

INTRODUCTION TO SESSION #2:

HOW INSTITUTIONS HAVE HANDLED INVESTIGATIONS OF PLAGIARISM

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This session focuses on the handling of cases of plagiarism and theft of ideas. Four speakers will talk about institutional experiences in handling a case or multiple cases of plagiarism and the difficulties that have arisen during the context of those investigations.

For your information, plagiarism and theft of ideas are the most common of the allegations of possible scientific misconduct that are reviewed by the Federal investigative offices and the research institutions.

In the annual reports that the Office of Research Integrity (ORI) received in 1990, in the context of institutional applications and grants from PHS, 70 institutions reported handling 58 cases related to plagiarism; that was 22 percent of the total allegations and cases reported. In 1991, 35 institutions reported that 26 percent (30 cases) of their total load involved plagiarism.

The National Science Foundation (NSF) Office of Inspector General (OIG) semi-annual reports for the last three years, and a 1993 *Science* article by Dr. Donald Buzzelli of their staff, indicated that about 70 of their 124 cases during these three years involved plagiarism or failure to credit and authorship disputes. So "plagiarism" is a very commonly made misconduct allegation.

ORI review and NSF review of such cases make it clear that institutions are often not finding scientific misconduct in such cases. Many cases do not seem to rise to a level serious enough to call it "misconduct," or they are resolvable outside the context of misconduct and do not actually amount to allegations of "plagiarism." It is particularly difficult when two people have worked together as collaborators, investigator/coinvestigator, or student/mentor, and then they had a falling out; it has been very difficult for an institutional committee to try to sort out whose ideas and whose words were whose, who "owned" them in an intellectual

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context. And very often, these are unresolvable questions, and the cases have not led to findings of misconduct. And ORI and NSF's OIG have generally concurred in those institutional determinations.

It is also apparent that institutional officials are making difficult judgments about the "minor" copying of words without use of quotation marks or without appropriate citation. The reference to the source may be cited, but it is not cited in the place where the words were copied. Whether the matter is serious enough to come to a conclusion that an investigation is warranted or that a finding of misconduct should be made is a difficult one. And in most of those cases, in fact, such a finding is not made by the institutions. There might be reprimand given for a failure to credit adequately, but not a finding of misconduct. Or the case may be judged to be an honest error, that someone has known the reference but just not included it, as an error or an oversight in preparing the paper.

Even in cases where the copying was found to be significant, institutions have often given a reprimand and not come to a finding of misconduct. However, in other cases, like the three speakers in this session will discuss, there have been findings of scientific misconduct made and some severe institutional sanctions imposed. In two of these cases, additional administrative actions were taken by the Public Health Service (PHS) after review of the institutional reports by ORI.

Our hope is that you in the audience will gain from a sharing of these institutional case studies. If you have others that you want to share, don't hesitate to do so. There may be some common ground here in approaches to the analysis of such cases, the significance of them, and the logic used in making the determination. This may help in the future to ensure that time and resources are not unnecessarily spent and that the investigative process of assigning misconduct is not abused or misused by disputes that really do not belong in this context. That is a fine line, which we know institutional officials have had to draw repeatedly in such cases.

NOTE: The *ORI Newsletter* (Vol. 3, No. 1, December 1994) provided guidance on how the definition of plagiarism is applied in ORI's cases. ORI generally considers plagiarism to include both the theft or misappropriation of intellectual property and the substantial unattributed textual copying of another's work. Substantial unattributed textual copying of another's work means the unattributed verbatim or nearly verbatim copying of sentences and paragraphs which materially mislead the ordinary reader regarding the contributions of the author. ORI generally does not pursue the limited use of identical or nearly-identical phrases which describe a commonly-used methodology or previous research because ORI does not consider such use as substantially misleading to the reader or of great significance. Note that many allegations of "plagiarism" involve disputes among former collaborators who participated jointly in the development or conduct of a research project, but who subsequently went their separate ways and made independent use of the jointly developed concepts, methods, descriptive language, or other product of the joint effort. The ownership of the intellectual property in such situations is seldom clear, and the collaborative history among the scientists often supports a presumption of implied consent to use the products of the collaboration by any of the former collaborators. ORI generally considers such disputes to be authorship or credit disputes, not "plagiarism" within the PHS definition.

ACADEMIC PEER REVIEW IN INVESTIGATION OF PLAGIARISM: A CASE STUDY

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Acts of fraud and misconduct in research, although rare, do occur. Universities and research institutes need effective mechanisms for the investigation and adjudication of allegations of fraud and misconduct. However, such processes are seldom straightforward and are usually fraught with problems. This report describes the investigation of an allegation of plagiarism conducted at Tulane University. The intent of this paper is to describe the procedure in the context of a specific case, so that some of the strengths and weaknesses of the process as a potential model can be discussed.

Case background

A faculty member at Tulane made a formal complaint to the appropriate dean that another faculty member had plagiarized passages and a figure from a publication of the complainant. In accordance to University's "Policy and Procedures to Deal with Fraud in Research," the dean sanctioned an inquiry. It was determined from the inquiry that sufficient evidence existed to warrant a full investigation into the allegation of plagiarism. The dean then requested that the standing Grievance Committee, as stipulated in the Policy and Procedures, act as an investigatory body of the allegations involving fraud and misconduct in research.

The Grievance Committee consisted of six voting members, one representing each of the five departments and a member-at-large elected by the faculty, as well as two non-voting *ex officio* members, the President of the General Faculty and the representative to the University Senate Committee on Academic Freedom Tenure and Responsibility, both of whom are elected by the faculty.

The charge of plagiarism included a second faculty member who was a co-author with the respondent on the paper in question. However, not enough evidence was found during the inquiry stage to warrant a full investigation of the co-author. During the inquiry, the respondent had accepted full responsibility for the writing of the paper.

Upon receiving the charge to conduct an investigation, the Grievance Committee initially reviewed the University's "Policy and Procedures to Deal with Fraud and Research," which had been drawn heavily from the "Framework for Institutional Policies and Procedures to Deal with Fraud in Research," prepared by the Association of American Universities and the National Association of State Universities and Land-Grant Colleges. The Policy also took into consideration the 1989 Public Health Service (PHS) regulations, "Responsibilities of PHS Awardee and Applicant Institutions for Dealing with the Reporting of Possible Misconduct in Science," and the 1987 National Science Foundation regulations, "Misconduct in Science and Engineering Research." The Committee also met with the University counsel to have questions about the Policy and Procedures answered.

The function of the Grievance Committee as an investigatory body was to gather evidence on the allegations, to produce a finding, and to recommend disciplinary actions. The investigative process was governed by the following principles: confidentiality must be preserved to protect the respondent's rights, committee members with conflict of interests must recuse themselves, and the burden of proof rests with the University and should be based upon a preponderance of evidence. However, the Grievance Committee was not bound by strict rules of legal evidence.

The investigation was conducted in three phases. The first phase of the investigation was to develop a working definition of plagiarism and to establish criteria by which plagiarism could be evaluated. During the second phase, the Committee examined the evidence in hand and determined the extent of duplication between the complainant's paper and the respondent's paper. The Committee then sought, as a third phase, some outside opinions on whether the duplicated passages in question constituted plagiarism.

Phase I--Definition of plagiarism

Before gathering and examining the evidence, the Grievance Committee decided to develop a definition for plagiarism and to determine criteria by which to evaluate plagiarism. The Committee consulted with several knowledgeable individuals outside of the University in regards to a definition. These sources included: a staff member of the Office of Scientific Integrity at the National Institutes of Health, former and current journal editors, and a member of the National Academy of Sciences' Panel on Scientific Responsibility and Conduct of Research. These contacts provided the Committee with documents and additional information on the definition of plagiarism, along with criteria for evaluating plagiarism.

The search for a definition revealed that no legal definition of plagiarism *per se* exists. Furthermore, plagiarism is not a "criminal act," but an "academic sin." The consensus definition is probably best represented by the following: "Taking over the ideas, methods, or written words of another, without acknowledgment and with the intention that they be taken as the work of the deceiver, is plagiarism" (from the "Statement on Plagiarism" published in the Policy Documents & Reports, 1990 edition, of the American Association of University Professors).

Criteria by which plagiarism could be judged and evaluated were also developed. Some of the issues that the Committee felt should be addressed in determining whether plagiarism has occurred and to be used in determining disciplinary recommendations were: (1) the extent and frequency of the duplicated passages, (2) the intent to defraud the source authors, (3) the nature of the source material, (4) the rank and training of the person committing plagiarism, and (5) any past incidences of plagiarism of the respondent.

Intuitively, large passages encompassing several paragraphs of duplicated material would more likely constitute an act of plagiarism than a few scattered sentences or phrases. In addition, the nature of the material being copied should also be evaluated. For example, questions in regard to whether the plagiarized passages represent a theft of original ideas or thoughts (as opposed to the copying of words) along with the nature of the source material need to be addressed. In other words, did the plagiarist intend to defraud or devalue the source authors, or could the act be attributable to carelessness? In addition, the circumstance surrounding the act of plagiarism, such as rank and training of the individual and whether the plagiarism was an isolated incident or part of a pattern, could also affect the seriousness of the act.

Phase II--Examination of evidence

Careful examination of the two papers in question revealed that a total of five separate passages in the paper by the respondent had essentially duplicated writing of the complainant. The first of these passages in the paper by the respondent consists of 3 sentences involving 75 words. The second passage is almost 2 paragraphs in length involving 200 words. The last three passages involve single sentences of varying length. There was no documentation of verbatim usage and no immediate citation to the source article directly associated with any of these passages. However, the respondent had cited the complainant's paper numerous other times. The Committee also examined all other papers cited in the respondent's paper, and no other incidences of duplicated passages were found.

A figure from the complainant's paper had also been duplicated in the respondent's paper without documentation of the source of the figure. This figure was not a data figure, but expressed a concept which was often used by others in the field to illustrate the methodology. The respondent presented evidence that the figure was in the public domain and therefore needed no documentation. The Committee concurred that the concept represented by the figure was in the public domain, but that all of the previously published versions of the figure contained sufficient variations. The Committee concluded that the respondent had duplicated an individualized presentation of the complainant and the source of the figure should have been cited.

Phase III--Outside opinions

The Committee solicited outside opinions from two individuals knowledgeable in the standards of scholarly writing (a chair of an English department and an editor) as well as one expert knowledgeable in the field of research relevant to the case. Copies of papers, in which any

identifying references to the respondent were blacked out, were sent to these individuals for evaluation. In addition, the respondent had solicited opinions from four individuals also knowledgeable in this particular field of research. All of the individuals who read both papers indicated that extensive passages had been copied without proper attribution, although not all of them were willing to call the act one of plagiarism. The individuals knowledgeable in the relevant research field all indicated that the duplicated passages did not represent an "intellectual theft" in that the concepts illustrated by the figure and passages had been in the literature for several years.

Previous acts of plagiarism

During the investigation the Committee also became aware of another act of plagiarism committed by the respondent. An inquiry had been previously held in regard to an allegation that the respondent had submitted a paper for publication that contained plagiarized material. In this case, a figure and one sentence had been duplicated. The respondent had signed a statement admitting to the plagiarism, and he waived his right to a full investigation and hearing. He remedied the situation by altering the manuscript, so proper attribution to the source authors had been given. A letter of reprimand was placed in his file as a disciplinary action.

Hearing and findings

The final phase of the current investigation was to provide the respondent with all of the documentary evidence gathered by the Grievance Committee. The respondent provided the Committee with a written statement and rebuttal concerning this evidence. A hearing was held in which the respondent presented his case. The complainant and one of the respondent's co-authors were called as witnesses. The committee then met in a closed session to discuss the case and come to a final decision. The Policy and Procedures mandated that the outcome of the investigation should fall into one of the following: (1) a finding of fraud, (2) a finding of serious scientific misconduct short of fraud, (3) no scientific misconduct but serious errors were committed, or (4) no fraud, misconduct or serious errors were committed.

The Committee felt that some form of fraud, misconduct, or error had been committed. This was based upon the amount of duplication and the fact that everyone who had examined the material felt that proper attribution had not been made. That this act constituted fraud was ruled out, since concepts contained in the duplicated passages and figure were widely known and used by other researchers in the same field. In addition, the respondent had favorably cited the work of the complainant in other parts of the paper, which indicated to the Committee that the respondent had not intended to defraud the complainant.

In deciding whether this act of plagiarism constituted misconduct or scientific error, the Committee evaluated the incidence of plagiarism in regards to the criteria developed earlier (Table I). Based upon these factors, the Committee ruled that an act of misconduct had been committed. As discussed above, the Committee felt that the amount of material duplicated was moderate but that the respondent did not intend to defraud the complainant of original thoughts and ideas. However, plagiarism had obviously occurred and although the respondent had not intended to defraud the source authors, the act of duplicating the figure and passages must have been intentional. The source material was a previously published article, and standard scholarly practice dictates that the verbatim use of previously published material must receive proper attribution. The fact that the respondent had admitted to a previous incident of plagiarism and that the respondent was a full professor with tenure contributed significantly to the decision that this act of plagiarism should be classified as misconduct rather than scientific error. The Committee felt that a person responsible for directing graduate students and junior faculty must not only be familiar with standard scholarly practice, but the person also must always perform accordingly.

In light of the finding of serious scientific misconduct and the previous incidence of plagiarism, the Committee recommended that dismissal procedures be initiated. In addition, the Committee recommended that letters of apology sent to the complainant and co-author and that a letter of public apology be published in the journal in which the respondent's paper had been published.

Discussion

Faculty peer review in investigating cases of scientific misconduct has strengths and weaknesses.

TABLE I
EVALUATION OF PLAGIARISM

CRITERIA	WEIGHT ^a
Extent and Frequency	++
Intent to Defraud	+
Source Material	++
Rank and Experience	+++
Previous Plagiarism	+++

^a Weight refers to the severity of the infraction for each criteria in this particular case and not to the

One of the major strengths lies in the nature of peer review. The best people to judge whether some form of fraud or misconduct has been committed are those who also work within the same confines of academia.

However, especially in the case of plagiarism, what constitutes misconduct may be viewed quite differently between individuals from diverse scholarly disciplines. For example, in some disciplines the actual words may be more valuable than the concepts expressed; whereas in other disciplines, the concepts, theories, and ideas may be more highly regarded than the actual words. It is therefore important that investigatory bodies be comprised of individuals from a variety of disciplines and that factors in addition to the passages in question be evaluated during the investigation into alleged plagiarism.

One potential weakness in the peer review of misconduct is the inexperience that academic faculty will generally have in dealing with issues of misconduct, due to the rarity of such cases. This potential lack of experience necessitates that clear and unambiguous guidelines exist on how to conduct such investigations and proceedings. Overall the procedures followed in this particular case seemed adequate. The only major shortcoming was a lack of a definition for plagiarism. To alleviate this shortcoming, the Committee made a considerable effort in researching the topic of plagiarism before proceeding with the investigation. However, this raises the question as to whether an investigatory body has the right to define the "crime," as well as to "judge it and pass sentence." To avoid this potential conflict of interest, it may be necessary to have a universally accepted definition of plagiarism and criteria for evaluating plagiarism as part of the procedures for conducting investigations into allegations of plagiarism. Developing such a definition will probably be difficult, since different disciplines will likely view plagiarism differently. Definitions and criteria for the other forms of misconduct, such as fabrication and falsification, may also be in order.

Other issues related to an operational definition of plagiarism also arose during the investigation, but they were not resolved. For example, is there a standard or definition by which claims of "material in the public domain" can be assessed? Similarly, are there defined and accepted standards for what constitutes adequate acknowledgement? Investigations into plagiarism may come down to determining whether adequate acknowledgement was given or whether the material was in the public domain. Another related unresolved issue is whether proof of intent to defraud the source authors needs to be incorporated into the definition of plagiarism, and what would constitute such proof. Although the definition of plagiarism appears quite simple and people generally feel they would recognize plagiarism when they see it, several complex issues must be dealt with during misconduct investigations.

