Kennedy: Pushing for More Public Input in Research

When it became clear a few years ago that Edward M. Kennedy (D–Mass.) was going to assume Senate leadership for health and biomedical affairs, researchers across the country breathed a small sigh of relief. It felt good to have Kennedy on their side, especially at a time when Richard Nixon's men were seen as challenging science from the White House. Physicians in clinical research, as well as basic scientists, believed Kennedy would comprehend the importance of what they were doing and, more important, support the way they went about it. There was hope that the twin causes of untargeted research and scientific autonomy had found a powerful political ally.

Well, anyone who thinks that Kennedy today is the standard-bearer for the traditional ethos of science had better think again. While he declares that he endorses the conventional values of biomedical science, it is becoming very plain that he is not satisfied with the status quo and that he has little sympathy with the notion that scientists know best how to go about their business. During the last 6 months, Kennedy has, on a number of occasions, made it a point to tell biomedical scientists that they are not doing enough to meet the needs of the public. And he is implicitly threatening to use his legislative clout to change things for what he considers the better if scientists themselves do not take the initiative.

Every investigator will remember that the late President Lyndon Johnson shocked scientific sensibilities by demanding "payoffs" from research. Kennedy, too, is demanding payoffs, particularly in the area of chronic and social diseases. And he is taking his demands one step further. He wants the public involved in science decision-making in a significant way and has been pressing the theme that, because citizens are paying for research, citizens should have a say in what research is being done, and under what circumstances.

During the past 10 years, a move to have broad public involvement in decision-making has occurred in several areas of social endeavor. The poor no longer allow social workers and urban planners to rehabilitate their neighborhoods without a lot of citizen representation. Students are no longer content to let the faculty run the universities, making decisions about curricula or evaluations of teaching performance on their own. Patients, no longer willing to blindly assume that doctors know best, are demanding information about their care and asserting their rights to informed consent. And so it goes. It appears that science is next, that its arcane walls are going to be penetrated. Kennedy sounds like a man who is preparing to lead the movement, which has led to some bewilderment, and some resistance within the scientific community.

An Emphasis on Commissions

For the moment, Kennedy apparently will rely on the idea of national commissions to evaluate research as his mechanism for achieving public involvement. It was he who conceived and introduced the legislation that led to the moratorium on fetal research and the creation of the National Commission for the Protection of Human Subjects of Biomedical and Behavioral Research, which, by law, has a majority of nonscientists as its members (*Science*, 4 October 1974). And it is Kennedy who is thinking about a bill to create a similar commission to review basic research that does not necessarily involve human subjects, such as work in recombinant DNA, popularly referred to as "genetic engineering" or the "creation of entirely new forms of life."

Last December, Kennedy expressed his feelings that the scientific community is not responsive to public needs in what, in retrospect, seems like a tempered speech at Yale University Medical School. In that address, which generated a bit of alarm among the academic medical community at the time, he attacked schools for failing to solve problems of maldistribution of doctors.

In April, Kennedy took on the recombinant DNA issue in hearings on the Asilomar conference (*Science*, 14 March) at which a group of more than 120 scientists and a handful of laymen issued guidelines about the circumstances under which revolutionary genetic experiments could be undertaken (*Science*, 6 June). He found the conference wanting.

In May, his current position on a number of aspects of biomedical research was crystallized in an address he gave at the Harvard School of Public Health. The following excerpts from that speech are representative.

[On the National Institutes of Health (NIH) and all the biomedical institutions it supports:]

Public support, which implies confidence and trust, has become so substantial that many of them believe it is theirs by right. It is not. The plain truth is that the National Institutes of Health has been a "sacred cow." Those days are gone.

The NIH is no longer immune from responsible and penetrating inquiry by those who underwrite its program—the American people.

Last December . . . I announced that I would undertake a thorough examination of the programs and policies of NIH. This provoked a considerable degree of anxiety in the scientific community and a considerable degree of support from the public.

