INTRODUCTION TO SESSION #4:

THE COMPUTER ERA AND ITS IMPACT ON PROTECTING WORDS AND IDEAS AND RESOLVING CASES OF PLAGIARISM AND THEFT OF IDEAS

Dr. Albert Teich
AAAS

As the Director of the Directorate for Science and Policy Programs at the American Association for the Advancement of Science (AAAS), I am the moderator for this session entitled, "The Computer Era and Its Impact on Protecting Words and Ideas in Research." We heard earlier in this Conference about the ways in which computer technology can be and has been applied to the detection and investigation of plagiarism. What we heard was, I thought, encouraging at least at a technological level. The software and packages that were mentioned seemed like important and promising applications.

However, computer technology has another side, a side which can create new opportunities for plagiarism and can make the detection and investigation of plagiarism even more difficult and complicated than it is now. I am talking about the use of computers for the creation of documents, graphics, and images as well as text and for electronic publication and dissemination of information.

Computer word processors with such peripherals as scanners and laser printers make it substantially easier to appropriate another person's text, to manipulate that text, to publish and distribute that text, to use the text in unpublished documents, such as a proposal, or to manipulate or alter graphics or even photographic images. All of this can be done from virtually anywhere in the world, without leaving a trace of the previous version of the document. Gone are the days of the marked-up manuscript, the typed manuscript with handwritten insertions. An individual can write the methods section once and use it in every paper or proposal that he or she creates, effortlessly with a word processor. In collaborations, two or more authors can write and revise a manuscript together, using various forms of advanced communications software, so that their respective contributions are essentially indistinguishable from one another.

We are just entering the era of electronic publishing. When you publish something electronically, it goes into a whole different environment with all kinds of "unchartered waters." We are beginning to see journals that exist without paper. When articles or contributions are submitted and published electronically, the usual clues to authorship and other means of tracing the provenance of the work become an entirely different matter. Our speakers are going to give us some navigational aids to these unchartered waters.

OBSERVATIONS OF AN EDITOR CONCERNING
ELECTRONIC PUBLICATION AND PLAGIARISM

Edward Huth, M.D.
Editor, *Online Journal of Current Clinical Trials*

I would like to thank Dr. Alan Price for our invitation to be here. This has been a very challenging topic. Elsewhere I have commented that the development of electronic journals is about where the automobile was in 1905. I think the speakers yesterday were describing "mayhem on the current highways" of communication, and I am in the position now of trying to predict what the "mayhem" is going to be 25 or 50 years from now, a very interesting job.

I thought I should say just a bit more initially about the *Online Journal of Current Clinical Trials* as background for most of my remarks. Pat Morgan and Maria LeBron are here for *OJCCT* from the AAAS headquarters. In some ways I think we are carrying a more rigorous peer review than has generally been the case with medical journals. The editorial group includes three persons with expertise in methodology. Papers are initially looked at by two members of the editorial group, one of whom is one of the methodologic editors. Then papers judged to be reasonable to send out for external peer review will generally go to two additional external reviewers. Many papers we have published have gone through two or three revisions before acceptance, and much of that was getting more detailed methodologic descriptions.

The chief gain in speed of publication has been in getting around the press schedule that is characteristic of paper journals. The peer review process has in general run slightly faster than in paper journals, but not radically faster because we are at the mercy of external reviewers.

The chief gain is in getting around the publication lag. As you probably know, with the possible exception of *The Lancet*, which can get a scientific paper out in about ten days once accepted, most scientific journals take at least six weeks to two months to get a paper out. The average is generally much longer than that, sometimes up to nine months or a year. Once we have accepted a paper, mark-up begins with a version of standard generalized mark-up language, which is needed to have the paper formatted properly on the display screen. It goes off to Dublin, Ohio, the OCLC computer center, and then generally is available in the system within 48 hours or 24 hours of acceptance. That is the chief gain.

What is likely to drive electronic publishing, specifically journal publishing? The speed of publication is one factor, which can be highly attractive to some authors and some subscribers. I am inclined to think that publishing is largely driven by economic considerations. Now, consider that in general the total editorial costs, which go into saying this is valid and useful scientific

information, represents only about 15 to 30 percent of the total publication costs; the remaining 85 to 70 percent is the press, printing, paper, and distribution costs. So when scientific journals begin to get into thousands of pages per year and each paper is read by a very small fraction of the total audience, then the economic question comes up. It is now desirable to shift to having the papers readily available, but not shipping all of them out on paper to subscribers or libraries.

Part of the question, of course, is what will be the cost of the alternative, the cost of electronic systems? Already there are clues that these systems may lead to transfer of costs onto the end user, in terms of paying for individual papers rather than paying such large amounts for individual subscriptions. I do not know how it will play it out any further, but I do think emphasis ought to be put on the economic determinants of how this will develop.

In regard to how electronic publishing might affect opportunities for plagiarism, I do not know what is the fraction of established or proved plagiarism of full journal articles or major fractions of journal articles. I was struck with the degree to which the cases we heard about represented use of unpublished materials, grant applications, and that kind of thing. I would be very happy to hear later on about some kind of enumeration of plagiarism of entire articles. I think some has occurred, but my impression is that it is a minor fraction of what we are discussing.

First, I think we need some definitions. When we talk about electronic journal publishing, we are talking about the issuance and continuing availability of formal, methodologically structured, fixed documents (reports of research, review articles, meta-analyses, so forth)--not bulletin board documents, which are what I suppose librarians would class as "ephemeral literature."

The second question is a definition of plagiarism. There is a great spectrum of plagiarism, ranging all the way from verbatim reproduction of complete articles down to pieces pulled out and worked into the fabric of an otherwise original paper.

Then I think it is important for me to raise the question of "self-plagiarism," which Dr. Price said means "duplicate publication." It does not carry with it the "injury" that the "garden variety" of plagiarism carries in that you are not "stealing" from someone else. But self-plagiarism is, I think, an "offense" in scientific communication in that it, in many instances, has obscured the relation of documents to each other. In the case of clinical trials reports, it has led to serious ambiguities as to whether one trial is being reported, or two trials, or three trials. So there are substantial problems with self-plagiarism.

In trying to come up with some analysis of the possible effects of electronic systems on opportunities for plagiarism, I had to think about the general conditions that favor the possibilities for plagiarism. I am sure that sociologists here might be able to extend this analysis. I have not carried out a formal literature search to try to dissect out all the possibilities. I am sure Dr. Marcel LaFollette and Dr. Leonard Saxe as sociologists might be able to throw more light on this.

First of all, there is the institutional pressure to get visibility and credit for publishing, which can be a driving force in plagiarism. A second factor is publication of plagiarized material in outlets obscure to potential detectors; lifting of material out of grant applications is an illustration of this point. The third item is a kind of variation on the obscurity factor, that is, a large gap in time between publication of the original version of a paper and that of the plagiarized version, such as a book was published in 1935 and then a plagiarized version of that material appeared many years later. Fourth, we have had allusion already from
Dr. Teich about the ready capacity to veil, dilute, filter, and otherwise manipulate a plagiarized original, which makes detection difficult. This is, of course, a growing new possibility which did not exist 20 or 30 years ago. Last, I think the lack or inadequate indexing of the plagiarized original might reduce the probability that the original could be found and, therefore, raise the possibility that the plagiarism would escape undetected.

What are the developments in electronic publishing that may raise the possibilities for plagiarism? First is the relative invisibility or obscurity of published documents. We are all accustomed to having journals come in on paper, we see titles of papers and names of authors, and there is a kind of a passive aspect to this. You do not have to do any work to become aware of who is doing what. But at least as electronic systems stand now, somebody has to turn on a computer and look at the screen. It requires more active participation, which means then, in a relative sense, that electronically published material is likely to be less visible, to be more obscure, than the paper journal on your desk.

