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A few pages of material will follow by FAX later tonight.

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Ethics & Behavior
Feder

QUESTIONS OF SCIENTIFIC RESPONSIBILITY:

THE BALTIMORE CASE

by Serge Lang
1 January 1992

Content

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Introduction

A number of cases of questionable behavior in science have been extensively reported in the media during the last two or three years. What standards are upheld by the scientific community affect the community internally, and also affect its relations with society at large, including Congress.

I wish here to address questions of scientific responsibility, using the Baltimore case as a concrete instance where they came up. The first part containing historical background is necessary to provide readers with documentation so that they can have some factual basis on which to evaluate respective positions and my conclusions that follow - based on further but more succinctly summarized documentation. I have reproduced many quotes because I firmly believe people are entitled to be represented by their own wording. I also do not ask to be trusted. By providing numerous references, I hope that readers who find my documentation insufficient can follow up by looking up these references.

To address questions of scientific responsibility does not necessarily imply that one needs technical competence in a particular field (e.g. biology) to evaluate certain technical matters. The evaluation of scientific responsibilities can legitimately be done without such technical competence. For example, at no point do I take a position whether certain experiments validate a theory or not, or whether the theory is valid or not; but I do take a position about the ways scientific responsibilities were exercised in raising questions or answering questions about those experiments.

The article is in six parts:

Part I gives mostly a historical background of the early phases of the Baltimore case.

Part II presents a discussion of certain scientific responsibilities based on that background, specifically: the responsibility of answering questions about one's work, and the responsibility wherther to submit to authority.

Part III summarizes the two NIH investigations.

Part IV deals with the responsibilities of a Congressional Committee vis a vis science.

Part V goes into an open ended discussion of many issues of responsibility facing scientists, vis a vis themselves and vis a vis society at large, including Congress. The list is long, and readers can look at the section and paragraph headings to get an idea of their content.

Part VI deals with the factor of personal credibility and the shift at the scientific grass roots.

The conclusion is an appeal to the scientific community to reassert the traditional standards of science.

I. HISTORICAL BACKGROUND

§1. How the Baltimore case arose: Margot O'Toole

In April 1986, the prestigious journal *Cell* published a paper cosigned by several authors, among which the three main authors were Thereza Imanishi-Kari, David Baltimore, and David Weaver. In May 1986, Margot O'Toole, a postdoc working in the lab, was reviewing the records for some experiments, and became convinced that the experimental data for that paper had been presented in a misleading fashion. She studied especially 17 pages of these records. As she testified to the Dingell Subcommittee on Oversight and Investigation in Congress later (9 May 1989, p. 181 and also 191):

> After I had studied the 17 pages, I knew that the published paper contained false statements. There was another related experiment reported in the paper to support the same point called into question by the 17 pages. I decided that if the data for this experiment was solid, the finding could still be supported by data and that I could justify doing nothing. Without telling Dr. Imanishi-Kari about my concerns or the reasons for them, I asked her if I could study the data for the other experiment. Dr. Imanishi-Kari told me she could not find the records and she did not know where they could possibly be. I decided to go to a more senior scientist for advice.

Margot O'Toole then contacted Drs. Huber and Wortis of Tufts University, and Dr. Woodland of the University of Massachussetts. Meetings were arranged with them and Imanishi-Kari on May 16 and May 23. Answers to Margot O'Toole's scientific objections were unsatisfactory, and I find it extraordinarily important that the scientific community should be informed of what it was like to raise a scientific challenge. As a result, I shall quote extensively from Margot O'Toole's own words, for which there are no substitutes. She testified:

> I asked to see the original data, meaning the results of the experimental steps that would have had to precede the results she [Imanishi-Kari] was now showing me. Dr. Imanishi-Kari did not reply. After a long silence, Dr. Wortis told me to deal with the data I was being shown. We reviewed the data but they did not answer my objections. Drs. Huber and Wortis agreed with me that the problems were very serious. A large series of experiments, described in the paper and on which the central claim relied, had not even been performed. Dr. Imanishi-Kari said that all the problems were the results of inadvertent errors, and I did not question her explanation. She said she would never forgive me for the way I had handled the matter -- embarrassing her in front of her colleagues and raising questions that could reflect on her integrity. I left the meeting thankful that the unpleasant situation had been resolved, albeit at a high price for me. I was relieved that the paper would be corrected under the agreement I had made with Dr. Wortis. These are the events as they occurred. I should add that both Drs. Huber and Wortis have stated that my account is false.
>
> The next day, Dr. Huber called me and told me that there was no doubt I was right scientifically. However, she and Dr. Wortis were convinced there was no fraudulent intent. She said that a correction would have a devastating effect on Dr. Imanishi-Kari's career. They had therefore decided that no correction would be submitted. I was shocked. I said the paper had to be corrected because others were relying upon it. Dr. Huber replied that there were so many faulty papers in the literature, that one more did not matter. She said that no matter what I did, she and Dr. Wortis would back Dr. Imanishi-Kari and that her "strong advice" to me was to drop the matter. I said that I would have to speak to Dr. Wortis and make sure there was no misunderstanding. Dr. Wortis and I then went through the problems with the paper and he acknowledged them point by point, but restated Dr. Huber's position - no correction would be submitted. I persisted but Dr. Wortis then said that my insistence was calling my motives into question. This was almost more than I could bear from my own thesis advisor with whom I

previously had a good relationship.

As advised by Dr. Flax, the chairman at Tufts, I had kept an MIT official, Dr. Mary Rowe, informed of these developments. I had assured her that I felt the matter could be resolved through the informal process at Tufts. After my conversation with Dr. Wortis I had to admit that I was wrong. Dr. Rowe pressed me to bring formal charges at MIT. I told her that I did not wish to challenge Dr. Imanishi-Kari's explanation that the misstatements in the paper were the result of a series of errors and not due to deliberate fraud. I added that Dr. Imanishi-Kari and I had not been getting along and that I felt that my motives were being unfairly questioned. I did, however, feel a strong professional responsibility that the false statements be corrected. Dr. Rowe assured me that MIT could handle the matter in an ethical way without the formal charge of fraud we both knew could have devastating consequences. I also stated my strong belief that a formal charge of fraud was not warranted by the information available to me at the time.

I discussed at length with Dr. Rowe the professional consequences to me if I did as she recommended. I pointed out that upsetting as the experience of the Tufts review had been, the matter had been kept among friends. I had a non-tenure track appointment at Tufts, and I had an opportunity to apply for grants as a Tufts researcher; and I intended to do this. If I pursued the matter further, I felt certain Dr. Wortis, who was very influential at Tufts, would seek to prevent my return. Dr. Rowe assured me that coming forward was the right thing to do and that she would speak to the Dean and the Chairman and enlist them in making sure that a position would be found for me in an MIT lab. This kind of position was very much less attractive to me, because the Tufts position offered independence and scientific freedom. I had been a post-doctoral fellow for over six years and I felt ready for more independence. However Dr. Rowe pressed, saying that I had a professional obligation to come forward.

Having assured me that MIT could deal with the matter in an equitable way without a formal charge of fraud, Dr. Rowe called Dean Brown and arranged for me to talk to him. I described my concerns to Dean Brown. He said the serious nature of the problems sounded like fraud to him. He told me to charge fraud or drop the matter entirely. I told him that neither of those options were acceptable, but that of the two I would choose the latter. After I left, Dean Brown evidently rethought his position and arranged for Dr. Eisen to call me.

Dr. Eisen invited me to come and discuss my concerns on May 30. When I showed him the records he became very uneasy and said that by merely showing him the records I was charging fraud. I said that Dr. Imanishi-Kari said the discrepancies were errors. I said I was willing to accept this explanation but that I did not agree that the misstatements could be ignored simply because they did not occur with fraudulent intent. Dr. Eisen chided me for having placed him in a difficult position and told me that my concerns would have to be put in writing before he would address them.

I prepared a memo. Dr Eisen gave it to the authors and arranged a meeting with Drs. Baltimore, Imanishi-Kari, Weaver and myself. Dr. Rowe advised me to bring someone to represent my interests to the meeting. I asked Dr. Eisen if this would be all right, but he said no, it would be intrusive on the science. I asked if I could bring a scientist, but Dr. Eisen said this, too, would be intrusive.

At the meeting, Dr. Imanishi-Kari did not present any new relevant information. She immediately conceded that Figure 1 did not accurately represent the specificity of the Bet-1 reagent. Dr. Baltimore asked where the data for the figure came from, and Dr. Imanishi-Kari said that Dr. Reis must have obtained "this result once". Dr. Baltimore replied that "this was not good enough" and added later that he would deal with this matter in private with Dr. Imanishi-Kari. I then went over my concerns about Table 2, the principal support for the central claim of the questionable paper, and I showed a copy of the original data to Dr. Baltimore. He gave them a short examination and said that the claims could not be based upon them. This was precisely my point.

Dr. Imanishi-Kari and Dr. Baltimore acknowledged that some necessary experiments had not been done and they discussed how this error had been made...

Dr. Baltimore acknowledged that the finding I challenged did not have the claimed experimental support. However, he suggested some experiments that Dr. Imanishi-Kari could now do to find out what was really going on. He stated that there were portions of the paper that were sound. I have always agreed that parts of the paper are not false. He then said that as long as parts of the paper were true, he felt no obligation to issue a retraction. I disagreed and he said that I could attempt to submit a correction on my own, but that he would submit a note challenging my corrections...

A day or two later, I called Dr. Eisen and protested his failure to insist that false claims be corrected. I discussed specific scientific issues we had covered at the meeting, but Dr. Eisen said he could not remember them. He said that my continuing pursuance of the matter indicated vindictiveness. I called Dr. Rowe and she indicated that Dr. Eisen had made a verbal report to her and indicated that there were no problems with the paper. I reminded Dr. Rowe that she had said that she would help me to secure another laboratory position. I felt that I needed her assistance to explain why I was suddenly without a job or a recommendation. Dr. Rowe replied that I should have arranged the position before I had handed in my memo and that the time during which she could have helped me had now passed. I asked if I would receive a copy of the report Drs. Baltimore and Eisen had agreed Dr. Eisen would submit. Dr. Rowe said she had told Dr. Eisen that it was better not to submit a report, that reports were usually not filed in cases like this. She said this was in my interest. I presumed she meant that the report, if filed, would be unfavorable to me. She said that the matter was now in the hands of God and she wished me well.

Among other things, the above quotes document the extent to which higher ups were forcing Margot O'Toole and themselves into extreme alternative positions: either deal with a charge of fraud, or there is no necessity to do anything about a scientific challenge. I shall return later to my own objections to alternatives phrased or conceived in this way.

§2. Conflicting versions: The Tufts Ad Hoc Committee, Herman Eisen at MIT, and David Baltimore

I have quoted at length from Margot O'Toole because for several years the establishment press (as we shall see below) represented her position and the issues improperly.

As Margot O'Toole reports, officials at Tufts and MIT investigated her complaint. At Tufts, this investigation was carried out by an Ad Hoc Committee chaired by Henry Wortis, with Brigitte Huber and Robert Woodward as the other members. I quote their conclusions:[1]

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[1] From the Minutes of the Ad Hoc Committee at Tufts, dated 4 June 1986. The document is appended to the Dingell Subcommittee hearings, 9 May 1989, p. 303. These hearings are available from the Government Printing Office, Serial No. 101-64. I give here additional quotes from the testimony given by Wortis to the Dingell Subcommittee on 9 May 1989, p. 255-256. These quotes will further help readers evaluate the reliability of the Tufts investigation and conclusions.

Mr. DINGELL. Well, when were you first aware of the existence of the June subcloning data?

Ms. HUBER. We did look at subclone data in our first meeting with Dr. Woodland - Dr. Wortis and myself were present.

Mr. WORTIS. Let me add something on that because I can remember this very specifically. Again, I don't

OVERALL CONCLUSIONS...
NO EVIDENCE OF FALSIFICATION
NO EVIDENCE OF DELIBERATE MISREPRESENTATION
ALTERNATIVE INTERPRETATIONS OF THE EXISTING DATA CAN BE MADE, BUT THAT IS THE STUFF OF SCIENCE.

Herman Eisen at MIT came to similar conclusions. I quote from a memorandum by Eisen:[2]

> Re: Allegations of misconduct by Thereza Imanishi-Kari in a research study...
>
> The allegations of misrepresentation were brought by Dr. Margot O'Toole...
> Dr. O'Toole cited four issues, three of which challenge the conclusions drawn in the paper on grounds that some assays were not sufficiently sensitive or that they were misinterpreted. The issues raised by these three objections seem to be matters of judgment and could not be described as evidence of misconduct...But one of O'Toole's allegations was disturbing because it raised a serious question about deliberate misrepresentation of data. The allegation concerns a monoclonal antibody termed "BET-1"...
> My conclusion is that O'Toole is correct in claiming that there is an error in the paper; but it is not a flagrant error...The correction would be too minor to rate a letter to the journal; it certainly does not warrant a retraction, especially because the paper contains a substantial body of other data that is clear and impressive.
> The other issues raised by O'Toole, which are largely matters of interpretation and judgement, are best dealt with by allowing the scientific process to take its course. Other laboratories are trying to extend the findings. In this way we will know if the interpretations are right or wrong.

David Baltimore himself presented matters differently when he published an article describing his point of view, and when he wrote[3]:

> At the outset, the substance of the dispute was not unlike others that occur regularly in biology labs. It was simply a disagreement over scientific matters between two scientists [Imanishi Kari and O'Toole]...
> In these reviews [at Tufts and MIT] completed by early summer of 1986, all the issues were scientific. No one had accused anyone of unethical or criminal behavior; O'Toole simply said she thought the conclusions of the paper were not borne out by the data generated in the Imanishi-Kari laboratory. After the second review, I thought that the matter was closed.

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want to go down the road of science, but Dr. O'Toole had raised questions about the existence of the supernatants of the wells shown in Table 2 of some transgene product, and Dr. Imanishi-Kari said yes, but those were wells - those were not yet cloned, and therefore the reason that there is some transgene product is that there are several different cells and one of them is producing transgene and others may not be.

Mr. DINGELL. Does that mean that the data had been generated at that time?

Mr. WORTIS. No, you can't do it then. Let me finish. So, we said yes, but what's important is whether those particular wells have been cloned, and Dr. Imanishi-Kari said yes, I've done those, and we said we would like to see that data, and at that point she began to cry and she said, you didn't believe me? You don't trust me? We said no. We want to see the data and she got out the data and we looked at it then.

Mr. DINGELL. Did she get the data out right there?

Mr. WORTIS. Right there.

Mr. DINGELL. Well, maybe you can tell me when you became first aware of the existence of the June subcloning data. Did that occur at the meeting referred to?

Mr. WORTIS. I don't know which June subcloning data you're talking about.

[2]dated December 30, 1986, reproduced in the Dingell Hearings p. 312

[3]"Baltimore's Travels", *Issues in Science and Technology* (a publication of the National Academy of Sciences), Summer 1989

All three accounts are misleading, and in some ways are incompatible. Of course O'Toole's complaints dealt with scientific matters. However, she made factual assertions, whose evaluation was not a matter of "interpretation", but of determining correctness or incorrectness. As Margot O'Toole stated in her testimony to the Dingell Subcommittee (9 May 1989, p. 200): "My opponents in this dispute have convinced the scientific community that my differences with them involve alternative interpretations of data. This is not the case. I did not challenge the paper because I felt I had a better interpretation. We scientists discuss alternative interpretations every day. All authors are free to present their own interpretations...I challenged the paper because it represented evidence that simply did not exist, period. This is not a complicated concept. It is one thing to believe that something is true. It is another to present experimental evidence in support of the claim. This is the crux of my dispute with the authors."

Furthermore, although Margot O'Toole did not write down charges of "misconduct", "falsification" or "misrepresentation", the factual evidence[4] she was bringing to the attention of various officials at Tufts and MIT immediately brought to their mind such possibilities, as evidenced by the quotes reproduced above, and by the following testimony by Eisen himself, testifying to the Dingell Subcommittee (9 May 1989, p. 290), when he acknowledged:

> In dealing with her charges - Dr. O'Toole's charges of error - I was not unaware of the possibility that she had in mind fraud and was unwilling to say so, and in carrying out my evaluation, this concerned me. This was one of the reasons it took a long time. I couldn't rush through this. I wanted to do a lot of talking to people and thinking about it and thinking about the science,[5] and I was aware quite distinctly of the possibility that it deserved a fair

[4]For example, in a memo addressed to Herman Eisen dated June 6, 1986, Margot O'Toole wrote factually: "The hybridomas of Table 2 were not checked for isotypes other than mu, according to Dr. T. Imanishi-Kari and Dr. M. Reis; the statement on page 250 that the majority of these hybridomas express gamma 2b is based on an analysis of a number of hybridomas from another fusion. These data will be reviewed below." The statement that "hybridomas...were not checked..." is *not* a statement giving rise to a problem of "interpretation", as Eisen asserts; it is an assertion - true or false, but an assertion of fact. On the other hand, the *Cell* paper claimed that the hybridomas were indeed checked.

[5]The question arises how thorough were the investigations at Tufts and MIT - whatever that means. Within minutes after the above statement, in his testimony to the Dingell Subcommittee on that same day (p. 291), we have the following exchange with Dingell:

> Mr. DINGELL. ...Did the inquiry ever review the data, Dr. Eisen?
> Mr. EISEN. I think you really asked me whether the inquiry ever looked at notebooks.
> Mr. DINGELL. Did you look at the notebooks?
> Mr. EISEN. I did not look at notebooks.
> Mr. DINGELL. Did you know at the time that you were inquiring into this matter whether all of the experiments which were purported to have been made were in fact made or not made?
> Mr. EISEN. No, I did not know that, and I couldn't have known that, and in fact the question was never raised. That whole question of experiments not having been done was not raised by Dr. O'Toole or until very recently so far as I know.

Eisen's testimony should be compared with that of the Provost at MIT, John Deutch, testifying the same day (p. 299):

> Mr. DINGELL. Well, Doctor, we have the situation where at first she didn't even raise a question of fraud, and she found it dangerous. All she said is there's error.
> Mr. DEUTCH. Her inquiry was treated in a very serious way because it clearly had attention both with regard to whether some mistakes had been made in the paper which hadn't been acknowledged and, in addition, as has been mentioned several times here, it suggested that there was a possibility of misconduct.

Since Eisen stated that he "did not look at the notebooks", the question arises as to what Deutch means when he consideration without her having to charge it or without having to - or without triggering off the full-scale investigation until some preliminary evidence was found that would suggest it merited such a detailed investigation.

Such possibilities were also on Baltimore's mind when he wrote to Herman Eisen a letter dated September 9, 1986, and marked "confidential" (but the letter is reproduced in the public hearings, 4 May 1989, p. 164). I quote from this letter:

> After much thought about the situation brought on by my collaboration with Thereza Imanishi-Kari, my opinion has gelled around the following analysis.
>
> 1. The evidence that the Bet-1 antibody doesn't do as described in the paper is clear. Thereza's statement to you that she knew it all the time is a remarkable admission of guilt. Neither David Weaver nor I had any idea that there was a problem or an ambiguity with the serum. Why Thereza chose to use the data and to mislead both of us and those who read the paper is beyond me.
>
> 2. Given that the analysis is meaningless, does this change the paper? Not really and certainly not in a fundamental sense...
>
> 3. A retraction would be difficult because David Weaver would be identified as senior author and he really had nothing to do with those data. All authors do have to take responsibility for a manuscript, so all of us are in a sense culpable, but I would hate to see David's integrity questioned for something he accepted in good faith and where his contribution is what makes the paper strong.
>
> ...In summary, I think that a retraction would harm the innocent and raise doubts about quite solid work. I think we should, however, acknowledge to colleagues that the Bet-1 results are not reliable and I, for one, will be skeptical of Thereza's work in the future.

Thus Baltimore himself on 9 September 1986, "after much thought", had on his mind that Imanishi-Kari "chose to use the data and to mislead...". Baltimore stated subsequently at the hearings: "Mr. Dingell, I've gone to great lengths to apologize for that letter, to explain the conditions under which that letter was written...That letter was completely inoperative in its significance within a couple of days of its writing...It was known to me in a day or so that there was nothing wrong with it [the mu analysis] and therefore absolutely no reason to retract it."[6] Compare this explanation with the first sentence of the letter: "After much thought...my opinion has gelled...The evidence...is clear."

In the next section, we shall see that possibilities of fraud or misrepresentations were also brought to the mind of scientific reviewers for the National Institutes of Health (NIH).

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says that "her inquiry was treated in a very serious way". Also take note of Deutch stating: "...as has been mentioned several times here, it suggested that there was a possibility of misconduct" - which gives additional evidence that Baltimore's version in *Issues in Science and Technology* was misleading.

[6]In his testimony to the Dingell Subcommittee (4 May 1991), Baltimore stated: "The problem of her communication caused a serious misunderstanding when, in September, 1986, Dr. Herman Eisen in a chance conversation with Professor Imanishi-Kari, thought he heard her say that the Bet-1 antibody really did not work as described in the *Cell* paper. Dr. Eisen immediately called me with this news and I, unfortunately, instead of checking with Professor Imanishi-Kari, went home and began to stew about it. Without having thought through the significance of my proposals, I sent a letter to Dr. Eisen discussing a course of action...As it turned out, within another day or so Dr. Eisen talked again to Professor Imanishi-Kari and realized that the previous discussion involved a total misunderstanding..."

§3. The "17 pages" and the intervention of Stewart-Feder The issue of publication

We shall now deal with the issue of the 17 pages of notebooks on which Margot O'Toole and subsequently other people's objections were based. I start with an account from Baltimore's article in *Issues in Science and Technology* (p. 49):

> Meanwhile, Charles Maplethorpe, who received his Ph.D. for work done with Dr. Imanishi-Kari but who had left her laboratory before the paper was published and had not been involved in the study, got himself involved. Maplethorpe contacted two scientists at the National Institutes of Health (NIH), Walter Stewart and Ned Feder, who had made reputations for themselves by publishing papers analyzing cases of previously demonstrated fraud in science. They received from O'Toole copies of 17 pages of laboratory notes taken from the Imanishi-Kari laboratory. Those pages, of more than a thousand pages collected during the study, included data from a number of failed experiments. On the basis of the 17 pages, plus conversations with O'Toole and Maplethorpe, Stewart and Feder mounted a challenge of their own.
>
> The Stewart and Feder challenge soon developed into a *cause celèbre* because of the manner in which they conducted it. First they wrote a lengthy manuscript clearly charging that our paper was consciously misleading. The Stewart-Feder manuscript was submitted to a number of journals, all of which rejected it. Frustrated by their inability to publish what journal editors told them was not a scientific article that could be refereed, Stewart and Feder went public. They circulated the manuscript widely to scientists, asking for comment. They also began speaking about their "investigation" on university campuses and at scientific meetings and offered to send a complete file of correspondence to anyone asking for it...[7]

Stewart-Feder, upon receipt of the 17 pages, did write a paper dealing with the possibility that some of the *Cell* paper findings were not borne out by the primary data. At no point did Stewart-Feder make allegations of "misconduct", and they repeatedly emphasized that they did not. They were dealing with factual accuracy and scientific analyses of data. On the other hand, just as it happened with the Wortis Committee and Herman Eisen, NIH administrators chose to use the word "misconduct" to characterize what Stewart-Feder were objecting to. In addition, they were inclined to see the matter as a case to be handled according to certain quasi-legal procedures.[8]

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[7]Baltimore repeated the same version in his testimony to the Dingell Subcommittee, including the following statement (4 May 1989, p. 102):

> Mr. Stewart is a man of significant analytic skills but poor judgement. This is shown well in the draft manuscript he produced analyzing the *Cell* paper. That manuscript is based on 17 pages of selected data from a study that ran perhaps 1000 pages. No one with any experience in science should think that such an analysis could get at the basic truth or falsity of the whole study. I believe that the rest of the Subcommittee staff as well as the Members who are on the Subcommittee, not being versed in the ways of science, have been misled into thinking that this method is appropriate for judging science.

Concerning Baltimore's statement that "no one with any experience in science should think that such an analysis could get at the basic truth or falsity of the whole study": no one had questioned "the whole study". Only parts of the study were questioned. See also the reports of NIH reviewers quoted below.

[8]For instance, in a Memorandum dated October 17, 1986, the NIH Acting Deputy Director for Extramural Research and Training George Galasso wrote to Stewart-Feder that the "subject" was concerned with an "Evaluation of Alleged Misconduct in Science"; and he stated: "Because it appears that an inquiry - and possibly an investigation - is warranted, it is essential that such be carried out according to established NIH policy and practice."

Stewart-Feder had to ask NIH for permission to submit their paper for publication. Permission was at first refused. But every one of the three referees to whom NIH sent the paper for reviewing expressed the thought that if the 17 pages on which the Stewart-Feder paper was based were authentic, then these data raise serious doubts about the validity of some results in the *Cell* paper. One reviewer explicitly stated that the Stewart-Feder paper raised "serious issues of scientific fraud". All three reviewers suggested that Stewart-Feder contact the authors of the *Cell* paper to get their evaluation and explanations. Stewart-Feder followed the reviewers' suggestion. Baltimore wrote them his position in a key letter dated January 21, 1987:[9]

> I have been aware for some time that a discontented post-doctoral fellow previously at MIT has raised questions about some of the data in that paper...
>
> Your notion of doing an "internal audit" of the data is not one I can accept. Such a principle, if established, would tie up the scientific community in continuous wrangles...External reviews of data are only relevant when probable causes of fraud have been established. In this case, a number of respected immunologists not involved in the work examined the situation and did not find probable cause.

Baltimore thus raised a fundamental issue concerning the responsibility of scientists to address criticisms of their work. I shall deal with this issue specifically later. Here, I continue with the account of the Stewart-Feder intervention. They also heard from Henry Wortis in a letter dated March 2, 1967: "It would not be useful to pursue the questions you have about the published results in *Cell* 45:24, 1986...No doubt you are curious about the relationship between the information you have been given and the published material. But there is no social or scientific gain in satisfying your curiosity." Thus although Stewart-Feder were following the recommendations of the reviewers, the principals involved stonewalled attempts to confirm or invalidate the data on which the *Cell* paper was originally based, and on which the Stewart-Feder paper was based.

In light of the fact that Stewart-Feder requested material from the groups at MIT and Tufts who reviewed the work originally, but complained about not receiving such material, Baltimore then wrote

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[9]He repeated this position in his testimony to the Dingell Subcommittee, 4 May 1989, p. 102. He also stated: "The proceedings here today indicate that the Subcommittee wishes to do away with the standard criteria and substitute a whole new standard for judging science. They have chosen a prosecutorial style. The message is to do your science with an eye towards facing prosecution of the style of your science. The order of pages in your notebook will be a primary concern. Never overwrite a date or add a page or the Secret Service will catch you. We could call this the Sovietizing of American science except that the Soviet Union has seen the errors of its past and is moving to a more American style. If the hearing here today represents the Congressional view of how science should be done, then American science is in trouble. Science will become an enterprise based on form, not substance. Young people will be afraid to be audacious for fear that they will be prosecuted for transgressing orthodoxy.

Having said that the appropriate form of judging science is replication and the foundation of further progress, how well has the 1986 *Cell* paper fared in the 3 years since it appeared? Very well! No result of the paper has been proved wrong, a number have been replicated and there has been significant progress building on the foundation of its results. Because this is the heart of the matter in question, in an appendix I show how five papers published since the *Cell* article appeared have supported its conclusions.

Why has a Congressional Committee tried to define this new and pernicious form of scientific verification? The answer, I believe, lies in the presence of one man on the Subcommittee staff, Mr. Walter Stewart. In his paper on the Darsee affair he first developed the notion of an "internal audit" of scientific records and in his first letter to me, in December 1986, he said that his goal was to use this paper as a test case of his methods. In May of 1989 we are seeing just what he meant and it makes me proud that when I first saw his request, I resisted it as a destructive interference in the scientific process." [*The testimony continues as in footnote 7.*]...

on 17 March 1987 to Edward Rall (NIH Deputy Director for Intramural Research), giving ground on having a "further review of the data". However, he formulated conditions for that review as follows.

> From Stewart and Feder's "manuscript", it is clear that only someone familiar with immunologic procedures and concepts can provide a review. Therefore I suggest that you appoint a couple of immunologists to do an examination of Stewart and Feder's charges. If you decide that this is the right action, please tell Stewart and Feder that we have suggested this review. For the review to be meaningful, they must agree to abide by whatever decisions are reached. This means that they must promise to cease all discussion of this issue and to send an apology to all concerned if the review group finds that the norms of scientific research were not transgressed. The apology is absolutely necessary to counter the publicity that the issue has already received. The reputations of young scientists (never mind an older one) have been impugned by Stewart and Feder's activities and this wrong must be righted by them.
>
> I should emphasize that by this request for a review, I am in no way suggesting that any of Stewart and Feder's allegations have a basis in fact. In reality, to my knowledge, there has been no official charge of fraudulent behavior made. This request to you is only made in the interest of clearing the air of what I consider false allegations made in a privately, but widely circulated form, so as to remove any tarnish from the reputation of the involved scientists.

