ARTICLES

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THE IMPORTANCE OF BEING RIGHT VS. THE IMPORTANCE OF BEING EARNEST: PUBLIC ACCOUNTABILITY OF SCIENCE IN THE BALTIMORE CASE

The Three Dimensions of "Good Science": Good Results, Good Ethics, and Good Public Image of Science

In this article I will examine a recent episode involving government-sponsored research in the United States. At its most fundamental level, this case concerns the ability of science to police itself, a crucial question in today's competitive world of research. There are three main interested parties when it comes to regulation of federally funded research: the scientists holding government-sponsored grants, the grant-administering scientific research organizations (the largest of which is the National Institutes of Health, NIH, with a budget of 8 billion dollars) and various governmental bodies overseeing the proper use of the taxpayers' money (for instance, Congressional Committees and their Subcommittees of Investigation and Oversight). The question is, Who is the proper authority to judge what is "good science" and acceptable standards for scientific conduct, and what is the adequate form and amount of control?. More generally, the question concerns the meaning of 'public accountability of science'.

The differences in prevailing views is beautifully illustrated in a recent case of NIH-funded research, the so-called "Baltimore case," involving a paper published in 1986 in the journal *Cell* , with the Nobel laureate David Baltimore as the most famous of its six coauthors (Weaver et al., 1986). This case grew from an "internal" scientific question about scientific error to a much-publicized case of scientific misconduct aired in Congressional hearings and the media, and which involved the unprecedented step of bringing in the Secret Service to analyze laboratory notebooks. The case was protracted over more than five years, involved five highly publicized Congressional hearings, innumerable editorials and comments in leading hournals and newspapers. Even

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though it can be argued that the case would never have reached such notoriety had it not involved a Nobel laureate, and had it not been of interest first to a couple of self-appointed scientific "fraud hunters" at NIH and later to a powerful Congressman, the fact that it was largely debated in the public arena gives us valuable insights into different views on the nature of scientific research. Also, exactly because in this case the science involved was relatively obscure and there were no perceived "dangerous" implications for the public (as, for example, in conjunction with sociobiology, see Segerstråle, 1983, 1992a), we have here a relatively "pure case" if we want to analyze the interaction of scientific and ethical concerns in the reasoning about error and fraud in science.

As I shall argue, this case illustrates a problem that has gone largely unrecognized in discussions about scientific fraud or misconduct so far. This is that *three different considerations* are simultaneously involved in the assessment of the gravity of any particular scientific misbehavior: its relevance for the growth of scientific knowledge, its relevance for general morality, and its relevance for the public image of science. The same considerations apply to the status and importance of scientific error. For most scientists, error is not the same as fraud, and error is not perceived to be as detrimental to science as the general public would perhaps believe. Maintaining this in an unqualified manner, however, may be unwise, as the Baltimore case shows. ('Misconduct' is a new term which started being used during this case. It is a term that is broader than fraud, and may, arguably, spread all the way to error. In fact, the very vagueness of the term seems to have been one of the reasons for the problems encountered in the resolution of this case; cf. Culliton, 1991. This will be examined more closely later.)

The relationship between knowledge, moral and image concerns for science in cases of misconduct have come under at least indirect discussion because of the Baltimore affair. Scientists have been forced to confront some basic questions about the conduct of scientific research and to explicate their standards for "good science." In this article I shall argue that the inherent ambiguity of science led to an interesting turning point in the attitude of other scientists to this whole affair. At a specific point, there seems to have happened a veritable Gestalt switch, where even former supporters of Baltimore changed their mind. Although it first seemed as if the scientific community, or at least the Scientific Establishment, was prepared to rally around one of their own prominent members against the perceived threat to science from Congress, later on the tide turned. As we shall see, the turning point in this affair involved a resolution to what may be an inherent tension between "internal" scientific judgment and general ethical standards at a point when the public image of science was at stake. This forced a change in the inherently unstable equilibrium in science between two fundamental concerns: The Importance of being Earnest and The Importance of Being Right. At this point, a public ritual had to be enacted, whereby the relationship between the government and science was restored, and the public accountability of science reaffirmed (cf. Segerstråle, 1992b)

The Background Situation: Notorious Fraud Cases, Incompetent Investigations and the Whistleblower as Hero

Much of current university research is indeed sponsored by federal funds. Traditionally, this has not carried a particular meaning for the individual scientist, except as a steady source of support for research and the prestige that getting a grant entails. It is only in conjunction with some notorious fraud cases in the 1980s, all involving federal money, that the relationship between the scientist, the university, and the governmental grant-giving agency has become an issue. As the most notorious cases have amply documented, when a suspicion of fraud has arisen, universities have often shown amaz-

ing unwillingness or incompetence in their dealing with such cases, failing to find wrongdoing in cases which have later turned out to be indisputably fraudulent. Also, their treatment of "whistleblowers" on fraud has often been wanting (see e.g., Hollis, 1987; Jacobstein, 1987; Sprague, 1987). In the 1980s, several attempts were made to improve the scientific community's capability of handling cases of misconduct, giving rise to several conferences, reports, and guidelines (for an overview, see e.g., National Academy of Sciences, 1992).

There is no systematic data showing whether fraud is rampant. Informal questionnaires sometimes suggest that many scientists know of fraudulent cases, but such surveys have been criticized on various methodological grounds (cf. Anderson, 1989, 1991c, 1993b; Hamilton 1991a). Some commentators (typically non-scientists) seem willing to believe that current fraud cases are just the tip of the iceberg. Others (typically scientists) say that fraud cannot be very prevalent. According to these scientists, any important fraudulent claim could not escape detection, since important claims get replicated and built on in some way. But other scientists say that in such fields as immunological or biomedical research, the literature is full of claims which are de facto unreplicable (e.g., Crewdson, 1989). Even in organic chemistry, replicability of new syntheses is not easily attained (Bergson, 1989).

It is obviously hard for the usual review process to spot any misrepresentations. This is particularly true if the reported experimental data are totally fabricated, which has indeed been the case in quite a few fraud cases. As books written about the most famous fraud cases clearly demonstrate, the existing control systems for science: peer review, refereeing and replication, are not sufficient for coping with outright fraud, because they are simply not devised for that purpose (Broad and Wade, 1983; Kohn, 1986). Some would say that the present control systems cannot even deal with blatant error. The truth of this was demonstrated by Ned Feder and Walter Stewart, who documented a lot of minor and major errors in a set of co-authored papers, involving a famous fraudulent Harvard scientist, John Darsee. They did not ask why nobody spotted Darsee's fraudulent data in the papers, but instead how the sometimes glaring errors could have been overlooked, first by Darsee's co-authors and later by the journal referees (Stewart and Feder, 1987; see also Editorial in Nature, 1987; on the Darsee case, see Culliton, 1983).