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[On the reaction of the scientific community to any suggestion that the public participate in policy:]

. . . academia has been on the defensive. It has chosen to view public scrutiny as a threat to scientific independence. It has chosen to view public involvement in particular research areas as inappropriate and representative of a trend toward anti-intellectualism.

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[On the idea that the public should decide how much money should be spent on research but that scientists alone should then decide how to disperse their alloted sum:]

. . . I believe this elitist and acutely parochial approach does not serve the country well. It is an approach that will assure continuing tension between the scientific community and the public. . . . The tension arises from a concern about how research priorities are set by the scientific community. It arises from a concern about the implications of research; it arises from a concern as to whether certain research should be done at all, and, if so, under what conditions. Finally, it arises from a concern about the safety of some research prospects.

Kennedy's interest in the Asilomar conference has become somewhat controversial. Some people think he is looking out for the public; some think he is meddling. Whichever it is, he has made it clear that he intends to keep on. He already has said he will hold hearings on NIH. And it seems that, although he believes the safety issues involved at Asilomar are important in themselves, his other interest in the conference lies in the fact that it is illustrative of a broader range of questions about the relationship between science and society.

Kennedy Not Satisfied

Kennedy stated his opinion of the actions at Asilomar quite plainly in his Harvard speech. He acknowledged that, "For literally the first time in the history of science, researchers stopped their work to consider its implications; to see if they should go forward at all. . . ." But Kennedy was not satisfied with what they did.

It was commendable that scientists attempted to think through the social consequences of their work. It was commendable, but it was inadequate. It was inadequate because scientists alone decided to impose the moratorium and scientists alone decided to lift it. Yet the factors under consideration extend far beyond their technical competence. In fact they were making public policy. And they were making it in private.

The Asilomar conferees may have been making policy without broad public participation, but they were hardly making it in private. Sixteen reporters were taking down every word.

A very significant, and very troublesome, part of what promises to be a strong and enduring debate about public involvement in science is that no one is explicit about what public involvement means in a practical sense. That was apparent at the Senate hearing on Asilomar, which was one measure of how ill-defined the issues are.

A brief chronicle of events may be useful. It has been almost 2 years since members of the biomedical community first brought to public attention the potential hazards inherent in rapidly developing techniques for easily joining together genes in biologically functional combinations that do not naturally occur. By using a genetic element called a plasmid as a vehicle for linking genes in new combinations, it may be possible to produce hormones or enzymes or drugs that are currently difficult, or impossible, to synthesize. The potential benefits of the new technology for medicine and agriculture are great. But so is the potential, though hypothetical, risk. Plasmids can splice genes from viruses, for example, into bacteria. One could, therefore, incorporate an animal tumor virus in *Escherichia coli* which grow in the human gut. No one would want such a menacing hybrid to "escape" from the laboratory.

Scientists attending the 1973 Gordon Conference on Nucleic Acids became so alarmed by potential problems of the experiments that they instructed the meeting chairmen, Maxine Singer of NIH and Dieter Soll of Yale, to write to the presidents of the National Academy of Sciences and the Institute of Medicine about it. That letter, which suggested the establishment of a study committee, was published in *Science* (21 September 1973) and elsewhere.

Such a committee was established, with Paul Berg of Stanford University as its chairman, and, in the summer of 1974, it called for an international moratorium on those aspects of the gene combination experiments that were potentially threatening (*Science*, 26 July 1974). Their call for a temporary embargo was made at a full-dress press conference and was widely publicized. At that time, they also announced plans for the Asilomar conference at which scientists (and others) would evaluate the new technology and decide how to handle it.

The scientists who did all of this, did so out of a deep sense of social responsibility. They remembered the past, when scientists failed to alert the public to the possible consequences of their work, and they were determined not to repeat previous failures. And they believed that, by making their concerns public, they were inviting public scrutiny, encouraging public debate, and, in every sense, involving the public.

But now they seem to be in a no-win situation.