It is no surprise that Stewart and Feder refused to accept Baltimore's proposal.

Rall still decided not to allow Stewart-Feder to submit their paper for publication. As a result, they wrote to Rall once more on 9 April 1987. They recounted the past events, and they first asserted that "the arguments for approval of our manuscript are clear and convincing." They summarized these arguments as follows.

(a) They pointed out that they followed the referees' suggestions to contact the authors of the Cell paper and to ask these authors for their comments.
(b) They pointed out that none of the referees had questioned the accuracy of their analysis.
(c) They pointed out that none of the letters they had received from Baltimore or other coauthors questioned the authenticity of the data in the 17 pages on which they based their analysis. For this and other reasons stemming from their correspondence, they concluded that the data was authentic.

As for the deeper problems of scientific responsibility, they asserted among other things:

> - The arguments against prior restraint on publication have a sound basis, particularly as applied to publication of comments on the accuracy of scientific papers. NIH should not embark on the unproductive and dangerous course of censorship of scientific publications...
> - Free and open communication on issues of scientific importance has a long tradition in the scientific community in general and at NIH in particular...
> - It is NIH policy to "Restrict writing and speaking by its employees only to the extent *required* by law or regulation or to assure compliance with established NIH and DHHS policy" [emphasis added] (NIH Manual, chapter 1184, 3/8/81, "Dissemination of Scientific and Professional Information by NIH Employees,", page 2, part E).
>
> We are not aware of any law, regulation, or established policy that requires NIH to prevent our paper from being submitted in the normal way to the scientific public.
>
> Accordingly, prompt approval of our manuscript is clearly required by chapter 1184...

Stewart-Feder also addressed a fundamental question of scientific responsibility for themselves:

> ...Your directive poses a dilemma.
>
> As we have noted, we believe it is generally accepted by almost all scientists that a scientist such as Dr. O'Toole or ourselves with unique knowledge showing that a published paper is probably wrong has an affirmative obligation to ensure that the knowledge is made public...

> The problem with your recent directive is that it requires us to violate accepted standards of conduct. If we abide by your decision, we are in a position of covering up poor science, a position that is arguably similar to that of Dr. Baltimore, with the difference that his actions are active and ours would be passive. Either way, the damage to science and the public welfare are the same.
>
> Naturally we will have to consider as a matter of professional conscience whether in light of our unique knowledge and our belief that damage is occurring, we are justified in continuing to remain silent. In reaching this difficult decision we shall seek the advice of senior scientists known for their accomplishments in research and for their integrity. We request that you furnish us with any reasons which in your opinion would justify us in agreeing to the course of conduct you propose.

Rall's reply was to resubmit the Stewart-Feder manuscript to reviewers once again. One of them wrote clearly supporting publication:

> *Reviewer R.* I have reviewed the set of papers submitted by Drs. Stewart and Feder. I cannot interpret the data xeroxed from the original notebooks. One possible solution is to allow publication of their manuscript with a disclaimer that relieves NIH from formally approving the contents of the manuscript since it is not a traditional research document.

Four others recommended against allowing Stewart-Feder to submit their paper for publication, for various reasons, as follows:

> *Reviewer X.* ...Regardless of the possible validity of the arguments raised by Feder and Stewart, I do not think that the scientific literature is an appropriate forum for the resolution of such issues. The entire matter should be referred to an appropriate impartial committee at the institution in question (it is unclear to me whether or not this is what has already been done at MIT). The authors and the scientific community should then be willing to accept the judgment of such a committee. If a further publication clarifying the issue is deemed necessary, it should come from the authors and not from a third party...

> *Reviewer Y.* ...I do not believe that their paper should be published nor do I believe that this investigation should continue. I do not deny the possible validity of the raw data included with their paper, however, as you know pages from notebooks taken out of context can be quite misleading. At any rate I do not see what useful purpose can be served by extending this investigation.

> *Reviewer Z.* ...The manuscript by Stewart and Feder makes several valid points concerning the manuscript by Weaver et al. that was published in the journal Cell... [Follow several such technical points.]
>
> Having said that certain valid points are made in the Stewart and Feder manuscript, I still cannot recommend its publication. It must be asked what positive purpose would be served by exposition of these possible lapses in a manuscript from the laboratory of a famous scientist.[10] What actions would follow publication of such a manuscript?...
>
> It is my opinion that the possibility is remote that anything more than some misguided enthusiasm on the part of junior staff and sloppy editorial procedures on the part of senior staff is involved here. It is not worth the time or the expense to expend more effort on cataloging the flaws in the Weaver et al. manuscript. It would seem that better examples of fraud than those given in the present manuscript could be found by those interested by such pursuits.

[10]Here we find documented a recommendation to do nothing, on the ground that it would serve no "positive purpose" to expose "lapses in a manuscript from the laboratory of a famous scientist". Is that what fame, (including a Nobel Prize) is for? To protect a scientist from being accountable to the scientific community?

Reviewer W. This is in response to your request for comment on the subject manuscript.

1. Despite oral claims by Feder and Stewart to the contrary, the subject manuscript alleges serious misconduct by Weaver et al. Assessment of the validity of the allegations is complicated by (a) the uncertain significance of 17 pages of Weaver's laboratory records, of which Feder and Stewart have a copy and (b) Feder's and Stewart's failure to gain the cooperation of Weaver et al regarding the issues raised in the subject manuscript. On the other hand, the specificity of the allegations and the detailed supporting the rationale suggest that Feder's and Stewart's adverse findings are readily testable.

2. In view of the seriousness and specificity of the allegations and the fact that the subject manuscript, in various drafts, already has been shared with many scientists outside the NIH, action by the Office of Extramural Research (OER) is indicated to resolve the matter, if possible, in an equitable and publically [sic] visible way. At the least, this means some fact finding to establish the relevance of the 17 pages of laboratory records and the view of the responsible scientists and institutional officials. At most, OER may need to conduct a formal investigation.

3. While the OER inquiry/investigation is in progress, no further action seems warranted regarding the request by Feder and Stewart to submit the subject manuscript for possible publication. Once the OER process has been completed and its findings made a matter of public record, an appropriately revised or updated version of the subject manuscript might merit your clearance for submission to a journal.

The scientific community will have to evaluate these reviewers' reports, which constitute documentation and primary sources (as the historians say) on how the scientific community reacts.[11] I regard such primary sources as exceedingly important, and that is the reason I have quoted so extensively from them.

These reviewers' reports also document the extent to which the scientific data were immediately perceived by some scientists as implying more than error, including possibly misconduct or fraud.

The NIH continued to refuse permission for Stewart-Feder to submit their article for publication. Then the ACLU intervened. In a letter dated 14 July 1987, the law offices of Morrison & Forrester representing the ACLU wrote to Robert Lanman, Legal Advisor to the NIH, "that NIH has no reasonable basis for denying publication, particularly as it is NIH's professed policy to encourage and assist its employees in disseminating information about their scientific research and professional activities. NIH Manual, Chapter 1184(A) and (E), Dissemination of Scientific and Professional Information by NIH Employees (3/18/81). In fact, we believe that NIH's continued refusal to allow Dr. Feder and Mr. Stewart to seek publication of their article constitutes an unlawful prior restraint..."

In a Memorandum dated 17 July 1987, the NIH Deputy Director for Intramural Research J. E. Rall then wrote to Stewart and Feder in a way compatible with the recommendation of Reviewer R and the ACLU request and gave permission for Stewart-Feder to submit their article for publication.

At that point, Stewart-Feder submitted their paper successively to *Cell*, *Science* and *Nature*. All three scientific journals rejected the paper.

Rejection of the Stewart-Feder paper by *Cell*. The rejection by *Cell* is noteworthy because *Cell* editor Benjamin Lewin iterated Baltimore's way of answering scientific challenges in his letter to Stewart dated October 19, 1987. First he asserted that the Stewart-Feder paper "claims fraud" (as opposed to error). He did not document that assertion, which is false. Lewin did not answer Stewart's request to document his assertion. (Of course, like others before him, Lewin interpreted the factual documentation of the Stewart-Feder papers as evidence of fraud - which is something else.) Second, he proposed the

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[11] I entirely agree with the first reviewer's recommendation: publish, with a disclaimer. I object to the positions expressed by the other four reviewers. See also the comment by *Nature*'s editor quoted in V, §1 (a) below.

creation of "an impartial committee of immunologists to investigate the allegation, on the basis of full access to the original laboratory notebooks"; however, Stewart-Feder or others would not have access to such data. He also asked that Stewart-Feder abide by the decision of such a committee:

> If it should find that the original data are acceptable within the norms of experimental work, then of course the matter will have been resolved, and you will want to state so directly to those people with whom you have been corresponding about the work.

II. THE FIRST ISSUES OF RESPONSIBILITY

We now pause in the development of the historical account to comment more extensively on the problems of scientific responsibilities which have arisen, and which involve:

the responsibility of answering questions about one's work;
the responsibility whether to submit to authority.

Both these problems of responsibility are raised by the positions taken by Baltimore, Wortis and *Cell* Editor Benjamin Lewin. I find Baltimore's position and Lewin's letter to Walter Stewart remarkable, and going against the traditional standards of science:

- Baltimore and Lewin's position goes against the open discussion of claimed scientific results.
- Baltimore and Lewin improperly ask scientists to abide by the decisions of a committee of experts without the scientists having access to data. They thereby ask scientists to take scientific results on authority.
- Finally, Baltimore and Lewin's proposals do not deal with the scientific factual questions raised by Stewart-Feder, but with whether "the norms of scientific research were not transgressed", and with whether "original data are acceptable within the norms of experimental work". By such a formulation, considering only transgressions of the norms of science, authorities may arbitrarily redefine whatever norms are convenient to prevent factual questioning concerning the bases and justifications for the conclusions of a paper reporting on an experiment.

According to the norms of science as I have always known them, the determination of correctness and significance of results in science cannot be done under the authority of a committee or a single person or organization. It can be arrived at only by open discussion, based on publicly available data that anyone can check. Experiments must be reproducible, based on the data of the experimenters. These norms require that scientists answer questions about their works; and that data (in the case of experimental sciences) or proofs (in the case of mathematics) be supplied on demand, if for some reason they were not part of the published paper announcing scientific results.[12]

I claim that the *Cell* editor's letter to Walter Stewart is a *prima facie* case of improper scientific

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[12]In this connection, I quote from a letter dated January 31, 1989, written to Baltimore by NIH Director James Wyngaarden, who communicated to Baltimore the conclusions of a panel appointed by NIH to look into the matter. The panel is known by the name of its Chairman, Joseph Davie, and included Hugh McDevitt and Ursula Storb in addition. (See III below.) Wyngaarden wrote to Baltimore:

> It appears that even though the allegations have been known to you and the other coauthors of the *Cell* paper since the Spring of 1986, the coauthors never met to consider seriously the allegations or to reexamine the data to determine whether there might be some basis for the allegations. Such an analysis on the part of the paper's coauthors, followed by appropriate action to correct such errors of oversights, may well have made a full investigation unnecessary.

conduct. Baltimore expressed himself even more strongly when he wrote directly to Stewart and Feder on 24 March:

> I made two suggestions: that you either accept the judgements of Eisen and Wortis or have other, independently chosen, immunologists consider the question.
>
> **If you do not wish to take the words of Drs. Eisen and Wortis, it merely shows how far removed you are from the ordinary behavior of scientists who look to each other for judgement and critical evaluation. If you consider that you want more than a statement that the paper in question is viewed by independently chosen immunologists as within the norms of scientific communication, then you are asking to judge by a criterion you have established for yourselves.** [*Bold face added for emphasis.*] Although that is your right, there is no reason for the rest of the scientific community to go along with your particular desires.
>
> ...I am tired of this and convinced that it serves no purpose. Please leave me out of your further attempts to enjoy yourselves at the expense of others.

I think there is every reason for the scientific community to decide very clearly whether it goes along with Baltimore's position that the ordinary behavior of scientists is "to take the words" of experts or authorities, rather than to arrive at independent judgments, based on freely available data. Baltimore's position represents a profound disagreement concerning standards for making scientific criticisms. If Baltimore's view, that scientists who do not take the words of authorities are far removed from the ordinary behavior of scientists, prevails in the scientific community, then something fundamental, very serious, and very disturbing is happening to the scientific community.

The traditional view, which is completely opposite to Baltimore's, was well expressed by Feynman:[13]

> Other kinds of errors are more characteristic of poor science. When I was at Cornell, I often talked to the people in the psychology department. One of the students told me she wanted to do an experiment that went something like this – it had been found by others that under certain circumstances, X, rats did something. A. She was curious as to whether, if she changed the circumstances to Y, they would still do A. So her proposal was to do the experiment under circumstances Y and see if they still did A.
>
> I explained to her that it was necessary first to repeat in her laboratory the experiment of the other person – to do it under condition X to see if she could also get result A, and then change to Y and see if A changed. Then she would know that the real difference was the thing she thought she had under control.
>
> She was very delighted with this new idea, and went to her professor. And his reply was, no, you cannot do that, because the experiment has already been done and you would be wasting time. This was in about 1947 or so, and it seems to have been the general policy then to not try to repeat psychological experiments, but only to change the conditions and see what happens.
>
> Nowadays there's a certain danger of the same thing happening, even in the famous field of physics. I was shocked to hear of an experiment done at the big accelerator at the National Accelerator Laboratory, where a person used deuterium. In order to compare his heavy hydrogen

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[13]Feynman in his book also tells the story of what happened after Millikan's experiment determining the charge of the electron, showing how experts can fool themselves by trusting authority. As Feynman says: "It's a thing that scientists are ashamed of - this history...". Indeed, it turned out that Millikan used a slightly wrong value for the viscosity of air. For years physicists found a higher charge, but they fooled themselves. Trusting Millikan, they looked for and found reasons to discard their answers if these were too high compared to Millikan's. If they found a value closer to Millikan's, they did not look so hard. As a result, the value of the charge increased as a function of time, until the value settled down to a number definitely higher than Millikan's. Feynman adds: "We've learned those tricks nowadays, and now we don't have that kind of a disease." In light of the Baltimore case, the extent to which the scientific community has "that kind of a disease" remains to be seen.

results to what might happen with light hydrogen, he had to use data from someone else's experiment on light hydrogen, which was done on different apparatus. When asked why, he said it was because he couldn't get time on the program (because there's so little time and it's such expensive apparatus) to do the experiment with light hydrogen on this apparatus because there wouldn't be any new result. And so the men in charge of programs at NASL are so anxious for new results, in order to get more money to keep the thing going for public relations purposes, they are destroying - possibly - the value of the experiments themselves, which is the whole purpose of the thing. It is often hard for the experimenters there to complete their work as their scientific integrity demands.

From *Surely You're Joking, Mr. Feynman*, last section,
adapted from the Caltech 1974 Commencement Address

What would Feynman say about the Baltimore position, which goes so much further than what Feynman describes in corrupting the traditional norms of science! And it isn't just Baltimore or Wortis, or Lewin individually. Their position was backed up by the scientific establishment at Tufts, MIT, and elsewhere, for instance by the reviewers who recommended against the publication of the Stewart-Feder article.

III. THE NIH INVESTIGATIONS

The first NIH Panel. In late 1987 or early 1988 the NIH started investigating matters more formally. The NIH first formed a committee, against which Stewart-Feder and Dingell (among others) raised objections because some members of this committee might have a conflict of interest, having been closely associated with Baltimore. Although the first official position of the NIH was that "peer review procedures do not invariably exclude all co-authors and former associates",[14] the NIH in May 1988 created another investigative Panel. Three scientists served as members: Joseph Davie (President for Research and Development at Searle); Hugh McDevitt (Professor and Chairman, Department of Microbiology and Immunology, Stanford University School of Medicine); and Ursula Storb (Professor, Department of Molecular Genetics and Cell Biology, University of Chicago). The charge to the panel was to determine if the *Cell* paper was accurate, as judged by the existing laboratory data. The panel was also asked to describe the nature and extent of inaccuracies, if any, and to state whether misrepresentation or other misconduct was involved. The Report of this Panel was submitted to NIH 18 January 1989. In that report, they wrote:

> ...The Panel found significant errors of misstatement and omission, as well as lapses in scientific judgment and interlaboratory communication. However, no evidence of fraud, conscious misrepresentation, or manipulation of data was found...
>
> With regard to Table 2, the following conclusions were made by the Panel. First, isotyping was not done, but was claimed to have been done. Second, the data are for wells, not clones...
>
> ...the panel was impressed by the amount of work done in support of the studies published in the paper in Cell, by the completeness of the records, and by the abilities of both Drs. Imanishi-Kari and Weaver to find, accurately interpret, and present data on experiments that were performed as much as three or four years earlier...
>
> RECOMMENDATIONS
>
> 1. The Panel felt that the inaccuracies in Table 2 of this paper are sufficiently serious to merit a letter to Cell informing the editors of this fact...In addition, it should be stated that the data originally presented in Table 2 were not from clones, and that isotype determination was not performed.

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[14]Memorandum from Mary L. Miers to Walter Stewart, 11 March 1988

2. Clerical errors in Table 3 should be corrected.
3. The Panel recommends that the problems in the relative sensitivity and specificity of the Bet-1 and anti-idiotype reagents and assays be reported to *Cell* in a brief report...
4. In view of the fact that the panel found no evidence of fraud, misconduct, manipulation of data, or serious conceptual error, the Panel felt that no further action was required, other than those identified above...

The Panel has been provided with a copy of a letter that the coauthors of the *Cell* paper have submitted to the editors of that journal...However, the corrective action taken by the coauthors does not fully meet with the recommendations of the Panel as identified above.

Therefore in January 1989, NIH was still saying officially that there was found no evidence of "fraud, misconduct, manipulation of data, or serious conceptual error", despite the fact that both from internal NIH memoranda and NIH reviewers' reports on the Stewart-Feder paper cited above, in 1986 and 1987 a number of scientists (including some at NIH) interpreted the situation from the start as involving misconduct or fraud, possibly warranting an investigation. The official report should also be compared with the testimony given by Davie himself at the Dingell hearings, 4 May 1989.[15]

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[15] I quote an exchange:

Mr. CHAFIN. So they described an experiment that was not done.
Mr. DAVIE. That's correct.
Mr. CHAFIN. Is that misconduct?
Mr. DAVIE. I believe it is misconduct. I believe that we also recommended that that be corrected in a subsequent --
Mr. CHAFIN. Was it corrected?
Mr. DAVIE. Yes.
Mr. CHAFIN. Did you say misconduct in your report?
Mr. DAVIE. We called it a serious error.
Mr. CHAFIN. In an earlier draft, it said that there was no fraud, misconduct, and, I believe several other things. I think you actually struck out the word misconduct. You were saying there was no fraud and, I guess, saying there was misconduct. What happened to that? You were the chairman of the panel.
Mr. DAVIE. We obviously discussed how to describe these problems.
Mr. CHAFIN. I understand.
Mr. DAVIE. In English. And while I believe everybody understands what was done or what wasn't done, how you describe it is not so easy. You may call it misconduct. I cannot argue with that. Yes it's misconduct. Any time one describes something that's not accurate, that's not right. That's misconduct.
Mr. WYDEN. Mr. Chairman?
Mr. DAVIE. But we called it a serious error. It is a subtlety. I'm not sure I can defend it at this juncture.
Mr. WYDEN. Dr. Davie, if you believe it was scientific misconduct, why wasn't it described as scientific misconduct in the report?
Mr. DAVIE. If you call misconduct an intent to deceive or to do wrong, that's a problem. That's clearly misconduct. It was not possible for us to be sure that they intended to deceive in that way...
Mr. CHAFIN. Were you ever asked to look at intent by Dr. Wyngaarden or as part of your charter?
Mr. DAVIE. Part of our charter was to deal with whether there were data in the data books that fit the data in the cell paper and whether the conclusions were correct.
Mr. CHAFIN. Right. So there wasn't any look for intent--
Mr. DAVIE. Right.
Mr. CHAFIN. So you're saying then that you conduct an investigation and, in terms of putting it into a certain category, you need to know the intent, but you're not asked to look for the intent. It's an interesting catch-22.
Mr. DAVIE. That's correct...

[*The above exchange occurs on page 16 of the hearings; but on page 28 we find:*]

Mr. DAVIE. We talked about that before when Mr. Chafin was addressing that issue. I really gave misinformation. Part of the charge of the Committee was to come forward with an opinion of whether misconduct has been perpetrated in generating that information. So we were asked by the NIH to come forward with our opinions of whether misconduct was involved. Our impression that is misconduct means

The OSI Draft Report. Congressional hearings by Dingell and press articles reporting unfavorably on science put pressure on NIH to do more than had been done in the past to clear matters up. The Dingell Subcommittee had asked the Secret Service to make forensic analyses of the Imanishi-Kari lab notebooks to verify the authenticity of the entries relevant to the *Cell* paper. These analyses were presented at the hearings of 4 May 1989, and showed that key pieces of evidence relied upon by the Davie Panel had been recorded after Margot O'Toole's challenge to the paper, and one to two years after the nominal date of the experiment. Then in Summer 1989 NIH instituted still another investigation via its Office of Scientific Integrity (OSI). The OSI Draft Report was leaked in March 1991, and was widely reported in the press. I quote here from one of their main conclusions:

> The forensic evidence and the extensive statistical analyses establish that the June subcloning data and the January fusion data are fabricated. It remains unclear if these experiments actually were done...
>
> Dr. Imanishi-Kari repeatedly presented false and misleading information to the NIH and OSI and to the expert scientific panels...
>
> It is probable that a substantial portion of the I-1 notebook, the major source of data provided to substantiate the Weaver et al. *Cell* paper, was falsified...

The OSI Draft Report in addition to determining that data had been fabricated, also questioned the way Baltimore exercised scientific responsibility. They wrote:

> Dr. Baltimore's most recently-expressed views concerning the investigation are the most deeply troubling. These were the statements Dr. Baltimore made on April 10, 1990, when he was interviewed by the OSI investigation team. Dr. Baltimore disputed the significance of the June subcloning data and he asserted that if they were fabricated, the NIH was somehow responsible for this act of scientific misconduct: "If those data were not real, then she (Dr. Imanishi-Kari) was driven by the process of investigation into an unseemly act. But, it does not go to the heart of any scientific issue..." (p. 65). (Dr. Baltimore apparently was referring to the requirement by the NIH at the conclusion of the first investigation that the coauthors publish the June subcloning data as a correction to the Cell paper.)
>
> Dr. Baltimore went on to say that "...if something is not published, it's in your notebooks and it's not published, that it is not then a matter for those rules to be followed" (p. 66). "...[I]n my mind you can make up anything that you want in your notebooks, but you can't call it fraud if it wasn't published. Now, you managed to trick us into publishing - sort of tricked Thereza - into publishing a few numbers and now you're going to go back and see if you can produce those as fraud. But, I think you should see that was a forced situation..." (p. 68).
>
> The OSI found Dr. Baltimore's statements to be extraordinary. They are all the more startling when one considers that Dr. Baltimore, by virtue of his seniority and standing, might have been instrumental in affecting a resolution of the concerns about the *Cell* paper early on, possibly before Dr. Imanishi-Kari fabricated some of the data later found to be fraudulent.

In addition, the OSI Draft Report had substantial words of praise for Margot O'Toole:

> Dr. O'Toole suffered substantially for the simple act of raising questions about the accuracy of a scientific paper. The loss of her position in Dr. Imanishi-Kari's laboratory is only the most

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intent.

Mr. WYDEN. Was it your opinion that there was misconduct in this area, Dr. Davie?

Mr. DAVIE. No.

Mr. WYDEN. Pardon me?

Mr. DAVIE. No.

visible symbol for the price exacted of her after she raised the challenge to the paper. Notwithstanding the losses and costs she incurred, Dr. O'Toole maintained her commitment to scientific integrity throughout the several reviews and investigations that followed her challenge to the Cell paper.

Dr. O'Toole was invaluable to the effectiveness of the OSI investigation...

Dr. O'Toole's actions were heroic in many respects. She deserves the approbation and gratitude of the scientific community for her courage and her dedication to the belief that truth in science matters.

A Minority Opinion was submitted by Drs. Hugh McDevitt and Ursula Storb, NIH Panel members on the panel investigating the Weaver et al. 1986 *Cell* paper, dissenting from some of the conclusions.[16]

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[16]They state that some of the major conclusions are "not justified" in their judgment. They state: "In each of these sections [to which they object] the findings are open to several alternative interpretations, and depending on what scenario one considers most likely, the interpretations can be radically different. While the inconsistencies which are cited in these sections are certainly disturbing, and are compatible with the conclusions given in the report, alternate scenarios are sufficiently plausible that we cannot agree with the conclusions as stated in the report." However, as Margot O'Toole wrote in a reply to OSI (printed in *Nature*, 16 May 1991), they "do not describe a single alternative scenario that could explain away any of the evidence compiled by OSI. I therefore consider it imperative that the minority opinion be amended to include the scenarios that Drs. McDevitt and Storb believe are plausible explanations."

The Minority Opinion also objects to the "statistical analyses" as "new and untried", and they do not agree with conclusions based on such analyses. They do, however, state: "The second line of evidence presented in Sections IV A&B [of the OSI Draft Report] is much more convincing." [This evidence concerns forensic analyses.] They express one reservation about not having been given "access to the actual chromatograms which led the secret service to make the conclusions referred to above", but "with this reservation, the findings as stated, combined with the other inconsistencies found on the pages referred to above, make it seem likely that the data on these pages are not the result of experiments performed at or near the time stated, but in fact are data from other experiments performed as much as three years earlier. The significance of these data, their relevance to the initial and subsequent investigations, and the corresponding reasons why these data are legitimate targets for the present investigation, are all well described in the report, and we are in agreement with that description."

Finally, the Minority Report states that the general conclusions concerning David Baltimore, David Weaver, and Margot O'Toole "do not accurately reflect our own conclusions", but they do not state specifically in what respects, and they do not state "their own conclusions". In her reply to OSI, Margot O'Toole requested that the Minority Opinion be amended to give reasons for their dissent. I remind readers that McDevitt and Storb were the two members of the first NIH (Davie) Panel, besides the Chair. Words of praise for Margot O'Toole were deleted from a draft of the Davie Panel Report, and did not appear in the final version. Dingell inquired about this at the hearings of 4 May 1989. I reproduce most of the relevant exchange starting p. 39:

Mr. DINGELL. Now, Dr. Wyngaarden [Director of NIH], why was it in the NIH final report that there was no mention of your views with regard to the behavior of Dr. O'Toole, expressed here this morning by you and your associates, [that] her behavior throughout this matter was entirely correct?

Mr. WYNGAARDEN. Well, I can't answer for you the sequence of that comment being in an earlier draft and not in the final, but it was not with - I'm sorry?

...

Mr. DINGELL. Well, in an early draft of the NIH report, there were such comments about Dr. O'Toole, but they appear to have been excised before the report was finished, because they do not appear in the final draft. You are aware of this, are you not?

MR. WYNGAARDEN. Yes, I've heard that. I -

Mr. DINGELL. Why were those comments with regard to Dr. O'Toole's behavior excised from the report?

Mr. WYNGAARDEN. All I can say is that I did not excise them. I do not know.

Mr. DINGELL. [To Joseph M. Davie, Chairman of the investigating panel] ...Why were they excised?

Mr. DAVIE. I cannot answer that. I do not know.

Mr. DINGELL. ...Were those kinds of comments incorrect or inappropriate in a report of this kind?

Mr. DAVIE. No.

Mr. DINGELL. No, but they got removed. Did the removal occur after these matters left the panel or before?

Mr. DAVIE. I believe it occurred before. Again–
Mr. DINGELL. ...So the panel then removed them?
Mr. DAVIE. I believe that's correct.
Mr. DINGELL. As Chairman of the panel, why were these remarks removed?
Mr. DAVIE. ...We're saying that we do not have a good explanation for why that specific reference was removed.

...

