PERSPECTIVES
On the Professions

A periodical of the Center for the Study of Ethics in the Professions (CSEP), Illinois Institute of Technology

HUB Mezzanine, Room 204, 3241 S. Federal Street, Chicago, IL 60616-3793
Tel: 312.567.3017, Fax: 312.567.3016, email: csep@iit.edu

Vol. 8, No. 2 January 1989

"The Ethics of Scientific Research"

Michael Davis, Editor, CSEP, Illinois Institute of Technology

The Fall Issue of the Professional Ethics Report, a newsletter of the American Association for the Advancement of Science's Committee on Scientific Freedom and Responsibility. was not at all about the science most of us grew up admiring. Consider only the news items on the front page.

The first reported that Congress has appropriated $2,760,000 to establish a National Practitioner's Data Bank for information of adverse action taken by state medical boards, courts, hospitals, medical societies, insurance companies, peer review committees, and the like against physicians and other "health care workers. "The Data Bank is supposed to make it harder for physicians and medical researchers to avoid the harsh consequences of wrongdoing simply by moving from one job (or jurisdiction) to another.

The newsletter's second item announced that the period for public comment had ended on new Public Health Service regulations enlarging the responsibilities of grantee institutions and funding agencies to respond to charges of scientific fraud or other misconduct.

A third item reported a hearing before the House Subcommittee on Human Resources and Intergovernmental Relations in which its chair, Rep. Ted Weirs, expressed concern that neither universities nor the National Institutes of Health seem capable of dealing with a charge of scientific misconduct or of protecting whistleblowers. Rep. Weirs also suggested that the increasingly close relationships universities and individual researchers are developing with pharmaceutical houses and other businesses could jeopardize the objectivity of related university research.

Yet another story reported that Stephen E. Breuning, a researcher who had falsified some government-funded drug treatment studies, had been sentenced by a U.S. district court to five years probation, 60 days in a halfway house, and performing 250 hours of community service. He was also ordered to repay $77,352 to the National Institute of Mental Health and to refrain from psychological research while on probation. Breuning will go down in history as the first scientist whose scientific misconduct was punished as a criminal act. He does not seem destined to be the last.

The Professional Ethics Report carried one more story in this vein. The National Association of Social Workers (NASW) is reviewing the ease of a researcher who submitted copies of a fabricated article to over 100 professional journals as part of a study to assess their review procedures. Several of the journals filed complaints against the researcher, charging that he had violated the NASW's code of ethics by deceiving the subjects of his research. The standard of good research today seems to differ considerably from what it was a decade or two ago when deceiving research subjects was still routine.

These news items suggest great changes in our understanding of scientific research-and, perhaps too, of science generally. They also suggest a need to look much more closely at the ethics of science. We have taken that suggestion.

This issue of Perspectives begins with Robert Sprague's account of how he blew the whistle on Stephen Breuning, why he did it, and what happened as a result. Except for Sprague's tenacity, it is a sad story indeed. Though Sprague stresses the importance of Breuning's research for ordinary people, we are likely to be struck more try how most of the scientists, university administrators, and government officials involved seem to have acted as if the research affected no one at all.

In a second piece, Ullica Segerstrale, a sociologist, tries to understand why contemporary physics has so far seemed free of the misconduct be deviling the health and social sciences. Her preliminary research suggests that physics is not so much free of scientific misconduct as such as distant enough from application of its research to allow bad work to be detected as one researcher tries to build on the work of another. Physicists do not much care what the cause of error might have been.

In a third piece, Robert Bergman describes the difficulties a chemist routinely has in replicating the experiments of others. The difficulties seem to be much greater than in physics-at least according to Segerstrale's physicists. Bergman suggests that most failures to replicate probably result from unnoticed variations in laboratory practice, not from "trimming," "cooking,' "fudging," or other unethical conduct.

Last, Dow Woodward, a biologist, argues for a very wide interpretation of research ethics, especially for biology. He in fact thinks that "bioethics" (as he calls it) must begin with an appreciation of what science really is (and of what it can be). Woodward (like Sprague) now teaches a course in the ethics of scientific research (or, as he might call it, in the ethics of science). Such courses may some day be as common as courses in business ethics or biomedical ethics are today.

"Irreproducibility in the Scientific Literature: How Often Do Scientists Tell the Whole Truth and Nothing But the Truth?"