The second factor(s) is an interesting line to pursue, one I have had to think about in connection with the AAAS project. First of all, the constraints on space in a practical sense drop away, because we are not buying tons of paper. The space in computers is relatively cheap. Much of what goes on now in paper journals editorially is driven simply by the economic constraint. The paper editor gets a budget, a certain number of pages per year, and many of the editorial decisions that are made do not involve judgments of scientific validity, but simply judgments about the size of the audience for an article and its potential for influencing further thinking. When you drop away those constraints of space, then you have to shift mental gears a bit, because why should an electronic journal then not carry papers that

may be of interest to a very small audience? Further, there is the opportunity to publish very long papers, very complete reports electronically. Again, there is a certain obscurity factor there, because there could be with very long papers some pieces lifted out that might not come to the notice of persons looking for plagiarized material.

There is also the opportunity to publish more so-called "negative results" papers. It is well established now that a fair number of reports of clinical trials are not published, because the trial shows no substantial difference between the standard treatment and a new treatment. One good example of this is in the pharmaceutical industry where substantial numbers of clinical trials are carried out as part of the New Drug Application process, and these may not show any radical advantage of a new drug over some standard drug, but nevertheless the drug is satisfactory for approval by the FDA.

In general, pharmaceutical firms have had great difficulty in getting reports of those trials published, because the editor says the drug is going to be approved, so why take up journal space with trials reports on it. It does not really seem to add anything, and the readers can read the "package stuffer" about how to use the drug. But what has become increasingly apparent is that meta-analyses, which have to draw on published reports, can have their conclusions distorted if so-called "negative results" papers are not published. Then there is a bias towards conclusions drawn from the bias toward publishing only positive results papers. I think that this is one of the substantial opportunities in our project, to make sure that both negative results and positive results are available for those carrying out meta-analyses in the future.

I have commented on the problem of indexing. If formal electronic publishing is not indexed, then when questions arise about the possible sources for materials suspected of representing plagiarized material, those originals cannot be found. We would be really hampered in "rooting out" misconduct of that kind.

There is secondary aspect to the indexing matter, that is, the growth in numbers of documents that may become available now puts a great responsibility on the persons responsible for indexing to make it possible to get to what you need efficiently. If there is a substantially larger amount of formal literature available, but the indexing is not up to efficiently getting you to what you need, then we have a new problem.

What are developments likely to reduce plagiarism or facilitate its detection? This is interesting to speculate about. There may be an accelerated rate of publication, and it may be that papers that are plagiarism in full are more readily identified as such, or perhaps suspected, because they have a "stale" quality. They represent material that is relatively old in a particular field.

Next is the shift from conventional review articles to meta-analytic documents. Of course, the appearance of meta-analyses does not depend on electronic publishing, and it is a kind of coincidental development. The connection to electronic publishing is, for example, that we are

expecting to be able to publish a series of important meta-analytic documents from a center that is very skilled in carrying out this process. When a sufficient number of additional trials are reported to justify updating the meta-analyses, we can readily do so--not by altering the initial report, but by publishing a second report (version two or edition two), dating it, and then making it available. It has been relatively easy to plagiarize conventional review articles, but I think that with this shift, plagiarism of that kind of material may become more difficult. Versions of the meta-analyses would be appearing more frequently and become more readily recognized coming from identifiable, well known sources.

The third point is that there is an increasing value of widely identified participation in certain kinds of research and a decreasing value of individual papers. This was suggested to me by Dr. Ian Chalmers, who is an English expert in clinical epidemiology and meta-analysis. He says that in the United Kingdom he gets the sense that the academic credits are going to go to persons who are identified as being parts of an apparatus that will track ongoing studies and results in various fields (for example, breast cancer treatment), and visibility in that process will be more valuable than the visibility that comes from writing individual reports. This is a development that is possible with the National Health Service in the United Kingdom and its unified records system. I do not know that it will play out this way in the United States, but that is an interesting possibility. It could mean a reduced value in published papers plagiarized as credit.

A growing number of multi-authored papers are being published, in part because of the increasing scientific complexity of science and in part because of the development of clinical trials with multi-centered environments. I assume that it would become increasingly, difficult for plagiarists to persuade a group of colleagues to become participants in plagiarism. Maybe not, as some of the conduct of academic persons in connection with some fraudulent papers makes me wonder. But this difficulty would probably tend to reduce the possibilities for getting away with plagiarism.

Finally, a new kind of protection against plagiarism could be the *a priori* identification of research studies in a national or international registry of clinical trials. If a trial were registered and the participants or planned participants were listed, when a subsequent paper or a clinical trials report comes in on a particular subject, one could turn to the registry and see a correspondence between the authors on the paper and the registry information about the to-be authors. This would be another way of detecting persons not legitimately connected with the research.

With regard to the indexing of electronic publications, I personally think that this is a wonderful opportunity for the National Library of Medicine at NIH to define formal electronic publishing. Historically, the indexing business has worked the other way; the journals have existed and are then indexed. Librarians know what journals are, because they existed before there were libraries and indexing. But I think now there could be great advantages through having a formal definition of what constitutes formal electronic publishing.

CONCERNS IN ELECTRONIC PUBLISHING:
A VIEW FROM THE AMERICAN CHEMICAL SOCIETY

Dr. Lorrin Garson
ACS

The American Chemical Society (ACS) is among the larger publishers, publishing 22 scientific journals, which contain about 100,000 composed pages a year. ACS also publishes *Chemical Abstracts* in Columbus, Ohio. The ACS has been involved with electronic publishing for quite some time. In terms of thinking about theft of intellectual ideas and plagiarism, I want to talk about some of the possibilities and some of the threats that I think electronic publishing offers.

The whole issue of plagiarism and theft of ideas has come up recently, not only in this forum but in terms of our own publication, called *Today's Chemists at Work*, which had an article on the "lonely road of the whistleblower." This particular publication is aimed toward bench chemists and other people involved with chemical technology.

Chemical and Engineering News recently published an article on the dispute over the definition of scientific misconduct. This journal is available electronically this year; an issue is "put to bed" on Thursday night, printed on Friday, and mailed Saturday, as well as loaded on STN International on Sunday, so it is available Monday morning, long before the printed copy gets out to people's desks.

What I want to talk about here are three aspects. One is that electronic information is easy to copy and this may be viewed as a "threat," from two aspects. One is an economic one, which I will not dwell on, but it is a concern of publishers of their information being downloaded. That probably is a "red herring," but nevertheless many publishers are concerned about it.

It is also a threat in terms of plagiarism because things are so easy to copy. We saw yesterday an example where somebody was working on a grant application and had literally "cut and pasted" material, and that was used as a trace to follow the "crime." One can do this electronically, very easily, to say the least. Information in electronic form offers unprecedented opportunities for search and retrieval. The work of Stewart and Feder demonstrates very nicely some of the aspects that are available when things are in soft copy

form. Electronic information also offers opportunities for authentication, which is not possible in traditional ink-on-paper. I will give you some examples of these, at least in the prototype form.

I did a computer search on plagiarism for this Conference. We found an article taken from *Compton's Multi-Media Encyclopedia*. Up popped a biography of this author of *Quiet on the Dawn*. In the last paragraph there was some mention of plagiarism. That was on a CD ROM and was very easy to copy; it took less than a second to copy that article into my computer. I have downloaded articles from our full-text data base in Columbus, Ohio, over a T-1 line with the Internet; a complete article can easily be downloaded within less than a second. It took me five seconds, however, to reformat it. I did not change any of the words, but I changed it from Helvetica to Times font, I changed the lead in, and I also highlighted the word plagiarism. So copying is indeed very easy.

It does not make much difference whether it is from a CD ROM over the Internet or from one word processing file to another. Electronic texts have been around for quite some time. I first started in this area in 1980, working with BRS to load the *Journal of Medicinal Chemistry* on line as a full-text file, which is currently on-line on STN International. These files probably represent somewhere between six and eight percent of all the chemical information in the world. The ACS publishes approximately four percent, and the other titles that are available here are another probably two to four percent. That means, at the present time, a relatively small amount of chemical information is available electronically, but it is not an insignificant amount.

Our collection, the CJAX file, has 22 journals, going back to 1982, so we have a little over a decade of back history files, which are all independently searchable or can be searched concurrently. Our journals, which are in the Chemical Journals On-Line file, will be familiar to you: *Biochemistry*, *Journal of Medicinal Chemistry*, *Chemical Information and Computer Science*, etc.

