3 April 1981, Volume 212, Number 4490 SCIENCE

# Two Aspects of Scientific Responsibility

John T. Edsall

*Of all the traits which qualify a scientist for citizenship in the republic of science, I would put a sense of responsibility as a scientist at the very top. A scientist can be brilliant, imaginative, clever with his hands, profound, broad, narrow—but he is not much as a scientist unless he is responsible. The essence of scientific responsibility is the inner drive, the inner necessity to get to the bottom of things: to be discontented until one has done so; to express one's reservations fully and honestly; and to be prepared to admit error.*

—ALVIN WEINBERG (*1*)

I agree with this assessment of the central role of scientific responsibility but not in all respects concerning what constitutes responsible behavior in some difficult situations. There are two major kinds of scientific responsibility. There is the pattern of responsible behavior that is associated with basic research and the communication of the results. And there are the problems that arise when scientists deal with issues involving social responsibility—such matters as the control of nuclear and other weapons, the uses and hazards of toxic chemicals and radioactive materials, the choice among various modes of producing or conserving energy, or the criteria for deciding whether to dam a river or let it flow freely. These are very different problems from those involved in basic research; the decisions reached involve value judgments. They are, and indeed should and must be, political decisions. Nevertheless, applied scientific knowledge is an important element in the making of such decisions. Scientists who enter these disputed areas encounter problems of responsible behavior that are considerably more complex than those of the scientist who is working out basic problems in the laboratory, or in thought and calculation. However the two areas also have much in common, and the problems of social responsibility cannot be considered properly without keeping in mind the general code of scientific behavior that has evolved over the last few centuries.

The pattern of conduct that has developed in basic research serves to maintain what Robert Merton called the ethos of science (*2*). It involves the acceptance or rejection of reported findings of other workers on the basis of what Merton terms "preestablished impersonal criteria," and the public presentation of scientific findings (usually, and preferably, after critical review by editors and referees) so that they are available to the whole community. It also involves the social system of "organized skepticism" that subjects reported findings to constant critical review, with no assurance of finality. Scientists are expected to point out the limits of uncertainty in their findings and the inferences they draw, and they are expected to acknowledge their debts to others whose work, both published and unpublished, has contributed to what they have achieved. Science is a communal enterprise; every contribution builds upon the work of others.

This is an idealized picture. Acknowledging the debt to other workers is indeed central in the ethos of science, yet it would be intolerable to cite a massive set of references for an ordinary paper. Aggressive scientists are sometimes skillful in getting credit for ideas that others may have published before, but they may also be genuinely ignorant of the earlier work. Even those who are quite scrupulous may pick up ideas from papers for which they serve as referees, or from serving on a panel that reviews grant applications, and they may remain quite unconscious of the source of their ideas. Since recognition of significant originality in discovery is the main road to scientific prestige and honor, most scientists are understandably sensitive to the failure of others to acknowledge their work. A few unusual people are dramatically different: they cast forth their ideas freely, and are happy to see others pick them up. This is what Jacques Monod (*3*) wrote about Leo Szilard:

> Most scientists of course do not formulate any significant new ideas of their own. The few that do are inordinately jealous of, and unduly faithful to, their own precious little ideas. Not so with Szilard: he was as generous with his ideas as a Maori chief with his wives. Indeed he loved ideas, especially his own. But he felt that these lovely objects only revealed all their virtues and charms by being tossed around, circulated, shared, and played with.

I am not an anthropologist and cannot claim knowledge of how Maori chieftains share their wives, but Monod's description certainly characterizes Szilard and other unusual individuals.

The pursuit of knowledge in basic science is inevitably full of rivalry and competition, especially in the fields that are most active, but it usually proceeds in an atmosphere in which there is a great deal of free communication of ideas and active discussion. When obvious major practical results begin to appear, a trend toward secretiveness usually sets in. The most dramatic example is the effect on physicists of the discovery of nuclear fission and the secrecy that followed. More than one distinguished physicist has recalled nostalgically the intellectual freedom of exchange in physics in the years before 1939. A somewhat similar change appears to be taking place among the molecular biologists today, as the techniques of gene cloning hold forth the promise of manufacturing substances of great biological importance, cheaply and

The author is professor emeritus of biochemistry, Department of Biochemistry and Molecular Biology, Harvard University, 7 Divinity Avenue, Cambridge, Massachusetts 02138, and former chairman of the AAAS Committee on Scientific Freedom and Responsibility. This article is based on his address to a joint meeting of The Royal Society and the American Philosophical Society in London, England, on 5 June 1980, published in a symposium on "The Social Responsibility of Scientists" (The Royal Society, London, 1980). It is reprinted with minor revisions, by permission of The Royal Society.


