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On the Professions

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"Fraud and Sloppiness in Science"

Warren Schmaus, Illinois Institute of Technology

This past year has seen a flurry of governmental activity concerned with fraud in science. Congressman Albert Gore, Jr., Chairman of the Subcommittee on Investigations and Oversight of the House Committee on Science and Technology, held hearings on this issue March 31 and April 1. Not to be outdone, Senator Orrin G. Hatch, Chairman of the Senate Committee on Labor and Human Resources, held a similar set of hearings on June 2, which were closely followed on June 5 by a special meeting in Boston of the President's Commission for the Study of Ethical Problems in Medicine and Biomedical and Behavioral Research.

Both of these latter hearings focussed on the case of Marc J. Straus, who in 1978 was the principal investigator of a research team at Boston University that submitted falsified data to the Eastern Cooperative Oncology Group as part of a project funded by the National Cancer Institute (1). When the fraud was revealed. ECOG purged all of its B. U. data and expelled the B. U. research unit from the collaborative study.

Although the NCI was notified at the time, it did not begin an investigation of the affair until two years later, after the Boston Globe had already run a five-part series on the Straus case, from June 29 to July 3, 1980. In the meantime, Straus had moved to the New York Medical College in Valhalla, N.Y., and had been awarded a three-year research grant from the NCI for aver 1.3 million dollars, of which he had already received one third.

Senator Hatch has charged the NCI with mismanagement of public funds for failing to debar a researcher known to be connected with a lab scandal from further grants.(Science 212 (1981): 1366-67.) Dr. Vincent T. DeVita, Director of the NCI, expressing concern over wasting tax dollars, nevertheless defended the Institute's decision on the grounds that the investigation of Dr. Straus for the 1978 case was not yet complete, and that he ought to be considered innocent until proven guilty. It was, after all, Straus' assistants who were directly responsible for fabricating patients' records. The new Department of Health and Human Services regulations concerning the debarment of researchers from federal grants unfortunately say nothing explicit about whether or not the principal investigator ought to be held accountable for the actions of his subordinates.

The publicity surrounding this and other cases has raised fears that there is a lot more fraud which goes undetected. In a talk presented at the Annual Conference of the Council of Biology Editors in Boston this April, Frank B. Colley expressed the view that the sensational cases of unethical conduct reported in Science are but the "tip of an iceberg." He holds the lack of interest in repeating experiments to be at least in part responsible for a rise in fraud.

Many of those who testified before Congressman Gore's subcommittee, however, do not share Galley's apprehensions. Philip Handler, former President of the National Academy of Sciences, argued that fraud with respect to significant work will ultimately be disclosed either through repeating experiments or through other developments. He furthermore refused to see undiscovered fraud in less significant work as presenting a serious problem. Even if the experiments in question are not repeated, fraudulent work will be brought to light because of the cumulative nature of science, as Donald S. Frederickson, former Director of the National Institutes of Health, has pointed cut. Questionable research will be revealed when any attempt to build on it creates insuperable difficulties.

The weak point in such arguments is that the self-correcting feature of science is designed to disclose falsity, not fraud. Honest mistakes are revealed and lucky guesses pass muster as genuine science. As Ian St. James-Roberts has reasoned, only the researcher himself knows when he has committed fraud, as only he has access to his intentions. (New Scientist 71 (1976): 481.)

It is also a mistake to argue, as William Raub of the NIH seems to do, that fraud in science is limited to a few nasty individuals in an otherwise well-policed profession. There are simply no statistics on this problem, and the suggestion is controverted by the fact that such great lights in the history of science as Galileo, John Dalton, and Gregor Mendel have been guilty of fraud. (Science 183 (1974): 1165-67.)

I believe that intentional misrepresentation of experimental data may actually be fairly widespread, but at the same time I do not see it as presenting a serious problem. Much of the hubbub has been caused by a misconception of science. It is simply a mistake to believe that scientists are, or should be, objective collec toys of facts. A proper understanding of the work of scientists will do two things: (1) it will explain why scientists have not been especially concerned with developing means of detecting and dealing with fraud, and (2) it will reveal that the extent to which it does present a problem for society, it does so only as part of a much greater problem: sloppiness in science.

The Aims of Science

Deena Weinstein has argued that fraud is a more serious problem for science than for any other institution. Representing what is no doubt a common view that science "has the pursuit of truth as its dominant value," she finds fraud to be "antithetical to the very aims of science." (Social Science Quarterly 59 (1979): 639).

The problem with this view is that it fails to distinguish among different kinds of truths, not all of which are of equal importance. Scientists are more interested in establishing the truth of theoretical claims than establishing that of claims regarding observations and experiments. Fraud is not a serious problem for science simply because it is not possible to fake a theory, regardless of how easy it maybe to fake an experiment.

At this point I can imagine the reader objecting: "Aren't scientific theories based on observed facts? Won't fraudulent observations then yield false theories?" Not necessarily. It is at least logically possible to draw true conclusions from false premises.

A deeper problem with this objection is that it relies on a distorted view of how scientists work. According to this view, scientists are supposed to begin by collecting facts without any preconceived notions or biases. These facts are then to be recorded, analyzed, and classified in order to allow the scientist to reason to a general theory concerning them. Further experiments are then performed to corroborate this theory.

This narrowly defined inductivist view of science is simply untenable. Potentially, there is a limitless number of facts in nature which a scientist may observe. The philosopher Carl Hempel asks:"Are we to examine, for example, all the grains of sand in all the deserts and on all the beaches, and are we to record their shapes, their weights, their chemical composition, their distances from each other, their constantly changing temperature, and their equally changing distance from the center of the moon? Are we to record the floating thoughts that cross our minds in the tedious process? The shapes of the clouds overhead, the changing color of the sky? The construction and the trade name of our writing equipment? Our own life histories and those of our fellow investigators?."

This problem is compounded by the fact that there will also be an indefinite number of ways to analyze and to classify any given set of facts.

In order to get his research off the ground, a scientist must have some way of distinguishing facts which are relevant to his problem from those which are not. He does this through imagining some tentative solution to his problem, which then serves as an hypothesis which he can test in the laboratory. Steven Brush quotes Einstein on this point: "But on principle, it is quite wrong to try founding a theory on observable magnitudes alone. In reality, the very apposite happens. It is the theory which decides what we can observe." (Science 183 (1974): 1167.)

But even if it is true that scientists begin with hypotheses and not with unbiased observations, the reader may ask, is it not also true that an honest scientist will reject any hypothesis which conflicts with experimental results?

Again, the answer must be "not necessarily." When a researcher confronts an unexpected turn of events in the laboratory, it is not always clear to him where he went wrong. In designing and predicting the outcome of any experiment, a scientist must make a host of assumptions in addition to the hypothesis under investigation. As a simple example, he must take the laws of optics on faith if he is to regard his telescopic or microscopic observations as reliable. In actual experiments, the situation is far more complex than this. As the philosopher-scientist Pierre Duhem has argued, in cases where a scientist has incorrectly predicted the outcome of his experiment, it is not always possible to determine which assumptions are at fault (2).

Kuhn suggests that for these reasons we have always maintained even our most important theories in' the face of anomalous experimental results, in the hope that these will eventually be explained away in such a manner that our favorite theories will not be threatened. Some, such as Steven Brush, are even willing to argue that the history of science reveals that theoretical considerations have generally carried more weight than experimental results in debates over new hypotheses. (Science 183 (1974): 1169.) A scientist is in far more trouble if his ideas conflict with our most cherished scientific theories than if they contradict a mere handful of experiments.

A scrupulous scientist will nevertheless be quite frank about the problems confronting his theory. Indeed, it is only through taking such a critical attitude that science is able to progress at all. Confronted with a troubling experiment, one hopes that through further testing one may be able to weed out those parts of one's theory which are false.

In this weeding-out process, the question of how various scientific beliefs were arrived at is irrelevant. A geneticist confronting an anomalous experiment would not be tempted to throw out Mendel's Laws simply because Mendel may have faked his experiments. (Annals of Science 1 (1936): 115.) Scientists did not revert to a theory of blending inheritance when Mendel's fraud was disclosed because his laws have received support from further experimental work. Indeed it is precisely because the question of how certain results were obtained is not always relevant to that of the truth of one's theories that science has not evolved procedures for detecting and dealing with fraud.

For this reason, when a scientist is absolutely certain that he is right, he may be tempted to represent fictions as experimental facts. This happened in the case of Robert J. Collis of the Max Planck Institute for Biochemistry in West Germany. Gallia had faked certain results concerning the levels of cyclic GMP and AMP in neuroblastoma cells and hybrid cells. He later confessed his crime in the following way:

"I wish to disclose the fact that papers published in several journals with myself as principal author are not reliable. The curves and values published are mere figments of my imagination, and during my short research career I published my hypotheses rather than my experimental results. The reason was that I was so convinced of my ideas that I simply put them down on paper; it was not because of the tremendous importance of published papers to the career of a scientist. (Science News 111 (1977): 150-51.1

Fraud and Science Policy

Although disclosure of fraud may be of little concern to scientists themselves, the reader may now object, it is important to remember that science does not operate in a social and political vacuum. Policy decisions may be based on scientific work, especially in areas concerned with health. energy, and the environment. Furthermore, society supports scientific research through its tax dollars. For these reasons society at large cannot tolerate scientific fraud.