Let me give you an example of search and retrieval. I took a paragraph from an article, and I highlighted four terms. Then I searched the CJAX file. The "double equals greater than" symbol in the search statement says to search for term A in the same paragraph as term B and term C in the same paragraph as term D. There is also a sentence operator available within this software, which is called Messenger; I could have said search for these terms within the same sentence. Or if I had used the Boolean "and," I could search within the same article. A "hit" represents a document. So the precision in searching this manner is quite good. The actual paragraph, or the whole article, can be downloaded for the hits. The "special

characters" are really mutants in themselves; there is not a good way to represent an alpha, a Greek letter a, and many of the other several hundred special characters used in science on an ASCII terminal. But even with a search that hits 135,000 documents, none required more than three seconds.

It strikes me that this may be a useful mechanism to determine whether things have been copied. This file is publicly available. The information in it can be downloaded for processing within the confines of copyright, if other individuals wish to use it. The software is publicly available, and I think this may be one useful approach to the detection of plagiarism problem. Even being slow in typing and thinking, 13 minutes run time costs only $24.00, and if you really want to make this efficient, you probably could do multiple large searches in less than two minutes. I think this gives an illustration of what might be done in terms of search and retrieval with electronic information.

Now, for authentication, what one can do, both for text and graphics, is to calculate what is called a "hash total." Within UNIX, there are a number of utilities, one being the "sum utility," which goes through a file and calculates a 16-bit number, based upon each byte within the information.

For example, one paragraph might have a hash total of 34,629. The second one in which there is an extra space has a totally different hash value. The hash total is basically a "fingerprint" of that particular file. One can use the same technology for graphics. They do not have to be ASCII files, but they can be binary files. So one can indeed fingerprint electronic files rather handily. This is just a prototype. I am not suggesting that one should use the sum utility within UNIX, but I use it as an illustration to tell you that this can be done relatively easily.

There are other things that can be done. The original poem, "Mary Had a Little Lamb," has a hash total. If you shift all the characters in it to an upper case, which is done with a program called TR, a very simple utility, you get a different hash total. If you remove all the spaces with the next command and remove all the line feeds, you get a new hash total. The reason you should remove spaces and line feeds should be evident--because somebody may have an (accidental) extra space in the text, and you would get a different hash total. DOS, for example, has a carriage-return line feed pair at the end of each line. UNIX has just a line feed. Again, this is just illustrative of the type of things which can be easily done to process text, to get a common platform for authentication purposes.

Finally, let me cite an interesting reference by Komatsu and Tominega,[88] in which they have proposed creating an "electronic watermark" for digital images. It is a very weak bit pattern underneath the image itself, which acts just like a watermark on stationery, an interesting concept for authentication. That means if you change the image, you may indeed change the authenticating underlying watermark.

I would conclude that, while electronic publishing certainly provides or offers a certain degree of threat, it also offers a great deal of methodology to counteract the "threat" that it creates. As Dr. Eugene Garfield mentioned yesterday, you should not ignore the whole corpus of information science, which has addressed some of these things. There are many possibilities.

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[88]Komatsu, N. & Tominega, H. (1990). "A proposal on digital watermark in document image communication and its application to realizing a signature." *Electronics and Communications in Japan* 73(5):22-33, Part 1. (Translated from *Denshi Joho Tsushin Gakkai Ronbunshi* 72-B1(3):206-218, March 1989.)

PLAGIARISM AND FALSIFICATION OF GRAPHICS

Paul Anderson, M.D.
Editor, *Journal of Histochemistry & Cytochemistry*

I have three introductory comments that I would like to offer. First, concerning this Conference, the Office of Research Integrity has done a superb job of pulling all of us together, and I am pleased at the quality and range of information that has been presented, anecdotal though most of it may be. I support Dr. Rennie's suggestion that any future conference on this subject emphasize research on this topic.

My second comment is that my presentation is partially self-plagiarized. Mention was made yesterday of a conference held by the Fidia Foundation several years ago. The proceedings of that conference were recently published by the University Publishing Group under the title "Ethical Issues in Research."[89] What I have to say now is not very different from what I presented at that conference, but a few embellishments have been added.

Finally, I would like to give you a little background on where I'm coming from. The publication that I edit, the *Journal of Histochemistry & Cytochemistry*, is a basic science journal that is devoted to the identification of chemical constituents in cells and tissues. Our authors or investigators are concerned with developing methods and their application to biological and biomedical problems. The sponsoring scientific society is a not-for-profit organization. I and my associate editors as well as our editorial board are not paid for our services. All phases of publication are conducted in the editorial office. There are no other intermediaries, and all income reverts back to the parent society for its scientific development.

The journal office handles about 1,000 manuscripts per year with a press run of about 5,000. It publishes about 2,400 pages per annual volume. There is a 40 to 50 percent acceptance rate. There are no page charges. The frequency of "detected" plagiarism is three to five per year, and most of these cases are some form of "self-plagiarism." The "detectors" are either other "victimized" authors or our manuscript referees.

We rely heavily on our referees to assist us, not only in judging quality, but to sometimes identify offenses of the kind that we are discussing at this conference. Our manuscripts are usually subjected to review by three anonymous reviewers, who remain unknown to the authors. The reviewers' comments are transmitted to the authors along with editorial decisions for rejection,

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[89]Anderson, P.J. (1993). "Authorship and illustrations: The challenges of computer manipulation." In D. Cheney, Ed., *Ethical issues in research* (chapter 5, pp. 41-44). Frederick, MD: University Publishing Group.

acceptance, or revision.

The type of science that we deal with depends heavily on morphology. Most of our investigators use some form of morphology as their basic science tool. Our authors include histologists, anatomists, immunocytochemists, and many other kinds of biomedical researchers who rely heavily on data that can be verified by graphic images, that is to say, photographs, halftones, color images, and line art. To these scientists, the images *are* the data. That is not to say that their science is not quantitative. There are many quantitative methods incorporated into their reports, but ultimately the graphic image remains the proof of their experimental observations.

Every editor who works in morphology-dependent sciences has had to deal with the fraudulent use of illustrations at some time or other. A typical offense would be for a photograph from an earlier publication to be modified and submitted as support for a new subject or a new manuscript. Common disguises include cropping, enlarging, reducing, reversing, rotating, and other manipulations of the original illustration.

Example one

The photograph in the upper left corner is an electron micrograph of cells in which a chemical reaction has been performed and visualized by a color reaction or a change in density of the reaction product. The nature of the material displayed in the photograph is unimportant for our discussion, but all of the empty spaces and the black granules are significant in this investigation, because they identify certain kinds of material in relation to specific cellular structures.

The photograph with an explanatory caption was submitted by an author with a manuscript for review. The methods employed by the author seemed scientifically sound by all the criteria that we could apply, but the referees felt that there were a few technical problems. One very alert referee pointed out that this photograph appeared to be part of a photograph that had appeared in a previous publication by the same author. We were able to identify the publication from another journal published approximately one year earlier. The photograph on the right is the previously published one, and the photograph on the left was submitted with the new manuscript. The new manuscript did not cite the previous publication, and the author's letter of transmittal asserted that none of the work reported in the new manuscript had been previously published.

At first glance the two figures do not seem too similar. By rotating the right-hand figure 90 degrees, however, both photographs show precisely the same configuration, although at different magnifications. There is no doubt that the previously published right-hand figure had been adjusted (that is, cropped, magnified, and rotated) and submitted as part of a new manuscript.

The "felony" was compounded by changes in the figure captions. The legend for the previously published figure, described conditions that were different from those in the legend for the newly-submitted figure, so there was (a) self-plagiarism (with violation of copyright) and (b) misrepresentation in the figure legend.