Mr. DINGELL. Did anybody on the panel take them out? Did you take them out? You were the chairman. Did you taken them out, Doctor?
Mr. DAVIE. I'm not sure.
Mr. DINGELL. Well, did you take them out or not? Obviously, if you did, you would know.
Mr. DAVIE. This was a report, a draft that was –
Mr. DINGELL. Did you take them out? Yes or No?
Mr. DAVIE. I do not know, sir.
Mr. DINGELL. [To panel member Ursula Storb] Dr. Storb, did you take them out?
Ms. STORB. I do not - I did not take it out. I'm quite sure.
Mr. DINGELL. Do you know who did?
Ms. STORB. I was actually - I heard yesterday about this statement is -
Mr. DINGELL. Do you know who did?
Ms. STORB. [continuing]. Not in the report. It's a surprise to me, but, I'm sorry, I cannot offer any explanation as to how it was -
Mr. DINGELL. [To panel member Hugh McDevitt] How about you, Doctor, do you know who took them out?
Mr. McDEVITT. I did not take them out and it was my oversight and I guess all of our oversight that we did not notice they were taken out and insist that they go back in.
Mr. DINGELL. Did anybody take them out?...
Mr. McDEVITT. Since the whole thing was done in the central NIH office, it could have been done by any one of a number of people who...
Mr. DINGELL. Could it have been done by some person other than a panel member?
Mr. McDEVITT. Yes.
Mr. DINGELL. That's a curious way for a panel to work. Did the panel write the report or did somebody else write the report?
Mr. McDEVITT. We clearly wrote the report.

...

Mr. DINGELL. ...The early drafts had laudatory remarks about Dr. O'Toole and subsequent reports lacked them. Now, why?
Mr. DAVIE. You're simply asking questions that we really did not address in our review of this. You're talking about early drafts of the substance. We can't really recall.

...

Mr. DINGELL. You were an independent panel, but the NIH staff then edited your work, is that what you're telling me?
Mr. DAVIE. They helped us in the editing of the final draft....
Mr. DINGELL. Dr. Wyngaarden, how is this an independent panel which has its work edited by the NIH staff? Is that the way your independent panels function out at NIH?
Mr. WYNGAARDEN. The drafts were reviewed internally, and there were some editorial suggestions. I have a list of them, if you're interested in them.
Mr. DINGELL. Well, none of the panel remembers getting this editorial suggestion. None of them knows who made the change. They've indicated to me that this whole report was edited by NIH's staff.

...

Mr. DINGELL. ...Now, I'm trying to figure out if this is the way you function with your independent committees which are assigned specific responsibilities at NIH...

...

Mr. WYNGAARDEN. ...I'm not aware that any of the NIH staff changed any of the content of the report.
Mr. DINGELL. Well, nobody knows who changed it. They say that the NIH staff reviewed. They don't remember doing it. That means that it must have been the NIH or some other person, perhaps somebody in the dark of night. I'm just trying to find out how, and what is the integrity of these panels that you set up.

The Draft Report gave rise to extensive press coverage, in the press at large and in the scientific press. This coverage was overwhelmingly supportive of Margot O'Toole.

Baltimore himself issued a statement of contrition (*Nature*, 9 May 1991): "I now recognize that I was too willing to accept Imanishi-Kari's explanations, and to excuse discrepancies as mere sloppiness. Further, I did too little to seek an independent verification of her data and conclusions...I am shocked and saddened by the revelations of possible alteration and fabrication of data."[17] He was also quoted widely in the general press (e.g. *New York Times*, 21 March 1991): "...[the Draft Report], if it stands without major changes, raises very serious questions about serological data in the paper. Therefore I am today asking the other authors to join with me in requesting that the journal [*Cell*] retract the paper until such time as the questions are resolved. It is up to Thereza Imanishi-Kari to resolve them."

Nevertheless, subsequently, the OSI Draft Report came under attack for having been made and leaked under conditions lacking "due process". The incoming Director of NIH, Bernadine Healy, was a major force in undermining the credibility of OSI during summer 1991. She forced the resignation of Suzanne Hadley, formerly Deputy Director of OSI, from the Baltimore and Gallo investigations. As the *Journal of NIH Research* reports (September 1991):

> Many of the issues being raised by Healy and others are crucial to OSI's investigations - past, present, and future...Should OSI's procedures be changed? Do they afford whistle-blowers and the accused adequate protection and access to information?[18] Is the office adequately staffed and funded?...Have OSI's previous investigations been conducted appropriately? Will OSI's findings in the cases stand up to tougher scrutiny, for example, in the current criminal proceeding against Imanishi-Kari by the U.S. Attorney's Office in Maryland?
>
> Raising such questions is one thing. Attacking OSI publicly and repeatedly before the questions can be addressed is another. But that is what Healy has done...
>
> Some fear that Healy's criticisms - whatever her intentions - may damage irrevocably OSI's investigations of the Baltimore and Gallo cases.
>
> "Dr. Healy is trying to give anyone who wants to attack [Suzanne] Hadley or OSI the ammunition to do it." says a staffer on Rep. John Dingell's (D.-Mich) Subcommittee on Oversight and Investigations...The staffer says that attorneys who represent the defendants in the Baltimore and Gallo cases are "licking their chops," because now they can use the words of NIH's director to challenge the credibility of OSI's investigations.
>
> The staffers' fears appear to be well-founded. In an interview, Baltimore pointed directly to Healy's testimony and used it to defend his own position...
>
> There is no doubt that the uproar over Healy, Hadley and OSI has diverted attention from criticisms of Baltimore that, until recently, seemed an important new trend in the case...
>
> The future of OSI is uncertain. Healy sees the office as being at a "crossroads," Dingell wants to protect it, and Hadley and [OSI Director] Hallum continue to defend it...

IV. THE DINGELL SUBCOMMITTEE

§1. Attacks on the Dingell Subcommittee

Hearings on the Baltimore case were held by the Subcommittee on Oversight and Investigations, Chaired by John Dingell (D.-Mich) on 12 April 1988; on 4, 9 May 1989; and on 14 May 1990. At the time of the May 1989 Dingell hearings, Baltimore and his supporters mounted a major campaign attacking

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[17] For longer quotes and an analysis, see VI, and footnote 31.

[18] Both Margot O'Toole and Imanishi-Kari have complained (at different times, on different occasions) that they did not have access to information in some of the NIH investigations. See footnotes 24 and 25.

Dingell. Their position was that Dingell was improperly interfering with science, that he was hurting science, that he was unable to distinguish "error" from "fraud", and that he was about to legislate against "error". (Cf. Baltimore's testimony quoted in footnote 9.) Baltimore contributed a preliminary shot to this campaign a year before, in a "Dear Colleague" letter (19 May 1988), and followed it up with his article "Baltimore's Travels" in *Issues of Science and Technology*, Summer 1989.[19] Among the contributors to the campaign:

(a) Phillip A. Sharp, Director of the Center for Cancer Research at MIT, also wrote a "Dear Colleague" letter dated April 18, 1989, as follows:

> I am writing to you to ask your help in countering the continuing activities of Rep. John Dingell's subcommittee in Congress...At a meeting of the subcommittee in April 1988, the authors of the paper were vilified for their "fraudulent" work, even though there was no scientific basis for such a statement.
>
> Since that meeting, an NIH review panel has examined the paper and found no evidence of fraud or misrepresentation. Nonetheless, Rep. Dingell and his staff have continued on the attack...
>
> It seems obvious that the Congressional subcommittee has decided to continue to hassle David [Baltimore] and other authors and this has serious implications for all of us...
>
> Enclosed is a draft of an article that Bernard Davis has written for *Science*. Also enclosed is a fact sheet that could serve you as a sample for a letter or perhaps an "op-ed" piece. Here's what I'm asking you to do:
>
> **If you agree with me, write a letter (please don't use my sample exactly). Send it to: 1) your congressman and, if you can, to members of the subcommittee; and 2) write the editor of your local newspaper with a note asking that it be published before May 1. If you're so inclined, you might ask your colleagues to do the same. If this works, we will have gotten the message out to a large and influential segment of the population in a timely way.** [*boldface in the original*]
>
> The fight won't end there, but it's a good beginning. Please let me know if you will - or won't - join me in this. I'm writing to my congressman and to the *Boston Globe*.

(b) Dean Robert E. Pollack of Columbia College had a *New York Times* op-ed piece (2 May 1989):

> ### In Science, Error isn't Fraud
> **Dingell's inquiry is a witch hunt**
>
> Prof. David Baltimore of the Massachussetts Institute of Technology is under attack by Representative John Dingell of Michigan...
>
> Dr. Baltimore's reputation is at stake, but the rest of us will be affected by the outcome of these investigations as well. What has come under a legislative cloud for the first time in a very long time, perhaps ever in this country, is the legitimacy of the scientific method itself. This is an immediate and serious threat to science and medicine...the process of scientific investigation itself is at stake...
>
> I fear that science is about to be put to an unfair and dangerous test by Congress. I cannot claim to be an uninterested party: I am a grateful recipient of N.I.H. grant support for my own research, and it would be disingenuous not to acknowledge my deep respect for the peer review processes that define the funding of bio-medical research in this country.
>
> But as dean of Columbia College, I have a second concern. Already, too few young people are choosing to be scientists...If Congress legislates against error in science, there is no chance that a

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[19]To an uninformed reader, this article may appear reasonable. Someone to whom I mailed both Baltimore's article and O'Toole's testimony to the Dingell Subcommittee reacted by stating that if he had not seen that testimony, he would not have realized the extent to which Baltimore was giving a tendentious presentation of the facts and issues, and he would not have realized what Margot O'Toole went through.

sensible young person will choose to be a scientist, and there will be precious few of us to continue the work...

I would welcome a Congressional initiative to deal with fraud as such, but I fear that the way Dr. Baltimore is being treated means that witch-hunts are in the offing.

(c) Stephen J. Gould published a long article in the *The New York Times* (30 July 1989):

Ideas and Trends
Judging the Perils of Official Hostility to Scientific error

We all learned that Galileo discovered some of the moons of Jupiter and the phases of Venus. Few people realize, however, that he made a gigantic goof about Saturn...

We also all learned that Galileo was later convicted for defending and teaching the Copernican system, and that he spent the rest of his life under house arrest. We continue to deplore Galileo's fate and rank him first in the noble army of scientific martyrs. And yet, in the light of recent developments in Washington, I'm not so sure that Galileo might not be in more trouble today. Several Congressional Committees have been investigating scientific misconduct and some seem ready to view error as a cause for investigation into the misuse of Federal Funds. On this model, the Medicis of Florence might consider prosecuting Galileo for his misreading of Saturn.

"Scientific misconduct" is the subject under scrutiny by Representative John D. Dingell, Democrat of Michigan, and his Oversight and Investigations Subcommittee. The cause célèbre, a paper written by David Baltimore and colleagues, has been placed under the forensic equivalent of an electron microscope. This paper contains some errors, and some evidence of poor record keeping. The more public charge of fraud cannot be sustained.

Fraud and error are as different as arsenic and apple pie. The first is a pathology and a poison, the second an unavoidable consequence of any complex activity...

(d) Barbara Culliton's *Science* article "The Dingell Probe Finally Goes Public" (12 May 1989) was equally tendentious. I quote the beginning and the end.

Congressman John Dingell did his level best to pillory Nobel laureate David Baltimore last week. His principal stratagem: to catch Theresa Imanishi-Kari at fraud and watch her drag Baltimore down with her. He succeeded in neither count...

As the hearing broke up, Baltimore and Imanishi-Kari were deluged with congratulatory hugs. "It was heartwarming to get such support," Baltimore said. "I needed it."

But there was also a sense of caution, even fear. "Dingell now is like a wounded animal," said one. "There's no telling what will come next."

This is the type of journalism which affected the thinking of the scientific community.[20] Nevertheless, as correctly reported in Dan Greenberg's *Science and Government Report* 15 May 1989: "Baltimore Wins PR Battle. But Key Issues Remain."

§2. Congressional responsibility and scientific responsibility

A year later, Dingell opened the hearings of 14 May 1990 with a general statement running in part as follows:

In exploring the issue of scientific fraud and misconduct, this Subcommittee has focused on the

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[20] Be it noted that Barbara Culliton is now an editor of *Nature*. *Caveat emptor*.

> ability and will of major research institutions and the National Institutes of Health to police themselves when concerns about scientific misconduct are raised. We have seen a number of cases of proven misconduct that have been mishandled...[21]
>
> The distressing fact is that none of today's forensics should have been necessary. Former Director of NIH Wyngaarden put this clearly when he wrote Dr. Baltimore on January 31, 1989: [*see the quote in footnote 12*] Significantly, this statement was made over a year ago, and before the reopening of the current NIH investigation. The statement is even more relevant now.
>
> Scientists throughout the United States have claimed that this Subcommittee wishes to conduct a forensic analysis of every notebook involved in a prominent discovery. This is nonsense. They have claimed that the Subcommittee wants to "police" science. That is also nonsense. The Subcommittee expects the community of scientists to police itself. We have, of course, been severely disappointed by the response of the scientific community on a number of occasions...
>
> This disappointment extends particularly to the present instance. A number of prominent scientists, under a promise of confidentiality, examined the suspect notebook and agreed that it was obviously bogus. But these same scientists were unwilling to advance their professional opinions in public for fear of the disapproval of their colleagues. This reluctance by prominent scientists to deal fully and frankly with the problem of scientific fraud and misconduct has greatly complicated not only the present investigation, but others as well. There are signs of hope, however, that the current NIH investigation will resolve the allegations in this case in a factual manner.
>
> The standard in science was, and should always remain, a single thing: truth. It is only when allegations are minimized, data is not examined, and people do not behave in a straightforward manner, that it is necessary to employ forensic methods to settle questions of fact.

To Dingell's statement I would like to add what Margot O'Toole said in her testimony of 9 May 1989 (p. 187):

> Critics say that the activities of the subcommittee will make science a less attractive career for young people. Mr. Chairman and members of the subcommittee, the opposite is true. If you

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[21] I reproduce in footnote some more technical aspects of Dingell's statement, which ran as follows. "The key notebook furnished by Imanishi-Kari was indeed a curious sight. It contained pages bearing counter-tapes that had obviously been moved from their original positions, sliced into small pieces, and remounted. The pages showed a number of careful alterations that changed the meaning of the experiments and a number of carefully altered dates. Finally, the notebook purported to document some extraordinary experiments – experiments far better than those the authors had chosen to publish.

To clarify the meaning of these suspect records, the Subcommittee asked the Secret Service to conduct an objective analysis of the key notebooks. Their results indicated that the records had not been created at the time claimed...

Confronted with the Secret Service evidence in May of 1989 that the inks, the paper, and the impression analysis of her laboratory notebooks established convincingly that many pages were prepared in 1986, after the challenge to the paper, Dr. Imanishi-Kari admitted for the first time that much of the key notebooks had, in fact, been prepared in 1986. However, she continued to assert that the counter-tapes were strictly contemporaneous with the experiments...

Dr. Imanishi-Kari and her attorney have repeatedly claimed that they have not been informed of the NIH's allegations against her. These statements are simply false. In a letter of February 1, 1990, from Dr. Hadley, then Acting Director of NIH's Office of Scientific Integrity, to Dr. Imanishi-Kari, Dr. Hadley detailed three charges that constitute the focus of the OSI investigations:

(1) The possibility that substantial portions of the claims related to the immunological aspects of the *Cell* paper were not supported by proper experiments and reliable data at the time of the paper's submission;

(2) the possibility that after the problems with the paper were brought to light, there was systematic fabrication and falsification of data to support the pages; and

(3) the possibility that falsified and fabricated data regarding immunological aspects of the paper were included in representations to the National Institutes of Health and in published letters of correction to the 1986 *Cell* paper."

succeed in your goal of ensuring that concerns of junior scientists receive a fair and unbiased investigation, you will have provided a service for all scientists, and you will have made the profession more attractive. It seems many scientists believe that investigations of this type are unnecessary, even detrimental, because science itself, through the process of further experimentation is self-correcting.

I submit to them that an integral part of the self-correcting process is actions such as mine, and I challenge them to explain to me why this whole branch of the self-correcting process must be blocked.

This has been a long and agonizing affair. However, thanks to you and your staff, Mr. Chairman, there is now a good chance that a proper examination of the evidence will finally occur. The facts will be established based on the evidence, not based on who says I am wrong. The forensic evidence is now part of the equation, and I hope this issue will be settled soon.

When I appeared before this subcommittee last year, I was somewhat reluctant and very afraid. I knew my account would be labeled untrue by other principals in the case, and it has been. I felt they and not I would be believed and this, too, has been the case. I told of my experiences because the subcommittee requested that I do so. As a result of my testimony and my actions predating it, my competence and motives have been attacked by scientists from all over the world. But I had two very powerful factors in my favor. I knew the facts cold, and I was telling the truth. All I needed was a fair and thorough investigation.

The evidence is now proving that I have been telling the truth all along. For instance, when the scientific panel interviewed me in June 1988, I told them that a data page now dated November 1984 had been shown to me on May 23, 1986. I identified this page as one of the two shown to me in response to my challenge. This page bore notations in my handwriting. I further told them that at that time, May 1986, 5 months after the paper was submitted, it was stated that the data had just been generated. The panel paid no attention when I told them this. I did not know and neither did the panel that the Secret Service would later date this page to May 1986, contradicting the written date of May 1984.

This episode demonstrates a beautiful fact of nature and a basic tenet of science: the only version of events that can fit all the evidence is the true version. All that is required now is that the rest of the evidence be gathered and analyzed without bias.

Mr. Chairman and members of the subcommittee, I know you are under fire for insisting on a fair and thorough investigation, but I ask you to continue your interest in this case during this final stage of the NIH investigation.

V. FURTHER ISSUES OF RESPONSIBILITY

§1. Failures of the establishment press.

(a) Scientific journals such as *Cell, Science* and *Nature* originally turned down the paper by Stewart-Feder, analyzing the article by Baltimore *et al* which had been questioned by Margot O'Toole. They were thus closing off what should have been the natural channels of scientific criticism and exchange. After the NIH Draft Report, in *Nature* (28 March 1991), Editor John Maddox acknowledged: "It may be of some interest that, in 1987 and 1988, *Nature* declined to publish two versions of a manuscript in which Feder and Stewart spelt out what they considered to be errors in the published paper [of Baltimore *et al*], partly because it then seemed probable that the matter would be properly investigated. In retrospect, their arguments are more appealing than they may then have seemed." I regard the argument that "the matter would be properly investigated" as illegitimate. *Nature* was preventing the scientific community from learning of certain issues first hand at a crucial time. Hence *Nature* has a substantial (but of course not exclusive) responsibility for the escalation of the whole case.

After the NIH Draft Report, and after being criticized for its original obstructions, *Nature* then decided to publish an abbreviated version of the Stewart-Feder paper,[22] so that there is now a published record of this paper. Furthermore, almost every week for about five months after the NIH Draft Report, *Nature* published statements and rebuttals from the principal parties involved, and from scientists commenting on the issues.[23]

(b) The National Academy of Sciences *Issues in Science and Technology* published only Baltimore's point of view in "Baltimore's travels" (Summer 1989). They did not publish an opposite point of view, for instance Margot O'Toole's testimony to the Dingell Subcommittee. I wrote to Steven Marcus, editor of *Issues in Science and Technology,* to suggest publishing that testimony, even after the OSI Draft Report, because I think it is essential that readers should know first hand what was withheld from them. The testimony was not published.

I also submitted a piece for publication: "Aftermath of the NIH Draft Report on the Baltimore Case", (a piece similar to Parts IV and V of the present article, but shorter). The editor refused publication.

(c) In the *New York Times* editorial of 26 March 1991 commenting on the NIH Draft Report, and describing past behavior of Baltimore and colleagues, the editors stated:

> [Dr. Baltimore] orchestrated a chorus of support from sympathetic colleagues by sending a letter to 400 scientists warning that Congressional intervention could "cripple American science".

What the *New York Times* does *not* say is that it itself helped the orchestration when it published the article "Judging the Perils of Official Hostility to Scientific Error" by Stephen J. Gould; the op-ed page piece "In Science, Error isn't Fraud" by Dean Pollack of Columbia University; and other material. The position of the establishment backing Baltimore at the time was that there was no fraud, but only mere scientific error, and that Dingell's interference in science was inappropriate and dangerous. Two years later, immediately after the NIH Draft Report, the *New York Times* published a series of very informative news articles. My principal objection at this time was that the *New York Times* failed to reveal its role in having misled the public previously. Specifically, in the article "How Charges of Lab Fraud Grew Into a Cause Célèbre" (26 March 1991) the *New York Times* appropriately recalled *what* Gould wrote: "For example, Dr. Stephen J. Gould, a Harvard geology professor, wrote in 1989, 'in

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[22] 27 June 1991. However, *Nature* changed the title from "Original data contradict published claims: Analysis of a recent paper" to another title: "Analysis of a whistle blowing", which affects the context of the paper. I regard this change as improper journalism. If the editors of *Nature* wanted to put in their own title, it was incumbent on them to inform readers explicitly of their editorial intervention.

[23] The series started after a journalistically questionable prelude. First on 9 May 1991, *Nature* printed a statement of contrition by Baltimore. It was accompanied by an unsigned editorial, entitled "The end of the Baltimore saga", followed by the comment: "One of the most corrosive disputes of recent years in the research community should be ended with the open acknowledgment of the errors of an excess of trust by the principal in the case." The editorial goes on: "...There is some unfinished business, but one thing is clear: Baltimore has said enough to restore his own reputation as a fine scientist, a man of public spirit and potentially superb and certainly imaginative president of an outstanding and distinctive research university. Some among his colleagues may be tempted to use this public acknowledgement of error by their leader as an excuse for furthering their own narrow causes, but they should instead reflect on what Baltimore's ingenuity may eventually accomplish for their institution. To make an error may reflect on a person's judgement, but to confess it in the circumstances in which Baltimore now finds himself is a mark of courage. He deserves a break..." The *Nature* editorial is tendentious on several counts, one of them being the innuendo concerning those who had specific criticisms of Baltimore's position throughout the affair ("may be tempted..."). Aside from that, *Nature* had to reverse its position, because the very next week in the issue of 16 May, it printed "Margot O'Toole's record of events", with the comment: "The Baltimore saga continues. These excerpts from Dr. Margot O'Toole's comments on the recent draft report by the US NIH's Office of Scientific Integrity contradict at several points last week's statement by Dr. David Baltimore."

For still another reversal by *Nature*, see footnote 27, and for other implications of Baltimore's statement, see VI and footnote 31.

the light of recent developments in Washington, I'm not sure that Galileo might not be in more trouble today.'" But the *New York Times* gave no reference for *where* Gould wrote, and in particular it did *not* say that Gould's comparison of Dingell with the inquisitors who caused Galileo trouble occurred precisely in the *New York Times* article "Judging the Perils of Official Hostility to Scientific Error".

As for Dean Pollack, he stated in his op-ed piece: "Science differs from politics, or religion, in precisely this one discipline: We agree in advance to simply reject our own findings when they have been shown to be in error. There is no shame to this." But Dean Pollack's statement reproduced only the rhetoric of science, not the reality. He misrepresented the reality and thereby misled the public, with the full force of a *New York Times* op-ed piece.

(d) The rhetoric and the reality. Speaking out - when? Publishing - when? The reality has been the opposite of the rhetoric especially when questions have been raised about eminent figures in the establishment. The discrepancy between the rhetoric and the reality is partly documented by scientific journals refusing to publish an article critical of the Baltimore paper, and is further documented in the way *Nature*'s editor John Maddox described first hand Baltimore's reaction in his *Nature* comments of 28 March 1991: "Some of Baltimore's friends (this one in particular) urged him to make some kind of public statement shedding full personal responsibility for the study. But they revealed an angrily defensive person, most offended that work with which he had been associated should be challenged." Maddox also published a *New York Times* op-ed piece "Dr. Baltimore's experiment in hubris" (31 March) after the NIH Draft Report came out, stating in part: "Loyalty to one's colleagues is admirable, but the ferocity of Dr. Baltimore's defense has been arrogant. He angrily rejected suggestions from friends (myself included) that he should publicly allow the possibility of error." So immediately after the NIH Draft Report, influential and powerful people such as Maddox tripped over themselves to dump on Baltimore. Why didn't Maddox write publicly two years before, or a year before, or six months before that "the ferocity of Dr. Baltimore's defense has been arrogant"? I personally object to John Maddox' conduct in not having made his point publicly sooner, and I want the scientific community to evaluate the journalistic responsibility of Maddox and others like him, in this context. The scientific community can also evaluate the journalism of the *New York Times*, which published Maddox's criticism of Baltimore in 1991, instead of, say, spring 1989 when they serviced the campaign against Dingell.

(e) Parallel sources of information. Throughout the Baltimore case, until the NIH Draft Report, one could not rely on the establishment press for systematic and correct information. One had to look elsewhere. The most notable place for the Baltimore case was Dan Greenberg's *Science and Government Report*. Greenberg's articles systematically provided extensive documentation. Among other things, in his articles, Greenberg quotes extensively from past and present reports, Congressional hearings and original sources, giving primary references from which readers may form their own conclusion, or may follow up his documentation.

Stewart and Feder also distributed a great deal of information, aside from submitting their scientific paper. All three, Greenberg, Stewart and Feder deserve the appreciation of the scientific and academic community.

(f) Obstructions in general. I entirely agree with the position expressed by Herman Eisen himself, *but only after the OSI Draft Report came out*, when the *New York Times* quoted him as saying (26 March 1991): "I think there ought to be some instrument to allow a person like Dr. O'Toole who was dissatisfied, to publish her objections. I blame myself very much for not urging Dr. O'Toole to publish. That was a major mistake. Of course, journals might not have published...".

§2. Closing ranks.

Members of the science establishment mostly backed Baltimore. Besides Phillip Sharp, Dean Pollack, Stephen J. Gould, figures such as Maxine Singer and Bernard Davis contributed to this backing. As reported in the *Washington Times* article "Scientists' empire strikes back at one who dared to challenge it" (16 June 1989):

> Ms. O'Toole has tried to respond to the avalanche of what she believes to be misleading information by contacting individual scientists who speak out publicly on the affair.
>
> Her experience with Maxine Singer is typical. In an unpublished letter to The Washington Post circulated among scientists, Ms. Singer, editorial board chairman of the National Academy of Sciences, replied to a Washington Post editorial that mentioned Ms. O'Toole's role as whistleblower in the Baltimore affair: "Whistleblowers' integrity is compromised," wrote Ms. Singer, "when they confuse scientific criticism with allegations of fraud."
>
> Margot O'Toole responded in a private letter: "Since the editorial you mention identifies only one whistleblower by name, me, you have publicly impugned my integrity." She went on to request that Ms. Singer "support your damaging statements about my integrity with actual evidence and publicly withdraw your assertions."
>
> Nearly a year later, after Ms. O'Toole left a phone message, Ms. Singer replied in writing: "You must have read a copy of my letter. However, because the Post never published my letter, there seemed (and seems) no purpose in responding to your comments."

Maxine Singer did indeed write her letter as described above on official NAS stationery, bearing her identification as Editorial Board Chairman, and her letter was indeed circulated among scientists (I have a copy). The *Washington Times* continued by describing the role of Bernard Davis, who had been invoked by Phillip Sharp in his "Dear Colleague" letter:

> Bernard Davis, a professor at Harvard Medical School who has criticized Congress for launching "a paralytic legislative crusade for an unattainable degree of purity," expresses a sentiment shared by many scientists: "Whose judgment am I to take more seriously? I have to look at the fantastically productive record of Dr. Baltimore, not only before, but since he won the Nobel Prize [vs.] a postdoc [Ms. O'Toole]. You can call that a ganging up, or covering up, but whose judgment am I to take more seriously?"
>
> While having written on the subject he hasn't examined the data personally. "I felt with so many experts close to the field going over it, it would be presumptuous of me to think that I could shed any new light."

Here we behold a scientist going against one of the basic tenets of science by relying on authority and big time certifications such as the Nobel prize, thinking it "presumptuous" to engage in his own independent analyses.

In his *New York Times* piece of 31 March 1991, John Maddox had this to say about the responsibility of scientists and the damage to science due to closing ranks:

> When the Dingell inquiry was announced, he [Baltimore] circulated a letter to the scientific community warning of the dangers of Congressional interference in science. That danger is one of dark consequence. Another is the damage to O'Toole, who behaved properly throughout. She was without a job for three years. Those who have carried out the investigations at Tufts, M.I.T. and the N.I.H. have been made to look foolish, dupes of Dr. Baltimore's glittering reputation. It has taken the Secret Service to show that the disputed data were fabricated.
>
> The damage to the scientific community's reputation will also be considerable, after a decade's anxiety about laboratory fraud...
>
> But this case will seem proof that the scientific community can cover up the errors of eminent insiders at the expense of unestablished whistleblowers. The disputed article would not have survived had not Dr. Baltimore been its champion.

§3. The legalization of scientific responsibility?