0036-8075/81/0403-0011$01.00/0 

on a large scale. Some of my younger colleagues have told me that they find scientific meetings less interesting than they were, even 5 or 6 years ago; too many people, they say, are clearly holding back information, presumably with an eye to applying for patents on new processes. There have even been charges that some authors of reports are deliberately failing to cite relevant work of others in hopes of claiming a patent on some new biological process or product.

This competitive atmosphere has sometimes led to publicity of a sort previously not practiced among scientists. In a recent article entitled "Gene cloning by press conference," Spyros Andreopoulos (*4*) of the Stanford Medical Center News Bureau quotes a letter from Joshua Lederberg to Senator Gaylord Nelson of Wisconsin. "The possibility of profit—especially when other funding is so tight—will be a distorting influence on open communication and on the pursuit of basic scholarship," Lederberg wrote, although he noted that many, perhaps most, university scientists disagreed with his views. Andreopoulos showed that some new developments announced at press conferences receive wide publicity, before they appear in the scientific literature, while other work of at least equal significance passes through the regular channels of critical reviewing before it appears. Reports at press conferences can be misleading; for example, one new account of the production of human insulin by recombinant DNA techniques created the impression that the product was biologically active; the later publication of the data in a journal showed that this was not so (*4*).

The traditional patterns of scientific reporting and communication—the scientific ethos, in Merton's phrase—may be in danger of undergoing significant erosion. As a believer in the classical tradition of operation in basic science, I hope that the erosion may be halted.

### Independent Scientists and Issues of Public Policy

A more difficult subject is the role of scientists in matters of public policy. Let me begin with a classic example from nearly 20 years ago: the publication of Rachel Carson's *Silent Spring* (*5*) with its vigorous attack on what she considered the gross misuse of pesticides. She was both a trained scientist and a gifted writer. The biological community had been concerned about the ecological damage from widespread use of pesticides such as DDT, but no authoritative body had made a critical study of the problem and publicized its conclusions. The book had an immense impact. It was also attacked by many agriculturists and nutritionists, who called it misinformed, fanatical, or even a hoax. The President's Science Advisory Committee, however, took Carson's charges seriously and set up a special panel of experts to investigate the problem. After 8 months of hearings they produced a report (*6*) that in large measure vindicated Carson's claims and also concluded that massive attempts to eradicate certain insects by pesticides were unrealistic and ecologically dangerous and that "elimination of the use of persistent toxic pesticides should be the goal." President Kennedy released the report in May 1963 and requested the responsible agencies to implement its recommendations.

The pattern of subsequent events is complex; but it would not please either the strong supporters or the fervent opponents of chemical pesticides. Some strong controls have indeed been imposed; DDT, which was the principal focus of Carson's attack, has been banned; but the general use of chemical pesticides in agriculture is probably as widespread as ever, if not more so. Many of the current pesticides are more toxic to humans than DDT. Other poisons, such as the polychlorinated biphenyls (PCB's), used in industry rather than in agriculture, have been recognized as serious environmental hazards. Highly specific pesticides for particular species of insects, such as the juvenile hormones, have been developed but as yet have found little practical use.

Mention of Carson's book can still rouse both enthusiasm and denunciation. Undoubtably in some respects she exaggerated the damage done by pesticides. My own view is that, on balance, she performed a great public service and deserves to be remembered with honor. Certainly the sense of responsibility for the environment that she inculcated is now implanted in a vast number of people.

This episode exemplifies many of the problems that scientists encounter when they become involved in issues of social responsibility. Carson was a trained scientist, but not in the field of agricultural ecology. She had much to learn, and she did learn, in the process of preparing to write the book. The agriculturists still did not regard her as a real professional in their field. However, many, if not most, of the agriculturists had financial and career ties to the use of pesticides and to the industries that produced them. The committees of the National Academy of Sciences that dealt with such matters in those days tended to be dominated by people who had similar biases. The Academy has changed and now examines systematically the industrial and other connections of the members of its committees. The aim is not to eliminate all people with possible bias—that would eliminate most of the experts, in some fields at least—but to obtain a balanced spectrum of people with different kinds of bias, together with some who might be genuinely dispassionate in considering the issues.