In the case of public policy decisions, however, I submit that the problem again is one of distinguishing the true from the false rather than disclosing fraud. With respect to the testing of new drugs, for example, which the FDA has attempted to regulate since 1962, an honest mistake can have just as disastrous consequences as a deliberately fabricated report. So might an outdated theory. In order to guard against all forms of error, policy decisions influenced by the testimony of scientists ought never to be regarded as a "closed book," but rather ought to remain open to revision in the light of future scientific developments.

Fraud in science does seem to prosent a serious problem with regard to public support of research. We may want to develop some means of distinguishing fraudulent from honest work for the purpose of debarring unscrupulous scientists from receiving further grants. This is in

fact one of the ends which the Department of Health and Human Services has tried to achieve through regulations established in the fall of 1980. (See story an p. 9.)

These regulations are vague and ambiguous when it comes to spelling out causes for debarment. Nowhere is the term "fraud" explicitly defined. Instead we find reference to "serious unsatisfactory performance" and to" any other cause . . . of sufficiently serious nature as determined by the Secretary to warrant debarment."

To distinguish fraudulent misrepresentation of scientific results from errors resulting from honest mistakes, as we have said, requires access to the intentions of the scientist in question. For practical purposes this often means that one cannot establish fraud unless a scientist confesses to it. John Long's cell cultures provide a case in point.

Although permanent cell lines of other kinds of human cancer cells have been around for some time, Long, a researcher at Massachusetts General Hospital, enjoyed the distinction of having been the first scientist to establish permanent cell lines from patients with Hodgkin's disease. (Science 211 (1981): 102225.) His research career had been quite successful until 1978 when a junior colleague in his laboratory. Steven Quay, obtained some unexpected results in an experiment on these cells. Quay then left for a two week vacation, and on his return, Long informed him that he had repeated Quay's experiment and obtained the expected results. Long then published these results.

Quay was incredulous, and asked to see Longs laboratory notes. Careful study of these and other records revealed that Lang had fabricated his results. When confronted with the evidence, Long resigned, but insisted that this was the only experiment he had ever faked. Nevertheless, his other work was called into question and an investigation ensued.

A troublesome problem with Longs cell lines, which he had openly admitted in his grant application to the National Cancer Institute, was that they contained a gene for a form of an enzyme found only in black people, although Long claimed to have taken his cultures from white patients. Long accounted for this anomaly by assuming that his patents must have been heterozygous-that is, that they had some genes for both the "black" and the "white" forms of the enzyme in question.

Further tests revealed that Longs cell cultures were not even of human origin, but had come from a brown-footed owl monkey. This monkey contains the enzyme in question in a farm similar to that found in blacks. Longs laboratory records revealed that he had indeed performed experiments on cells from such a monkey, and the conclusion was that cell cultures from patients with Hodgkin's disease were contaminated with monkey cells.

The question then arose as to whether this contamination was doliberate or accidental. Accidental contamination of cell cultures is, as a matter of fact, a very common occurrence. Since Long maintains that he did not intentionally tamper with his cell lines, there is no way to prove that he did.

A more thoroughgoing scientist, however, might have conceived the possibility of contamination as soon as he was confronted with the anomalous gene. Even if Lang was sincere about his cell lines, was he not also negligent in failing to pursue this lead? Should his research have been funded by the NCI to the tune of $750,000 in full cognizance of this problem with his cells?

Cases such as this raise the following question: Is it any less wasteful to support sloppy research than to support fraudulent work? A similar question could be raised in the case of Marc Straus. Even if his honesty is ultimately established, is he not guilty of failing to establish some procedures for validating the results of his research team?

The issue of the degree to which a senior investigator is responsible for the data of junior colleagues was also raised in the case of Drs. Philip Felig and Vijay Soman at Yale. Felig had co-authored a paper with Soman, who, unbeknown to Felig, had fudged some data. Although Felig is not directly responsible for any fraud in this case, is he not guilty of some sort of intellectual negligence for failing to familiarize himself with Somari s experiments? Felig admitted to an investigating committee at Columbia College of Physicians and Surgeons, where he was being considered for a faculty position, that he "was not fully conversant with the methodology of Dr. Soman." (Science 213 (1981): 115.)

A good case could be made that such carelessness ought not to be rewarded with tax dollars for further research. I suspect that

more money is wasted through sloppy research practices than through outright fraud. In the Straus case, we recall, Dr. DeVita justified the decision to award Straus a new grant on the grounds that it had not been conclusively established that Straus was guilty of fraud.

Senator Howard M. Metzenbaum suggested that the practice of assuming a suspect innocent until proven guilty may not be appropriate in this context. (Science 212 (1981): 1367-69)I`his point has been elaborated by St. James-Roberts:"No doubt an argument could be made for something along the lines of the legal system to determine sufficient ground for intent and lack of it. Parsimony suggests that a more realistic approach may be to assume guilt unless evidence dictates otherwise. Such an approach may seem odious and may be less than fair on occasion, but it can be justified using the argument that the scientist's first responsibility is to be critical of his work. We may also reasonably ask whether such careless individuals are likely to prove of benefit to science, their colleagues, or themselves by remaining in research. (New Scientist 72 (1976): 469.)

This principle could, of course, be pushed too far. We do not want to debar every scientist who makes a simple mistake. Indeed, it can be argued that we learn through our mistakes. At the June 5 hearings of the President's Commission regarding his case, Straws argued that it would be "unrealistic" to hold a principal investigator accountable for all the results submitted by his research team (3).

Dr. Kenneth J. Ryan of Harvard Medical School, who was present at this investigation, agreed that the accountability of the principal investigator for the actions of members of his research team "should not be absolute." He then attempted to define the investigator's responsibilities as follows:

(1) having proper procedures for the selection of research personnel.

(2) giving proper instructions to the members of his team.

(3) having sound procedures for monitoring or auditing the conduct of the research.

(4) keeping up-to-date on the progress of the research.

(5) protecting him/herself against charges of misconduct.

Although same such provisions as these would be a welcome addition to the present HHS debarment regulations, and might save the tax-payer the expense incurred through erroneous research due to carelessness as well as fraud, these provisions as stated would be difficult to apply. What constitutes "proper procedures and instructions" and "sound procedures?" How would government agencies be able to obtain evidence that a researcher had been careless in any of these ways? In practice, it may prove to be more difficult to sanction a researcher for carelessness than for outright fraud.

Conclusion:

A moral philosopher might object to my analysis on the grounds that it considers fraud strictly in terms of its consequences. Don't we have a duty always to tell the truth, regardless of the casts?

However, one must be careful not to confuse lying with fraud. To maintain that honesty is absolutely obligatory would be to prohibit the socalled "white lie" as well as fraud. Fraud is distinguished from lying in general in so far as fraud is defined as intentional misrepresentation which results in some harm.

For this reason, I have found it necessary to weigh the seriousness of fraud in science on the basis of the damage it may cause. In this regard, carelessness proves to be a more important problem in science than fraud.

Indeed, one of the consequences of sloppiness in science is that it may tempt a scientist to commit fraud. Long and his cell cultures provide a case in paint. The famous "patchwork mouse" affair provides another. Summerlin had successfully transplanted a white patch onto a black mouse. The significance of this result was challenged by Medawar, who suggested that Summerlin had performed this experiment on a heterozygous mouse (Science 184 (1974): 544-50.)

That is, if the black mouse had one white parent and one black parent, a successful skin graft from a white mouse would come as no surprise. On the other hand, if Summerlin could produce a white mouse with a successful skin graft from a black mouse, he would have a significant result, since white mice can only come from two white parents.

In short, Medawar was implying that Summerlin was a careless researcher. Summerlin had no

choice but to produce a white mouse with a black patch. Repeated failure led to his desperate painting of two white mice. If he had given more careful thought to his original experiment with black mice, he might have saved himself from this predicament.

Footnotes

1. My information of the Straws case derives from testimony presented to Gore's subcommittee by Alexander Capron and Barbara Mishkin of the President's Commission for the Study of Ethical Problems in Medicine and Biomedical and Behavioral Research.
2. Pierre Detrain, The Aim and Structure of Physical Theory, (New York: Atheneum, 1974).
3. I would like to thank the President's Commission for the Study of Ethical Problems in Medicine and Biomedical and Behavioral Research for making the minutes of their June 5 hearing available to the Ethics Center at IIT.

"On the Importance of Fraud in Science"

Deena Weinstein, Department of Sociology, DePaul University

"The old chemist's maxim had been, 'Lege, lege, lege, labora, ore, et relege.' Lavoisier's method was not to read and pray, but to dream that some long and complicated chemical process would have a certain effect. to put it into practice with dull patience, after its inevitable failure, to dream that with some modification it would have another result, and to end by publishing the last dream as a fact: his way was to carry his mind into his laboratory, and literally to make of his alembics and cucurbits instruments of thought, giving a new conception of reasoning as something which was to be done with one's eyes open, in manipulating real things instead of words and fancies. "

The topic of fraud in science-its causes, its significance, and the ways in which it might be reducedhas recently generated a brisk debate among observers and interpreters of contemporary science. In a review article in the April 1981 issue of Science William J. Broad remarks: "There is little doubt that a dark side of science has emerged during the past decade. In ever increasing detail, the scientific and general press have reported the pirating of papers and the falsification of data." (212:137). As Broad notes, revelations of fraud in scientific work have created concern among practicing scientists and among those who support scientific research and who use its results, especially because science is a "profession that places an unusual premium on honesty[1]."