How is, then, fraud typically found, or at least suspected? Very often, it seems that someone has results that are "too good to be true". On the other hand, this is very often said with hindsight, since at first such "beautiful" results tend to dazzle scientists, particularly if they fulfil theoretical expectations. Sometimes it is a coworker who gets suspicious, for instance knowing that the amount of experimental data reported could not have been obtained within the claimed time, or because he or she sees another scientist tampering with experiments (for examples, see Broad and Wade, 1983; Kohn, 1986). In many cases it is the person who is being hurt by the fraud that acts as a whistleblower, for instance in cases of misrepresentation of results in joint research, stolen data, or plagiarism (e.g., Hollis, 1987).

All the cases and media coverage of fraud (e.g., a Time magazine cover called "Science under Siege", August 26, 1991) and books with names such as *Betrayers of the Truth* (Broad and Wade, 1983) and False *Prophets* (Kohn, 1986) have created an overall climate where scandal and drama is almost expected in science, and where the public after several well-publicized controversies involving big names may have lost some trust in the idealized picture of science they have taken for granted. It could be argued that in this climate, the public's natural sympathy for the underdog and suspicion of elitist old-boy networks also has provided an easy ready-made framework for interpreting almost any scientific dispute. For instance, in a controversy involving a junior female challenging a senior male, the public's and the medias' natural tendency would be to take

the junior seriously. This is exactly what happened in the Baltimore case, when the issue became a matter of public debate. In this situation, the inherent "undemocracy" of the scientific profession, which is that senior scientists most often *do* have better scientific judgment than juniors, was now pitted against the possible alternative scenario of junior whistleblowing and senior cover-up.

The Baltimore Case: An Overview

The Baltimore case involves a coauthored paper in the journal *Cell* , which through a chain of events came to the attention of first Ned Feder and Walter Stewart, the self-appointed fraud team at NIH, and later Representative John Dingell (a Democrat). It was Dingell, who with his headline-grabbing Congressional hearings made a post-doctoral student's allegation of erroneous or fabricated data in this collaborative immunological paper between two laboratories at Massachusetts Institute of Technology (MIT) into a cause celebre.

One would assume that Congressmen have better things to do than get involved in details of research on how an implanted gene affects the regulation of the immune system in so-called "transgenic mice". But as it happens, Dingell's own mission as the Chair of the Congressional Committee on Energy and Commerce and also of its Subcommittee for Investigations and Oversight is exactly to oversee the activities at the National Institutes of Health. His involvement in this case and the scandals created have made some regard him as an enemy of science. Dingell himself has consistently maintained that he is a friend of science (his brother is a well-known NIH scientist, and his father, also a congressman, helped found NIH). On the other hand, this big-game hunter, famous for revealing government fraud (such as the famous Navy contracts for $600 toilet seats) and protecting whistleblowers, had just successfully gone after Stanford's president Donald Kennedy's misuse of that university's NIH research-related funds and forced his resignation (e.g., Roush, 1992). It appears that the Baltimore case, involving NIH research funds as it did, suited the Congressman perfectly in his current mission. It fed straight into Dingell's legitimate interest in the proper use of NIH funds, the ability of universities to investigate themselves and the role of NIH in supervising university misconduct investigations; it involved a whistleblower – a post-doctoral student – and finally, it involved a famous Nobel laureate. For Dingell, the question was: if the post-doc's allegation of serious error or fraud in the *Cell* paper was in fact true, how come that her claim had not been taken more seriously by the two informal university investigatory committees who had been looking into the case; instead they had declared the paper sound and the dispute about data one of interpretation, not misrepresentation? This case had the potential to serve as a demonstration of the need for better self-policing of science.

By what mechanism did now an initially internal dispute in science about the raw data for a published scientific paper come out in the open and reach the front pages of leading newspapers? It turns out that Charles Maplethorpe, a former doctoral student in the laboratory of Thereza Imanishi-Kari, the director of the immunological part of the research for the *Cell* publication (Baltimore was the director of the molecular biological part in this collaborative research effort) decided to contact Feder and Stewart. He told them that Margot O'Toole, a post-doc in Imanishi-Kari's laboratory had challenged some data in the *Cell* paper that she believed were central for the conclusions of the paper, and that she had already unsuccessfully tried to point out to authorities at Tufts University and MIT that the Cell paper contained errors (the reason Tufts got involved as that O'Toole's mentor, Henry Wortis, at Tufts had sent her to Imanishi-Kari; interestingly, Imanishi-Kari was also applying for a tenure-track position there). Basically, while Imanishi-Kari and Baltimore believed the data showed that an implanted gene through some unknown mechanism caused a "transgenic" mouse's

own immune system to start producing antibodies corresponding to the implanted gene, O'Toole maintained that the observed antibody production simply was a direct result of the implanted gene, i.e. involved no changes in the recipient mouse's immune system (e.g., Hamilton, 1991). Thus, what seemed as an interesting and important claim in the *Cell* paper would, according to O'Toole, be attributable to experimental error.

It is important to note that after the two informal investigations, O'Toole herself had chosen not to press the matter further (also, both she and Imanishi-Kari had left MIT). However, as soon as Feder and Stewart had been alerted, it was they who now pressed the reluctant O'Toole to come forth with her evidence in the case (17 pages xeroxed from a laboratory notebook), which she claimed showed that some of the data in the published paper were wrong (e.g., Culliton, 1988a,b). It seems that Feder and Stewart were primarily interested in using the case as a new test case for scientific misconduct. In the case of the papers of Darsee's coauthors, they had merely looked for published errors: here was a chance to compare published data with laboratory notebooks, this is, conduct an "internal audit", just as they had suggested at the end of their 1987 paper. After they obtained the 17 pages, they set out to analyze them; the result was a critical paper on the *Cell* publication. Because of the criticism of internal NIH referees that it was not clear what the 17 pages represented within the totality of data for the paper, Feder and Stewart's next move was to write to the six coauthors of the Cell paper, asking for all the original laboratory data. Baltimore was outraged at this initiative. He stated that he did not recognize their right "to set up themselves as guardians of scientific purity", and that their wish to do this kind of "audit" of the data was unacceptable. It would set a precedent where "outsiders" would "tie up the scientific community in continuous wrangles". This happened in January 1987 (Culliton, 1988a).

