I. THE TYPES OF SCIENTIFIC MISCONDUCT

Negligence

There are two broad categories of scientific misconduct that must initially be taken into consideration. The first category is perhaps best classified as *scientific negligence*. For this classification, we will include those instances where scientists have provided erroneous information, but have not set out from the beginning with the intent to defraud. For these cases, not only is the public "fooled", but the scientist is also deceived. The scientist who experiences this *self-deception* is one who has no premeditated plans to be dishonest. The researcher is exposed as having human faults, a trait that may be considered by many to be inappropriate for the scientist to exhibit.

The false information promulgated by such erroneous research may not ever be discovered. This would depend on a number of factors; the primary of these being the relative importance of the work in that specific field of research. As a case in point, consider the cold fusion *fiasco* of recent years.(3) Is it reasonable to assume that such a fuss would have been made over erroneous results reported with regard to relatively "uninteresting" bits of research? It is most unlikely.

It is noteworthy that contemporary cases of proven negligence are not well documented in the pertinent literature. An exception to this observation is the aforementioned cold fusion problem. The details associated with the attempt by scientists to achieve cold fusion have been described elsewhere.(3) The controversy itself stems from the claimed success of achieving fusion at room temperatures by two scientists, B. Stanley Pons (University of Utah) and Martin Fleischmann (University of Southampton in England). Initially many of their contemporaries believed that Pons and Fleischmann achieved their results through experimental error and poor procedures; however, a panel of outside scientists has deemed that the research performed at the Utah National Cold Fusion Institute is scientifically sound.(4) So why all the fuss?

Pons and Fleischmann did not follow the commonly accepted procedures for announcing their experimental findings. It is standard practice for scientists to submit their experimental methodologies and collected data to a scholarly journal for review and eventual publication. Evidently, Pons and Fleischmann did submit a paper to the journal Nature; however, it was rejected on the grounds that it contained insufficient detail regarding their experiments. Thus, in their haste to go public with the claim, their research methods and results were not reviewed by a panel of their peers prior to being released to the public via the media and popular press. They didn't

even have the opportunity to perform basic control experiments of their own.

Why did they release information about their work prematurely? The opportunity to claim priority for the discovery of cold fusion was undoubtedly an important reason. In another frequently publicized accusation of misconduct (perhaps originally stemming from an act of negligence), it has been suggested that Robert Gallo may not actually have priority in his claim to have co-discovered the cause of the AIDS virus.(5) Initially, one might wonder "Why make such a fuss? Why does it really matter who was first in discovering the cause of AIDS?". Two primary motivating factors have been illuminated: gain of notoriety (which includes the soon-to-follow prestige and bolstering of reputation) and financial gain. Gallo's group was able to patent their sensitive method of detecting the AIDS virus. Therein lies a portion of the financial motivation. Granted, many scientists are personally motivated by the self-satisfaction and excitement associated with doing novel research. Many practicing scientists are extremely dedicated to their work; however, being a scientist is also a **job**. Scientists must foster the growth of their careers no differently than persons in other vocations. They must prove that they are proficient in the performance of the tasks that they set out to accomplish. A claim of priority in a new discovery is strong supporting evidence. Returning to the cold fusion example, it has been suggested that administrators at the University of Utah encouraged Pons and Fleischmann to release information about their experimentation prematurely. The university would have shared not only in the prestige associated with the claim, but they stood to gain significant financial allocations from the state and federal governments as well.

Would things have turned out differently if Pons and Fleischmann had waited for the completion of the peer review process? This is an impossible question to answer. It may only be surmised that their peers would have recognized the paper describing the cold fusion process to be flawed. However, this might not have been the case. It has been noted that "...in physics, textbook science may be about 90% right whereas the primary literature [published research] is probably 90% wrong.".(6) There are many documented cases illustrating the ease with which unsound research papers have succeeded in achieving publication.(1) An additional difficulty associated with the cold fusion case rests with reproducibility of Pons and Fleischmann's experimental work. Soon after they released the details concerning their physical apparatus and experimental methods, other laboratories attempted to reproduce this room temperature fusion. Some laboratories claimed success and others reported failure. The jury is still out regarding the likelihood of room temperature fusion. Even as recently as March 27, 1992 (the third anniversary of the announcement of cold fusion), advocates for cold fusion research "...bitterly attacked the scientific establishment for its rejection

and disregard of the controversial phenomenon.".(7) The same article reports that Pons recently told Italian journalists that he expected a cold fusion demonstration device to be unveiled before the end of the year. As for the possibility of cold fusion, only time will tell. If it actually works, Pons and Fleischmann will assuredly not be remembered solely for their negligence.

Deliberate Dishonesty

The second category of scientific misconduct involves the deliberate attempt by a scientist to be dishonest. Included are premeditated acts of fraud that may include forged or fabricated data, falsified or invented results, plagiarism, piracy, hoaxes, and other such malicious acts.