Indeed, one might argue that science is the only institution for which the pursuit of truth is the dominant value, other sectors of society holding truth subservient to other values such as profit, love, order, or justice. Fraud would seem, on the face of it, to be antithetical to the essential aims of science. Yet, as Broad points out, some interpreters of science hold that great concern about fraud is probably misplaced because the core of scientific knowledge and the central processes by which it is achieved are not much affected by fraud. A key issue in the discussion of fraud in science seems to be, then, whether the discussion itself is important. The following remarks will assess some of the arguments for holding scientific fraud to be of secondary importance for the integrity of science and will present some reasons for questioning that view.

In his essay "Fraud and Sloppiness in Science," Warren Schmaus has taken the present author to task for holding that fraud is subversive to the aims of science. He argues that the view that science has the pursuit of truth as its dominant value fails to distinguish among different kinds of truth, not all of which are of equal importance." According to Schmaus the major interest of scientists is in "establishing the truth of theoretical claims" and that establishing the truth of claims "regarding observations and experiments" is a secondary concern. Thus, he argues that fraud is not a major problem for science "simply because it is not possible to fake a theory, regardless of how easy it may be to fake an experiment." Schmaus, then, does not dispute that truth seeking is the essential aim of science, but only that the primary kind of truth sought is theoretical rather than experimental.

In order to bolster his case he calls upon Albert Einstein's assertion that "it is the theory which docides what we can observe" and Thomas Kuhn's contention that in the past scientists have held on to their theories even when confronted with anomalous experimental results. Schmaus, however, does not adopt the extreme view that theories determine the outcome of experiments, thereby making experimental evidence an

illustration rather than a test. Instead, he holds that the function of a "troubling experiment" is to encourage further testing aimed at weeding out the false parts of the theory in question. As an example, Schmaus suggests that a "geneticist confronting an anomalous experiment would not be tempted to throw out Mendel's Laws simply because Mendel may have faked his experiments." Whatever Mendel did, Schmaus argues, "his laws have received support from further experimental work." Schmaus concludes that "it is precisely because the question of how certain results were obtained is not always relevant to that of the truth of one's theories that science has not evolved procedures for detecting and dealing with fraud."

A close look at Schmaus' argument shows that it seems to say more than it does. Although he begins by making a distinction between theoretical and experimental truth he ends by implying that the faking of experimental results in particular cases is not a serious problem because scientific knowledge is not established by any single experiment but by a body of work verifying hypotheses and confirming theoretical propositions. Schmaus presupposes here that the preponderance of experimental research is not faked, that most experimenters are honest. If a critical number of experimenters were committing fraud, then the significance of an anomalous experimental result would be impossible to determine. Fraud in science, then, is a secondary issue for Schmaus not because "the question of how certain results were obtained is not always relevant to that of the truth of one's theories," but because he presumes that there is not enough fraud to warrant mistrust of any large body of experimental results.

We do not, of course, know the extent of fraud in science, a condition that leads defenders of the scientific community to assume that there is very little and critics of that community to argue that reported cases of fraud probably are the "tip of an iceberg." Indeed, my study "Fraud in Science", Social Science Quarterly 59 (1979), is an attempt to show how the permeability of contemporary science to business and governmental institutions increases the likelihood that fraud will occur.

But regardless of how much fraud there actually is the relevant consideration here is that experimental truth turns out to be as important for Schmaus as theoretical truth, only the honesty of any particular experimenter is not usually important for the integrity of scientific knowledge as a whole. Schmaus does not address the question of what degree of reported fraud would undermine the trust in experimental results necessary to sustain contemporary science, which is characterized by an intensive division of labor, or that of how much actual fraud would impair theoretical truth. Both of these questions are germane to his view point so long as he acknowledges that experimental truth is integral to scientific truth

In the scientific division of labor far more scientists are experimenters, are in the laboratory, than are theorists. What kind of moral viewpoint should experimental scientists adopt towards their work? At the very end of his discussion of "the aims of science" Schemes tells the tale of Robert I. Gullis of the Max Planck Institute for Biochemistry in West Germany who faked results concerning the levels of cyclic GMP and AMP in neuroblastoma cells and hybrid cells. Goths claimed to have published "figments" of his imagination because he was so convinced of his ideas. Schmaus cites this story as an example of his point that "the question of how certain results were obtained is not always relevant to that of the truth of one's theories" or at least of one's conviction about their truth. Schmaus does not advocate that experimental scientists behave as Goths did, but neither does he draw any other conclusions from the case.

If the honest reporting of experimental results is not a regulative norm of scientific activity, then what guidelines should scientists follow? Should they try to predict whether or not honest reporting will further or hinder theoretical truth? The absurdity of that question paints to the conclusion that whether or not fraud in any particular instance damages scientific knowledge, the internal morality of scientific activity requires honest reporting of results. Indeed, it is the honest reporting of experimental results by armies of Kuhn's puzzle-solvers that eventually supports theories or undermines them, in the latter case paving the way for new theories.

Thus far I have argued within the general context set by Schmaus and have tried to show that insofar as experimental truth is relevant to theoretical truth a preponderance of honest reporting is necessary to

the establishment of theoretical truth. I have further suggested that in light of the foregoing consideration the individual scientist has a good reason to be honest and to eschew fraud in addition to any direct commitment to honesty. Now I will turn to some other considerations which lie outside of Schmaus' context.

First, it is not clear that "scientists are more interested in establishing the truth of theoretical claims than establishing that of claims regarding observations and experiments." In the twentieth century science has become increasingly specialized and, although the overall aim of science is experimentally verified theory, many theories are of narrow range and depend for their verification on the construction of elaborate experiments which create data that are not found outside the laboratory. In such cases, which abound in chemistry and biology, and which have analogies in the social sciences (survey research end small group experimentation, for example), the experiments often overshadow in importance the theories to which they are supposed to refer. Further, the growth of specialization and intensive division of labor makes it more difficult to rely on a preponderance of honest reports to counterbalance instances of fraudulent reporting. There just may not be a large enough body of "further experimental work" to cancel out or to confirm fraudulent results. Also, as specialization increased scientific research tends to concentrate on the production of more precise data that often involves costly procedures. Replication of experiments or even similar experiments may not be performed because of financial constraints.

Specialization then, results in a vast amount of experimentation and testing, but it may not, as Broad says the "defenders of the conventional wisdom" believe, lead to "the accumulation of scientific 'truth'." (p.140). Fraud in contemporary science may not affect very much the "paradigms" of which Kuhn speaks, but it may undermine truth, even theoretical truth, very severely in specialized areas. Throughout science the coupling between general theory and specialized research is loose. The dangers of fraud to reliable knowledge in particular areas should not be minimized.

A second consideration is that experimental science is Janusfaced. Only one of its faces is turned towards theory; the other faces technology. The recent concern with fraud in science is probably in great part a result of awareness that contemporary experimental science is often interchangeable with technology and that technologies, particularly in the "life sciences," directly affect the well?being of humans. In Newsweek of October 12, 1981 Sheldon Penman, a biologist at MIT, is quoted as saying: "There's no distinction between biotechnology and basic science." (98:87). It is no accident, also, that the four cases of fraud discussed by Broad are drawn from bio?medical research. The case of Marc Straus, who conducted cancer research at Boston University and "submitted reports containing repeated falsifications." is especially relevant here. Broad notes: "Replication of a multi?institutional clinical trial, such as the one at Boston University that Straus worked with, is financially and structurally impossible. In terms of the self?correcting mechanism (of science) these are not applicable areas of research, although they may be important in terms of patient welfare." (p. 212). Where experimental science is continuous with applied science then fraud becomes a highly important problem in terms of general moral considerations.

Fraud in science, then, is a primary problem for the institution of science wherever "experimental truth" makes a difference, and it does make a difference throughout the range of scientific activity. By confining himself to the discussion of theoretical truth Schmaus draws the debate about the importance of fraud in science towards that area in which it makes the least difference, though it is still significant. In highly specialized laboratory science "experimental truth" is often of great importance and when experimental science is continuous with applied science and technology it is of overriding importance. It is only by ignoring the nature of contemporary science that scientific fraud can be made a secondary issue.

Footnote

1. Charles Sanders Peirce, Philosophical Writings (New York: Dover, 1955) p.6.

Reply to Weinstein

I am somewhat mystified as to how Deena Weinstein is able to criticize my "arguments for holding scientific fraud to be of secondary importance" without taking into consideration what I hold to be the problem of primary importance: an intellectual

sloppiness which leads to negligent actions on the part of scientists. I take this to be a more serious problem insofar as it is this attitude which creates a climate in which not only fraud, but bad science in general, can flourish.

The case of Doctors Fetig and Soman provides an apt illustration: How was Dr. Fetig able to remain ignorant of Dr. Somari s crime for an entire year, when an outside auditor was able to discover it in three hours? (New York Times Magazine, November 1, 1981, p. 70.) If Dr. Felig had carefully scrutinized Dr. Somari s work before he added his name to Dr. Soman's articles. Dr. Soman would have never even dared attempt to doctor his data. If the lazy, careless approach to science of the Dr. Feligs makes it possible for the outright fraud of the Dr. Somans to find its way into publication, how much genuine, honest error must be creeping into print through the very same route?

"NCI Director Interrogated by Senate Committee"

With the express intent of insuring that federal cancer research money is well spent, Senator Hatch's Committee on Labor and Human Resources held hearings on the National Career Institute's contracting and procurement procedures on June 2, 1981. Although a number of cases of conflict of interest and bookkeeping irregularities were brought to light, the highlight of the meeting consisted of an inquiry into the problem of falsification of research data, with special attention focussed on the case of Dr. Marc J. Straus.