Objectivity is another potential weakness in the peer review process of scientific misconduct. In this particular case, the charge of plagiarism was filed in conjunction with a grievance involving a termination. There is a danger that these other circumstances could exaggerate the charge of plagiarism by the complainant. This can be especially problematic in cases where a standing grievance committee is responsible for hearing both the grievance complaint and for conducting the investigation into the allegation of plagiarism. It is extremely important that the two cases not be confused.

In this instance, the investigation into the charge of plagiarism was conducted in its entirety before hearing the grievance case on the charge of unjust termination. If separating the cases temporally is not possible, it will be necessary to establish which issues are involved in the allegation of plagiarism, before gathering evidence and conducting a hearing and to exclude those issues not related to the allegation from the investigation.

The other aspect of objectivity concerns the fact that universities are often divided into smaller schools that function as administrative units. In many cases the members of grievance committees will know the persons involved in cases involving misconduct. This opens up the possibility that the personalities of the complainant and respondent may be confused with the allegations. Objectivity may also be compromised in circumstances where members of the grievance committee learn of aspects of the case before formal charges are filed. In addition, there may be a tendency to cover up such cases to save embarrassment to the school or university. Establishing standing committees which include a member or members from outside of the school or university may help to alleviate some of these potential lapses in objectivity.

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AN INSTITUTIONAL HANDLING OF TWO ALLEGATIONS OF PLAGIARISM

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I appreciate the opportunity to be here for this Conference. One observation from those of us "working in the trenches" is that sometimes you have the feeling that you are the only one dealing with these situations. Then you come here and hear virtually the "same case" with which you have dealt described by somebody else at another institution. However, I want to emphasize that it is my view that sometimes we can get "carried away" with the "bad cases." We forget that there are a lot of scholars out there, working very hard, doing very good work in a very ethical way.

For example, at Ohio State University the faculty is about 5,000. Of them, I think maybe there have been six or eight cases over the last five to six years of significant misconduct. Now that is not to say that is not important, but it is important to keep in perspective that we are talking about a relatively small number of cases in a "sea" of what otherwise is very ethical behavior.

My own contact with this area goes back only a relatively short time. After I had been Dean of the Law School, I returned to the law school faculty. About three years later the President of the University asked me to temporarily fill in as General Counsel, where I ended up for about three and a half years. That was my first contact with these kinds of cases, but it was very peripheral; I was called in only on an occasional situation that needed a legal opinion. Then when I resigned that position and returned to the faculty, the Vice President for Research asked me to continue to handle scholarly misconduct cases and also conflict of interest situations a little over a year ago. While returning to a full-time faculty position, I find myself now working at least half time as the Research Integrity Officer for Ohio State.

I want to talk in detail about one case, superficially about another case, and then touch on a few other issues that I have observed and that I think are worth mentioning in this kind of a context. The first case is closed and is the subject of one of the ORI press releases, so I can talk fairly openly and candidly about this case. The second case is not yet closed, and so I will be a little more guarded in what I say about that case.

The first case involved a faculty member who was accused of plagiarizing material from a grant application. The faculty member accused here was a very well known member of our faculty in his field, who had a lot of postdoctoral fellows as well as graduate students working with him. The accused had received the application as a member of an NIH peer review committee. He never denied that he had seen the application. The accusation was that some of that material (it was unclear how much and how important it was) surfaced about a year or two years later in one of his own applications to PHS. The importance of the material was arguable, as being not terribly central to the research involved. It was background or related material not directly related to the subject of the research grant, so the importance in the scale of plagiarism arguably was relatively minor.

The accused initially denied the plagiarism. But when the inquiry committee came down with a fairly clear finding that the similarities simply were beyond what was acceptable as "accident" or "coincidence," the faculty member said that he had looked back further to see what exactly had happened and then came forward with the following version. He occasionally would share an application that he was reviewing for the funding agency with selected postdocs who were working with him, with the idea that for them it was an educational experience; the postdocs would see the process and would learn from it. In this case he had shared the other scientist's NIH application with a postdoc, because it was generally in the area that the postdoc was working in. He also required his postdocs, when they left their position at Ohio State, to file a report on where they thought the research in the lab ought to go over the next six months or so.

Now the accused faculty member claimed that the postdoc had essentially plagiarized the original application in the report that he filed with the faculty member. Then the faculty member reproduced the plagiarized material of the postdoc in the new application being submitted to the Public Health Service without, the faculty member said, knowing of its source. So it was a two stage situation.

Now the difficulty and special twist here is that through the whole process and even up to today, the faculty member involved has refused to disclose the identity of the postdoc. He says that this is a young person and that the young person should not be charged with this problem and should not have to carry it with him or her for the rest of his/her career. Therefore, the faculty member refuses to identify the postdoc. He also told the committee that he instructed the postdoc that he was going to refuse to do so; therefore, if the postdoc were contacted, he/she should simply deny any information about it.

So the issue that the committee was faced with was, first, a fairly clear misuse of confidential information from the peer review process for grant applications and the need to make an assessment as to how serious a violation it was. Second, there was the arguable plagiarism of the postdoc's report, and there was some disagreement as to how clear it had been made to the postdocs working for the faculty member that their work might be lifted and used in his future applications without their consent. Third, there was a problem, if nothing else, of the carelessness

of the faculty member in tracking down sources of the material in the application that he was submitting. Fourth, there was the refusal to reveal the identity of the postdoc and how to deal with that. Of course, upon that issue, to some extent, the credibility of his whole story revolved. Did you believe the faculty member about the postdoc being responsible or not?

Now the committee on the fourth issue ultimately decided they did not care, that it was unimportant, because the faculty member was guilty. I should add that, right from the start when he came forward with this version, he took full responsibility and said, "It is my fault." So there was no real question of guilt. The question was the severity of the guilt. And whether one could say, "Well, it was just a couple of happenstances that coincided and gave rise to a violation, but a not very serious one." To buy that argument, you had to agree to his credibility 100 percent, since he would not identify the postdoc to verify the story. Or, you could say, as the committee ultimately did, "This is a fairly serious violation; we find plagiarism, we find misconduct, and we recommend sanctions."

So the identity of the postdoc became irrelevant. Now quite frankly, ORI pushed the university fairly hard to try to identify the postdoc, because they were troubled by this same problem. How severe the PHS sanctions should be turned, to some extent, on the credibility of this story in their view. The university made a choice that it was not worth the effort to try to track down the postdoc to verify or not verify the story. We were assuming the worst; therefore, from our point of view, it was not necessary to track down the postdoc. Anyway, the faculty member had told us that it would not do any good, because the postdoc would not be honest with us.

As it turns out, there is no easy way at Ohio State to track down the identity of a former postdoc, among as many as 25 or 30 postdocs that we would have to try to narrow down and identify, perhaps with no ultimate conclusion. The university reached the conclusion that it was not worth the time, expense, and effort for what might well be a useless enterprise anyway. So we reported back to ORI that we were taking the faculty member at his word and putting the full responsibility on him. And after ORI pushed and talked to him, they closed the case, accepting the finding and imposing PHS sanctions on him.

The second case also involved the misuse of a confidential peer review application. Here a datum, a particular formula, was allegedly appropriated from the original grant application and appeared in a printed article by the faculty member. The complaint came to us from the funding agency on the basis of a misuse of the peer review confidentiality process. After consulting with experts in our investigation, it appeared to be marginal whether the precise information was something that was "in the public domain." Anybody could have or might have known about it, as an easily derivable formula, and therefore it may not clearly have come out of the grant application.

Moreover, the faculty member accused here presented a plausible version of where he had gotten the data. He asserted that it had been given to him by a colleague in Germany, as the result of some work by a Japanese individual four or five years ago; he presented some documentation to

support that claim, although it was "soft."

The complication was, when we started checking, that it just did not quite fall into place. The person in Germany said, "It's something I might have told him back in 1989 or 1988, but I don't really remember." The Japanese person and the document that it was supposed to have come from could not be found. Moreover, the work of that person, which was never published as far as we could tell, did not quite agree. There was enough difference to think that it probably did not come from that source.

Incidentally, this allegedly plagiarized data was in the introductory, background part of the paper, sort of the literature survey part, not in the central core of the paper. When the investigative committee started looking at this carefully, we discovered that the structure of about six or eight paragraphs in the two documents were very similar, but not identical. There were several identical sentences, but many were slightly different from the application. But the real "kicker" was that the footnotes were identical; six or seven of the footnotes were absolutely identical. The faculty member's excuse was, "But those are the standard references that anybody would use in doing that background material." Then we investigated the references and discovered that in virtually every footnote there was at least one spelling mistake, a mistake in a person's initial, or a mistake in the page number cited. There were identical mistakes in both documents, so this convinced the committee that there clearly was plagiarism.

I think the questions that this case raises are the following. First, it could have been the result of mere carelessness. As Dr. Bivens said, it is not clear what is plagiarism when one is dealing with this kind of situation, because it is sort of common, "public forum" material, and it could get incorporated simply by carelessness. The second issue is the violation of the confidentiality of the peer review process, and our committee took that very seriously in this case. Other factors to consider included our committee's being aware of the fact that this was a very senior member of our faculty who handled a lot of graduate students and, therefore, was a role model to the next generation of scholars. The committee felt that this was a very significant problem, to have that kind of role model engaged in even relatively minor plagiarism. And finally, and perhaps most important, it was a "repeat offense." The faculty member had been accused and found guilty earlier of plagiarism, and therefore, the committee felt that made this a more serious case.

One of the procedural issues is at what point in the process of investigation do you inform the committee that there was an earlier violation or an earlier case? We made the decision to go through the process in this case and not to inform the committee until it had reached its findings on the merits and then to bring in the other case, in terms of setting sanctions. I think that can be argued both ways; I do not think it is clear cut how you handle that. But our concern was that we wanted a clear finding on the merits in the second case. We did not want it "corrupted" by the possibility that a violation was found the second time because there had been a previous violation. In other words, we were concerned about prejudicing the review process. In this case it worked, because the committee came to the decision that the second case was a serious violation without

any knowledge about the first. The first violation came into play in considering the sanction recommendations.

Now I want to run through fairly quickly six additional problems that I have run into in my brief sojourn in this area. They raise difficult problems for those of us in the trenches.

One is the problem of the point that a "statute of limitations" runs on these cases. One case that we had involved a graduate student who got a Ph.D. about nine years ago; recently it was discovered that she had plagiarized in the sources part of her dissertation. The problem was what the University ought to do about it. We put it through our student discipline process and considered taking the Ph.D. away as the ultimate sanction. But the faculty committee decided to allow her to rewrite that portion of the dissertation that had been plagiarized. Part of that decision rested upon the fact that it happened nine years ago. She had been out there working in a job, which incidentally required a Ph.D., for the nine years. In the committee's view, it was a relatively minor case of plagiarism, not meriting "the death penalty." So they decided to correct the literature. She rewrote and resubmitted that portion of the Ph.D. dissertation. It is now on file at the Ohio State University library with a notation attached to it, which says that the first version was plagiarized and this is a revised version. So it is on public record what happened, although the decision was made not to withdraw the Ph.D. degree.

The faculty member who was the original advisor is incensed with that. He views the decision as a violation of his academic freedom, and he would have the university withdraw the degree. A couple of problems with that are presented, however. One of the problems is the nine years delay; at some point, the statute of limitations runs out. Secondly, there is some sentiment that the faculty member should have caught the plagiarism nine years ago, and the question of who really ought to be sanctioned at this point is at least an open question.

The second area of problems that I have run into is what to do with faculty on relatively minor plagiarism issues. We are working with a university discipline process that provides that, if you go beyond simply correcting the research or dealing with the research element--if you want to take job action against the individual--then the disciplinary rules provide for only two sanctions, letter of reprimand or de-tenurization. In other words, a slap on the wrist or "the death penalty," and there are no degrees between that. I think that poses a really difficult problem for how to handle relatively minor cases. We are in the process at Ohio State of trying to get our university rules changed to give us much more flexibility in the kind of job action we can take.

I would like to see us have "publicity" as one of the degrees of the sanctions, whether we administer a private reprimand or a public reprimand. A minor case can be handled, particularly when you think that the person can be rehabilitated or where there is not much chance of potential repetitive behavior, without a lot of publicity. That way you do not ruin a person's career. On the other hand, in serious cases, I would think a public reprimand at a minimum, and it probably ought to go much further than that, would be appropriate.

Another area of concern are cases that the universities hear about, get suspicious about, but find that no one is actually willing to come forward and "charge" someone. The problem is what do we do about such potential cases? Sometimes I think the allegation is clear enough that the institution can take its own the initiative and institute the investigation. But I have people who come to us on a confidential basis and seek advice on whether they ought to do something. My experience is that they swear me to secrecy before they discuss the problem with me. Then I am left with the dilemma that, if I think it is a serious problem, whether to breach the confidentiality and start the process, or whether to let it ride until a complainant is ready to come forward. That is an ethical dilemma that we have in our position. If that kind of a case involves an external funding agency, then you have an additional problem of responsibility to the agency as well.

I have already alluded to the difficulty of the complex case that is going to take a lot of time and effort and whether it always is the best use of university resources. That is a very difficult question. It is posed dramatically, I think, in one or two kinds of situations. First of all, a member of the faculty may have resigned, obviously under pressure, but frequently that is the best solution for a case like that. To try to document and prove the charges in a manner that will stand up in court in many of these cases is going to take much more time, expense, and effort than most of us have time to put into it.

The other thing is that we are being told in the legal profession now that the thing you want to try to accomplish is an amicable resolution of these kind of conflicts, not an adversarial confrontation. So this tension between using some kind of informal dispute resolution technique, perhaps involving resignation or something like that, versus "hell bent for leather" adversarial contests is a really difficult problem for those of us administering these programs.

The other two parts of this that have to be kept in mind is that we are not usually in a position to force testimony. We do not have subpoena power. We cannot get information, frequently, except by the cooperation of the parties involved. And we are not trained as police investigators. We do not have that kind of expertise and training, by and large. So the techniques for really pinning the facts down sometimes are difficult. I do not think in my own mind that is as much of a problem in the plagiarism cases as it is in the cases of falsification and fabrication. But even in the plagiarism cases sometimes it is a problem, as we have heard here today.

Another real difficulty is keeping on a time schedule when you are dealing with a faculty peer committee, where all the faculty members are also full-time teachers and researchers and have diverse schedules. You may be able to get a meeting only every three weeks of the whole group, and yet you want to keep continuity.

I already mentioned the problem of appropriating student work and passing it off as your own. In my view there is no problem on how you come out on that, but there certainly is in some disciplines, where it is common practice.

I also share the view that, whenever possible, you bring in neutral outsiders to help, to be a check on the process. This is partially important because it is hard to come up with individuals to serve on these review committees who do not know all of the people involved and already have formed judgments about their work and so forth. So I am an adherent to trying to get outside, neutral parties involved.

Finally, an educational effort with faculty about what plagiarism is, and for that matter, what other forms of misconduct are as well, is very badly needed. I think all of us have expressed this idea. Sure, everybody "knows" what plagiarism is, but when you actually start asking faculty members about specific cases, you would be amazed at the variety of viewpoints and thoughts that you get back. For example, one view is that, unless the work is published, it is not plagiarism. We had a case where a piece was submitted for publication, somebody "blew the whistle," so the piece was withdrawn, and the faculty member said it was not plagiarism because it was not published. Now obviously we disagreed with that, but the idea that it has to be in print is one idea that apparently is fairly well held, but in my view it is nonsense.

The second thing is that very few faculty members understand that plagiarism in a grant application is "plagiarism." And yet, as demonstrated here, many of the cases we are getting are coming out of the grant application process.