Since nearly all controversial issues of this sort involve technology, as well as basic science, the disputes cannot be resolved in terms of "preestablished impersonal criteria." Scientific facts and value judgments are so closely interwoven that it is exceedingly difficult to disentangle them, and the inferences to be drawn are inconclusive. Scientists can honestly disagree as to what inferences can legitimately be drawn from the facts.

Thus we are operating in a quite different domain from that of basic science. The Federation of American Scientists (FAS), which addressed this problem (*7*), accepted as inevitable ". . . that scientists involved in public debate will have to go beyond discussing what is scientifically known for certain," since public policy matters involve the making of decisions in the face of enormous uncertainties. At the same time, the FAS report said that scientists who take an active part in public debates should avoid dogmatic claims, be willing to admit and correct errors in their statements, and reason with those with whom they disagree. However, the report concluded that professional scientific societies are generally unqualified to monitor and pass judgment on the conduct of scientists involved in such debates. The societies are accustomed to dealing with more traditional patterns of conduct within the scientific community and are unequipped to deal with the far more unruly debates that arise when social and political questions are involved. It is the community of scientists who do take an active part in public debate on these controversial issues who must work out appropriate guidelines for responsible conduct. As the debate proceeds, it will become clear who the scientists are who are speaking responsibly and with due respect for the facts.

Weinberg (*1*) holds that the essential sense of responsibility is being eroded in the current debates on such matters as energy policy and environmental protection, with scientists making sweeping pronouncements on issues far outside their own fields of competence. He believes, for instance, that a scientist who thinks he has evidence that current standards of environmental protection are too lax should submit his findings to a refereed scientific journal before publicizing them. If the journal rejects the report, the author may honestly believe that the reviewers are biased. In that case he may be justified in bringing the matter before the public, while admitting that others disagree with him.

There are many cases in which such a procedure will help bring more rationality into the debate; but scientists discussing public issues are often involved in public discussions, or interviews on television, where the limited time makes it impossible to state all the reservations that a careful scientist might add to qualify his remarks. In the heat of debate there is also the tendency to overstate the case. Politicians and others would like simple answers to complex questions. Certainly scientists should be prepared to state publicly that they have made erroneous statements, and correct them; on this vital point there is no disagreement between Weinberg's position and that of the FAS.

Among the value judgments involved in these controversial issues, a fundamental difference of view is often present. If, for instance, the evidence is inconclusive about the toxicity of some industrial product, should it be banned until it is proved safe or used until it is proved dangerous? Until the last two or three decades, the latter policy was most commonly accepted. Recently the more cautious policy has prevailed; the increasingly severe standards for the licensing of drugs by the Food and Drug Administration represent perhaps the most striking example. Such caution has its penalties as well as its merits; for example, Carl Djerassi (*8*) pointed out the difficulties in the development of new and better contraceptives that the strict rules of testing have imposed. Sometimes more is lost than gained by excessive zeal in testing before release. This is likely to be true for the selective pesticides that act by inhibiting the development of certain species of insects.

Decisions on such matters as building an airport or a power plant, or damming a river, inevitably involve value judgments as well as technical facts. They require estimates of future needs, which are often highly unreliable. For example, the estimates made a decade ago about future needs for electric power in the United States have been drastically scaled down in the light of experience. Expert testimony in such matters is likely to be colored, consciously or unconconsciously, by the expert's system of values.

Cost-benefit analysis in such situations of conflict is a treacherous game; the costs and benefits are usually quite incommensurable; ultimately decisions are likely to be made by the political process in which the public perception of what is desirable counts for more than the cost-benefit calculations of experts. Lord Ashby (*9*) concluded that it is probably better so:

> All attempts to rely on quantification in such decisions as these, to create them out of computer scenarios, to deduce them from cost-benefit balance sheets, are likely to make the decision worse, not better; for in the process of getting hard data, the fragile values, the unquantified information, the emotive elements which nourish the public conscience, all run through the filter and are lost, and so the quantified information assumes an importance out of proportion to its real value.

## Whistle-Blowing and Professional Responsibility

Scientific and technical professional employees, in industry or government, on occasion have reason to sound warnings of dangers about processes or products, or sometimes to call attention to opportunities for improvement that they believe are being neglected. Obviously employees should approach their superiors, point out the source of trouble, and urge correction. If the superiors fail to respond and the issue is really serious, the employee can bring it before the public. People who do this are commonly called whistle-blowers (*10*).