The Boston University research team headed by Dr. Straus was, among other things, in effect keeping two sets of records on patients involved in a government funded cancer study. One set was kept for the purpose of having reliable information to serve as a basis for treating patients. The other fictionalized set was kept for the purpose of reporting successful results in the hope of attracting more research grants.

When this fraud was revealed in 1978, the NCI did not investigate the matter. Dr. Arthur Upton, who was the director of the NCI at the time, explained his position as follows: "the NCI cannot intervene in the internal affairs of institutions, or pass judgment on individuals, in situations in which we are not directly in volved." Dr. Straus was then able to successfully compete for further research monies from the NCI.

Dr. Devito, the current director of the NCI, defended his decision to make this award to Dr. Straus in two ways: First, he argued that Dr. Straus' new grant was not for a clinical study, but rather for more basic research with cell cultures. Second, he pointed out that at the time Dr. Straus had not been proven guilty of any charges of misconduct. When questioned about the case by Senator Hatch, Dr. DeVita replied: "Indeed, Mr. Chairman, at the very least we know this: We know that Dr. Straus was the chairman of a unit where these kinds of infractions took place. He has to bear that kind of responsibility at the very least. At this point in time, he is no longer doing those kinds of things at the Cancer Institute. His current grant is a completely different grant. We, in fact, let it proceed for that particular reason."

It was then brought out that Dr. Straus' new grant had originally contained a proposal for some follow-up clinical work, but that this portion of the experiment had been dropped. Senator Hatch then asked why the alleged fraud should not have been a matter of concern in any event:

The Chairman. We are aware that the peer review committee did cut that portion of the grant out. However, if you had to do it again, would you not notify them immediately about this problem?

Dr. DeVita. Absolutely. There is no question in my own mind, in spite of the Institute's decision not to initiate an investigation-which I again emphasize I did not share in-that any clinical work that involved treating humans would have required an investigation.

The Chairman. Dr. DeVita, do you feel that medical ethics, or the alleged lack of medical ethics, should be taken into consideration when Federal funds are applied for, or are being spent on, scientific research situations?

Dr. DeVita. Yes, indeed, Mr. Chairman. After this investigation is completed-and we expect it to be completed by mid to late summerthen I believe, if these allegations are true, the system will react accordingly and take these things into consideration.

The Chairman. If I understand

you correctly, you would presently feel that medical ethics in this case should have been made a factor before further award of funds was made available to Dr. Straws, and that in the future you will make them a factor?

Dr. DeVita. Absolutely.

The Chairman. Do you think that your conduct in this case will permeate NCI and cause others to be a little more concerned about the use or misuse of Federal funds and the use or misuse of research efforts?

Dr. DeVita. I do not believe so, Mr. Chairman. I think we have received a great deal of inquiry about this particular case, a great deal of criticism. Our level of awareness now is very high. We have discussed it many times. It was a matter of judgment at the time. I cannot go back and redo it.

Under the circumstances, I could construct a different scenario which I would rather have faced this issue. Had we had a debarment regulation in place, then it would have been quite simple for me to initiate some statement at the board meeting and delay consideration of the grant until the investigation was completed.

If the investigation shows that Dr. Straws is guilty of all those charges, all his support in the Institute will be reexamined and reconsidered for continuation.

The investigation then turned to Senator Kennedy, the ranking minority member of the Committee:

Senator Kennedy. Are there no other ways, or procedures which you think can be devised within the Institute to try and deal with either such a potential or real problem, as the falsification of materials.?

Dr. DeVita. Basically when you get dawn to the bottom line, Senator Kennedy, an individual who wants to falsify data entirely in a vacuum can, in fact, falsify data. Our system is based to a large degree on the assumption that the Institution and the individuals have some degree of honesty.

However, in our kind of system, as has happened in both the case of Dr. Long and the case of Straws, people do not work in a vacuum. If you produce a scientific result, the first procedure that is followed is to try to reproduce the scientific result. The failure to reproduce that scientific result brings to mind visions of technical problems in the laboratory, but it also raises the question of whether, in fact, someone has falsified the results.

In Dr. Longs case, the Institution found the falsification. Dr. Long admitted it. It was quickly handled.

I believe there is a built-in mechanism for controlling this sort of thing. It will not ever be perfect, but then again I know of no other system, frankly, at this point in time that will be perfect. Our system of peer review can be referred to the way Winston Churchill referred to democracy: "The worst system invented except for every other system."

Senator Kennedy. The most troubling aspect of this is our inability to really know the answer to the following question, and that is whether this is really the tip of the iceberg or is this the iceberg itself?

We have had hearings in this committee with, as I mentioned, as many as 31 clinical investigators from different settings involved in the fabrication of data. I think we have to find out how serious a problem this is from a national point of view. Do we know that definitively? Can we say how long it will take to find out? I suppose a reasonable question is to ask whether there are ways and means that we can find out.

The FDA has developed a process and procedure to do monitoring, which was a direct result of a series of hearings that we held in this committee.However, I think the question on peoples' minds, with the kinds of allegations and charges we have heard about this morning, is-how many other situations like this are there out across this country?

Dr. DeVita. Senator Kennedy, it is my sincere belief that this is a very small problem for the following reasons. This is one of the points that we debate often with the Food and Drug Administration. The organization that Dr. Straws belonged to, the Eastern Cooperative Oncology Group, is a member of one type of cooperative group, of which we have many; 40,000 patients a year are studied under their protocols.

There is not any single individual who runs a protocol independent of the other individuals. They each have to submit data separately to the operations office of the group. One immediately has the possibility of contrasting

the data from one institution against the other. There is no way one institution can know what another has submitted. Data far out of line with the existing information from other institutions is very likely to be perceived as far out of line and checked at the group meetings by the executive committees.

At the encouragement of the Food and Drug Administration, we now have a monitoring contract that does monitoring on early drug studies, that we can turn to, to check data, should we have that kind of suspicion.

I think it is rare.

Senator Kennedy. The matter that concerns me, with all due respect, is I do not know whether you can say that it really is rare. We had the head of the Food and Drug Administration sitting in that very same seat. When we were looking at research in the FDA, he said that they thought it was rare, until investigators went on out, did a review of a series of their contracts, and found out that it was not so rare.

The question is, do we have to rely upon the whistleblowers to raise these kinds of questions with the research? Can there not be some kind of a system that can be established within the NIH, let alone within the Institute?

Dr. DeVita. I believe that we do have to depend on the whistleblowers. I, too, am glad that there are people at places like Boston University who find data that does not fit with what it is supposed to be and call it to our attention. I do not know that we will ever be able to do without them entirely.

Senators Hatch and Metzenbaum then took Dr. DeVita to task for defending the Institute's decision to make a new award to Dr. Straws on the grounds that he should be regarded as innocent until proven guilty.

The Chairman. Doctor, it seems to me you are a little too blase about some of these important things . . .

I mean you are not running some hale kiddie game here. You are running a $1 billion a year, very sophisticated, very important, and, hopefully, very successful project. It worries me that you do not understand the difference between how important it is to manage and how important it is to reach the final results.

Dr. DeVita. Mr. Chairman, I do not disagree with you one bit. I do not feel blase about these things. These are very important issues.On the other hand, we cannot act until our investigation is complete. The investigation is outside of the Cancer Institute so the Institute itself will not be the main judge. I am waiting far those results. I think we will act expeditiously when we have the is in place. However, I agree with you.

Senator Metzenbaum. Would you yield for a minute?

The Chairman. Senator Metzenbaum?

Senator Metzenbaum. I do not quite follow the point that you cannot act until your investigation is completed.

The Chairman. I do not, either.

Senator Metzenhaum. I was thinking about this in relation to private business. Do you think somebody in private business would tolerate a situation and give a new contract for $900,000 to somebody where there is such evidence?

You are not in the position of being a jury. You are not running a courtroom. You are the Director of the National Cancer Institute. I do not know why you have to wait for a year or a year and a half for an investigation to be completed.

As I gather from what you said, after this investigation is completed, there will then be a committee appointed and there will be a further consideration of matters. Is that right?

Dr. DeVita. I believe that will be the procedure, yes.

Senator Metzenbaum. I can only say to you, Dr. DeVita, that I have a lot of respect for you as a professional, but, as an administrative leader, I share the chairman's concern about being blase.

There are times when it takes some dynamics. It seems tome that in this situation the dynamics are totally absent. What you have permitted to occur is to make a distinction between thefirst contract that involved direct patient treatments and the second contract that involved laboratory work only and to say they are different.

That brings to mind that old saying of if you fool me once, you are a fool; if you fool me twice, I am a fool. I think you are putting yourself in a position to be foaled twice with $900,000 of the

Federal Government's money . .

Somewhere in your mind, you came up with the conclusion that people were entitled to grants unless they were proven guilty. You used the term guilty. It is not a term I would even use. My question to you is how many grants are pending at your shop where there are similar kinds of problems that the Boston Globe has exposed in this instance and that Senator Hatch has referred to in a detailed recitation? What is the responsibility, of the Director of the National Cancer Institute? Do you play a certain kind of judge and jury role, or do you have a responsibility that a private business person would have to move in and say we are not going to do business with this man because there is too much of a grey area, there are too many charges that have been made, and there are too many problems that are existing?

What bothers me is you. Dr. Straws does not bother me as much as you do because you knew of these facts, you learned about them, you continued on down the same road, and you are sitting here today saying we are waiting for the investigation to be completed, and, when that is done, we are going to appoint a committee.