Questions have arisen about the policing of science. Who is responsible for the policing? My answer is: all of us. I object to a legal approach when settling questions of science or scientific behavior. In policing science, I favor applying the norms of science, which require scientists to answer criticisms of their works openly and publicly. What may start as scientific objections may evolve into questions of fraud or misconduct. I object to the attempt which has been made to drive someone from the start into the position that either there is only "error", in which case there is no need for investigation, and corrections need not be made; or there is an allegation of "fraud" or "misconduct", and then one must establish legal or quasi-legal procedures, as described in "Baltimore's travels".[24]

Some editorials in *Nature* have been phrased in terms of the legalization of investigations, for instance the unsigned editorial "Even misconduct trials should be fair" (*Nature* 28 March 1991). Of course I am not against being "fair", but the word means different things to different people, especially lawyers. I do not agree that the OSI investigation was a "trial". I disagree with the conclusion: "Because OSI is a quasi-legal office, it should in fairness adopt the safeguards of the legal system." The legal system safeguards can only too easily be used to put constraints on the open confrontations of ideas and challenges which form part of the traditional norms of science. On the other hand, the editorial takes OSI severely to task for not giving "due process" to the "accused". Similar charges were repeated in an editorial "NIH need clear definition of fraud" by Barbara Culliton (*Nature*, 15 August 1991). The validity of such charges was not documented in the *Nature* editorials.[25] In any case, I do not

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[24] In his article "Baltimore's travels" (*Issues of Science and Technology*, Summer 1989), Baltimore wrote on the policing of science, by describing what he says is being done at the Whitehead Institute of MIT, and he suggests the following procedure as a model.

> **Policing science**
> -A question about the possibility of scientific misconduct can be raised by anyone in an entirely confidential manner.
> -Once a question has been raised, the director [of the Whitehead Institute] appoints a committee of inquiry, composed of appropriate and knowledgeable people; selections are made confidentially...
> -Both at the outset of an investigation and after it has been concluded, funding agencies are fully informed.

Baltimore's emphasis on "misconduct" is misleading. I object to Baltimore's way of setting up alternatives, involving "misconduct" and legal terminology as he does ("fraud", "verdict", "accuser", "accused"... see p. 54 of his article). The original questions by Margot O'Toole did not deal with "misconduct" but with the existence of data concerning an experiment, the need to publish a correction, and the refusal to publish a correction. Similarly the paper submitted by Stewart-Feder to *Cell* dealt with questions of scientific fact and was making a correction., but was not accepted for publication. Baltimore does not address himself to the situation at hand.

Who decides what is "appropriate"? Who is "knowledgeable"? Who determines what constitutes "misconduct" and when? In the Baltimore case, different committees have arrived at different conclusions at different times. For instance, in 1988-1989 the NIH had appointed the Davie panel to look into the objections raised about the *Cell* paper. That panel came to the conclusion that "no evidence was found of fraud, misconduct, manipulation of data, or serious conceptual error..." It actually turned out that the paper was contaminated by what the subsequent NIH Draft Report did find. Even the minority report accepted the existence of problems.

Why the confidentiality when questioning scientific results? Why inform only funding agencies? The scientific journals are read potentially by all scientists. I think it is entirely legitimate to raise questions about the public availability of data, and to hold scientists accountable to themselves and to the public. Agencies outside the scientific community such as the Dingell Subcommittee got involved because the scientific avenues for resolving scientific questioning turned out to be clogged and could not be trusted by some scientists. As far as I am concerned, only open exchanges within the scientific community can legitimately evaluate conduct.

[25] The situation involves a morass of conflicting statements. Culliton, speaking of Suzanne Hadley (former acting director of OSI), writes: "Hadley's organizing principle for OSI has been that if someone sees all of the evidence, he or she is in a better position to explain it away. Better to keep the allegedly damaging data secret, available to the accused only in summary form." On the other hand, Suzanne Hadley answered in a letter to *Nature* (19 September 1991): "This assertion is flatly incorrect. I always followed PHS Policies and Procedures...which state

agree that NIH needs a "clear definition of fraud". Scientific responsibilities traditionally transcend such a legal approach. I do not know of any clear definition of "fraud": some extreme cases are universally accepted as "fraud", but there is no agreement on all cases. As far as I can judge, any such definition will simply reinforce a point of view currently supported by some scientists that if what one does is not "illegal", then it's acceptable; and it will contribute to the legalistic morass. Such a point of view undermines the exercise of scientific responsibilities, as distinguished from legal responsibilities. I share Feynman's view of scientific responsibilities, which I shall quote further below.

The 28 March *Nature* editorial also stated: "Investigations of scientific misconduct should be subject to the 'sunshine' laws that apply to many areas of government business. NIH should develop a system whereby the prosecutor - OSI - and the defendant could present their respective cases to an appropriately constituted panel of peers in an open hearing." I object to the first phrase, viewing OSI as a "prosecutor" and certain scientists as "defendants". However, in the present circumstances, I agree that the NIH investigations should have proceeded in open hearings, in full view of the scientific community, which should have had documentation immediately available as a basis for independent judgment. The function of OSI would not be so much a "prosecution" but would be more to provide an open forum in cases when scientists refuse to answer other scientists' challenges about their work or provide insufficient answers, and when scientific journals refuse to publish such challenges.[26]

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that subjects of an inquiry or investigation "are provided access to any research data under review...[and] are provided with an opportunity to review and comment on significant investigatory documents...". Culliton also wrote that "...OSI's definition of due process...expressly denies the accused the right to see at first hand all of the evidence against him or her." In her letter to *Nature*, Hadley replied: "As I have said this assertion is not correct. The only instance of which I am aware in which there was any departure from PHS policy occurred when the OSI did not have possession or control of certain pieces of evidence." Culliton then wrote: "Why, if damaging data are so faithfully provided, do so many scientists and their lawyers complain of secrecy?" Such a question is a rhetorical thrust, which is not a substitute for documentation.

On the one hand, Susan Hadley's letter was misleading in one respect. As pointed out in a letter from Philip Siekevitz to *Nature* (Vol. 353, 17 October 1991), the text of the *Federal Register* does put a limitation on what subjects of an investigation are provided with to review and comment, namely "documents identified by OSI unless such disclosure would violate individual confidentiality or significantly impair the investigation...No opportunity is provided for subjects to confront and cross-examine other witnesses interviewed by OSI."

On the other hand, what precisely was withheld and by whom? *Science* (2 January 1992) reports: "Imanishi-Kari has refused to comment on the draft report (beyond an attack on the overall process) until she is allowed to see the original laboratory notebooks on which OSI and other federal investigators carried out the forensic analysis that led them to conclude that data had been fabricated. Many of those notebooks, however, are in the hands of the US attorney, who has declined OSI's repeated request to turn them over to Imanishi-Kari. If a grand jury reaches an indictment, a legal process known as 'discovery' will allow Imanishi-Kari and her lawyer to examine the data. Until then, OSI considers its hands tied." This sort of concrete information (not given in *Nature*) puts a different light on OSI and Susan Hadley's assertions in *Nature*.

With respect to open hearings, Margot O'Toole gave the following testimony to the Dingell Subcommittee concerning the Davie Panel investigation, not the OSI investigation (9 May 1989, p. 201): "From my perspective the NIH investigative process has been flawed in a number of important respects....Another flaw in the NIH process is that it does not allow for challengers to see evidence...In my case I asked to be allowed to examine certain data, but the NIH released the report before the data was sent to me... We are scientists and we should examine data...A scientist is one who analyzes facts in order to reach conclusions. We should always examine the evidence to support our claims, and we should be able to do so in a collegial fashion. That we have lost the ability to do this indicts us all. In my opinion, it is for this reason, and this reason alone, that these hearings have become necessary."

[26]For comments on OSI procedures from the point of view of OSI people, and the problems of what constitutes "protection" of subjects on whatever side of an issue in such an investigation, I recommend an interview in Dan Greenberg's *Science and Government Report* (1 October 1991), with OSI Director Jules Hallum, Deputy Director Clyde Watkins, and Senior Scientist Alan Price. Hallum states, among other things: "What some of them are asking for, and they don't know the consequences, is an open hearing kind of investigation. That's this due-

I also object to the part of the 15 August editorial where Barbara Culliton asserts:

> A fundamental point needs to be resolved in this debate. Is it the government's job to ferret out and punish scientists who commit fraud in the course of conducting federally funded research, or should the government's official fraud office extend its reach to matters that are properly defined as error and carelessness? A logical response is that the government should stick to the former, leaving judgements about the adequacy of footnotes, perfection of data presentation in published tables or handling of students to editors, tenure committees and the bodies that award prizes to people whose behaviour is so exemplary that it sets them above the average.

Culliton phrases the alternatives in a tendentious manner. (In the Baltimore case, did the NIH "ferret out", or did it respond to complaints by scientists, or what?) I do not find her response "logical". Who is to evaluate what constitutes "exemplary" behaviour? What does "average" mean? At the start of a scientific challenge, one does not know whether a problem is due to the adequacy of a footnote, sloppy data, fabricated data, or worse. One traditional view of scientific responsibility is that one cannot rely on any one's authority to determine the merits of a case, but that full documentation must be publicly available, to provide the possibility of independent judgment. After ordinary scientific channels have shown themselves to be clogged, I don't see what is so illogical for a government agency such as NIH via OSI to provide an independent open forum for the presentation of scientific challenges. Fundamentally, if Baltimore and co-authors accepted to answer publicly and through scientific channels questions raised about their work; and if the editors of scientific journals (*Nature*, *Science*, *Cell*) allowed papers such as the one submitted by Stewart-Feder to appear and to form a basis for public scientific discussion, then the whole problem of whether to "investigate" and under what conditions would be moot as far as the responsibilities of the scientific community were concerned. The responsibility for a granting agency whether to make or continue a grant might require them to make an investigation, but that would be a separate matter. However, nowhere do the two *Nature* editorials consider the fundamental problem of scientists not answering scientific criticisms of their work, not allowing publication of criticisms, or requiring other scientists to submit to various authorities. The effect of such editorials is to draw attention away from this fundamental problem, while discrediting attempts by government agencies to prod scientists to maintain certain standards, when the scientific community itself is failing.

Two months later, subsequent to a fundamental point raised by Paul Doty (quoted in §5 below), *Nature* (10 October 1991) finally had a very different editorial (unsigned) facing the problem head-on:

> **Baltimore's defence**
> Although the final decision of the Imanishi-Kari case is some way off, one issue in the case is already clear...But there is one issue, raised by Professor Paul Doty (*Nature* 352, 183; 1991) which requires a decision by the scientific community rather than by an office of the US National Institutes of Health (NIH): what are the responsibilities of the authors of a published research report?
> The issue is simply stated. Dr David Baltimore, the most celebrated although not the

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process consideration. I think that's going to be very dangerous to science...There will be no protection for the whistle-blower, no protection for the reputation of the respondent. And another thing they haven't thought about is that if we're forced to that, then our procedures are the same as practically all the university [misconduct] procedures in the country. And then will less be expected of the universities, or will they have to go to an open court-like hearing, as well? I suspect they will. The criticism that makes me the most angry was the charge that we inhibit creative, new, and innovative research. That was a damned lie. I challenge the head of that society to show me one case where we have inhibited research. He said we're teaching the young scientists to do only safe science, because they're so terrified of being investigated by us for doing something original. Why aren't they terrified of the local organization?..."

> principal author of a disputed paper (*Cell* 45, 247; 1986) has from the outset taken the view that it is for the scientific community at large, and for others working in the field concerned, eventually to demonstrate the validity or otherwise of the disputed data and the conclusions drawn from them. It is a point of view, but hardly a defensible one, especially when the authenticity of the data on which the disputed paper's conclusions were supposedly based has been sharply questioned...The plain truth is that the authors of all published research reports have a personal responsibility for their aftercare...So much has hitherto been generally accepted. Were it otherwise, science itself would be undermined. For the presumption would be falsified that what appears in the literature can be regarded, at least provisionally, as authentic...

This editorial provides still another instance of a pattern, whereby the editors of *Nature* write up "the plain truth" only after someone such as Paul Doty comes out; and only years after others have been on the line to uphold the traditional norms of science, and when those on the line needed "the plain truth" about scientific standards to be expressed in the establishment press.[27]

The norms of science as I have always known them hold that the evaluation of experiments or data cannot be done confidentially, and that it is highly improper to have the ultimate verification entrusted to the authority of a single agent, whether a person or a committee. The scientific community is entitled to have full information on which to base an independent judgment.

I share the Feynman position (again, same reference as before):

> But there is *one* feature I notice that is generally missing in cargo cult science. That is the idea that we all hope you have learned in studying science in school - we never explicitly say what this is, but just hope that you catch on by all the examples of scientific investigation. It is interesting, therefore, to bring it out now and speak of it explicitly. It's a kind of scientific integrity, a principle of scientific thought that corresponds to a kind of utter honesty - a kind of leaning over backwards. For example, if you're doing an experiment, you should report everything that you think might make it invalid - not only what you think is right about it: other causes that could possibly explain your results; and things you thought of that you've eliminated by some other experiment, and how they worked - to make sure the other fellow can tell they have been eliminated.
>
> Details that could throw doubt on your interpretation must be given, if you know them. You must do the best you can - if you know anything at all wrong, or possibly wrong - to explain it. If you make a theory, for example, and advertise it, or put it out, then you must also put down all the facts that disagree with it, as well as those that agree with it...In summary, the idea is to try to give *all* the information to help others to judge the value of your contribution; not just the information that leads to judgment in one particular direction or another.

The scientific community is again faced with a choice: to uphold the legal concept of scientific responsibility as in "Baltimore's Travels" and in the *Nature* editorials of 28 March, 9 May, and 15 August, or the traditional concept of scientific responsibility represented by the *Nature* editorial of 10 October and Feynman.

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[27]For other instances, see §1a and §1b. *Nature* had plenty of opportunity to write up "the plain truth" long ago: when Stewart-Feder were submitting their paper for publication, but *Nature* refused it; when Baltimore was writing them: "If you do not wish to take the words of Drs. Eisen and Wortis, it merely shows how far removed you are from the ordinary behavior of scientists who look to each other for judgement and critical evaluation..."; when Baltimore was promoting his position in *Issues in Science and Technology* and in letters to colleagues; when Baltimore testified to the Dingell Committee; etc. More recently, instead of writing "the plain truth", *Nature* wrote in its editorial of 9 May 1991, "The end of the Baltimore Saga", quoted in footnote 22: "...Baltimore has said enough to restore his reputation as a fine scientist...He deserves a break."

§4. The panelization of scientific responsibility?

Throughout the Baltimore affair and in other cases, we have met with one panel after another. In the Baltimore case, this panelization started with the panels at MIT and Tufts. Then we had the Davie Panel at NIH. Benjamin Lewin, Editor of *Cell* proposed constituting an "impartial committee of immunologists" instead of publishing the article by Stewart-Feder, who of course refused to accept such a proposal since they would have had to surrender their independent judgment as scientists and would not themselves have had access to the laboratory data. After that, we had the Office of Scientific Integrity and its Draft Report.

Finally in late 1990, the NAS Committee on Science, Engineering, and Public Policy (COSEPUP) appointed a panel (still another one!) chaired by Edward David, with the charge:

1. To review modern research practices and analyze factors that could affect the integrity of research.
2. Examine the advantages and disadvantages of explicit guidelines to strengthen scientific standards for scientists and their institutions.
3. Clarify roles for public and private institutions in promoting responsible research practices, and assess institutional experience with current procedures for handling allegations of misconduct in science.

The *New York Times* of 28 March quoted an interview with the chairman Edward David: "It is terribly important for the country that the science community keep its ability to self-govern. That is what is being called in question now - the ability of universities and laboratories to govern themselves. And if we don't perform well to maintain that, we are in trouble." In that same article, the *New York Times* also quoted Robert Rosenzweig, President of the Association of American Universities: "Our main concern is to keep the universities' feet to the fire."

But it's not only a question of the universities.

(a) The NAS. The David-COSEPUP Panel might also keep the NAS feet to the fire. After all, the NAS was part of Baltimore's orchestra when it published only Baltimore's point of view in *Issues in Science and Technology*, and when Maxine Singer wrote to the *Washington Post* on NAS editorial stationery. It would be quite appropriate for the David-COSEPUP Panel to bite the hand that feeds it.

(b) Students and young scientists or established scientists? Despite the fact that it is established scientists who have transgressed the rhetoric of scientific conduct, there have been repeated suggestions laying blame on young scientists, or on the lack of courses in ethical matters for students.

-The NAS Committee on the Conduct of Science put out the pamphlet "On being a scientist". NAS President Frank Press wrote in his preface: "This booklet is written primarily for students who are beginning to do scientific research... The mechanisms that operate within science to maintain honesty and self-correction must therefore be honored and protected..." There is the rhetoric. We have seen the reality. Phillip Sharp, who orchestrated the campaign against Dingell, is one of the coauthors of this NAS pamphlet.

-Francisco Ayala chaired the NAS Committee that put out the pamphlet. He also published the article "For Young Scientists, Questions of Protocol and Propriety Can Be Bewildering" (*Chronicle of Higher Education* 22 November 1989) in which he invokes the pamphlet. In the title of his article and elsewhere, Ayala makes it appear as if the problem is for "young scientists"; but one thing bewildering to Margot O'Toole was that Baltimore did not immediately and routinely make a correction. What do the big time generalities of the NAS pamphlet mean in practice? What steps have Frank Press, Phillip Sharp, Francisco Ayala and the NAS taken to protect scientific whistleblowers?

Ayala also brought up "mentors" in his article: "The mentor system, in which a university faculty member serves as adviser and laboratory director for a group of graduate students, is the centerpiece of what always has been an oral tradition of passing on the values, ethics, and practices of science. " The article by Ayala is hypocritical and revealing in light of the performance of established scientists during the Baltimore case. Given the repeated failures of senior scientists, and in the Baltimore case of a large part of the scientific establishment, one might conclude more legitimately that the "mentor system" might push young scientists away from scientific values.[28] Ayala's centerpiece has shown itself to a large extent to be a centerpiece of misrepresentations, subservience to authority, intimidation, and arbitrary power (scientific and journalistic).

-A *Boston Globe* article titled "Rep. Dingell asks scrutiny of MIT, Tufts over handling of fraud probe" (22 March 1991) reports that in a letter to MIT faculty and researchers, the university's president, Charles M. Vest, called for development of a program "to provide career guidance and mentoring" and to communicate the values of science, which "demand the pursuit of truth with integrity and ethical rigor." The irony of Vest's program "to provide career guidance" would be laughable if it were not so sad, in light of what Baltimore and higher ups at MIT and Tufts did to Margot O'Toole's career.[29]

-The *New York Times* article of 28 March on the David-COSEPUP Panel states: "Another recommendation of the draft report [of this panel] is to have required courses in ethics and conduct within the regular curriculum for science students."

-Dr. Judith P. Swazey (of the Acadia Institute) is quoted in the same *Times* article as stating: "In our survey we found that deans felt it was terribly important for their students to learn about ethical issues, but most said they had no courses that taught the subject and no expectation that they would. There is a major gap between good intentions and practices." How does Swazey know what "intentions" are? Has Swazey surveyed Dean Pollack at Columbia University about Pollack's charge of "witch-

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[28]As for the mentor system, I quote from a letter to *Nature* (12 September 1991) by Louis DeFelice, Division of Biology, Caltech: "John Maddox (*Nature* 350, 269:1991 [28 March 1991]) asks why a scientist like David Baltimore should have so vigorously defended what has now proved to be a false position...As a scientist saddled with the aftermath of a parallel scandal, the Darsee affair, I wish to suggest a possible answer: researchers routinely accept a certain level of dishonesty and therefore defend larger transgressions that involve the same vice. The particular corruption that I speak of is unearned authorship.

...Because of administrative or monetary relationships between senior and junior scientists, marginal contributions may be elevated to authorship.

Earned authorship in scientific papers means doing the experiments, analysing the data, working out the theory, writing the paper, reading the literature...Merely encompassing the work under the umbrella of one's interest and ideas does not.

Why do scientists expose themselves in this way? For one thing, putting one's name where it does not belong is rewarded by granting agencies and tenure committees...

At Emory University, during the investigation of the Darsee affair, the argument was made that junior scientists cultivate senior co-authorships in order to get their work published...Regardless, this argument puts the blame on the privates and lets off the generals. Established scientists, under pressure to obtain extramural funds, are burdened with the baggage of success: leadership in national societies, membership of editorial boards and grant review panels, travel and lectures, committees and administration. These activities drain the time and the energy of every established investigator, and they make bench research nearly impossible. Yet the pressures to present oneself as being at the vanguard of research are greater than ever.

By accepting or insisting upon unearned authorship, much of the scientific community has forfeited the right to bear witness. Thus when investigations reveal unbecoming conduct that involves the same crime, scientists close their ranks, because many are guilty of far less spectacular but similar infractions."

[29]John Edsall, Emeritus Professor of Biochemistry at Harvard, had a "prepared statement" for the Dingell Subcommittee hearings of 12 April 1988, reading in part as follows (p. 149): "If a young scientist believes that he or she has witnessed a case of fraud, and comes to ask me about reporting it to the authorities, I would have to warn him or her emphatically about the dangers of doing so. If the potential whistle-blower decided nevertheless to proceed, I would admire and greatly respect the person and the decision, but I would have serious anxiety about the future of that individual, as the system operates today."

hunts" in his *New York Times* op-ed piece? What about the deans at Tufts and MIT?

In fact, the problems have not been with students; there has been no evidence in the recent cases of public notoriety (Baltimore, Gallo, Darsee, Breuning, Freeman, Braunwald,...) that students need to learn about ethical issues. Problems have arisen with established scientific figures whose teaching may consist of questionable examples. As I have pointed out repeatedly, problems in the Baltimore case (and others) have not been with "young scientists" either (e.g. Margot O'Toole); but I have seen problems with Imanishi-Kari, David Baltimore, Dean Pollack, Stephen J. Gould, Phillip Sharp, Bernard Davis, Maxine Singer, S. Marcus, established scientists all. Who would determine the content of courses in scientific ethics? On what examples would these courses be based? With what choice of material? Would it be exclusively the quoted views of the above scientists, or of Bernard Davis and the 143 scientists who published "An open letter on OSI's methods" in *Nature* (27 June 1991), stating:

> As scientists, we are deeply disturbed by the way in which the charges against Dr. Thereza Imanishi-Kari...have been handled by the Office of Scientific Integrity of the National Institutes of Health. The need for formal, thorough and fair investigations of possible scientific fraud is clear. However, it is apparent that the procedures followed by the OSI have serious shortcomings, and have not permitted Imanishi-Kari the opportunity to defend herself by a public examination of the evidence against her...Under the circumstances, we reserve judgement about the facts of this case until Imanishi-Kari has had an adequate opportunity to defend herself. It is not clear to us that the current procedures will allow this to occur.

Bernard Davis was among the signers. He also published a scathing attack on Margot O'Toole in the *Wall Street Journal* (22 July 1991), entitled "Dingell's Witness for the Persecution". Aside from a direct personal attack, he wrote:

> It is hard to avoid the suspicion that the OSI - recently created in response to congressional criticisms - has been excessively eager to establish fraud in this case...The scientific community has been split into passionate defenders and equally passionate critics of Margot O'Toole. But all would agree that Rep. Dingell's choice of this case was tragic and the costs have been excessive. These include a great deal of time and money, serious damage to reputations and to the public image of science, and the possible impairment of the future contributions of David Baltimore as an exceptional scientist and administrator. Even more serious, however, is the danger that we may end up imposing on science the kind of bureaucratic system of policing familiar to legislators. After all, it is asked, are scientists entitled to any more autonomy than bankers?
>
> But this is the wrong question. The issue is not entitlement: it is the value of autonomy in promoting creative research.[30]

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[30]In a **Commentary** (*The Scientist,* 13 May 1991), entitled "Is the Office of Scientific Integrity Too Zealous?", Bernard Davis also wrote: "While it is necessary to strengthen the NIH mechanisms for dealing with fraud, the existence of two offices, for a function that could well be performed by one, wastes both money and time. More disturbing than their structure is the broad mandate of these new offices, which instructs them not only to monitor and conduct investigations of misconduct but also to 'promote high standards of laboratory and clinical investigations in science through a prevention and education program.' This phrasing is fraught with possibilities for encouraging the government to mix problems of misconduct with problems of quality in the conduct of research...Another concern is that the mission of these offices is now being pursued with excessive zeal...I conclude that the new offices have become grotesque in their evident aim of purifying science root and branch, without recognition that the cure would do more harm than the disease...Though NIH enjoys a respected and even affectionate relationship with the scientific community, its overreaction to political pressure in combating fraud threatens the welfare of science on a much wider scale."

Bernard Davis has also published elsewhere in the press at large, as in the piece "More than a test-tube tempest", *Los Angeles Times,* 10 December 1991, where he iterates his distrust of government intervention "in dealing with scientific fraud", and casts aspersions on the credibility of OSI and Margot O'Toole. For instance, he writes: "Excessive reliance on O'Toole's sincere but often-contradicted testimony certainly contributed to the conclusion by the Office of Scientific Integrity of the National Institutes of Health that Baltimore's collaborator, Bernard Davis writes tendentiously, and he also writes presumptuously that "all would agree...". The "costs" have indeed been great, but what does "excessive" mean? Did the scientific community's failures deserve the costs? Was the damage to reputations (whose?) deserved? Not all would agree with the judgments of Bernard Davis that Dingell is the one responsible for the "costs", or that Dingell was undermining the "autonomy in promoting creative research". I, for one, do not agree. I do not recognize being an "exceptional scientist" as a license to throw one's weight around to avoid answering scientific criticisms.

§5. Some scientists speaking out.

In an article "Cover up charge puts scientists under microscope", the *Detroit News* of 29 October 1989 quotes one scientist: "Harvard's Walter Gilbert, who won a Nobel Prize in 1980 for DNA research, told *The Detroit News* the implication of the Secret Service findings to date is that 'those experiments weren't done at that time - or they were not done at all.'" After the OSI Draft Report came out, Walter Gilbert was further quoted in the *New York Times* (22 March 1991):

> The Whistle-blower herself did not want to call this fraud in the beginning, but she reported it to people who should have known better. The people in authority, and in my opinion that means at M.I.T and Tufts, failed to investigate properly. Neither of them seriously entertained the question of whether there had been fraud and what should be done.
>
> That is the greatest failure of the institutions. There is a canon of the establishment which says that when someone objects, that person must be a malcontent and be badly motivated and that science is holier than anyone or anything. This is the issue: what happens when a scientist is called upon to be unsure of his or her work.

In *Science and Government Report* of 15 October 1989, Dan Greenberg had quoted some scientists similarly, but anonymously:

> ...a number of highly respected scientists are just plain dismayed about Baltimore and the *Cell* paper. One of them, a Nobel laureate, told SGR on a non-attributable basis last June that "I have reservations about the [Baltimore] paper. Lots of people think, I too, that the broad claim of the paper is wrong." He said he agreed with another scientist's characterization of the paper as "sloppy," and added that "a set of experiments were done, probably sloppily, and others maybe were not done" - which is one of the allegations under investigation by NIH and the Dingell committee. The same scientist also said he felt Baltimore and allies had sought to obscure the dispute about the paper by raising the "issue of Congress attacking science." He said he deplored this as "a misrepresentation on the public".
>
> Another scientist, a member of the National Academy of Sciences who is on the faculty of a major university, told SGR - also on a non-attributable basis - that "Baltimore's extraordinary claim (in the *Cell* paper) is almost certainly wrong. The tragedy," he added, "is David's refusal to admit what anyone else would admit." Asked about assertions by Baltimore and several colleagues that the "central claim" of the paper has been replicated, this scientist said: "It's a goddam lie."

How come these anonymous scientists did not speak on the record? By definition, the anonymity they required at the time reflects a certain intimidation and even fear in the world of science. Other scientists (how many?) did not even allow such anonymous quotes from them, even though they might

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Thereza Imanishi-Kari, had committed fraud..." Readers of the present article can compare the documentation with the tendentious phrase "sincere but often-contradicted".

agree (contrary to those on the roster named above). Panels and courses cannot deal effectively with cases of intimidation, and in some cases panels have actually contributed to intimidation or to covering up. As *Nature* quotes Imanishi-Kari herself (27 September 1990): "If OSI reaches the conclusion that there was misconduct on my part, then you have to conclude that MIT covered up and Tufts covered up." The OSI has now reached that conclusion, and in this instance I agree with Imanishi-Kari.

On the other hand, after the NIH draft report, more scientists did come out on the record. I quote from two of them.

An open letter to an officer of the NAS
by John Cairns, Department of Cancer Biology
Harvard School of Public Health
(Published in *Nature*, 11 July 1991)

...