A CASE STUDY OF A COMPLEX INSTITUTIONAL INVESTIGATION OF PLAGIARISM

Dr. R. Douglas Wilkerson
Medical College of Ohio

Let me tell you about a couple of different approaches that we found useful in our relatively modest experience with an investigation of plagiarism. We have a standing committee on academic and scientific misconduct, which handles all inquiries at our institution. Our feeling is that if there is one place that fairness and consistency are tantamount, it is in handling these sort of cases. We feel that by having a standing committee, in which only a third of the members rotate off each year, we can maintain that consistency.

The investigating committee, on the other hand, is an *ad hoc* committee. Frequently there is some overlap, though, with the standing committee. One is an individual from outside of the institution; sometimes we will borrow a faculty member from another institution in town, but it clearly will not be someone from our college. The second is a person who is "selected" by the respondent; we ask the respondent for the names of one, two, or three people that he or she trusts and that he or she would feel comfortable with on the committee, and we make one such person a part of the committee. Those two strategies have served us pretty well, and I would recommend them to anyone.

The case I was invited to discuss did not arise within our institution, but at NIH. I received a phone call from OSI (now ORI) saying, in effect, you may have a problem, and then I received copies of two RO1 research grant applications. One was by a very young individual at another institution, dated and signed in 1990. The other application was by a very distinguished scientist at our institution, who had submitted a new RO1 application himself in 1991. The individual was well funded by NIH and industrial sources, but this was for him an avenue of proposed research that was relatively new, only peripherally related to his current funding.

The two applications were dated a year apart. OSI had already done their own review of them, because there were these sections in each that were bracketed. And there were relatively large "chunks" of text that one could identify as not just similar; they were identical. In addition, there were eighteen references in each application that were identical in sequence and text. Now virtually all the identical material between these applications was background material, a review of literature, if you will, related to the scientific area in which the applications were written.

As for the history of the "other" application, as I will refer to it, the one from the other applicant's institution, it was submitted in 1990 and reviewed by an NIH study section, whose membership included the respondent from our institution. We later found out that the respondent was either a primary or a secondary reviewer on the other guy's application; he clearly had access to that application, which was not funded.

As to the history of the application from our institution, it was submitted in 1991. Because the respondent was a regular member of a study section to which his application would typically have gone, a special review committee was formed at NIH to review his application and perhaps other applications of a similar nature.

In forming that special review committee, one of the scientists that the NIH Scientific Review Administrator (SRA) asked to serve was the author of the application from the other institution. She sent him a copy of the respondent's application, and it is my understanding that he called the SRA back and said, "Look, I can't review this application," perhaps told her about his own application, and sent her the two applications back.

At that point the Misconduct Program Officer in DRG (Dr. Luecke) must have gotten in touch with OSI, and then these two applications were sent to my institution for handling in accordance with our assurance on scientific misconduct. When I opened this envelope from OSI and looked at the applications, it appeared obvious that plagiarism had occurred. I thought, how simple can this be? You have two documents that have authenticated dates on them. The Dean of our institution and I met with the respondent and apprised him of the situation.

Now I have to interject that, unfortunately in this case, I think NIH made a mistake. Shortly after the respondent knew that his application was scheduled to be reviewed, he had called the SRA to ask how his application did during the review. The SRA indicated that there was a problem and his application had been referred to the DRG Misconduct Program Officer. So the respondent was alerted about the problem about a month before our institution was informed. This being the case, we felt sure that an attempt to securing the files would prove pointless (but it later turned out not to be).

When the Dean and I met with the respondent and said we had a problem, he looked at us and agreed, but he said that it is not the way that it appeared. He said that he did not plagiarize the other application, the other application was actually plagiarized from a document that he wrote in 1989. Remember that the two applications were dated 1990 and 1991. The respondent claimed that he had an idea for a new grant in 1989 and started writing a draft of a grant application. He said he had written it out in his own handwriting. After he had written it, he said he sent it to three colleagues, one of whom was the author of the other application in question. He later told us that he had mailed the draft to one colleague on July 10, 1989.

Thus, the respondent claimed that this draft that he had written in 1989 had been used by the other applicant in 1990 and that he had allegedly plagiarized the respondent's draft. So now we thought was going to be a real simple allegation has been turned around 180 degrees. Rather than having two well documented applications to deal with, we had a claim for a third document, drafted in 1989. But the respondent said that, unfortunately, neither he nor his colleagues to whom he had sent it for review could find a copy.

At this point, the respondent was going out of town for a couple of weeks to a scientific meeting, so the inquiry committee interviewed him within 24 hours of our receiving the materials from OSI. While he was away, the committee began to look very closely at the two documents that we were available. It turned out that there were some "literary differences" that just kept popping up between the two documents. The most obvious was what I would call the inappropriate use of quotation marks. Some people apparently have a habit of putting quotation marks around just about everything. That was present in both of these applications. But we looked back at a lot of the writings of the respondent, and we could not find anything like that. We also looked back at a lot of writings of the other applicant, and that style was everywhere.

We also noticed that a postdoctoral fellow had been named in the respondent's application, and I remembered that the postdoc had submitted a postdoctoral fellowship application to NIH and a couple of other agencies in 1991. So we looked at them and, to our surprise, we found the same alleged passages and the same inappropriate quotation marks. So now we had not one application, but three or four applications to deal with.

However, early in our discussions with the respondent, he had told us that the postdoc did not write those applications, that he was not even at our institution when the first ones were written. The respondent said that if there were any problem in those applications, he was responsible for it. So the postdoc was pretty well cleared of the issue. But the very first appearance of this allegedly plagiarized material was in the postdoc's original fellowship application, which was submitted three or four months earlier than the 1991 grant application by the respondent.

In the process of dealing with the postdoc, we went to see him in the laboratory and took all of his files. In sorting through the postdoc's files, we found a typical rough draft of a manuscript, which many of us might create, where segments of typed material were taken from an application were scotch-taped onto a legal pad with interspersed handwritten segments, sort of a "cut-and-paste" rough draft. Indeed, material like the other guy's application was clearly identifiable in that cut-and-paste version. It turned out that that cut-and-paste rough draft paper included sections of the postdoc's fellowship application and was used to create the respondent's grant application that was in question. In the process of that transition, there were some improvements made. There were misspellings corrected. Some errors like those in the other guy's 1990 application were carried forward verbatim into the fellowship application, but they were corrected in the respondent's handwriting in the cut-and-paste version. You could clearly see the evolution to the grant application from the cut-and-paste draft, which clearly appeared to be a early rough draft of the respondent's 1991 grant application in question. At this point, the respondent came back after two weeks at his out-of-town meeting. A few days later he came to the next meeting of the investigation committee and stated that one of the colleagues to whom he had sent the handwritten document in 1989 had found it and put it in the mail to him.

Several days later, the respondent came to my office with an envelope, not a postal envelope, but an interdepartmental mail envelope, and said that the night before he left for the out-of-town meeting, about 2:00 a.m., he had been searching through things at home trying to find this handwritten rough draft, and he had found it. At this point, the respondent produced a photocopy of a handwritten manuscript. I asked him if this was what he had found at home, a photocopy? And he said, yes, that is what he found at home, and he indicated that he did not know where the original was.

Days later in another meeting with the committee, the issue came up of the second colleague to whom he said he had mailed a copy to in 1989, and the respondent delivered a sealed envelope with the postmark from where we knew this individual to be. We opened the envelope, and it contained another copy--a photocopy again--of this same handwritten rough draft, purported to have been written in 1989, presumably the missing copy that the colleague had found and promised to mail back.

So we called the second individual, who had mailed the rough draft. In our case, we always had two people on the phone at our end, as a conference call, and we would also transcribe it, not from a tape recording, but from memory immediately afterwards. Then we sent out the summary of the phone call to the individual at the other end and had him sign off on it. Anyway, in talking with this individual, I asked when he got this draft that he had just mailed to the respondent. Was it the one he sent to you in 1989? The individual said, "No, I don't remember him ever sending me one in 1989." He said the respondent had given him the copy that last week at a meeting and asked him to look it over and mail it back to him.

So we were now left with two photocopies of a handwritten rough draft, the original of which was not available. We began looking even more carefully at the cut-and-paste rough draft,

convinced that it was the actual rough draft of the respondent's 1991 application. The question now became, what was the purpose of the handwritten rough draft document, and when was it written? Was it in 1989, as claimed, or more recently?

So this very dedicated committee, which spent a lot of time dealing with this case, began going through that handwritten rough draft and other documents with a "fine tooth comb."

As this process continued, some important findings surfaced. For example, there were places in typed segments of the cut-and-paste version where words were inserted in the respondent's handwriting. When we looked at the handwritten rough draft, all of those inserts appeared in the normal flow of text. So it now began to appear that the handwritten rough draft was written after the cut-and-paste version.

Now the handwritten rough draft was purported to have been written in 1989. The name of this scientist's department had been changed in 1990, yet the new name appeared in the handwritten "1989" draft. There was also one reference published in 1989 by the respondent that was cited in the handwritten draft, with the full citation given. We called the publisher to determine the earliest date that the respondent could have gotten the full citation for it from the journal. We were told that the earliest date was December 10, 1989. This date was significant, because the respondent had stated unequivocally that he had mailed this draft to a colleague in July.

There were lots of little things like that. It became very clear after a while that the handwritten draft could not have been written as early as July 1989. At one of the investigation committee interviews, we asked the postdoc if he had ever seen this handwritten rough draft. He said he had seen it recently, that the respondent gave it to him to look at few weeks ago. The postdoc also indicated that he had written in mid-to-late 1991 material that appeared as portions of this handwritten text. He marked for us those sections that he had written. Thus, it became very clear to all concerned that the respondent's handwritten document could not have been produced in 1989, and it must have been written sometime subsequent to mid-1991.

In summary, this case is a good example of a plagiarism issue that seemed to start out "absolutely crystal clear," with two documents that had authentic dates on them. When we compared them, it was very clear that they had sections of identical text, and the assumption right away was that one that had the earliest date had precedence, and this other one must have been a copy of it. But all of the sudden, things can get turned around on you (by a respondent who apparently created or misrepresented other documents).

USE OF A COMPUTER PROGRAM FOR EXAMINING THE EXTENT OF PLAGIARISM

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I am going to switch gears. I am not going to talk about particular cases. I am going to talk about a particular technique for resolving the problem that arises in cases where you have one demonstrated instance of plagiarism, and the question is, do you have one hideous mistake or part of a pattern? This is extremely important when you are trying to think about how to resolve a case and develop a sanction. The technique that I am going to talk about for resolving this question is not mine; I did not develop it. I do not own it. It was developed by Walter Stewart and Ned Feder. I have used it. It is not presently available, because of the events within NIH, but I am going to tell you how it works. If there are questions about it, it is up to them if they wish to answer them.

This technology has come to be known as the "plagiarism machine." I think that carries with it connotations of *1984* and "Big Brother" and a lot of ugliness, so I would like to point out a couple of things about it. First, it is a tool. It is only a tool, and it has to be used by human beings. It has to be used with judgment. It can be used in a variety of ways. Tools are susceptible to misuse. They are also susceptible to very positive use. I believe the uses that we and others have made of this particular tool have been quite positive.

We have used this tool to quantify and objectify facts. I told you earlier that one of the most important things to do is to establish the facts, and that is what we have used this tool to do. We have used it both to substantiate and to refute allegations of plagiarism. This is not a self-executing inquisition, it is merely a tool.

I am going to give you some examples. What happens in these cases is that you have an identified instance of plagiarism. Then the question is, do you have someone who has made this one hideous mistake, with which everybody sort of identifies: "Oh dear, what if I really blew it?"--or do you have somebody who is "piggy-backing" on the work of others and getting away with it? That is the issue.

In that circumstance, human beings must first determine what items authored by the accused person will be examined. You have to select the pieces from their body of work. Then you have to find the work against which you will compare them.

I have selected two pieces, with the permission of Dartmouth College's Committee on Sources, from something called *Sources, Their Use and Acknowledgement*. This is one of the guidelines for undergraduates used to talk about what is and is not plagiarism, what is and is not appropriate paraphrasing and use of sources. I have two examples of text to illustrate the technique here. They illustrate an incorrect use of paraphrase and use of somebody else's text. Once you have selected such works to be compared, you have to get them in electronic (digital) form. When we have used this technique, we have scanned the works into a computer.

Walter Stewart has developed a very elegant piece of software (I suppose it was Walter and Ned Feder--I think of it as Walter's software, because Walter is the one who talks most on the telephone, but that may not be giving credit where credit is due). As I understand the software, it uses an algorithm developed for sequencing DNA. It divides the text into character strings--we chose 30 characters--and runs huge collisions of every character string against every other character string. Then it spits out two pieces of output.

My office staff and I have prepared a simulated output based on our experience with the program, since the software presently is unavailable. The software puts in boldface every collision of the text (i.e., overlap from one document to the other), and it labels the collisions or overlaps in pairs. For example, one item is labeled "2,6." This means it is the second bolded section in the first document and the sixth section in the other document.

These numbered pairs turn out to be very important, because this tool will find instances of these collisions anywhere in the text. This is something you really cannot do by hand. You can make comparisons by hand fairly well so long as the two texts track logically and the text matches within a page or two. But when things have been rearranged dramatically, you do not catch them very easily by hand. With these numbered pairs, you can go through the documents and correlate sections of text, one against the other, regardless of their relative locations within the documents. Now please note that the fact that the text appears in two separate documents does not mean that it is "plagiarism." The tool simply identifies instances of "overlapping text." So even with the computer, there is considerable human involvement in selecting the items to scan and determining what to do with the overlapping text once it is identified.

After the computer picks out these instances of overlapping text, you have to go through the identified texts and determine whether, for example, the common text is a quote that is properly cited and attributed. It will be identified every time, but it is not plagiarism. It is, however, a place where text matches.

What we have done next is to highlight the sections in the main text identified by the computer as overlapping each other. You may notice that if you compare the boldfacing on output with what is highlighted in the text, there is more highlighted than was boldfaced. This happens because you always "catch things" in the immediate vicinity of the boldfaced portion that for one reason or another (i.e., they were not identical character-for-character) the computer did not catch. In using long texts with committees and for other purposes, we have gone through and labeled each portion, so we can find the paragraph, line, or snippet that matches one to one, and the two sets of text can be correlated. Once again, at each stage you have to make a decision, because the things that the program flags are not necessarily plagiarism. After they are identified, a judgment must be made.

After the overlapping text is identified and labeled, we try to quantify. With fairly short passages as examples, we have counted words -- 76 out of 112, or 67 percent -- in the target document are identical to those in the source document. Now you can have a lot of words that are identical. It does not work particularly well to count words. It works better when you talk about sentences or paragraphs. It does not matter that the words are the same. What is more important here is that words *in sequence*--these 30 character strings--are the same.

By the time all this processing and evaluation is done, what a committee or an evaluative group has available to it is not the computer output, but the human-processed two sets of text, highlighted and labeled so you can correlate the duplicate sections, and some sort of quantification of the extent to which they overlap.

In another example from a case at another institution with vast amounts of text at issue, there was a single page summary made for each set of comparisons. When you have a massive problem, you have to go through and quantify the whole. The summary shows how text A and text B, the chapters from which the pieces came, overlap: 55 percent total overlap, 74 percent partial overlap. This is counted by hand, and you make some judgments. It was a fairly large case. It shows how to quantify a case and how that helps us to get a handle on the magnitude of the problem and lay out the facts.

Then we have a summary of the chapter by chapter comparisons between Title A and Title B: 74 percent, 95 percent, etc., from title A and title B of the comparisons on the sections of overlap. One of them had frequent paraphrasing, that the computer only barely signals. Again, you have to catch that by hand. Here there was heavy editing, but the logical structure followed with the words.