We returned the manuscript with a detailed letter, explaining why we would not consider this manuscript for publication. A copy of the letter was sent to the editor of the original journal and to the Director of the Research Council that supported the author's research in the country from which the paper originated. The Director responded with a plea that a mistake had been made, that the author was most regretful, and that the author was going to correct the mistake by seeking permission to reproduce the figure from the original source. I responded simply that this was not acceptable because in our view there was a clear intent to use the photograph in another manner, and its relabeling made it into the kind of science that we could not accept.

Detection of this kind of misuse of illustrations by conventional manipulations is not perfect by any means. Besides screening by alert editors and referees, computer searches of an author's previous publications can be helpful but time-consuming. The difficulty is that there are much better methods for circumventing the conventional production of illustrations.

Example two

Normally we would expect authors to retain the negatives, prints, and other documentary evidence of their visual proof. But such documentation can now be ignored by authors, because the chemical and physical restraints that have governed photographic technique for 150 years are no longer necessary. It is now a simple matter to modify images or create new images simply by connection to an appropriate computer. Digital cameras may be used to channel the images directly to the computer, where they can be modified to a fraction of a pixel. Thus a totally seamless, virtually perfect, grainless illustration can be produced within ten minutes. The technology for this is potentially available to almost every investigator.

The printing industry now uses computer-driven scanners to prepare halftones. These, too, are easily manipulated. The conventional technique for producing halftones from original photographic prints is to use a halftone camera that contains screens of various patterns of mesh-like fineness that decompose the original image into a series of black and white dots. The resulting image is then applied to a plate that can be inked for transfer of the image to paper in a printing press. During the photographing of the original image, the tonal values (or gray scale) can be shifted to lighter or darker tones by adjusting the exposures in the halftone camera. This is

frequently done to enhance certain details in the photograph, especially in the middle tones. This type of enhancement also changes the tonal values in all other areas of the gray scale, that is, in both the lighter and darker areas of the photograph. The use of digital scanners in halftone production, however, permits very selective adjustment of image resolution and contrast without corresponding compromise of the tonal values in other regions of the gray scale.

My next material shows the results of some experiments with a digital halftone scanner that I conducted with our printer (Capital City Press). An electropherogram, a common subject for reproduction in basic science and clinical journals, illustrates the separation or migration of proteins, derived from cells or tissues, through an electrical field (see Figure 1). The rate of migration varies with the molecular weight of the individual proteins. The position of each protein is identified by a chemical reaction that displays them as a series of contrasting lines of the type that are visible in the electrophoretic control strip on the left. The experimental result is shown in the strip to the right of the control strip.

This particular experiment depended on the demonstration of an increased concentration of the slow moving proteins at the top of the experimental strip. In the original photographs for this experiment, these protein lines near the top of the experimental result are only slightly more dense (that is, more concentrated) than those in the control strip on the left. To accentuate these lines by conventional halftone reproduction, it would be necessary to shift the midtones of the original "experimental" photograph towards the lighter tones, but this would cause a corresponding lightening of the density of the darker tones in the gray scale of the halftone. This would cause an overall change in the density of all the other protein lines in the "experimental" strip that would be visually incompatible with the "control" strip.

This technical problem is easily solved with the digital halftone scanner. The area that the author may wish to emphasize can be selectively altered without affecting other zones in the original photograph. A halftone manipulated is this manner is shown on the right. Note that the control strip is unchanged while the faint lines in the original experimental strip are now darker and more prominent. A halftone of this quality could be easily printed and the investigator would thus be able to publish an experimental result that would be more "convincing" to any editor, referee, or reader.

The authors of the article from which these illustrations are taken did not, in fact, attempt to do this. This is not a case of published falsification, except as I performed it with my printer to show that it *can* be done.

Conclusions

It is obvious that as techniques for computer-assisted manipulation of illustrations become more widely available to investigators, the temptation to enhance photographs and other illustrations will become stronger. This is especially true since this kind of manipulation is virtually undetectable. It is "seamless," as some illustrators have called it. There is no record of what was changed in the original illustration. There is no trace of the kind left by photographic prints, negatives, or other conventional image products.[90]

I have no solutions for the problems I've discussed, but I would like to suggest a few controls we might exercise. As a first step we must increase awareness that there are risks in computerized imagery, and these risks can occur at many stages in the publication process involving the author, the editor, the printer, and the publisher.

In step two, as it becomes easier to tamper with the experimental product, those involved in the process of publishing must be able to vouch for the authenticity of their product. This is a formidable task, and I'm not sure how we can master it. At the very least it means that the author, photographer, graphic artist, editor, publisher, and printer should be prepared to document the details of image production that they have contributed to the publication of research reports.

Finally, the professional organizations concerned with publication standards should formally address this need by developing standards and guidelines for all of those involved in publication of graphic data that support scientific research.[91]

Photographs shown.

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[90]Ritchin, F. (1990). *In our own image: The coming revolution in photography*. New York: Aperture Foundation.

[91]"Scientific Illustration Committee: Legal and ethical considerations." (1988). In: *Illustrating science: Standards for publication* (chapter 11, pp. 251-263). Bethesda, MD: Council of Biology Editors.

DISCUSSION SESSION #4

Dr. Stuart Offenbach (Purdue Univ.): Dr. Anderson, in the past when we have produced images or articles, we have hard copies that reflect the transition from an original draft to a final draft. The procedures that you are describing now eliminate many of those intermediary steps. Not only do they make it difficult to trace plagiarism, but they do the opposite: they make people more vulnerable to an accusation of plagiarism. I am just wondering how the authors, particularly with images, which are a little different than text, can protect themselves?

Dr. Paul Anderson (Mt. Sinai): That is a good point, and I have encountered it with some frequency. Perhaps a third of the challenges of plagiarism that come to us, mainly from referees but sometimes from other authors, have no basis. The only way we can show that is to investigate it ourselves by going to the sources, by comparing the material and having the rest of our editors do the same on the same material. In those cases, no further investigation is performed. The originating author is not informed of this at all. Other than that kind of procedure in the editorial office, I do not know what else could be done.

Dr. Edward Huth (OJCCC): Dr. Anderson, is it possible to detect differences in the basic structure of the image? In photographs you get down to microscopic level; you have silver grains, which in fact are highly irregular. In computer-produced images, you generally have square pixels. Now, is it possible to examine the images submitted to you at that level, to find out the basis for their production?

Dr. Anderson: Yes, it would be if you had the equipment and facilities to do it or you delegated that kind of investigation to others. The problem is that computer-generated images are now standard fare, so the matter of fact that it is computer-created does not put it into a "suspect" category. Many images are now coming directly off computer screens in various forms, some of them not as good in image quality as the more conventional things, but many of them, depending upon the technique, are superior to the original techniques. So I think it is possible, but I am not sure, in the flood of material that we are getting, that it would help.

Dr. Lorrin Garson (A.C.S.): I would comment that in chemistry we are starting now to receive soft copy images, both of line art and halftones, which have been prepared by authors as computer technology, rather than prints and the negatives we formerly received. We are not handling images yet either, except in terms of prototypes. We have a project going with Cornell University, called a core project, which has the full graphics, mathematics, tables, and so forth. It is our intent within the next few years to be able to have the full graphics available on STN, both for our own publications and others. But as of today in the files I showed you, there are no graphic data.

Dr. Huth asked me to describe the CCT system, which does indeed handle images, tables, and mathematics with varying degrees of sophistication. From our standpoint at the ACS, the table handling as well as the mathematic handling is not sufficiently robust for chemistry, but I think in the area of clinical trials it seems to be satisfactory. But there the data are stored. The files that are sent from AAAS are sent on SGML. They are then loaded on the Newton data base at OCL, which is a traditional data base. It is a Z39 dot 50 compatible system, and one has graphic images, which are then downloaded over telecommunications either by the Internet or by dial-up blinds. The document structure of the CCT is best defined as a compound document. It is not an image of a page. But it has components that are addressable elements within the page itself that are reformatted on the fly.