The Kennedy hearing (only one other member of the Senate health subcommittee showed up, and he stayed no more than 15 minutes) was set up in the form of a debate. Stanley Cohen of Stanford, who first developed the techniques for recombinant DNA, and Donald Brown of the Carnegie Institution of Washington, in Baltimore, were cast as being opposed to public involvement. Each of them was at Asilomar. Willard Gaylin, president of the Institute of Society, Ethics and the Life Sciences at Hastings-on-Hudson, New York, and Halsted Holman of Stanford, were introduced as advocates of the position that scientists cannot be left to act alone.

From the point of view of Cohen and Brown, the debate format was unfortunate. One observer declared the hearing a "disaster." Neither man *meant* to come out sounding anti-public, yet in the minds of most persons present at the event, they did. In part, that is because of what they said, but the debate format did serve to intensify their position.

In an interview a couple of weeks after the hearing, Cohen was adamant in saying that he had been misinterpreted, as had Brown. "The impression that prevailed at

Kennedy Has Rocky in to Talk

Senator Edward M. Kennedy (D–Mass.) had Vice President Nelson Rockefeller over to the Senate on 6 June for a brief, public "White House advisory conference." The subject of the meeting was the role of the new science and technology adviser to the President. The purpose was to give senators from three science-related committees a face-to-face meeting with Rockefeller, who has been the champion of the science adviser idea within the White House.

The meeting was thoroughly cordial, and Rockefeller extolled science and technology as the key to solving society's ills. He also cleared up some doubts by reassuring the senators that military research and development will indeed be within the purview of the new adviser.

Otherwise, the main significance of the meeting was that Kennedy, chairman of the science subcommittee of the Committee on Labor and Public Welfare, got the jump on Olin E. Teague (D–Tex.), chairman of the House Committee on Science and Technology. Rockefeller had already agreed to deliver the opening statement at Teague's hearings on the Administration bill creating the new science office. By having Rockefeller over on the eve of congressional hearings (scheduled for the week of 9 June), Kennedy got to him first. (Because it is almost unprecedented for a Vice President to be called to testify on Capitol Hill, care has been taken that neither of Rockefeller's appearances cast him as a witness.)

Passage of the White House bill creating the office of a science and technology adviser is likely to be swift, assuming neither house loads the measure with too much excess baggage. In the Senate, Kennedy's hardy perennial, S.32, will again be taken up along with the White House bill. S.32 contains a provision for a three-man science advisory panel as well as procedures to facilitate long-range planning and priority-setting in government science activities. In the House, Teague's National Science Policy and Organization Act of 1975, which, among other things, would create a cabinet-level Department of Research and Technology Operations, will be considered along with the Administration bill. But the Administration's science adviser measure may be handled separately in the interest of speed, says a staff man.

No one seems to have any idea who President Ford wants for the new post. But it is pretty clear that H. Guyford Stever, head of the National Science Foundation, is out of the running. Rockefeller said at the 6 June meeting that the two jobs were too much for one man, and there has been no hint that Stever would be called away from the NSF.—C.H.

the hearing was that Don Brown and I were arguing against public involvement. That simply is not so. I certainly thought that I was taking a very pro-public involvement position."

It all comes back to the issue of just what public involvement means. Cohen believes the wide publicity given Asilomar constitutes public involvement. "The deferral of the experiments was to allow time for public reaction," he says. And he maintains there was public participation at the Asilomar conference itself, although he concedes that the high ratio of scientists to laymen could be a point of dispute. Cohen suggests that the way we handle radioisotopes in this country is a good model of what public involvement should be.

> . . . radioisotope use is subject to regulations designed to ensure the safety of laboratory personnel and the general public, and there is public involvement in the enforcement of these safety procedures. However, the merit or lack of merit of specific experiments that employ radioactive materials is entirely a scientific judgment that is determined by the peer review system.

Brown, too, leaned heavily on the virtues of peer review in his testimony, arguing that, whereas the public should participate in decisions about the application of scientific advances, only scientists themselves are qualified to direct research itself and recognize inherent problems. "Scientists have the special knowledge to recognize potential hazards of their research and to devise constructive solutions."

