks for whom, and whom to hold accountable. We all like to think we speak for the public interest, but decibel levels are hardly the tests for representativeness.

It is suggested in the article that in some situations the emphasis on participation is motivated by a desire to manage or reorganize the political system. It is sobering to remember that the slogan "power to the people" has been a revolutionary cry at least since the French Revolution in 1789. In reviewing the various theories of community or political power, the article concludes that in all societies the few end up governing the many. Even those who participate and get involved are themselves an elite often speaking for special interests and expressing special values.

contributors Senator Hubert Humphrey and the late Senator Clinton P. Anderson, Dr. Alan Waterman, and the Hon. Arthur S. Flemming.

My presence here today is based particularly on recent research and writing on the subject of citizen participation in policy making, and it is primarily to this subject that I will direct my comments.

But first, as a political scientist, let me pay tribute to this Committee for holding this kind of informational or seminar type hearing. I know that over the years a number of Committees in both House and Senate have used this approach, supported as in this case by excellent background studies prepared by the Congressional Research Service of the Library of Congress. I feel that it is particularly important, at a time when it is often asserted that confidence in government is at low ebb, that the public be informed of the conscientious, scholarly, and detached approach being taken by this Committee in formulating public policies with respect to major issues of science and technology. Public awareness of how this Committee approaches its responsibility would, I am sure, contribute to increased trust in our governmental institutions. And certainly, I am pleased to be a part of this proceeding and thank the Committee for inviting me.

"Citizen Participation: Practice in Search of a Theory"

About a year ago the Natural Resources Journal, (volume 16, pages 23-40) published an article which I wrote entitled "Citizen Participation: Practice in Search of a Theory." This article deals with many of the general issues associated with citizen participation so I want to

1952; Natural Resources and the Political Struggle, 1955; Administration of Natural Resources, 1961; (with George M. Walker, Jr.) Urban Water Policies and Decision Making, 1970; Urban-Metropolitan Institutions for Water Planning, Development and Management, 1972; Impact on the Human Environment of Proposed Oil Shale Development in Garfield County, 1974; Property Rights in Land: A Comparative Exploration of German and American Concepts and Problems, 1974; Public Participation in Water Resources Development, 1974; Land Use Planning and Control in the German Federal Republic, 1975; The Political Allocation of Burdens and Benefits: Externalities and Due Process in Environmental Protection, 1976; also chpts. in other books, Editor: Institutions for Urban-Metropolitan Water Management, 1972; co-editor: The Energy Crisis; Reality or Myth, 1973. Home: 1225 Teakwood Dr. Fort Collins, CO 80521.

extension the problem would apply to all the state university campuses in California as well as New York, which together with the University of Michigan and the NIH make up, I think, a good fraction of where this work is done. So, with all due respect, Maxine, I can't agree that pre-existing agencies are strong enough to regulate what they are supposed to, let alone to take on new responsibility. On the question of licensing of enzymes, it would seem to me that it's quite likely that nucleotides, nuclides, radioactive materials would probably be much poorer in specificity if their sale were not licensed. So possibly licensing is a way to upgrade the crummy corporate production of these enzymes. [Laughter]

DR. SINGER: It's of course feasible to license that because the sources are unlimited. But the fact is that anybody can make a restriction enzyme if they have a biochemistry lab.

DR. POLLACK: I want to have the last word on this because there is an intrinsic danger to using a crummy restriction enzyme. The entire technology is built on the very remarkable specificity of these enzymes for finding specific sequences within DNA. They are not merely scissors, they're aimed scissors, they're targeted scissors. With them, when they work properly, you can cut out known pieces of DNA from within a genome. The moment you have a bottled enzyme which says something on its label but doesn't contain that specificity within it, you are perforce performing a random experiment with this technology. So, the sale of crummy enzymes is in fact perhaps one of the more dangerous aspects of this technology.

MODERATOR RANDAL: Dr. Pollack, you have just had the last word because I've been told we are out of time, and I'm going to turn this over very briefly to Alan McGowan, and then we will adjourn.

But before the panelists leave, if they would be willing to turn in their papers.

DR. POLLACK: I didn't know we were going to be graded. [Laughter]

MR. McGOWAN: It's my pleasant duty to thank the panelists, and I'm stopping this only six minutes late because they have all literally bent their schedules out of shape to participate in this seminar, and I want to be sensitive to that. And thank you all very much. [Applause]

[The seminar was concluded at 12:43 p.m.]

DR. CAVALIERI: What was that number? Could I get that Public Law?

MODERATOR RANDAL: This is Public Law 93-348. That's the Commission for the Protection of Human Subjects—

DR. SINGER: Actually, last year Senator Kennedy introduced into the Senate a bill for the extension of that commission and a revision of its charge. The revision of the charge states that the commission is to look into recombinant DNA. That bill, I believe, passed the Senate in the last session but never did pass in the House, and obviously I don't know what's going to happen to it in the coming session. But I would assume it would be reintroduced.

MODERATOR RANDAL: Next question is: "What sorts of experiments are going on now with this technique? In other words, do we really know who is doing what and on what scale, and at what they are aiming?"

Who wants to tackle that?

DR. SINSHEIMER: We probably don't know everything that's going on. I would say that we know that experiments are going on in at least 30 to 40 laboratories in the world. Primarily, these are experiments to introduce genetic materials from higher organisms into *coli* for the reasons that were mentioned, to enable the isolation and amplification of particular genetic segments with an idea to learn both the organization of the genes in those segments and, if possible, something about the ways in which they may be controlled.

There are also experiments going on of the type that were alluded to a little earlier, to learn whether or not the genes of higher organisms which we now know can be grown and amplified in bacterial cells can be expressed or decoded in bacterial cells. Experiments of that kind are going on. There are also some experiments where one takes the DNA which one has generated in some way, corresponding to a particular gene, and puts that in a bacterial cell in order to grow up enough of it, for example, to do DNA sequence analysis. Those are the most common kinds that I can think of. Maybe others can think of other parameters.

MODERATOR RANDAL: We'll go on now. "Would each panel member please comment about what specific action they would like to see Congress take regarding recombinant DNA research."

Let's start with you, Dr. Sinsheimer. We'll just go up the line here.

DR. SINSHEIMER: For the reasons that have been mentioned, I think there does have to be some kind of legislation in order to make sure that the restrictions apply to everyone and not only to federal grantees. The actions that I would like to see taken are probably threefold. One, to restrict this work to P-4 type facilities. This would require also, of course, some funds be provided to build at least several of these around the country where they could be made available.

Secondly, I think some form—as was mentioned— perhaps of licensing of some of the reagents involved so as to make their availability a little more difficult. Obviously they can in principle be produced from nature, but at least that takes a higher level of expertise.

And, thirdly, I'd like to see, although I'm not sure how you legislate it, some encouragement given towards the carrying out of experiments to assay some of the potential dangers, and to look toward the replacement of *E. coli* with some organism less intimately associated with man.

DR. CAVALIERI: I would agree with what Dr. Sinsheimer said on that point or two. I think that there ought to be regional laboratories. And he didn't say it but I guess it was implied, that there should be inspection, in addition to all the rest of it. And I think that the number of problems that should be worked on in the laboratory should be limited. And what we have now is a free-for-all, which might not be the right way to say it. Anyone can do anything he pleases, provided of course it is within the guidelines. I think that the research should be limited to specific questions and mainly—as I said in my talk—about how we can answer some of the questions about hazards which might arise and not questions about how to make cheaper insulin.

And, furthermore, there really ought to be, I would emphasize, a strong, legislative effort, as far as industry is concerned. I think there's where a lot of the trouble lies.

DR. POLLACK: I think that the Congress ought to enact some form of the guidelines as written as a mandatory set of restrictions on research. I think that the decisions of regulation should be separated

I would agree that there was an effort made to involve the public, but I don't think it was an adequate effort. Those meetings, of the ad hoc advisory committee, I found—although I was a member of the committee—unsatisfactory. There was not enough time. There was no time for the committee to ever discuss the guidelines among themselves. There was no time for the members of the committee, after they had made suggestions, to present or discuss them with the guidelines committee. As far as I'm aware, the guidelines committee made only the most minor and trivial modifications to the guidelines in response to the suggestions that were made. I really don't feel—I have to say personally— that the process was adequate.

DR. SINGER: Bob, the meetings of the advisory committee that devised the guidelines and recommended them have been open. They have, in fact, heard anybody who was ever interested in coming and said they wanted to come.

DR. SINSHEIMER: Maxine, I don't feel that I am very well insulated from the scientific process. I never knew that committee was holding hearings until the meetings in December 1975 in La Jolla were practically finished.

DR. SINGER: The meetings were always announced.

DR. SINSHEIMER: Well, by some mechanism.

MODERATOR RANDAL: They are published in the Federal Register, aren't they?

DR. SINGER: That's right.

MODERATOR RANDAL: But I must say not everybody is a loyal reader thereof. [Laughter]

DR. SINGER: Yes, but presumably if you're interested in making comments, then you find out where the meetings are and you go in to make your comment.

MODERATOR RANDAL: Let's move on to another question here. This is one for Dr. Sinsheimer. And it says: "Do you advocate complete government control of genetic recombinant research based on the model of the Atomic Energy Commission?" [Laughter]

DR. SINSHEIMER: I'm not sure I would be terribly happy with the model. But there are two points that come to mind. I think it's important that, in that model, we have come in time to the stage where we have separated the regulatory and what might be called the promoter aspects of the process—that is, the Atomic Energy Commission and the Nuclear Regulatory Commission are now separate entities. I think that that might well be desirable here, wherein the NIH is both in a sense the promoter of recombinant DNA research and at the same time the regulator.

To what extent the government should be the regulator, I'm not sure. I do think that there should be, as I think was mentioned by Dr. Cavalieri— possibly by Dr. Pollack—some supervision of recombinant DNA research within the containment facilities beyond that given by the scientists themselves. Again, it's the same problem that Dr. Pollack referred to, scientists having to regulate other scientists, which they don't find a very congenial activity. And I do think it is necessary to be certain that the guidelines, whether they are in their present form or a more stringent form, as I would advocate, are in fact carried out with the maximum rigor possible.

DR. POLLACK: One small point. I think it's only fair to Maxine and to the history of the situation to say that the NIH involvement in the guidelines did not arise by a desire to be both a promoter and regulator, but rather because no one else would touch the problem. And now it's perhaps time to have the legislative branch touch it directly. But at that time it was NIH or nobody.

MODERATOR RANDAL: It's interesting to me that, as a member of the press who has reported on this extensively, whoever made the comment that Congress didn't pay any attention is, of course, correct, because this was extensively reported in the press.

Anyway, here is a comment rather than a question, and then we'll go on to another question, unless somebody has a further comment. This is someone who is not an American, and it says: "In the discussion of recombinant DNA research, insufficient attention has been given to the international dimension. The United States cannot take effective unilateral action to control the curiosity of the human race nor indeed to secure its survival."

Here's a question. This is addressed to Dr. Singer. "You say parties agree on registering industry or multinational work. I am concerned about disclosure. The *Washington Post* in late November, I think, reported that industry representatives told the Commerce Department and the NIH representatives

international registries. In particular, the International Council on Scientific Unions, which is a non-governmental umbrella organization for a whole variety of scientific unions, has established a standing committee on recombinant DNA. And one of the charges to that committee is to establish a registry that would be a worldwide registry for experiments that are going on.

MODERATOR RANDAL: Maxine, one thing that I'm puzzled about—in Britain, as I understand it, there's something called an Official Secrets Act that deals with industry, and I'm wonderiug, if the British were interested in the commercial promotion of DNA recombinants and their applications, how one would find out what was really going on in the face of this Official Secrets Act.