(2) Nothing now is likely to stop the affair from progressing to its final disastrous conclusion, and I do not see how the authors of the paper can escape public censure at the very least. About the only question remaining is whether anyone will actually go to jail....

(4) Some of the blame falls on the scientific community - on those who arranged and conducted the initial, perfunctory inquiries - on the National Academy for not demanding a proper investigation - and on the many scientists who did not look at the evidence and, instead, construed the whole business as a Congressional manoeuvre to attack the scientific establishment. (I remember that originally I too felt that the row was probably a political stunt.)

(5) Because the establishment has played such an undistinguished role, we may find it increasingly difficult to maintain the idea that science is a genuine search for truth and that scientists are generally honourable and deserving members of society...

(6) So I believe that, although it [is] now too late to do much good, the Academy should be issuing a statement (a) reaffirming the aims of science and (b) pointing out that if the rules and principles of science had been observed we wouldn't now be in this mess. For most scientists, science is the pursuit of a truth that is external to our wishes. This truth is quite unlike the verdict of a court of law because it does not depend on advocacy. Instead, each of us has to be responsible for the accuracy of our own statements; we cannot simply count on others to correct out mistakes. Each of us knows more about our own experiments than anyone else, and when something goes wrong we have to speak up. If the Academy does not say something like that, American scientists may end up with the same kind of public image as many of the contry's lawyers and politicians - which would do a great disservice to all young scientists.

Commentary by Paul Doty
Mallinckrodt Professor of Biochemistry Emeritus
Harvard University
(Published in *Nature*, 18 July 1991)

...Moreover, until the final OSI report is released, we will not know the extent to which the opposing views of the authors of the *Cell* paper will have affected final judgements. And the acceptance by the OSI draft report of compelling evidence for falsification of data may not be settled until there has been a court review. But in my view, the case for egregious departure from the usual standards of carrying out and reporting research stand independent of these remaining conflicts. The same applies to the succession of failures of reviews of the paper and the procedures used to address complaints against it once serious questions had been raised...

...Consider first a few of the lapses in scientific standards seen in the actions of various authors. The recording of data, especially by Dr. Thereza Imanishi-Kari, was so sloppy as to insult the scientific method. Reviewing the case strictly from Baltimore's published account reveals at least four lapses from what have been the traditional standards of science. He (1) failed to examine critically the quality and sufficiency of the data before publication; (2) failed to examine the data and report the possibility of error after serious criticisms were made; (3) instead organized an attack on his critics and discouraged publication of their views; and (4) did not subject the conclusions to further tests or check the reproducibility of what had been reported in a timely manner...

Baltimore's attitude towards the responsibility of authors checking the reliability of their own data is a critical departure from commons standards...To forgo this obligation - to leave to others the responsibility of establishing the validity of what you have published - is not only a fundamental retreat from responsibility but, if it became accepted practice, would erode the way science works...

But the essence of change must come within the scientific community by its reassertion of its ability to police itself...

This challenge to readdress the fundamental tenets of acceptable behaviour in science comes at a time when the traditions of the scientific enterprise are under new threats arising from new stresses and temptations. The growth of the enterprise itself with its accompanying bureaucracy, the near cut-throat competition for grants, the possible corruption, on occasion of peer review, the growing members of cases of deception in scientific papers, scientists' acquiescence in the increasing avoidance of meaningful review in direct congressional grants for research buildings and projects - all these contribute to the pressure to compromise and erode the high principles of the past. As a result the scientific community may already be experiencing a gradual departure from the traditional scientific standards; this could be abetted by condoning the behaviour seen in this present case. In this way we risk sliding down toward the standards of some other professions where the validity of action is decided by whether one can get away with it. For science to drift toward such a course would be fatal - not only to itself and the inspiration which carries it forward, but to the public trust which is its provider.

VI. Personal credibility and the shift at the scientific grass roots

I have emphasized fundamental issues of scientific responsibility. However, factors of personal credibility have also been important in the resolution of the Baltimore case (as far as it goes, changing with time). I shall put here together a chain of events from May to December 1991 which led to a disavowal of Baltimore among an important segment of the science community.

Immediately after the NIH Draft Report in spring 1991, Baltimore and some of his co-authors retracted the *Cell* article, and Baltimore published a relatively long statement in *Nature* (9 May 1991), in which he praised O'Toole; he attributed his own failures to "an excess of trust" in a co-worker (Imanishi-Kari); he cited the inquiries at Tufts and MIT which had found no "deliberate falsification or misrepresentation" but only different "interpretations"; he mentioned the retraction of the *Cell* paper "in light of the revelations" of the OSI Draft Report; and he acknowledged the "legitimate role of government...to protect the public interest and hold the scientific community accountable...".[31]

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[31] I quote several passages from that statement.

Dr Baltimore says "sorry"
Dr. Baltimore says he had no knowledge of the fabrication of data in a paper in *Cell* of which he was a co-author, says he will work to develop new guidelines for misconduct and apologizes to Dr. Margot O'Toole.

...I wish to state at the outset that my defence of Imanishi-Kari was not due to any lack of regard for Dr. Margot O'Toole, the postdoctoral fellow who first uncovered certain discrepancies in Imanishi-Kari's research. I have tremendous respect for O'Toole, personally and as a scientist, and I have consistently maintained that I believe that her analyses were insightful, her expressions of concern were proper and appropriate, and her motives were pure. Rather, my defence of my co-author was fuelled by my respect for

Although Baltimore's statement was accepted at face value in some quarters (e.g. in the unsigned *Nature* editorial accompanying the statement, see Footnote 22), it lacked credibility in other quarters. For instance, Paul Doty in his subsequent Commentary in *Nature* observed that "the apology, although welcome, does not erase from the record the behavior that occurred and was defended over five years and omits mention of many other actions." Baltimore had stated in particular that he was "shocked and saddened by the revelations of possible alterations and fabrication of data".[32] However the *New*

Imanishi-Kari's demonstrated abilities as a scientist, by my belief that the paper's scientific conclusions were sound, and by my trust in the efficacy of the peer review process...

Those experts [at Tufts] concluded in June 1986 that there was no evidence of deliberate falsification or misrepresentation and characterized the availability of alternative interpretations of the data as "the stuff of science". A later review at MIT reinforced that conclusion. The expert there found that O'Toole had correctly identified a minor error, but explained that the error was too insignificant to warrant a retraction in the light of "a substantial body of other data that is "clear and impressive". The MIT report echoed the sentiments of the Tufts reviewers and noted that "other issues raised by Dr. O'Toole, which are largely matters of interpretation and judgment, are best dealt with by allowing the scientific process to take its course...

In good conscience I feared a rush to judgement, and I accorded my colleague the benefit of every doubt. I now recognize that I was too willing to accept Imanishi-Kari's explanations, and to excuse discrepancies as mere sloppiness. Further I did too little to seek an independent verification of her data and conclusions. I acknowledge that, for too long, I focused narrowly on the question of whether the paper could stand...

...I am shocked and saddened by the revelations of possible alteration and fabrication of data...Science must be an objective search for truth. It was my belief in science and faith in my fellow scientists which led me to set my threshold of suspicion so high...

For their work scientists are entrusted with public funds. I have come better to appreciate the legitimate role of government as the public sponsor of scientific research and to respect its duty to protect the public interest and hold the scientific community accountable for its stewardship of public funds. Such accountability can be entirely consistent with the essential objectivity of scientific inquiry...

I have learned from this experience that the accountability to ensure the responsible use of public funds rests not only with each individual scientists but with the scientific community as a whole...

In conclusion I commend Dr. O'Toole for her courage and her determination, and I regret and apologize to her for my failure to act vigorously enough in my investigation of her doubts. I recognize that I may well have been blinded to the full implications of the mounting evidence by an excess of trust...This entire episode has reminded me of the importance of humility in the face of scientific data.

Baltimore's statement in *Nature* should also be compared with the statements of some of his supporters, such as those of Bernard Davis quoted in footnote 29. These are incompatible. Baltimore's statement was also contradicted at several points in a response by Margot O'Toole in *Nature*, 16 May 1991. Then Baltimore replied that her comments "create a misleading impression...I feel that it is necessary to demonstrate publicly that her charges lack substance". Herman Eisen also replied that he found Margot O'Toole's "extreme statements...to be inaccurate or grossly to misrepresent the true events". (*Nature* 30 May 1991)

[32]In connection with the alteration and fabrication of data, I quote from the NIH Draft Report, pp. 7 and 8, showing what Baltimore and his lawyers were aware of.

Also during this time NIH became aware for the first time that Dr. Imanishi-Kari's notebooks had not been compiled contemporaneously with the conduct of the reported experiments. Rather, the notebooks were assembled specifically to respond to the challenges to the paper. Subsequent information provided by Dr. Baltimore and his attorney, Normand Smith, Jr., indicated they were aware of Dr. Imanishi-Kari's having organized the notebooks to respond to the NIH and Congressman Dingell's subcommittee. During the April 31, 990 interview with Dr. Baltimore, Mr. Smith said a meeting was held "where Thereza came with all of her data and there was a discussion...as to whether we should just dump it on the doorstep of the committee...or should she go through her data, catalog it and put it in order and try and make it as comprehensible as possible." Mr. Smith said " I think I may have been instrumental in advising her to do the latter, which I think was, in large part, her undoing" (Interview Transcript, page 71). Mr. Smith said some

York Times article "Nobelist Apologizes for Defending Research Paper With Faulty Data" (4 May 1991) reported: "Dr. Walter Gilbert, a Nobel Laureate in molecular biology at Harvard who has criticized Dr. Baltimore's behavior in the case, said that he found the tone of the report odd and disappointing, adding: "It reminds me of that moment in the movie Casablanca, where Claude Rains stands in the bar and says, 'There is gambling going on here? I'm shocked! I'm shocked!' There is very little admission in it."

Baltimore himself subsequently negated whatever admissions he had made when he replied to Paul Doty's letter in *Nature* (5 September 1991). In that reply, Baltimore reaffirmed earlier positions putting the burden of justifying or verifying a paper not on the authors but on the rest of the scientific community, and in effect he retracted his own retraction of the paper when he said that his "science - including the Weaver *et al.* paper - is done with rigour and criticality...For the Weaver *et al.* paper, the data have proved more durable than the data in most papers...the science has stood up to the toughest test of all, the test of history." Baltimore also nullified his praise of O'Toole, when he wrote that Doty's judgment did "not depend on complete evidence" but that his "verdicts" were "based mainly on the unsubstantiated, and often refuted, allegations of one participant in events five years old." Baltimore's latest turnaround provoked Doty to question further Baltimore's credibility as follows (*Nature*, 10 October):

> Rather than replying in detail to David Baltimore's open letter to me (*Nature* 353, 9; 1991), I would suggest that interested readers compare his letter with my Commentary article (*Nature* 352, 183; 1991). The disconnection is nearly total. I stated: "Reviewing the case strictly from Baltimore's published account reveals at least four lapses...". Baltimore counters with: "...your verdicts are...based mainly on the unsubstantiated, and often refuted, allegations of one participant...". My statement is true, his is not... [*Doty gives other concrete examples.*]
>
> ...The part of my Commentary concerned with Baltimore dealt almost entirely with criticizing his behaviour and urging that it should not contribute to debasing past standards of conducting and reporting research. In his reply to me, Baltimore ignores this central theme and insists that he has always abided by the higher standards. This is the ultimate disconnection: alas it shows no sign of being bridged.

The role of Baltimore's personal credibility was especially important in his own institution. Baltimore accepted the Presidency of Rockefeller University in 1989. Even then approximately one third of the faculty, including Anthony Cerami (dean of graduate and postgraduate study), objected to his appointment publicly, and let their objections be known in the press[33]. During the next two years, two important faculty groups (one group headed by Cerami, and another group headed by

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data were in folders, some in spiral notebooks, and "there were [sic] a lot of just loose paper." According to Dr. Baltimore, Dr. Imanishi-Kari took the data home and organized it "...entirely on her own...over a weekend" (Interview Transcript, page 72). Dr. Imanishi-Kari herself acknolwedged some data were not entered into laboratory notebooks, but directly into figures, as in Figure 1. Dr. James Wyngaarden, testifying before the Dingell Subcommittee in 1989 (see below), referred to these as "unorthodox data handling practices." Based on these discoveries, the NIH decided it would reopen its investigation."

[33]For instance, in the *New York Times* 10 October 1989: "Dispute on New President Shatters Tranquil Study at Rockefeller U." The *New York Times* reporter had a "conversation...with 15 of the university's 42 full professors...All said they opposed the Baltimore candidacy, for various reasons and in varying degrees, and they added that informal polls indicate that perhaps half the full professors oppose him too." See also the NYT editorial of 12 October 1989, ending with: "Universities are still trying to devise ways of dealing with disputed research, and Dr. Baltimore did not commit the crime of the century in mishandling the inquiry into this particular case. Whether or not this one incident casts doubt on his candidacy is a matter for Rockefeller University to decide. But the trustees took a risk in extending the invitation before the dust had settled. They should not be surprised that some faculty members wish to understand the case better."

Gerald Edelman, a Nobel Laureate) left Rockefeller University, and tensions mounted. On the other hand, during that same period, the trustees repeatedly and publicly asserted their confidence in Baltimore, expressing "unconditional support" as late as 25 November 1991. In October this confidence was accompanied by a gift of $20 million from David Rockefeller. However, a number of factors having to do with Baltimore's view of his own responsibilities and the destruction of his personal credibility vis a vis his colleagues led to his resignation as President of Rockefeller University on 3 December 1991.

This resignation was extensively reported in the press[34]. The *New York Times* described the vanishing of support for Baltimore, including the tendered resignation of Rockefeller's Vice President for Academic Affairs James Darnell. The four page article in *Science* "David Baltimore's Final Days" recounted in some details the final development of an essentially unanimous faculty against his remaining as President of Rockefeller University. The article also described some of the factors which influenced the faculty, especially the exchange with Doty in *Nature*. As *Science* writes:

> ...Yet the exchange in *Nature* had a telling effect. A Rockefeller professor recently gave a glum summation of how Baltimore's reply influenced the faculty. "He even retracted his retraction...That's what made the faculty upset. They said, 'we can't support those arguments.' No one can defend this position. He was saying 'The paper still stands up as well as any other in the literature.' Do people believe that?..."

Thus finally some of the scientific grass roots reacted.

Conclusion

The circumstances of the Baltimore case are extraordinary, involving extraordinary forces and pressures. They include the courage, stamina, and clear-headedness of Margot O'Toole as well as the independence of Dan Greenberg's journalism and Stewart-Feder. They include the influence of Dingell's

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[34] I list a few reports.

New York Times 3 December 1991: "Nobelist Caught Up in Fraud Case Resigns as Head of Rockefeller U." ; and 4 December: "Science and the Stain of Scandal - Role at Rockefeller U. Fatally undermined by Fraud Case". In its articles and editorial of March 1991 the *New York Times* had failed to point to its own role in having helped Baltimore's anti-Dingell campaign. (See V, §1(c).) At that time I made my objections known to the *Times* . In the article of 3 December 1991, the *New York Times* did report more completely: "Dr. Baltimore led many scientists and others in a campaign of letter-writing, speeches and opinion pieces opposing a Congressional investigation of the paper, which appeared in many magazines and newspapers including the New York Times."

The articles of 3, 4 December were followed by an editorial "Rough Justice for Dr. Baltimore" on 5 December, ending with the statement: "But the deeper judgment is clear: The scientific community must police itself more effectively. It should not take four Congressional hearings, two university inquiries, two investigations at the National Institutes of Health and a Federal grand jury to unravel a case that could have been settled years ago by Dr. Baltimore, had he been less interested in protecting his reputation and more determined to get at the truth."

Washington Post 3 December: "Nobel Winner Quits as University Chief, Citing Role in Scientific Fraud Probe."

Wall Street Journal editorial 4 December: "Dingell Gets Baltimore".

Boston Globe 5 December: "Baltimore's legacy: concern about oversight of scientists".

Nature: a brief article "Baltimore resigns" 5 December, and an unsigned editorial "Baltimore defeat a defeat for research", 12 December;

Science 13 December: "David Baltimore's Final Days".

Dan Greenberg's *Science and Government Report* 15 December: "Baltimore Steps Down from Rockefeller Presidency". Greenberg gives a notably informative one-page-and-a-half account of the context of Baltimore's resignation, starting: " 'Dingell Gets Baltimore,' the title of a fulminating *Wall Street Journal* editorial on December 4, summarizes a widespread interpretation of Nobel laureate David Baltimore's resignation from the presidency of Rockefeller University. In reality, however, Baltimore got Baltimore..."

Subcommittee to force NIH to investigate seriously. They include the Congressional Weiss Report documenting a dozen cases of scientific misconduct or worse.[35] They include an unfavorable press against science in many newspapers and magazines, causing concern in the top science establishment about the public image of science in the country.[36] But these circumstances have illustrated and exposed problems which exist independently of these circumstances. The Baltimore case has only provided one concrete illustration of such problems. Among those problems is the way the scientific community at large exercises its responsibilities. As documented in the Baltimore case, in certain cases of challenges, some of those in power leave no alternative but to submit to authority or to escalate the challenge. The dynamics of this process are very clearly exhibited in the successive steps: the Tufts proceedings, the proceedings at MIT, the obstructions to publish via standard scientific journals, the first NIH panel, the Dingell hearings with the Secret Service forensic investigations, the OSI Draft Report, and finally the accounts in the press at large. Usually this escalation process stops early because those raising the challenge do not have the resources to engage in such an escalation, nor do the circumstances afford an opportunity for such an escalation.

What to do about the problems which have been exposed by the circumstances of the Baltimore case? Ultimately, to uphold the traditional standards of science, scientists cannot rely on authority, they cannot rely on panels, they cannot rely on big-time certifications such as those coming from Nobel Prizes or the National Academy of Sciences. They cannot count on the press and they cannot count on Congressional Committees to bring the problems of the scientific community to their own attention, or to police the scientific community. They must rely on individual responsibility, and they must create an atmosphere and conditions where scientists, both young and established, can exercise this responsibility without fear - fear of retaliation, fear for their careers, fear for their funding, fear for their publications, fear of the tensions which come from a challenge, fear of being uncollegial, whatever. Will they?[37]

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[35] 101st Congress, 2d Session, House Report 101-688; Ninenteenth Report, Committee on Government Operations, Human Resources and Intergovernmental Relations Subcommittee, Ted Weiss (New York) Chairman, September 10, 1990. See also the "Point of View" by Weiss, "Too many scientists who 'blow the whistle' end up losing their jobs and careers", *Chronicle of Higher Education* 26 June 1991 p. A-36

[36] See among others:
- *The Economist*: When science turns nasty (9 June 1990); The Baltimore Affair, Ignoble (30 March 1991); Searching for a bigger can (5 October 1991)
- TIME: Thin skins and fraud at MIT (1 April 1991)
- *The Detroit News*, editorial: Dingell: He Was Right (25 March 1991)
- *The Plain Dealer* (Cleveland) editorial: A sad case of scientific hubris (27 March 1991)
- *Wall Street Journal* , editorial: Politics and Science (29 March 1991) (pro-Baltimore, anti Dingell)
- *New York Times* articles: 21 March, 22 March, 24 March, 26 March, 31 March, 1 April, among many others
- TIME, Science under Siege: Crisis in The Labs (26 August 1991)
- *U.S. News and World Report*: The Best of America; Setting wrongs right - Congress's most feared Democrat (26 August - 2 September 1991)

[37] See MIT's *Technology Review*: "When Scientists Judge Themselves - The Misuse of Peer Review" (October 1991); and "John Dingell: Dark Knight of Science" (January 1992). These articles give evidence that the scientific community is beginning to understand better certain failures to police itself properly.

Scientific Misconduct

George S. Hammond, Distinguished Visiting Professor, Center for Photochemical Sciences

Various kinds of misconduct by scientists have been exposed (or alleged) and widely discussed in recent years. The subject is exceedingly unpleasant and scientists have, for the most part, shied away from discussing it seriously. While I do not believe that the general subject, or any particular example, should become a *cause celebre,* I think that it deserves our serious attention.

Some scientists are guilty of sundry forms of social misconduct, e.g. cruelty, embezzlement, unjustified violence, etc., which are to be found in all groups in our society. I believe that the incidence of such culpable behavior is no more, and probably less, common among scientists than among people in general. I do wish to discuss misconduct which I believe to be especially likely to occur in and do damage to science.

Some kinds of misbehavior, whether intentional or not, have potential for eroding the very fabric from which science is woven. Science is a body of knowledge and concepts built from attempts of humankind to study and understand the universe in which we exist. Science evolves as the cumulative product of the work and thoughts of people who make those attempts. It is obviously counterproductive to enter false or misleading information into the record which builds science; to do so may well constitute misconduct.

It is a common experience in science that reports appearing in the literature are later shown to be in error. In many cases the "error" is completely honest and a consequence of the ambiguities inherent in the exploratory character of research. However, if the original misleading entry was the result of careless work, over-eagerness to fit results to a particular theoretical model or laziness in writing, editing or reviewing, the act is misconduct, although perhaps only a petty offense.

The damage to science from faulty reports in the literature arises in several ways. If an erroneous communication is corrected, time, energy and money are usually required to make the correction and publish it. If it remains unchallenged it stands as a "factoid" upon which others may build their research plans. In the latter case a correction may be made, but it is at least as likely that the fault will propagate. The costs to science of these flaws in the literature may be small or large, depending on the circumstances, but they **are finite**. I think that many scientists tend to minimize the impact of such misbehavior, perhaps because we all fear that we may in some way be among the misbehavers.

I know that I have been guilty. An incident which is still a burden on my conscience occurred many years ago. A postdoctoral fellow working in my lab reported results which I found exciting, **largely because they seemed to confirm a mechanistic model which I was developing.** The work was immediately submitted and published as a communication [Hammond, G.S. and Ravve, A., **J. Am. Chem. Soc.** 73, 1891 (1951)]. Subsequently, it was shown by Benkeser and Gosnell [**J. Am. Chem. Soc.** 78, 2339 (1957)] that the results were in error. When I went to the laboratory and attempted to reproduce the work with my own hands, the results bore no resemblance to those which had been reported to me. Furthermore, when I examined Dr. Ravve's notebook, I quickly discovered that the results which he recorded were in gross disagreement with the Law of Conservation of Mass.

The incident involved two kinds of misconduct. First, I am convinced that Ravve lied about his work, a crystal clear example of scientific fraud. Second, I was irrresponsible in rushing to publish a result which I liked with essentially no scrutiny of the facts. On a micro scale the story illustrates the propagation of false science.

Of the cases which have recently surfaced and generated widespread discussion, the latest involves work in the laboratory of Professor J. O'M. Bockris at Texas A&M University. It is maintained that the transmutation of elements has been observed. (This is not a simple extension of the questionable claims for cold fusion of deuterium nuclei, but a resurrection of the alchemical goal of interconversion of heavier elements.)

Thus far, no reports of the experiments have, to my knowledge, entered the research literature, but the claims have been made by way of the public press. An aspect of the matter which has helped to attract the attention of the extra-scientific public is the decision of Bockris and the university to accept a substantial grant from an entrepreneurial investor to support the work.

One can ask what damage the episode does to science if the present scorn of most scientists muffles the noise and no report is made in the primary literature. If the reports are fraudulent, the public image of science is corroded both because of the fraud and because of the avidity with which the researcher and the university grabbed for money to support an enterprise of dubious merit. Furthermore, unless some definitive closure is brought to the case, there will be a lingering suspicion that the freedom of investigation by a scientist has been breached and that scientists narrow-mindedly reject as false results which are discordant with existent theory. Such damage is especially hurtful at a time when scientists are struggling to arrest decay of both their public image and support of their work.

I believe that, because of the noise that has been generated, scientists should insist that exceptional effort be devoted to either verify or totally discredit the Bockris alchemy. I urge that the experiments be repeated, not all over the world, but in the laboratory where they were first carried out and by the same experimenters; however, the entire process should be done under observation of a small team of scientists from outside the university. If repeated attempts fail completely to replicate any of the original claims, it should be publicly reported that the claims were a **fraud beyond any reasonable doubt**.

I realize that there is no conceivable way to subject every suspect report to such scrutiny. However, I do think that it is important that scientists and university administrators go on record as recognizing that reprehensible scientific behavior does occur and that it is **wrong**. I see a pressing need to affirm that there is a **morality of science** and that the health, perhaps even the life, of science depends upon adherence to certain moral standards.

I have touched on only one aspect of scientific misconduct. There are many others including plagiarism, theft of intellectual property revealed under terms of confidentiality, making highly exaggerated claims to support proposed research plans, exploitation by senior scientists of junior colleagues, wasteful use of scarce research funds and many others. I do not have the time, energy or space to explore them all.

I do not propose to spell out a moral code for science, nor do I consider it mandatory that it be done. Possibly some suitable scientific body should undertake the task; but my only goal at this time is to gain acceptance of the view that there are "right" and "wrong" behaviors in science.

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Dr. Alfred Bader
2961 North Shepard Avenue
Milwaukee, Wisconsin 53211

October 1, 1992

Mr. Walter Stewart
Building 8, Room B2A-15
National Institutes of Health
Bethesda, Maryland 20892

Dear Walter:

Thank you so much for sending me copies of your letters of September 23rd to Dr. Nagarkatti.

You should have become a diplomat, and you were wise not to mention my name specifically. Jai is really a very fine person, but he is in a very difficult position because he is so close to Tom Cori.

Best regards to you and Ned.

As always,

Dr. Alfred R. Bader
2961 North Shepard Avenue
Milwaukee, Wisconsin 53211

September 2, 1992

Mr. Walter Stewart
12308 Piney Glen Lane
Potomac, Maryland 20854

Dear Walter:

I hope that you received the Federal Express package I sent to you, and might even have had a chance to talk to Prof. Gassman.

Needless to say, I am anxious to know your thoughts.

Best regards.

As always,

Fax No. 414 277 0709
Telephone No. 414 277 0730

Chemical Manufacturers Association opposes new tax proposals

New taxes on oil and ozone-depleting substances and changes in the tax treatment of environmental cleanup costs "would seriously harm the U.S. chemical industry and the nation's economy and impede a jobs-producing recovery," according to the Chemical Manufacturers Association (CMA). The trade association testified last week before the House Ways & Means Committee's Subcommittee on Select Revenues, which was reviewing more than 40 miscellaneous revenue-raising proposals. CMA said it strongly opposes a proposal to increase the tariff on imported crude oil, noting that energy-intensive industries like the chemical industry would be especially hard hit. The group also objects to adding methyl bromide, hydrochlorofluorocarbons (HCFCs), and hydrobromofluorocarbons to the list of ozone-depleting chemicals—such as chlorofluorocarbons (CFCs)—that are already being taxed. One reason behind taxing CFCs in the first place, CMA points out, was to provide an incentive for users to switch to HCFCs, which are less damaging to the ozone layer, as interim substitutes. CMA also argues that industry should be allowed to continue deducting the cost of cleaning up environmental problems as a business expense.

NIH fraud busters ordered to report to new job assignments

Walter W. Stewart and Ned Feder, the NIH employees who were told in April to give up their work on scientific misconduct, have been ordered to report to new job assignments at NIH on Sept. 27. They have been on administrative leave while the Department of Health & Human Services (HHS) tried to figure out how to handle the controversial pair. In April, after a historian complained about their investigation of his alleged plagiarism, their boss at the National Institute of Diabetes, Digestive & Kidney Diseases (NIDDK) told them they had strayed too far from the agency's mission. The pair received wide support from whistleblowers and some members of Congress when Stewart went on a monthlong hunger strike in May after NIH denied them access to their files (C&EN, June 21, page 8). Since then, the men have been working out of Feder's home. Recently, the HHS Office of General Counsel has been trying to get a university to sponsor their work in an unusual arrangement under which NIH would continue to pay their salaries. A potential arrangement with the University of Illinois, Urbana-Champaign, fell through, however, when the pair refused to agree to HHS's condition that they not work on specific cases of scientific misconduct. Stewart says he and Feder will report to their new jobs in protein research and in grants management, respectively, but that he may begin another hunger strike.

Opposition gels to executive orders on pollution prevention

A coalition of industrial and business organizations has asked the chairmen of eight key House and Senate committees to look into the Clinton Administration's use of two executive orders to establish environmental policies. These two orders would mandate pollution prevention and reporting activities at federal agencies and commit agencies to using "environmentally preferable" products in the future. They are the subject of considerable concern by the chemical industry (C&EN, Sept. 6, page 24). The Synthetic Organic Chemical Manufacturers Association (SOCMA), joining the appeal to Congress, says the orders are not based on good science and they establish major public policy that goes beyond EPA and Congressional authorities. The 24 organizations in the coalition claim that the Aug. 4 executive order and Aug. 6 draft order would arbitrarily ban chemicals with no consideration of use because federal agencies would be directed to procure so-called less toxic chemicals. SOCMA says a significant number of specialty chemicals would be affected by these requirements since the burden of proof and added costs would be borne by the manufacturers of procured chemicals.