Dr. Horace Judson (Stanford): It seems to me that Dr. Huth's concerns with the process of indexing, as a separate step in preparing things for making access possible to electronic publications, is in a sense subverted by what Dr. Garson was saying about the possibility, in a sense, of indexing everything. That is, the indexing process itself vanishes as a separate step because the final user can in effect index for any word that he chooses to be the key word. The ideal index becomes no index at all in other words. But, that said, what then happens in the detection of plagiarism, as a contrast or comparison with Walter Stewart's and Ned Feder's coincidence machine? Is there any sense in which what Dr. Garson is saying sort of obviates the necessity for the coincidence machine? I would also be interested in Mr. Stewart's comments on that. What I am asking really is whether the kinds of indexing, that is, the kinds of access to be able to detect potential instances of plagiarism simply by using a few key words as the means of seeing whether the same topics have been picked up, has any relevance to or eliminates the need for something like the coincidence machine that Stewart and Feder developed? Or is it merely a first step towards then applying the coincidence machine techniques to those documents that you have found?

Dr. Garson: I would view them as complementary. I do not think one would replace the other at all. One is the basis of searching for words, the co-occurrence of words. The other is a string comparison, as I understand it. They are compatible.

Mr. Walter Stewart (NIH): I agree with you that the two are, in fact, distinct. I considered, of course, word searches before I did the other. It is totally impractical to look for plagiarism on a paragraph by paragraph basis, and that is an N-squared algorithm, where N is the number of paragraphs in your total data base. It just goes on forever. This other one is an N log N, which means it is in practice like a billion times faster. So if your goal is to actually find plagiarism, key words would get you there, but only extraordinarily slowly. A faster method that does all of the text all at the same time and finds any plagiarism that co-exists in any pairs of documents is a much stronger algorithm.

Dr. Garson: But our program was not slow; I mean, the whole data base was searched within a matter of seconds on roughly two million paragraphs.

Mr. Stewart: Right, for one paragraph against two million, but in order to find the plagiarism there, you have to search every paragraph against every other one, and that is an N squared algorithm and it will be two million seconds to find all plagiarism rather than one paragraph.

Dr. Garson: No, I think your assumption that you have to search every paragraph against every one is not correct.

Mr. Stewart: But that is correct if your goal is to find out, in a given set of documents, is there plagiarism. I am saying that is a much stronger question than saying is a particular paragraph plagiarized. In other words, it depends what your goal is. If your goal is just to find one paragraph plagiarized, you can do it in one second, but if your goal is to find all the plagiarism in a set of documents, you do much better to compare them all.

Dr. Garson: Yes, that is correct.

Dr. John Krueger (ORI): Dr. Anderson, have you looked at the gray scale image histogram on those image manipulations? Would that be of use in trying to decide what has been done to an image? Would it be a fingerprint of an image?

Dr. Anderson: I think the question is, if you were concerned about misuse of an illustration and had the gray scale, would that help you make a decision as to what manipulation had occurred? Probably not. The reason is, and keep in mind we are speaking now of the final product as a paper product from a printing press in which the original image has been processed or manipulated along the way, a halftone nevertheless would be produced, but that halftone would be produced on a manipulated image or the halftone process itself would be the manipulating instrument. In either case there would be a gray scale attached to the proof product, before it goes to the printer, and that should be reviewed by the editor and the others in the editorial office. But the gray scale is not going to be able to detect that there is a manipulation in only one area of the gray scale spectrum. It will show a continuous gradation and except that the emphasis, the shift in dot density, will be discernible easily in the area that has been manipulated, but the gray scale will match it, so you have no way of knowing this.

Dr. Krueger: Are you talking about the image or the image histogram? Not necessarily the histogram, the spectrum, the distribution of pixel densities.

Dr. Anderson: No, our procedure is to produce a proof in which the gray scale is included in the print, so we are speaking of the gray scale that accompanies that print. In the case that I showed you, it would include in image one and two that, plus the gray scale off to one side or the other, so you can make the judgments as to what happened. But because, you see, if there had been manipulation of the type I described, only at the halftone exposure level, bumping the dot, so to speak, in all areas, that would be discernible. But that is a conventional technique anyway, but if it is manipulated very selectively by computer, that would not be discernible by use of the gray scale or for that matter any other method that I can think of, except the interesting suggestion about the "electronic watermark." That really intrigues me, and I think it is something we ought to be looking at.

Dr. Garson: I would like to point out one other area that we have started to encounter as a problem with electronic information. We are receiving a significant number of manuscripts now in soft copy. The authors submit hard copy, which undergoes a traditional peer review, and then they send in a diskette to us for publication, but in some cases the diskette they send us does not correspond to the hard copy. Now, if one is paranoid about this, you think, ah, they are trying to pull a "fast one" here. I think in all the cases we have seen to date, it is simply a problem of revision control at the author's end. When we encounter this, we politely send back the diskette, and we cannot use it. We will keyboard the material that was in the hard copy and has undergone peer review. Sometimes the author sends back a new diskette with apologies, and everybody is happy. But this is an area I think that is a possibility of fraud, and the publishers must be aware of this potential.

Dr. Alan Price (ORI): If we find two of the gels or autoradiograms with different exposures, which appear as final photographs in print, but they seem to be "self-plagiarized" or copies from other sources, what can we do as investigators to prove the origin or identity of those sources? We can make the standard comparison, I take it, of the photographs themselves, but as you said, they may just be different gray scale exposures. However, can we also go back into the computer files, as Dr. Krueger was asking, sequester those, and look to see whether there has been manipulation? How can we use this as an investigative technique?

Dr. Anderson: I do not want you to think my answer is facetious, but who keeps computer files? That is part of the problem. There is a way that might help. It is very expensive in time and that is to ask the investigators under question, those who may have been challenged, for the original data books, photographic images, or whatever the material may be. There are occasions when we do this. Since a lot of our material is done with electron microscopy and light microscopy, we have requested, for example, histologic slides for review by the editorial board or maybe a referee. Similarly with electron micrographs, there are usually pretty good

records and photographs of how these images were produced. Most good electron microscopy laboratories have an ongoing file on this against investigations of this kind perhaps. That is the only way I could think of to investigate it, but I am sure you can appreciate the expense involved.

Dr. S. Yoshikami (NIH): This addresses the question that extends to the authenticity of the date of entry of data into a lab notebook into a computer. You know, they can be easily changed. There must be some device, something similar to the watermark, that sets the date. Do you have any news on developments in this area?

Dr. Garson: I do not offhand. I certainly can see the problem. I am trying to think to what extent in the pharmaceutical industry soft copy manuscripts are allowed, but I am not aware of that. Does anybody here know what is acceptable to industry at this point?

Mr. John Michael Williams (MD): As a computer security and computer science consultant, I am aware of work in the computer field that is directly relevant, specifically in commenting on Dr. Garson's use of hashing as a technique for authentication. Hashing is a powerful and valid technique that is becoming more and more available commercially, and it will apply to imagery as well. Hashing could be used to protect and identify an archival electronic submission on the part of the author, for example. It would be possible to apply these new techniques to a version that is submitted for publication and then later verify it, if an investigation comes up.

I would also call to your attention that the National Institutes of Standards and Technology is attempting to standardize a secure hashing algorithm that would apply to this purpose and a digital signature system, which would be used to sign that, in a cryptographically strong way, to prevent its alteration by other kinds of elicit techniques. So there is promise. However, I would also say that the cost is liable to be very much larger because of the inherent size and complexity of the images to be protected, and the technology is not yet here to do so economically at the average work station.

Dr. Leonard Saxe (C.U.N.Y.): I find the discussion very interesting, although much of the focus seems to be on the forensic aspects. How do you detect images that are plagiarized materials? What impresses me from this discussion is that it has, in fact, gotten much easier to both self-plagiarize, as well as to plagiarize other's material. The question is: how do we prevent it? My sense of the data I am familiar with, some that I have collected, is that the detection rate is relatively small to the incidence rate. If you have a dollar, better than spending it on an investigation would be to spend it on something that would prevent the occurrence of plagiarism. I would be interested in general reactions to what can we do to create either new norms or reinforce old norms? How do we make it explicit that certain forms of "self-plagiarism" are not okay, and certain uses of images are not okay. As the technology changes and we have on-line journals, digital versions of other's manuscripts, and so on, how do we make clear to people in each of our fields that it is not appropriate to copy this material?