Whistle-blowing is obviously a high-risk occupation, and those who practice it must be prepared for trouble. A classic example arose during the building of the Bay Area Rapid Transit (BART) system in San Francisco (*11*). A major feature of the system was the automated train control, developed under a contract with the Westinghouse Corporation. Three engineers, Max Blankenzee, Holger Hjortsvang, and Robert Bruder, concluded that the system design had grave defects, but their concerns were disregarded by the management. Finally, early in 1972, they went to BART's board of directors which, after a hearing, voted 10 to 2 with management. The three engineers were fired. Subsequent dangerous failures of the automated train control, which occurred after the system started to operate, fully vindicated the engineers. The California Society of Professional Engineers investigated the case and decided that the dissenting engineers "had acted in the best interest of the public welfare." The California legislature conducted an investigation that confirmed the validity of the engineers' warnings. The three then sued Westinghouse for $885,000 but eventually settled out of court for a relatively modest sum, which was probably quite inadequate compensation.

A more recent case involved Clifford Richter, a health physicist at a state hospital in Columbia, Missouri (*12*). He reported certain violations of safety regulations at the hospital to the Nuclear Regulatory Commission, as he was in duty bound to do by law. The hospital management retaliated by abolishing his job. A federal court eventually ordered his reinstatement, under the employee protection section of the Energy Reorganization Act, and the payment of back salary. The reinstatement has been challenged, however, and appealed to the U.S. Supreme Court.

In another case, Morris Baslow, a marine biologist, was fired after he presented evidence, in a court hearing on a U.S. Environmental Protection Agency inquiry, concerning the effects of cooling water from power plants on fish in the Hudson River (*13*). He urged his employers to present the evidence, but when they ignored his recommendations, he finally presented the data to the court directly. Eventually he reached an agreement with his former employers, but only after many months of delay, while he was out of work.

In these cases the whistle-blowers put their jobs and reputations in jeopardy. It is obviously in the interest of public health and safety that such people should be heard and fairly judged; and if their views are upheld after a hearing by a suitable body, they deserve commendation, and perhaps promotion, not discharge. Congress has passed several laws in recent years to protect the rights of employees who report to their employers matters that call for correction. The Nuclear Regulatory Commission is now formulating rules that should encourage employees to report matters of concern to higher officials, with guarantees against reprisals, whether or not the employee's recommendations are accepted. This represents an encouraging trend in the Executive Branch of the government. Rules to protect employees are still nonexistent in most private businesses, though a few firms have begun pioneer moves in this direction. David Ewing of the Harvard Business School has outlined detailed proposals for further reform (*14*).

Of course whistle-blowers are not always right. They might be motivated by personal malice, they may be cranks, or they may be honest, but mistaken. Both common sense, and a sense of loyalty to the employer, dictate an earnest effort to settle differences of opinion by working within the organization. However, if higher authorities fail to respond, and if the matter appears to involve serious issues of human safety and health, it may be necessary to bring the matter to public attention. The individual who takes such a risk obviously needs good legal advice and other kinds of help (*15*). Our complex society needs increasing input from those who perceive otherwise unnoted risks or opportunities and bring messages that may be unwelcome to established authorities. To use criticism and dissent constructively in dealing with both risks and opportunities, clear policies are needed, with definitions of procedures for due process in controversial cases and, if necessary, formal hearings and a possibility of appeal.

The polarization of opinions on some issues today is disturbing. The conflict between the advocates and enemies of nuclear power is one example; the dispute over the origins of cancer is becoming another. Richard Peto (*16*) described the distortions and untruths promoted by tobacco companies in their efforts to discredit the overwhelming evidence for the relation between smoking and lung cancer. At the same time he severely criticized some of the alleged evidence that would ascribe nearly all cancers to toxic substances introduced by man. S. S. Epstein, whom Peto sharply criticized, has responded vigorously (*17*). The gravity of the hazard from industrial carcinogens, to workers and others, is clear; but their relative role in the totality of human cancers is still hotly debated. In the bitterness of such controversies, either side may distort data. As Peto remarked, "Scientists on both sides of the environmentalist debate now have career interests at stake." But it is important above all that the passion for getting at the truth should be the dominant passion for scientific workers when they are trying to act as responsible scientists. That may appear sometimes to be an unattainable goal in the atmosphere of current debate, but it is worth striving for, both to maintain the confidence of the public and to keep confidence in ourselves.