Sustainable farming practices can compete economically

Based on a series of case studies, the World Resources Institute (WRI), Washington, D.C., concludes that resource-conserving farming practices can compete economically and financially with conventional ones. Agricultural policies, however, can hinder the adoption of more sustainable farming methods and cause major fiscal and environmental losses. In the report titled "Agricultural Policy and Sustainability: Case Studies from India, Chile, the Philippines and the U.S.," WRI recommends that governments eliminate fertilizer and pesticide subsidies that encourage degradation of natural resources and that farm income support programs should be tied not to production but to stewardship of the natural resource base. Also, publicly funded research into sustainable farming practices should be given higher priority, and the definition of agricultural productivity used in cost-benefit analyses should be broadened to include environmental costs and benefits.

Government roundup

- NSF has awarded grants totaling $37.1 million to 56 colleges and universities nationwide to help modernize their research facilities. The awards range in size from $100,000 for the University of Nebraska, Kearney, for converting its general chemistry lab space to research space to $42 million for the University of Delaware to renovate its chemistry research facility.
- EPA has announced a proposed ban on the use of the stratospheric ozone depleters—hydrochlorofluorocarbons—in certain consumer and industrial products, such as aerosol spray cans and foam cushions. The agency pointed out it has no choice in the matter since Congress mandated that the products be banned by Jan. 1, 1994.
- New York Gov. Mario M. Cuomo has announced that $7.2 million will be distributed to 13 centers for advanced technology across the state. In addition, the State Science & Technology Foundation is providing $1.7 million for various high-technology projects.
- John B. Hunt, deputy director of NSF's chemistry division, has been named acting director in place of Kenneth G. Hancock, who died unexpectedly earlier this month (C&EN, Sept. 20, page 8).

File Feder

DEPARTMENT OF HEALTH & HUMAN SERVICES

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DEPARTMENT OF HEALTH & HUMAN SERVICES

DEPARTMENT OF HEALTH & HUMAN SERVICES

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[illegible]

John T. [illegible]
Department of Biochemistry and [illegible]
Harvard University, 7 Divinity Avenue
Cambridge MA 02138-2092

[illegible]

To Dr. Louis Sullivan, Secretary
Department of Health and Human Services

Dear Dr. Sullivan,

ON April [illegible] Dr. Ned Feder and Mr. Walter Stewart, at the National Institutes of Health [illegible] received an administrative order to close down their program on scientific conduct and misconduct, surrender their records, and accept specified assignments in other laboratories or administrative offices at NIH. I emphatically protest this action, which I consider to be an arbitrary exercise of administrative power, unjust as a matter of procedure and fundamentally wrong as a matter of policy.

The reason offered for this action against Feder and Stewart is their recent study, with their computer technique for the detection of plagiarism, of a biography of Abraham Lincoln by Dr. Stephen Oates, which they compared with an earlier Lincoln biography by the late Benjamin Thomas. The charge against them is that their work is [illegible] carrying them outside the domain of the NIH, which is focused on biomedical research. In fact this excursion into historical scholarship took them only about a month — a trivial fraction of the [illegible] years of work that they have devoted to problems of scientific integrity — and a significant contribution to the wider scholarly community, of which scientists are only a part. In contrast to this harsh action of suddenly closing down their research, they have recently received "Excellent" ratings on their work from the NIH [illegible] the previous [illegible] years.

Feder and Stewart have been carrying on, for several years now, their unique studies of scientific honesty and dishonesty in the doing and reporting of research. These [illegible] have led to important results in leading to the correction of some problematic [illegible] that have grown up in recent years, such as the [illegible] of the names of "honorary authors" on papers to which they contributed little or nothing (See NATURE 325 (1987) 207-214). I note that they had great difficulty, for several years, in getting this paper published because of the threat of lawsuits for libel that were threatened by some of the [illegible] they criticized. [illegible]

acquired enemies in the course of their work, including some influential people.

Feder and Stewart have supported various responsible whistle-blowers, such as Professor Robert Sprague of the University of Illinois, who suffered a grim ordeal in his ultimately successful effort to correct and expose the frauds perpetrated by Dr. Stephen Breuning. I know that Professor Sprague has already written to you with strong support for Feder and Stewart in the present action against them. He received much help from them in his painful struggle.

The NIH now proposes to reassign Feder to a rather routine administrative position in another division, which I believe will be a waste of his talents. It proposes to assign Stewart to the laboratory of Dr. William Eaton, whom I know well. Dr. Eaton is doing important research on hemoglobin, and is indeed one of the world's top authorities on the chemistry of sickle-cell hemoglobin. He called me up recently, to discuss the problems raised by the assignment of Mr. Stewart to his lab, since he found that Stewart has no interest at all in working on his (Eaton's) problems, and therefore would be more of a problem to him than a help. Altogether I conclude that this scheme, devised by NIH administrators, is a typical example of some administrative concoctions that are put forward without any real consideration of the best use of the talents of the people involved.

I think that the proposal to stop the work of Feder and Stewart and reassign them separately elsewhere, was a great mistake and ought to be rescinded. If it is not rescinded, I shall feel compelled to attack this action of NIH publicly. I would greatly regret being forced to do such a thing, since NIH is a great institution, where I have many friends, and I have happy memories of the months I spent there as a Fogarty Scholar in 1970-71. I hope that you will spare me the need of taking such action.

Yours sincerely,

John T. Edsall
Professor of Biochemistry, Emeritus
Member, National Academy of Sciences

FROM THE EDITOR

Scientific Misconduct: The End Of An Era?

NIH aide fasts in protest

Fraud investigators are told to shut office

Institute officials have said that the two would be reassigned because they had strayed from the agency's scientific mission.

Plagiarists Take Note: Machine's On Guard

In go the data; out comes evidence of wrongdoing.

By PHILIP J. HILTS

WASHINGTON

[illegible]

Barry Kaplan for The New York Times

Ned Feder, standing, and Walter Stewart at the National Institutes of Health in Bethesda, Md., where they scan scientific documents with a computer for indications of plagiarism.

[illegible]

"[illegible]," said Dr. [illegible] [illegible], [illegible] of the [illegible] [illegible], a [illegible] organization in Washington. [illegible]"

[illegible] Stewart and Dr. Feder [illegible] [illegible] [illegible] [illegible] but she does not make the same allowance for their machine. "Of the various uses modern technology would be put to, this machine is [illegible] we didn't expect. We would have expected the C.I.A. or Interpol to use it, [illegible] [illegible]."

An article in the British journal Nature fretted: "An untested misconduct machine would be dangerous at any speed. With the power to ruin careers, even a test-drive could cause [illegible]."

The pair did not begin with plagiarism, but somehow have become [illegible] [illegible] perhaps obsessed with misconduct in science. They have played [illegible] roles in half a dozen [illegible] [illegible] of misconduct, bringing re[illegible] [illegible] [illegible] from wariness to an[illegible] among other scientists.

Vindication Is Seen

The case of a Cell paper by Dr. [illegible] Imanishi-Kari that was [illegible] to have been falsified is their most recent success. Against vigorous opposition by Dr. David Baltimore, a co-author of the paper [illegible] [illegible] [illegible] Massachusetts Institute of Technology at the time, and the failure of the authorities at M.I.T. and [illegible] University to find anything wrong, Mr. Stewart and Dr. Feder championed the cause of a young whistle-blower in Dr. Imanishi-Kari's laboratory who had persistently questioned the validity of the published [illegible].

[illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] National Institutes of Health [illegible] Dr. Imanishi-Kari [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] — a [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] for Mr. Stewart and Dr. Feder. [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible].

Mr. Stewart and Dr. Feder [illegible]. Along the way, [illegible] [illegible] [illegible] [illegible] [illegible] has been [illegible].

Some critics view a 'misconduct machine' as dangerous.

Dr. [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] he thinks the two are foolish. "I have been [illegible] with them for years to get out of the [illegible] and do some science," Dr. [illegible] said. But [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] will be right."

Dr. Drummond Rennie, deputy editor of The Journal of the American Medical Association and a professor at the University of California at San Francisco, said of them, "They have a [illegible] [illegible] and have [illegible] like pit bulls. But [illegible] [illegible] [illegible] behaving in that way [illegible] people very uncomfortable and makes them [illegible]. Fundamentally, though, I think they are very good for biomedical science.

"Their basic points are correct: whistle-blowers have a rotten time in science, and everybody in science has a real responsibility for the science we turn out."

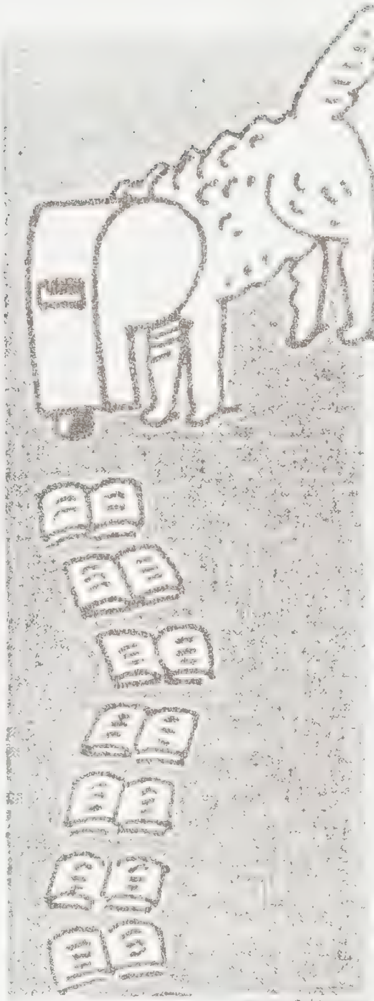
The machine devised by Mr. Stewart and Dr. Feder has already been used to settle actual charges of plagiarism. Like other forensic techniques, it can absolve a defendant as well as track down the guilty. C. Kristina Gunsalus, the research standards officer at the University of Illinois, has used the machine in two cases and [illegible] [illegible] [illegible] [illegible] [illegible].

"This is a serious tool," she said. "Humans still must make informed judgments in using it, but it makes possible things that simply could not be done without it."

She guesses, based on experience, [illegible] thinks that more than half of all cases of misconduct in science involve plagiarism. But plagiarism is much more difficult to [illegible] [illegible] might be expected.

"I might read something and [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] the same language [illegible] [illegible] in [illegible] [illegible]," Mr. Stewart said. "And even if one [illegible] [illegible] [illegible] — [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] of a pattern of plagiarism?"

Mr. Stewart said [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] comparing a [illegible] [illegible] [illegible] [illegible] [illegible] from which it was [illegible]



[illegible] Goldenberg

[illegible] plagiarised. "With this system the comparison itself would take about 2[illegible] seconds," Mr. Stewart said.

Using a combination of computer programs, some commercially available and others written by Mr. Stewart, he and Dr. Feder reckon that with clerical help, they could in a single day test a 2,000-page article for plagiarism from articles in an entire [illegible] of [illegible].

[illegible] [illegible] [illegible] [illegible] [illegible] [illegible] from [illegible] [illegible] [illegible] into a scanner that [illegible] [illegible] [illegible] [illegible] of [illegible] [illegible] [illegible] [illegible] [illegible] [illegible]. The electronic [illegible] of the text is [illegible] broken down by the computer program into strings of 30 characters each, including letters, numbers and spaces. The first string begins with the first word of the first [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] the article.

[illegible]

[illegible] Mr. Stewart said. [illegible]

[illegible]

[illegible]

[illegible]

[illegible]

[illegible]

Mr. Stewart said [illegible]

[illegible]

[illegible]

[illegible]

[illegible]

[illegible]

[illegible] sewage to avoid [illegible] [illegible]

[illegible] hanging on their [illegible] sewage to avoid [illegible]

[illegible]

[illegible]

[illegible] like a mouth [illegible]

[illegible]

[illegible] sometimes there [illegible]

[illegible]

[illegible] inferiority [illegible]

[illegible]

[illegible]

girded with [illegible]

[illegible]

[illegible] with [illegible]

[illegible]

[illegible] Cate, [illegible]

[illegible]

[illegible] that [illegible]

[illegible] the [illegible] long [illegible]

[illegible] time [illegible]

[illegible] and all the Complete, [illegible] and [illegible] Lone Grove [illegible] all of these people and a [illegible]

[illegible] sense of [illegible] along with the [illegible]

[illegible] and all the [illegible] Highlows and [illegible] all these people and [illegible]

[illegible]

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[illegible]

[illegible]

[illegible]

[illegible]

[illegible]

[illegible]

May 13, 1993

To: Alfred Bader
Voice: 414-962-5169
FAX: 414-962-8322

From: Ned and Walter
301-530-7621

Alfred: Many letters have been written on our behalf. I enclose one such letter (by John Edsall).
NF

12 pages follow (more than you may want!)

MAY 13 '93 05:25AM P.2

DEPARTMENT OF HEALTH & HUMAN SERVICES

Public Health Service
National Institutes of Health

National Institute of Diabetes and
Digestive and Kidney Diseases
Bethesda, Maryland 20892

April 9, 1993

Ned Feder, M.D.
Medical Officer (Research)
Biophysical Histology Section
Laboratory of Analytical Chemistry, NIDDK
National Institutes of Health
Building 8, Room B2A15
Bethesda, Maryland 20892

Dear Dr. Feder:

I am writing to inform you that effective May 1, 1993, the Biophysical Histology Section, Laboratory of Analytical Chemistry, will be abolished. Also effective May 1, you will be reassigned to the position of Medical Officer (Research), GS-0602-15, Review Branch, Division of Extramural Activities, National Institute of Diabetes and Digestive and Kidney Diseases (NIDDK). Mr. Walter Stewart, will be reassigned to the Laboratory of Chemical Physics, Division of Intramural Research, NIDDK.

This action is being taken because the work that you and Mr. Walter Stewart have been doing over the past several years in the area of scientific practice, including the analyses of plagiarism, has progressively moved outside the mission, responsibility and authority of the NIDDK. At a time when this Institute's personnel resources are limited, it is essential that I take action to assure that they are focused on accomplishing our numerous high-priority responsibilities for the conduct and support of biomedical research on the diseases within our mission.

Between now and May 1, your work efforts should be directed at an orderly close-out of your activities and files. Should you have any information regarding scientific misconduct, you should turn such allegations over to the Office of Research Integrity or to the Office of the Inspector General in accordance with applicable NIH and DHHS policy. I have asked two members of my immediate staff, Mr. Tom Johnson and Ms. Lynda Eckard, to work with you to assure the appropriate disposition of your files and equipment.

I will make an appointment, at a mutually convenient time, for you to meet with Dr. Walter Stolz, Director, Division of Extramural Activities, and Dr. Robert Hammond, Chief, Review

Branch, Division of Extramural Activities, NIDDK, who will be your new supervisor. I am hopeful that this reassignment will not only meet the needs of the NIDDK, but will also serve as an interesting and satisfying experience in your career.

Sincerely,

L. Earl Laurence

L. Earl Laurence
Executive Officer and
Acting Deputy Director

DEPARTMENT OF HEALTH & HUMAN SERVICES

Public Health Service

National Institutes of Health
Bethesda, Maryland 20892

12 April 1993

TO: Mr. L. Earl Laurence
Acting Deputy Director, NIDDK
National Institutes of Health

FROM: NF Ned Feder, Chief
WWS Walter W. Stewart, Research Physicist
Biophysical Histology Section, NIDDK
National Institutes of Health

SUBJECT: Termination of Section and reassignments

On Friday morning, 9 April, you handed us letters indicating that in three weeks -- by 1 May -- our Section would be abolished, that our work of ten years' standing on scientific conduct would be terminated, and that our current jobs would be abolished and we would be reassigned to new jobs. We have no prior experience, competence, or interest in the jobs to which we have been assigned. The actions were taken without any form of consultation with us and without any prior notice. Moreover, we were not told in advance about problems that required correction, nor were we afforded the opportunity to correct any problems that might exist. At the conference we were repeatedly informed orally that we had no legal right to appeal the matter.

For several years you had prepared and approved a Performance Plan for each of us; both Performance Plans specifically included our work on scientific conduct. Your ratings or approvals of our performance have consistently been "Excellent."

In addition, the NIH has shown its support by investing hundreds of thousands of dollars in our work on scientific misconduct. As recently as three weeks ago we received approval for the purchase of $9500 of computer equipment, clearly sending the message that our work was considered worthy of continuing support by NIH.

Our efforts are continuing to yield results, but they are to be terminated with three weeks' notice. The stated reasons are manifestly not the real ones. When we inquired what we should do if we see an error in a published paper, you replied that the Institute, NIDDK, did not wish us to respond as we had, namely, by pointing out the error.

When we asked why we had not received notice through our Performance Evaluations of the claimed discrepancy between the work, which has been repeatedly approved, funded, and praised, and the mission of the NIDDK, you responded that the evaluation process in our case had been "deeply flawed." But

this was an evaluation process of the Institute's own design and implementation! It does not seem reasonable to base far-reaching actions on undisclosed shortcomings in the evaluation process not of our making or responsibility.

When we asked upon what you based your recent discovery that the work you had repeatedly approved was outside the mission of NIDDK, you responded that you knew very little about our work. When we asked why, in light of your lack of knowledge, we had not been allowed to demonstrate the relevance of our work, you did not answer.

Termination of our work with three weeks' notice not only injures our professional careers, but is a flagrant waste of taxpayers' money. The NIH has supported our work on scientific misconduct for ten years. That work has produced results that are widely known and very influential. Our work is steadily producing results, but now it is to be shut down precipitously.

Why fund a project generously and then shut it down when it produces results?

The new supervisor of one of us (WWS), Dr. William Eaton, speculated on a possible answer. He said it appeared that certain administrators were attempting to shut us up.

You say that a number of unnamed officials wanted us fired, and that other unnamed officials wanted us investigated and then fired. You said that instead we were being reassigned. This circumstance, as well as others, shows this to be an adverse action thinly disguised as a reassignment.

This appears, according to the attached analysis (Appendix), to be a prohibited personnel practice. As such it would be a violation of federal law.

We feel certain we can demonstrate the relevance of our work on scientific misconduct to the mission of the NIH. We would be happy to do so either in a private conference or in a public forum. Indeed, our work on scientific misconduct generally receives wide public recognition just because of its obvious relevance to the integrity of the biomedical research process.

Several points of fairness. You told us that some officials wanted us investigated and fired, and that other officials wanted us just fired. You refused to identify these officials. This is not fair.

We were not told in advance about problems that required correction, nor were we given the opportunity to correct any problems that might exist. That is not fair.

You promised, in view of the tight deadline you had established for the elimination of our work, to give us the rules governing appeal that same day. We repeatedly emphasized that one of us (WWS) would be out of the country for two weeks starting Monday morning, 12 April. Despite repeated phone calls to your office, we have not received the rules under which we may appeal. That is not fair.

We were told that as a part of the process by which you discovered, in the last few days, that the work you had approved was outside the mission of the Institute, you used letters of complaint about us. You said that you would give us copies of these letters that same day. Despite repeated requests on our part for these letters, you have not yet furnished them. You now state that we must file a FOIA request for them. Since they evidently were a part of the process by which you allegedly discovered that your funding of our work for a period of several years was not appropriate, we believe we have a right to see the letters in order that we may respond. Indeed, we consider it a matter of basic fairness that we be allowed to respond to the letters <u>before</u> a decision is made to eliminate our Section and terminate our work.

DEPARTMENT OF HEALTH & HUMAN SERVICES Public Health Service

National Institutes of Health
Bethesda, Maryland 20892

26 April 1993

TO: **All NIDDK scientists** -- c/o Section Chiefs

FROM: Ned Feder, NF Chief, Section on Biophysical Histology
Walter W. Stewart WWS

SUBJECT: Forced reassignment: a new way of stifling dissent at NIDDK

On 9 April 1993 we were summarily handed letters stating that our work of ten years on the professional practices of scientists had just been found to have moved outside the mission of NIDDK. We were given 3 weeks (later extended to 4 weeks) to terminate our work and pack our files in boxes, which will be shipped to dead storage. We are told that on 10 May the Section on Biophysical Histology will be abolished, our computers will be reassigned, and we will be expected to report to new jobs. Ned will become a grants administrator, a job he does not want, and Walter is assigned to Bill Eaton's lab, to a job that he likewise does not want.

Our work has shown, by specific example, that famous and respected scientists can behave in professionally dishonorable ways, that dishonest science may be more common than is generally recognized, and that the whistleblower who discovers cheating in research is often punished far worse than the scientist who cheats.

The question here is not the merit or lack of merit of our work on the professional practices of scientists. NIDDK, by its actions, is maintaining the proposition that it can shuffle around scientists like so many interchangeable parts. They state this is not an adverse action, and perhaps they will be found right in the narrow legal sense. In a professional sense it is a disaster: the abrupt and unilateral termination of a scientist's work, the loss of all files and research instrumentation, and the forced reassignment to an undesired job.

Our point is that no self-respecting academic or research institution behaves like this, and neither should NIDDK. We may be the first to be stifled by this bureaucratic maneuver, but you can be sure we will not be the last.

DEPARTMENT OF HEALTH & HUMAN SERVICES

Public Health Service
National Institutes of Health

National Institute of Diabetes and
Digestive and Kidney Diseases
Bethesda, Maryland 20892

DATE: May 5, 1993

TO: Ned Feder, M.D. and Mr. Walter Stewart

FROM: Deputy Executive Officer, NIDDK

SUBJECT: Conversation of May 4, 1993

As you requested, I am writing to confirm our conversation on the above date:

- You are permitted to continue closing out your laboratory on Saturday and Sunday, May 8 and 9, 1993. This is a change in the directive you were originally given.

- You are to report to your new assignment at 8:30 a.m. on May 10, 1993, as originally directed.

- On Monday, May 10, 1993, the boxes and file cabinets you identified in the hall and storage area of Building 8 will be moved into Room B2A15, Building 8, with the rest of the files and equipment presently in the rooms. The rooms will be secured and the key maintained by the Division of Security Operations, NIH. A record will be kept of those permitted access.

- Until such time as a decision is made as to permanent disposition of the files, access will be permitted for official purposes such as legal proceedings or FOIA requests.

- It is mandatory that NIH respond to the request for documents as outlined in the note from Susan E. Sherman, The Office of the General Counsel, which I provided to you. They should be delivered to her by c.o.b. Friday, May 7.

Thomas A. Johnson

DEPARTMENT OF HEALTH & HUMAN SERVICES

Public Health Service

National Institutes of Health
Bethesda, Maryland 20892

6 May 1993

TO: Mr. Earl Laurence, Executive Officer and Acting Deputy Director, NIDDK
Dr. Phillip Gorden, Director, NIDDK

FROM: NF Ned Feder, Chief, Section on Biophysical Histology
Walter W. Stewart WWS

SUBJECT: Turning over confidential information is a breach of trust

On 9 April 1993 we received a pair of letters directing us as follows: "Should you have any information regarding scientific misconduct, you should turn such allegations over to the Office of Research Integrity or the Office of the Inspector General in accordance with applicable NIH and DHHS policy." On 30 April 1993 we received a similar instruction: "As noted in my April 9 letter, you should send any information in your files regarding alleged scientific misconduct to the Office of Research Integrity or to the Office of the Inspector General in accordance with applicable regulations."

We do in fact have information on scientific misconduct. The problem is that we received most of this information on our explicit promise that we would not turn it over to the authorities identified in the letters.

Breaching this promise would be a clear violation of the first principle of the Code of Ethics for Government Service: "Put loyalty to the highest moral principles and to country above loyalty to persons, party, or Government department" (Public Law 96-303).

We have been receiving such confidential information for several years with the explicit knowledge of our supervisor, Mr. Earl Laurence. We have on more than one occasion informed him that those supplying us with information have requested and received our promise to keep the material confidential. Those who have requested confidentiality fear that they will be harmed if the material is turned over to authorities. (We believe their fear is well founded.) We understood from our conferences with Mr. Laurence that we had a right to make and to honor such promises.

There is another problem with this directive: the amount of time we were given is entirely inadequate for the job we were assigned. We have perhaps 150 boxes of confidential information. Many of the cases are complex. It would take many months to transfer this information to another government body, and, because the information is highly technical, the recipients would have to possess or to acquire a detailed technical background.

John T. Edsall
Department of Biochemistry and Molecular Biology
Harvard University, 7 Divinity Avenue
Cambridge MA 02138-2092

May 5, 1993

To Dr. Donna Shalala, Secretary
Department of Health and Human Services

Dear Dr. Shalala,

ON April 9, Dr. Ned Feder and Mr. Walter Stewart, at the National Institutes of Health (NIDDK) received an administrative order to close down their research on scientific conduct and misconduct, surrender their records, and accept specified assignments to other laboratories or administrative offices at NIH. I emphatically protest this action, which I consider to be an arbitrary exercise of administrative power, unjust as a matter of procedure and fundamentally wrong as a matter of policy.

The excuse offered for this action against Feder and Stewart is their recent study, with their computer technique for the detection of plagiarism, of a biography of Abraham Lincoln by Dr. Stephen Oates, which they compared with an earlier Lincoln biography by the late Benjamin Thomas. The charge against them is that their work is now carrying them outside the domain of the NIH, which is limited to biomedical research. In fact this excursion into historical biography took them only about a month --- a trivial fraction of the ten years of work that they have devoted to problems of scientific honesty, and a significant contribution to the wider scholarly community, of which scientists are only a part. In contrast to this harsh action of suddenly closing down their research, they have steadily received "Excellent" ratings on their work from the NIH, until the present crisis arose.

Feder and Stewart have been carrying on, for over ten years, their unique studies of scientific honesty and dishonesty, in the doing and reporting of research. These have had an important influence in leading to the correction of some unfortunate practices that have grown up in recent years, such as the attachment of the names of "honorary authors" to papers to which they contributed little or nothing. (See NATURE 325 (1987) 207-214). I note that they had great difficulty, for several years, in getting this paper published, because of the threats of lawsuits by people who felt themselves threatened by some of the facts they recorded. Inevitably they have

acquired enemies in the course of their work, including some influential people.

Feder and Stewart have supported various responsible whistle blowers, such as Professor Robert Sprague of the University of Illinois, who suffered a grim ordeal in his ultimately successful effort to correct and expose the frauds perpetrated by Dr. Stephen Breuning. I know that Professor Sprague has already written to you with strong support for Feder and Stewart in the present action against them. He received much help from them in his painful struggle.

The NIH now proposes to reassign Feder to a rather routine administrative position in another division, which I believe will be a waste of his talents. It proposes to assign Stewart to the laboratory of Dr. William Eaton, whom I know well. Dr. Eaton is doing important research on hemoglobin, and is indeed one of the world's top authorities on the chemistry of sickle-cell hemoglobin. He called me up recently, to discuss the problems raised by the assignment of Mr. Stewart to his lab, since he found that Stewart has no interest at all in working on his (Eaton's) problems, and therefore would be more of a problem to him than a help. Altogether I conclude that this scheme, devised by NIH administrators, is a typical example of some administrative concoctions that are put forward without any real consideration of the best use of the talents of the people involved.

I think that the proposal to stop the work of Feder and Stewart, and reassign them separately elsewhere, was a great mistake and ought to be rescinded. If it is not rescinded, I shall feel compelled to attack this action of NIH publicly. I would greatly regret being forced to do such a thing, since NIH is a great institution, where I have many friends, and I have happy memories of the months I spent there as a Fogarty Scholar in 1970-71. I hope that you will spare me the need of taking such action.

Yours sincerely,

John T. Edsall
Professor of Biochemistry Emeritus
Member, National Academy of Sciences

[This letter was sent to Dr. Shalala via Federal Express on May 5.]

FROM THE EDITOR

Scientific Misconduct: The End Of An Era?

"Some people will say, 'Why now?' Others will say 'Why not earlier?'"

Earl Laurence, acting deputy director of the National Institute of Diabetes and Digestive and Kidney Diseases (NIDDK), states the two most obvious questions about his recent decision to reassign Walter Stewart and Ned Feder within NIDDK to a physical chemistry lab and an extramural-grants-review position, respectively. As of May 7, Stewart and Feder, best known for their investigations of scientific misconduct, will no longer be funded by NIDDK to use their "plagiarism machine" to search the literature for instances of plagiarism or otherwise investigate alleged misconduct.

Stewart and Feder are not happy about the reassignment. They view it as an "adverse personnel action"—NIH bureaucratese meaning that the action is punitive and therefore subject to appeal. Laurence says the reassignments are clearly not adverse actions; both Stewart and Feder are being maintained at their previous grade, status, and salary.