That is not what private business persons would do if they were spending their own money. They would say they were not going to spend any more money until they find out. It is their job to prove that all these allegations are totally false.

Dr. DeVita. I do not know the answer to whether I can, in fact, move in and stop that grant at this point in time. I do not believe it is possible. I am not sure it is correct far me to do that. It is not a matter of my trying to protect an individual. If the individual is guilty, I believe the behavior is absolutely reprehensible and I could not condemn it more.

Senator Metzenbaum. However, do you have to find somebody guilty until you say you are not going to give them another grant? That is my question. That is the issue.

Dr. DeVita. Again, Senator Metzenbaum, at this point, if that happened, we would delay the funding of a grant. We would have an investigation and we would not fund any grant while the investigation was in process.

When Senator Hawkins took her turn, she pursued a different line of questioning. Rather than attacking Dr. DeVita for acting as judge and jury, she turned to the other argument he brought forth in his defense. In essence, Senator Hawkins did not see the presence or absence of clinical studies as relevant to the question as to whether or not Dr. Straws was entitled to federal research dollars.

Senator Hawkins. We are talking to scientists about scientific data. It is either true or false. Yet I am sure that if we have the court reporter read back your answer to one of the first questions that we have all referred to-Senator Kennedy, Senator Metzenbaum, Senator Hatch, and myself-I wrote down in disbelief that you said you felt it was all right to give Dr. Straus the second award-almost $1 million. I know that is not much to you when you are dealing with hundreds of millions. But, I think as you stated, although he was alleged to be dishonest in clinical work, he would not be dishonest in basic research.

Are you telling me that scientists have situation ethics?

Dr. DeVita. No, Senator Hawkins, I am not.

Senator Hawkins. You are either honest or you are dishonest.

Dr. DeVita. I think the issue at hand was patient safety in the sense that we would not, even in the case where the allegations against an investigator had not been proven, allow any clinical therapeutic research to proceed because that matter would require an investigation.

Senator Hawkins. Let us follow that further. You now have him in a laboratory somewhere, doing experiments, and the deductions, data, and knowledge which is gained from those experiments may be translated into recommended therapy to human beings, as I understand it.

If you look at the information that has been given you on some of the cases we all have seen and that Senator Hatch referred to, we are talking about one patient whose white blood cell counts were changed. That is dishonest. We are talking about a human being and his treatment.

Is it logical, or, in your mind, is it completely different in a test tube environment with just test tubes? Is he not going to change his results in the test tube environment as he allegedly did in his clinical work with human beings?

Dr. DeVita. That is certainly possible, Senator Hawkins.

Senator Hawkins. You are saying those are allegations and, until it was proven, you felt that he was innocent, and that I am safe from him, if you put him in a laboratory, close the door, and let him conduct $919,000 worth of experiments, the results of which may be translated into applications to this same patient.

"HHS Regulates Sponsored Research"

Robert F. Ladenson, Editor, CSEP, Illinois Institute of Technology

On November 10, 1980 the Department of Health and Human Services (HI-IS) introduced new regulations for rendering persons guilty of fraud or other abuse who are responsible for federal funds ineligible to receive financial assistance from HHS. The regulations provide for two basic sanctions, debarment and suspension. 'Debarment' refers to exclusion from eligibility for financial assistance awarded or administered by HHS for a specified period of time. Suspension involves an immediate exclusion from eligibility for financial assistance without a prior hearing pending completion of debarment or other proceedings. These sanctions apply not only to individuals but also to institutions. With regard to the latter, the regulations explicitly call for imputing individual conduct to associated institutions where the conduct in question took place within the scope of its authority and under conditions such that its responsible officials knew or should have known about it. In addition, where an institution is the primary offender, any individual who knowingly participated in the debarable conduct may also be debarred.

The regulations specify seven causes for debarment: (1) conviction for any criminal offense committed as an incident to obtaining or attempting to obtain a public or private contract or any form of financial assistance; (2) conviction under the Organized Crime Act of 1970, or conviction far such crimes as embezzlement, forgery, bribery, etc., which indicate "a lack of business integrity"; (3) conviction under the Federal Antitrust Statutes arising out of the submission of bids, applications or proposals; (4) serious violations of the applicable statutes, regulations, or other terms of a previous award of financial assistance; (5) a record of serious unsatisfactory performance or failure to perform under one or more prior awards of financial assistance; (6) debarment from government contracting or financial assistance by a government agency (7) any other cause significantly affecting responsibility as a recipient under a federal program.

The decision to initiate proceedings for debarment lies entirely with the Secretary of Health and Human Services as do decisions concerning the length of the debarment period. In this regard, however, the period must be for a definite period of time commensurate with the seriousness of the offense. In addition, the regulations explicitly allow the Secretary to take into account the degree of seriousness of the offense as a mitigating factor both in deciding whether or not to debar and in setting the period of debarment.

As for the procedure that HHS must follow when seeking to debar an individual or institution, such action is initiated by serving the party in question with written notice from the Secretary. This notice states that debarment is being considered, sets forth the reasons for the proposed debarment and its proposed length of time, and indicates that the party will have an opportunity for sharing if requested. The party has thirty days to make such a request. If none is received within this period then the Secretary proceeds to make a final determination and notifies the party to that effect.

Parties that request a hearing have a right to counsel. A hearing officer who is an employee of HHS but not previously involved in the matter at issue presides over the hearing. HHS is represented by its general counsel or a designee. After completion of the hearing the hearing officer makes a written determination on the evidence presented. He or she then transmits this to the Secretary of HHS and the parties. The parties have a right of appeal to the Secretary, and the Secretary may also decide on his or her own motion, or at the request of the general counsel, to review the findings of the hearing officer. If the Secretary reviews the matter then he or she provides written notice to all parties of the determination. When an individual or institution is debarred HIS publishes a notice in the Federal Register containing

the names of the parties debarred, the authority under which the action was taken, a brief explanation of the reasons for doing so, and the extent of restrictions imposed including effective dates.

The procedures for suspension resemble those far debarment with the following exceptions. Suspension is only undertaken when compelling reasons dictate that the interests of the United States would be jeopardized by waiting for completion of debarment proceedings. Further more, except where suspension is based on criminal indictment, debarment proceedings must be initiated within six months after notice of suspension. If not, then the suspension is automatically terminated. When debarment proceedings are initiated within the six month period, the suspension continues in effect pending the completion of debarment proceedings or for a period of no more than twelve months after the date of the notice of the proposed debarment, whichever occurs first. When suspension is based upon criminal indictment, it may continue until completion of the criminal proceedings or for eighteen months, whichever first occurs.

"Congressmen Investigate Scientific Fraud"

Hearings on the falsification of biomedical research data were conducted early this spring by Congressman Albert Gore, Jr., Chairman of the Subcommittee on investigations and Oversight of the House Committee on Science and Technology. The hearings lasted two days, March 31 and April 1, during which testimony regarding the seriousness of the problem was taken from a variety of sources, from leaders of the scientific community to persons personally involved in cases of fraud.

The first two witnesses, Dr. Donald S. Frederickson, Director of the National Institutes of Health, and Dr. Philip Handler, President of the National Academy of Sciences, argued that protective measures against fraud are inherent in the very nature of science. Excerpts from their prepared statements follow:

Dr. Frederickson:

"Our subject today is not the history of medicine or science, yet reference to the past is useful for perspective. From the beginning certain patterns inherent in science and scientists werevisible and have persisted: intense competition for priority, a high premium on originality, the insistence that discoveries be greeted skeptically and accepted only after intensive examination, repetition and revalidation of the proofs. The rational, presumably value-free, system of judgment distinguishes the natural sciences from most other ways of deriving knowledge.

The "internal ethic" of science, the systemization of this rationalism, was established early. Occasional violations also did not wait long to begin. By "violations" I do not mean here, Mr. Chairman, the errors in observation or experimental design, or false deductions that occur frequently and are corrected by the very system we describe. These conflicts, which to outsiders may seem often to be arising in science, largely consist of this necessary part of the normal process, the shakedown of findings and conclusions until only the truth is left.

Instead, we are speaking today of violations of the scientific ethic. Among the lesser ones are false claims of priority or plagiarism. One has to read only a little of the sociological studies of science, for example, by scholars like Robert Merton, to realize that conflicts and quarrels over priority-even charges of plagiarism-have occurred over and over in science for hundreds of years. Such quarrels have even involved some of the greatest names in early science, like Galileo, Newton, and Descartes.

Mostly I presume, however, that we are here concerned with that most serious abuse, the fraudulent construction of experimental data. Cases of downright fraud in science have always been rare. Detection of some examples has taken many years, and revelation of old irregularities may be further expected. To give one startling example, modern statistical analyses have made it highly likely that some figures in Gregor Mendel's classic studies, an early window to modern genetics, could not possibly have been generated by the experiments. Whether these were intentional or unconscious errors, and whether they were Mendel's or his gardener's will never be known. Such questions and ambiguities surround most instances of scientific fraud. And some are never answerable.

I do not know. Mr. Chairman,

whether scientific fraud, in less spectacular forms, occurs more frequently today than it has in the long history of science. There has been an exponential increase in the number of scientists practicing in the past 30 years over those working previously. The probability of some increase in abuses is therefore high. The likelihood of their being detected is also greater because of an increased density of peers and the development of ever more sophisticated techniques. The strength of the scientific process, however, and the dedication and vigilance of the institutions in which the standards of that process are maintained, do not appear to me to be weakened. Neither, in my opinion, is the public's huge investment in science endangered in any way. Indeed, the current production of useful new knowledge is nothing short of spectacular, and testifies to a vigorous state of health in the life sciences . . .