Robert G. Bergman, Professor of Chemistry, University of California, Berkeley

Much has been written recently about scientific. fraud. Articles on this subject are noteworthy not simply because they appear, but also because they are often written with a sense of shock. Our society has accommodated itself to regular tales of criminal activity among the general public as well as occasional stories of misconduct among professionals. Outraged by medical and legal malpractice, most of us nevertheless fully expect such incidents to occur and agree that there should be formal mechanisms for dealing with them. Scientific "malpractice," however, still surprises us.

There are several types of scientific misconduct, but the one most frequently discussed is data fabrication. The first suspicion that this type of misconduct has occurred often comes from a breakdown in scientific reproducibility-the inability of a scientist to reproduce a result obtained by a different individual (usually in another laboratory) and published in the scientific literature. This arises from the general expectation that a published experiment, measurement, or calculation contains information sufficient to allow a second investigator to repeat it and obtain results identical to those obtained by the initial experimenter (within the inherent error of the measurements involved). Most scientists also assume that data recorded in an experiment are objective rather than subjective-that is, that the recorded observations are independent of the investigator making them and will not change because a different individual makes the measurement.

How realistic are these assumptions? Clearly scientific misconduct exists, and a number of cases have come to light recently. However, even in the absence of explicit data manipulation, more subtle problems inherent in the way scientists carry out their research make the scientific literature much less reproducible than most people-including some scientists-take for granted.

It is difficult for any one individual to address the question of reproducibility for all scientific disciplines. I will therefore try to provide some information about it in an area close to my own-that of synthetic chemistry, the activity of making complex molecules from smaller (usually commercially available) compounds. It is common for an investigator to start a new project by repeating (or attempting to repeat) a preparation of a compound whose synthesis has been published in the literature, so that the material may be utilized in a new chemical transformation. Research proceeds in a similar way in biology and biochemistry, where the availability and characterization of a previously discovered bacterial strain or other organism can be crucial to the development of a new project.

The startling fact is that almost half of the literature's synthetic procedures we attempt to repeat initially fail in one way or another. A reasonably large fraction of these "recipes" can be reproduced after modification or discussions with the author. Some, however, cannot be repeated in our hands no matter what we do.

Is this troubling experience with reproducibility a general phenomenon? Fortunately, in chemistry we have two unusual journals that pro vide information about this question. They are called Organic Syntheses and Inorganic Syntheses. These journals differ from nearly all others. They were established specifically to publish only synthetic articles that had been deliberately checked in a laboratory different from the one in which they were devised. The names of the checkers appear along with the names of the authors when the articles are finally published.

Discussions with the editors of these journals, who do essentially all the checking of preparations in their own laboratories, is enlightening. Even though a scientist who submits an article knows it will be checked immediately, the experience of checkers is similar to that of the scientists who try to repeat synthesis from the open literature nearly half of the preparations submitted cannot be repeated in just the way they were described by the submitting authors. In some cases the problem is relatively minor, such as when the correct product is obtained but its isolated yield is lower than that recorded by the submitter. In other cases, the product cannot be obtained at all.

However, when such a difficulty occurs in a preparation submitted to Organic Synthesis or Inorganic Syntheses, a control mechanism comes into play: at the recommendation of the journal, the two individuals involved establish direct communication and attempt to resolve the problem so that the preparation can be reproduced in the checker's laboratory. The paper will be accepted for publication only after sufficient details have been communicated so that the synthesis is workable, with comparable results, in both laboratories.

Normally this results in a solution to the problem-but sometimes it does not. The experiments in three of the thirty articles I know of-ten percent-could never be repeated even after extensive communication with the authors. Was this due to fraud? Perhaps the following anecdote will provide some insight into this question.

One checker spent several weeks trying to duplicate a synthesis that seemed to proceed well in the laboratory of a submitter who was a well established, careful investigator. After weeks of work and numerous telephone conversations, it was discovered that a procedure for evaporation of solvent from the product was being carried out for only fifteen seconds in the submitter's laboratory. This was not stated in the written description of the experiment because it had become automatic. In the checker's laboratory, on the other hand, solvent was evaporated under vacuum for a longer period of time. Because the product of the reaction in question was relatively volatile, it was being lost in the procedure.