In his April 9 letter to Stewart, Laurence wrote, "…the work that you and Dr. Ned Feder have been doing…has progressively moved outside the mission, responsibility, and authority of the NIDDK." Laurence did not cite the reason now widely regarded as the trigger for his action, that Stewart and Feder had investigated the writings of Stephen Oates, a University of Massachusetts historian, who was not receiving any federal funds.

Stewart and Feder did not seek out the Oates case. Five historians had previously accused Oates of plagiarism, and in 1991 the American Historical Association (AHA) investigated their claims. AHA found that Oates failed to give a key Lincoln biographer "sufficient attribution," but AHA did not accuse Oates of plagiarism. Oates threatened to sue one of his accusers, who then asked Stewart for help. So Stewart and Feder used their computer to search four of Oates' books—on Abraham Lincoln, William Faulkner, and Martin Luther King—for instances of plagiarism. In February, they reported more than 400 such instances to AHA, and also explained the limitations of their analysis. Oates repeatedly and vigorously denied all allegations of plagiarism.

Stewart and Feder say that they had had several discussions with Laurence about the Oates investigation and that Laurence, their NIDDK supervisor, offered no objection or specific caution. Laurence says he cannot remember when he first learned about the case.

It is reasonable to argue that Stewart and Feder should not have used NIDDK resources for investigating a non–federally funded historian. The most important question, however, is whether Stewart and Feder, in their 10-year-long drive to ferret out scientific misconduct, have contributed to science. It is clear that they have. They listened to whistleblowers when no one else would. They raised the consciousness of all scientists about the problem of misconduct. And they repeatedly urged scientists to do a better job of policing themselves.

A larger issue now is whether an efficient and fair system currently exists for investigating allegations of misconduct in biomedical research. Last year, the Office of Research Integrity (ORI) replaced NIH's Office of Scientific Integrity. Stewart and Feder operated outside that system, a maverick position that Stewart justifies as essential because of the freedom and lack of bias it afforded.

It no longer seems appropriate for an individual NIH institute to support researchers to investigate allegations of misconduct anywhere in the sphere of federally funded biomedical research. But if such investigations contribute to science in ways that ORI cannot, then funds should be found elsewhere in the Public Health Service to support the effort.

Deborah M. Barnes

—DEBORAH M. BARNES

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THE JOURNAL OF NIH RESEARCH
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Boston Globe, May 12, 1993, page 3

NIH aide fasts in protest

Fraud investigators are told to shut office

By Peter G. Gosselin
GLOBE STAFF

WASHINGTON – A federal researcher who gained national prominence for his investigations of scientific fraud has begun a hunger strike to protest a decision by superiors to close his office and lock up his files.

Walter Stewart, a scientist with the National Institutes of Health, said yesterday that he stopped eating after officials ordered him and a colleague, Ned Feder, out of their office on the institute's Bethesda, Md., campus Monday and changed the locks.

Institute officials have said that the two would be reassigned because they had strayed from the agency's scientific mission by leveling charges of plagiarism against a University of Massachusetts historian.

NIH's move to end the pair's work on misconduct and Stewart's decision to fast set up a strange confrontation, and opens the latest chapter in the men's decade-long career as scientific gadflies.

Since the early 1980s, Stewart and Feder have exposed a string of frauds that have shaken the confidence of many people in the honesty of federally funded research.

They were instrumental in investigating the case of Harvard cardiologist John Darsee, who was shown to have faked research data, and in proving that key evidence for a scientific paper written by Nobel laureate David Baltimore of the Massachusetts Institute of Technology and Thereza Imanishi-Kari of Tufts University had been forged.

GLOBE PHOTO PAM PRICE

NIH colleagues Walter Stewart (left) and Ned Feder talk Monday while a worker changes the locks on their laboratory.

Institute officials have said that the two would be reassigned because they had strayed from the agency's scientific mission.

The pair's current troubles began when they developed a computer program to uncover what they described as plagiarism in a biography of Abraham Lincoln, "With Malice Toward None," by Stephen B. Oates, a UMass historian.

Oates vehemently denied any wrongdoing and complained to, among others, Sen. Paul Simon, an Illinois Democrat who has written about Lincoln. In a March 17 letter to NIH officials, Simon labeled Stewart and Feder's allegations against Oates "baseless" and demanded to know why they had been permitted to use agency money to study the issue.

L. Earl Laurence, acting director of the institute where the two men work, said that he knew of the Simon letter when he ended the pair's work on misconduct and reassigned them, but said that he had reached his decision independently.

Laurence originally ordered that the researchers' records be seized and sent to storage. He relented after Rep. John D. Dingell of Michigan, who has conducted high-profile hearings on scientific fraud, questioned whether the order was "an overreaction to an isolated incident."

Stewart said he and Feder first focused on the Oates book as a means of testing their computer program for uncovering plagiarism and only later became embroiled in the controversy over it. The two have filed a complaint with the American Historical Association, alleging that Oates wrongly copied hundreds of phrases from an earlier biography of Lincoln.

Stewart said that the pair kept Laurence and other NIH officials apprised of their work and received permission to proceed. He said that he was fasting to protest the NIH's decision to lock up his records, which he said contain information that is "critical" to proving new cases of scientific fraud and which, in many instances, was provided by whistle-blowers.

Despite infuriating many researchers, Stewart and Feder also have attracted considerable support among scientists. John T. Edsall, a Harvard biochemistry professor and member of the National Academy of Sciences, has written NIH officials that their decision to stop the pair's work is a "great mistake and ought to be rescinded."

New York Times
Jan. 7, 1992
p. C1, C9

Plagiarists Take Note: Machine's On Guard

In go the data; out comes evidence of wrongdoing.

By PHILIP J. HILTS

WASHINGTON

FEW figures in science have engendered more emotion than Walter Stewart and Dr. Ned Feder — and that was before they invented their little "plagiarism machine."

"You put the papers in here," Mr. Stewart said as he bent forward and peered through thick glasses bound to his head by a rubber band. The scanner digests the paper, transforming it into a computer file ready for the test. "It can look at two documents, or compare one paper to a whole field of papers, and it boldfaces text whenever 30 characters or more are identical," he said.

In principle, the entire literature of science could be scanned for plagiarism with this device, Mr. Stewart said.

But it seems unlikely that anyone would be willing to spend the time or money to do that. Rather, the machine's use most likely will be the one to which it has already been put: when plagiarism is suspected, the machine can compare the work of one author with the rest of the literature in his field for any instance of copying.

Mr. Stewart and Dr. Feder work a stone's throw from the office of the director of the National Institutes of Health, but she works on the carpeted upper floors and they in a subbasement room at the campus in Bethesda, Md. They have two narrow rooms once filled with thousands of bottles of snails.

In one, a dozen pieces of computer equipment and several screens hum and blink, while the next room is a throwback to an earlier age. It contains paper, ceiling-high stacks of paper, tidily arranged in file folders, which in turn are arranged in boxes. Here, they have have just begun to read scientific articles into the machine, inserting perhaps 7,000 articles and books from two subfields of science so far.

Plagiarism, the appropriation of another author's words or ideas, is a much despised crime in the academic world, where intellectual property is the basis of advancement. The word-for-word copying of another researcher's articles might seem the least likely form of plagiarism because the theft, once detected, catches the perpetrator red-faced and red-handed. But along with the other forms of fraud that have surfaced in science have been several startling cases of plagiarism.

Any device that helped detect or deter such a blot on science might seem to deserve the heartfelt support of scientific leaders. But the plagiarism machine developed by Mr. Stewart and Dr. Feder has not received a rapturous welcome so far.

Marty Katz for The New York Times

Ned Feder, standing, and Walter Stewart at the National Institutes of Health in Bethesda, Md., where they scan scientific documents with a computer for indications of plagiarism.

"I find it chilling," said Dr. Maxine Singer, president of the Carnegie Institution, a research organization in Washington. "We don't normally in our society go looking for behavior not consistent with accepted practices. The whole system is designed to protect people. I don't know why in science we have to do these more threatening kinds of things."

Mr. Stewart and Dr. Feder "may be well-intentioned," Dr. Singer said, but she does not make the same allowance for their machine. "Of the various uses modern technology would be put to, this machine is one we didn't expect. We would have expected the C.I.A. or Interpol to use it, not scientists."

An article in the British journal Nature fretted: "An untested misconduct machine would be dangerous at any speed. With the power to ruin careers, even a test-drive could cause disaster."

The pair did not begin with plagiarism, but somehow have become fascinated, perhaps obsessed, with misconduct in science. They have played significant roles in half a dozen major cases of misconduct, bringing reactions varying from wariness to anger among other scientists.

Vindication Is Seen

The case of a Cell paper by Dr. Thereza Imanishi-Kari that was judged to have been falsified is their most recent success. Against vigorous opposition by Dr. David Baltimore, a co-author of the paper who was at the Massachusetts Institute of Technology at the time, and the failure of the authorities at M.I.T. and Tufts University to find anything wrong, Mr. Stewart and Dr. Feder championed the cause of a young whistle-blower in Dr. Imanishi-Kari's laboratory who had persistently questioned the validity of the published article.

The recent finding by a committee of the National Institutes of Health that Dr. Imanishi-Kari had indeed misrepresented data in the paper — a charge she continues to deny — was seen as a vindication for Mr. Stewart and Dr. Feder. It was also a factor in Dr. Baltimore's recent resignation as president of Rockefeller University.

Mr. Stewart and Dr. Feder are now preparing to testify in a court case involving a 6,000-page manual of plastic surgery, which is reported to contain scores of pages copied word for word from a leading textbook, "Reconstructive Plastic Surgery" (W. B. Saunders), edited by Dr. John M. Converse. They have also played roles in many other cases that have not reached the public eye. Along the way, their own research in science has been shelved.

Some critics view a 'misconduct machine' as dangerous.

Dr. J. Edward Rall, who until recently was their boss as deputy director of research at the Institutes, says he thinks the two are foolish. "I have been expostulating with them for years to get out of the gutter and do some science," Dr. Rall said. But then, he said, he has been around long enough to know that a young man with an idea that seems crazy could well be right.

Dr. Drummond Rennie, deputy editor of The Journal of the American Medical Association and a professor at the University of California at San Francisco, said of them, "They have a burning cause and have become like pit bulls. But having a cause, behaving in that way, makes people very uncomfortable and makes them loathed. Fundamentally, though, I think they are very good for biomedical science.

"Their basic points are correct: whistle-blowers have a rotten time in science, and everybody in science has a real responsibility for the science we turn out."

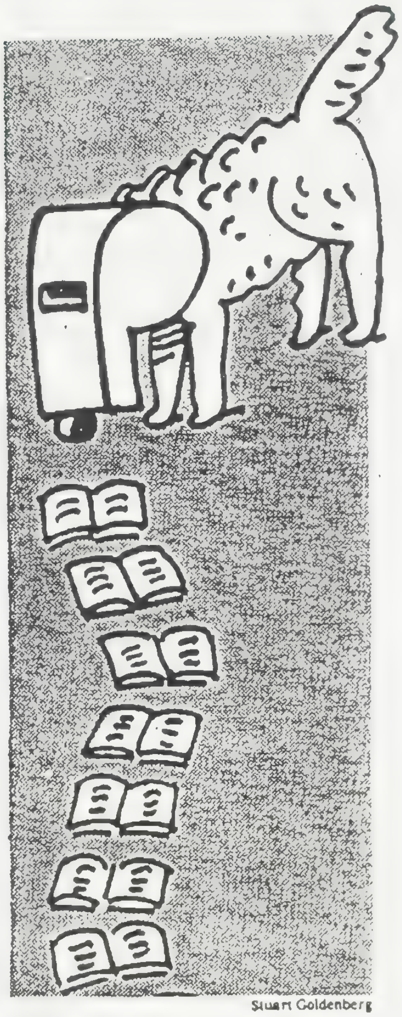
The machine devised by Mr. Stewart and Dr. Feder has already been used to settle actual charges of plagiarism. Like other forensic techniques, it can absolve a defendant as well as track down the guilty. C. Kristina Gunsalus, the research standards officer at the University of Illinois, has used the machine in two cases and does not find it worrisome.

"This is a serious tool," she said. "Humans still must make important judgments in using it, but it makes possible things that simply could not be done without it."

She guesses, based on experiences at Illinois, that more than half of all cases of misconduct in science involve plagiarism. But plagiarism is much more difficult to detect than might be expected.

"I might read something and months later remember the facts, but I wouldn't be likely to remember that precisely the same language was used in some passages," Mr. Stewart said. "And even if you do find one instance — was that one a mistake or bad judgment one time, or is it a part of a pattern of plagiary?"

Mr. Stewart said he knew of a person who had spent an entire month comparing a book chapter with the material from which it was supposedly plagiarised. "With this system, the comparison itself would take about 20 seconds," Mr. Stewart said.



Stuart Goldenberg

Using a combination of computer programs, some commercially available and others written by Mr. Stewart, he and Dr. Feder reckon that, with clerical help, they could in a single day test a 2,000-page article for plagiarism from articles in an entire subfield of science.

Their method is to feed pages from the suspect article into a scanner that can read a variety of typefaces and convert them into electronic form. The electronic version of the text is then broken down by the computer program into strings of 30 characters each, including letters, numbers and spaces. The first string begins with the first word of the first paragraph, the second begins with the second word, and so forth, building overlapping strings throughout the article.

To compare all the strings in one text with all the strings in the rest of a field's scientific literature would take an inordinate amount of computer time. Instead, the program sorts all the strings in a computer equivalent of alphabetical order, thus putting identical pairs next to each other, which the computer then prints in boldface.

After doing thousands of such runs, Mr. Stewart said: "The most surprising thing is how unique human language is. We find very, very few duplicates — even in highly technical text talking about the same thing." As it turns out, in the 7,000 or so manuscripts he has looked at so far, Mr. Stewart has found that only about one string in 200 may be duplicated by chance alone. That rate is about five "millifreemans" in the units of plagiarism created by Dr. Feder and Mr. Stewart.

The basic unit, one freeman, "refers to the ultimate case, the theft of an entire document word for word, changing only the author's name," said Mr. Stewart. The unit is named after an individual whom he regards as having committed large-scale plagiarism. Attempts to reach the individual by telephone were unavailing.

Mr. Stewart says that at the level of 10 millifreemans and above, "there is serious reason to look at two documents to see if there is plagiary or the identical passages have been properly attributed."

As the two men have worked on cases of plagiarism in two subfields of science, they have noted in passing half a dozen cases of likely plagiarism that have gone unnoticed in the two fields. They do not have time to pursue them now, they say.

After much experience with plagiarism, they have learned at least two facts: "That plagiarism is rare; and that people who copy do so from obscure places and chiefly from dead authors," Mr. Stewart said, adding sotto voce, "There is something specially disturbing about that, isn't there?"

A Future Task

Mr. Stewart and Dr. Feder said they hoped, apart from examining the papers in occasional cases of misconduct, to use the machine to document the general practices of citing others' work in science. When scientists use the material of others, how fully do they give credit? Do they use quotations or paraphrases? What is considered adequate credit?

Remarkably enough, in a profession that feeds on data, very little data have been gathered about the behavior of scientists themselves. "Fraud contaminates the literature, and a climate in which people do not deal frankly and fully with errors is inimical to the true values of science," Mr. Stewart said. "We need mechanisms to find out what the facts are."

Crazy as it may seem, he says, the plagiarism machine could be one of them.

A Comparison of Passages by Oates and Earlier Authors

Passages written by Stephen Oates are in the right column.
Phrases identical or nearly so in the two columns are **boldfaced**.

Lincoln

Lolling on the low deck, giving an occasional **tug on the slender sweeps to avoid the snags and sandbars** .... (Thomas, 1952, p. 17)

At last they came to the tumultuous Mississippi and headed southward in its tempestuous currents, **tugging on their slender sweeps to avoid snags and sandbars**.... (Oates, 1977, p. 14)

Lincoln awkwardly dished out the oysters. (Thomas, 1952, p. 453)

... **Lincoln awkwardly dished out** fried oysters to everyone. (Oates, 1977, p. 401)

Sherman's boys hit South Carolina **like a horde of avenging** Goths. (Thomas, 1952, p. 505)

In February, Sherman's army stormed into South Carolina **like a horde of avenging** angels.... (Oates, 1977, p. 415)

Martin Luther King

Up to 25 profanity-laced telephone calls a day came to the King home. **Sometimes there was only the hawk of a throat** and the splash of spittle against the ear piece. (_Time_ magazine, Feb. 18, 1957, p. 19)

Then there were the obscene phone calls -- as many as twenty-five a day now. **Sometimes there was only the hawk of a throat**, the sound of spit against the receiver. (Oates, 1982, p. 83-84)

I must return to the ... valley filled with misguided, bloodthirsty mobs, but a valley filled at the same time with little Negro boys and girls who grow up with ominous **clouds of inferiority** formed in their **little mental skies**.... (M. L. King, quoted by Miller, 1968, p. 206-207)

King took his daughter there [to a formerly segregated amusement park] for a day of cotton candy and whirling rides, and clouds of uninhibited delight now replaced **clouds of inferiority** in her **little mental skies**. (Oates, 1982, p. 269)

Faulkner

...the Warner lot -- 135 acres, **girded with walls like a** medieval city. (Blotner, 1974, 1117)

...Warner's Burbank studio, **girded with walls like a** prison. (Oates, 1987, p. 190)

Albert Isaac Bezzerides was a strong, **dark, massive man** of thirty-six. (Blotner, 1974, p. 1130)

...A. I. "Buzz" Bezzerides, a **dark, massive man** of Turkish birth.... (Oates, 1987, p. 190)

The time was 1924 and the place was Oxford, Mississippi, where Billy [Faulkner] **had just lost the only job he had ever held for any length of time.** (Blotner, 1974, p. 376)

And he [Faulkner] was penniless -- **had just lost the only job he had held for any length of time** -- and was trying to be a writer. (Oates, 1987, p. 41)

And I am **deep in memory**, as if summoned there by a trumpet blast. **Dilsey and Benjy and Luster and all the Compsons, Hightower and Byron Bunch and Flem Snopes and the gentle Lena Grove -- all of these people and a score of others come swarming back** comically and villainously and tragically in my mind with a kind of mnemonic **sense of utter reality, along with the tumultuous landscape and the fierce and tender weather, and the whole maddened, miraculous vision of life** wrested, as all art is wrested, out of nothingness. (Styron, 1973, 262)

In the funeral procession, novelist William Styron found himself **deep in memory**, as **Dilsey and Benjy and all the Compsons, Hightower and Byron Bunch and Flem Snopes and the gentle Lena Grove, all these people and scores of others came swarming back** in Styron's mind with a **sense of utter reality, along with the tumultuous landscape, the fierce and gentle weather,** and **the whole "maddened, miraculous vision of life"** that had created them. (Oates, 1987, 321)

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27 April 1993
WWS, NF

May 13, 1993

To: Alfred Bader
Voice: 414-962-5169
FAX: 414-962-8322

From: Ned and Walter
301-530-7621

Alfred: Many letters have been written on our behalf. I enclose one such letter (by John Edsall).
NF

12 pages follow (more than you may want!)

DEPARTMENT OF HEALTH & HUMAN SERVICES

Public Health Service
National Institutes of Health

National Institute of Diabetes and
Digestive and Kidney Diseases
Bethesda, Maryland 20892

April 9, 1993

Ned Feder, M.D.
Medical Officer (Research)
Biophysical Histology Section
Laboratory of Analytical Chemistry, NIDDK
National Institutes of Health
Building 8, Room B2A15
Bethesda, Maryland 20892

Dear Dr. Feder:

I am writing to inform you that effective May 1, 1993, the Biophysical Histology Section, Laboratory of Analytical Chemistry, will be abolished. Also effective May 1, you will be reassigned to the position of Medical Officer (Research), GS-0602-15, Review Branch, Division of Extramural Activities, National Institute of Diabetes and Digestive and Kidney Diseases (NIDDK). Mr. Walter Stewart, will be reassigned to the Laboratory of Chemical Physics, Division of Intramural Research, NIDDK.

This action is being taken because the work that you and Mr. Walter Stewart have been doing over the past several years in the area of scientific practice, including the analyses of plagiarism, has progressively moved outside the mission, responsibility and authority of the NIDDK. At a time when this Institute's personnel resources are limited, it is essential that I take action to assure that they are focused on accomplishing our numerous high-priority responsibilities for the conduct and support of biomedical research on the diseases within our mission.

Between now and May 1, your work efforts should be directed at an orderly close-out of your activities and files. Should you have any information regarding scientific misconduct, you should turn such allegations over to the Office of Research Integrity or to the Office of the Inspector General in accordance with applicable NIH and DHHS policy. I have asked two members of my immediate staff, Mr. Tom Johnson and Ms. Lynda Eckard, to work with you to assure the appropriate disposition of your files and equipment.

I will make an appointment, at a mutually convenient time, for you to meet with Dr. Walter Stolz, Director, Division of Extramural Activities, and Dr. Robert Hammond, Chief, Review

Branch, Division of Extramural Activities, NIDDK, who will be your new supervisor. I am hopeful that this reassignment will not only meet the needs of the NIDDK, but will also serve as an interesting and satisfying experience in your career.

Sincerely,

L. Earl Laurence

L. Earl Laurence
Executive Officer and
Acting Deputy Director

DEPARTMENT OF HEALTH & HUMAN SERVICES

Public Health Service

National Institutes of Health
Bethesda, Maryland 20892

12 April 1993

TO: Mr. L. Earl Laurence
Acting Deputy Director, NIDDK
National Institutes of Health

FROM: NF Ned Feder, Chief
WWS Walter W. Stewart, Research Physicist
Biophysical Histology Section, NIDDK
National Institutes of Health

SUBJECT: Termination of Section and reassignments

On Friday morning, 9 April, you handed us letters indicating that in three weeks -- by 1 May -- our Section would be abolished, that our work of ten years' standing on scientific conduct would be terminated, and that our current jobs would be abolished and we would be reassigned to new jobs. We have no prior experience, competence, or interest in the jobs to which we have been assigned. The actions were taken without any form of consultation with us and without any prior notice. Moreover, we were not told in advance about problems that required correction, nor were we afforded the opportunity to correct any problems that might exist. At the conference we were repeatedly informed orally that we had no legal right to appeal the matter.

For several years you had prepared and approved a Performance Plan for each of us; both Performance Plans specifically included our work on scientific conduct. Your ratings or approvals of our performance have consistently been "Excellent."

In addition, the NIH has shown its support by investing hundreds of thousands of dollars in our work on scientific misconduct. As recently as three weeks ago we received approval for the purchase of $9500 of computer equipment, clearly sending the message that our work was considered worthy of continuing support by NIH.

Our efforts are continuing to yield results, but they are to be terminated with three weeks' notice. The stated reasons are manifestly not the real ones. When we inquired what we should do if we see an error in a published paper, you replied that the Institute, NIDDK, did not wish us to respond as we had, namely, by pointing out the error.

When we asked why we had not received notice through our Performance Evaluations of the claimed discrepancy between the work, which has been repeatedly approved, funded, and praised, and the mission of the NIDDK, you responded that the evaluation process in our case had been "deeply flawed." But

this was an evaluation process of the Institute's own design and implementation! It does not seem reasonable to base far-reaching actions on undisclosed shortcomings in the evaluation process not of our making or responsibility.

When we asked upon what you based your recent discovery that the work you had repeatedly approved was outside the mission of NIDDK, you responded that you knew very little about our work. When we asked why, in light of your lack of knowledge, we had not been allowed to demonstrate the relevance of our work, you did not answer.

Termination of our work with three weeks' notice not only injures our professional careers, but is a flagrant waste of taxpayers' money. The NIH has supported our work on scientific misconduct for ten years. That work has produced results that are widely known and very influential. Our work is steadily producing results, but now it is to be shut down precipitously.

Why fund a project generously and then shut it down when it produces results?

The new supervisor of one of us (WWS), Dr. William Eaton, speculated on a possible answer. He said it appeared that certain administrators were attempting to shut us up.

You say that a number of unnamed officials wanted us fired, and that other unnamed officials wanted us investigated and then fired. You said that instead we were being reassigned. This circumstance, as well as others, shows this to be an adverse action thinly disguised as a reassignment.

This appears, according to the attached analysis (Appendix), to be a prohibited personnel practice. As such it would be a violation of federal law.

We feel certain we can demonstrate the relevance of our work on scientific misconduct to the mission of the NIH. We would be happy to do so either in a private conference or in a public forum. Indeed, our work on scientific misconduct generally receives wide public recognition just because of its obvious relevance to the integrity of the biomedical research process.

Several points of fairness. You told us that some officials wanted us investigated and fired, and that other officials wanted us just fired. You refused to identify these officials. This is not fair.

We were not told in advance about problems that required correction, nor were we given the opportunity to correct any problems that might exist. That is not fair.

You promised, in view of the tight deadline you had established for the elimination of our work, to give us the rules governing appeal that same day. We repeatedly emphasized that one of us (WWS) would be out of the country for two weeks starting Monday morning, 12 April. Despite repeated phone calls to your office, we have not received the rules under which we may appeal. That is not fair.

We were told that as a part of the process by which you discovered, in the last few days, that the work you had approved was outside the mission of the Institute, you used letters of complaint about us. You said that you would give us copies of these letters that same day. Despite repeated requests on our part for these letters, you have not yet furnished them. You now state that we must file a FOIA request for them. Since they evidently were a part of the process by which you allegedly discovered that your funding of our work for a period of several years was not appropriate, we believe we have a right to see the letters in order that we may respond. Indeed, we consider it a matter of basic fairness that we be allowed to respond to the letters <u>before</u> a decision is made to eliminate our Section and terminate our work.

DEPARTMENT OF HEALTH & HUMAN SERVICES

Public Health Service

National Institutes of Health
Bethesda, Maryland 20892

26 April 1993

TO: **All NIDDK scientists** -- c/o Section Chiefs

FROM: Ned Feder, NF Chief, Section on Biophysical Histology
Walter W. Stewart WWS

SUBJECT: Forced reassignment: a new way of stifling dissent at NIDDK

On 9 April 1993 we were summarily handed letters stating that our work of ten years on the professional practices of scientists had just been found to have moved outside the mission of NIDDK. We were given 3 weeks (later extended to 4 weeks) to terminate our work and pack our files in boxes, which will be shipped to dead storage. We are told that on 10 May the Section on Biophysical Histology will be abolished, our computers will be reassigned, and we will be expected to report to new jobs. Ned will become a grants administrator, a job he does not want, and Walter is assigned to Bill Eaton's lab, to a job that he likewise does not want.

Our work has shown, by specific example, that famous and respected scientists can behave in professionally dishonorable ways, that dishonest science may be more common than is generally recognized, and that the whistleblower who discovers cheating in research is often punished far worse than the scientist who cheats.

The question here is not the merit or lack of merit of our work on the professional practices of scientists. NIDDK, by its actions, is maintaining the proposition that it can shuffle around scientists like so many interchangeable parts. They state this is not an adverse action, and perhaps they will be found right in the narrow legal sense. In a professional sense it is a disaster: the abrupt and unilateral termination of a scientist's work, the loss of all files and research instrumentation, and the forced reassignment to an undesired job.

Our point is that no self-respecting academic or research institution behaves like this, and neither should NIDDK. We may be the first to be stifled by this bureaucratic maneuver, but you can be sure we will not be the last.

DEPARTMENT OF HEALTH & HUMAN SERVICES

Public Health Service
National Institutes of Health

National Institute of Diabetes and
Digestive and Kidney Diseases
Bethesda, Maryland 20892

DATE: May 5, 1993

TO: Ned Feder, M.D. and Mr. Walter Stewart

FROM: Deputy Executive Officer, NIDDK

SUBJECT: Conversation of May 4, 1993

As you requested, I am writing to confirm our conversation on the above date:

- You are permitted to continue closing out your laboratory on Saturday and Sunday, May 8 and 9, 1993. This is a change in the directive you were originally given.

- You are to report to your new assignment at 8:30 a.m. on May 10, 1993, as originally directed.

- On Monday, May 10, 1993, the boxes and file cabinets you identified in the hall and storage area of Building 8 will be moved into Room B2A15, Building 8, with the rest of the files and equipment presently in the rooms. The rooms will be secured and the key maintained by the Division of Security Operations, NIH. A record will be kept of those permitted access.

- Until such time as a decision is made as to permanent disposition of the files, access will be permitted for official purposes such as legal proceedings or FOIA requests.

- It is mandatory that NIH respond to the request for documents as outlined in the note from Susan E. Sherman, The Office of the General Counsel, which I provided to you. They should be delivered to her by c.o.b. Friday, May 7.

