

April 30, 1975

Willard Gaylin, M.D.  
Institute of Society, Ethics and the Life Sciences  
623 Warburton Avenue  
Hastings-on-Hudson, New York, 10706

Dear Will:

When we met briefly prior to the Hearing of the Senate Subcommittee on Health on Tuesday, April 22, 1975, you mentioned that you were concerned about the growing "anti-science" image of the Hastings Institute. As a bench scientist who has valued the discussions of the Hastings Institute I too have been concerned with that evolving reputation and its possible influence on the effectiveness of your efforts. The enormously constructive role played by several members of the Institute at the International Meeting on Recombinant DNA Molecules at Asilomar seemed partly to counteract the image and I hoped that your testimony would be equally constructive. I did not find it so. Indeed, "antiscience", is not an unfair characterization of your remarks. Should this image of the Institute be broadly perceived, it is doubtful that the Institute can function constructively in its chosen role.

The subject of the hearing, as is pointed out on page 1 of your prepared testimony, was Genetic Engineering, or Manipulation, as discussed at the Asilomar Conference. While important critical comments of the Asilomar Conference and its antecedents might have been presented to the Committee, the testimony pertains rather to a quite separate and more general series of problems. Although I can understand the wish to generalize, and recognize the statement that you would so generalize, your remarks will, in fact, be interpreted by the Committee and the public in relation to the Asilomar Meeting on Recombinant DNA. The failure to recognize this, together with the provocative nature of the testimony, may well result in an undermining of Asilomar and its lessons. The hope of extending the approach used on the Recombinant DNA problem to other incipient and future problems in biological research will thereby be diminished although that approach is consistent with many of the concerns of the Institute.

The provocative nature is exemplified in the first paragraph. The distrust mentioned therein was explicitly recognized in open discussion at Asilomar. However, predictions of open hostility ("hatred") can be the first step in elevating the distrust to such hostility. The series of rhetorical questions in the middle of page 5 again indicate anger and provocation. Scientists do not, as a group, reject Kant and his contributions, nor do they dismiss the New Testament. Modern science recognizes as essential the notion that all conclusions are tentative. The implication that Darwin is unacceptable is also ridiculous. (A fascinating documentation of the seriousness with which scientists take Kant and Darwin is in an article by the eminent molecular biologist, Gunther Stent, in SCIENCE, issues of March 21, 1975, Volume 187, Page 1052).

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This implication of ignorance and narrowmindedness on the part of scientists was more explicit in the unprepared remarks, when you stated that the general education of the scientists often is neglected. Presumably this is one reason why the scientist is considered "untrained to understand the moral and political implications of his work" (Page 4). However, the public at large (Page 4) is no more so trained than is the scientist. And it is not certain that those who are specifically trained in matters of morality and politics are any more likely to reflect the public view than are the scientists. With regard to general education, it seems to me that the shoe is on the other foot. While many scientists are reasonably well educated and are active, appreciative participants in political, literary and artistic activities, those in nonscientific fields rarely know even the minimum about science, in an irrational defiance of the importance of science to their own lives.

Considering the substance of the rhetorical questions on page 5 rather than their provocative expression, suspicion and mistrust of science are clear. The social and political implications of particular scientific inquiries and discoveries can be questioned without denigrating the scientific method itself. All human questions can be analyzed in a variety of ways with a variety of insights thus to be gained. The scientific method, with its striving for objectivity, is a productive way to analyze many questions.

The lack of careful definition (paragraph on the middle of page 3) turns what might have been a constructive point into a destructive one. Doctors Cohen and Brown did not disagree with the notion that the public is responsible for determining the level to which it wishes to support various areas of science. But Dr. Brown quite properly tried to distinguish between such a general decision and the implementation of the public interest. He said that decisions about how to proceed in a given area of research should be left to the scientists if the public interest is to be served and the public's money well spent. This is because those intimately connected with a given area of research can best judge whether a particular approach is feasible, whether a particular question can profitably be studied at a given time, and who is best qualified to undertake the research. The public may, for example, decide to build roads, and where to build them, but, by and large, it leaves the design of the roads to the engineers. Untrained or poorly informed people will make poor decisions and the public's desire for information or new knowledge or good roads will rarely be satisfied. It is not helpful to obscure these different levels of decision-making. Alex Capron, in the remarks quoted on Page 6, taught at Asilomar that the public does have the right to err in a democracy. However, he made it without rancor and the lesson was accepted and learned. He never implied that the public wished to make avoidable technical errors.

Another definitional error is the lack of distinction between those areas of current research which do present ethical questions and the great bulk of research which does not. Not all biology is medically oriented.

The use of the Hearing on Asilomar as a forum to plead the cause of budgetary increases for consideration of ethical problems was unfortunate. It perpetuated the notion of ethicists and scientists as adversaries rather than attempting to bring the two together. It was also beside the point.

Thus, while recognizing that polemic is unproductive (page 7) the testimony invited polemic by failing to define a reasonable and useful set of questions and problems, and by using inflammatory words and phrases in what can only be called a destructive manner.

The testimony overlooks important factors in the history of the discussion on recombinant DNA molecules. Therefore, it may be useful to review that history.