Two recent texts concerned with instances of scientific misconduct have been quite explicit in defining the types of premeditated cheating that may occur.(1,2) Broad & Wade attribute the descriptions of "trimming", "cooking", and forging to Charles Babbage's 1830 text *Reflections on the Decline of Science in England*. It would seem that there were concerns about premeditated scientific misconduct well over one-hundred and fifty years ago. *Trimming*, according to Babbage, would essentially be the act of forcing observations to fit a desired mean by removing portions of those data points that deviate in excess and adding these portions to those data points that deviate in the other extreme. Babbage defined *cooking* as the process of making many measurements and then only reporting those choice measurements that are deemed satisfactory by the appropriate standards. Finally, he describes *forgery* as the act of recording fictitious results by one who wishes to build a scientific reputation.

Kohn attempts to translate these terms into contemporary jargon in Chapter 1 of his text.(2) Cooking, he writes, would now be recognized as *finagling* and trimming will be recognized as *massaging the data* or *fudging*. He also notes that plagiarism may simply be considered as another type of forgery.

A thorough discussion of the documented cases of scientific dishonesty will not be found here. Interested readers are encouraged to consult the following texts: Betrayers of the Truth, by Broad and Wade, and False Prophets, by Kohn.(1,2) Both texts cite not only contemporary instances of misconduct, but many historical cases as well. Bauer has written that "It is difficult enough to prove a living person guilty of deliberate deceit, even greater caution is appropriate in finding guilty those who came before.".(6) Heeding this warning, we will consider as an example of scientific fraud the eminent and contemporary instance commonly referred to as the Baltimore case.

In 1986, a researcher in the field of immunology, Margot O'Toole,

raised questions about a paper submitted to the journal *Cell* by Thereza Imanishi-Kiri. At that time, O'Toole was a postdoctoral student in Imanishi-Kiri's laboratory and she lost her job as a result of this *whistle-blowing*.(8) David Baltimore, former president of Rockefeller University, shared responsibility for the paper as one of five authors. Baltimore discussed the disputed data with Imanishi-Kiri in 1986; however, the records weren't scrutinized due to reported disorganization of the notebooks and supporting documents. Baltimore staunchly defended the work of his co-author; a retraction of the paper in question was not initiated. O'Toole was not satisfied. She brought her concerns to immunologists at Tufts University and in June of 1986 they concluded that there was no evidence of foul play. This conclusion was supported by a following review at MIT. O'Toole persisted. In January 1989, an NIH panel investigated the matter. Although some questions were raised regarding the acquisition of the scrutinized data, the panel concluded that the paper was essentially sound. Baltimore continued to support the work of his colleague and placed 100% trust in the integrity of the *Cell* paper.

In May of 1989, Baltimore, Imanishi-Kiri, O'Toole, and others participated in official hearings before a U.S. Congress investigations subcommittee. The Secret Service offered potential evidence of foul play obtained from forensic analyses of ribbon ink, printer, and paper upon which the data in question were produced. Baltimore dismissed this evidence and continued to adamantly defend his colleague. He provided a stirring commentary regarding these hearings for the periodical Technology Review. Titled "Self-Regulation of Science", Baltimore offered the view that "The worth of a piece of research is determined when scientific peers attempt to reproduce or, more commonly, extend an experimenter's results.".(9) He argued that there were those who

> "want to substitute criteria and methods more appropriate for ferreting out corporate fraud than for evaluating a scientific investigation. They wish to impose rules that would not merely regulate science but regiment it. This poses a danger to the integrity of the scientific process.".(9)

It is ironic that Baltimore would agree in his short essay that "We must be alert to indications of fraud and misconduct, and ready to discipline the perpetrators.". In March of 1991, NIH investigators concluded that Imanishi-Kiri forged entire sets of data during the years from 1986 to 1988 in order to support her *Cell* paper. The investigation didn't resolve the question of whether simple error, mistakes, or fraud led to the original discrepancies in the paper. Once it came under scrutiny however, Imanishi-Kiri began to systematically fabricate data to support it.(10) In March of 1991, Baltimore finally requested that *Cell* retract the paper in question. In May, he issued a statement to NIH that included an apology to

Margot O'Toole.(11) He cited that his defense of his colleague was fueled "by my respect for Dr. Imanishi-Kiri'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.". It is tragic that Baltimore would argue so adamantly for the self-regulation of science and yet not make a positive contribution to the process himself.

Those who read Baltimore's literary accounts will find that he is a very careful writer. Many of his statements express keen insight into the workings of science as an institution. At the same time, his handling of the affair as portrayed by the media is a terribly embarrassing example of 'how not to handle an allegation of scientific misconduct'. Dr. Baltimore wanted to be such a strong spokesperson for his colleagues (and seemingly *defend the honor of science as well*), that he neglected to mind the affairs at hand. He made extraordinary statements before NIH investigators; such as "You can make up anything that you want in your notebooks, but you can't call it fraud unless it's published."!(10) He responded with hostility toward the initiators of the congressional investigative committees in which he was invited to participate. Baltimore's credibility was soon damaged if not completely destroyed. He has since resigned from his position as president of Rockefeller University. Even Nobel laureates share human character flaws and motives -- both his personal and scientific reputations have undoubtedly suffered dearly following this spectacle. A full literary account of this affair will undoubtedly provide for interesting reading.

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