Dr. Edward Huth (OJCCC): I have had the same impression that "self-plagiarism" is much more frequent by far than out-and-out plagiarism. That is only a sense, and I have been publicly urging editors that they begin to systematically go into MEDLINE at the point of accepting a paper and see what else has been produced by any of the authors. It adds a little cost in time, but I was dismayed when I was Editor of the *Annals of Internal Medicine* with the number of instances in which members of my own editorial board were "slipping retreads" onto me. They were not "bizarre, psychopathic types" at lower levels of academic service, but professors. Now, as to the problem of real plagiarism, I do not know what to do about that. I think the cost and effort involved would be too steep at this point.

Dr. Garson: One thing that the American Chemical Society has done is to devote a considerable amount of attention to ethical guidelines and in trying to educate authors. One is a statement, which was developed by all of the ACS editors in collaboration, that addresses the issue of obligations of the journals, of the authors, and of the reviewers. It also addresses the issue of publishing outside of the scientific literature, which can also be a problem and really has not been addressed here. For example, occasionally we have the case where somebody will publish a result in a newspaper, ahead of scientific publication, and it creates some problems, at least some ethical problems. Now addressing the issue of electronic copying and that whole environment, we have not really begun to think about it yet. At the present time (June 1993), approximately 30 percent of our manuscripts are coming in as soft copy. This is up from five percent in January 1993, and I suspect it will be over 50 percent by the end of the year. Obviously we have to look at some of these issues and will do so.

Dr. Anderson: The only solution I have been able to apply in a pragmatic sense is to use the instrument of the transmitting letter by the authors. We have a very detailed set of requirements for a transmittal letter, the letter accompanying a submitted manuscript. The authors signify that it is original, that it has never been published before, that it is not under consideration elsewhere, that it is new and original work, that the role of each investigator or author is identified, that no conflicts of interest exist with any of the products contained therein, and so forth. All of the authors are required to sign it. If you are persistent at the editorial level, your authors will follow your desires, and indeed they do sign the letters in the way we have prescribed. Nevertheless, we found that the "offenders" always had a letter that was signed "appropriately."

Mr. Stewart: Addressing Dr. Leonard Saxe's question, which was what are the standards and how can we make them felt, the actual standards are not set by "verbiage," but by example. The fact that Albert Einstein College of Medicine for seven years can continue to defend Professor Freeman after he appropriated a whole manuscript, promote him, and fire the whistleblower, sets the "standards." The "standards" are that plagiarism is, in fact, tolerated--in fact, even "encouraged."

POLICY AND EDUCATION ACTIVITIES OF THE OFFICE OF RESEARCH INTEGRITY

Dr. Lawrence Rhoades
ORI

As the Director of the Policy and Education Division (DPE) within the Office of Research Integrity (ORI), I would like to tell you something about our activities in the year since ORI was created in June 1992.

DPE is the unit within ORI that has primary responsibility for the development of policy and the development of educational and outreach activities, such as this Conference on Plagiarism and Theft of Ideas.

One activity which DPE has initiated is a quarterly *ORI Newsletter*, the third issue of which will be coming out during the Summer of 1993. The *ORI Newsletter* is also available on the computer-networked OASH Bulletin Board. We are hoping to establish the newsletter as a two-way communication mechanism between the academic/scientific community and ORI.

The other activity in which DPE is currently engaged is the development of an ORI Annual Report. The only report that exists on scientific misconduct in the PHS integrity program was published by the former Office of Scientific Integrity Review, covering the period from March 1989 to December 1990. The new report will cover calendar years 1991 and 1992. Beginning with 1993, we will publish a report annually. It will include some demographic statistics for our cases on respondents, complainants, sanctions, and case summaries.

The other area of DPE responsibility is policy studies. One that we have been planning for some time is on the consequences of whistleblowing for whistleblowers. We have pretested a questionnaire and have gotten very good results and a pretty good return, so the whistleblowers seem to be willing to participate in such a survey, which we hope to have in the field in 1993.

The other action in relationship to whistleblowers derives from the mandate in the NIH Revitalization Act for the development of a regulation on the protection of whistleblowers. Eventually that will be published in the *Federal Register* for comment.

One of the activities recommended by the PHS Advisory Committee on Research Integrity in 1992-1993 is the development of a research program related to misconduct in science. We are beginning to explore the possibilities and trying to stimulate researchers in the field to start to consider doing such research. At this Conference, there have been several suggestions for such studies.

Another area in which DPE/ORI is engaged in a more formalized process than previously is what we call "institutional compliance reviews." According to the PHS regulation, which was published in August of 1989, each institution that receives funds from the PHS is required to establish an administrative process for handling allegations of scientific misconduct that complies with the Regulation, and then they are required to comply with that process in the conduct of any inquiry of investigation. In our reviews, we will check on that and look at allegations of "coverups" on the part of institutions or allegations of their retaliation against whistleblowers.

PLAGIARISM IN A MULTICULTURAL ACADEMIC SETTING

Dr. Nelson Kiang
MIT

My own personal experience with plagiarism started almost 30 years ago. A colleague, whom I had recommended for a position at another major university, had subsequently become so successful that he was asked to write a chapter for one of the major physiology texts. One day another of my colleagues mentioned to me that part of this chapter was taken almost verbatim from a technical report that he had written, based on his thesis. I said, "Well, that is simple. I am going to this other university next week for something else anyway, and I will just speak to him and have him correct this in the next edition."   So, I went there and spoke to my old friend. To my surprise, he became angry, and he refused to consider doing anything about it. He acknowledged that he had taken parts of the technical report and put it into his chapter, but he never apologized to his junior colleague or corrected his misdeed. I did not push the matter, and for almost 30 years, we did not see one another; it was an embarrassing situation that changed our personal relationship, which had been close up to that time. It was only a few years ago that we became reconciled. From this incident, I learned that plagiarism is an emotionally charged subject.

In 1984 I was appointed to the Harvard Faculty Conduct Committee, which was formed after the John Darsee misconduct incident, with which I am sure most of you are familiar. Through the efforts of Stewart and Feder, it was pretty clear that the Harvard Medical School Ad Hoc Committee, which reviewed the case, had not done a complete job. So the dean decided to form a standing committee which could advise him on cases of alleged misconduct in the future.

Soon after I joined that committee, we were given a complaint of alleged plagiarism. I can speak about that case because it has been thoroughly aired in the popular press, and
Dr. Rennie referred to it yesterday. This case involved a senior professor of psychiatry at the Harvard Medical School, a well respected, even revered, figure in the field. He was a former head of the National Institute of Mental Health and the head of one of the best-known psychiatric hospitals in the Northeast. A graduate student at the University of Rochester had been working on a thesis; on reviewing the literature, he noted the similarity between some of the professor's writings and published articles by others in journals or magazines, such as *Scientific American*. He sent the dean several examples of what he felt was plagiarism; at the same time, he sent copies of his letter to others, including the *New York Times*.   The dean

referred the case to our committee. One of the first facts that we wanted to establish was whether these were isolated examples of neglected attribution or whether there was a repeated and systematic pattern of appropriation.

Our methodology was to look at some of his other papers and compare them with other papers in the literature on that subject. Most of the publications of this professor were review articles, not primary reports of research. What emerged from the examination by our staff was that appropriation of other people's writings took place throughout his career. We were able to find many additional instances where his articles contained segments cut and stitched together from other people's papers. In fact, this appeared to be a long-standing modus operandi, the results of which had contributed to his successful career. Thus there was no question as to the determination that multiple instances of plagiarism had occurred. The only question was choosing a sanction to recommend.

In order to decide on a sanction, we tried to explore every facet of the professor's working style. In one of the articles, some sections appeared to have been plagiarized from the work of a close colleague of his. That colleague said that he would have given full permission for our professor to take anything he wanted from his article without attribution. This raises an interesting question: Can plagiarism be condoned by the original author? We decided that it could not, that, unlike with a copyright violation, the original author could not absolve a plagiarizer. Our professor had offered to resign previously, and we recommended to the dean that the resignation be accepted. Because a Medical School appointment is necessary to be the administrative head of his hospital, he also had to give up that position, but he retained his status as a psychiatrist there. We felt obliged to recommend what many regard as an excessively harsh sanction, because Harvard students are warned not to commit plagiarism; if they are caught, they can be dismissed. One can hardly hold faculty to a lesser standard than is applied to students.