DR. SINGER: I think the way the committee there appears to be set up, it isn't clear how extensively the information they gather will be available. Presumably there will be some kind of submission to a registry. But the committee that devised the guidelines in Great Britain did not function in the open, as is the custom in the British Government—that is, it is the custom not to have such committees operate in the open, and they didn't. And there is no indication that their considerations of specific research proposals will be carried out in the open. So that I don't think there is much way for us to know just how much we will know.

MODERATOR RANDAL: Let's go on to another question. This questioner wants to know: "Don't traditional breeding experiments or inducing mutations create new organisms as much as recombinant DNA experts do?" Does someone want to tackle that?

DR. SINSHEIMER: No. Traditional breeding experiments, of course, do promote genetic combinations but only by providing new assortments of the genes within a particular species.

MODERATOR RANDAL: Unless it's between, say, a horse and a donkey, in which case the offspring is sterile.

DR. SINSHEIMER: The obvious thing is that this technique permits crossing of species barriers. That's just a sequitur.

DR. POLLACK: When I was a post-doctoral fellow at NYU Medical School in 1966, I worked in a laboratory along with a scientist named Mary Weiss who, at that time, to my astonishment, constructed a viable eukaryotic cell containing chromosomes from both a man and a mouse. Some of you might have seen that in the *New York Times*. It got on the front page of the *Times* at the time. Walter Sullivan wrote an article about it. That means that for a decade the technology has existed for making hybrid cells in culture. This is not a hybrid organism. This is a hybrid cell; it's viable. That means it produces daughter cells that have genes from two species. I raise this because this novelty in nature was not seen at the time as a threat to anyone, presumably because these cells are totally non-infectious. They're cells. They are not viruses or bacteria. What is remarkable is that the construction of these hybrids has led directly to our ability to map human genes to their chromosomes—and within their chromosomes—so that now man, rather than drosophila or the laboratory mouse, is, in genetic terms, the most well understood eukaryotic organism. This extraordinary advance took really less than a decade. Only in the last five years has it been possible to stain the chromosomes of mammals in such a way as to localize genes within one chromosome, making the full force of this technology possible. And for the understanding and prediction and possible early detection of, and even possible treatment of, many inborn errors of metabolism, many inherited diseases, it's an absolute boon to have the gene map of humans be so finely resolved. This boon derives from a technology which itself includes the crossing of species barriers. There's no way one can get around the fact that a hybrid cell contains genetic information from two different organisms, in some cases moved into one chromosome. So, I want to point out that this rather benign and not frightening technology has existed for a decade and is now in use in many laboratories all over the world without any sign of untoward effects. I don't know, perhaps some day our ecological niche will be disturbed by it, but, given our experience here, it is not clear to me that it automatically follows that crossing a genetic barrier between mammalian species, which normally cannot be crossed in nature, automatically is a disaster for any species. It doesn't follow in this case.

DR. SINSHEIMER: I think the distinction is the obvious one that these cells that you produce in the laboratory are not viable in the long term. They don't continue to maintain a hybrid chromosome set unless you establish some special conditions that enforce it. They are not free-living organisms.

DR. CAVALIERI: I'd also like to comment that that problem is not a problem really. It falls into the category of puzzle solving. And if anything is going to happen with that technology, now would be the time to try to look into that. To map the genes on chromosomes is a perfectly harmless puzzle.

safer manner, using more classical approaches which will provide answers as they have been doing, although perhaps at a slower rate.

And let no one fool you; we can get answers to important biological questions without recombinant DNA techniques. The other proposed benefits are strictly long-range hopes. These include curing cancer and genetic diseases as well as mopping up oil spills, to mention a few.

I've heard scientists testify that to stop research on recombinant DNA would amount to stopping progress toward a cure for cancer or heart disease. These are irresponsible comments which only confuse the real issue. Furthermore, they invite a public backlash against science in that people are encouraged to expect medical miracles in recombinant DNA research. Such hopes are almost certain to be dashed. Indeed, no one can predict when or how a cancer cure will be achieved.

This brings us to the benefit/risk factor which has been discussed here already. If research into recombinant DNA is permitted to continue under the present conditions, successes will no doubt be achieved which will invite applications to plants, animals, and even man. Still the unknown dangers of laboratory accident and human misjudgment will haunt the enterprise. We are talking about making new organisms, new life forms. Do we want to attempt to make new plants for food in the face of these risks?

Who decides whether we like the taste? Who decides whether we even need more food? The answer, "to feed the starving peoples of the world," is pure sophistry. Can we ever hope to feed an ever expanding population? Do we want to alter cattle so as to increase the yield of milk or beef? Do we want to attempt to increase the general level of human intelligence by manipulating human genes, say, in embryos? Who is to choose and by what mechanisms are the future decisions to be made?

What we are talking about is a re-evolution of life on our planet. One does not have to be a biologist to know that evolution involves literally an infinite number of variables whose manipulation by man could easily upset the balance which has taken eons to achieve. Besides, we all know that the variety and complexity of the life already on earth stagger the imagination. We have more genes on earth already than we can begin to exploit. The idea of creating new and better ones is presumptuous and profane.

I'd like to close by making a specific suggestion. In 1973 Senator Mondale introduced a resolution into the Senate. I quote from that: "To establish a two-year study commission with 15 members appointed by the President from a broad variety of disciplines. The commission would study the ethical, social, and legal implications of advances in biomedical research and technology. It would make full use of relevant studies conducted by other public or private groups. After two years, it would report its findings and conclusions to the President and the Congress. Its final report would include such recommendations for action by public and private bodies and individuals as it deemed advisable."

Vice President-elect Mondale is now in an excellent position to implement such a commission. In the meantime, I suggest that recombinant DNA research be limited to a few research centers operating under strict government inspection and control. I suggest that only a limited number of problems be investigated, namely, those which would answer specific questions concerning potential hazards. Experiments involving a crossing of genetic barriers should be banned. We should act before some catastrophe is upon us. Otherwise, we will be forced to rush precipitously into corrective measures and legislation; severe curtailing of research would follow almost inevitably.

I suggest that we would regulate genetic technology more equitably and more successfully if we start now to evaluate its social implications and to monitor its development accordingly.

Finally, I'd like to point out that research on hoof and mouth disease, which causes fatal illness in cattle, has been successfully limited for years to a P-4 laboratory on Plum Island. The cattlemen think it's important research and should be carried on but under conditions which will not jeopardize their cattle. Are we humans not to be permitted the same kind of protection?

MODERATOR RANDAL: Thank you very much. Yes, Dr. Pollack?

DR. POLLACK: I would like to speak to two things Dr. Cavalieri said. Perhaps my objections are based entirely on ignorance, but it seems to me that if you ban experiments crossing genetic barriers, you ban the production of all vaccines, especially flu vaccine, which is a recombinant between a pig virus, a human virus, and a chick virus. And it seems to me you pretty much ban all biomedical research if you are clever about interpreting such a restriction. So, I think this is a good example of what Dr.

Third opinion: The National Institutes of Health guidelines for recombinant DNA research are workable with the cooperation and education of persons responsible for the research. Education and cooperation do not come without effort. I believe it's the purpose of this meeting to decide to what extent that cooperation can come without compulsion and to what extent compulsion of a legal sort is necessary.

Minimally, the effort necessary includes a commitment to open our discussions about proposed work to the general public and a commitment to take seriously the restrictions on free inquiry that are imposed by the acceptance of any guidelines. That is, it doesn't make any sense to say you accept them and then not really work as if you did.

Opinion among scientists as to the effectiveness of the guidelines is divided. I believe that work within the guidelines will be safe. That is, when one works with sufficient physical isolation and when one's plasmids are in an *E. coli* of sufficiently suicidal genetic makeup, then one can reduce the probability of recombinant DNA molecules entering the environment to as low a probability as one wants. Obviously the variable is funding. How much will you — the Congress — spend to do this?

Indeed, in their dependence upon physical isolation and upon the fastidious, suicidal nature of the organism carrying the recombinant plasmid, I think the guidelines have been designed precisely to take into account the absence of an assay for the risk at hand.

Currently the guidelines are advisory and therefore it is up to each scientist who chooses to abide by them to convince those scientists who don't that they must, and to convince those who are against all work to permit work to be done within the guidelines. This constant need to convince is a great strain on all concerned, and it's the main reason why I'm here.

I am aware of a concern that scientists as people must be self-serving and must be expected to argue only for those guidelines that are in their own selfish interests. I cannot see how this is the case, given the intense disagreement I observe within the scientific community. That is, we're all scientists; we're all trying to get information out of this technology. Yet we differ one to the other rather aggressively in our opinion about what should be done. While I am, in fact, disturbed by the intensity of these arguments, I am convinced that they indicate at least that scientists are behaving in a democratic way, as responsible citizens in this case.

This need to convince one another introduces a kind of intellectual hazard which is different from a biohazard but which to my mind is equally disturbing to all stable kinds of research.

Because I believe the guidelines to be adequate, and because I do not wish to see the period of self-enforcement prolonged any longer than necessary, I have come to the conclusion that the guidelines should have the force of law, with requirements for the handling of radioactive material as a model. The guidelines are workable and effective, I think, but also I believe they are necessary, to relieve practicing scientists of the constant need to pass quasi-legal judgment on each other, a process I find to be inherently painful, non-scientific, and certainly more destructive to free inquiry than the guidelines themselves would be if they were law.

My final opinion: The radioactive materials law, at least in the State of New York, the one I'm familiar with, is based on the Geiger counter, which detects radioactive spills by detection of the emitted radioactive particles. We have no equivalent counter for biological hazard. In lieu of any counter to assay biological hazard, the guidelines have provided assays of physical and biological containment. Therefore, they operate under the tacit assumption, that the risk, which is unknown, is likely to be proportional to dose, which is known. This seems sensible to me.

However, making the assumption that risk is proportional to dose implies that you accept the idea that a big dose of any novel organism is intrinsically more dangerous than a little one. As such, facilities that generate big doses—that is, large volumes of bacteria carrying recombinant DNA—are the facilities most likely to provide a risk to the public. Since such large facilities are likely to be industrial and since they are not likely to be supported by federal or state grants, any proposed law should apply across the board to all facilities independent of their support and independent of their purposes.

MODERATOR RANDAL: Thank you. Dr. Cavalieri, may we hear from you before we go on with some more questions?

DR. LIEBE CAVALIERI: I have been involved in molecular biological research for 25 years, but I am not now nor do I ever intend to carry on laboratory investigative work in the field of recombinant

so, the implication seems to be that you thought that recombinant DNA experiments could result in the alteration of the evolutionary process as regards complex organisms. In trying to think through the mechanisms by which that might occur, they are not at all clear to me. I can understand about bacteria, but very specifically, for the implication for the evolution of complex organisms, I think it would be useful if you could amplify that for us.

DR. SINSHEIMER: Let me give a two-part answer to that. I really was still referring in large part to microorganisms and to the fact that you might change these so as to make them into vectors for viruses that grow in higher organisms and so on, and that obviously would have an effect on the higher organism.

The other point which I touched on—I didn't think I made a large point of it—is that I see no reason to believe that in the future recombinant DNA technology would be restricted to microorganisms. There is no reason to believe it could not be applied to invertebrates, to vertebrates, and as you said earlier, even to man. And, indeed, as we know, some of the current experiments are pointed in that direction, experiments we haven't discussed here today which involve the use of oncogenic viruses as vectors. Experiments have been done in the opposite direction to those we have been discussing—genes from prokaryotes have been put into oncogenic viruses as vectors and then inserted into animal cells. That's another class of problem that we aren't really discussing today. But that potential clearly exists, and that's what I had in mind.

MODERATOR RANDAL: Dr. Sinsheimer, perhaps you would tell the audience what prokaryotes are. I think a lot of our audience don't understand the language and perhaps don't understand the term "oncogenic" either.