Tom

Thomas A. Johnson

DEPARTMENT OF HEALTH & HUMAN SERVICES

Public Health Service

National Institutes of Health
Bethesda, Maryland 20892

6 May 1993

TO: Mr. Earl Laurence, Executive Officer and Acting Deputy Director, NIDDK
Dr. Phillip Gorden, Director, NIDDK

FROM: NF Ned Feder, Chief, Section on Biophysical Histology
Walter W. Stewart WWS

SUBJECT: Turning over confidential information is a breach of trust

On 9 April 1993 we received a pair of letters directing us as follows: "Should you have any information regarding scientific misconduct, you should turn such allegations over to the Office of Research Integrity or the Office of the Inspector General in accordance with applicable NIH and DHHS policy." On 30 April 1993 we received a similar instruction: "As noted in my April 9 letter, you should send any information in your files regarding alleged scientific misconduct to the Office of Research Integrity or to the Office of the Inspector General in accordance with applicable regulations."

We do in fact have information on scientific misconduct. The problem is that we received most of this information on our explicit promise that we would not turn it over to the authorities identified in the letters.

Breaching this promise would be a clear violation of the first principle of the Code of Ethics for Government Service: "Put loyalty to the highest moral principles and to country above loyalty to persons, party, or Government department" (Public Law 96-303).

We have been receiving such confidential information for several years with the explicit knowledge of our supervisor, Mr. Earl Laurence. We have on more than one occasion informed him that those supplying us with information have requested and received our promise to keep the material confidential. Those who have requested confidentiality fear that they will be harmed if the material is turned over to authorities. (We believe their fear is well founded.) We understood from our conferences with Mr. Laurence that we had a right to make and to honor such promises.

There is another problem with this directive: the amount of time we were given is entirely inadequate for the job we were assigned. We have perhaps 150 boxes of confidential information. Many of the cases are complex. It would take many months to transfer this information to another government body, and, because the information is highly technical, the recipients would have to possess or to acquire a detailed technical background.

John T. Edsall
Department of Biochemistry and Molecular Biology
Harvard University, 7 Divinity Avenue
Cambridge MA 02138-2092

May 5, 1993

To Dr. Donna Shalala, Secretary
Department of Health and Human Services

Dear Dr. Shalala,

ON April 9, Dr. Ned Feder and Mr. Walter Stewart, at the National Institutes of Health (NIDDK) received an administrative order to close down their research on scientific conduct and misconduct, surrender their records, and accept specified assignments to other laboratories or administrative offices at NIH. I emphatically protest this action, which I consider to be an arbitrary exercise of administrative power, unjust as a matter of procedure and fundamentally wrong as a matter of policy.

The excuse offered for this action against Feder and Stewart is their recent study, with their computer technique for the detection of plagiarism, of a biography of Abraham Lincoln by Dr. Stephen Oates, which they compared with an earlier Lincoln biography by the late Benjamin Thomas. The charge against them is that their work is now carrying them outside the domain of the NIH, which is limited to biomedical research. In fact this excursion into historical biography took them only about a month --- a trivial fraction of the ten years of work that they have devoted to problems of scientific honesty, and a significant contribution to the wider scholarly community, of which scientists are only a part. In contrast to this harsh action of suddenly closing down their research, they have steadily received "Excellent" ratings on their work from the NIH, until the present crisis arose.

Feder and Stewart have been carrying on, for over ten years, their unique studies of scientific honesty and dishonesty, in the doing and reporting of research. These have had an important influence in leading to the correction of some unfortunate practices that have grown up in recent years, such as the attachment of the names of "honorary authors" to papers to which they contributed little or nothing. (See NATURE 325 (1987) 207-214). I note that they had great difficulty, for several years, in getting this paper published, because of the threats of lawsuits by people who felt themselves threatened by some of the facts they recorded. Inevitably they have

acquired enemies in the course of their work, including some influential people.

Feder and Stewart have supported various responsible whistle blowers, such as Professor Robert Sprague of the University of Illinois, who suffered a grim ordeal in his ultimately successful effort to correct and expose the frauds perpetrated by Dr. Stephen Breuning. I know that Professor Sprague has already written to you with strong support for Feder and Stewart in the present action against them. He received much help from them in his painful struggle.

The NIH now proposes to reassign Feder to a rather routine administrative position in another division, which I believe will be a waste of his talents. It proposes to assign Stewart to the laboratory of Dr. William Eaton, whom I know well. Dr. Eaton is doing important research on hemoglobin, and is indeed one of the world's top authorities on the chemistry of sickle-cell hemoglobin. He called me up recently, to discuss the problems raised by the assignment of Mr. Stewart to his lab, since he found that Stewart has no interest at all in working on his (Eaton's) problems, and therefore would be more of a problem to him than a help. Altogether I conclude that this scheme, devised by NIH administrators, is a typical example of some administrative concoctions that are put forward without any real consideration of the best use of the talents of the people involved.

I think that the proposal to stop the work of Feder and Stewart, and reassign them separately elsewhere, was a great mistake and ought to be rescinded. If it is not rescinded, I shall feel compelled to attack this action of NIH publicly. I would greatly regret being forced to do such a thing, since NIH is a great institution, where I have many friends, and I have happy memories of the months I spent there as a Fogarty Scholar in 1970-71. I hope that you will spare me the need of taking such action.

Yours sincerely,

John T. Edsall
Professor of Biochemistry Emeritus
Member, National Academy of Sciences

[This letter was sent to Dr. Shalala via Federal Express on May 5.]

FROM THE EDITOR

Scientific Misconduct: The End Of An Era?

"Some people will say, 'Why now?' Others will say 'Why not earlier?'"

Earl Laurence, acting deputy director of the National Institute of Diabetes and Digestive and Kidney Diseases (NIDDK), states the two most obvious questions about his recent decision to reassign Walter Stewart and Ned Feder within NIDDK to a physical chemistry lab and an extramural-grants-review position, respectively. As of May 7, Stewart and Feder, best known for their investigations of scientific misconduct, will no longer be funded by NIDDK to use their "plagiarism machine" to search the literature for instances of plagiarism or otherwise investigate alleged misconduct.

Stewart and Feder are not happy about the reassignment. They view it as an "adverse personnel action"—NIH bureaucratese meaning that the action is punitive and therefore subject to appeal. Laurence says the reassignments are clearly not adverse actions; both Stewart and Feder are being maintained at their previous grade, status, and salary.

In his April 9 letter to Stewart, Laurence wrote, "...the work that you and Dr. Ned Feder have been doing...has progressively moved outside the mission, responsibility, and authority of the NIDDK." Laurence did not cite the reason now widely regarded as the trigger for his action, that Stewart and Feder had investigated the writings of Stephen Oates, a University of Massachusetts historian, who was not receiving any federal funds.

Stewart and Feder did not seek out the Oates case. Five historians had previously accused Oates of plagiarism, and in 1991 the American Historical Association (AHA) investigated their claims. AHA found that Oates failed to give a key Lincoln biographer "sufficient attribution," but AHA did not accuse Oates of plagiarism. Oates threatened to sue one of his accusers, who then asked Stewart for help. So Stewart and Feder used their computer to search four of Oates' books—on Abraham Lincoln, William Faulkner, and Martin Luther King—for instances of plagiarism. In February, they reported more than 400 such instances to AHA, and also explained the limitations of their analysis. Oates repeatedly and vigorously denied all allegations of plagiarism.

Stewart and Feder say that they had had several discussions with Laurence about the Oates investigation and that Laurence, their NIDDK supervisor, offered no objection or specific caution. Laurence says he cannot remember when he first learned about the case.

It is reasonable to argue that Stewart and Feder should not have used NIDDK resources for investigating a non–federally funded historian. The most important question, however, is whether Stewart and Feder, in their 10-year-long drive to ferret out scientific misconduct, have contributed to science. It is clear that they have. They listened to whistleblowers when no one else would. They raised the consciousness of all scientists about the problem of misconduct. And they repeatedly urged scientists to do a better job of policing themselves.

A larger issue now is whether an efficient and fair system currently exists for investigating allegations of misconduct in biomedical research. Last year, the Office of Research Integrity (ORI) replaced NIH's Office of Scientific Integrity. Stewart and Feder operated outside that system, a maverick position that Stewart justifies as essential because of the freedom and lack of bias it afforded.

It no longer seems appropriate for an individual NIH institute to support researchers to investigate allegations of misconduct anywhere in the sphere of federally funded biomedical research. But if such investigations contribute to science in ways that ORI cannot, then funds should be found elsewhere in the Public Health Service to support the effort.

Deborah M. Barnes

—DEBORAH M. BARNES

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Boston Globe, May 12, 1993, page 3

NIH aide fasts in protest

Fraud investigators are told to shut office

By Peter G. Gosselin
GLOBE STAFF

WASHINGTON – A federal researcher who gained national prominence for his investigations of scientific fraud has begun a hunger strike to protest a decision by superiors to close his office and lock up his files.

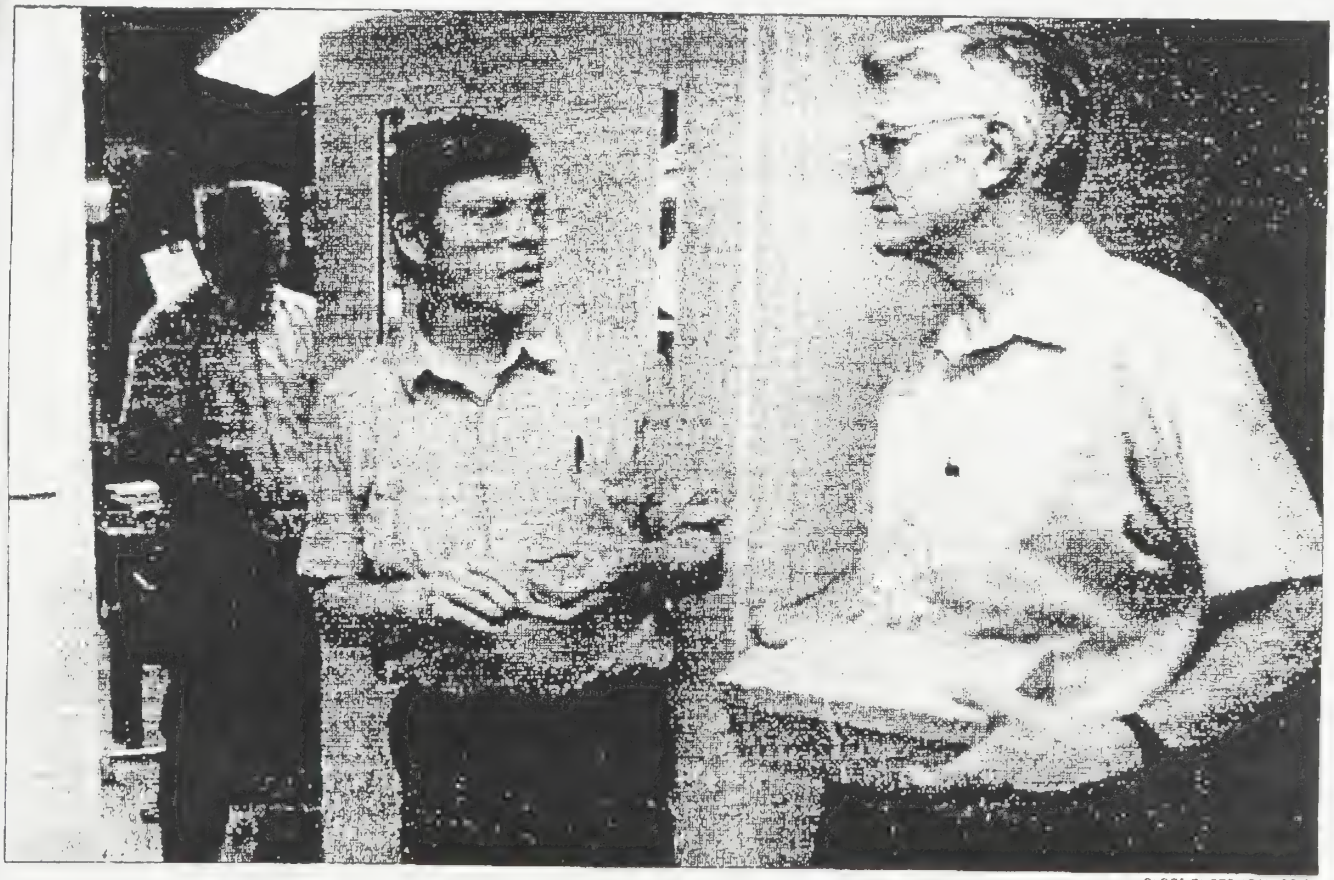
Walter Stewart, a scientist with the National Institutes of Health, said yesterday that he stopped eating after officials ordered him and a colleague, Ned Feder, out of their office on the institute's Bethesda, Md., campus Monday and changed the locks.

Institute officials have said that the two would be reassigned because they had strayed from the agency's scientific mission by leveling charges of plagiarism against a University of Massachusetts historian.

NIH's move to end the pair's work on misconduct and Stewart's decision to fast set up a strange confrontation, and opens the latest chapter in the men's decade-long career as scientific gadflies.

Since the early 1980s, Stewart and Feder have exposed a string of frauds that have shaken the confidence of many people in the honesty of federally funded research.

They were instrumental in investigating the case of Harvard cardiologist John Darsee, who was shown to have faked research data, and in proving that key evidence for a scientific paper written by Nobel laureate David Baltimore of the Massachusetts Institute of Technology and Thereza Imanishi-Kari of Tufts University had been forged.



GLOBE PHOTO / PAM PRICE

NIH colleagues Walter Stewart (left) and Ned Feder talk Monday while a worker changes the locks on their laboratory.

Institute officials have said that the two would be reassigned because they had strayed from the agency's scientific mission.

The pair's current troubles began when they developed a computer program to uncover what they described as plagiarism in a biography of Abraham Lincoln, "With Malice Toward None," by Stephen B. Oates, a UMass historian.

Oates vehemently denied any wrongdoing and complained to, among others, Sen. Paul Simon, an Illinois Democrat who has written about Lincoln. In a March 17 letter to NIH officials, Simon labeled Stewart and Feder's allegations against Oates "baseless" and demanded to know why they had been permitted to use agency money to study the issue.

L. Earl Laurence, acting director of the institute where the two men work, said that he knew of the Simon letter when he ended the pair's work on misconduct and reassigned them, but said that he had reached his decision independently.

Laurence originally ordered that the researchers' records be seized and sent to storage. He relented after Rep. John D. Dingell of Michigan, who has conducted high-profile hearings on scientific fraud, questioned whether the order was "an overreaction to an isolated incident."

Stewart said he and Feder first focused on the Oates book as a means of testing their computer program for uncovering plagiarism and only later became embroiled in the controversy over it. The two have filed a complaint with the American Historical Association, alleging that Oates wrongly copied hundreds of phrases from an earlier biography of Lincoln.

Stewart said that the pair kept Laurence and other NIH officials apprised of their work and received permission to proceed. He said that he was fasting to protest the NIH's decision to lock up his records, which he said contain information that is "critical" to proving new cases of scientific fraud and which, in many instances, was provided by whistle-blowers.

Despite infuriating many researchers, Stewart and Feder also have attracted considerable support among scientists. John T. Edsall, a Harvard biochemistry professor and member of the National Academy of Sciences, has written NIH officials that their decision to stop the pair's work is a "great mistake and ought to be rescinded."

New York Times
Jan. 7, 1992
p. C1, C9

Plagiarists Take Note: Machine's On Guard

In go the data; out comes evidence of wrongdoing.

By PHILIP J. HILTS

WASHINGTON

FEW figures in science have engendered more emotion than Walter Stewart and Dr. Ned Feder — and that was before they invented their little "plagiarism machine."

"You put the papers in here," Mr. Stewart said as he bent forward and peered through thick glasses bound to his head by a rubber band. The scanner digests the paper, transforming it into a computer file ready for the test. "It can look at two documents, or compare one paper to a whole field of papers, and it boldfaces text whenever 30 characters or more are identical," he said.

In principle, the entire literature of science could be scanned for plagiarism with this device, Mr. Stewart said.

But it seems unlikely that anyone would be willing to spend the time or money to do that. Rather, the machine's use most likely will be the one to which it has already been put: when plagiarism is suspected, the machine can compare the work of one author with the rest of the literature in his field for any instance of copying.

Mr. Stewart and Dr. Feder work a stone's throw from the office of the director of the National Institutes of Health, but she works on the carpeted upper floors and they in a subbasement room at the campus in Bethesda, Md. They have two narrow rooms once filled with thousands of bottles of snails.

In one, a dozen pieces of computer equipment and several screens hum and blink, while the next room is a throwback to an earlier age. It contains paper, ceiling-high stacks of paper, tidily arranged in file folders, which in turn are arranged in boxes. Here, they have have just begun to read scientific articles into the machine, inserting perhaps 7,000 articles and books from two subfields of science so far.

Plagiarism, the appropriation of another author's words or ideas, is a much despised crime in the academic world, where intellectual property is the basis of advancement. The word-for-word copying of another researcher's articles might seem the least likely form of plagiarism because the theft, once detected, catches the perpetrator red-faced and red-handed. But along with the other forms of fraud that have surfaced in science have been several startling cases of plagiarism.

Any device that helped detect or deter such a blot on science might seem to deserve the heartfelt support of scientific leaders. But the plagiarism machine developed by Mr. Stewart and Dr. Feder has not received a rapturous welcome so far.

Marty Katz for The New York Times

Ned Feder, standing, and Walter Stewart at the National Institutes of Health in Bethesda, Md., where they scan scientific documents with a computer for indications of plagiarism.

"I find it chilling," said Dr. Maxine Singer, president of the Carnegie Institution, a research organization in Washington. "We don't normally in our society go looking for behavior not consistent with accepted practices. The whole system is designed to protect people. I don't know why in science we have to do these more threatening kinds of things."

Mr. Stewart and Dr. Feder "may be well-intentioned," Dr. Singer said, but she does not make the same allowance for their machine. "Of the various uses modern technology would be put to, this machine is one we didn't expect. We would have expected the C.I.A. or Interpol to use it, not scientists."

An article in the British journal Nature fretted: "An untested misconduct machine would be dangerous at any speed. With the power to ruin careers, even a test-drive could cause disaster."

The pair did not begin with plagiarism, but somehow have become fascinated, perhaps obsessed, with misconduct in science. They have played significant roles in half a dozen major cases of misconduct, bringing reactions varying from wariness to anger among other scientists.

Vindication Is Seen

The case of a Cell paper by Dr. Thereza Imanishi-Kari that was judged to have been falsified is their most recent success. Against vigorous opposition by Dr. David Baltimore, a co-author of the paper who was at the Massachusetts Institute of Technology at the time, and the failure of the authorities at M.I.T. and Tufts University to find anything wrong, Mr. Stewart and Dr. Feder championed the cause of a young whistle-blower in Dr. Imanishi-Kari's laboratory who had persistently questioned the validity of the published article.

The recent finding by a committee of the National Institutes of Health that Dr. Imanishi-Kari had indeed misrepresented data in the paper — a charge she continues to deny — was seen as a vindication for Mr. Stewart and Dr. Feder. It was also a factor in Dr. Baltimore's recent resignation as president of Rockefeller University.

Mr. Stewart and Dr. Feder are now preparing to testify in a court case involving a 6,000-page manual of plastic surgery, which is reported to contain scores of pages copied word for word from a leading textbook, "Reconstructive Plastic Surgery" (W. B. Saunders), edited by Dr. John M. Converse. They have also played roles in many other cases that have not reached the public eye. Along the way, their own research in science has been shelved.

Some critics view a 'misconduct machine' as dangerous.

Dr. J. Edward Rall, who until recently was their boss as deputy director of research at the Institutes, says he thinks the two are foolish. "I have been expostulating with them for years to get out of the gutter and do some science," Dr. Rall said. But then, he said, he has been around long enough to know that a young man with an idea that seems crazy could well be right.

Dr. Drummond Rennie, deputy editor of The Journal of the American Medical Association and a professor at the University of California at San Francisco, said of them, "They have a burning cause and have become like pit bulls. But having a cause, behaving in that way, makes people very uncomfortable and makes them loathed. Fundamentally, though, I think they are very good for biomedical science.

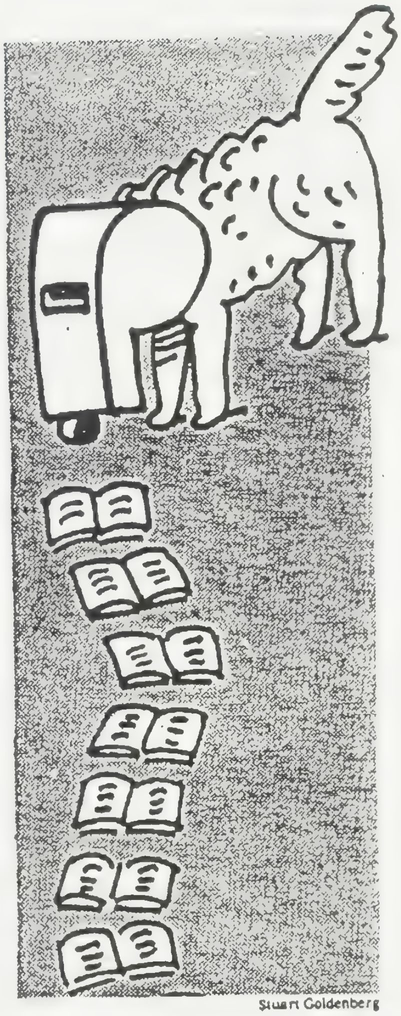
"Their basic points are correct: whistle-blowers have a rotten time in science, and everybody in science has a real responsibility for the science we turn out."

The machine devised by Mr. Stewart and Dr. Feder has already been used to settle actual charges of plagiarism. Like other forensic techniques, it can absolve a defendant as well as track down the guilty. C. Kristina Gunsalus, the research standards officer at the University of Illinois, has used the machine in two cases and does not find it worrisome. "This is a serious tool," she said. "Humans still must make important judgments in using it, but it makes possible things that simply could not be done without it."

She guesses, based on experiences at Illinois, that more than half of all cases of misconduct in science involve plagiarism. But plagiarism is much more difficult to detect than might be expected.

"I might read something and months later remember the facts, but I wouldn't be likely to remember that precisely the same language was used in some passages," Mr. Stewart said. "And even if you do find one instance — was that one a mistake or bad judgment one time, or is it a part of a pattern of plagiary?"

Mr. Stewart said he knew of a person who had spent an entire month comparing a book chapter with the material from which it was supposedly plagiarised. "With this system, the comparison itself would take about 20 seconds," Mr. Stewart said.



Stuart Goldenberg

Using a combination of computer programs, some commercially available and others written by Mr. Stewart, he and Dr. Feder reckon that, with clerical help, they could in a single day test a 2,000-page article for plagiarism from articles in an entire subfield of science.

Their method is to feed pages from the suspect article into a scanner that can read a variety of typefaces and convert them into electronic form. The electronic version of the text is then broken down by the computer program into strings of 30 characters each, including letters, numbers and spaces. The first string begins with the first word of the first paragraph, the second begins with the second word, and so forth, building overlapping strings throughout the article.

To compare all the strings in one text with all the strings in the rest of a field's scientific literature would take an inordinate amount of computer time. Instead, the program sorts all the strings in a computer equivalent of alphabetical order, thus putting identical pairs next to each other, which the computer then prints in boldface.

After doing thousands of such runs, Mr. Stewart said: "The most surprising thing is how unique human language is. We find very, very few duplicates — even in highly technical text talking about the same thing." As it turns out, in the 7,000 or so manuscripts he has looked at so far, Mr. Stewart has found that only about one string in 200 may be duplicated by chance alone. That rate is about five "millifreemans" in the units of plagiarism created by Dr. Feder and Mr. Stewart.

The basic unit, one freeman, "refers to the ultimate case, the theft of an entire document word for word, changing only the author's name," said Mr. Stewart. The unit is named after an individual whom he regards as having committed large-scale plagiarism. Attempts to reach the individual by telephone were unavailing.

Mr. Stewart says that at the level of 10 millifreemans and above, "there is serious reason to look at two documents to see if there is plagiary or the identical passages have been properly attributed."

As the two men have worked on cases of plagiarism in two subfields of science, they have noted in passing half a dozen cases of likely plagiarism that have gone unnoticed in the two fields. They do not have time to pursue them now, they say.

After much experience with plagiarism, they have learned at least two facts: "That plagiarism is rare; and that people who copy do so from obscure places and chiefly from dead authors," Mr. Stewart said, adding sotto voce, "There is something specially disturbing about that, isn't there?"

A Future Task

Mr. Stewart and Dr. Feder said they hoped, apart from examining the papers in occasional cases of misconduct, to use the machine to document the general practices of citing others' work in science. When scientists use the material of others, how fully do they give credit? Do they use quotations or paraphrases? What is considered adequate credit?

Remarkably enough, in a profession that feeds on data, very little data have been gathered about the behavior of scientists themselves. "Fraud contaminates the literature, and a climate in which people do not deal frankly and fully with errors is inimical to the true values of science," Mr. Stewart said. "We need mechanisms to find out what the facts are."

Crazy as it may seem, he says, the plagiarism machine could be one of them.

A Comparison of Passages by Oates and Earlier Authors

Passages written by Stephen Oates are in the right column. Phrases identical or nearly so in the two columns are **boldfaced**.

Lincoln

Lolling on the low deck, giving an occasional **tug on the slender sweeps to avoid the snags and sandbars ....** (Thomas, 1952, p. 17)

At last they came to the tumultuous Mississippi and headed southward in its tempestuous currents, **tugging on their slender sweeps to avoid snags and sandbars....** (Oates, 1977, p. 14)

Lincoln awkwardly dished out the oysters. (Thomas, 1952, p. 453)

... **Lincoln awkwardly dished out** fried oysters to everyone. (Oates, 1977, p. 401)

Sherman's boys hit South Carolina **like a horde of avenging** Goths. (Thomas, 1952, p. 505)

In February, Sherman's army stormed into South Carolina **like a horde of avenging** angels.... (Oates, 1977, p. 415)

Martin Luther King

Up to 25 profanity-laced telephone calls a day came to the King home. **Sometimes there was only the hawk of a throat** and the splash of spittle against the ear piece. (Time magazine, Feb. 18, 1957, p. 19)

Then there were the obscene phone calls -- as many as twenty-five a day now. **Sometimes there was only the hawk of a throat**, the sound of spit against the receiver. (Oates, 1982, p. 83-84)

I must return to the ... valley filled with misguided, bloodthirsty mobs, but a valley filled at the same time with little Negro boys and girls who grow up with ominous **clouds of inferiority** formed in their **little mental skies....** (M. L. King, quoted by Miller, 1968, p. 206-207)

King took his daughter there [to a formerly segregated amusement park] for a day of cotton candy and whirling rides, and clouds of uninhibited delight now replaced **clouds of inferiority** in her **little mental skies.** (Oates, 1982, p. 269)

<u>Faulkner</u>

...the Warner lot -- 135 acres, **girded with walls like a** medieval city. (Blotner, 1974, 1117)

...Warner's Burbank studio, **girded with walls like a** prison. (Oates, 1987, p. 190)

Albert Isaac Bezzerides was a strong, **dark, massive man** of thirty-six. (Blotner, 1974, p. 1130)

...A. I. "Buzz" Bezzerides, a **dark, massive man** of Turkish birth.... (Oates, 1987, p. 190)

The time was 1924 and the place was Oxford, Mississippi, where Billy [Faulkner] **had just lost the only job he had ever held for any length of time.** (Blotner, 1974, p. 376).

And he [Faulkner] was penniless -- **had just lost the only job he had held for any length of time** -- and was trying to be a writer. (Oates, 1987, p. 41)

And I am **deep in memory**, as if summoned there by a trumpet blast. **Dilsey and Benjy and Luster and all the Compsons, Hightower and Byron Bunch and Flem Snopes and the gentle Lena Grove -- all of these people and a score of others come swarming back** comically and villainously and tragically in my mind with a kind of mnemonic **sense of utter reality, along with the tumultuous landscape and the fierce and tender weather, and the whole maddened, miraculous vision of** life wrested, as all art is wrested, out of nothingness. (Styron, 1973, 262)

In the funeral procession, novelist William Styron found himself **deep in memory**, as **Dilsey and Benjy and all the Compsons, Hightower and Byron Bunch and Flem Snopes and the gentle Lena Grove, all these people and scores of others came swarming back** in Styron's **mind with a sense of utter reality, along with the tumultuous landscape, the fierce and gentle weather, and the whole "maddened, miraculous vision of life"** that had created them. (Oates, 1987, 321)

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27 April 1993
WWS, NF