We developed a color coding system--again, this was Walter Stewart's idea--the red, blue, and

purple differentiating between pieces with and without common authors. So that is an outline of how the tool works. Our experience when we first started using this tool was that there was simply was nothing else like it available anywhere. As far as I am aware, there is no way to do this at all right now, because this stuff is shut down and locked up. But I think that even if that particular set of software does not become available again--and I hope it does--it is a function that will be recreated. When you have concerns of this magnitude, this is the only way that I know of to deal with them. If there are things that I can answer or Walter Stewart and Ned Feder can answer about this technology--there is a lot of mythology about this technology and them out there--I would be pleased to try to do that.

DISCUSSION SESSION #2

Dr. Drummond Rennie (Univ. Calif. San. Fran.): Dr. Wiser, your last question says "should findings of plagiarism be made public?" Now I do understand the reasons for confidentiality and secrecy and so on, but I find it unbelievable that you should even consider that that answer should not be answered with the word, "yes." Because if the first finding of plagiarism had been made public, perhaps the second would never have happened. And if the second happens, then you have a plagiarizer who goes on plagiarizing. And people get more and more demoralized, because they see such people going on and on to the top. And you get students who have their own "code," who find they get buried. So how can you possibly ask that question?

Dr. Wiser (Tulane): The author of the slide, Dr. Sally Blakley, felt very strongly, "yes." She told me that. I am inclined to say that yes, it should be made somewhat public, at least among the universities. At least in this case I feel that the message, if it had been made public, was that Tulane University deals very seriously with people who plagiarize. I think it is. I agree. But in terms of the second case, I should have clarified it was more complicated than that, because in an historical context, the previous plagiarism had actually happened subsequent to the case we were investigating, although it had "come to light" sooner, during a peer review process in which the journal editor contacted the dean. So there was that complication in that the case we were doing was not actually his second act of plagiarism. As far as we knew, it was his first "act," but his second "case" did have that reversal in there.

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Dr. Alan Price (ORI): I will comment that the case at Tulane was one that the PHS had no jurisdiction over, so ORI took no action on that. In the MCO case, we concurred with the finding of scientific misconduct, not only for the initial plagiarism, but for the abuse of peer review in misappropriating another's grant application from an NIH study section, followed by coverup, the false counterallegation, the apparent fabrication of another document for the coverup, and the attempt to solicit colleagues in the field to coverup or confuse this matter. So what started as a "normal" plagiarism case, relatively minor plagiarism of some background material and one methods paragraph, became a major case. PHS's "normal sanction" (three years of certification of the person's application by them and their institution and prohibition of them from serving on PHS advisory committees) changed in this case to a ten year prohibition and a three year debarment on this respondent; so he is no longer eligible to receive PHS and other Federal funds at all. These two cases give you some pretty good examples of the intricacies, difficulties, and clever investigative methods that can arise in a case.

Speaker (Unidentified): I thought early today we were rather convincingly urged that it would be a sensible practice to make names public. I observed that neither one was named, even where we are here for an expressed purpose, and I think that just adds to the problem of how these

things are publicized and whether we can really talk about what happened. I feel that in both of the cases we just heard, the names should be made public. In one case, apparently, it is not a policy of the institution to do so. If we cannot do any more like this, how do we ever ask universities to actually do so?

Dr. Lyle Bivens (ORI): I do not know what the two cases were, as I have had to be out since the coffee break. But the ORI policy now is that when a case is closed and there is a misconduct finding, we will publicize that through a notice in the *Federal Register* and in the *NIH Guide to Grants and Contracts*. If you have been talking about two cases that are of that status, we can give you the names of those people. In fact, the first *Federal Register* notice implementing this new policy is appearing today.

I think there is a hurdle we have to get over, a psychological hurdle to start to name names. The first time we published our *ORI Newsletter* that had the names of individuals found to have committed scientific misconduct in it, it was sort of an uncomfortable or almost wrenching experience to open up and see us naming names. It is a change for us. And I think it is something that we have to accept, because most people agree that it is a very positive step for correcting the literature, for helping institutional officials make informed decisions in hiring, and a number of other reasons. But again, it is a psychological hurdle that you have to jump over, and maybe we have not quite gotten there in discussions today.

The *NIH Guide* goes to some 34,000 addresses, and that is a very wide distribution in the scientific and academic community. The *ORI Newsletter* of course, goes to over 3,000 institutions that file assurances with our office, as well as to a substantial number of other people on the list. I think we give it very broad publicity.

Dr. Clyde Watkins (ORI): As Dr. Price recalls, we had discussions about this issue as we were developing this Conference. We felt very strongly that we wanted to foster a free discussion of individual experiences and principles at this Conference. So we made a decision that we would ask that individual's names not be used here. A number of our speakers were uncomfortable in participating if individual respondents were targeted. Also, if a fear of announcing someone else's name would keep someone from raising a hand in the audience, then we wanted to avoid that, and hopefully we still can do so. Nonetheless, the ORI is obviously on record, not only as believing, but taking action on publicizing names of individuals found to commit misconduct.

Dr. Leonard Saxe (City Univ. of New York): While I would favor the actual discussion of names, I must say that I am very pleased that we are using the "P word" this morning. I think that we are making a lot of progress by calling this Conference, the Conference on Plagiarism, by openly using the word. As someone who has been a writer about the issues of honesty, but has also been the victim of plagiarism, one of the interesting experiences that I have had is that no one, university committees or professional associations, is willing in their report to actually call plagiarism, "plagiarism." There is "substantial duplication," there is almost "entire duplication," there is "incorrect referencing," and every euphemism that you can think of being used.

Dr. Heidi Weissmann (NY): As an observation about the last case that was presented, we sat here and chuckled at the blatant nature of some of what was attempted in terms of a ruse to hide the initial discovery of plagiarism. Yet in plagiarism of my work, or other cases I have become familiar with, often the actions are rather blatant, the attempts at passing off bogus, and the denials also pretty blatant. In fact, I think the "big lie" strategy is very applicable in a lot of these cases.

I think this discussion about confidentiality and the importance of not having confidentiality goes to the heart of the issue. For example, here you have a senior professor in one case who thought he could not only get away with a misconduct, but fool the institutional committee. And you are to be commended for having done the work you did to uncover what he did. But he was a senior professor, who obviously has some degree of intellect. He had some stature in the field and in the institution. We sit here and laugh at how blatant he was now, but he thought he could get away with it, and he tried.

I think we should be looking more at what that says about the system and how professors view the system, including the peer review process--that they could think they could get away with something, which is laughable in this forum. Not having confidentiality and publicizing these cases and issues, I think, would help to dispel the mistaken view that some professors have that they can get away with these kinds of actions.

Dr. Kenneth Pimple (Indiana Univ.): What do you think the severity of this case would have been if the plagiarist had said, "Oh yes, I took that from him, instead of trying to fabricate all of this? If he said, yes, I took that from another proposal, but it was background material, and it was just a proposal; there is no way I can cite that." If he could make the claim that it was the sort of common knowledge, would this have still been considered a very serious case, or a slap-on-the-wrist sort of case?

Dr. R.D. Wilkerson (MCO): In fact, that is the way things finally came around. The defense in the end, after the handwritten manuscript "fell apart," was that it is only "background." It is not important, it was not important to getting the application funded, and nobody was hurt by it. His reasoning did not mean anything to the institution, which did what it had to do, and I think did the right thing. I do not agree with the institution about confidentiality issues; outside of that, I think the institution handled the case impeccably. Officially, there were no sanctions, since the individual is now in retirement.

Mr. Walter Stewart (NIH): That is "the bad old system" in which the problem gets "hushed up" quietly, the public gets no notice or protection, and there is nothing to stop him from going to another university. The comments that have been made by Dr. Heidi Weissman and by Ms. Tina Gunsalus about "openness," I think are essential to dealing with it. I would like to second Dr. Weissman's comments about the fact that professors not only think they can get away with this, but in most cases they absolutely can and do get away with it. I know of many cases. And I hold up the following question to ORI. Is it not true that the standard "punishment" for plagiarism is just a "slap-on-the-wrist?" They have to furnish an "affidavit" with the new application that they have not plagiarized; they cannot serve on peer review committees. In lots of cases, they are not on them in the first place, and there is, effectively, no "effective sanction" against plagiarism coming from ORI.

Dr. Price (ORI): That is exactly what I addressed a few minutes ago, that although that was the "typical" sanction for "minor plagiarism," in this case the person was debarred from receiving funding by HHS for 3 years, as well as given a 10 year certification requirement and a prohibition from PHS service. I think the more difficult issue was the impact on the young professor, the person whose work was plagiarized in this case. Raising an allegation against a distinguished person in the field and having that person make false counter-allegations and solicit testimony from his colleagues in the field gave a very negative implication about this young professor, who in fact had done nothing wrong and could document that. But just the allegation coming forward and his name being associated with it, even though it was referred "anonymously" by ORI to the institution, was damaging to that person because of the respondent's actions. We have a great deal of difficulty in trying to help him restore that reputation. Part of it will be that this public announcement will be made, and in fact has been made today by ORI, of findings and sanctions against this senior professor.

Speaker (Unidentified): I would like to followup on Dr. Weissman's point about what people accused of plagiarism will do to distract attention away from the charge or to get back at their accusers. I myself filed a plagiarism complaint against Steven B. Oates, a professor of history at Massachusetts, who then threatened me with a libel suit, who then tried to prove that I had committed plagiarism in my doctoral dissertation and that AHA staff members had committed plagiarism in their doctoral dissertations. I then asked two members of the NIH staff (Walter Stewart and Ned Feder) to help me detect plagiarism in this guy's work, because I was worried about being hauled into court, and he went after these two guys, and they were reassigned.

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Dr. Joann Delnick (CT): I have a comment that comes out of some of the things Mr. Meeks said regarding the seeming peripheralness of the material that is appropriated. I could say that in my research background, I had a degree in laboratory research. The way the data lined up helped in constructing the kind of introduction that I felt someone else then appropriated. And I would think that possibly the reason for this is that this was the newest way of looking at the standard stuff. It is innocuous enough that they could use it and never get nailed for having done it. But it may, as you might have suggested, also allude to the fact that it might be going on in other areas or in other times. But it just seems ironic that those are the kinds of material that get called up and reviewed, and I just offer that as an explanation.

Mr. James Meeks (Ohio State): Yes, I think that it is still plagiarism. It is just a question of how serious it is. And if it is background, sort of "public information," then I do not view it to be as serious, in terms of the sanctions. It is one of those things that you take into consideration. But as soon as you are talking about someone who has put it together in a new way or a new form, or with a new slant to it or a new emphasis, then you are talking about something that is more original, more unique, and in my view much more of a serious plagiarism problem. Quite frankly, I think the only way we know that is to consult with experts in the field and rely upon their judgment to as whether this is just sort of a common way anybody would do this or whether it really has sort of a unique different slant to it.

Dr. Delnick: The point I was trying to make though is that in constructing it, sometimes it is six of one and half a dozen of the other whether someone else actually sees it as new or not. But as a person from whom the material is coming, I could say that I needed the laboratory data in order to put it in just that configuration, and maybe I am the only expert who understands that. And the other person who misappropriated it just simply sees it as the newest form. And never mind the data. But if that might be the case, if there is a pattern like this, consider that possibility, because why could he not just do his same old literature or methodologies, if it is common standard methodology and fairly insignificant. Why do they do it?

Dr. Charles McCutchen (NIH): A lot of our past discussion seems to have revolved around "crimes," all of which would have been obviated by 15 minutes of rapid paraphrasing. Now this is an odd sort of "crime," and I think if the "man on the street" were to come in here and listen to this, he would think we were "insane." Really, if anyone has read *The Glass Bead Game*, it seems these people are cheating at the glass bead game.

Mr. Meeks: Yes, I tend to agree. Every case I have seen has not involved really the theft of a central idea or anything really original or new. Now I am not saying that does not happen. I think it does on occasion. But in most of the cases we actually see, that is not the case. Now, as I say, that does not make it innocent. It is still plagiarism, and it is still worth trying to deal with, because of the various things that were talked about in terms of the difficulty. But I do think it affects the seriousness in sanctioning.

Dr. McCutchen: Is it not a commentary on the "speed" that these guys are operating at, that they do not take fifteen minutes to paraphrase?

Mr. Meeks: Well, yes and no. This is one of those standard defenses: "I am so busy; I do not have time to check everything." Quite frankly, I am a scholar, and I do not think that "washes." I think if I am going to put my name on something, it is going to be something that I am going to take responsibility for.

Dr. McCutchen: It was not intended as a defense; it was only intended as an observation.

Mr. Meeks: But it is a defense that I have run into: "The university expects so much out of us in terms of our research. We are bringing in all these dollars, and we cannot keep track of everything that every one of our postdocs and graduate students does to us."

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Ms. C.K. Gunsalus (Illinois): In my talk, what I was aiming to do was to tell you the kind of work that we do. We have used the program developed by Mr. Walter Stewart and Dr. Ned Feder in the past; I hope to be able to use it again in the future. It is extremely useful; it has a function, a unique function as far as I know. I assume that some clever person out there could recreate it; that seems like a waste of resources to me, to have it done. But I wanted to describe it to you, in the event that this problem sorts itself out or somebody recreates it, that you know what sort of technology is available for assisting with these cases.

Dr. Drummond Rennie (U.C.S.F.): Let me ask Walter Stewart, how did you come up with a string of 30 characters?

Mr. Walter Stewart (NIH): The particular restriction that we are under simply bars us in our private capacity from revealing anything that is not available to the public. The ACLU has taken the position this is improper, so I will just go ahead and answer questions if I can. It turns out that the software string is setable in software as to what you want to look for, and 30 is just a convenient nominal figure. And in the case of Steven B. Oates, it turns out that he "sliced stuff up so fine" that you actually do better going down to 20. In the things you were showing, what we did was start with 30, but then we enlarged them to the biggest size possible, while keeping perfect identify. The software is very fast, but very dumb. It turns out that you can set something called a "keeper size," which is different. So if you want, you can look at the things that are 40, 50, 60 long--any number that you specify in the software. So 30 is just a nominal figure, and it sometimes gets adjusted.

Ms. Gunsalus: If you have looked at the document from Dr. St. Onge at Southern Illinois University on "The Threshold of Plagiarism," it has a statement that says, given the very limited syntactical length of phrases when authors write on the same topic, the longer the text, the higher the probability of some correspondence between phrases in the two texts. One of the issues that surprised me at my first exposure to this software was in the fact that there is relatively little overlap of common phrases in huge bodies of literature. Given the body of literature that we were looking at the time, which has some fairly arcane technology and sort of terms of art that appear over and over again, I fully expected that we would see huge sort of boiler plate things that everybody used. And that just did not happen.

Mr. Stewart: I found the same thing that Ms. Gunsalus did, and it likewise surprised me enormously. We thought there would be a huge "gray area" of overlap. The actual fact of the matter is that it appears there is a tremendous "absence" of intermediate gauging. What we find is that you can instantly tell, is this copying or not, as a practical matter. You get quite good at it, looking at examples. Exceptions are like terms of art, like "American Association for the Advancement of Science," that turn up all the time, but it is so plain that it is not being copied; it is just the name of something. So I find that you can just run your eye down the "boldfacing" and instantly make a decision about the likelihood of copying.

Ms. Gunsalus: My assistant and I have done a fair amount of this for my institution and for others for whom we have consulted and helped, and that is the case. Once you start to look at this output, it is fairly easy, particularly when you can start correlating the numbered pairs. One of the things that happens is that when the two documents are quoting each other, or are in fact quoting a common third source, you will see these things. But then when you go and look at the text, you will see the quotes are there, and right away you just sort of disregard the whole thing. I have never myself used the software. I have shipped digitized text or things that Walter Stewart and Ned Feder have scanned, and they have run the software themselves. I have never possessed the software myself.