DR. SINSHEIMER: I'm sorry. I was referring to experiments in which certain viruses known to be tumorgenic, oncogenic viruses such as polyoma viruses, which are known to integrate into the genetic apparatus of cells of higher organisms, are used then as vehicles for carrying genes into the genomes of higher organisms, just as some of the viruses that were previously referred to can be used as vehicles for carrying genes into the genomes of microorganisms. Some experiments of this kind, wherein genes actually taken from microorganisms have been placed, using tumor viruses as vehicles, into the genomes of tissue culture cells of higher organisms. That's a very experimental kind of project at the present time, and those cells are merely tissue culture cells. They are not whole animals. But that's what I was referring to.

DR. POLLACK: It's an important point of fact—and I'm about to reveal my ignorance about it—but as far as I know, while hybrid (animal-prokaryotic) viruses can grow in eukaryotic cells, I know of no published experiment on transformation by them—that is, the stable integration of such a hybrid genome—in a eukaryotic cell leading to the expression of the prokaryotic gene in a stable way. On the way to killing the cells, the viruses may express these prokaryotic genes. But, so far as I know, persistent expression is a branch of this technology which has been successfully inhibited by the guidelines. And if you know of a situation where that has been published, I'd like to know about it. Maxine doesn't seem to know about it either.

MODERATOR RANDAL: I am going to exercise my prerogative to try and get us back on schedule and stop the question period for the moment and ask Professor Pollack if he will speak next and if he will explain in the course of his talk what the difference is between eukaryote and prokaryote because I think we're getting hung up on technical language again.

DR. ROBERT POLLACK: I'm a cell biologist. That is to say, I study eukaryotic cells. Eukaryotic cells are cells with a defined nucleus enclosed within a membrane. The nucleus contains the genes of the cell strung in groups called chromosomes. Prokaryote are simpler organisms and have no visible nuclear membrane; hence, this distinction by Dr. Sinsheimer. But more importantly, prokaryotes have relatively little social life. By social interaction among themselves, eukaryotic cells construct a whole eukaryotic organism, which each one of us is, from a single eukaryotic cell, the fertilized egg.

So, as a cell biologist, my main interest is in the way that eukaryotic cells interact with each other to do such marvelous things as make roses and people. I'm engaged in the study of one minor perturbation of that process of normal cellular interaction; that is, the appearance of a disease in which that interaction breaks down. The disease is called cancer. I work with viruses that cause this disease in animals, and I study those viruses' effects on cells outside the bodies of animals, to try to understand by these simpler systems how the disease arises in people.

a laboratory that's equipped with several hundred thousand dollars worth of equipment and if you have some years of experience in the technical manipulations, it's not a difficult thing to do. I don't think that one can realistically say that an amateur could walk into a garage and carry out these experiments. I think that's just not feasible.

DR. SINSHEIMER: Could I comment on that?

MODERATOR RANDAL: Of course.

DR. SINSHEIMER: I think that it's not that hard to do recombinant DNA experiments. It's much harder to know what you've done, I would agree. To prove that you had made recombinants, and to try to figure out what they were is much harder. But to actually make them by these techniques, is not that difficult.

And with regard to the restriction enzymes, they can be purchased inexpensively. It is true, of course, they can be made. That takes a more sophisticated arrangement than buying them, obviously. And it seems to me that one thing one might want to consider at some point is whether one should license, for example, the sale of restriction enzymes as we do the sale of radioisotopes.

MODERATOR RANDAL: We will turn to a question from the floor. This is one for Dr. Singer. It says: "If safeguarding the public health has been central to the development of the guidelines, why has the recombinant DNA advisory committee of the NIH ignored the original mandated purpose of the advisory committee, as published in the Federal Register of October, 1974 '(1) to investigate the current state of knowledge and technology regarding DNA recombinants, their survival in nature, and transferability to other organisms; and (2) to recommend programs of research to assess the possibility of spread of specific DNA recombinants and the possible hazards to public health in the environment, and to recommend guidelines on the basis of the research results'?" The question goes on to say: "The committee is mandated as a technical committee established to look at a specific problem."

The second part of the question—this gets a little more complicated.

DR. SINGER: Why don't we do the first part first.

MODERATOR RANDAL: Fine.

DR. SINGER: The committee, as I understand it, has several charges, one of which was the development of guidelines and one of which was to foster the accumulation of information that would be useful in devising guidelines. It was nevertheless necessary to have some guidelines governing work prior to having all the knowledge that one might want. And, therefore, they proceeded. It's my understanding that they therefore decided to proceed with the development of guidelines in order to have some kind of governance on this work.

They have announced, I believe, the availability of money on a contract basis for doing the kinds of studies that were mentioned. It's my understanding that they have not been very successful in the number of applicants who are willing to undertake those contracts, and that has stood in their way. There are, to my knowledge, only very few contracts which are actually in process. But primarily this reflects the very small number of takers that they have had for that.

MODERATOR RANDAL: The second part of the question is: "You indicate that the guidelines are protection against the unprecedented hazards of this new technology. Yet the guidelines (1) are voluntary only, (2) are unenforceable, (3) do not include private, military, and national security sectors, (4) encourage proliferation in popular areas of campus and communities"—I guess this means where population is relatively dense with graduate students—I can't read the other word—"and (5) would permit high school students to pursue this research."

Some of those we just discussed, but has anybody any comments on that?

DR. SINGER: For the first part, I would say again, as I said before, that one must be very clear that we do not in fact know that any of these agents would be hazardous. That's number one. So, we have to talk always about a potential hazard. And rephrasing it that way, I would say the following: That the NIH assumed responsibility for their grantees and contractors and staff in a manner which they appear to have deemed appropriate. There has been indication, at various times, that other governmental agencies or the Congress would be interested in undertaking a serious development of policy in this regard, but only the NIH has, to this date, done so.

There was a hearing in May of 1975 held by the Senate Subcommittee on Health, but, in fact, no specific actions have come from any of that.

When the DNA of the echinoderm is prepared and cloned, one will inevitably prepare and clone in some small proportion the DNA of these small companions. That these small companions might include the spores of deadly bacilli or the viruses of human waste seems to have received scant thought.

More broadly still, we and all higher organisms live, metaphorically, immersed in a sea of microorganisms with which we have, of necessity, intimate contact on the metabolic level. As far as is known, we do not have interaction with the microbial world on the genetic level. And it might be that higher organisms have elaborated specific mechanisms to prevent such interactions — to prevent, for instance, the conceivable dissemination of human viruses through the microbial substratum. Might the human introduction of genetic discourse between higher organisms and lower in time lead to such unforeseen and unfortunate consequences? I submit that we do not know.

The guidelines reflect a view of nature as a static domain, wholly subject to our dominion. They regard our ecological niche as wholly secure, deeply insulated from potential onslaught, with no chinks or unguarded stretches of perimeter. I cannot be so sanguine. How secure is our niche? In simple truth, just one, only one, penetration of our niche could be sufficient to produce a calamity. Such a penetration could of course arise in nature without our intervention, and it may. But these innovations may significantly increase the base from which such a penetration may come.

I think there has been inadequate appreciation of the fact that we are here concerned with potentially irreversible processes. Living organisms, if they find a suitable niche, are self-perpetuating and, even more, are subject to their own future evolution wholly beyond our control. This is a novel circumstance in the history of man-derived hazards. If DDT or fluorocarbons prove to be unfortunate, their manufacture can be ceased, and, in time, they and the hazard will vanish. Once released, self-propagating organisms will be with us, potentially, forever. A new pathogen need be created literally only once to cause untold harm.

In fairness to the proponents of the guidelines, they will argue that what is proposed may not be irreversible, that these man-made variants may not be able to compete in nature with the well-adapted species already present and will die out. Others may argue that these organisms are not even novel, that means may exist in nature for the exchange of genetic material between higher organisms and microbes and, to carry the argument one step further, that the reason we have no evidence for such exchange is, again, that such organisms always die out.

And lastly, they may argue that even if we should somehow generate a dangerous organism, we now know how to cope with and restrict disease and it could not become a major threat. All of which just might be true. But we don't really know.

To sum up, what I'm saying is that in my view we lack the knowledge, both the scientific knowledge and the knowledge to assess the social hazard, to be so confident that the development of this technology will not lead by inadvertence or design to truly grievous calamities. In the absence of evidence to the contrary, I suggest that we are creating by these means novel self-propagating organisms. In view of the magnitude of the potential dangers they pose, I believe that we should take every possible precaution to exclude them from our biosphere while, at the same time, seeking to reap the benefits implicit in this powerful research.

I said once before that if we accept these guidelines and nothing untoward happens, we will owe more to good fortune than to human wisdom. We might be lucky. Our niche may in fact be more secure than we know or have reason to expect. But I'd rather not gamble with these stakes and it's not necessary.

MODERATOR RANDAL: Thank you, Dr. Sinsheimer.

I wonder if I can exercise the moderator's prerogative here by asking the first question. You said it isn't necessary, and I've wondered about the possibility of using alternative technologies to arrive at some of the same research information. To what extent has that been considered and to what extent is it possible? I would like to hear what you have to say about it and what anybody else on the panel wishes to say, and then we'll get on to the formal part of the question period.

DR. SINSHEIMER: When I said it was not necessary, what I meant was three things. One, that all this work could be done in maximum containment, P-4 type facilities.

Secondly, that at least the possibility exists for doing it in organisms less intimately associated with man than a species of *coli*.

Dr. Sinsheimer, will you be next? And then we will have a question and answer period. And don't forget, if you want to ask questions, to see that they get handed up.

DR. ROBERT SINSHEIMER: I've assumed that Dr. Singer would present, as she has very ably, the case for the NIH guidelines and for the reasons why scientists want to proceed with the development and application of recombinant DNA techniques. And I would concur with her evaluation and that given by Dr. Randal of the positive benefits that we may anticipate from further development along these lines.

I would also agree that one should carefully differentiate between recombinant DNA technology as such and genetic engineering as applied to man. The latter has its own set of terribly complicated problems, and we don't have to deal with them today.

Unfortunately, however, the recombinant DNA technology has also, in my view, a darker potential, and herein lies the source of the controversy that has erupted as to whether the NIH guidelines, as they have been developed, can be considered to be adequate to the danger. I wish I could consider the guidelines to be adequate and we could simply get on with our science, but I do not. And I am deeply troubled by the prospects, and that's why I'm on this panel today.

While there are almost infinite nuances of detail, I don't believe the critical questions are very difficult to comprehend. Essentially one may ask whether novel, potentially harmful organisms are likely to arise out of recombinant DNA research either by inadvertence or by malevolent design. The latter is easier to answer. I know of few scientists who do not believe that it would be possible by means of this new technology to create novel pathogens, viruses and microorganisms toxic to man, animals or plants, as deadly as any now known. Indeed they would very likely be more deadly for our species since we would have had no experience with them, and thus would have acquired no resistance.

If I may quote from Fenner and White's *Medical Virology:* "Successful evolution of a satisfactory host-parasite relationship requires thousands of years. An unscheduled encounter between man and a virus that the human species has not met before may have lethal consequences." And history has many examples.

There is no reason to believe that nature has exhausted the design of toxins or completed the spectrum of possible pathogens. The issue of the potential misuse of recombinant DNA technology is hardly addressed in the NIH guidelines. Indeed, perhaps the problem is inappropriate for NIH to consider. But surely this potential must be evaluated somewhere in the formulation of a national and, ideally, international policy. If I may draw a partial analogy, I expect there would be a considerable unease if 50 to 100 laboratories in this country had the capacity to create a nuclear weapon quietly and within a period measurable in months. Yet a novel pathogen could be at least as deadly as a nuclear weapon.