Baltimore particularly objected to the fact that Feder and Stewart had no official standing and were not working in the field of the paper: immunology and serology. He pointed out that O'Toole's charges had already some time ago been looked into in informal university reviews by immunologists (see above). Baltimore even suggested that the matter might be settled by yet another set of immunologists, who would review Feder and Stewart's charges. But they did not agree to this. Stewart argued that Baltimore's suggestion in fact "contained novel and strict sanctions against open scientific debate" (Culliton, 1988a, Baltimore 1989 a).

Feder and Stewart's widely publicized assertions that the case had not been adequately dealt with (they had sent out a letter and a copy of their unpublished manuscript to 100 eminent scientists, complaining that they had been forbidden to publish) made them useful as key witnesses in two Congressional hearings in the Spring of 1988, one of them arranged by Dingell. These hearings questioned the ability of institutions to police themselves in the face of rampant fraud, bringing up a few well-known fraud cases. The Baltimore case was also brought up in both hearings. In each case, Feder and Stewart told the Congressional committee that based on evidence from the notebooks they had concluded that "the published paper contained a number of serious misrepresentations of scientific fact." Neither Baltimore, nor the paper coauthors, nor the paper reviewers, were invited to testify. However, O'Toole was called in as a witness (Culliton, 1988 a,b; Holden, 1988).

These hearings and the front page news stories about them now changed the focus on the *Cell* article from one of "error" (O'Toole's original charge) to one of "fraud and misconduct". This change in emphasis also came about because after the Congressional hearings, the office at NIH which handled research fraud decided to look into the matter and appointed its own investigatory committee to assess the *Cell* publication. In February, 1989, this committee "acquitted" Baltimore of charges of misconduct, but criticized him and his coauthors for not having examined the article more closely for

errors that in fact were there (Culliton, 1989a; Wheeler, 1989).

Meanwhile, the interest of the U. S. Congress in this case continued. From Dingell's point of view, here was a case where not only two university investigations but now also a NIH committee had freed the paper authors from fraud charges. This meant that the case could now be regarded on top of everything as a test of NIH's own capacity for investigating fraud. Dingell decided to hold more hearings. Meanwhile he put his feared investigative staff to work. The particular power of a Congressman in this kind of investigation is that he can use subpoena to extract data from reluctant researchers. Starting with the notebooks, soon Dingell had access to all the data, memoranda, reports and correspondence in the case. His next action was to "borrow" Feder and Stewart from NIH for a time (it turned out to be almost two years, Culliton 1990b). But he did not stop here: he went as far as enlisting the Secret Service to help with the investigation. Dingell's stated aim was to find out whether the reported data had in fact been collected as stated in the *Cell* paper, and he was prepared not to give up until he got a satisfactory answer.

In his next Congressional hearing, in May 1989, Dingell called in Baltimore and the other authors of the paper. By consulting Washington experts, Baltimore had learnt that it was he who was Dingell's real target. This is why he decided to lead the defense. These Congressional hearings provided a good show for the many scientists and students in the audience. Dingell brought forth the Secret Service studies, showing irregularities in laboratory note keeping; Imanishi-Kari admitted that she was a poor record keeper, sometimes entering data from experiments a long time afterwards, but said that she knew her data. Baltimore defined the case as an attempt by the government to harass science. His eloquence about the need for freedom from oversight in order to maintain scientific creativity and productivity, both in these hearings and in later writings (U. S. Congress, Hearings, May 4 and 9, 1989; Baltimore, 1989a b) gained him support among his fellow scientists. In fact, many eminent scientists had already before the hearings engaged in a letter-writing campaign expressing concern over Congressional involvement in internal scientific matters (Culliton, 1989b).

Thus, the first round could be interpreted as Baltimore's victory over Dingell. Baltimore was applauded by his scientific colleagues. He had succeeded in making the feared Secret Service look ridiculous by explaining that sloppy notebook-keeping is a matter of scientific style; that trust is important in science and that he trusted Imanishi-Kari's capability; and that the judgment of the claims in a scientific paper was a matter for science, not Congress.

However, one effect of these hearings and the Secret Service testimony was that NIH now felt obliged to reopen the investigation. (Another reason for the reopening of the investigation was that O'Toole had been dissatisfied with the NIH expert panel report and raised new questions about the data. Particularly, she had now suggested that NIH conduct an "audit" of all the laboratory data, not just of a sample; U. S. Congress, Hearings, May 4 and 9, 1989: 10). Also, by the time of these hearings, NIH had established a brand new Office for Scientific Integrity (OSI) to handle misconduct cases. Thus, it could be said that the well-publicized hearings were "forcing" NIH to demonstrate to its overseer, Representative Dingell, that it certainly had the will and competence to handle misconduct investigations.

The practice adopted by OSI in the investigation of misconduct cases was first to collect evidence and then have the suspected person respond to the ready results of the investigation; i.e. not allowing him or her to know or challenge the evidence involved, cross-examine witnesses, etc. (OSI defended this as proper for what they saw as a type of "internal" investigation by scientists of scientists. As we shall see, OSI's methods and the lack of legal protection for accused scientists later became an issue). In the Baltimore case, OSI prepared its

draft report on the case for a long time, reportedly collaborating closely with the chief whistleblower, O'Toole (U. S. Congress, Hearings, March 6 and August 1, 1991: 165; see also Culliton 1990c; Hamilton, 1991c). When finally ready, this confidential draft report was duly distributed to the paper authors for their responses; these would be included in an official final OSI report on the case.

It appears that this preliminary OSI report was the turning point in the Baltimore affair. Despite the supposed confidentiality of the report, soon everybody knew its content. It was reported in the media and scientific journals, and it was distributed widely in the scientific community. Little or no attention was paid to the fact that it was not a final report and that it did not yet contain the authors' response to the allegations made against them (a typical headline was "NIH Finds Fraud in *Cell* Paper;" Hamilton, 1991a). In addition to strong criticism of Theresa Imanishi-Kari and of Baltimore's defense of her and the paper, the report also contained complex Secret Service analyses of ink samples from loose leaf notes that Imanishi-Kari had presented as the real data for the questioned experiments (some of which had already been published in *Cell* as a correction to the paper at NIH's request), statistical analyses of the probability that the recorded numbers represented real experimental data, and even a discourse on the match of color and ink between graph paper presented as data for the *Cell* paper compared to other graphs done in the MIT laboratory before and after the research for the paper (Anderson, 1991a; Hamilton, 1991a; see also Maddox, 1991).