One of the remaining problems was that there were some parts of his papers that we felt were not in his style, but we were never able to find the source. Some years later, the psychiatrist applied to be reinstated at the Medical School, and the position of the committee was that for a different outcome, we would have to see new evidence. So he supplied us with some old documents that he kept in his garage; when these were examined, we found that some of the suspect parts had been lifted from unpublished essays written by his students. Now this raises a new issue: when a professor works with students on papers, how does one assess their relative contributions, and is the professor entitled to use material like term papers of students in his own writings without giving credit? We did not explore that issue deeply because we found no reason to change our original verdict, but it is bound to arise again elsewhere in more urgent forms.

Shortly afterwards, another case came up which raised the question of complex motives that lead authors to accuse someone else of plagiarizing their work. A well-established Harvard researcher was accused of plagiarism by a scientific popularizer. Normally, one might expect plagiarism to occur the other way around. When we looked into this accusation, we found that these two had for some time a tangled relationship, wherein the popularizer had hoped to obtain a Harvard connection. To this end, he had sent a lot of his publications tothe Harvard faculty member, who at one point used some of that wording in his own writings, without giving proper attribution. This was a case where plagiarism did occur, but the situation that led to the complaint was more complicated than it appeared to be at first--chock full of motivational subtleties. I think this case bears some resemblance to certain kinds of malpractice suits. If there is a very good relationship with their doctor, patients rarely sue, even when there is some poor doctoring. Many, perhaps most, malpractice suits occur when there is some faulty relationship between doctor and patient. It may be that resentful personal relationships can lead to accusations of plagiarism, which might otherwise never have risen above the threshold level for filing complaints.

Another interesting case that we encountered concerned a scientist who had access to a grant submitted by someone from another institution through his serving on a peer review committee. The Harvard faculty member was not a native English speaker, and he acknowledged having some difficulties with writing well. For the background section in his application, he appropriated virtually the whole section from the other application, which had been sent to another agency. As luck would have it, the application of our Harvard scientist went to a peer review committee with a member who showed it to a colleague for comments. That "consultant" looked at the proposal and said, in effect, "Gee, this is my writing." Faced with the evidence, our faculty member readily admitted that he had committed plagiarism, and he threw himself on the mercy of the committee. He was placed on probation, and notations were placed in his record, with restrictions placed on his applying for grants and serving on study sections, but he was not asked to resign.

Recently, one of my own colleagues from another country submitted a draft manuscript for comments from our laboratory. Some of our scientists (who had been in active collaboration with the other group) noted with dismay that significant portions of the draft contained wording that was suspiciously close to, and sometimes taken verbatim from, our grant applications, to which our foreign colleagues had access. We raised the issue on a visit by the foreign colleague to our laboratory, and he was shocked that there was any feeling of misbehavior. He averred that the scientific issues had been so well stated in our grant applications that it would have been almost "criminal" to substitute his own awkward wording. Eventually, he was convinced that attributions were necessary, and then he was suddenly struck by another thought.

Their group had previously submitted another manuscript which was rejected with comments from one reviewer which had puzzled him and his chief, who was a coauthor. The comments alluded vaguely to resemblances in expression with portions of other published papers in the literature. These comments did not make sense to them at the time, but the light dawned as we talked. In this case, we are absolutely certain that this colleague and his chief had no clear concept of what plagiarism is or of its implications. I suspect that they are not the only ones and that there is considerable confusion in many quarters, with, perhaps, a liberal dash of rationalization.

From extensive discussions with many friends and colleagues, I have surmised that academicians in the humanities (such as English, history, or literature) are the most acutely aware of plagiarism as being unacceptable behavior. Practitioners in professions, such an engineering and medicine, tend to be less clear in their attitudes towards plagiarism. When forced to think about it, they will readily admit that it is undesirable and even reprehensible, but they do not feel as intensely as scholars for whom words and expressions are their main *raisons d'etre*.

Another case with a different twist involved my own department, so I could not take part in the proceedings. The story started with a Request for Applications from NIH for a particular area of research. The chairman felt that there were several people in the department who were interested in this topic, but who were located in different hospitals. Trying to promote collegial collaboration, he suggested that three investigators get together and write a joint proposal. They did so, but they found in writing the proposal that they could not agree on the science. The collaboration proved unworkable, so they decided to part company. One of the investigators came from a different institution than the other two. The two that were from the same hospital decided to join in applying for the grant without bringing in the third person. However, in writing the application, they included a section describing clinical trials which had been written by their rejected comrade for the previous version. They did not change the wording because, in their view, this section was simply "boilerplate," in which the words of the Request for Applications were simply turned around, in effect saying, "If this is what you request, this is what we will do." They did not feel that this section represented a true intellectual contribution by the third investigator. That worthy person, however, was "smarting" under the rejection by his colleagues, and he formally accused them of plagiarism. He communicated his remarks to NIH, so "the fat was in the fire."

This situation was eventually looked into by two committees, and it was finally determined that, while technically there was "plagiarism," it was so minimal that there was "no misconduct." However, it was also recommended that the institution withdraw the application, so that the reputation of the institution would be unblemished. Because this was a one-chance Request for Applications, the decision effectively eliminated any possibility of funding of the work from that NIH initiative. Such a result raises the issue of whether charges can be made mischievously in such a way that, even if the charges are eventually found to be without merit, the intended effect is realized. Alternatively, from the standpoint of the complainant, the fruits of his labors, however

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slight, had certainly been used without permission. Life can be difficult.

Having been sensitized to plagiarism while serving on the Harvard committee, I found that students were busily committing plagiarism in my own classes at MIT. In a course on Behavior and Neuroscience offered to undergraduates, I decided one year to assign grades on the basis of a term paper, with the midterm grade to be based on a first draft. On receiving the first drafts, the teaching assistant and I detected immediately that one of the submissions had been plagiarized, because we recognized the well-known article which had been appropriated *in toto*. I told the student, "Sol Snyder would have received an A on this draft, but you are going to get an F." This student felt aggrieved that she had been caught, but she insisted that it was the first time.

A second paper in the group was written so professionally that no junior undergraduate student could possibly have authored it. So the teaching assistant started looking for the source and found that it had been entirely copied from a review paper in the literature. We brought our evidence to the attention of the student, and her defense was that she thought plagiarism applied to the final version of the paper. This was only a first draft, and she was going to change it before the final. Both those cases were handled within the class, and I did not take it any higher. In retrospect, perhaps I should have.

An example of "double-dipping" occurred when, by chance, I found that one of the papers submitted had been used as a paper in another course that the student had previously taken. Aside from complimenting the student on conservation of effort, I suggested that the manuscript be revised to reflect a slightly different stance. On reflection, one can hardly blame the student for an action that is not expressly prohibited by most rule-setting bodies. Even full-fledged, card-carrying scientists often publish virtually the same paper in several outlets. We all know colleagues who routinely fatten their publication lists with minimal effort by such means.

Three years ago, I was asked to serve on the MIT Committee on Discipline, just in time to encounter the infamous Course 100 incident, in which 78 students were accused of cheating in a single class. This was a programming course in which students were not allowed to copy solutions to problem sets that counted for 60 percent of the grade. They could study and work together, but could not submit duplicate code. This was an introductory programming course at MIT and was, therefore, a large class of about 200 students. Partway through the semester, the professor was told by some of the students that there was widespread cheating in the course. Being an MIT professor, he naturally wrote a quick program to detect duplicate codes. Most of the homework was turned in by electronic means, and he found a plethora of duplicate code in homework problems, mostly bunched in small clusters, rather than being part of one gigantic conspiracy. This was too major an incident for him to handle within the context of the class, so he brought it to the Committee on Discipline, and we had to hear the 78 cases individually or in small groups.