Could such agents arise by inadvertence? Here the issue is more clouded, indeed befogged, by our present ignorance. Because of that ignorance it becomes very difficult, in my opinion, to be confident that we can and do foresee all of the conceivable hazards. And because of that ignorance, it becomes a question of judgment and policy as to whether the precautions so far proposed are truly adequate.

We know, for instance, as yet so little about the ecology of the human intestinal flora, about the factors which govern its composition, about its role in nutrition or even in some forms of cancer. Yet most of this research is performed in an organism, as has been described, which is at least a member of the tribe of the common intestinal inhabitant, *Escherichia coli.*

We are ignorant of the ecology of this organism in other habitats. If I may quote a recent article by E. A. Gray: "Although *E. coli* is assumed to have a short life when separated from a host, this is not to say there are no observations to the contrary. The evidence is conflicting and admitted to be so."

In recombinant DNA research we introduce into this organism new sets of genes, which may number 10 or 20 or 40, very often of wholly unknown character. It is simply assumed with a blind faith in statistical probability that these new and undefined genetic factors will in no instance alter the characteristics of this organism directly or indirectly so as to cause it to produce a toxin or alter the nature of its ill-defined ecological interaction in any potentially harmful way.

We can easily become trapped here in a maze of uncertainty. In an effort to achieve some perspective on this issue — some measure of what we are about — I have attempted to view it from the standpoint of biological evolution. In that perspective we can perhaps glimpse the significance of what has now been accomplished.

Each member of the committee which recommended the deferral of these experiments agreed that the associated risks were likely to be viewed as clearcut by scientists in the field and that the anticipated benefits did not justify the potential hazards.

The risks associated with certain other recombinant DNA experiments were less clear, and therefore only caution and further consideration were urged. Still other types of recombinant DNA experiments were not, and are not, considered risky at all.

In the Asilomar recommendations, and in the NIH guidelines, the experiments proscribed initially either remain proscribed or can be performed only under extremely stringent containment methods. Indeed, the list of experiments proscribed in the guidelines is substantially longer than was initially requested in the call for a deferral. And the list includes experiments that some have used to devise fearsome scenarios about the uncontrolled spread of cancer, scenarios which have also assumed a nonexisting understanding of the causes of human cancer.

The adequacy of the containment requirements-mandated by the NIH guidelines for those experiments that are permitted remains an important issue. Some regard the requirements as inadequate. Others believe them to be more stringent than is necessary for safety. Some scientists who are not represented here today remain unconvinced that any realistic potential for hazard exists.

My own view is that the experimental and laboratory designs specified in the guidelines afford the security needed to meet the possible risks. The guidelines classify permissible experiments according to the best available estimates of potential risk. In the absence of much needed data, these estimates involve informed judgment in many instances.

For example, not all recombinant DNA experiments yield novel combinations of DNA. Recombination between the DNAs of organisms known to exchange genetic information in nature do not add uniquely man-made cells to the biosphere. In these cases, the guidelines follow the principle that the experiments are to be carried out under conditions generally used to handle the most hazardous parent of the recombinant. When DNA from species not known to exchange genetic material in nature are recombined, more stringent and strictly defined containment is required, thereby increasing the physical isolation of the experimental material from both the experimenter and the outside world.

There is documented experience on which to judge the efficacy of various physical barriers in preventing the escape of organisms. Moreover, in most such experiments it is mandatory to use modified agents that have been certified by NIH as unlikely either to propagate outside of rigorously defined laboratory environments or to transfer the recombined DNA to other cells. These agents include certain derivatives of the bacterial species that was mentioned before, called *E. coli*. The use of this bacteria has caused wide concern, and certain facts need to be emphasized.

Only one strain of *E. coli*, called K-12, is permitted by the guidelines. Strain K-12 is one of a large group of bacteria, all of which are called *E. coli* because they share certain properties in common. But they do not all have identical properties. Some *E. coli* live normally in the intestines of healthy people and healthy animals. Others are pathogens—that is, disease producers. K-12, which is rarely found in nature and does not normally colonize the human or animal intestines, is a greatly enfeebled strain whose principal successful ecological niche is in the laboratories of molecular biologists and geneticists. It is not pathogenic. If it were, you would not be here worrying about this research since all the molecular biologists would long since have disappeared.

Pathogenicity is a complex phenomenon dependent on several properties of the pathogen as well as on the properties of the species being infected. It is very unlikely that alterations of K-12 brought about by insertion of recombined DNA will make it into a pathogen. But it is not impossible. It is this remote possibility with which we are all concerned. We are attempting to protect against an unlikely, uncertain, yet unacceptable event.

Thirty years of study of the genetic chemistry of *E. coli*, strain K-12, provides confidence that the capacity of these bacteria to escape and spread in the environment can be reduced to immeasurable levels. Thus, should pathogenic organisms arise, it is not likely they would survive to cause disease. Nor is it likely that bacteria containing recombined DNA would survive to evolve in unique and fearsome manners.

Nevertheless, because of K-12's relation to common strains of *E. coli*, reservations about its use persist. It is certainly important to investigate alternative organisms, but it is not at all certain that use-

The moratorium was in itself an extraordinary event in the history of science, since no group of scientists had ever before voluntarily put a stop to their research. But perhaps just as extraordinary was the international conference that followed in February, 1975, that I was privileged to attend. Held in a former chapel at the Asilomar State Park in California, the conference met far into the night throughout a very rainy week, making a beginning stab at outlining what the guidelines should contain. All the guidelines ending the moratorium that were finally published last June have been greatly refined, but the principles remain those adopted at Asilomar.

In brief, the idea has been to divide proposed experiments into categories according to their estimated risk and to then decide which can be done, and under what sorts of conditions, and which are so potentially hazardous that they cannot be performed now, if ever. For those experiments deemed justifiable, there are two kinds of safeguards, laboratory and biological. Under the system, laboratories where the least dangerous studies are to be done are designated P-1 (for precautionary). Those where somewhat more dangerous studies may be carried out are P-2, and so on. P-4 laboratories have the most elaborate features to prevent the contamination of personnel or the escape of bacteria and are the only facilities where the highest risk experiments may be performed.

The laboratories at Fort Detrick, Maryland now used by the National Cancer Institute, which were built for the now abandoned germ warfare program, are the outstanding example of a P-4 laboratory. The multimillion dollar lunar receiving laboratory at the Johnson Space Center in Houston, designed specifically for the isolation and containment of moon germs, would probably also qualify as P-4. As it turned out, of course, there were no moon germs.

In the case of DNA recombinants, however, scientists are by no means sure that laboratory containment is enough. They have, therefore, gone to the trouble of breeding special enfeebled strains of bacteria for their experiments that are dependent on special feeding and other special conditions—ultraviolet light or extreme heat or cold, for instance—in order to survive. In the absence of these special conditions, the theory is that the gene-shuffled organisms will be uuable to reproduce, even if they should somehow find their way into the outside world.

Thus, the safeguards of the NIH guidelines are intended to be failsafe in that they combine physical containment and careful housekeeping practices with a form of biological birth control.

However, no guidelines guarantee immunity from human error. Accidents and even deaths have occurred in the most highly regulated microbiological laboratories. I expect then that our panelists will be telling us not only how adequate they think the guidelines are but also how confident the public can be that they will be observed. Scientists are independent-minded people, and I further suspect that if the regulations are drawn too tightly, they will be observed in the breach. But perhaps those on the panel will not agree.

I also wonder about what might be called the hazards of success. General Electric, for example, has applied for patents on DNA recombinants engineered to dispel oil spills. But what would happen if these petroleum-gobbling bacteria accidentally found their way into pipelines or oil storage tanks or the wing tanks of commercial jet aircraft in flight? For that matter, would it be desirable to program bacteria to manufacture drugs like antibiotics more cheaply than they can be manufactured already? There are presently indications that antibiotics are overprescribed, and most experts feel this has contributed to the development of resistance to antibiotics.

In other words, isn't it possible that DNA recombinant technology will add to the already present problem of having too much of a good thing?

Finally, I wonder how strong a handle the NIH guidelines would provide on other government agencies and private industry with its understandable penchant for trade secrets—which brings us to the topic of this conference, the possible need for legislation. So, for the answers to these and other questions, we go to our experts: Dr. Maxine Singer of the National Cancer Institute, which is a part, of course, of the National Institutes of Health; Dr. Robert Sinsheimer, Chairman of the Division of Biology at the California Institute of Technology; Dr. Robert Pollack, Associate Professor of Microbiology at the State University of New York at Stony Brook; and Dr. Liebe Cavalieri, Professor of Biochemistry at the Cornell University Graduate School of Medical Sciences and a member of the Sloan-Kettering Institute for Cancer Research.

As the program is arranged, Dr. Singer and Dr. Sinsheimer will speak first, followed by a question period, after which Drs. Pollack and Cavalieri will make their presentations before we have a second

PROCEEDINGS

MR. GUDE: Ladies and gentlemen, good morning. Welcome to this morning's discussion of recombinant DNA research. The Environmental Study Conference, which is composed of members of Congress from both sides of the aisle and also from both Houses of Congress, is co-sponsoring this briefing with the Scientists' Institute for Public Information in order to inform members of Congress and their staffs about a subject of increasing concern to the public. This is the fourth briefing sponsored jointly by the Environmental Study Conference and the Scientists' Institute for Public Information.

Citizens in Cambridge, Massachusetts and other areas with scientific laboratories have already started to play a role in the debate on research into genetic engineering. Citizen concern is focused primarily on identifying risks associated with genetic research and the complex moral issues raised by genetic engineering.

Our briefing today will address the environmental implications of this issue and speak to what, if any, government regulation is necessary.

We are very pleased that we have a panel of four distinguished scientists this morning who represent a range of views on how, if at all, this type of research should be conducted. We at the Environmental Study Conference hope their comments will prove useful to you in considering legislation that is expected to come up in both Houses at the next session.

I hand the floor over at this point to Alan McGowan, from the Scientists' Institute for Public Information, who will have a few words to say before we begin.

MR. McGOWAN: Thank you very much, Representative Gude, and let me add my welcome and thanks for appearing at the fourth Congressional Seminar, co-sponsored by SIPI and the Environmental Study Conference.

Some of you may have noticed the setting up of TV cameras and lights and the taking down of TV cameras and lights. We are conducting this briefing for the members of the audience. And to allow for a free discussion and questions and comments and the maximum amount of information, we felt that this was the appropriate way in which to proceed. I would also like just to mention that SIPI has recently established an office in Washington. And our representative in Washington, Miss Joyce Wood, who is now standing at the back, has been primarily responsible with the Environmental Study Conference for putting it together. If any of you have any questions about any of our activities or programs, please feel free to contact Miss Wood.

I think the subject of this briefing, this seminar, is extraordinarily important for all of us because we have realized that in all areas, science impinges on our lives and impinges on our future. I was privileged to be able to attend a weekend meeting two weeks ago sponsored by the National Science Foundation in their Ethical Values in Science and Technology program. And to start off that meeting, Stephen Toulmin, a well known historian and philosopher of science at the University of Chicago, made what I think is the telling point, and that is that there appears to be a changing of the compact by which science is related to society; that we no longer can think or feel that there is not a functional relationship between research priorities and the needs of society. It seems to me that this is basically what this seminar is all about.

Now, to get to what you came here for, I'd like to introduce Miss Judy Randal, science correspondent in the Washington Bureau of the New York *Daily News,* who has been following this controversy for some time, who will be the moderator of this morning's session. Judy.

MODERATOR RANDAL: Thank you, Alan.

As Alan said, I'm Judy Randal, the science correspondent in the Washington Bureau of the New York *Daily News.* And we are going to get to our distinguished guests and the more interesting part of the program very soon. But before we do, I thought it might be helpful to tell you a little bit about DNA recombinant technology, what it is, and why it has become an issue so important that it is hard for anyone interested in public policy to ignore.