The overriding problem in such cases is that the researchers fail to describe exactly what they did in carrying out an experiment. It may seem incredible that this should happen to professional scientists. But, it is easy-too easy-in experimental work to fail to write down everything you did, especially when some procedures become automatic in one's laboratory. When this happens, it lakes insight, experience, and intelligence to identify the problem.

Let us turn now to the question of data objectivity. The complete fabrication of an experiment from start to finish is probably rare. On the other hand, "massaging" data-tidying up results, fudging the statistics a little, finding reasons for reporting only favorable data-could well be just as common as scientific gossip assumes it to be. There are undoubtedly many cases, for example, in which straight lines have been drawn through data that, with more experimentation or lower error, would have been clearly demonstrated to represent nonlinear relationships.

The reason for this is that all scientists have expectations about how their experiments will turn out and therefore have a tendency to see what they want to see and ignore what goes against their preconceived ideas. Responsible scientists must consciously force themselves to be suspicious of their own results-especially if they agree with expectations. We must continually ask "could this really nice result be wrong?" We should not trust any result we have measured only once. If a result comes out a certain way, we should try to find a way to get the answer in a different way or from a different perspective. How hard it is to convince research students of this-it seems such a great waste of time!

The most important tool we have available to deal with the problems 1 have discussed is education. Scientists focus so tightly on communicating to students the technical details of our profession that discussion of ethical and psychological issues often falls by the wayside. Sadly, the bulk of discussion of ethical issues that does occur in most scientific laboratories too often takes the form of gossip. Surely we can do better. We can consciously discuss with our students and colleagues situations in which ethical problems arise and try to encourage thetas to think about how to handle them. In the psychological area, it is important for us to consider more explicitly how we make observations and report them, how scientific breakthroughs occur, how old ideas persist when they are no longer valid, and how new ideas are generated and eventually take hold.

If our experiences in research have taught us anything about the nature of the investigative process itself, it is that there is a tendency in research to look for things that support our initial hypotheses. We must therefore convince ourselves and our students not just to double-check things that appear to be wrong-but to be even more suspicious of things that appear to be right.

"A Case of Whistleblowing in Research"

Robert L. Sprague, Institute for Research on Human Development, University of Illinois, Urbana-Champagne

Late in December 1983 I wrote an unusual letter. It alleged scientific fraud on the part of Dr. Stephen E. Breuning, a young University of Pittsburgh psychologist working on one of my research grants. The letter was six pages long with 43 pages of appendices. It was addressed to the National Institute of Mental Health (NIMH), the federal funding agency that had supported my research on psychotropic medication for many years.

I was quite naive at that time. I thought that the most painful part was over-suspecting wrongdoing, investigating it, and finally reporting a promising young researcher. Those activities wrecked a close personal relationship.

In many ways, it is fortunate that we cannot foretell the future. I had no idea at that time that an even more difficult period of five years lay ahead and that this scientific fraud case would end with Breuning being sentenced in a federal court. During those five long years, Breuning's university attempted to cover up his activities and NIMH's investigation moved with glacier-like speed.

Even more emotionally painful for me was my wife's kidney failure in September 1984 after 30 years as a diabetic. She was placed on many medications and hemodialysis to sustain her life. She lived until late April 1986. Her long illness brought home to me how important it is that the research supporting medical treatment be without any hint of scientific misconduct. The lives of people depend on truthfulness and honesty in medical research. Breuning's fraudulent research writings had major policy and medical implications for tens of thousands of quite vulnerable mentally retarded people receiving the neuroleptic medication (tranquilizers) about which Breuning wrote. My wife's suffering and death gave me the motivation to pursue the Breuning case to its conclusion.

When I first met Breuning in 1979, he was employed as a psychologist at Coldwater Regional Center, a residential facility for mentally retarded people in Michigan. In January 1981 he moved to the Department of Psychiatry at the University of Pittsburgh. When I visited Breuning there in September 1983 to discuss research activities, his research assistant told me Breuning was obtaining perfect, is. 100% agreement, between nurses independently rating the abnormal involuntary movements of a neurological disorder, tardive dyskinesia, caused by the long-term use of neuroleptic medication.

Rating the severity of the complex movements of a patient is difficult. No matter how clever a researcher you are, you cannot obtain perfect agreement among nurses who rate the patients. The research assistant's statement may have been meant to belittle me for not being able to produce the outstanding results Breuning was supposedly obtaining. But what she implicitly told me was that he was cheating.