How come that a confidential report became available before its time? The answer is rather amazing: it was "leaked" as soon as it was ready, and, it seems, by no one else than Walter Stewart, who sent it straight to Nobel laureate Walter Gilbert at Harvard, who then distributed it to his scientific colleagues. The report was also leaked to other scientists and the media at the same time (Greenberg, 1991b). This happened in March 1991. Immediately afterwards, the *Cell* paper was retracted (e.g., Wheeler, 1991).

But this was not the end of the saga. Soon after the Dingell hearing in 1989, Baltimore had accepted the Presidency of Rockefeller University; evidently the university's trustees had been convinced that his name was solid enough despite the brewing scandal (Beardsley, 1992). When the leaked OSI preliminary report came out in March, however, the situation became harder to contain. In April 1991, there was turmoil at the yearly meeting of the National Academy of Sciences, causing some supporters of Baltimore to suggest a letter-writing campaign to assure David Rockefeller and the trustees of Rockefeller University that Baltimore's reputation was not in question (Greenberg, 1991a).

In the comments to the OSI draft report (a large portion of which was published in *Nature*), Thereza Imanishi-Kari, Margot O'Toole and David Baltimore all give their reactions. Imanishi-Kari does not admit any wrongdoing (Imanishi-Kari, 1991), O'Toole says that she (O'Toole) was right all the time (O'Toole, 1991a,b), and Baltimore apologises, also to O'Toole (Baltimore, 1991a). Baltimore now says: "The OSI report raises very serious questions about the veracity of the serological data. I am shocked and saddened by the revelations of possible alteration and fabrication of data." He admits he should have looked more closely at the data after serious questions had been raised, but he trusted Imanishi-Karis "demonstrated abilities as a scientist." He continues:

> 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 would stand; what was important to me was that the solid molecular data gathered by my laboratory seemed to lend credence to the serological findings. In other words, as a scientist, my concern was always for the science: is the result correct? Can it be

> replicated and built upon?... It was my belief in science and faith in my fellow scientists which led me to set my treshold of suspicion so high... I have learned from this experience that one must temper trust with a healthy dose of scepticism. This entire episode has reminded me of the importance of humility in the face of scientific data." (Baltimore, 1991a).

After this, the discussion was opened up for scientists at large. *Nature* published letters from eminent Harvard scientists critical of Baltimore's conduct (Cairns, 1991; Doty, 1991) or of the claims of the *Cell* paper (Ptashne, 1991). Meanwhile, a coworker of Imanishi-Kari stepped forward and praised her as a critical, open-minded scientist and stated that the experiments had been repeated many times over before and after (Yannoutsos, 1991). In a move that many found surprising, O'Toole, despite the recognition and praise given to her in the OSI draft report and despite Baltimore's explicit apology to her in his above-mentioned comment ("I have tremendous respect for O'Toole, personally and as a scientist, and I have consistently maintained that I believe her analyses were insightsful, her expressions of concern were proper, and her motives were pure" and "I commend Dr O'Toole for her courage and determination and apologize to her for my failure to act vigorously enough in my investigation of her doubts," Baltimore 1991a), now chose to attack everybody who had not taken her word from the very beginning (O'Toole, 1991a).

According to O'Toole, the OSI had now proven that she had been right all along that the paper was fraudulent and that therefore its central claim did not hold up. What was worse, Baltimore had known about this from the very beginning, and so had the Tuft and MIT scientists involved. For instance, Baltimore was present at one of the early informal meetings when, according to O'Toole, Imanishi-Kari "candidly admitted" that some experiments had never been done. O'Toole went on to say that throughout this 5 year affair, she had been the victim of "slander and libel" while she had just been "adhering to the professed standards of the profession" (O'Toole, 1991a).

In turn, the attacked scientists (Baltimore and Imanishi-Kari, Brigitte Huber, Henry Wortis and Robert Woodland from the early Tufts investigation and Herman Eisen from the early MIT one), in their responses to O'Toole's assault, pointed out that she was mistaken: her present charges about the paper were different from her initial ones; they had acted on the basis of the available evidence at the time; they could not recall any "candid admission," and they had consistently respected her scientific complaints (Huber, Woodland, Wortis, 1991; Eisen, 1991a; this was rebutted by O'Toole, 1991b). Imansihi-Kari said that she was innocent and that the OSI report had got the whole story with the specificity of the reagent backwards (Imanishi-Kari, 1991). In his detailed response, Baltimore noted that O'Toole's comment, unlike the OSI draft report, contained allegations that he had been aware of data fabrication. According to him, this was not true; O'Toole's charges had changed over time, but as he had stated repeatedly, "consciously false claims, or fraud, by a scientist can never be excused or condoned." Finally, he hoped that "any assessment of the validity of her comments will be a measured one, based upon a consideration of all the facts and the entire record of this controversy, including Dr O'Toole's previous statements on the matter (Baltimore, 1991b).

Meanwhile, other scientists objected to the way the OSI investigation had been conducted. 143 scientists signed a statement pointing out that Imanishi-Kari had not been granted "due process": she had been accused of fraud without being able to confront the evidence against her, and her NIH funding had been withdrawn (Abu-hadid et al., 1991). Among the co-signers were several eminent immunologists, including Imanishi-Kari's mentor from Cologne (Hamilton, 1991c). Thus, not only the leak but also the

whole investigatory activity of the Office of Scientific Integrity at NIH came under criticism (Hamilton, 1991e). Amazingly, at this point it was found that, because of a technicality, the OSI rules were in fact not legally valid (Hamilton, 1991a), and that therefore Imanishi-Kari could not be charged, and the case was dropped (Anderson, 1991b), although the case was still being examined for criminal violations by the U. S. Attorney (*Science and Government Report*, July 1, 1991, p. 1). Ever since the involvement of the Secret Service, Imanishi-Kari and her lawyer had been requesting the Secret Service data to conduct their own "counter" forensic analysis on them. Unbelievably, these original Secret Service data (mostly glass plates), were recently reported damaged in transport back to Washington, D. C., and there was a question whether they were still usable (Anderson, 1993a).