In 1972, using a technique somewhat different from those presently being discussed, Paul Berg and his colleagues at Stanford constructed a DNA molecule made up in part of DNA from the oncogenic virus, Simian Virus 40, and in part of DNA from E. coli. Berg recognized the possible hazards of reinserting the newly constructed molecule into E. coli and decided not to proceed with such insertion (although it was the next logical step in the experiments). Berg discussed this problem with a variety of his colleagues and friends, including members of the Hastings Institute, and his final decision was influenced by these conversations. The decision was widely known, and was germinal to the next incident in the story, which took place at the June 1973 Gordon Research Conference on Nucleic Acids. At the meeting, experiments indicating a new way to join DNA from any organism with DNA from any other organism, and most particularly, with bacterial plasmids, were described. The new method was less tedious and simpler than the one used by Berg. The range of previously intractable questions about genetic expression that could be answered by utilizing the new method was enormous and widely perceived. Parenthetically, the discovery basic to the new capabilities was of a group of enzymes making specific cleavages in DNA molecules at given sequences of nucleotide bases. These enzymes have many other important uses in genetics and molecular biology beside their use for recombination of unrelated DNAs. The description of the enzymes also elucidated certain long standing observations in bacterial genetics. There was no way to predict that these enzymes would permit in vitro recombination. . . . the relevant properties of the enzymes were unexpected and unique.

The intellectual excitement engendered by the new experiments was tempered when some members of that Gordon Conference immediately pointed out the potential hazards. That evening, when the formal sessions were completed, serious discussion of the problem of the hazards went on in an informal manner. The next morning, the last of the meeting, the group voted overwhelmingly to write to the President of the National Academy of Sciences asking the Academy to set up a study group to consider the issues raised by the experiments. In a less overwhelming vote, but by a substantial majority, the group decided to make the letter public by publishing it in Science Magazine. The less overwhelming vote was a specific reflection of the concern that making the matter public would provoke fears, and might result in a negative reaction on the part of the public. However, the understanding that this was a matter for public discussion prevailed. Both of the votes were substantially repeated when a mail ballot was sent to those attending the conference

after the letter was drafted by the cochairmen of the conference (Dieter Soll of Yale and myself). In the fall of 1973, at a meeting of the Executive Committee of the Assembly of Life Sciences of the Academy, the recommendation of the Gordon Conference was accepted and Paul Berg was asked to set up a committee. The results of the group's deliberations was the letter published last July (1974). The letter was published in Science, Nature and the Proceedings of the National Academy of Science and a press conference was held in Washington. The letter was widely publicized in the public press. The Berg committee recognized the need for public discussion.

The letter of July 1974 said several things. First, it asked that colleagues of the signatories all over the world join them in deferring two types of experiments deemed to be of considerable potential hazard, and to consider carefully a third type of experiment whose potential hazard was less clear to the committee. Second, the committee recognized its own inability to make a full estimate of possible hazards and the means to minimize the hazards. They therefore called for a larger meeting, with international scope, and they urged that the meeting be held promptly. Third, and very significantly, they called for the National Institutes of Health, as the relevant official governmental body in the United States, to set up an Advisory Committee on Recombinant DNA to deal with these questions in a publicly responsible manner. These actions clearly reflect an understanding of the public nature of the questions at hand and an appreciation of the limits inherent to the ad hoc group.

The meeting at Asilomar was the international meeting called for in the Berg letter. Seventeen countries were represented and all joined in the consensus. It was important that the meeting be international because all the required scientific expertise was not available in the United States and also because the problem is international. Micro organisms do not recognize international borders. In this regard, the terms of your testimony, which are applicable to American societal and governmental processes, do not account for the fact that other countries and other cultures will have very different mechanisms for dealing with the situation. Nowhere in the testimony is this recognized. The discussion is of little relevance in, for example, the Soviet Union, and the approach would more than likely inhibit any cooperation on the part of Soviet scientists. This consideration is not relevant only to Recombinant DNA.

The Organizing Committee for Asilomar recognized at the outset the need for continual public involvement. Sixteen representatives of the International scientific and general press were welcomed at all sessions of the meeting except for the deliberations of the Organizing Committee. They were privy to every provisional document, to every discussion. Long stories appeared on the front page of most major newspapers of this country on the day following the end of the meeting.

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The Organizing Committee also realized, from the start, the importance of having other than scientists at the meeting. To this end, a member of your Institute, a lawyer, was invited to arrange one of the sessions. Representatives of the NIH and NSF, not practicing scientists but those responsible for funding research and making policy were in attendance. In addition, all the members of the NIH Advisory Committee on Recombinant DNA were invited and many attended as did the committee of the European Molecular Biology Organization which is charged with making recommendations for policies within western Europe and Israel.

Finally, the report emanating from Asilomar explicitly states that the report will be advisory to official bodies in all countries. The report does not present binding regulations...rather guidelines to be used until official action ensures. In the United States it is presumably the NSF and the NIH that will evolve official policy, with the Asilomar Report to inform it.

The whole story is a good example of what the Hastings Institute has been talking about all these years and can be taken as a measure of your success. There are many in the scientific community who thought, and still think, that the actions of the Gordon Conference, the Berg Committee, and the Asilomar Meeting, can only lead to "anti-scientific" reaction. I have believed all along, and still believe, that such a reaction is not a necessary consequence and will be less likely the more rational and open our discussions. Nevertheless, after hearing you the other day, I think my more pessimistic colleagues may turn out to be right.

Sincerely yours,

Maxine F. Singer  
Member, Organizing Committee for Asilomar  
Conference on Recombinant DNA

cc: ✓ Dr. Paul Berg  
✓ Dr. Don Brown  
✓ Dr. Stanley Cohen  
✓ Dr. Richard Roblin  
✓ Dr. David Baltimore  
✓ Dr. Larry Horowitz