I then launched my own investigation into Breuning's research. Three months later I found the "smoking gun" in his abstract for a symposium on tardive dyskinesia I was planning for a December 1983 meeting. The abstract claimed that he examined 45 patients at Coldwater Center every six months for two year after he left the facility. Since I knew that he left no assistants at the Center and never returned from Pittsburgh during that time to conduct the examinations himself, I demanded he provide me with documentation of the examinations. When he could not, I wrote the letter to NIMH. NIMH asked the University of Pittsburgh to investigate.

Early in the University's investigation, Breuning confessed to falsifying the abstract. In February 1984, the faculty investigating committee reported to the Dean of the School of Medicine that: "Dr. Breuning admitted to us that statements in the abstract were false." Nevertheless, the Dean wrote NIMH in July 1984, summarizing the findings of the investigation, "Briefly stated, our Hearing Board can find no serious fault with Dr. Breuning's activities here in Pittsburgh ...I have no ground to take action against him…"

In February 1985, more than a year after my letter, NIMH finally appointed an investigative panel of five scientists to examine my allegations. Although the panel conducted interviews, no public action was taken until Science magazine (December 1986) published a critical article stating that the investigation "has been dragging on for almost 21/2 years."

Only 23 days later (and over the Christmas holidays), NIMH issued a draft report. Although the final report was delayed, it was finally issued April 20, 1987. It contained a stinging condemnation, "it is the unanimous conclusion of the Panel that "Stephen E. Breuning knowingly, willfully, and repeatedly engaged in misleading and deceptive practices in reporting results of research supported by. . . Public Health Service grants ...."

The final report and evidence was turned over to the United States Attorney in Baltimore. In April 1988 Breuning was indicted on three counts, apparently the first scientist holding a federal research grant to be indicted for scientific misconduct. He was sentenced by a federal judge in Baltimore in November 1988 almost five years after I wrote the letter to NIMH.

Since my NIMH research grant was to end in April 1987, I applied for a renewal. At that time I had been funded by NIMH continuously for more that 16 years. A committee of my peers unanimously approved my renewal application with a priority score (which usually guarantees funding). But in February 1987, little more than a month before my grant was to end, I received a letter from NIMH stating that the grant would not be renewed. After considerable discussion of NIMH's action in the media, another peer committee reviewed the application and recommended that it be funded-but for only one year and at about 10% of the original requested amount.

These events were discussed at a Congressional hearing conducted in April 1988 by Representative Ted Weiss, Chairman of the Human Resources and Intergovernmental Relations Subcommittee. One subcommittee member remarked, "The coincidence, Dr. Cowdry [NIMH's Acting Deputy Director], of this research coming to an end at the same time, it really stretches my believability that they were not related and somehow a reflection on Dr. Sprague. That is one I will have to assign to people that sell bridges in New York. It just is very hard for me to believe that all of this was coincidental."

There is a myth that science is beautifully self-correcting. The few students of scientific misconduct who have written about the topic paint out that the correction is slow and often painful and that peer review seldom catches the misconduct. Usually an insider with information generally not available to others blows the whistle on the scientist who is cheating. Quite often the whistleblower pays dearly for the action.

Such a picture contrasts starkly with what most people believe about misconduct in science. However, it is a picture repeatedly painted in the last few years as case after case of scientific misconduct has been reported in the media. The cases have caught the attention of Congress which is likely to enact legislation about the matter during the next session. Many people believe that Congress may over-react and pass cumbersome laws. Clearly it is time for scientists to do something to clean their own house.

But what can be done? First, scientists can inform themselves on the issues involved. Second, reasonable guidelines can be proposed and established. It will not work to react to the proposed new legislation or federal guidelines by proclaiming that science rarely has misconduct and should be left alone. It seems to me that the mood of the public and Congress will not tolerate such an attitude any longer.

What are some reasonable changes? The record of universities investigating charges of scientific misconduct and taking appropriate action is quite poor-with a few notable exceptions. University administrators and faculty should be willing to investigate speedily and appropriately such charges and take disciplinary action when necessary. To fail to do so only makes the matter worse when the misconduct is publicized (as much misconduct eventually is).

Journals should take the lead in reporting substantiated cases of scientific misconduct and correcting the literature when necessary. Unfortunately, the journals are either reluctant or slow to take corrective action.