While it may be true that only scientists have the technical expertise to carry out certain types of experiments and to make judgments about the technology necessary to contain hazardous material, it is not true that only scientists have the intelligence to comprehend the general nature of the research and its social implications. Scientists hiding behind the jargon of the laboratory were, in part, the targets of testimony from Gaylin and Holman.

Gaylin, who stated at the outset that Asilomar was important only as it represented a "class of problems" involving science and society, challenged the position that, because science has been so successful in the past, it must therefore be left alone.

> . . . success has no claims on freedom. Quite the opposite. Success in the service of society may gain profit and prestige but never autonomy. The more a service ceases to be trivial and incidental, the more it becomes essential to the values or the survival of a society, the more restricted becomes its autonomy.

Gaylin also disputed the position, taken by Cohen and Brown, that the issues at Asilomar were properly ones of biological containment and safety. "Because an issue arises within the territory of science, is couched in the technology of science, and phrased with the language of science, [that] does not make it a scientific issue."

Holman, in his testimony, said that the actions of the Asilomar organizers were "perceptive and sensitive," but he was firm in his view that "actions taken by scientists alone are not sufficient." (It is reasonable to presume that Holman's position influenced Kennedy's Harvard remarks that Asilomar was not adequate.) Holman attacked the tyranny of governance by experts and declared that, by leaving the public out, major social relationships among the institutions of society could be threatened. He warned that "When knowledge is treated as a private possession . . . the public remains ignorant and may become apathetic or hostile."

Holman, who proposed the creation of some sort of national commission to oversee science, suggested that the model of informed consent as it applies to human experimentation is valid for basic research as well. The public, he maintained, has a right to give its informed consent to scientific experimentation. In an interview a few weeks after the hearings, he suggested that one device for letting the public in on research would be to require universities to hold public discussions about work going on at their campuses. In his opinion, such discussions at Stanford, which began out of concerns about the relationship of the engineering faculty to the Vietnam war effort, have led to a number of new, interdisciplinary courses that are quite worthwhile.

The question now in many minds is whether there should be a second Asilomar. Perhaps one should have been planned in the first place. Cohen emphasized in his testimony that questions associated with "ethical and religious issues of human experimentation . . . are quite peripheral to the biological safety questions considered at Asilomar." But it is the ethical and moral questions that concern the public. It has been said that there simply was not time for the Asilomar conferees to deal with both the technological and the social aspects of problems of recombinant DNA. Maybe so. But it is for this that they are being implicitly attacked. And it is the social problems that Kennedy is going to address, and for which he may propose legislative solutions. —BARBARA J. CULLITON

Navy Oceanographic Move: Renewal or Disaster for Basic Research?

The Navy, in recent months, has become embroiled in a dispute with its own oceanographers, with a number of prominent university scientists, and several members of Congress over a proposal to move 80 percent of its oceanographic activities from the Washington, D.C., area to a nearly empty group of federal laboratories in Bay Saint Louis, Mississippi. The site happens to be in the home state of Senator John Stennis, the Democrat who is chairman of the Senate Armed Services Committee and thus wields great power over the military budget.

The Oceanographer of the Navy, Rear Admiral J. Edward Snyder, put forward the proposal in draft form to the Secretary of the Navy last February. Snyder, the proposal's chief advocate, argues that the site—which was built as part of the space program but never occupied—can become a worldwide "center of excellence" in oceanography and revitalize Navy's programs, which have declined in recent years.

But the proposal ran into fierce opposition from university oceanographers. Under Snyder's plan, the 30-man oceanography section of the Office of Naval Research (ONR), which now supports basic research in 50 universities around the country, would be towed to Mississippi along with bigger applied research and operational offices. The basic oceanography work of ONR, opponents fear, will be swallowed up by these other groups.

Also, the oceanographers say that the move could be a first step in the dismantling of ONR, which is, from their vantage point, the country's most sacrosanct sponsor of basic science. The ONR was founded in 1946 to advance basic work in physics, chemistry, oceanography, and other disciplines—and it largely taught the