DNA, of course, is an abbreviation for deoxyribonucleic acid. This is the molecule of heredity, shaped something like a spiral staircase, of which the genes of all living things are made. When we talk

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STATEMENT ON RECOMBINANT DNA RESEARCH

Board of Directors
Scientists' Institute for Public Information

The discussion over recombinant DNA research, conducted within the scientific community for a number of years, has now broken sharply into public debate. That debate carries with it major implications concerning the public's right to know about basic scientific research programs and the capacity of the public to influence constructively the course of basic scientific research. Indeed, the public may be required for the first time to determine for itself whether an unprecedented basic science program can be safely conducted at all without posing a major threat to the future health and security of mankind.

In July, 1976, the Cambridge, Massachusetts City Council, after holding two well-attended public hearings on the subject, voted to ask Harvard University to delay for three months recombinant DNA research that Harvard had decided to conduct in a new, specifically constructed laboratory facility. The ban was later extended to mid-December. Similar discussions are going on in other cities where such research is proceeding or being contemplated; the issue also has been discussed in the United States Senate.

Although there is some precedence for public discussion and regulation of research programs entailing recognized high risk, the recombinant DNA case is perhaps unique. The significance of the events in Cambridge and elsewhere lies in the fact that a local government body has initiated public debate on a highly technical scientific controversy which hitherto had been the province of the scientists themselves, and has taken sides in that controversy on the public behalf. The action of the City Council has become part of a national debate which has had the effect of forcing scientists, sometimes unwillingly, into the spotlight as advocates of public policy, and awakened lay citizens to their responsibility to oversee the direction of scientific research which may have a profound effect on their lives, and on the lives of future generations.

In the United States as elsewhere, the public has become increasingly aware of the relation of basic scientific research to the public good. Generally speaking, the public has held scientists and their work in high esteem. Therefore, it has consistently approved the appropriation of major public tax monies to fund scientific enterprises.

Unqualified public support for scientific research programs began to erode with the development of environmental consciousness. The public began to realize that technological development, frequently undifferentiated from the basic research which made it possible, was not without costs. Increasingly, technological advances have been subjected to cost-benefit analysis, with the result that some technology has been found to be wasteful and dangerous to the public welfare.

The present controversy centers on modification of the double-stranded DNA molecule, the principal means of genetic transfer of hereditary traits. In recombinant DNA research, this modification is accomplished by inserting into a living host cell DNA segments taken from the living cells of widely divergent species, using a virus or plasmid (a loose ring of DNA) as an intermediary. The modified or "recombinant" DNA thus produced becomes a permanent part of the host cell's genetic makeup and is faithfully reproduced as the cell divides.

The question of whether or not the "recombinant DNA" material will affect the behavior of the bacteria is the critical question. This question is not answered by any of the experiments conducted so far. If the genetic material does "express itself" — that is, if the behavior of the cell is modified by the material that has been introduced — both the opponents' fears and the proponents' hopes will be given greater justification. For if this happens, it will be proven possible to create new forms of life.

Chemicals called "restriction enzymes" are used to split the DNA into fragments, and may be obtained commercially or produced in the laboratory. Indeed, the technology to perform recombinant

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Although confidence in public institutions has been on the wane, the scientific community has fared relatively well recently despite the scientific controversies which have been so publicly prominent. In fact, I believe that the relative confidence enjoyed by the scientific community is a result of this controversy. Public awareness of scientific controversies promotes a feeling of involvement in the life of the scientific community as well as a public sense of satisfaction at having an impact on the issues.

Public involvement must increase. I would therefore urge the creation of a National Commission on Biomedical Research which would have as its charge the following:

1) The encouragement of discussion within the biomedical research community of the potential ethical and safety considerations;

2) The development of an independent information service which would develop and publicize (using radio and television as well as the printed media) all pertinent information relating to biomedical research including that from private industry;

3) The convening of periodic hearings around the country to encourage lay and professional citizens to question and comment on the conduct of biomedical research;

4) The development, perhaps in conjunction with the National Science Teachers Association, or some other appropriate group, of educational materials which consider the implications of biomedical research particularly as they relate to biohazardous research and genetic engineering;

5) The initiation of international discussions -- for efforts to deal with scientific problems, and promises, cannot be limited by national boundaries.

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Testimony of Alan McGowan before the Subcommittee on Science, Research and Technology, May 4th, 1977.

President Carter has called for an "independent information system" to resolve the difficult and thorny issues surrounding the development of a National Energy Program. Surely such a system is also needed for the equally important area of biomedical research, particularly when one considers the extremely difficult and complex set of issues surrounding human genetic engineering -- which it must be stressed is different from recombinant DNA research -- which we seem to be moving closer to achieving. To make any policy workable in a democratic system the policy makers and the public must have adequate independent information.

The issue of recombinant DNA research has thrust a scientific controversy into the public domain as never before. Although the basic issues far transcend specific research on the DNA molecule, it does serve as the paradigm for future public involvement in the determination of research priorities and it is useful to explore it in this light.

Although the participants have not yet found the controversy easy to live with, much has been learned as a result of this controversy. For better or for worse -- and I think it is for the better -- the public knows far more about the course of biomedical research than was the case five years ago.

ALAN McGOWAN

Curriculum Vitae

EXPERIENCE

Current

March 1973 - present -- President, Scientists' Institute for Public Information, New York, New York
July 1971 - present -- Trustee and Vice President, Institute for Environmental Education, Cleveland, Ohio

Previous

1974 - 1976 -- Chairman, Subcommittee on Alternative Energy Sources, Governor's Task Force on Energy Problems
Summers 1969, 1970 -- Program Director, Water Pollution Project, Tilton School,Tilton, New Hampshire
June 1969 - June 1974 -- Scientific Administrator, Center for the Biology of Natural Systems, Washington University, St. Louis, Missouri
February 1966 - June 1969 -- Adjunct instructor of physical science, Pace College, New York
September 1957 - June 1969 -- Teacher of physics, chemistry, biology, and mathematics in secondary schools

EDUCATION

Yale University -- B.E. Mechanical Engineering, 1957
Polytechnic Institute of Brooklyn -- Physics
Tufts University Master of Science Institute for Teachers of Science and Mathematics (National Science Foundation stipend) Summers of 1961 and 1962

PROFESSIONAL ASSOCIATIONS & AWARDS

Office of Technology Assessment, U.S. Congress, Task Force on Public Participation
Yale Science and Engineering Association, Executive Board; Chairman, Activities Committee
American Association for the Advancement of Science, Commission on Science Education (1970-1972)
American Association for the Advancement of Science, Chairman, Youth Council (1971-1972)
Committee for Environmental Information, Chairman, Education Oivision (1970-1974)
Scholastic Magazine, Environmental Advisory Board (1971-1973)
American Association of Physics Teachers, Teacher Recognition Award June 1968
Sigma Xi, June 1969

IS ALL KNOWLEDGE GOOD?

"The Stone Age may return on the gleaming wings of science, and what might now shower commensurable material blessings upon mankind, may even bring about its total destruction. Beware, I say, time may be short."

— Winston Churchill

Since the atomic bomb, socially concerned scientists in the Western democracies have become slightly less sure about what had formerly been an axiom of scientific thought: the value of knowledge. This touch of ambivalence can be documented in the statements of two of FAS's most profound commentators on scientific freedom and social responsibility.

In a March 4 rally in 1970, Victor Weisskopf said: "The main responsibility of a scientist was, and is, the development of knowledge within his own science by teaching and research. But in these days, when the detrimental effects accumulate so rapidly, scientists must be concerned about the physical and social effects of their work. It may turn out that it will be too dangerous to create new scientific knowledge. *The result of the scientists' concern may be a decision to stop scientific progress."* (Emphasis added).

This was a daring statement. Nevertheless, a few minutes later, Professor Weisskopf ended his speech with the sentence: "Whatever your viewpoint, it is good to know more."

In the AAAS Report on Scientific Freedom and Responsibility drafted by John Edsall there is a sentence:

"The Committee believes that the vigor and integrity of science require that all areas of potential knowledge be open to inquiry; but the means of inquiry are open to change, particularly where life processes and human behavior are involved." (Emphasis added).

However, Professor Edsall is less sure, himself, about this point of view and in a submission to NIH supporting the guidelines on recombinant DNA, he remarked:

"I should add that I do not hold the view that the increase of knowledge is necessarily good."

He believes, in particular that, if a general nuclear war occurs, the net impact of the last few hundred years of science on mankind could be negative despite the enormous benefits of science to date.

Philip Handler, President of the National Academy of Science, felt obliged to respond to this kind of question in his recent speech to ICSU. He remarked:

"Particularly troublesome is the ever more frequent expression of the notion that there are questions that should not be asked, that there are fields of research that should be eschewed because mankind cannot live with the answers. NONSENSE! No such decision can be *rational*, much less acceptable." (Emphasis added).

While acknowledging the possibility of temporary delay because of "uncertainty" concerning risks to the public or investigator, Dr. Handler said there could never be a time when "the avoidance of knowledge should be mistaken for wisdom." The "foolish" government which knowingly interfered with the course of science "will itself be the inevitable victim of that crime."

It is thought-provoking that these ultimate technological assessments, which are far from dispassionate, wholly beyond proof, and stated, at best, much too flatly, were contained in a speech which chided "scientist-advocates" for lack of dispassion. □

FBI CLEARED FAS IN 1950 AND PROMPTLY FORGOT ALL ABOUT US

FAS asked the FBI for its file on FAS and discovered that the FBI had investigated FAS from 1946-1950 during the period when FAS sought civilian control of atomic energy in the form of an Atomic Energy Commission. The conclusion reached by the FBI was that FAS was neither communist dominated nor had pro-communist policies. The FBI summary conclusion in full read as follows:

"The Federation of American Scientists has been active in opposing military control of atomic energy, supporting civilian and international control; critical of security procedures concerning personnel engaged in atomic energy; and in favor of less secrecy concerning atomic energy. This organization was the subject of a security investigation by this Bureau from 1946-1950. The investigation failed to disclose that the organization was communist dominated or that its policies were pro-communist although some of its members throughout this country, both on a national and local scale, have been described as communists or pro-communist."

As to what members the FBI has in mind, we find that the FBI has a report from that period on FAS provided by the Army and it listed the following past FAS officers as having engaged in "communist front activity":

J. Robert Oppenheimer — father of the atomic bomb

Harlow Shapley — the most eminent astronomer of this century and a former AAAS president

Edward U. Condon — former head of the U.S. Bureau of Standards in the Commerce Department

Harold Urey — Nobel Prize winner in Chemistry

John P. Peters — FAS records do not indicate that Peters, who was a Yale University medical professor, was ever an officer of FAS; Peters' name was cleared by the Supreme Court in a loyalty case decision in 1955.

The Army concluded, however, that association with FAS should "not in itself be construed as derrogatory information" since a "reliable Federal agency" (presumably FBI) has stated that there is no evidence that the FAS is "in any way dominated by the Communist Party." Signed by a Colonel in G2, this memorandum is undated.

The FBI had extraordinarily little in its 30 year old file on FAS after it closed its investigation in 1950. Only two crank letters asking about us in three decades were filed and only a few pages in the seventies, including a letter from FAS's director to Mr. Kelly. The entire file is only about one inch thick, of which about one-third is a copy of an FAS report mentioned below. (This does not, however, include the file on the investigatory period 1946-1950 for which we have not yet asked, accepting the summary memoranda as a surrogate at least for the present).