Courses about the ethics of science should help make students aware of the problems and, hopefully, discourage those few among us who are willing to break the rules. I taught such a course in the fall semester of 1988 at the University of Illinois. I am willing to share my syllabus and materials with anyone wanting the information.

Finally and most important, a change of the scientist's attitude toward the problem is needed. Nobody has accurate information on the prevalence of scientific misconduct. But that is not the important question. The problem is serious, it needs to be recognized and action must be taken.

""Right is Might": Physicists on Fraud, Fudging, and ‘Good Science’"

Ullica Segerstrale, Social Sciences, Illinois Institute of Technology

There is now a long "dishonor roll" in science. Contemporary physicists are notably absent. Why? Is misconduct less common in physics than in other sciences? Or are there skeletons waiting in the closet?

One way to make a start at answering such questions is to ask physicists. I asked R, P, B, and S (as I shall call them). What follows is some of what I was told.

R (whose motto is "Do it right") suggested that in high energy physics, teamwork makes the difference. Very few experiments are done only by one or a couple of persons these days. There would have to be a huge conspiracy to pull off real fraud. People would not keep their mouths shut. The temptation would be greater if you were a solitary person, with no one looking over your shoulder. True, physics is very competitive, but physicists check each other. "People are interested in turning out the correct results. If it is not correct, it is garbage."

According to P (a physics department chairman), experiments are getting increasingly automated. It is not uncommon to have data taken totally by computer. That, he thinks, is part of the difference between physics and fields such as biology. But there are other differences: "As I understand it, in physics the questions are: Under what controlled conditions are you taking the data? How accurately do you make the measurements? What are the sources of random error? What is the signal to noise ratio? and so on. In biology, they don't seem to worry about things like that at all . . . The important thing is whether you have an effect or not. If you see it, it tends to knock you over the head."

Another thing differentiating physics from other fields, according to P, is that physics is seeking fundamental knowledge about simple objects. Physicists try to analyze the simplest of all possible situations, using the simplest of materials. Then they go on to study complexities. This means that the experiments are easily repeatable: "When one lab made high temperature superconductors and described the procedure, hundreds of people could do the same thing and did it overnight. The minute these people published, the materials were accessible to everybody. So-if they hadn't been absolutely careful in their description-credit, Nobel prizes, fame, fortune, and funding would all have gone out the window. They wanted to be very careful before they announced the result."

P also suggested that reproducibility is easy because physics is a much more unified discipline than chemistry or biology. In physics, everybody has the same fundamental training.

How then do you learn "good science"? According to P, "We try to teach our students to estimate things within an order of magnitude so that they have a feeling for when things are going astray. Then, when they see something unusual, it has to be something that doesn't go away, something that is perfect; no matter how you look at it and try to eliminate it as an extraneous effect, it stays. It is not sufficient just to see something unusual, it happens all the time. . .You'd better do everything, because you'd better be right. My thesis advisor once told me: 'Be sure you are right whenever you publish something, because other people will soon forget, but you will always remember.'"

R tries to teach his students by example. "They grow up in a certain environment, like my children." Asked whether he could formulate any guidelines for good physics, he simply said: Do it right!

The biophysicist B has a darker view of the situation: "There is fraud in physics, but the physicists respond to it in a more clubby manner." B offers as an example Joseph Weber's experiment with gravitational waves (19613-1975). Weber had a gravitational detector at Princeton and was doing time series analysis. He claimed that he was seeing pulses in his computer data. An independent experiment was made at Argonne, where they didn't find any pulses. Weber had the computer tapes sent to him and claimed he found the same pulses at the expected places in the Argonne tapes. The story I've heard is that-unknown to Weber-the clocks at Princeton and Argonne were set at, respectively, Eastern standard and Greenwich mean time... So he proved himself to be a fraud."

"Fraud?" I asked, "You mean self deception?"

B replied: "That is the way the physics community would choose to interpret it. Whereas the biological community would say: the guy is a fraud! the physicists say: over enthusiastic interpretation."

P also mentioned Weber's gravitational waves, adding that Weber himself has never changed his mind. R also knew the story. Physicists seem to have only a few good stories.