What of the status of the paper itself? Has it been replicated? This is a tricky question. According to Baltimore, writing in the fall of 1991, "there is much published evidence and more coming that supports the paper's results in remarkable detail" (Baltimore, 1991c). But earlier on, at least one of the scientists whose work he was citing had herself made findings that "disagree with the broadness of the conclusion" in the paper (Leonore Herzenberg, quoted in Holden, 1988). In her comment in the summer of 1991, O'Toole had said she was "pleased that the OSI draft report included statements that the central and challenged claim of the paper has not been replicated" adding: "Indeed, the results have never been obtained, not even once" (O'Toole, 1991a). The same summer, a discussion in *Nature* as to whether or not the results of other laboratories supported the *Cell* paper's claims turned instead into a question about which results might be legitimately compared with the *Cell* paper's claims, considering the compared systems were all different. Not surprisingly, here the critics and supporters of Baltimore were of different opinions (cf. Ptashne, 1991; Eisen, 1991; Selsing, 1991). In 1993, Imanishi-Kari, who had consistently pleaded innocent and stood by her data (she had not signed the retraction of the paper), reported that she had now replicated the experiments and confirmed the results in the *Cell* paper. (She had earlier invited anyone who wanted to use her mice to replicate her experiments).

In December 1991 Baltimore resigned as president for Rockefeller University. The situation had become untenable. Eminent research scientists were leaving, and more were threatening to do so. In fact, the great majority of the faculty had been opposed to his presidency (Berdsley, 1992). Dingell could add one more exemplar of big game to his collection (see also Editorial, *Nature,* 12 December 1991).

Conflicting Views about the Importance of Error

How bad is error in science? The conventional scientific view is that errors will be eliminated in the long run. A good example of this position is Peter Medawar's exhortation: do more science! This was also the view that Baltimore tried to convey at the Congressional hearings, both to the science journalists in the corridors, and in his popular articles published after the hearings (Baltimore 1989 a,b). What is of interest here is that Baltimore represents a view quite different from that of O'Toole. As we shall see, the Baltimore case is at least in part a conflict between different views of how error is related to "good science".

In his article explaining his plight as a scientist faced by the powers and ways of Washington, aptly entitled "Baltimore's Travels", Baltimore eloquently characterized science as a continuing dialogue (Baltimore, 1989a). Other scientists build upon, correct and extend your findings. This is the way science works. Publication is a decision: you publish when you think you can tell a story, but this does not mean you believe it is the final truth. In fact, you expect others to correct you. Baltimore employed the term 'peer review' for this process. The basic question

is how well the results of your paper can withstand others' attempts to offer different interpretations; this is also the way in which results are replicated in practice. There is no point in going after error per se. In contrast, O'Toole's position on error is that error should be corrected. A scientist should not lead others on the wrong path, because this involves a waste of precious time and money for them. This was O'Toole's position from the very beginning (at that point, she had charged only error even though she may have suspected fraud) (U. S. Congress, Hearings, April 12, 1988; and O'Toole, 1991a).

But was it fraud or was it error? O'Toole later said she suspected fraud all along (O'Toole, 1991b). Feder and Stewart had freely used the term 'misconduct' and 'fraud' in their widespread lecturing about the case. In 1991, the case had been included in Congressional hearings devoted to 'scientific fraud,' thus connecting it to known fraud cases. Meanwhile, a scientific committee of immunologists appointed by NIH had "acquitted" Baltimore of all fraud charges (Culliton, 1989a). The Baltimore case soon triggered an animated public discussion in newspapers and journals, showing examples of the contrasting positions in this case. Some judged the case as fraud and found it shocking. An article in *Time* magazine portrayed Baltimore with a long nose made out of a test tube (Ehrenreich, 1991). A response to this article, in turn, accused the author of "sanctimony," saying that "society will only lose if the case diminishes the use of its exceptional talents and if it bureaucratizes science in the search of unattainable perfection" (Davis, 1991a). An editorial in *The New York Times* (26 March, 1991) spoke of a "scientific Watergate," while articles by scientists maintained that "error is not fraud" (Pollack, 1989; Loehle, 1989; Cooper, 1991). Those who defended the case as one of error typically pointed to the history of science (e.g., Loehle, 1989).

What is, then, the conception among bench scientists about how to handle error in science? The following is a general reasoning about error in science by a scientist totally unconnected to the present case (a physicist). According to him, part of learning to do "good science" is being able to identify significant from insignificant error. Since this takes time and experience, for instance graduate students may not yet have the correct intuition. It some cases, after they have worked with a group one year, they may come out and say: "Those guys were fudging everything!" This physicist remembers how he himself believed his supervisor was fraudulent; but he decided to wait and see. Later he realized that the supervisor had indeed been right. Surprisingly, it seems that the view that errors will be corrected or fall by the wayside is also shared by many journal editors. There is no merit in "cluttering the literature" with notes of correction or retraction. Thus, at least there does not seem to exist an established convention of publishing corrections (even though notes of retraction occasionally do appear) (biomedical scientist, personal communication; Davis, 1991c).

What is then the praxis of individual scientists who discover errors in their *own* research after they have published? If they cannot publish a correction, it would seem natural that they at least in their next paper would tell other scientists that they were wrong in a previous paper. Does this happen? According to an informant in the biomedical field, scientists correct their errors in their next paper, but without pointing that out. There is no overt admission of error; the next paper is instead presented as a further elaboration or specification of the earlier paper. Also, according to the same informant, even as a scientist publishes a paper, he or she may know that the claims in the paper are probably justified, while all possible alternative explanations have not been eliminated. This is not reported: the burden of proof is on those who will try to build further on the results. The paper author is in a problematic situation and has to use good judgment: it would be too expensive timewise to be absolutely sure and, unless the referees request it, why bring it up at all?

Thus, in a sense one could talk about a collective conspiracy and cover-up in science, with everybody involved, including journal editors.

If the established praxis is such that scientists do not admit their own mistakes and editors are not eager to publish corrections, it is obvious that a breach of this praxis would be considered unusual and therefore attract attention. Thus, although O'Toole seems to have wanted a correction or retraction of the *Cell* paper, there were many reasons having to do with scientific convention why this would have been seen as warranted only if a clear case of fraud could have been demonstrated in the first university inquiries. Since the question of fraud had not been formally raised, the heads of the Tufts and MIT informal investigations, Wortis and Eisen, saw no reason for an official correction of the paper. Also O'Toole's later complaint that she was discouraged from writing a Letter to *Cell* (e.g., O'Toole, 1991a) can now be given a alternative explanation as in fact professional advice: such an action would have deviated from the convention of "criticism through more research."