The NIH cannot guarantee the behavior of scientists or certify the quality of their work through independent analyses, fraud squads, or special statutes. Fortunately, none is necessary, for the natural sciences contain ultimate correctives for any debasement of the knowledge dorived from research. Science is cumulative. It is like a building that is never finished. Any serious flaw in the foundation eventually will be revealed by the weight of the structure above it. If the extension of a wing shows faults in previous construction, the faults are corrected and the design changed. The rational nature of the scientific process makes this feasible and inevitable . . .

I do not believe there is an increase in fraud or other abuses of the scientific method in this work, and I know of no statistical evidence to confirm or deny this opinion. A second opinion is that the system contains safeguards which detect fraudulent data. And a third is that fraud in science carries severe personal penalties for the erring scientist-punishments which are necessarily administered mainly by the scientific community itself-and it is this feature of the system which is the ultimate deterrent."

Dr. Handler:

"The matter of falsification of data, I contend, need not be a matter of general societal concern. It's rather a relatively small matter which is generated in and is normally effectively managed by that smaller segment of the larger society which is the scientific community. This occurs in a system that operates in a highly effective democratic, self-correcting made-the very "peer review system" .

The well publicized instances of apparent falsification of research data [are] an aberration that is difficult for the rest of us to understand. For those scientists for whom the very doing of science is not a sufficiency, of itself, as its own reward, all else derives from the esteem of his peers. Such esteem is enormously gratifying and, moreover, it is from that very esteem that all other forms of reward-promotion, income, position, status-necessarily flow. If the subject of the research and the doliberately contrived data are trivial, the act may go overlooked for some time. But in that case the rewards will also be negligible; hence there must be little temptation for so doing. How frequently such may occur is beyond my knowing. On the other hand, if motivation is to be found in an intense desire for recognition and esteem, only falsification of data relevant to some question regarded as of major significance by the relevant scientific community will serve the purpose. But in that case, glory must be short-lived, indeed. Then, the matter will very soon be found out in other laboratories which will either repeat the contrived experiment or find that the reported results are incompatible with subsequent developments.

It is surely true that the circumstances of our times have generated pressures which, one might think, might occasion an overall increase in the frequency of such undesirable occurrences. The reward system, the "publish or perish syndrome," the sensed, indeed quite real need to present evidence of scientific progress in seeking continuing financial support of one's research, the knowledge of a decline in the availability of research funds,-all of these could be imagined to so weigh upon the mind of a troubled individual as to lead him or her to succumb-and then engage in frank falsification of data. But as we have seen, in the hot cauldron of the operation of peer review in the scientific world, such data cannot long survive undetected. And it is the culprit, not society, that is injured on the rare occasion in which this occurs."

Dr. Felig:

Dr. Phillip Felig of the Yale School of Medicine was among the others who offered testimony during the first day of hearings. In 1980 Dr. Felig had co-signed a

research paper with his junior colleague, Dr. Vijay Soman, in which the latter, without the knowledge of Dr. Felig, had invented some of the data.

In answer to the Chairman's question as to whether competition for shrinking research funds is altering the research environment, and whether that competition is influencing data interpretation, Dr. Felig had the following to say: ". . . it is my belief that it is not the system of academic advancement which inherently influences the use or interpretation of research data. Where such misuse or misinterpretation does occur, it is the unfortunate reaction on the part of some individual to that system."

During the course of Congressman Gore's interrogation Dr. Felig revealed that after he had discovered the fraud, he sent letters of retraction to two journals. These journals, Nature and the Journal of Clinical Investigation, refused to publish the retractions. The latter did not publish correspondence and the former needed definitive proof of fraud. In response to this situation, Dr. Felig expressed the opinion that "a journal which previously published an article has an obligation to publish a subsequent communication if there is evidence or reason to doubt the reliability of the original work."

Dr. Fetig then considered a proposal for detecting and preventing fraud. "To prevent such situations might require all scientists to submit their data notebooks together with grant requests or manuscripts for publication. Such a system, I believe, would be counterproductive because it would be extremely cumbersome, and foster an environment of mistrust . . .

But more importantly, Mr. Chairman, what we have to realize is that if we require such submission it is possible that there would be those individuals who would falsify such sources of data.

In other wards, how could we then be sure ultimately? It has to be based on trust, and the question that I don't have the answer far and which obviously you are addressing is, to what extent do we build in safeguards so as to make sure that that trust is not being violated to any substantial or significant or counterproductive way. I don't know the answer.

Mr. Capron:

On the second day of hearings, Mr. Alexander M. Capron, Executive Director of the President's Commission for the Study of Ethical Problems in Medicine and Biomedical and Behavioral Research, reparted on the results of his investigation of scientific fraud. Among other things he said, Mr. Capron criticized the new Department of Health and Human Services deparment regulations for failing to provide for cases involving falsification of data.

Commenting on a memorandum regarding this problem written by Dr. Frederickson to the Secretary of Health, Education and Welfare, Mr. Capron said these new "regulations were designed to preclude persons guilty of fiscal mismanagement or fraud from receiving further HHS grants or contracts. As written, the regulations do not apply well to the kind of scientific fraud or abuse we are discussing here.

Moreover, although the HHS department rules specifically provide debarment (from NIH grants and contracts) for individuals debarred by other HHS components far irregularities in conducting an activity supported by departmental funds, debarment by the FDA would not appear to trigger further debarment under this provision.

Speaking for myself, it seems dubious to erect on paper a regulatory structure that has so little effective substance. If the protection against fraud and abuse of subjects promised by the regulations is important, the absence of actual mechanisms is worrisome; if the enforcement of the rules as written would be too burdensome, then one would be wary of the false assurances provided by the apparent reality of the written rules."

Dr. Raub:

The next person to make a statement, Dr. William H. Raub of the National Institutes of Health, also discussed the issues raised by Dr. Frederickson's memo. Like Drs. Frederickson and Handler on the previous day, he refused to see fraud in science as presenting a serious problem.

Today's hearing on fraud in science is timely, in view of the recent press reports about possible deceptive research practices. We do not know with certainty if this represents an increased incidence or simply a higher rate of public interest, although it is my strong belief that the latter is the case. I

find little reason to be concerned that we are faced with a burgeoning epidemic of fraudulent research practices by scientists.

In fiscal and business matters it has long been recognized that awarding agencies have a responsibility to protect the financial interests of the public, and that actual or potential recipients of Federal funds are entitled to procedural safeguards and appeals. Present procedures are designed primarily to handle violations of business manage ment policies (or to adjudicate varying interpretations of administrative requirements), and they serve those purposes well.

Fraudulent research practices present a different set of problems. In the first place, the process by which such violations are discovered is quite different. It has been NIH's experience that derogatory information is typically brought to light not by an agency administrator or auditor, but by an institutional colleague, a member of the public, or through the validation processes built into scientific publications. And while the scientific method is subject to a general body of ethics (as I believe was described by several panelists yesterday), accepted medical practice, consensus as to the real or apparent existence and seriousness of specific violations may not be easily attained.

One may legitimately ask what, if any, role should allegations of misconduct play in consideration of grant applications and contract proposals, and how should that information be introduced into the review process? Traditionally, the peer review process has included an evaluation of the qualifications and past performance of the applicant investigator as a key factor in the assessment of scientific merit. Implicit in (and essential to) this review, however, is a measure of objectivity and consensus about the facts concerning alleged misconduct, as well as the presumption of innocence until guilt is established. In cases of suspected scientific fraud, it is very difficult for reviewers to reconcile allegations about inappropriate scientific practices with a principal investigator's assertion of innocence and traditional measures of his other performance as an investigator, especially when these measures indicate high quality performance and excellent productivity. And the advisory councils and boards, while technically responsible for advising on the policy aspects of particular awards, are in no better position than initial review groups to look behind the allegations.

We believe that the responsibility for dealing with such issues rests squarely on the agency officials. We also recognize that this causes considerable discomfort to all concerned. In general, scientistsincluding those who manage our research programs-are not at home in the legal/regulatory milieu. On the business management side, complaints and violations are rooted, for the most part, in the chronically imperfect fit between legal and accounting requirements on the one hand and the academic research environment on the other. Despite continuing and often vociferous debate on specifics, the research community never has contested the principle that the agencies dispensing the funds have the right and responsiblity to protect the public interest by exacting certain requirements affecting their expenditures and accounting. Where the substance of science is involved, however, a more strongly held bias against administrative intervention comes into play.

Commenting on recent procedural changes in the awarding of grants, Dr. Raub went on to say: "Debarment procedures are appropriate for cases in which proof of wrongdoing exists. For cases in which such charges are yet unproved. NIH has long recognized that some sort of tracking system is desirable. In general, NIH science administrators have felt that decisions that might adversely affect the application should not be made too early in the review process, and the derogatory information should be handled on a "need to know" basis."

"The Misuse of Psychological Knowledge"

J. G. Morawski, Department of Psychology, Wesleyan University

Psychology is a bifurcated profession whose two domains, typically called the "clinical" and the "experimental" or "scientific," have developed essentially independent codes for ethical conduct. Clinical psychologists have numerous regulations and ethical standards for client treatment and health-care systems. Experimental psychologists, however, primarily have established guidelines for the

conduct of research with human participants or animals[1].