From B's point of view, non reproducibility is a fact of life in science, but it is not a problem as long as you have followed good laboratory practice: "There comes a point in science where there is no percentage in trying to correct something. All you do is try to measure it to the best of your ability and don't worry about why it differs from somebody else's. This is one of the mistakes that Feder and Stewart make. They pick on these little details. They are interesting, but they are not going to lead to anything useful. Whether it is 2.9 or 3.1, does this have any bearing on the principal points of the science? If [not], you should make sure that things are calibrated correctly, that proper care is being taken, and that things are reproducible in your own hands. What is central in science is that you use good laboratory practice..., and that you find an observation that is self-consistent in your hands. Then you proceed. It is usually no benefit to go and pursue why [someone else] found a near but noticeably different number than yourself."

What about cases of fudging data? I brought up Millikan s oil-drop experiments (1910-13) and the fact that Millikan stated in a publication that his result for the charge of the electron was based on the average of all the oil drops over a period of time when he had in fact omitted some bad readings. R said that of course it is a lie to say something like that, adding that it also happens nowadays: "'This is the average over the entire period.' That is a lie, but people do make statements like that. 1 don't think it is a terrible crime."

P's reaction to Millikan's claim that he had included all the oil drops was similar: "That is a misleading, possibly even a false statement, but I wouldn't say it is fraudulent. Things can go wrong with experiments, and sometimes you know some readings are not good but you don't know why. That was probably the case with Millikan. Subsequently, his experiment has been repeated and automated at Argonne by Ray Hagstrom. Hagstrom didn't find a single droplet that was mysterious, not a single deviation from the unit charge."

Physicists, it seems, are not so interested in how you came by your result, if later experiments confirm it. This is important for whistleblowing. P said that if you are going to call a scientist a cheat and liar, you challenge his most basic reason for existence. P's experience is that you'd better be right, and that in two ways. You have to show not only that the conclusion is unwarranted based on the data, but also that the conclusion is wrong: "Because if you simply accuse people of throwing away bad data, or of improving the statistics a little bit, or not taking into account systematic errors, etc. and the results are ultimately published as a number, if that number holds up in the future, then no matter how the person came up with the conclusion, he isn't going to look that bad .. .. On the other hand, if it is a straightforward experiment and someone in the future gets it to disagree by a substantial amount, then he will look bad."

But the skeptical physicist S thinks that physicists are unduly pleased with the present situation. He says he knows of thirty cases where teams have been wrong. The problem is that everybody trusts one another, while no one has access to the raw data. At different levels of analysis, important information tends to get lost. So, for S, physicists may well be doing it right, and still be wrong.

"The Challenge of Understanding and Teaching Broad Aspects of Bioethical Principles"

Dow D. Woodward, Biology, Stanford University

I find it difficult to separate teaching scientific ethics from the practice of science or indeed from the teaching of ethics in general. To teach bioethics critically means challenging cherished and popular beliefs.

Most important among these beliefs is that modern science is necessarily a reflection of reality, that its product is nothing but truth. Science can be and has been socially constructed, just as any other institution is. A scientific community reflects the values of the society that produces and supports it. A sexist and racist society will produce a sexist and racist science in spite of the claims of the scientific community to objectivity. In fact, the rhetoric of "scientific objectivity" is so integral to the mission of science that those scientists who actively question the social uses of science or the power relations which determine its direction, risk being classified by the scientific community as no longer "objective."

"Objectivity" has become a code word for the political, ethical, and social passivity of those scientists who have tacitly agreed to accept a privileged scientific or social position in return for their political silence. Members of the public who take on the ethical and social responsibility that most scientists refuse, are usually dismissed as uninformed alarmists. A critical issue is treated as an expression of popular anxiety. Experts are called in to calm the public rather than to articulate the grounds for concern. In other words, a society attempts to produce scientific knowledge serving its perceived economic and political interests.

Science has become a major social investment, to be funded by the state. This investment has been reproduced in universities and private corporations. Funds for science follow social priorities established by existing relations of power. An ethical critique of science might be used as a tool to determine what would be required to liberate science from the power structure and return it to the service of humanity.

In many ways, a discussion of bioethics is, in effect, a discussion of social ethic,. Whether or not we usher in the age of genetic engineering is no more a scientific decision than whether or not we develop nuclear arms. Such decisions are defined in part by profitability and in part by the uses anticipated.

A few biologists and bio-ethicists have warned society for a considerable period of time of the consequences that some manipulations of nature will have on future generations. That these warnings have been systematically ignored or diffused is testimony that those who depend most on exploitation affecting the ecosystem are only concerned about whether they individually outlive the resources they exploit.