Speaker (Unidentified): How hard is it to use the software? Is that true of the others as well,

everybody has shipped their material to you, Mr. Stewart?

Mr. Stewart: Yes. We are happy to give it out, but as it happens, it is written now so that you have to be a "computer nerd" really to use it. We did not have time to make it simple. We have also been glad to, what we call, "take in laundry." We have always been glad to do other people's cases. And Illinois seems to have made the most available. Of course, at the moment we cannot do anything. But if it turns out we are able, assuming we get our software and files back and stuff like that, we have a standing offer to the academic community to help with any investigation of any matter or any person. We are always glad to do that. It is certainly quite easy if other people digitize the files; then it is totally trivial. Of course, in principle, our software is totally available; we can give it away to anyone we want to.

Ms. Gunsalus: There are two observations that I would make. In the time that I have been using the software, Walter Stewart has been tinkering with it non-stop, so it has always been in sort of a prototype form. The other is that when at the very instance of our use of the software, it felt like a very risky thing to be doing, in part because of the word-of-mouth in the community about Walter Stewart and Ned Feder. I will tell you that we entered into it quite cautiously, with some trepidation. We had explicit arrangements about confidentiality. I asked them for references. I said to them that I could not afford for my case to be "grist for your mill." They gave me, I believe, six or eight references, all of whom I called and all of whom gave me sterling references--people who had worked with them in terms of their observation of confidentiality. My experience has been nothing but positive. In every respect, every agreement that I have had with them about the use of the materials that I have provided has been adhered to, and it has been nothing but helpful to dealing with real cases in my institution and others.

Dr. Paul Anderson (Mt. Sinai): My question has to do with target source documents. I assume that mostly the investigations have centered on suspected cases of plagiarism between two authors or two different sources. Have you had the opportunity to investigate "self-plagiarism?" This is a very important area of editors, who are frequently dealing with authors who are "plagiarizing themselves" in subtle ways.

Ms. Gunsalus: The first time we used this software we were not seeking anything between two targeted people. Our investigation panel, who were experts in the field and included somebody from outside of the institution, itself helped to select the source and target documents. The experts identified a body of literature that was used for the comparisons, as did internal experts within the university.

Dr. Alan Price (ORI): For your information, the point that Dr. Anderson made about "self-plagiarism" is one that we are often asked in ORI, and it is something that we believe falls outside the PHS definition of scientific misconduct. Plagiarism is appropriation of someone else's words, without giving proper credit. Self-plagiarism is really duplicate publication. As Dr. Anderson has said, it is inappropriate, a real problem for editors, and an expensive one. But basically it does not fall under "scientific misconduct." "Self-plagiarism" is not "plagiarism," it is something else.

Ms. Gunsalus: If you have a "compact with the reader" that says what you are reading is original work of my own, and then if you represent something that is not original work of my own and originally published, ask yourself what it is.

Dr. Jerome Rosenberg (Univ. Pittsburgh): You talked about having "quantitative markers." Can you tell us what your experience is? What quantitative measures turn out to be significant? What percent?

Ms. Gunsalus: I take a fairly hard line view on plagiarism myself, because of this "compact" that I believe one has with the reader. I think that even if it is a background section, if you are presenting it as original work of your own, it ought to be original work of your own, and you ought to write it yourself. In terms of the quantification, the cases that I have looked at tend to sort of separate. I have not ended up dealing personally with these things where people say, "Well it is minor;" I have not seen these things where you only get one sentence or one phrase. We have ended up dealing with things where it is a larger body of collision.

Dr. Edward Huth (OJCCC): I think that this is an important question. In other words, what are other controls? Has this kind of program been run against documents reasonably considered not to be plagiarism? For example, is the article on penicillin in the AMA drug evaluations book, as judged against the U.S.P. drug and information article, the same thing? I think, sooner or later, if this kind of analysis gets into court, there is going to have to be some statistical basis for the threshold judgment.

Mr. Stewart: What we found is this. We have not done formal statistical tests at all, and they would be contaminated by phrases like the "American Association for the Advancement of Science." I agree with Ms. Gunsalus that the only proper way to do this is to actually apply the criterion of, is it copying? What we find is that in very closely related documents on exactly the same subject, absent of plagiarism, there is no apparent copying. So we have run literally tens of thousands, as an underestimate, of pairs of chapters, and we detected no copying at all. So you get effectively a complete, total zero. It is like absolute clear water. Not to say there is not an occasional boldfaced phrase, but you can just look at the thing and say, in two or three or four seconds, there is no plagiarism at all. So there just does not appear to be a problem. And when you get copying, it is obvious there is copying.

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Ms. Gunsalus and I as a pair, and her assistant and I as a pair, have gone through things, and I do not recall that we have ever had any discussion about what is or is not copying. It is just sort of obvious.

Ms. Gunsalus: Nor have the committees that have looked at the printout.

Mr. Stewart: It just does not seem to be a problem. So it is a common sense test that is just self-evident. Is that an adequate answer?

Dr. Huth: No, I do not think so. That is a subjective judgment.

Mr. Stewart: The subjective judgment is intrinsically built in. As Ms. Gunsalus describes it, you cannot use this machine, at least in our own experience, without a subjective judgment. And we think it would be totally improper to do. The machine only locates things. And if the two things are boldfaced and they are quoting from the same scholar and they are both referenced, the machine will pick it up as duplicated text--but it is not plagiarism at all. So we have not yet invented a machine, and we do not have any plans to do so, that will take us away from the subjective. All this does is assist you in locating material for subjective evaluation.

Ms. Gunsalus: It makes it possible to do things that would not be possible otherwise, in time frames that otherwise would not be imaginable.

Dr. Horace Judson (Stanford): Although the particular examples that Ms. Gunsalus was using did come down to a sentence overlapping with words, because the particular passages were short, in fact it is not individual words in a percentage of the cases, it is long strings.

Dr. Huth: I understand that, and I am not saying that this is an improper approach. But I suggest that sooner or later there will have to be statistical definitions.

Ms. Gunsalus: Well, I think that your point is very well taken, which is that this is a technology, and the use of the technology is in its infancy. There is not a large body of experience with it. No matter how many hours and how much work a number of people put in using this technology, it is still fairly small.

Dr. Nelson Kiang (MIT): I think if you regard the machine as a "coincidence detector," rather than a "plagiarism machine," a lot of confusion will then dissipate.

Ms. Gunsalus: That is precisely what it is. It is a coincidence detector. That is exclusively what it does. It, in and of itself, does nothing else.

Dr. Jane Rosen (NY): I know that in copyright infringement cases, one of the arguments that is used is the "fair use" of words. I think, in particular, the technology here would be able to separate out what is fair use and what is not.

Ms. Gunsalus: I am not particularly sanguine that that is true, because I think a "fair use" determination is subjective.

Dr. Rosen: Well, would it not be easier to go through a document that you can now pick up one huge amount of overlap with this and then say, "Well what percentage is fair use versus what is not?"

Ms. Gunsalus: I really do not think you can. One of the quotes this morning was that four lines of a poem may be improper use and 26 lines of a huge work may be fair use. It depends so much on what it is and how it is being used.

Dr. Judson: Fair use must be subjective. For example, I wrote three long chapters about the discovery of the structure of DNA, and I quoted at great length from James Watson. All of that was fair use, because I used a small percentage of Dr. Watson's book, and it was a very small portion of my book. On the other hand, I quoted in there four lines of a poem by Edna St. Vincent Millay. I had to get explicit permission to do that.

Dr. Rosen: I see. So, in other words, for people who use this argument in litigation, without attribution, it is a weak argument.

Ms. Gunsalus: Use without attribution is not fair use, it is plagiarism.

Speaker: (Inaudible).

Ms. Gunsalus: The questioner is unclear on my position on self-quotation. My answer is that it depends. If you are quoting yourself and you are noting the source in which it originally appeared and the place it originally appeared, I think that that is as clean as it can be. Faculty members frequently say to me, I have a methods section, I always use the same one, picking it up from one paper and using it in the next paper, because I have written this method section the best that I possibly can. There are a lot of issues, and it does depend. Does the reader expect when reading something that it is an original statement of original work by the author? With a methods section, I do not know the answer to that. That is why it gets to be a sort of a "gray area" in places about what are the standards. They are not always clear. It gets less gray and more clear cut as it becomes the original analysis section of the paper. If you are using the same original analysis from one paper to the next, at some point you have probably crossed the line.

Speaker (Unidentified): But then your opinion really is that if you use a quotation once and then use it again, you should put it in quotes.

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Ms. Gunsalus: I think you can be absolutely proper, if you have published it before, if you cite it the next time you use it. I do not think there is any question about that.

Dr. Stuart Offenbach (Purdue): I think having some kind of formal criteria from a computer program like this is absolutely the wrong thing to do. I do not think that I trust any aspect of the legal or decisionmaking process to numbers. It is always an evaluative and subjective matter.

Ms. Gunsalus: Sure, but it is also useful to have a body of data. It is useful information.

Speaker (Unidentified): I guess you could simplify by saying the definition of plagiarism was the same before and after the advent of the "plagiarism machine." It just makes it easier to find it, that is all.

Speaker (Unidentified): How does this differ from the CompuServ service called "Docucomp?"

Ms. Gunsalus: Docucomp is "redlining software;" you can take two documents, and it will mark up the excisions and the additions from one version of a document to the next. Lawyers use it a lot. Docucomp shuts down after so many consecutive paragraphs of no matching. So it is not possible to use it.

Dr. Eugene Garfield (The Scientist): Why would you not have achieved what you wanted to do by doing straightforward word frequency analyses? There is a whole field of information science that has been doing this for 30 years.

Ms. Gunsalus: Because the issue is not whether you use the same words with what frequency. The issue is whether strings of text appear in order, in comparison of one document to the next. It is a different form of analysis.

Dr. Garfield: There has been an awful lot of textual analysis done in the field of scholarship. I think you are referring to the occurrences of the patterns; they sound very similar to what you get in indexing large bodies of documents.

Mr. Stewart: I can give a partial answer to that. These word frequency analyses can be used to establish authorship when that is not known, but it answers completely different questions. Copying of strings is a different question than word frequency analysis. Both are valid areas of scholarship, but they answer different questions. You could not reach an agreement on plagiarism by just a word frequency; no such analysis has ever been done that would give you information on plagiarism.

Dr. Garfield: I think that if you submitted a paper on this system to a journal of information science, you would then be questioned about hundreds of studies that have been done.

Ms. Gunsalus: But that is the beauty of my particular position and my own sort of profound ignorance, that I do not submit it to a journal. I am not a scholar. I am "in the trenches," using it for a particular application where it fits my needs and my time frame perfectly.

Dr. Garfield: Let us reverse it. Suppose you wanted to find a comparable document. You went on Lexus. How would you do the search? Is that not a similar problem?

Ms. Gunsalus: I would have to figure it out. I do not think it is a similar problem.

Dr. Heidi Weissmann: While I do think it is very important that a meeting like this be held and that the dialogue that is ensuing be developed, considering that one of the stated purposes was issues related to the handling of allegations of plagiarism and the theft of ideas in scientific research, I think "actions speak louder than words." Here we are at this conference at NIH, and you, Ms. Gunsalus, are describing Walter Stewart and Ned Feder's work, as opposed to our actually being able to hear them describe and discuss it at this very institute.

A lot of the time that was spent earlier in the day talking about whether or not we should publish names of people who are actually found to have committed scientific misconduct. Yet I look around and I see nice faces of people who have actually done nothing wrong, but stood up for blowing the whistle on misconduct, and they have suffered grave consequences, far worse than having their names published in a journal or the *Federal Register*.

Yet the dialogue often is weighted much more heavily on what happens to the person who is found guilty of misconduct and how do we protect them, to not unfairly or unjustly give them the death penalty. It sort of ignores what happens to people who fairly do come forward with allegations that are subsequently proved, and they suffer far worse fates. I just think it is unfortunate. It is probably an appropriate time to mention the dichotomy between the experiences of people like Walter Stewart and Ned Feder, who help whistleblowers and

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provide vital services and tools to try to elucidate these things fairly, for institutions as well as whistleblowers themselves. We need to have a more equal balance in these kinds of dialogues on all sides of the issues: how to protect people who are wrongly accused, how to deal with people who are justly accused, and how to protect people who justly accuse.

INTRODUCTION TO SESSION #3:

RESPONSES OF JOURNAL EDITORS AND FUNDING AGENCIES IN DEALING WITH PLAGIARISM AND THEFT OF IDEAS IN PEER REVIEW

**Dr. Clyde Watkins
ORI**

I recall from when I was a faculty member at two medical schools that there were four major components to a research project: (1) the generation and development of the idea; (2) the writing and submitting of the grant proposal; (3) the actual conduct of the experiments; and (4) then the reporting of the results in journal form, sometimes thought of as "selling the product." I also remember that, at least at the medical schools that I was associated with, there was a very heavy emphasis on grant-writing and, more importantly, on obtaining lots of money to do research and on the reporting of the research--that is, the development of a bibliography that you could sell to the dean for purposes of job seeking, maintenance, and advancement, not only in your job, but in the community.

Sometimes I felt that the other two components, the development of the idea and the actual conduct of the experiments paled by comparison in importance to getting the money and reporting the results. I do not think that situation has changed since I left the lab. In fact, friends of mine tell me that it has gotten worse. That puts a heavy emphasis on those two writing processes in order to move along in the community. Accordingly, the pressures on these processes have become great to sort of "do whatever it takes" to be successful. In ORI's experience, this has ranged from cutting corners to outright plagiarism, fabrication, and falsification of research data and methodology.

As regards plagiarism, much, if not most of it, is detected by the peer review system in the community. Today we are fortunate to have two representatives from both major aspects of peer review, one associated with scientific journals and the other associated with the review of grant proposals.

PLAGIARISM: THE PERSPECTIVE OF AN EDITOR OF A BIOMEDICAL JOURNAL

Drummond Rennie, M.D.

Journal of the American Medical Association
and University of California, San Francisco

I have been asked to address the topic of plagiarism, not in my role as a scientist or as an author, but as a biomedical editor. Science does not really exist until it is published, and plagiarism is a "peculiar crime" because it peculiarly tends to have to do with what appears in print.

As Marcel LaFollette has just said, we in science are very "chatty" people. Last night I dined with someone, not present today, who pressed upon me a manuscript on plagiarism he had in press. I was disconcerted to see in it several of the ideas I had in the manuscript for this very talk (which he had not read). Now I am dismayed lest he think that I am plagiarizing him. In July 1991, Shinji Sakai, president of Japan's largest new agency, having taken a 10% pay cut for a month and having demoted the managing editor and four other senior editors, resigned. It had been discovered that 51 articles on health that they had published had been plagiarized by an author, who was also dismissed. As an editor, I, of course, regard this as an absurd over-reaction and appalling precedent and ascribe it to the fact that the articles plagiarized had been published 17 years before.⁶¹

Tina Gunsalus has spoken of our "compact" with the readers. Let me return to that because it should inform all our discussions. Joshua Lederberg began a talk he gave at Woods Hole with these words: "Let me begin with a few truisms, just to be sure that we are operating on a common ground of reverence for the publication process. Above all, the act of publication is an inscription under oath, a testimony. There is an extremely high level of accountability for what is published under a given person's name . . . It is the essential ingredient to make scientific work responsible in the sense that one cannot readily retreat from assertions that have been signed, delivered to the printer, and made available to thousands."⁶²

⁶¹"Japan News Chief resigns after plagiarism." (1991, July 19). *New York Times*, p. A7.

⁶²Lederberg, J. (1993, February 8). "Communication as the root of scientific progress." *The Scientist*, pp. 10-14.