This asymmetry in ethical standards initially appears reasonable for it is the clinical contingent that dispenses professional services to the public while the experimental half works in laboratories or other scientific research centers. Those engaged principally in research are supposedly governed by the implicit norms of sound scientific practice and do not require extended principles to guide their conduct.

These dual standards of professional conduct in psychology are questionable on several points. Most notable is the fact that many scientific psychologists do become involved in the practical utilization of psychological knowledge. That is, within the field there is a healthy tradition fostering "the application of research findings to the promotion of public welfare[2]." The continuation of this tradition is frequently reaffirmed[3], and perhaps is most visible in psychologists' participation in the area of policymaking.

A recent study of the use of social science in policymaking by high-level government officials disclosed that psychology was used most frequently of all the social sciences[4]. A survey of psychologists interested in social issues reported that those researchers had influenced policy decisions in education, foreign affairs, environmental problems, crime prevention, drug abuse, civil rights, unemployment, the arms race, energy conservation, child care, mental health, and law enforcement, among others[5]. These examples signify professional involvements that are not addressed in or subsumed by the existing ethical standards for psychological research.

What are the consequences of this lacuna in the ethical standards established for experimental psychology? Cyril Burt's dramatic actions constitute a now well-known example of the misuse of psychological knowledge inside and outside the laboratory. Other cases are neither so clearly illicit nor so notorious. Same possibly controversial cases include the endorsement of the lie detector when only twenty studies, many inadequately designed and conducted, tested the instrument's reliability; the promotion of biofeedback techniques when the existing research failed to meet minimal scientific criteria of efficacy; or an announcement of the detrimental effects of day care that was followed sometime later by a public retraction based on additional research showing the absence of any such detrimental effects[6].

These examples cannot be adjudicated according to the aforementioned ethical codes yet they inti mate ways in which psychological knowledge can be misused. However, such potential for misuse must be examined in relation not only to documented codes of conduct but also to those more or less implicit rules or norms governing scientific activities.

Guided by the conventional conception of science, the American Psychological Association has devised standards for such conduct[7]. In this and similar statements, it is held that science offers valid knowledge of external reality given that scientists subscribe to empirical evidence, proper methods of inquiry, and logical consistency. From such premises it is assumed that scientific knowledge can be used to improve rational decisions including those of policymaking. It is assumed further that scientists, by virtue of their ethos of disinterestedness, universalism, and organized skepticism, seek knowledge according to these standards.

Hypothetically at least, scientists' professional engagements outside the laboratory can be assessed by the extent to which the norms of scientific practice are followed. Under these conditions misconduct generally consists of fraudulent actions of individual scientists, by disclosure of research that is known to be inconclusive or otherwise erroneous, or by some form of misuse on the part of non-scientists.

From a broader perspective, then, experimental psychology can be seen to function with two sets of codes of professional ethics: one for the conduct of research and one for other activities requiring scientific expertise. Identification of these extended principles of ethical conduct means that they can be employed to assess cases of potential misuse.

This procedure was adopted to examine three purported cases of misuse in psychology: eugenics, mental testing, and behavior modification in the classroom[8]. Two of the cases are historical thus permitting full analysis of the events: the eugenics movement (1900-1930) included numerous psychologists who conducted relevant research, applied their findings, and generally supported

those policies for selective human breeding of superior traits; and the widespread promotion of mental tests (1915-1930) to augment such policies as immigration restriction and education involved similar actions on the part of psychologists. The third case has more recent origins: the extension of behavioral science to techniques for modifying disruptive or otherwise undesirable classroom behavior.

These cases were examined for evidence that misuse outside the laboratory would entail the violation of commonly-held norms of scientific practice. Four of these norms were identified and applied. Two concern the handling of research (empirical methods and disinterested reporting) and two concern the application of findings (interpretation of research by nonscientists and public surveillance of such applications). For the most part there was no evidence for the following assumptions.

(1) That misuse involves distortion, fabrication, or other incorrect production of data. Although such practices do occur in science, only one instance was detected in the three cases. It involved the incorrect calculation of averages on intelligence tests. Considering the volume of data and the relatively undeveloped means of calculation, this error is hardly unreasonable.

(2) That misuse arises when scientists, motivated by some interest or political commitment, engage in deceitful practices (lying, misinterpreting results, "cooking" data). The three cases contained no blatant evidence of such activities. As the study did not replicate research or re-analyze data, it leaves open the possibility of deceitful practices. Yet there were some clear examples of scientists' conscientious efforts to prevent these occurrences including the cautious discussion of methodological problems and the public retraction of preliminary findings once they were found to be in error.

(3) That misuse results from the selective utilization of science by nonscientists (such as policy makers or politicians). The three cases revealed no obvious distortion of scientific information by nonscientists. If any selectivity occurred, it appears to have originated outside the nonscientists' domain.

(4) That misuse is or can be terminated once it becomes public knowledge. Although all three cases eventually received visible public admonitions for misusing scientific knowledge, there is evidence that the engagements were not abandoned.

For instance, the eugenics movements receded significantly with the public denunciation of its theoretical bases and its rising popularity in Nazi Germany, yet contemporary genetic research has spurred various eugenic proposals-from genetic screening to purported repositories for the sperm of Nobel laureates. The public criticism of mental tests in the twenties apparently did not seriously deter the continued use of tests to sort individuals according to psychological abilities. And while behavior modification has vocal critics, it remains part of many educational policies.

The similarities found among the cases challenge the conventional notions about the misuse of science far it does not always entail the distortion of "facts" by scientists or the selective use of "neutral" information by nonscientists. Nor can it be presumed that public disclosure prevents or arrests misuse.

Instead the study indicates that misuse is more closely related to certain assumptions about the place of science in society-assumptions that extend beyond the conventional norms for scientific method and evidence. The assumptions are often tacit and frame our thinking about the social function of science and scientists. It is the transgression of these broader and socially-based assumptions that is most relevant to misuse as it was detected in the three cases. Four of these assumptions were particularly salient.

(1) That (psychological) science is without cultural or implicit values. One of the most pervasive themes in the three cases is the professed value neutrality of research. Here psychologists proffered two arguments: that psychology did not prescribe the ultimate goals of society and that decisions made with psychological knowledge were more objective and, hence, preferable to decisions based solely on common sense or political beliefs. Their arguments effectively increased receptivity to the utilization of psychology and apparently decreased concerns about the adequacy of research.

The point is not that the values inherent in mental testing, eugenics, and behavior modification led to distortions or wild extrapolations but that they

affected the choice of research problems and variables, the generalization of results to certain policies, and the very images of the ideal individual and society. Thus, eugenics' researchers investigated differences between individuals while ignoring similarities or social psychological concepts such as group cooperation. They preferred the study of intellectual abilities over social, physical, aesthetic, or personality attributes. Mental test researchers concentrated primarily on methods for sorting individuals according to specific intellectual abilities, and they were convinced that such sorting should occur. Tests were designed as expedient means toward order, efficiency and a meritocratic system.

(2) That there usually is consensus about the veracity of psychological knowledge and if there is not, the contending evidence is presented. Many expect that psychologists acting as scientific experts furnish all the known evidence on a particular issue. Where the empirical findings are contradictory, it is assumed that the contending evidence is revealed and evaluated judiciously. In all three cases the evidence given was selective and not representative of the full range of relevant information. The presentation of alternative, contending, or controversial scientific ideas was not a noticeable part of the experts' role.

(3) That the role of psychologists in society, specifically in policy-making, is that of technical or scientific adviser. The cases provided substantial reference to the legitimate function of psychologists as experts or specialists who bring empirical evidence to bear on a particular problem. Yet this narrowly prescribed role of expert varies from the parts actually taken by psychologists. With regard to eugenics, psychologists actively lobbied, contributed to eugenics organizations, and made various other attempts to facilitate eugenics' measures. Mental test researchers volunteered assistance to political causes, economic ventures, and movements to amend educational and mental health policies. In these roles psychologists did not abandon their scientific identity but rather relied upon that persona far credibility.

(4) That citizens have rights to participate in decisions such as those involving the use of scientific knowledge in policy decisions. The democratic right of citizens to participate in government includes direct or indirect participation in policy decisions. The cases indicate that, to some extent, psychologists held that these rights should be waived when science-related issues are confronted and they supported this suggestion with three general claims: that citizens cannot always understand science, that citizens need to be informed about psychology in order that they can accept decisions, not participate in them, and that applying science to policy decisions is for the "good" or "protection" of citizens. These arguments are clearly implied in both eugenics and mental testing research where some psychologists believed that their research findings on the prevalence of low intelligence indicated the inability of many citizens to understand science or make rational decisions about society.

In the three cases, the violation of these tacit assumptions is related to the eventual apprehension of misuse. Although deterred somewhat by scientific advances (such as revised genetic theories) and by contentions of other psychologists, the most pervasive influences on the perception and ultimate demise of the uses of psychological knowledge came from more general changes in social values. More specifically, the uses were contended by social critics and events in society that diminished their desirability. Thus, the sobering aftermath of World War I cast eugenics as a rather unrealistic program for those postwar policies dedicated to urgent economic and social problems. Mental testing lost much of its sensationalism and impetus through social criticism and its apparent value was shadowed by the more urgent remedial policies that were enacted during the depression. Following visible social criticism and court actions, psychologists who continue to promote behavior modification in the classroom make visible efforts to ensure their legal, ethical, and social propriety.