If our moral obligations include unborn generations, we should not present a dying planet to its future inhabitants. Such a catastrophe can be avoided only if we learn from history and extrapolate into our future in all decision making. The possibility of nuclear annihilation of many life forms; destablization of the ecology; breakdown of the ozone layer; depletion of non-renewable resources; pollution of air and water; overpopulation; decreasing capabilities to provide food, water, and energy worldwide; and the systematic destruction of the rain forests; each creates a crisis. The very system that produces these crises also prevents their solution.

The victimization of people by

their own institutions must be challenged. The balloon payment for the high standard of living in America (and for the excessive wealth of some of its members) is about to come due. The ecosystem has been overtaxed and will not withstand continued exploitation. The establishment has, it is true, set up a variety of agencies to protect the environment. Yet, at the same time, the establishment has also set in motion machinery the by-products of which systematically destroy our resources and threaten life on the planet. This is the same mentality that requires cigarette packages to state that smoking is hazardous to health while providing money to subsidize the tobacco industry.

Merely presenting the data to those who occupy positions of economic or political power does not cause social change. Some hazardous technologies are staunchly defended by economic and political power because technology in general has become such an integral part of their very existence. But the same technologies, legitimized by their alleged ability to "better human life," often have byproducts that reduce the quality of life at an even faster rate.

In teaching bioethics, it is useful to adopt the practice of analyzing science historically to discover patterns. These patterns demonstrate the role of science in society historically, the uses to which science has been put, and the flaws in the scientific establishment's view of reality. One can then analyze science philosophically to discover how little philosophers and scientists have interacted and therefore how little they have learned from each other. Recently, a few have deviated from that pattern (e.g., Thomas Kuhn, Everett Mendelsohn, and Paul Feyerabend as philosophers and John Farley and Michael Ruse as historians). Their kind of interdisciplinary blend is, I think, essential to getting closer to how science actually functions within the scientific community and within society and how it has performed its legitimizing role.

It is important to note in all of this that teaching itself is only legitimate in the existing society in its role of reifying the status quo. The real anomaly of the myth of "free educational institutions in a free society" is that such an institution could only exist in a society not founded on exploitation. Freedom to teach dissenting ideas in this society has been minimal and mostly illusory. One is free to teach anything within well defined limits so long as it does not have any "undesired" effects. Teaching bioethics critically involves walking a fine line.

Perhaps more threatening than nuclear war (which overtly threatens everyone, exploiters included) to the long term survival of the planet (life forms intact) are the biological threats: What is the critical threshold of the ecosystem to the increasing breakdown of food chains by species extinctions, or the reduction in oxygen normally provided by the rain forests that are now being systematically depleted? Might the weak link be a genetically engineered world replacing the one produced by natural selection?

Of course, the world might not endure the other insults long enough for the genetic: engineers to do any serious harm. Nonetheless, what is already on the drawing boards of a few people shows an audacity roughly comparable to that of last century's eugenecists. These genetic engineers consider themselves more insightful about complex ecosystems than natural selection's millions of years of trial and error. Not many such people need "boldly go where no one has gone before" in order to create problems for us and future generations (as has been so amply demonstrated historically). The main difference between the present and the past is that with the aid of our technology, we now possess a much greater capacity to destroy.

Since the status quo cannot be defended ethically, practically or rationally, change is mandated. It is necessary to treat the concept of social change broadly. We must begin by debunking the myths of human nature that dominate current ideology (including sociobiology) and to resurrect the concept of malleable human nature that allows for the needed social change. We are all accustomed to the notion that we want our children to be shaped by "good influences." What are the material conditions within society that produce those "good influences" for society? What are the material conditions that produce mentally and physically healthy individuals? What are the conditions that generate in them cooperation, support, sharing? What conditions would teach them ethical sensitivity to the environment instead of the greed, competition, and dishonesty that this society has created?

We must challenge the exploiter's "right" to destroy planetary life, a "right" legitimized in part by the human institution of free enterprise. To sit idly by and watch a civilization find new ways to die is not an option for an enlightened, ethically aware person.

The Center for the Study of Ethics in the Professions at the Illinois Institute of Technology was established in 1970 for the purpose of promoting education and scholarship relating to ethical and policy issues of the professions.