The editor and the reviewers are there to try to assure quality, and one of the qualities they lay great emphasis upon is correct attribution, which must be the responsibility of the authors. The reason is obvious: as Milton observed, fame is the spur. More than anything, what we as researchers desire is *recognition*. The link between recognition and promotion, lab-space, salary, position, and power being so direct, what we resent most is to be robbed of that recognition by a competitor.

Editors get a good, or bad, view of misconduct and particularly plagiarism, because it is to the editors that the initial complaint, the "primeval howl," is directed by the author who feels "robbed." I will illustrate this view with cases, and you will see that being first to hear the complaint is not the only thing peculiar to editors.

What happened in 1979

I now quote myself⁶³: On February 15, 1979, the editor of the *New England Journal of Medicine* received a revision of a manuscript, now called an Elevation of Insulin Binding to Erythrocytes in Anorexia Nervosa: Restoration to Normal with Refeeding." With it came a disturbing and angry letter from the corresponding author, a young research worker in the Diabetes Branch of the NIH, named Helena Wachslicht-Rodbard. She said that on that day her boss, Dr. Jesse Roth, had received from the *American Journal of Medicine* a manuscript to review by workers at Yale, Drs. Soman and Felig, called "Insulin Binding to Monocytes and Insulin Sensitivity in Anorexia Nervosa." She pointed out that about a dozen paragraphs in the Yale paper (which she enclosed) were identical with her own and that the Yale paper and the most critical review of her own paper had been typed on the same typewriter, so she knew that the Yale people had reviewed her paper for the *New England Journal*. Comparison of the two manuscripts showed that she was right: there was plagiarism. In those innocent days, that is all I thought there was to it. My assessment contained an unconsciously prescient aside: "Felig's paper is better, clearer, neater, but not so original." I had not then learned the bitter lesson that frauds do not limit their areas of fraud and that plagiarism should raise "red flags" to look for other "crimes."

What happened next was due entirely to the brave "allegator," Dr. Wachslicht-Rodbard, whose paper we went ahead and published. She bombarded everyone with letters and calls and threatened to denounce the Yale workers at the big national meetings, against the wishes of her boss. She was proven right. During the next 18 months, two outside audits confirmed what Felig had been unable to discover in a year, namely, invented patients, fabricated data, smoothed Scatchard plots, and destruction of lab records.

⁶³Rennie, D. "The editor: Mark, dupe, patsy, accessory, weasel, and flatfoot." In *Ethics and policy in scientific publication*, (pp. 155-163). Bethesda, MD: Council of Biology Editors.

Soman admitted that he was the source of all the fabrication and plagiarism, and he disappeared to India. Astonishingly, their paper was published in the *American Journal of Medicine*. Meanwhile Felig was hired as chairman of medicine at Columbia, but after two months in the job, he was forced to resign and return to Yale. As for Soman, further investigation showed that 12 of 14 of his papers had missing or fraudulent data, and these papers had to be retracted.⁶⁴

What are the lessons I learned from this case? I learned that being robbed is acutely painful to academics who are robbed of their fame and, given the link between publication and promotion, their fortune. This woman was evidently prepared to sacrifice her career to expose the plagiary and thereby to assert her priority.

I learned that the finding of plagiarism of *words* is a warning that other crimes may have been committed: in this case, plagiarism of ideas, fabrication, and falsification--that the editor, who cannot be there in the laboratory, is powerless to investigate and that the other world of the editor, that of peer review, is a whole other arena for committing plagiarism.

Dr. Bridges of Baylor University, who was found by an NIH panel to have stolen ideas from a paper he had been sent to review by the *Proceedings of the National Academy of Sciences*, provides another example of plagiarism during peer review.⁶⁵ The paper from which he stole the idea was by Robert Rando *et al.* from Harvard. It was a major advance in understanding how the retinal pigment rhodopsin was regenerated. Bridges had kept the Rando paper several weeks and then returned it to the journal, having refused to review it, saying he was working on a nearly identical experiment, though he did add a handwritten note to the effect that the paper was messily written and lacking in primary data. Despite this harmful non-review, it was published in April 1987. In the meantime, Bridges sent a paper to *Science* magazine on the same subject, and it was accepted. The paper appeared in June 1987.

Baylor University investigated and found that Bridges had altered his records to "suggest falsely" that he had begun his work before seeing the Rando paper. NIH then investigated, finding that Bridges' paper was "based on the protocols and conclusions" of Rando. The NIH panel found that Bridges' *Science* paper was seriously flawed, that the research records were not substantiating, and that no evolution of experimentation existed that would have permitted Dr. Bridges to have conducted the experiments he reported without the aid of the privileged information." (Bridges had been working on a different line of research before.) Bridges had "failed to acknowledge properly the source of that information in his report to *Science*." The *Science* paper was found by the panel to have "internal inconsistencies, incomplete data and misrepresentation."

⁶⁴Lock, S. (1993). "Research misconduct: A resume of recent events." In: S. Lock and F. Wells (Eds.), *Fraud and misconduct in medical research* (pp. 5-24). London: BMJ Publishing.

⁶⁵Culliton, B.J. (1989). "NIH sees plagiarism in vision paper." *Science* 245:120-122.

What does this case teach us? That stealing the idea is as bad if not worse than theft of words.⁶⁶ Here again, it seems that plagiarism was associated with other serious deficiencies in the paper. Given the paranoia that surrounds peer review, I have called such cases (plagiarism during review) the "chain-saw massacres of peer review."⁶⁷

Of course, this other world of peer review, as the next case shows, is also a world for the discovery of plagiarism. The editor of the *Archives of Biochemistry and Biophysics* once sent a paper out for routine review.

This is what he received from the reviewer, Irwin Goldstein of the University of Michigan, who gave me a copy: "The authors are guilty of the most blatant and extensive plagiarism; entire paragraphs were lifted from the abstract, introduction, results and discussion from a paper by Roberts and Goldstein . . . and appear in this manuscript. In the event that the authors are not aware of the definition of plagiarism, it is defined as 'To steal and use (the ideas or words of another) as one's own; to appropriate passages or ideas from and use them as one's own.'⁶⁸ Should this paper be published, the authors would be guilty of copyright infringement and could be prosecuted by the *Journal of Biological Chemistry*."

This particular case illustrates several points. First, plagiarism is caught by the author and no one else. I know of only very few counter-examples, one of which I shall discuss below. Here, instead of the plagiarism occurring during peer review, it was detected in peer review, and since the editor goes to great lengths to get the reviewers closest to the subject, this is not rare. It cannot, however, be relied upon as any sort of quality assurance screen. In addition, this case shows that some scientists already know that copyright, not plagiarism, is what interests the lawyers.

The lawyer and the scientist

What I am going to discuss and illustrate next is what I believe to be a complete breakdown in understanding between two professions, the law and science. I believe that the consequences are grave for both and that it is important that the two are brought into line. Lawyers speak in terms of copyright: of property that is owned and whose ownership can be transferred, of intellectual property whose disposition is a legal matter. The copyright test might indeed be originality, but in practice, when the copyright forms are signed, this is never measured.

⁶⁶On being a scientist (p. 18). (1989). Washington, DC: National Academy of Sciences Press.

⁶⁷Rennie, D. (1991). "Problems in peer review and fraud. Cleave ever to the sunnier side of doubt." In: *Balancing act. Essays to honor Stephen Lock, editor of the British Medical Journal* (pp. 9-19). The Keynes Press.

⁶⁸Webster's New Collegiate Dictionary (p. 877). (1977). Springfield, Massachusetts: G. & C. Merriam Company.

In contrast, scientists think almost exclusively in terms of originality of thought. They live in a profession whose whole mores and method of preferment is based on priority of ideas and experimentation to test those ideas. Scientists think not in terms of the legal definitions of property so much as in terms of the ethics of proper attribution, acknowledgment, and recognition. The law is set up by lawyers and scientists have to live by it. I shall, therefore, argue that it behooves the lawyers to square the law with proper professional scientific practice.

Example 1

I mention the first case, not in an attempt to decide it, but to make a point about differing professional attitudes. When Dr. Heidi Weissmann found a chapter she had written in 1985 reproduced for a seminar in 1987 with her name removed and that of her senior colleague, Dr. Leonard Freeman, inserted, she brought charges against Leonard Freeman, and she did under the copyright law. Nothing shows the difference between the two professions, law and science, better than Judge Milton Pollak's 1988 verdict in Federal District Court. Freeman insisted that the chapter was the result of their joint efforts, although Freeman admitted that he had no hand in the preparation of the work. It was noted that Weissmann had done almost all the writing for all the 71 papers they had coauthored, but that in this case she had not left the manuscript for him to review. The judge felt that the chapter was merely a stock piece and that the fact that they had collaborated in the past meant that Freeman could use her work as his own. She was his junior, and the judge likened her relation to Freeman to that of anonymous law clerks assigned to draft an opinion for a judge. Anyhow, Freeman had received only \$250 for the seminar, so he cannot have been said to have stood to profit. The accusation, said the judge, was "grave insult to her mentor." He ruled in Freeman's favor.^{69,70,71,72,73}

⁶⁹Greenberg, D.S. (1990, May 1). "Squalor in academe. Plagiarist gets promoted, victim is out of her job." *Science and Government Report* 20(80):1.

⁷⁰"U.S. judge rejects claim of plagiarism." (1988, May 4). *New York Times*.

⁷¹Mervis, J. (1989). "Bitter suit over research work asks 'Who deserves the credit?'" *The Scientist* 3(8):1.

⁷²McGourty, C. (1989, August 24). *Nature* 340:585.

⁷³Greenberg, D.S. (1990, October 1). "Misconduct update: Slow progress on big cases." *Science and Government Report* 20(15):3.

Nothing illustrates the gulf separating the practices of the two professions so well as Judge Pollak's judgment.

The 2nd Circuit U.S. Court of Appeals threw out the first judgment, saying that it "stood copyright law on its head. Freeman stood to gain recognition among his peers . . . and authorship credit with his attempted use of Weissmann's article; and he did so without paying the usual price that accompanies scientific research and writing, that is to say, by the sweat of his brow . . . in an academic setting, profit is ill-measured in dollars. Instead, what is valuable is recognition . . ." The lower court had not considered such incentives. Freeman appealed, but the U.S. Supreme Court refused to hear the case.

Though the law might seem, from Weissmann's point of view, to have made amends, the difference in professional customs and practices is clear. Since the author seldom owns the copyright, the law, or at any rate the lawyer, does not seem to regard the actual author as having been harmed. On the other hand, nothing seems to show the feelings of the scientific medical profession about copyright infringement better than the fact that Freeman has, since the case began, been promoted, been commended by the president of his university, and been elected a member of Alpha Omega Alpha, while Weissmann remains without a job.

Example 2

A paper published in an obstetrical and gynecological review journal had an extensive number a passages of text taken without acknowledgment from two review articles that Dr. Anne Colsten Wentz (to whom I am indebted for the facts) had published. There was very extensive copying without attribution. It is fascinating to note that, throughout the extensive file, the word "plagiarism" is never used. The review's medical editor suggested that publication of the correct citations would solve the problem. The lawyer for the violated publisher, however, wrote to the lawyer of the robber's publisher that the editor's suggestion (publication of the correct citations) would be "totally unacceptable. Failure of attribution of source is not the problem and, therefore, proper credit is not the solution." (Correspondence with Dr. Anne Wentz, 10/26/92).

No scientist could ever have written this, and it is the sort of thing that drives scientists crazy: proper credit is exactly the chief solution. The matter was handled by lawyers, and the person who committed the crime was entirely divorced from the proceedings. There is no acknowledgment or apology from him and no action against him to be found in the entire file. The apology that Dr. Wentz received was from the lawyers for the publishers and was solely for their inadvertent failure "to seek permission to use the materials in question." There is no hint at the fact that they, the publishers, did not commit the crime, did not do the copying, and could not possibly have checked for it or prevented it, nor that the guilty got off "scot-free." Moreover, the *sub rosa* nature of all this means that justice, as a scientist would view the matter, could not possibly be done. The whole proceeding was like a stately minuet in a ballroom in Cloud-Cuckoo Land, while those most directly concerned--the robber and the robbed--were not merely banished

to the kitchens, but treated as if they did not exist. This sort of process and this sort of outcome maddens scientists.

Example 3

My third example is of an author, Dr. M (personal communication), who complained to me last month that when he looked at the second edition of a textbook under the editorship of Drs. A and B, he was shocked to find that the major part of the chapter that he had written for the first edition had been incorporated, word-for-word, into the second edition under new authorship by Dr. C, with no acknowledgment whatsoever to Dr. M. Dr. M had never been contacted by anyone. Dr. M complained, as any scientist would, that it was "a clear-cut case of plagiarism." He never alleged any copyright infringement.

As any scientist will understand, Dr. M was even more inflamed when he received a letter from the publishers, telling him that he was "incorrect." In the note, he was reminded that, years before, he had signed an agreement that stated his chapter was "work-made-for-hire," with Drs. A and B, the commissioning editors, being deemed to be the "author" under the copyright agreement. Dr. M was informed that what he had actually written with his own pen, as the world expert, had in reality been "authored" by the very editors, Drs. A and B, who had approached him and begged him to write it in the first place. His "proprietary rights had been in no way infringed," he was told. He had just forgotten.

However, he was informed that, thenceforth, the publishers would add a footnote saying that he had a hand in the first edition. In the meantime, the editors, Drs. A and B are upset, because they feel an academic colleague has been taken advantage of, and he has received none of the recognition that was his due, and now they seem to be the "bad guys." If Dr. M felt like the mother who has just been told that the birth of her baby had been solely a matter between the obstetrician and the hospital administrator, I would not blame him.

Now a lawyer will say that Dr. M should never have signed such an agreement in the first place if he did not understand its implications. I agree. I would go further: if no academics signed such agreements, this ludicrous state of affairs would not occur in the first place, because publishers' lawyers would ensure that the law was changed to square publishing practice with the practices of those who publish.

However, I would also urge that scientist-authors, and particularly scientist-editors, should make themselves aware of the legal conditions under which their writers write and have the courtesy and plain common sense to insure that the authors of previous versions are acknowledged freely in the text. At the very least, each revised chapter should state clearly its provenance.

What standards do the journals apply?

The importance of correct attribution is taught to every student of science. But one looks in vain for useful hints in journals' "Instructions to Authors." Neither the *Annals of Internal Medicine* nor the *New England Journal of Medicine* mention plagiarism in their instructions. Nor does the *Journal of the American Medical Association*, but it says manuscripts should be written according to the *AMA Manual of Style*, which describes plagiarism as when "an author passes off as his or her own the ideas, language, data, graphics, or even scientific protocols created by someone else, whether published or unpublished." It identifies four types of plagiarism (Northwestern University): "(1) direct verbatim lifting of passages, (2) rewording ideas from the original in the purported author's own style, (3) paraphrasing the original work without attribution, and (4) noting the original source of only some of what is borrowed. The common characteristic of all these kind of plagiarism is the failure to attribute words, ideas, or findings to their true author(s)."⁷⁴ Whether this counts as what lawyers call "notice," I do not know. The International Committee of Medical Journal Editors (the Vancouver group) has made no pronouncement on the subject.⁷⁵

I do not think there should be a statement in every biomedical journal to tell authors what is considered to be plagiarism. I suppose it is conceivable that some authors, who may not have ever been taught about plagiarism might thereby be educated, but the place for such education is in the research institutions. I would hate to think that plagiarists might get off free because the journal they used carried no definition.

How common is plagiarism?

Like practically everything else in the misconduct field, it is impossible to say because we have no data. It is true that, for example, the NSF reported that 20 of 41 allegations of misconduct that they reviewed in 1990 concerned plagiarism.⁷⁶ In the past two months, five complaints have been

⁷⁴Iverson, C.L., Dan, B.B., Glitman, P. et al. (1988) *American Medical Association Manual of Style*, 8th Edition (p. 72). Baltimore, MD: Williams & Wilkins.