It could thus be said that the informal mechanisms of science in various ways play down error and in this way indirectly or directly protect incorrect or even fraudulent science – in the short run. It was this informal gentlemen's agreement, which had worked relatively well for scientists so far, that now fell apart in an embarrassing way under Dingell's (and Feder and Stewart's) relentless scrutiny. Against Baltimore's eloquent explanations, Dingell found a quiet, strong point, which he doggedly repeated in his hearings: he was not interested in whether the claims in the Cell paper were correct or not – what he wanted to know was whether the paper was done as described. In an interview In *Science and Government Report* (May 15, 1991), Dingell said:

> I have been accused of running a replay on the trials of Galileo and the difficulties of Copernicus, but I would remind you that the charge was never made that they had performed improper experiments. The charge was that somebody disagreed with their conclusions. We have never disagreed with the conclusions in the case of Baltimore and Imanishi-Kari. The only thing we ever said was, we wanted to know if, in fact, the work was really done as reported...(W)here the federal government is paying for something, it ought to get what it is paying for. In other words, if somebody says they're performing scientific research, reporting it as scientific research, it, in fact, should have occurred the way they said it did.

Ever since the informal early university inquiries, the gist of Imanishi-Kari's explanation had been that even though there were some errors in a table in the paper, there existed other unpublished data that supported the conclusions. This had initially satisfied the university committees and also the first expert panel called by NIH, but some of this data had later been challenged in forensic analysis. Even so, a letter by Imanishi-Kari's coworker Yannoutsos stated that the experiments reported in the *Cell* paper had been repeated many times before and after in their laboratory: he wondered about the scientific rationale for the withdrawal of the paper (Yannoutsos, 1991). Also Baltimore repeatedly hinted that evidence from other laboratories showed that the paper's claim was correct and more results were forthcoming (Baltimore 1991b,c). But Dingell kept asking the concrete question: did the data for the *Cell* paper exist when they were supposed to exist?

Here we see a different emphasis on science as product or procedure. For scientists such as Baltimore, what matters is the long run. This means condoning brave leaps and creative experimentation and trusting one's scientific intuition. For non-scientists such as Dingell, what matters is simply knowing that the science was done as described. O'Toole would call this "telling the truth" (e.g., Carton, 1991). This has been seen as O'Toole's

moral quest, but there is also a quite practical *scientific* reason, why scientists' "telling the truth" is important for juniors like O'Toole. For beginning scientists, what matters is particularly being able to build on others' results. At this stage, they are more dependent on the details in a paper than more established scientists: they have to know that they are doing the experiment right in order to be able to correctly identify their own contribution and be able to relate it to the existing literature. In this case, we have a frustrated postdoc who cannot replicate her own initial findings at MIT, which had looked promising and she had been praised for early on. (Her experiments were related to the Cell paper and involved similar techniques). She now concludes that the lack of specificity of the reagent used in the transgenic experiments for the *Cell* paper may have produced spurious findings, which in her view undermine the paper's central claim. However, existing scientific praxis leaves little room in science for such an observation, even if correct. Furthermore, failure to replicate experiments can always be attributed to lack of training, or incompetence (in fact, this was Imanishi-Kari's view of O'Toole's lack of success; see O'Toole's testimony, U. S. Congress, Hearings April 12, 1988; Culliton, 1988a).

Obviously the "product" rather than "procedure" attitude to error has another side: it invites the possibility of self-deception, including collective self-deception, as has been shown in science in such cases as the N-rays, polywater, and (at least for a while) cold fusion. Here the results have not held up in the long run, but at least in the polywater case, the research went on for a decade at an international scale (Franks, 1981). Irving Langmuir (1989) has called such episodes in science "pathological science", and even provides a few rules of thumb for identifying likely candidates. But it could be argued that the only difference between the research conducted in these cases and "normal" research is that the results did not hold up – which was found only post hoc (cf. Segerstråle, 1990, 1993).

The Importance of Being Earnest vs. the Importance of Being Right

This case illustrates a basic contradiction in the scientific profession. In actual judgment of other scientists' behavior, scientists seem more concerned with the fact that their conlusions were *right* than exactly *how* they came to them. Again, there is an emphasis on product rather than procedure. If you got the "right" answer, this is ascribed to "good science". You followed your scientific intuition and judgment, even though you may not have been totally earnest. On the other hand, if you are "wrong", no one cares about how earnestly you were wrong.

This reasoning that it matters less *how* you came about your result, if it only holds up later can be seen in many scientists' reactions to supposedly "shocking" claims by historians of science, such as Newton's fudge factor (see e.g., Brush, 1974) , Millikan's oildrop experiments (Holton, 1978), or, most recently, the claim that Coulomb "could not" have reached Coulomb's law using the instrument he described (Dickman, 1993). One good example is some physicists' reactions to the famous case of Nobel laureate Robert Millikan's oildrop experiments (Millikan stated in a publication that his result for the charge of the electron was based on the average of ali the oildrops over a period of time, while his notebooks show that he had clearly omitted the bad ones, cf. Holton, 1978). According to one physicist, it is "of course a lie to say something like that," but it also happens nowadays. "People say such things as 'this is the average over the entire period.' That is a lie, but people do make statements like that. I don't think it is a terrible crime." According to another physicist, "that is a misleading, possibly even a false statement, but I wouldn't say it is fraudulent. Things can go wrong with experiments, and sometimes you know some readings are not good but you don't know why. That was probably the case with Millikan." Since Millikan's experiment has indeed been repeated (even though some replications have not succeeded, David Edge, personal communica-

tion), Millikan's misstatement is exonerated on the basis that he was "right": his claims held up later. This case continues to be a source of contention between ethicists and scientists.

This has important consequences for the manner in which it is seen proper to challenge other scientists' claims. According to one informant (again a physicist):

> "If you are going to call a scientist a cheat and a liar, you challenge his most basic reason for existence on this earth. If you do this, you had better be right, and that in *two* senses. You have to show that not only are the conclusions unwarranted based on the data, but in fact he is drawing the *wrong* conclusions. Because if you simply accuse people of throwing away bad data, or of improving the statistics a little bit, or not taking into account systematic errors, etc. and the results are ultimately published as a number, if that number holds up in the future, then no matter how the person came up with the conclusion, he isn't going to look that bad in the public eye. On the other hand, if it is a straightforward experiment and someone in the future gets it to disagree by a substantial amount, then he *will* look bad."