The FBI Freedom of Information Office — which cooperated cordially and with every indication of straightforwardness in all of FAS's requests — advised that this small bulk released does reflect the bulk of the file. In answer to our request, we were advised that while items can be withheld for reasons (classification, internal rules and practices, invasion of privacy, reveal sources, endanger personnel) these only involved small parts of documents, or scattered coversheets in our case. (FBI did

SAMPLE PROBLEMS PUBLIC INTEREST SCIENTISTS FACE

Most discussions of scientific responsibility avoid any illusion to dilemmas of responsibility. It is as if one were to discuss ethics without ethical dilemmas. Scientists reduce the problem to a few phrases: ("honesty, dispassion and objectivity") or assume away the problems ("We know when we speak scientific nonsense.") Obviously scientific responsibility, whatever it means, is a branch of ethics and does have dilemmas. Here are a few which members may wish to teeth on.

Speaking Out: Timing

E.g. — As a result of certain novel experiments you have undertaken, you believe that a common additive is, in reality, quite dangerous. It is impossible for you to be certain and a year more of tests are necessary. The health authorities are willing to do the tests but urge you not to discuss the implications of your work with the press lest "all hell break out." Do you hold a press conference or defer to established authority? And how do you decide?

Providing of Unsupported Opinions

E.g. — You have been voicing reasoned opposition to nuclear power for some years when an opportunity arises to appear on the NBC Today Show. After rather irrelevant questioning, the moderator says, "Well, now Dr. X, we have 30 seconds left, please tell us, all things considered are these reactors safe or unsafe?"

What do you say to the tens of millions of persons watching?

Problems of Allies

E.g. — You oppose the SST on a number of grounds but put less stress on others and consider still others wholly misleading. A leading Congressman asks for your help in preparing a paper opposing the SST but you discover that he cannot be dissuaded from emphasizing less important issues and at least one point you consider misleading. Do you assist him in preparing the speech or not?

Phrasing Conclusions

E.g. — You have read enough about the ABM, and had enough experience in Government, to believe that you understand one important aspect of the situation quite clearly and, indeed, that you can make a very plausible case for your position on the basis of bits and pieces of publicly released data. You are asked to testify. Should your testimony end by conveying the certainty you do indeed feel (for the reasons provided in the testimony) or should it end with assertions you do not really feel protesting that your failure to have all the data disqualifies you from reaching a conclusion?

Endorsements Under Uncertainty

E.g. — You are a chemist and, during testimony, you are asked whether all things considered you would endorse a certain toxic substances bill that has the best and only chance of passage. You have little certainty that the bill is really workable. You suspect that the problems could be worked out in practice and believe it is now or never for a toxic substances bill. But the bill is too complicated to be wholly grasped by you, and, possibly, anyone else. Do you endorse the bill?

Getting The Public's Attention

E.g. — You are persuaded that certain agricultural procedures are dangerous. You are convinced that, once attention is drawn to the issue, you will be able to persuade the relevant scientists on their own terms but you just are not being taken seriously. It becomes evident that no attention will be paid to you unless you appeal, in dramatic tones, to the public. Do you write a dramatic and somewhat sensationalized version of the situation to force the scientific comunity to investigate or do you suppress this impulse and keep plugging away?

This school of thought on responsibility is clearly more concerned with the effects on *science* of scientists participating in the public debate than in the effects on *society*. It wants scientists not to embarrass science by getting too involved. Here, for example, are the results of an interview with Dr. Handler in the *Wall Street Journal* of April 3, 1975:

"'. . . policymakers and the public must learn to use (science) properly and not expect more than it can reasonably produce.

At the same time, scientists must show greater restraint in their increasingly frequent forays into the policy-making world.'

These are the views of a man who's thought a lot about the subject: Philip Handler . . .

* * *

"'Scientists must take some of the blame themselves for their recent image problems,' Mr. Handler agrees, — 'for pretending to expertness they don't have, for giving advice in areas far outside their own competence, for advocating policies with unbecoming heat and shrillness.'

* * *

"'Once the scientific community has presented the facts, however, it must leave final decisions to the policymakers and the public,' Mr. Handler asserts. 'Science can contribute much to enhancing agricultural production,' he states, 'but American policy with respect to food aid is not intrinsically a scientific question'."

* * *

Similarly, science can study whether energy independence is technically feasible or whether Soviet underground nuclear tests can be detected, but, he insists, scientists must then let regular policymakers decide whether to try for energy independence or just what arms control proposals to put to the Russians."

The conservative *Wall Street Journal* concluded approvingly: "Both science and government seem well served by this reasonable man."

The Excluded Middle

But are they? This view *seems* defensible because it assumes away the entire problem. It is an over-simplification which might be termed that of the "excluded middle." On the one hand, science presents the "facts." On the other, "policymakers" and "public" decide what to do. It leaves out the scientific policy analyst and the scientist engaged in political action in or out of government. Are scientists to "drop out" of these middle roles lest science suffer "image problems?" (This would, in

SCIENCE AND THE THREE SOCIETIES

Broadly speaking, scientists face three kinds of societal working conditions. In the most difficult, they find themselves under right-wing dictatorships characterized by contempt for intellectuals, and fear of their libertarian tendencies. Examples are the governments of Chile, Argentina, South Korea, Thailand, Brazil, and Uganda. Here the scientists are neither prized nor free.

Typically the governments are ready to ignore the impact on their societies' development of repression of scientists. The result is often even less freedom for scientists than possessed by other members in the society.

A second class of governments prize their scientists and provide them with varying ranges of special perquisites but do not permit them scientific or political freedom. This is the condition of the communist world: the Soviet Union, the nations of Eastern Europe under Soviet hegemony, the People's Republic of China, Albania, Yugoslavia. Because these economies are planned, the scientific community's resources are allocated and directed. These states reject the notion that science for science's sake will maximize payoffs by permitting full rein to the scientists. Not only science but everything else (including chess and art) must have its purpose.

Marxism and Scientific Responsibility: Theory

The socialism of Marx could not be, in principle, more prone to favor "science." Marxists consider Marxism to be the "science of society"; in fact, Engels wrote that it was only with this scientific discovery that "the true history of mankind begins." This approach produces a faith in the social sciences that far exceeds that in the West, one which further enhances the popular faith in natural science.

In particular, also, the underdeveloped quality of tsarist Russia left little doubt in the minds of the revolutionaries that science would be critical to the salvation of the Soviet Union. The net result is prestige for scientists in the Soviet Union that is quite unparalleled in any other nation in the world. The members of the highest Soviet scientific rank (Academician), numbering about 400, are considered "immortals" with automatically commissioned biographies and special burial plots. They earn more than 10 times the average wage.

At the other end of scientific achievement, but also illustrating the principle, every chauffeur characterizes himself as "engineer." All in all, the Soviet scientific community is immense; one association of scientific workers has 7,000,000 members. A very large fraction of all higher education graduates are technical graduates in one sense or another and consider themselves scientists or scientific workers.

J. D. Bernal: Marxist Spokesman for Social Responsibility

What do Marxist scientists consider their social responsibilities to be? No better advocate of the theory of responsibility in European communist states exists than the late J. D. Bernal. Bernal was a committed Marxist ("For my part I can only understand the world as I have learned and experienced it, that is, largely in the light of Marxism . . .").

He was also pro-Soviet believing: that the Cold War had been deliberately fomented by the "privileged classes in America and Europe"; that Eastern Europe had been "liberated" and that the Sino-Soviet split was "bickering." He was also very able. His four-volume compendium, *Science in History* provides a remarkable Marxist analysis of the role of science from the Stone Age through the hydrogen bomb.

Bernal's approach to social responsibility can be seen in the Constitution of the World Federation of Scientific Workers, which he drafted, and for which he continues to be the patron intellectual saint:

> "The primary responsibility for the maintenance and development of science must lie with the scientific workers themselves, because they alone can understand the nature of the work and the direction in which advance is needed. The responsibility for the use of science, however, must be a joint responsibility of the scientific workers and of the people at large. Scientific workers neither have nor claim to have the control over the administrative, economic and technical powers of the communities in which they live. Nevertheless they have a special responsibility for pointing out where the neglect or abuse of scientific knowledge will lead to results detrimental to the community. At the same time, the community must be able and willing to appreciate and to use the possibilities offered by science, which can be achieved only through the widespread teaching of the methods and results of the natural and social sciences."

Bernal's major conclusion was that science had become too important to be left to scientists or politicians and that the "whole people must take a hand in it if it is to be a blessing and not a curse."

European Communism and Scientific Responsibility: Practice

Writing before, during, and immediately after World War II, Bernal was oblivious to the intellectual realities of Soviet life, in particular to the widespread apathy and cynicism. Other committed Marxists were more perceptive. Jean Paul Sartre, writing after the Czechoslovakia repression, remarked that "socialism has fallen back into the long night of its Middle Ages," and spoke of the "steady remorseless degeneration of Soviet socialism."

The scope for Soviet scientific responsibility, of the kind Bernal espoused, had been correspondingly limited by these practical realities. Scientists have had "primary" responsibility for the development of science but heavy pressure has been placed on them to avoid "bourgeois" abstractions. In a planned economy, all of the problems of bureaucratic direction and control have appeared.

Bernal's notion of "joint responsibility" for the use of science by scientists and the public at large cannot be recognized, much less vindicated, in the Soviet political process. The public has no voice, and no method exists for appeal to the public. A number of concrete ideological problems have arisen.*

We do see stirrings of scientific responsibility in the efforts of Sakharov to persuade Khruschev to sign a partial test ban treaty and, more generally, in the efforts of

*While Lysenkoism and its impact on biology is the best known example, Soviet scientists have had to wage continuing ideological struggles on other fronts, especially in coping with the philosophical demands of the official philosophy, dialectical materialism. Was the role of the observer in quantum mechanism a form of "idealism" opposed by materialists? Could relativity's appraisal of space and time be defended as having made them "forms of the existence of matter" or would the ideologies decide that relativity should be suppressed for having adopted the notion that space and time were products of "pure reason?" Was there a Marxist-Leninist notion that the Universe had to be infinite or could astronomers consider finite, closed, models? Did Marxist-Leninist materialists have to believe in some kind of spontaneous generation (at some level) to avoid the charge of religiousity?

THE VOICE OF SCIENCE ON CAPITOL HILL

F.A.S. *PUBLIC INTEREST REPORT*

Formerly the FAS Newsletter

THIS ISSUE:
SCIENTIFIC RESPONSIBILITY

Vol. 29, No. 10 December, 1976

TO WHOM ARE PUBLIC INTEREST SCIENTISTS RESPONSIBLE?

"Scientific responsibility" has, in practice, two quite different and partly opposed meanings. The supporters of these different interpretations are often quite innocent of any realization that the other interpretation exists. Thus an unholy alliance advances the bare notion of scientific responsibility. But certain attempts to apply the concept risk the outbreak of open warfare between the two schools.

The problems arise with regard to the participation of scientists in the public debate. On matters within the scientific community, there is no important difference in point of view among scientists on what constitutes scientific responsibility. All oppose such traditional forms of scientific irresponsibility as falsification of data, plagerism, suppression of opposing points of view, etc.

The underlying question at issue is whether the traditional notions of scientific responsibility, developed within the community, can cope adequately with the entirely different problems posed in the interface between science and the public. At the heart of the difference in perspective is the question: "responsibility to whom".

Responsible Conduct Seen as Issue

The narrow school of interpretation prefers to use the concept "responsible conduct of scientists" as its interpretation of the phrase. In its view, the "responsibility" at issue is a responsibility to the scientific community: not to demean the community or to diminish the standing of colleagues, by acting in ways dissonant with the traditions of science or its popular image. It sees improper actions as threats to the integrity of science and, sometimes, even to its funding.

In particular, this school often considers it vaguely or flatly irresponsible to make public assertions which are imprecise or, worse, unprovable; to generalize without firm grounds; and/or to speculate. It is often considered questionable to advocate policy decisions that involve science but go beyond it; to campaign for such policies; to ally oneself with non-scientists in such campaigns; to accept the undignified and inadequate conditions for presentation the media often require; to go "over the head" of the scientific community; and so on.