⁷⁵Uniform requirements for manuscripts submitted to biomedical journals and supplemental statements from the International Committee of Medical Journal Editors. (1993).

⁷⁶National Academy of Sciences. (1992). *Responsible science. Ensuring the integrity of the research process* (vol. I). Washington, DC: National Academy Press.

reported to me:

1. I have already mentioned the case of Dr. M.
2. A reviewer (again!) pointed out to me that whole paragraphs of a review that he had published in a rival journal had been copied verbatim in the manuscript I had sent him (because I had enjoyed his review), with only a citation. I wrote to the authors, in South America, pointing out the plagiarism and instructing them that in the United States this was a defined form of scientific misconduct. The reviewer has just told me that the manuscript, failure to attribute and all, but now with the addition of an author from the U.S., has now been published in an American journal.
3. A physician has written to me to ask my help in court with a suit he is bringing against a reviewer who, he alleges, not only recommended rejection but then plagiarized his manuscript.
4. A reviewer noticed an uncomfortably close concordance in arrangement, ideas, and language between a manuscript we had sent to him and his own paper, which we had published.
5. A few days ago, an author of a rejected paper accused the authors of a paper we had published of copying a draft of the rejected paper.

**Review articles: Is there a completely different standard?
Can they be plagiarized?**

In the Weissmann case, Montefiore Medical Center responded to the late Congressman Ted Weiss' Committee, who were looking into the matter, that "the allegations against Dr. Freeman did not constitute scientific misconduct, since the article in question was a review article rather than the reporting of research data." As Congressman Weiss's Committee report says: "This position is contrary to the definition of scientific misconduct required of PHS grantees, which includes plagiarism, regardless of the specific content of the information plagiarized. Moreover, the article in question reviewed scientific research, and therefore

would be considered misconduct even under the unusual definition that Montefiore suggested.⁷⁷ I would agree.

There is, in contrast, the sad case of Professor Shervert Frazier of Harvard, who was director of the National Institute of Mental Health from 1984 to 1986. When it was discovered that large sections of articles and chapters that he had authored in the 1960s and 1970s had been plagiarized, he was fired by Daniel Tosteson, Dean at Harvard.⁷⁸ Tosteson has since been subjected to angry attacks for doing to a distinguished psychiatrist (who, remember, became distinguished partly on the basis of his bibliography, some of which was plagiarized from the work of others) what Tosteson would not have been criticized for doing to a student. These people seem to contend that plagiarism is all right if the person is senior enough (and got there by plagiarism) and if the articles were reviews.

I believe that reviews are works of scholarship, and most of us who have sweated over them might feel cheated if someone plagiarized them. It seems to me, as someone who has frequently to concern himself with plagiarists and their excuses ("pressure to publish," "forgetfulness," "a student did it," "I had permission from the author, and he forgot he gave it," etc.) that we easily get our ethics muddled up.^{79,80} No editor who has spent as much time as I have commissioning scholarly review articles can regard them as anything but original works requiring enormous scholarship and effort. What allows anyone to steal their content with our attribution?

We can agree, I believe, that journals should have and publish good citation policies, especially for reviews, where accurate citation and attribution is of the essence. In order to limit references and enable authors to give proper attribution, journals might consider asking authors to go back merely to the last good published review of the subject. Stewart and Feder have shown, as we have heard, that reviews written by different authors do not repeat themselves, a very important finding. The lesson is that the occurrence in a review article of a pattern of identical, unattributed "quotations" is just as much plagiarism as it is in any other setting.

Detection

⁷⁷U.S. House of Representatives Committee on Government Operations. (1990, September 10). *Are scientific misconduct and conflicts of interest hazardous to our health?* Washington, DC: U.S. Government Printing Office.

⁷⁸Altman, L.K. (1988, November 29). "Eminent Harvard professor quits over plagiarism, university says." *New York Times*, p. 1.

⁷⁹Hunt, M. (1989, May 14). "Did the penalty fit the crime?" (Letter). *New York Times*.

⁸⁰Goleman, D. (1989, February 11). "Doctor forced out for plagiarism is reappointed to hospital's staff." *New York Times*.

This is always by chance, but as I have suggested by giving examples, the chance is increased by careful selection of reviewers, who are then the ones to find themselves plagiarized. As a quality control device, however, this is pathetically random. Detection is, of course, always by the author.

The exception was Shervert Frazier. It should be obvious, but never is, that the skill and motives of the whistleblower detecting and reporting plagiarism is irrelevant (personal communication, Tina Gunsalus). Paul Scatena, however, who blew the whistle on Frazier, was criticized for his "eagerness to nail a big gun," and for being "the scientific community's ambulance chaser"⁸¹ and the "self-appointed policer of other people's work."⁸² If we allow attitudes like that to flourish, we are doomed.

Deliberate detection by screening

For years, editors have been admonished to improve their act. They have been told, for example, that they should always check all the citations, read all the references to check that they say what the authors claim they say (they do not), do literature searches to make sure that there is always a fair summary of previous work, and check that there is no duplicate publication and that it has not all been done before (by the authors). These calls to do more and more quality control have become more cogent as literature searches have become far easier and cheaper.

Already almost all manuscripts come in on disk, and soon they will all be out there in some electronic, digitized fashion. It is obvious to me that there will be calls (not from me) for manuscripts to be checked for plagiarism using the "coincidence machine" techniques that Stewart and Feder have so brilliantly developed. The difference will be that here there will have been no accusation: the technique--the test--will be used routinely and entirely as a test. Under these conditions, the prevalence and prior probability of plagiarism will, presumably, be exceedingly low.

First, then, the test must be tested. The accuracy of the "plagiarism machine" must be tested against, say, the judgments of panels of scientists. They will have to agree on the length and other characteristics of a string of words or phrases, or a pattern of words or phrases, and on what reliably constitutes a string. And this will have to be tested to determine its sensitivity, its specificity, and its predictive powers.

All the caveats concerning the use of a test in circumstances where plagiarism is already suspected will have to be reemphasized when it is used in the low-prevalence screening mode. For example, will the operating characteristics of the test be the same where the prevalence is low? Then

⁸¹Graff, B. (1989). "Did the penalty fit the crime?" (Letter). *New York Times*.

⁸²Hershenov, E.B. (1989). "Did the penalty fit the crime?" (Letter). *New York Times*.

professional societies will have to decide whether they want it done, and if so how, by whom and who pays. And the journals will have to decide whether they want to go along. My guess is that the "coincidence machine" will prove to be a good method for helping to test a specific allegation. For all sorts of reasons, including damage to the atmosphere of trust and the danger of false positives, it will be a bad idea for screening.

When an allegation of plagiarism comes in, what does a journal do?

I think that as the mechanisms for investigating allegations of misconduct have become more standardized, and institutions have begun to face up to facts, editors who receive the allegations, who have not the capacity to investigate or the power to discipline, should immediately turn the matter over to the institution. But in the subsequent months, the editor must remember to "hold the institution's feet to the fire."

What editors must do is publicize and print retractions¹⁵ prominently and appropriately labeled (the title must include the title of the original article). The retraction must be linked to the original publications in Medline and carry the correct citation, and the text must explain the circumstances. A letter to the editor, which we have several times used, is simply not adequate or appropriate. Who can or should retract the article? The authors--all of them--but there are examples of retractions by everyone except the guilty author, by only the guilty author, and by various mixtures of these with institutional officials.

When there are issues still apparently undecided, a good system seems to be that used by the *Journal of Histochemistry and Cytochemistry* in 1991. Bob Thoolen was accused by Mathilde Boon⁸³ of plagiarizing a paper. The editor of the journal, Paul Anderson,⁸⁴ published the accusation and a rebuttal⁸⁵ as well as an editorial, leaving the matter for the readers to judge.

⁸³Boon, M. (1991). "BrdUrd labeling of 5-phase cells in testes and small intestine of mice, using microwave irradiation for immunogold-silver staining." *J. Histochem Cytochem.* 39:380-381.

⁸⁴Anderson, P.J. (1991). "The road to publication is sometimes paved with bad intentions." *J. Histochem. Cytochem.* 39:379.

⁸⁵Thoolen, B. (1990). "BrdUrd labeling of 5-phase cells in testes and small intestine of mice, using microwave irradiation for immunogold-silver staining: An immunocytochemical study." *J. of Histochem. Cytochem.* 38:267-273.

Conclusions

At an early meeting of the Advisory Committee on Scientific Integrity to the Public Health Service, it was proposed that "plagiarism" be removed from the PHS' definition of misconduct, since the government should not concern itself with that sort of misconduct. I argued vigorously against this proposal. Leaving aside the issue of whether scientific misconduct should be the purview of the Government (and experience has unfortunately shown us that without Government intervention, some institutions would still be denying that misconduct could ever occur), to drop plagiarism from "fabrication, falsification, and plagiarism" would send the message that the "crime" that researchers care most about matters least. In addition, my own experience and that of others has shown repeatedly that a plagiarist may have committed other forms of misconduct.

As an editor, I have discovered that plagiarism is common; that it is thought to be very wrong by those wronged; that the law talks copyright and ownership, with little reference to actual authorship, while academics talk plagiarism, originality, priority, recognition and ethics: the two must be squared. Editors receive allegations; they must cooperate by publishing retractions and the results of investigations into the crimes that sully their own pages.

Finally, my experience with the Peer Review Congress is encouraging. In 1986, I advertized that in three years time, there would be a conference to present research into peer review in biomedical publication, and I encouraged research workers in every field to do this research and present it.⁸⁶ By 1989, many such studies had actually been completed and were presented.⁸⁷ For the next Peer Review Congress, to be held in September 1993, we have already received over one hundred abstracts of new research. There is a great need to work on definitions of plagiarism and to decide about areas of research into it.

That is why I think this conference is a step forward, and Alan Price and Mark Frankel are to be congratulated on their foresight and enterprise in putting it together. I strongly suggest that at the next conference on plagiarism, only the results of research be presented, so that we can move from speculation to facts. Only then will we be able to make real headway and be able to assess, for example, the pioneering work of such people as Stewart and Feder.

⁸⁶Rennie, D. (1986). "Guarding the guardians: A conference on editorial peer review." *Journal of the American Medical Association* 256:2391-2392.

⁸⁷Rennie, D. (1990). "Editorial peer review in biomedical publication: The First International Congress." *Journal of the American Medical Association* 263:1317.

MISCONDUCT PROGRAM OFFICER'S PERSPECTIVE FROM THE REVIEW OFFICE OF A FUNDING AGENCY

**Dr. Donald Luecke
DRG/NIH**

I want to remind you at least of a couple of things about the National Institutes of Health (NIH) peer review system. One is that there is a split, a dual review system. Study sections constitute the major initial review group structure that is used at NIH, and they occur within the Division of Research Grants (DRG), where I am located. We have about 100 study sections, and we serve all of the institutes at NIH. We review about 80 percent or so of the NIH applications that are sent in. However, within each of the NIH institutes or centers, there are other kinds of initial review groups that serve much the same kind of purpose, to determine scientific and technical merit, but those initial review groups are not exactly synonymous with DRG study sections. DRG receives about 35,000 grant applications per year. Most of those are referable to the NIH, but we actually receive applications for the entire Public Health Service, so a small number of grant applications go off to other agencies (CDC, FDA, and so on). So the numbers of things that we see are, in fact, immense.

It was recognized in 1987 and 1988 that initial review groups or study sections certainly would have the capacity to help in "ferreting out" certain kinds of misconduct or at least to alert NIH and others to the possibility of misconduct. At that time I remember there was a lot of fear and trepidation. I think some of the earlier comments were along the line that, in addition to doing scientific and technical merit, study sections would be required in a sense to "certify" that no misconduct had occurred. Now that certainly is not what has evolved over the last several years.

But in fact there were a number of people who felt that peer reviewers would not be willing to stand up and raise questions with regard to possible misconduct. I am happy to say that those who had that fear were really wrong, because it has not happened. If anything, in the early days, we had some problems along the other way. I think that view partly reflected the fact that we at NIH had not really developed a set of procedures to be used by reviewers in trying to look at and to make recommendations to NIH, and now, of course, to the Office of Research Integrity outside of NIH.

We had in 1988, for instance, a pre-print, which was submitted along with appendices in a grant application, that contained some questionable controls. The review group summarily decided that it would defer the application. It really did not pertain to their understanding of the current grant application, but they felt there was something wrong, so it was deferred. We really did not know at that particular point in time what to do with that, but we were saved, because shortly after that meeting, there were great questions from the scientific community through the journal as to the integrity of that particular article. It turned out, in fact, that the information was falsified, and we were really off the hook. But what we were witnessing, I think, from our initial reviewers was a concern for possible misconduct.

Over the period of years since then, we have developed some working guidelines that serve us fairly well. As Dr. Rennie pointed out, peer review really is just a sort of a quality control approach. We simply cannot, under any circumstances, try to suggest that we are finding all of misconduct and plagiarism that occurs in grant applications as they come to NIH. But I must say that there is more than I had thought might be there when I first got involved at DRG. Sometimes the questions are raised prior to the initial review by people who may be assigned as primary or secondary reviewers or, in a rare instance, even by some of our own DRG staff. When that happens, then there is an immediate kind of discussion with the staff of the Office of Research Integrity about the questions.

More often than not, this issue comes up, not prior to the review meeting, but at the meeting. So now you have a situation where maybe 25 or 30 people are involved in a discussion of the issue. If it turns out that it is a substantial part of the review, then the question really has to be answered, what should we do with it? There are lots of reasons why, in the course of a review, peer reviewers ask for deferral. They may ask for it because they need additional information; hopefully that happens before the review, but it does not always happen then. On the other hand, if what really appears to be happening during the review discussion is some question about possible scientific misconduct, then the Scientific Review Administrator needs to ask questions. Are we able, under these circumstances, to set aside this problem with potential misconduct and transfer it to the Office of Research Integrity? Are we able then as a group to go ahead and review the application and to render an opinion on the scientific merit alone? Sometimes the answer is yes; sometimes the answer is no. If the answer is no, then the application is deferred. Both things have happened.

I want to cover with you a number of things that have come up during initial review that I think are interesting and cover a wide spectrum of activities, including plagiarism. First is the failure to cross-reference very similar articles from the same author in two journals that generally do not reach the same population. That is something that has been picked up in peer review and questioned. In one particular situation, a study section member, who also happened to be a reviewer for a major journal, indicated that the current application appeared to him to be very much like the research that had been sent to him a short time earlier in a manuscript. So the question was, is the application actually work to be done, or is it work that already has been

done?

This is the type of thing that can arise in the open system that we have, where we encourage people to bring up their concerns--but certainly not with the idea that any large number of those would turn ultimately into a situation of misconduct. This then raises another issue.

When discussed in a private forum like a study section meeting (although it involves peers and some 20 or 25 people), what happens with the results that came from their concern? We have had situations where, after extensive investigation by the institution and/or ORI, there was a finding of no misconduct at all. The question then is, what should we do in terms of getting that information back to those members of the scientific community who had at least raised the possibility of misconduct? We leave that to the grant applicants who had been thought to be potentially guilty of misconduct; in some cases they want the issue raised back with the initial review group or study section, but in some cases they do not.

Most potential misconduct as plagiarism is raised by the person who is being plagiarized, but that is not always the case. We have had situations where two very similar applications appear in fact to have been plagiarized, but it is not clear to the study section as to who is plagiarizing whom. Sometimes assumptions are made. We had a situation a number of years ago where a senior person received a very good technical merit score in review of the grant and where a junior person from a different institution, who had been a collaborator through a contractual arrangement, had done very similar work, and they had written some things together. The thought raised at initial review was that somehow this junior person had in fact taken advantage of what the senior person had done. In this particular case, neither party was "guilty," so it was cleared up. But that was certainly not something that initial review group could have known, given its limited amount of knowledge at the time of the initial review.