Thus, if the result is correct, condemning scientific error on exclusively moral grounds is probably atypical for practicing scientists. Obviously, it is an empirical question whether this is indeed the case, both in the field of physics and in other fields. In cases of doubtful claims, a practical attitude would appear to be to to wait and see, not to meddle with other scientists' claims, *unless* one can produce convincing alternative research results and explanations.

What are the consequences of such an attitude to scientific error? If what matters is to be right, and there is no perceived merit in correcting error as such, there will be a premium on being a quick and dirty first, not a conscientious or careful second. In fact, scientists seem to hold a certain amount of contempt for those who have been "scooped," even though the reason for this may have been exactly that they wanted to make sure that their experiments really yielded the reported results (biomedical scientist, personal communication). This serves to reinforce a gap in science between "leapers" and "plodders": the first ones operate with just enough supportive evidence in order to be able to convince peer reviewers, referees and readers of their published papers; the second group may take their time to convince themselves, or perhaps have an unrealistic expectation of when time is ripe to credibly stake a scientific claim. It is the second group, usually junior scientists, that suffer within the present system: they have less experience to know when they have enough evidence to publish, they have less credibility if they venture to make bold conjectures and they are likely to be more closely scrutinized than more established scientists. Finally, they are more dependent on the details of others' published papers and probably take these more seriously than more seasoned scientists.

For instance, the notion of "fruitful error", championed by more mature scientists, may be very far from the mind of a beginning researcher, desperately trying to learn the methodology of the field and dependent on data being exactly as reported. For such a person, typical scientific overstatements, such as "this is the average over the whole period" may be taken at face value. Overstatements become particularly problematic when it comes to methodological procedures. For instance, one graduate student got a particular method for separating cells to work only about half as well as an author of a paper had described. Upon meeting the author at a conference and asking him what he might be doing wrong, he was told that the author had grossly exaggerated the separation ratio "because otherwise one does not get published." Thus, there is a lot of tacit knowledge in science about just how seriously to take the claims of a paper; this is acquired with experience.

Concluding remarks

In its early stages, this case seems to have fitted the stereotypical expectations of scientists. Even if one was totally unfamiliar with the details of the science involved in this case, it may have appeared as a typical case of conflict between "insiders" and "outsiders". As we have seen, the early inquiries at Tufts and MIT followed the expected pattern. It was not until Feder and Stewart got involved that any change happened. They seemed to have identified this as a perfect test case for their own new interest in "auditing" science, got frustrated with being refused the raw data they needed in order to be able to write a paper on this case, and started their own campaign publicizing this case. In turn, it was not until Dingell got interested, arguably regarding this case as the perfect test of the self-policing of science and having potential as a precedent , that the Baltimore case became a cause celebre. But for the case to have desired pedagogical impact, it would have to be presented as a case of misconduct, not mere error. At the beginning, it was probably more important for Dingell to have the case just generally associated with the idea of misconduct rather than have clear evidence of fraud.

Another, more ominous, reason why the case may have benefited from being presented as misconduct, rather than error, might have been that it in this way also had the potential of becoming a legal case of fraud. There had just then been a precedent of a first scientist convicted of fraud, the case of Stephen Breuning (Anderson, 1988). That case involved fabricated data with direct implications for the treatment of mentally retarded patients. It also involved a major cover-up by the sponsoring agency and mistreatment of the whistleblower. It was probably no accident that this case of fraud was one of those selected to be connected with the Baltimore case at the first Dingell hearing in 1988.

Furthermore, the more O'Toole could be presented as a whistleblower, the more the case would fit the category of 'misconduct' and the better the case would serve as a just-so story for Dingell, the protector of whistleblowers on federal fraud. The media enthusiastically cooperated in image-building in this case, presenting O'Toole as a big-eyed, truth-loving young researcher, who lost her job and lost her house because she dared blow the whistle on big names. This was repeated over and over again in various reports. It did not fit the image-building to say that O'Toole was in fact in Imanishi-Kari's laboratory on a non-renewable one-year post-doctoral training grant, and it would probably only have occurred to an avid supporter of Baltimore to scout out that she in fact sold her house and bought another (Davis, 1991b). Even so, that fact did not become part of people's "knowledge" about the case, which was increasingly becoming a very black-and-white affair.

But why did the scientists who had earlier backed Baltimore suddenly change their minds? Was it the very suspicion that some of the data in the paper had been fabricated that so deeply shocked Baltimore's peers? It is true that one can hear scientists say that fabricating data is the "worst sin" in science (personal communication). Still, it is hard to believe that the scientists at Harvard would have been so deeply taken with this suspicion as such. Rather, the question of data was closely linked to other matters: the tarnish to the public image of science (important for continued funding), the scientists' moral outrage about having been asked to support a fraudulent claim and having de facto participated in what seemed as a cover-up operation (Cairns, 1991; Ptashne quoted in Foreman, 1991), and finally, the scientific embarrassment of having bet on the wrong horse – all this *if* the misrepresentation of data was indeed fraud, not error, and *if* this in turn meant that the paper's claim was incorrect.

Whatever their own views in the matter, there were important strategical reasons for the decision to go against Baltimore: it was necessary to uphold the image of science, even if that meant sacrificing one of their own. The reputation of science, rather than

Baltimore, had to be saved: everyone had better act shocked at the faking of data, since it would be impossible to present the subtler points of scientific research to the general public, who had been trained to believe in the solidity of scientific claims, or to pedantic Congressmen who wanted science to keep its kitchen in order.

An offence had been done: a ritual of repair was expected. Baltimore had committed hubris: he had upheld a facade of science as if everything was in order, but meanwhile, it had turned out that all was not well, which made his words now sound like a deliberate cover-up. It seems that Baltimore played his expected part well at this stage of the affair: he recognized the need for a public apology, also to O'Toole, and spoke of the humility of a scientist in the face of data (Baltimore, 1991a). According to an editorial in *Nature* (9 May, 1991), this was the right thing to do, and Baltimore should now be left alone.

But obviously, it was hard for Baltimore to take on this new role of humility: at the first provocation concerning the correctness of the science in the paper (O'Toole's statement that she had been right all along about the untenability of the paper's central claim, O'Toole, 1991a, and Paul Doty's reproach that he had not met the normal standards of science by failing to take on his responsibility as a senior author to check the data, Doty, 1991), Baltimore snapped back into his first mode: he could indeed show that the results of the paper were being upheld, and this was the most important standard there was in science! (Baltimore, 1991 b,c). Thus we have here an interesting illustration of a switch from The Importance of Being Right to The Importance of Being Earnest and back again, which may show the inherent disequilibrium between these two modes in scientific thought.