A broader interpretation of scientific responsibility conceives it primarily as a responsibility to society rather than to the scientific community; this school of thought prefers to use the phrase "social responsibility." It has acquiesced in the fact that virtually all arguable policy decisions inevitably go beyond science. It accepts as inevitable that scientists involved in public debate will have to go beyond discussing what is scientifically known for certain. In its view, the name of the public policy game is decision-making under enormous uncertainties; what is known for certain is usually uncontroversial and needing no exponents.

Perhaps the most important difference between these two interpretations of scientific responsibility is that the narrow view implicitly discourages involvement by scientists in public debate, while the broad view instructs them that such participation is their "social responsibility."

Let no one minimize the importance of this difference in perspective. At issue is the degree of participation in the public debate of hundreds of thousands of the most intelligent citizens in America, individuals whose special training and knowledge makes them especially well-suited to objective analysis of the issues in and around science.

—Continued on page 2

— Reviewed and Approved by the FAS Council

MEMBERS INVITED TO COMMENT ON SCIENTIFIC RESPONSIBILITY

Scientific responsibility is hard to define. And it is harder to practice than preach. But nothing is more important to FAS than an investigation of such issues; with the help of our members and others, we plan to turn our attention to this subject from time to time.

This preliminary discussion ponders the differences in meaning which "scientific responsibility" has in the ideological camps of other nations as well as the differences of view in our own debate. In a subsequent *Report*, later in the academic year, we plan to go somewhat further by discussing hypothetical but concrete vignettes to give substance to a discussion that is otherwise unworkably abstract.

We ask our readers to send us their reflections. What are the key issues of scientific freedom and responsibility? To whom is responsibility due? What kind of freedom is meant? Where are the contradictions between the different meanings? And what practical conclusions should FAS draw? Send your relevant complaints also — about FAS as well as others — and your commendations. □

FAS RECEIVES FBI FILE — Page 7; CHINESE AND SOVIET NUCLEAR TESTING — Page 8

who want to discuss the implications of all varieties of science and society issues. So much the better, I feel.

The point I want to emphasize is simply this. The Science for Citizens program is not a rip-off by public interest groups but an opportunity for the society to ensure that a powerful and valuable new segment of our democratic process, the public interest group, fulfills its functions in a scientifically responsible fashion, and that scientists who want to speak up, as you want them to, have a vehicle with which to do so.

MAKE IT FINANCIALLY FEASIBLE:

Another way for Congress to encourage scientific thought on the implications of science is to require the grant-making federal agencies, e.g. the National Institutes of Health (NIH), to spend a certain percentage of its overall grant funds (e.g. 1%) on grants discussing the societal implications of the work being funded with the other 99%. This would, I am confident, produce immediately a cottage industry of investigations into the implications of scientific advance.

I consider this to be perhaps the best approach. But there may be other ways. And offers of funds to the more traditional scientific societies might, in some cases, rejuvenate their consciences.* The scientific journals are suffering from the same problems facing other journals (high postage, printing and paper rates). Unfortunately, because the organizations are both tax-exempt and tax-deductible, no tax advantages can be offered them; instead, subsidies would be required. But grants from government agencies financing research might flow in their direction as proposed above.

COMMEND THE RIGHT AND CONDEMN THE WRONG:

Scientists (and scientific organizations) who do try go fulfill their public responsibilities should, from time to time, be commended in whatever way the Congress and Executive Branch

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* To get some idea of how reluctant these organizations are to work in public policy areas, one should examine Science Magazine, April 1, 1977, in which it is revealed that the scientific societies have thus far ignored Congressional encouragement to educational and charitable organizations to opt for the right to spend up to 20% of their time on legislative activity.

"public interest" simply asserts that they have no more financial vested interest in the outcome of their issues than that of the citizens at large.

These organizations function in a delicate ecological balance with the public. They can only survive in such proportion as the public's assessment of the importance of their issues and the correctness of their stands. For example, because more citizens are concerned with environmental issues than nuclear war, far more groups exist to pursue these objectives. Using direct mail solicitation for memberships and support, these groups must renew their constituency each year, and maintain the confidence of their supporters continually. This keeps them democratically responsive. At the same time they make public participation possible for any citizen, on virtually any issue, by his or her joining, writing, supporting and/or assisting, a suitable public interest group. This is a dramatic and irreversible new phenomenon of which the Congress should take careful note.

The new tax laws have wisely recognized that these groups play a role so useful that they should be permitted to engage in legislative activities up to 20% of their time even if organized as tax-deductible groups. And they have always been allowed to litigate.

In my experience, these groups are manned by persons who are surprisingly knowledgeable about their fields and highly dedicated, considering the low rate of pay normally available. Their record on a large number of issues is one of persistence, and vindication.

As FAS saw these groups expand and grow, we wondered if we could provide scientific expertise for them. We, and no doubt others, have experimented with card files of willing experts and so on. In our experience, however, scientists must work with, and within, these groups to be useful to them. It is not as if the groups needed to know some isolated fact, or the result of some esoteric single calculation. Science must infuse their program, and their perceptions of possibilities and risks. For this they need scientists working with their groups for months at a time. And if they had these scientists, I believe their programs would be still more mature and responsible, and still better thought out.

I recognize that the Subcommittee is concerned that the Science for Citizens program might assist public interest groups engaged in legislative action or legal actions. It wonders whether public funds should be used to support activities that can be controversial.

Specific action to encourage scientists to alert society to impending problems falls into various categories:

a) Ask more often

b) Listen better

c) Make it financially feasible

d) Commend the right and condemn the wrong

ASK MORE OFTEN:

Congress could ask the scientific community to alert it to the potential impact of new developments in a number of ways. Any one of a number of institutions could be requested, by contract, to provide brief summaries of possible implications of ongoing research. Such contracts could be let to the major professional societies (American Physical Society, American Mathematical Society, American Chemical Society, Federation of American Societies for Experimental Biology, etc.). Alternatively, the American Association for the Advancement of Science (AAAS) or the Office of Technology Assessment (OTA) could be involved. The National Academy of Sciences (NAS) is another possibility but its studies take so long to be completed that it might be less useful for the purpose in question of sounding an early alarm.*

* It might be well if Congress could persuade the National Academy of Sciences to pay scientists who work on its reports instead of relying upon volunteers; the cost could be easily incorporated into the Government contracts that finance most of NAS work. This might speed up the NAS studies and eliminate certain biases that result from the narrow selection implicit in looking for volunteers. On April 26, 1977, the Academy revealed that President Carter had expressed by letter his own similar concern that the Academy could be more helpful "...if, in addition to its long-range studies, it is prepared to accept and respond in a more timely manner to questions which demand early decision." In turn, the President of the Academy conveyed in a speech to the NAS membership his own uncertainty whether this would be possible.

In return for Congressional expressions of readiness to defray, in contracts, these wholly reasonable costs, Congress might try to nudge NAS into accepting the open meeting requirements of the Federal Advisory Committee Act with which, at the present, our Appellate Courts have held it need not comply. (Although carried on the pre-war government organization manual rolls as a part of the Legislative Branch, NAS has somehow made its way today to the ranks of the "quasi-official" organizations and was adjudged insufficiently Governmental for this act to apply).

I hasten to add that FAS has not discussed these possibilities with the NAS leadership because no useful purpose would have been served by doing so. But we do encourage the Committee to take these matters up with NAS itself if it considers them constructive.

Second, the recombinant DNA experience confirms the difficulty that society has in assessing the degree and nature of future hazards arising from new research. For example, in my judgment, the dangers due to "accidents" with recombinant DNA, though the most widely advertised and discussed, are in fact destined to be less important than the problem of deliberate misuse either by military establishments or by the mentally disturbed. This is analogous to shifts in emphasis on the dangers of nuclear reactors where the perils of accidents have recently given way to concern over proliferation and terrorism. It is often hard to gauge the future ability of mankind to cope and the publicity provided various dangers is not always in proportion to the dangers themselves but reflects certain media imperfections (e.g. sensationalist biases) as well as a human tendency to discuss those problems than can be and are being resolved (e.g. accidents rather than deliberate misuse).

Third, the recombinant DNA experience confirms the extreme difficulty, in the nation-state system in which we live, in controlling scientific developments. Even were we to wish to do so, we cannot prevent other nations from pursuing scientific developments and technology although they are often as likely to affect our lives as those of others.

Fourth, recombinant DNA experience seems to me unusual in raising the specter that the research itself may have hazards to the public at large. Normally, laboratory personnel, at most, are at risk from experimentation. Society's problem is usually that of digesting the technological possibilities provided by science.

Fifth, the recombinant DNA experience does reveal and reflect the rapid pace of biological advance which can be expected to gather momentum throughout this century and the next. The spotlight of scientific advance has shifted, in the last several decades, from chemistry in the thirties, to physics in the forties, fifties, and sixties, and we now see before us the possibility of understanding life and man himself. The uses of this knowledge may eventually interact with our civilization, and our everyday lives, to a greater extent than even have the advances of physics or chemistry.

Finally, I think that the scientific community should be reassured in reflecting on the treatment it will receive at the hands of public bodies. Recombinant DNA is an extreme

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CURRICULUM VITAE: Jeremy J. Stone

Dr. Stone, a mathematician by training, is the Director of the Federation of American Scientists and Editor of its "Public Interest Report". Since he became the Director of FAS in 1970, the 26-year-old organization has undergone a rejuvenation that has increased the membership by 500% and expanded its activities and effectiveness. The Federation's membership now includes half of America's Nobel Prize winners and former high-ranking officials from government agencies concerned with science and society problems.

Dr. Stone was graduated from Swarthmore College in 1957 with high honors, and took his Ph. D. in Mathematics at Stanford University in 1960. He has served as a Research Associate at Harvard University; as a member of the professional staff of the Hudson Institute; as an Assistant Professor at Pomona College; and was a Visiting Scholar in the Department of Economics at Stanford University. He was also an International Affairs Fellow of the Council on Foreign Relations.

As an expert on national security affairs, Dr. Stone has written widely on the subject and is the author of two books on the arms race, the better known of which is "Containing the Arms Race: Some Specific Proposals" (MIT Press, 1966).

Dr. Stone's present or former membership in professional societies includes Phi Beta Kappa, Sigma Xi, American Economic Association, American Mathematical Society, American Political Science Association, and the Council on Foreign Relations.

Dr. Stone resides with his wife, Dr. B. J. Stone (a mathematician) in Chevy Chase, Maryland.

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parents over birth defects of one child, which possibly could have been prevented by careful, serious, thoughtful research.

I did appreciate your very thoughtful statements.

Mr. THORNTON. Thank you very much, Mr. Dornan.

I want to thank the members of the panel again for your excellent testimony.

This hearing is now adjourned, to meet again in the morning at 9:30 in room 2325.

[Whereupon, at 11:10 a.m., the subcommittee adjourned, to reconvene at 9:30 a.m., on Wednesday, May 4, 1977.]

Mr. Fuqua, do you have any questions?

Mr. FUQUA. Thank you, Mr. Chairman.

Let me say that that was a very eloquent statement that you just made, and I would like to associate myself with those remarks.

I want to thank all of the panel for, I think, some very, very thought-provoking comments that you've made.

I would also like to congratulate Mayor Wheeler on his landslide victory. Maybe it was called in scientific circles a micro-victory.

Mr. WHEELER. Thank you, sir.

I do have one question. We do have a full committee meeting, and we're going to have to break up shortly.

I agree with the concept that's been put forth that we should proceed with extreme caution. I think the staff, Dr. King and others have very eloquently explained that if we make a mistake we'd be proceeding too fast, and that we should proceed with extreme caution in trying to resolve this issue, and proceed keeping in mind too the point that's made of local awareness, making the local people aware of what may be going on in their community, or what is going on, as far as DNA research.