There were two cases raised earlier today at this Conference that came about as a result of DRG study section questions. I was particularly concerned because both of the involved people were actively serving as NIH peer review study section members. As you heard from Ohio State University, that issue was actually factored in to their decision with regard to sanctions. In the MCO case, the plagiarism itself seemed to be fairly minor, but this person, having violated the confidentiality of peer review, received a greater sanction from PHS than he might otherwise have had.

Taking words from someone else's application and using it in one's own application certainly does not happen often, but that is no excuse. We have at any given time about 1,700 people who serve on regularly chartered study sections within the DRG at NIH, and we use at least 1,500 to 2,000 additional people in the course of peer review during a given year. But even one case is too much. It seems particularly egregious when people enter into a relationship with NIH in order to help in peer review and do not maintain the kind of confidentiality that is required of them. I would appreciate any comments that you might have about that. I think it is very, very difficult to

control. We normally send applications to reviewers from four to six weeks in advance of any meeting, so that they can have the time to really delve into them as we need them to do. During the course of that time, they could, if they so desired, make any number of copies of those applications and papers. So that simply returning the application during the review meeting, as all of them do, is really no assurance that they cannot be capturing information that is, in fact, confidential information.

At the same time I can tell you that, within the special scientific community, which was very "close knit," there was some knowledge of the circumstances of the Ohio State case. How, I do not know. But the PHS sanction that was published today does not preclude this individual from submitting a grant application to NIH; it requires some additional kind of certification of attribution and prohibits him from serving on PHS review committees. However, I am a bit concerned because I do believe that many of the scientists, upon whom we rely for review of such grant applications, feel that it was very wrong for someone as senior as this person to do what he did. He has submitted a number of applications since this issue became "talk" in that community, and his technical merit score evaluations have changed quite dramatically during that time. I now face a situation where there is nothing to prevent this person from submitting an application, and we are seeking ways in which to move his future applications out of that study section. We are not suggesting in any way that we want to give this person any particular advantage.

But initial review in the peer review system is done on the basis of scientific merit. It should not be "clouded" one way or the other with a sort of "halo" effect, whether it is positive or negative. This presents DRG with a potential problem, because we will review any applications that come in, and we try very hard to assure that, when they are reviewed, they are reviewed solely for scientific and technical merit.

DISCUSSION SESSION #3

Speaker (Unidentified): When you detect what to you is a clear case of plagiarism in an article submission, do you publicize these cases? Do you do anything to notify the readers?

Dr. Rennie (JAMA and U.C.S.F.): Yes. We ask the author, and we report to the institution. In every case that we have had that has been clearly defined as plagiarism, we have publicized it. We have made mistakes; we have done it several times in the "Letters to the Editor" column, which is not the right place. I think the Vancouver group indicates that you should have it in a special, very prominent place, with all the rules about linking it to the previous article, having all the authors who did the plagiarism signing it, so it goes into Medline under the same names and so on.

Speaker: Is this true of submissions that you receive?

Dr. Rennie: That is different; you are absolutely right. There your job is, if there is an allegation of plagiarism about a submission, to tell the institution and hand it over to them, saying that we have this allegation and will cooperate. This used to be a bit different, when you tried to do a bit of investigation yourself. But now the whole mechanism of inquiry and investigation has been set up, and even the most backward institutions now understand it. So your job is to tell them and "keep their feet to the fire," in other words, six months later you ask what is going on.

Speaker: In your experience, do editors communicate among themselves about these cases?

Dr. Rennie: Yes, sometimes, but that is completely informal.

Speaker (Unidentified): We are concentrating on plagiarism of text. There is something on biomedical science at the National Library of Medicine that summarizes thousands of pieces of literature and proposes literally hundreds of experiments in the future. Some of them even have brand new, creative ideas in there. If you open an issue right now in the National Library of Medicine, and you see there is a model, go to the references and trace where they got it from; the majority of them will not quote the originator of that model. To me, that is plagiarism, and I would like for this audience to eventually address that issue.

Dr. Rennie: I have already said that I think plagiarism of ideas is even worse than incorrect attribution. I would point out that I think all of us have done it, because sometimes you simply do not know all the literature, or it is not there, or it is being published at the time you are as well. I think it is more serious, but it is a great deal harder to deal with. And sometimes it is completely inadvertent.

Dr. Philip Shuchman (Rutgers Univ.): What do you or other editors do if they have selected a reviewer, and then another reviewer writes in to say that reviewer is mistaken? The other review spoke to plagiarism.

Dr. Rennie: Oh well, that is simple. You have two reviewers; one says, great paper; the other says, this was plagiarized from work (usually from my own work). Then you look into it just a little bit and then get the institution to look into it. In fact, my thesis would be that it is never going to be detected by two people (unless it is from Shakespeare).

Dr. Jane Rosen (NY): I have a question, which really is in reference to my particular case. It was curious to me that when I sent a manuscript to a well known journal at the same time that the alleged plagiarist submitted a similar manuscript to the same journal, the journal editor claimed that there was no process at the journal when receiving two exact manuscripts on the same topic from the same laboratory to note the coincidence. Secondly, they said that if they had been notified of this fact, of course they would have questioned it. So I then proceeded to notify them. Upon having been told of the second submission of the paper, based upon my work and without having notified me at all, they went ahead and published it. So I am just wondering, do journals have any sort of "code of ethics" about looking into these sort of situations, or at least notifying the person who is raising the red flag, and saying, we are looking into this?

Dr. Rennie: I wrote an exceedingly long editorial about a rather similar case, though people reached different conclusions from the same data. It seems to me that if editors are not "potted palms" or "paper tigers" or whatever, they can have their view about new circumstances. And just to say that we do not have a procedure for dealing with this is not a good idea. I do not think that we have to behave like that. The editors that I have worked with adapt. New things keep happening. Sometimes you say, that is an interesting thing--these people are "clawing like cats," so why do we not publish both papers? Or say, yes, we certainly cannot handle this, but their university has or ORI has to. Do editors have codes of ethics? Well, we have to sign onto a whole lot of things to do with conflicts and not having shares in the drug companies that the journal is selling pages to. We do have codes, but I do not think it is a shared one.

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Dr. James Zwolenik (NSF OIG): Dr. Luecke, what happens to a proposal for which there is some sort of suspicion or allegation of misconduct that you turn over to ORI? What happens to the processing, if there is no other reason to have any deferral in the process?

Dr. Donald Luecke (NIH DRG): There have been a few situations in the past, actually before ORI existed, where there was some length of time between that happening and the initial and a subsequent analysis or contact by the Office of Scientific Integrity in its early days. That presented a real problem in my view; it has to be coupled fairly quickly. It is not unusual for an application to be deferred; it is usually deferred for additional scientific information if we in DRG do so. Now it can be deferred administratively for other reasons, such as concerns about possible misconduct. When that happens, there has to be some fairly quick evaluation of the issue. We have developed a good system with ORI now, where we can share the information with ORI and where we get a reading in terms of their "comfort level," at least, with going on with the review.

Now in a case here discussed earlier, I actually called the investigator and told him that the application was being deferred for reasons that went beyond initial review and that he would be contacted by the Office of Research Integrity. There was in fact an unfortunate delay in that, and you heard what happened during that intervening period of time with regard to that particular investigator [he created a false document and a coverup]. We have to work this out in a way that allows ORI to move in and to begin its process very rapidly.

Dr. R. Douglas Wilkerson (Med. Coll. Ohio): The problem with the sanction that ORI is imposing now is that it requires an institutional certification on applications from some of these respondents found to have committed plagiarism. As an individual who signs a lot of PHS applications in the course of a year, I am not sure what good that certification is. To ask me to certify that there is no plagiarism in a particular application appears ridiculous; you are not saying anything.

Mr. Walter Stewart (NIH): I might second that comment, since it is the main sanction that is applied, and it appears ridiculous. It strikes me where this whole conference, to a certain extent, is affected by the hypocrisy of the "fact" that plagiarism actually just "does not matter" in a grant application or anywhere. The certification is an "affidavit" from the plagiarist. In many cases these plagiarists will not admit they have even plagiarized.

Dr. Clyde Watkins (ORI): We have disagreed with you on that point for a long time, and we will continue to do so. When an investigator signs that grant application's face sheet, normally he is certifying the integrity and accuracy of everything in that application. Your certification as an institutional official to NIH and ORI, under that typical sanction for plagiarism, is that you have received a certification from the individual that is acceptable to you that the application does not contain the same kind of problem that arose before. If there is another instance of the same kind of misconduct after a certification has been provided, it is tantamount to breaking an agreement, whereby a much more severe administrative action can be taken to protect the Public Health Service. That is, that a violation of the certification on its own can be basis for debarment from receiving Federal funds.

Dr. Wilkerson: In the application I cited this morning, somebody in our institute signed it, with a statement that says I certify the integrity of this, that, and the other thing.

Dr. Watkins: That signature may be a basis upon which the Public Health Service may recover funds. It comes as a surprise to many research administrators just what that signature means. It is becoming more common knowledge that it means you are accepting responsibility for that award. But that is a fairly recent phenomenon.

Dr. Luecke: I would take some issue with the notion that plagiarism is not seen as something that is "serious." I think it is. I was at least trying to give you some indication, most notably in the particular situation where the scientific community was outraged. They are outraged in part because this is a person who is not only well known here but internationally and who, under no circumstances, had to do what he did.

If the sanction had been stopping this person from applying for PHS monies, it would have been one thing, and you might argue as to whether or not that sanction should have been included in this case. But it was not. So when a future application comes in, it is my responsibility to make sure that it gets as fair a review as we can. Now I am not here to tell you that there are no mistakes made in peer review, but certainly we cannot entertain any kind of a notion that a community is "out to get somebody," and they will do that to this application or to any one that comes in the future.

Dr. Horace Judson (Stanford): Several people have brought up questions of whether there are influences operating within the study groups. You have already given one instance where you admit that there are such "political influences" operating. This opens a whole floodgate of questions about political influence of all sorts.

Dr. Luecke: Perhaps to you, but not to me. In fact, we sensed that, and it happened because of some things that were said outside of NIH by people who reported back to us that said this person "will not get a fundable priority score." Now I do not know who said it. I would like to know specifically. But in fact, we are not accommodating those applications by review in that particular study section.

Dr. Judson: Just turn it around the other way. You have a great deal of suspicion among the other researchers that if they, for example, go against the wishes of one of the more powerful people in that field, in particular if they blow the whistle on somebody, that their chances of getting research grants from NIH are going to plummet--there will be in effect a punishing process by the scientific community, operating through those people on the review group who are under the influence of the leaders of the scientific field.

Dr. Luecke: When anyone applies for a grant, they can name up to three different initial review groups that they think are competent. They could name more, frankly, but three is something that most people do not go beyond. They can also name the NIH institutes that they think should be most involved in looking at that application. That is open to everyone. Similarly, there are rosters for all of these peer review groups. And if anyone raises an issue with regard to a particular member on a study section, we will try to accommodate and review it in a different manner. Now we have been criticized on the one hand for doing that, because about 25 percent of our reviews in the past have been done by *ad hoc* groups for lots of reasons, some of which could be the one you are talking about. We heard this morning that regular study section members' applications are not allowed back into a standing study section of which they are a member, or where they were a member within at least one year of the submission of that application. So what I am trying to suggest is that if someone on the outside feels that there is a particular individual or maybe even individuals, who would show them some animosity with regard to the review, it is up to them to indicate that, and we try very hard to accommodate to their concerns.

Mr. Stewart: I was not challenging your particular view of the seriousness of plagiarism. I do not doubt what you say is true. But I am saying there is a certain farcical element, which is that plagiarism, no matter how extensive it is shown, it is my understanding that ORI will not allow that to be considered for debarment, that the punishment is always these "affidavits." I agree with the gentleman from Ohio that these institutional assurances that this application contains no plagiarism are in fact meaningless. And I do not see what is against a person plagiarizing, because if they are caught, nothing happens the first time.

Dr. Watkins (ORI): I have a different opinion on the effectiveness of that administrative action. To my knowledge, we have not had any repeat instances of plagiarism. Now we have not been in business all that long, but I know of no repeat instances of plagiarism after such an administrative action has been imposed. I think the recognition by your peers that you have been labeled as a "plagiarist" is an effective administrative action, and having to admit that and certify that any new grant application is free of such misconduct is a big step in this community. I think that, to my knowledge, it has been effective so far.

Now until that is proven otherwise, I will continue to believe that PHS's standard administrative action is effective and appropriate. However, in cases when there are exacerbating circumstances, as was noted in the MCO case earlier today, debarment has been deemed to be appropriate, followed by a very long period of certification as well as a prohibition against their providing advice in the PHS peer review process.

Dr. Leonard Saxe (C.U.N.Y.): By disciplinary birth, I am a social psychologist. And at least in theory, I like the idea of institutional responsibility, because I think too much of the discussion of plagiarism, both today and in other places, is about a "bad apple," and how do we detect them and how do we get them "out of the barrel?" I think that, whether it is in terms of a journal publishing

a statement rather than accepting a letter from an author, or an institution making a certification, the action acknowledges that this is a problem not just of bad individuals, but of institutions, systems, and organizations. And perhaps it will lead to institutions, organizations, journals, professional associations, universities, etc., taking positive steps to decrease the incentives to engage in plagiarism or other forms of misconduct. So at least in theory--I do not know about the practice right now--but at least in theory, I think this is a very important step forward.

Dr. Kell Julliard (Christine Kleinert Inst.): I want to talk about briefly the idea of prevention. The Conference seems to have been focused on the very important subject of dealing with plagiarism, but I feel that the whole preventive and educational aspect is extremely important. The research I am presenting on the poster here is one small step towards that.

The poster concerns the perceptions of plagiarism among medical faculty, medical students, and English faculty. They are asked to look at an original sample of two sentences and then three samples, which seem to be from other articles, to see if they believe that the samples are "plagiarism" of the original sample. The first sample contained a string of 48 words that were identical to the sample. From my training as an English major, it was clear plagiarism. But most physicians did not identify it as plagiarism. A majority of medical students did, as did a great majority of English faculty. So with exactly the same case, we had radically different assessments by the groups.

I believe that there is a real value in setting clear guidelines. In other words, a student or faculty will be able to know that in this direction, plagiarism lies. But what I found through my research is it is going to be very difficult to give people any kind of concrete sense of what is a good paraphrase, what is the smallest amount of verbatim material that you can use correctly. I agree with Dr. Rennie that the more research we can do to assist this process, the better. If anyone has ideas about how to conduct research to set such guidelines, please contact me. I am looking for funding to do such research, and maybe other people would do the same if they knew that research on plagiarism could be supported in some way.

Dr. Rennie: Well, that is terrific. By the way, now there are "physicians" and then "physicians." The physicians who write, are not just physicians. They are no better, but they are different. Now, who were your physicians?

Dr. Julliard: The people who we reported on were medical school faculty and medical journal editors. I polled 32 medical journal editors-in-chief and got a 50 percent response rate. I polled about 120 medical school faculty and got about a 60 percent response rate.

Dr. Rennie: Sociologists and Dr. Gene Garfield have techniques for looking into this, as well as Walter Stewart and Ned Feder and so on. There are many ideas. I do not see why you could not be organizing a totally different conference on plagiarism in three or four years time, with all the

research that people are going to be doing.

Dr. Victor Herbert (Mt. Sinai, New York): I would like to mention a new law to protect whistleblowers who blow the whistle on plagiarism or scientific misconduct. Suits are threatened against whistleblowers. I have been the victim of such. We pay an awful price in time and money, years in court defending, and then the case is thrown out because "there is no case." New York State has just passed a law which makes the person who sues you pay all your costs and throws out the case at the beginning. The suits are denominated as SLAPP suits (strategic lawsuit against public participation). New York State has just passed a law prohibiting SLAPP suits; other states should pass a similar law.