These three examples and others illustrate the inadequacy of ethical principles governing only research and implicitly-held standards of scientific activities. The cases show that the misuse of psychology also may be related to certain broader assumptions about the function of science in society. Relevant in these cases are the role of psychologists, the cultural dimensions of theory, the relation of the knowledge of science to that of social policy, and citizen participation. Detection of misuse may be tied to changes affecting the relevance of scientific ideas to

social conditions or the authority of scientists in social affairs.

These conclusions are disturbing to those who advocate the creation of better regulations or guidelines for the professional conduct of scientists. However, some reassurance can be gathered from the recent discussions about science and scientists in society.

First, an increased awareness and revised conceptions of science challenge the conventional views about scientific norms. It has become apparent that science can be utilized in ways that are discordant with human rights and values, that scientists are not always detached from political life (note the contrasting cases of Lysenko and Oppenheimer), and that fact and value are not dichotomous entities with science generating only facts. This thinking has led to various proposals for alternative models for using science, models which presume greater citizen involvement and sensitivity to the relation between scientific knowledge and policy decisions.

Second, many psychologists themselves are acquiring a similar awareness and consequently have prompted much-needed discourse on the epistemological questions of fact and value and the cultural bases of theory as well as on the responsibilities of psychologists involved in policymaking.

Finally, there is a growing recognition that the expectations placed on both scientists and citizens often have been unrealistic. In the past it has been too readily assumed that medical or scientific experts are able to extend their expertise to encompass complex questions of philosophy, law, and ethics. These reconsiderations and discoveries about the function of science and scientists suggest a need for humility from all sides. Perhaps the best realm for manifesting such humility continues to be education-for scientists, citizens, and policymakers.

Footnotes

1. American Psychological Association, Ethical Principles in the Conduct of Research with Human Participants (Washington: American Psychological Association, 1973); Principles for the, Core and Use of Animals (Washington: American Psychological Association, 1971). A number of government and international codes also concern experimental activities; these include the HHS Institutional Review Hoards' regulations, the Nuremberg code and the Declaration of Helsinki.
2. Bylaws printed fn the American Psychological Association Directory (Washington: American Psychological Association, 1968), p. xii.
3. For instance, see the comments of nine distinguished psychologists in "Psychology and the Future," American Psychologist, 1978, 33, pp. 631-647.
4. N. Caplan, A. Morrison, and R, 1. Stambaugh. The Use of Social Science Knowledge in Policy Decisions at the National Level. (Ann Arbor: The University of Michigan, Institute far Social Research, 1975).
5. P. Popper, P. Ebert-Flatteau, R. E. Love, and N. Marwell, "Survey of SPSSI members' public policy involvements," paper presented at the Annual Meeting of the American Psychological Association, Toronto, August, 1978.
6. T. Pettigrew, "Race, ethics and the social scientist," Hastings Center Report. 1979. 9, pp. 15-18; D. T. Lykken, "Uses and abuses of the polygraph," and E. S. Katkin, C. R. Fitzgerald, and D. Shapiro, "Clinical applications of biofeedback: Current status and future prospects," in H. L. Pick, Jr., H. W. Leibowitz, 1. E. Singer. A. Steinschneider, and H. W. Stevenson, (Eds.), Psychology From Research to Practice (New York. Plenum, 1978).
7. American Psychological Association, Ethical Standards of Psychologists. (Washington, American Psychological Association, 1977).
8. J. G. Morawski, The misuse of psychological knowledge in policy formulation: Three case studies. (Ottawa: Science Council of Canada, in press).

"Announcements"

CONFERENCES: The Popular Culture Association will be meeting in Louisville, Kentucky, April 1418, 1982. The theme of the conference will be "Images of the Professions." Please address inquiries to Professor Jennifer Tebbe, Massachusetts College of Pharmacy and Allied Health Sciences, Division of Liberal Arts, 179 Longwood Avenue, Boston, Massachusetts 02115. (617) 732-2904.

On November 17, 1981, the Rutgers University Committee on Professions and Public Accountability, in cooperation with the Bureau of Educational Research and Development, sponsored a forum entitled: "The Professions and Ethics: Views and

Realities in New Jersey." For further information, contact the BUREAU OF EDUCATIONAL RESEARCH AND DEVELOPMENT. Graduate School of Education, 10 Seminary Place, Rutgers University, New Brunswick, NJ 08903. (201) 932-7280.

The American Society of Law & Medicine, in cooperation with The Institute for the Interprofessional Study of Health Law of the University of Texas, will sponsor a "Human Life Symposium: An Interdisciplinary Approach to the Concept of Person." The symposium will be held March 11-13, 1982 at the Shamrock Hilton in Houston, Texas. Far further information, contact: American Society of Law and Medicine, 765 Commonwealth Avenue, 16th Floor, Boston, Massachusetts 02215. (617J 262-4990.

A conference on the "Legal & Ethical Aspects of Health Care for Children" will be sponsored by the American Society of Law & Medicine, March 31 to April 2, 1982, at the Biltmore Hotel, Los Angeles. California. A variety of professionals will meet in order to discuss problems of common interest. Contact: A. Edward Doudera, J. D., Executive Director, American Society of Law & Medicine. 765 Commonwealth Avenue, 16th Floor, Boston, Massachusetts 02215. (617) 262-4990.

CALL FOR PAPERS: The Society for Business Ethics will hold its Spring meeting, in conjunction with the Western Division Meetings of the American Philosophical Association, in Columbus. Ohio at the Hyatt Regency Hotel on Friday, April 30 at 7:00 p.m. The topic for the session will be: "THE RIGHT TO REGULATE." Papers dealing with any aspect of government regulation are welcome. Please limit reading time to 20 minutes (10-12 pages). Papers should be sent in duplicate to: The Society for Business Ethics, Loyola University of Chicago, 820 North Michigan Avenue, Chicago, IL 60611. Deadline: January 1, 1982.

NEW PUBLICATIONS: The Center far Philosophy and Public Policy, with the University of Maryland School of Law, announces the publication of the first two working papers in a series on legal ethics. David Luban, in "The Adversary System Excuse," reviews and rejects the traditional justifications offered for the adversary system, which often requires lawyers to serve clients in ways that contradict their ordinary moral obligations. The second paper, by Robert Condlin. "The Moral Failure of Clinical Education," challenges the assumption that the legal clinic is a superior method of moral instruction.

These working papers are the first results of a project to create curricular materials for teaching legal ethics, supported in part by grants from the National Endowment for the Humanities and the Maryland Bar Foundation. The working papers are available for $2.00 per copy by writing or calling: Elizabeth Cahoon, Center for Philosophy and Public Policy, University of Maryland, College Park, MD 20742. (301) 454-6573.

Caroline Whitbeck, a philosopher at the Institute for the Medical Humanities in Galveston, Texas, has designed a series of self-instructional units to teach basic ethical concepts to medical students. For more information, or to obtain the units (in return for comments on them), contact Caroline Whitbeck, Institute for the Medical Humanities, University of Texas Medical Branch, Galveston, Texas 77550.

The American Association for the Advancement of Science announces publication of two volumes. The first, Professional Ethics Activities in the Scientific and Engineering Societies, is a report which includes: detailed statistical information from the AAAS Professional Ethics Project Survey; summary of discussions held at a two-day workshop on professional ethics; papers presented at the workshop; detailed information on the ethics activities of thirteen professional activities; and a bibliography. Copies of this 240 page volume are $4.00. The second is a set of reprints of articles, editorials, and letters from Science on Scientific Freedom and Responsibility. Twenty reprints. 3-8 pages each, cost $10. Both can be ordered from: Dept. A, Order Dept., American Association for the Advancement of Science, 1515 Massachusetts Avenue, NW, Washington, D.C. 20036.

The Opinion Research Corporation conducted a survey, in 1980, of corporations and trade associations with codes of ethics, to determine current methods for code development, implementation, and assessment, and to ascertain the nature of anticipated and realized benefits deriving from the code. Copies of their book-length research report, titled "Implementation and Enforcement of Codes of Ethics in Corporations and Associations,"

may be obtained for $17.50 plus postage from Ethics Resource Center, 1730 Rhode Island Avenue, N.W., Washington,D.C. 20036. (202) 223-3411.

"News from the Center"

CALL FOR PROPOSALS, CURRICULUM MATERIALS IN ENGNEERING ETHICS. With the support of a two year grant from the Exxon Education Foundation, IIT's Center for the Study of Ethics in the Professions will produce a series of modules to be used in undergraduate courses in contemporary moral problems, technology and values, and engineering ethics and graduate and continuing education programs far engineers. The grant will enable the Center to prepare, test, evaluate, and distribute these self-contained instructional units, each treating in-depth a problem of engineering ethics. The modules will consist of an analytical essay, case studies, and a bibliography, all relating to a specific ethical issue.

Prospective authors should submit proposals on such topics as confidentiality, and the public welfare, whistle-blowing, informed consent, and pro bono responsibilities. Individuals from the fields of engineering, philosophy, behavioral and management sciences are eligible to apply, either as single or joint authors. A stipend of $1200 will be provided for each module. Proposals on the above or other topics should identify the problem and describe the approach in no more than five double-spaced pages, accompanied by a vita, and submitted no later than January 15 1982. A national advisory panel will evaluate proposals and oversee the entire project.

Submissions and inquiries should be addressed to: Dr. Vivian Weil, Series Editor, CSEP, IIT Center, Chicago, Illinois 60616.

The Center for the Study of Ethics in the Professions at the Illinois Institute of Technology was established in 1976 for the purpose of promoting education and scholarship relating to ethical and policy issues of the professions.