Should there be, if we get into this area, a Federal law concerning the structure by which this type of research may proceed? Should there be exclusion of local control, have stricter Federal control, or an exemption from local control?

I think, Mayor, you mentioned it in your testimony, or touched on it just briefly, that you felt the responsibility to the people who elected you, and probably that it should not have a local exemption.

I see Mrs. Taft indicating a desire to speak on that.

Mr. WHEELER. I think that this, like other major areas of concern, nuclear fission, and so forth, it's apparently necessary that there be a broad umbrella of Federal legislation to control certain aspects. But it is the local community whose sewer plant may be contaminated, whose air and whose people are exposed, and I don't think that Federal legislation can cover all of the concerns that a local community has. I think that the local community should have the right, and that should be an unequivocal right, to protect the health and safety of the people in that community.

So that under a broad umbrella there can be a great deal of flexibility, but when you take away any local government control, I think things just go haywire. I think that the local community ought to make the decision.

Mr. FUQUA. Mrs. Taft.

Mrs. TAFT. I'd like to reinforce the statement that I made earlier about having local community members on the institutional review boards, which is proposed in the Rodgers bill. This kind of person could be a direct contact, on top of the situation as to what's going on.

I am a chemist by training myself and I have worked in laboratories, biomedical research laboratories, long enough to know that what goes on in the laboratory isn't exactly always what the principal investigator thinks is going on.

I would also like to reinforce what Dr. King has said, that the bottle washer and the other technicians aren't always familiar with exactly the kinds of things that are necessary.

Mr. THORNTON. A very good point, too.

I brought here the material that was written by Science for the People and distributed in Cambridge, and you can read it to see if it's radical. It's titled "The Health Hazards of Gene Implantation." In Cambridge it was very important that there was available literature written by scientists for lay people. This stuff was paid for out of people's pockets. It was a major problem to get it written and distributed. We couldn't get it written and distributed through the normal funding mechanisms. That's one request, that groups like Friends of the Earth, the Sierra Club, that they be given help when they step in on the side of some citizen sector to give technical presentations.

The second is that the Congress seriously consider in its funding of scientific education—and I think it's a scandal that I've passed 20 years of education as a professional geneticist in the most prestigious institution in the United States without ever learning about the Race Hygiene Act in Germany in 1934. You should not be able to get a Ph. D. in genetics without knowing about the kinds of misuses that have occurred with biomedical technology. It should be a part of the education of geneticists, and NIH and NFS is going to resist that. They're going to see that as political interference in the process of science. But Congress must insure that scientists are literate, that you not only have to know what DNA means, but you have to know what "race hygiene" means.

Mr. DORNAN. Let me ask you a question on that so it's on the record now.

Adolf Hitler came into power on January 30, 1933. He certainly had no academic credentials. The amazing thing about him was how a little corporal could rise to this peculiarly horrendous level of power.

How did that act get passed so quickly? What scientific body took advantage of the political instability of the Weimar Republic, and this strange young leader, who at that time would have only been 44 years of age, to move this peculiar law forward? How did that come about?

Dr. KING. It's very interesting, and it's been studied by a number of American social historians. Right from the beginning of the rise of genetics in modern science in 1900, there was a constant fight between the research geneticist, who wanted to understand heredity, and the eugeneticist, who said, "We can do things to the human population with this knowledge." For example, the Immigration Restriction Act in the United States in 1924. If you read the House of Representatives testimony, the major testimony was, "We have to keep out these Italians and these other European people with inferior genes." That's what it's about.

Mr. DORNAN. They used those words, "inferior genes" ?

Dr. KING. Absolutely. There was lots of articles in the popular press and in learned journals talking about why are these people working in textile mills poor? "They're poor not because they're being paid 25 cents an hour. They're poor because they have poor genes." It was a raging controversy at that time within the genetics community.

In Germany there were many people within the scientific community who said, "We have this scientific knowledge, and we should be scientific, and we should put it to work, and our genes are far superior, and the genes are superior, it's not just propaganda." The moment Hitler came into power that sector within the scientific community then moved

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CLOSING STATEMENT

The continuation and application of research in gene implantation is likely to have global consequences. Some of the applications may (though not necessarily) be beneficial; should there be any accidents, they are likely to be irreversible and very damaging (39, q13).

We hope that more and more scientists will follow the example of R. Sinsheimer and E. Chargaff (43, q56, 58) who question the wisdom of continuing this research. Scientists have enormous power and must be responsible in handling it.

We urge the scientific community to take pause, ro reflect. This technology will remain exciting; for now, we must approach it with great caution.

SCIENCE for the PEOPLE

VIII. SUMMARY

I shall briefly summarize our point of view.

1. THE DECISION WHETHER AND UNDER WHAT CONDITIONS THIS WORK MAY BE CONTINUED SHOULD NOT BE UP TO ANY ONE SCIENTIST, OR EVEN UP TO ANY GROUP OF SCIENTISTS. EVERY CITIZEN SHOULD HAVE A VOTE.

2. THE SUPPOSED BENEFITS TO BE GAINED FROM THE PURSUIT OF WORK IN GENE IMPLANTATION ARE NOT REAL BENEFITS. THIS RESEARCH WILL NOT SOLVE THE WORLD'S AGRICULTURAL OR MEDICAL PROBLEMS.

3. IF THE PROJECTED HAZARDS OF THIS WORK BECOME FACT, THESE DANGERS WILL FAR OUTWEIGH THE SUPPOSED BENEFITS. THE INTERPRETATION OF DATA ABOUT HAZARDS IS DEPENDENT ON THE INTERPRETER'S INTERESTS AND POLITICS.

4. THIS IS NOT A PROBLEM OF FREEDOM OF INQUIRY BUT OF PROTECTING THE PUBLIC FROM A NEW HAZARD. WE QUESTION THE FREEDOM TO MANUFACTURE NOVEL ORGANISMS.

If this work is to be pursued, there must be an active search for a host other than E. coli, preferably one whose habitat is very limited and which is not naturally promiscuous.

That there is at present no mechanism for including the public in a decision-making process is no excuse to proceed without public participation. It is high time that we work to create such a mechanism. We recommend that thia research be delayed and that, during the delay, there be created a political institution for bringing public representation into the decision-making process.

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genetic material between widely disparate organisms (39, q43). The creation of barriers to genetic contact between groups of organisms allowing them to diverge is central to the evolutionary process. Molecular biologists can now short-circuit these barriers in the laboratory. Containment cannot be absolute, particularly if research proceeds under the present Guidelines. It is impossible to predict what impact the escaped recombinant organisms might have on the biosphere because not enough is known about evolutionary biology or ecology. However, if the effect turned out to be significant, there is little doubt that it would be disastrous. We all accept that human experimentation must be restricted. Is the biosphere a more appropriate experimental system for unrestricted investigation (23, 26, 36, 39, q44) ?[3]

Because of the possible impact of escaped recombinant organisms on the communities in the immediate vicinity of our research universities, we strongly recommend that this research be done only in a small number of laboratories in sparsely populated areas. Access to these laboratories should be available to qualified investigators from around the country, and the laboratories should provide maximum possible containment. Until we know more about the repercussions of this research, we would advise doing all work involving eukaryotic genes in a few such isolated maximum containment facilities.

VII. HISTORY OF SCIENCE

1. We are not at war

Before closing I want to refer again to the history of the Atom Bomb. Here is an example of a scientific breakthrough which resulted in a product which was even more dangerous than its inventors expected it to be. It is well known that many scientists were reluctant to pursue this work and that, at the last minute,

merged in the mind; rather, they draw us along on parallel paths. To begin to do genetic manipulations on human beings is to take a step toward the Brave New World. Each step counts, and the society as a whole must ask: Is this the direction of choice?

b. Imperfect cures and/or accidents may lead to replacing one disease with another.

A more obvious concern is that eager clinicians may jump in with imperfect cures before the basic research is done. Furthermore, consider the perhaps equal probability of spreading disease through laboratory accidents, which would result in trading one disease for another. Of course, this is a fear associated with many medical technologies. It is especially worrisome in this case because of the self-perpetuating nature of the projected afflictions.

3. Agriculture: The Green Revolution

At first, the agricultural applications seem not merely acceptable but exciting and wonderful. But let us take a step back in time and look at the Green Revolution , which involved introducing a new genetic variety of rice to underdeveloped countries. Because this new rice required special fertilizer and a different growing season, and had stalks which were unsuitable for use as fodder or thatch, the rich farmers got richer and the poor farmers poorer. Changing a crop may affect the economy and the ecological environment, but it cannot change the political conditions. Any increase in welfare becomes illusory. Though there are people in the United States who go hungry, it is not because we do not know how to grow food. No matter how much food is produced, it will not keep people from starving until it is distributed to the people who need it, regardless of the likelihood of profit to producers and distributors.

Neither is it trivial to worry about the possibility of contamination by other DNA's, or the possibility of creating a nitrogen-fixing crabgrass which would confound farmers by its virile growth(39, q30).

advantageous to be able to find an easy, inexpensive source of insulin (39, q30). Most of the insulin which is administered to diabetic patients is a mixture of bovine and porcine insulin. The apparent shortage of insulin is supposed to be due to a shortage of pigs. At the very least, we could implement more efficient ways to collect and use the pancreata from the pigs that are already being slaughtered. This would be safer than running the risk of having E. coli churning out insulin in our guts. Bovine insulin differs from human insulin by two amino acid residues; porcine insulin differs by only one and is adequate for most patients (15, 20, 49). For those few who have allergic reactions even to the porcine insulin, desensitization is almost always possible. For the very few who cannot be desensitized, we would recommend the development of a technology other than recombinant DNA for the production of human insulin in small quantities

e. Cancer: Cure or creation?

What about cancer? It has become clear that the immediate cause of most cancers is exposure to carcinogenic chemicals (31). Thus the research which would enable us to deal with cancer would involve the identification of these carcinogens; the elucidation of their mechanisms of action and of the complex relationships between the lengths of exposure and the effective doses of these substances.

If we really want to solve the cancer problem, we should spend more energy cleaning up the environment and changing our eating and smoking habits. At the NIH public hearings, Prof. Baltimore said he suspects that, since it is hard to change our habits, we should study oncogenic viruses to learn how to cure cancer as well as how to prevent it. We would be very glad if there were a cure for cancer, but we think that we must review our priorities. Prof. Baltimore encourages us to maintain the status quo, even though one in six Americans may die from doing just that (18). We certainly do not intend to accuse Dr. Baltimore and many others of malevolent intentions; however, we can hardly trust ourselves

2. Progress in agriculture

The world's food supply is limited and many people are starving. By the use of genetic engineering we might create variants of grains which could fix atmospheric nitrogen, and thus be independent of fertilizer. The creation of a single grain which would be a source of complete protein might also be possible.

3. Treatment of genetic diseases

These may be cured (in somatic cells) or prevented (in germ cells) via genetic manipulations.

4. Progress in cancer research

The technology might allow us to unveil the mystery of cancer and, hence, to cure it.

5. Progress in drug production

Genetic engineering affords us an easy, inexpensive way to manufacture insulin, antibiotics, and other biologically active substances.

V. CRITIQUE OF THE PREDICTIONS

I would like to offer an alternate way of looking at these benefits. I shall discuss the items in reverse order and give a few examples to illustrate our points.

1. Critique of General Medical Advances

a. Self-reproducing, biochemically active substances are dangerous

The massive manufacture of biochemically active substances by the use of their DNA in self-reproducing form poses an incalculable and irreversible danger. This is especially true if the host of the DNA is *E. coli*, always present in the human gut and capable of being a human pathogen. Moreover,