The scientific critics of Baltimore, on the other hand, apparently switched to the Earnest mode and did not snap back – at least not in public. However, it seems that for some of them their change of mind was intimately linked to their conviction that Baltimore was in fact *wrong* (Ptashne quoted in Foreman, 1991; Gilbert quoted in Raush, 1992; Cairns quoted in Hamilton, 1991d; see also Greenberg, 1991c). For other critics, the concern may have been more "purely" ethical (Doty, 1991; John Edsall, U. S. Congress, Hearings, April 12 1988). In any case, the Earnest mode was obviously the plausible one, considering that the public image of science may be in danger. Also O'Toole seems to have been shifting between the Right and the Earnest modes: she has been presented by the media as Earnest, but it seems that she was as much driven by the wish to be proven Right (thus, being more of a typical scientist than a mere whistleblower for truth, after all). Even Feder and Stewart have oscillated between the two modes: in fact, one of the driving forces for them in this case, even if they may have seemed motivated largely by the Earnest concern, was in fact a wish to be proven Right (about the paper being fraudulent; Dolnick, 1994).

But the restoration of the public image of science was not only dependent on chastising Baltimore as a senior scientist who should have shown more responsibility (critics repeatedly pointed out that his behavior deviated from normal scientific standards). What was also needed was a public shift of the scientific community to the Earnest mode: a demonstration of its ability to police itself and its sincere interest in any internal help in this matter. This was achieved by continued praise of O'Toole for her courage as a whistleblower. Also, she was given a job as a researcher by one of Baltimore's Harvard critics, Mark Ptashne.

While "accepted standards" were thus invoked by scientists in the Baltimore case, in 1992, the National Academy of Sciences (NAS), the highest authority on science in the United States, finally produced its own long-awaited report on standards for science, entitled *Responsible Science*. Since much confusion in the Baltimore case was caused by the open-ended meaning of 'scientific misconduct' the report's position of misconduct is of particular interest here.

Responsible Science militantly defines misconduct in science as only "fabrication, falsification, or plagiarism in proposing, performing, or reporting research". It expressly states that 'scientific misconduct' should *not* refer to "other serious deviations from accepted research practices" (National Academy of Sciences, 1992: 5). According to the report, examples of "questionable research practices" (but not misconduct), are such things as maintaining inadequate research records, exaggerating findings, and misrepresenting speculations as fact. The report goes on to say that such practices "do not directly damage the integrity of the research process" (p. 6). Interestingly, it can be seen that the definition of misconduct that the report objects to is in fact just the one used by OSI and Feder and Stewart in their investigation of the Baltimore case (which is not mentioned in the report). The stated rationale of the National Academy of Sciences for its decision is that including also "other serious deviations from accepted research practices" in the definition of 'misconduct' may curb or discourage "novel or unorthodox methods" in scientific research, i.e. scientific creativity.

'Misconduct' for NAS does not either include "errors of judgment: errors in the recording, selection, or analysis of data; differences in opinions involving the interpretation of data; or misconduct unrelated to the research process" (p. 6) Again, there seems to be a clear correspondence to the Baltimore case. At the same time, the National Academy of Sciences here appears to be defending exactly some well-known practices already identified by Charles Babbage in his famous *Reflections on the Decline of Science in England* (1830): "trimming" (moving extreme data points closer to the mean) and "cooking" (selecting the best data), or other methods of scientific persuasion. If in the Baltimore case the investigators often refused to distinquish between error and fraud, in the NAS report the matter appears to be pushed in the opposite direction: almost anything goes, except outright data fabrication or falsification!

Thus, ironically, while the scientific community in the Baltimore case in public shifted from an emphasis on product to an emphasis on procedure, or from The Importance of Being Right to The Importance of Being Earnest, the Scientific Establishment, at least in writing, was strongly promoting a return to "science as usual." The question that nobody seemed to ask was, Is the ethical acountability now increasingly required from science *in principle* attainable in a system where it is more important to be a quick-and-dirty first than a conscientious – and slower – second?

When Baltimore resigned as president for Rockefeller in December 1991 it seemed that Dingell had succeeded in toppling one more university president and thus reasserted his fearful power. But did he really win? Assuming for a moment that his basic interest was in fact what he said it was: improvement of the scientific community's ability to police itself, let us examine what really happened. The "science auditors" Feder and Stewart, who had been allowed to conduct their inquiries for a whole decade and who had been so useful for Dingell were finally found to have overstepped their legitimate realm of inquiry and put back to work as "normal" NIH scientists (the reason given by NIH was that they had gone too far in attempting to test their new invention, a "plagiarism machine" on a historian who had notheing to do with NIH grant money). Despite a 33 day hunger strike by Stewart, their fraud office at NIH was finally locked. Dingell did not come to their rescue (Editorial, *Science News*, 16 October, 1993).

As to NIH's much-criticized Office of Scientific Integrity (OSI), for several reasons Dingell decided to dissolve it altogether. He established a brand new bigger office, this time called Office of Research Integrity (ORI). Scientists who disagree with ORI's decisions, can appeal to an Appeals Board at a higher level (the Department of Health and Human Services, overseeing NIH). Unlike the OSI, which was supposedly an "internal" scientific investigatory body, part of the the new and bigger ORI's staff are law-

yers, thus guaranteeing due process for accused scientists. Under the new rules, in order to be able to demonstrate fraud, the office would have to point to *intent* to mislead. Mere sloppiness or unintended mistakes do not count. In light of these more legalistic specifications, the burden of proof is now on the ORI to show willful intent – a more difficult criterion than merely pointing out erroneous results (for an illustration of this, see Anderson, 1993c).

Thus, one could claim that we are back full circle: the attempt to improve science's internal policing of itself, and NIH's role in this endeavor as a "super investigator" of misconduct above university committees, in the end resulted in a situation, where, paradoxically, the likelihood is increased that misconduct *cannot* be proven (although error can). Who won, then, Representative Dingell or the scientific community? If Dingell's aim was to set a precedent, only one case was needed. Did the Baltimore case send a strong enough signal to science to improve its self-regulation? Dingell's actions at the very least forced scientists to discuss standards for scientific conduct in public, and thus recognize the legitimacy of concerns about procedure as well as product. It also became clear that scientists can effectively use moral arguments as a weapon against an eminent fellow scientist. Whether this seeming new emphasis on the Earnest mode will have a more lasting impact on the conduct of science remains to be seen.

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