### References and Notes


1. A. Weinberg, *Minerva* **16**, 1 (1978).
2. R. K. Merton, in *The Sociology of Science* (Univ. of Chicago Press, Chicago, 1973), pp. 267-278; A. Cournaud and M. Meyer, *Minerva* **14**, 79 (1976).
3. J. Monod, in *Collected Works of Leo Szilard: Scientific Papers*, B. T. Feld and G. W. Szilard, Eds. (MIT Press, Cambridge, Mass, 1972), p. xvi.
4. S. Andreopoulos, *N. Engl. J. Med.* **302**, 743 (1980).
5. R. Carson, *Silent Spring* (Houghton Mifflin, Boston, 1962).
6. U.S. President's Science Advisory Committee, "Use of pesticides" (The White House, Washington, D.C., 1963); the report was reprinted in *Chem. Eng. News* **41** (No. 21), 102 (1963). J. Primack and F. von Hippel [*Advice and Dissent: Scientists in the Political Arena* (Basic Books, New York, 1974), pp. 41-46] provide a useful account of the effect of the report.
7. Federation of American Scientists, "To whom are public interest scientists responsible" (Public Interest Report 29, Washington, D.C., 1976).
8. C. Djerassi, *Science* **169**, 941 (1970); *Bull Am. Acad. Arts Sci.* **32**, 22 (1978).
9. E. Ashby, *Perspect. Biol. Med.* **23**, 7 (1979).
10. R. Chalk and F. von Hippel, *Technol. Rev.* **81**, 48 (1979).
11. J. T. Edsall, *Scientific Freedom and Responsibility* (AAAS, Washington, D.C., 1975).
12. L. J. Carter, *Science* **207**, 1057 (1980).
13. C. Holden, *ibid.* **210**, 749 (1980); J. L. Lawler, *ibid.* **211**, 875 (1981).
14. D. W. Ewing, *Freedom Inside the Organization: Bringing Civil Liberties to the Workplace* (Dutton, New York, 1977). See also A. F. Westin, Ed., *Whistle Blowing! Loyalty and Dissent in the Corporation* (McGraw-Hill, New York, 1981).
15. P. Raven-Hansen, *Technol. Rev.* **82**, 34 (1980).
16. R. Peto, *Nature (London)* **284**, 297 (1980).
17. S. S. Epstein, *Nature (London)* **289**, 115 (1981); ——— and J. B. Swartz, *ibid.*, p. 127. Compare J. Cairns, *ibid.*, p. 353.


18. I am much indebted to my fellow members of the AAAS Committee on Scientific Freedom and Responsibility for our continued joint work relating to many of the problems discussed in this article. I am particularly indebted to R. Baum, F. von Hippel, J. Primack, and R. Chalk. This article is, however, an expression of my personal views. This work was supported by grant SOC7912543 from the National Science Foundation.


# Polypeptide-Binding Membrane Receptors: Analysis and Classification


Jerry Kaplan


Mammalian plasma membranes are in general impermeable to molecules of high molecular weight. Thus mechanisms are present by which the structural elements, or information content, of macromolecules are made available to cells. Fluid-phase pinocytosis is one mechanism by which macromolecules may gain access to intracellular compartments. However, fluid-phase pinocytosis is nonselective; that is, uptake of molecules is solely a linear function of their extracellular concentration (*1*). In addition, endocytosis is probably a mechanism for termination rather than initiation of information transfer. To overcome the lack of selectivity of fluid-phase endocytosis, cells have developed plasma membrane receptors capable of forming high-affinity complexes with specific ligands.

There is great diversity in the chemical nature of both ligands and the receptors to which they bind. However, studies in my laboratory and elsewhere suggest that membrane receptors for soluble polypeptides may be divided into two categories. The basic distinction between receptors is that the binding of ligand by class I receptors leads to changes in cell behavior or metabolism. These changes result from the interaction of ligand and receptor at the cell surface and, although ligand internalization may occur, it is not a prerequisite for ligand function. The major role of class II receptors is to mediate ligand internalization. Binding of ligand to class II receptors does not per se lead to alteration of cell activity. Modifications of cell behavior, if they occur at all, are consequences of ligand metabolism. Receptors in each class show similarities in their divalent ion requirements, topographical distribution, and regulation.

In this article I consider only receptors for soluble polypeptides, excluding those for cholera and diphtheria toxin. A major caveat to any analysis of receptor behavior is that, with rare exceptions, what is measured is not receptor molecules but


The author is an associate professor in the Department of Pathology, University of Utah Medical School, Salt Lake City 84132. The article was written when Dr. Kaplan was an assistant professor in the Department of Physiology, University of Connecticut Health Center, Farmington.