The range of offenses was unexpectedly broad. Most involved straightforward plagiarism. A few

students had genuinely worked together and submitted the same program as a result of their joint work, even though they understood that they were not supposed to do so. These were minimal offenses and we let these students off with an admonition. At the other extreme there were students who would break into their classmates' electronic files withoutpermission and appropriate their solutions, or students who would wait by the discard bin at the computer center for samples of the better students' versions to copy. Accordingly, the range of sanctions that were imposed was broad; in each case, the sanction was crafted to fit the circumstances.

The dynamics of that class are of some interest. With each problem set, the professor found that students were doing well, so he could crank up the difficulty of subsequent problems. As he did so, more and more students found that they could not do the assignments, and they resorted to copying the solutions of others, with or without permission. Meanwhile, the professor would continue to believe that students were coping with the more difficult assignments. This classic case of a positive-feedback loop resulted in unrealistic expectations on the part of the professor and a rising crescendo of desperation in the class, until "something had to crack."

As a result of the Course 100 case, MIT decided to examine cheating as part of the student culture. Questionnaires were sent to a large sample (891 students) of the undergraduate student body. Of the 44 percent answering, over 80 percent admitted to having cheated during that academic year. Even more significant was that, in general, students felt that almost all MIT students cheated at some time or other. In such an atmosphere, many students felt that they had to cheat in order to stay even. Most MIT students are not content merely to survive, they want to do well.

My own personal definition for cheating is "claiming credit for work not done." This definition covers most infractions, including plagiarism. The only addition needed is to include "abetting cheating by others," which is considered as serious as a primary offense. There is a broad range of actions that fall within the questionable category, but some are almost validated by tradition.

At MIT we have something called "bibles," which consist of files of previous examinations or term papers, usually preserved in fraternity houses. One of the fringe benefits of joining a fraternity is access to these files. Some teachers give the same exams or homework problems over and over again. There are legendary stories of professors who, in teaching a course that they had taken as students, find term papers submitted that are copies of papers they themselves had written when they were students in that fraternity. One hears of commercial companies that will supply students with copies of papers on any subject--I have never encountered such products personally and could easily be oblivious to ploys of this nature.

There can be interesting multi-cultural aspects which arise occasionally at a place like MIT. One of the cases that I did not participate in involved an Asian student recently arrived from Hong Kong for his freshman year. He had been accused of plagiarism in one class, and while the hearing on it was being scheduled, a second complaint of plagiarism came inagainst him from a

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different class. This, of course, did not elicit sympathy for him on the part of the committee that heard the case, so they suspended the student for a year. Subsequently, the committee received a letter from the father of the student, who wrote that he could not understand why MIT was so harsh on his son. For all of his life he had trained his son in the Chinese tradition, which was to copy the works of the great masters until one had thoroughly absorbed them. The mechanical act of copying the great works presumably imprints the intellectual ideas in the mind, and only after the works of the great masters had been mastered would one presume to do original work. This is an interesting argument, one which may have some scintilla of validity. However, the Committee on Discipline takes the view that while students are at MIT, they must accept and be part of the MIT culture. We make special efforts to work with the Advisor to International Students, so that every entering group of foreign students is informed of what the rules are at MIT with respect to plagiarism and cheating on homework assignments or examinations.

Cultural aspects surfaced again in another case where a Southern European student was accused of repeatedly being the donor of material for other students from the same country in several different courses. When this case came up, it appeared to be open and shut; since being a donor is as bad as being a recipient by our standards, one might have predicted that we would throw the book at the offending student. However, in preparing for the hearing, I consulted three distinguished professors from the relevant country, one from the University of Connecticut, one from MIT, and one from the Harvard Medical School. Each described his own experience as a student, and each said that being the best student, he was expected to pass the correct answers to weaker students; otherwise he would have been a social outcast in the local community of students.

One of these professors described how, during examinations, he would be surrounded by proctors, who knew the system as well as the students. Another added that his country had been ruled at various times by so many different foreign factions that the students had developed a "we-they" mentality against any authority. It was virtually considered a patriotic duty to circumvent faculty intentions. The offending student at MIT did, in fact, use "social pressure" as part of his defense, and I suspect we were a little bit easier on him than we might otherwise have been. Instead of asking for expulsion because he was a repeated donor, we recommended suspending him for a year. By all accounts, this was an outstanding student who did not himself need or seek illicit help.

One of the previous speakers claimed that a plagiarizer is always a repeat offender. I have some indications that, to some extent, he may be correct, although "always" is a strong term. There must be some who try it and do not like it. A student who had plagiarized material for a term paper was exposed by a visiting professor, who brought it to the attention of the committee. As the case was being prepared, the visiting professor, by chance, found that another colleague in the department had previously caught the student plagiarizing. That case had been handled within the department and one of the conditions set for leniency was that she had to write an essay on plagiarism. The student came before the Committee on Discipline and pleaded that this was her

first infraction, and that she had not known what plagiarism was. She was unaware that the Committee knew about the previous incident. Moreover, this student was already on probation for a completely different offense. As educators, we would like to believe in the possibility of reform, but sadly, there are repeat offenders who seem addicted to bad behavior.

In one case at MIT, a graduate student had received a master's degree and was working on her doctoral thesis, when the supervising professor found portions of the Ph.D. thesis that she was writing to have been plagiarized. He went back and examined her master's thesis more closely, and he found that she had done the same thing then. He brought this to the attention of the Committee, which was looking into recommending a recision of the master's degree, when the student threatened to commit suicide. The case then veered into a psychiatric mode and left the purview of the Committee on Discipline.

Another case involved a student who also threatened suicide if he were disciplined for plagiarism. After psychiatric consultation, the committee proceeded cautiously, and the student was suspended. This case had an added dimension in that the student was very well connected with people in high places, including a trustee of the Institute, who wrote on his behalf. This is a theme that we often see. It is apparently easy to obtain character references from well-intentioned folks, who are completely unaware of the dark side of one's personality and refuse to believe its existence, even when there is a proven pattern of repeated offenses.

Finally, there are sometimes larger implications of plagiarism. One case came to the attention of MIT through the popular press. An important official in a foreign country had been a graduate student at MIT some time ago. There were claims in the press that portions of his Ph.D. thesis had been plagiarized. These claims were highly publicized and, if confirmed, would have had powerful political repercussions for the government of that country. The relevant department finally decided to settle the matter internally, and we never received the case.

The Committee on Discipline does not initiate any cases itself; it adjudicates rather than investigates. Complaints have to be brought to the committee by some person or group at MIT. There is a natural desire for most institutional bodies to "contain" nasty incidents, to practice damage control, so to speak. This desire often leads to suppression of error signals that would help the whole institution to correct its behavior. Most complaints at MIT are handled at a very local level, with appeals or reviews moving upwards through established lines of authority. Each level of jurisdiction understandably usually tries to downplay the dispute to avoid embarrassing the institution. There is, thus, a built-in conflict of interest with great potential for whitewashing, even for serious incidents.

At MIT we have a standing faculty committee to handle complaints against students, but not one for complaints against faculty or administration. This omission is bound to create problems some day and perhaps has already done so. The Harvard Medical School found it necessary to establish the Faculty Conduct Committee as a result of a notorious case that went awry. In my opinion,

there is no substitute for an experienced standing committee with an established tradition of even-handed decisions. Ad hoc committees often mishandle cases because of inexperience; it is rare that any newly formed group can handle complex, emotionally charged disputes with dispatch and wisdom. What I have learned is that every case is different and trying to apply rigid rules in a mechanical way simply does not work and will not serve us well in the end.

SUMMARY OF THE CONFERENCE AND COMMENTS LESSONS LEARNED AND NEXT STEPS

Dr. Mark Frankel
AAAS

I want to extend a thank you, as a co-organizer of the Conference, to all our speakers, to those of you in the audience who have participated, at times vigorously, in the discussions, and to the ORI staff. My task is to try to distill some of the main themes that have surfaced during the course of the proceedings, as well as perhaps contribute some of my own thoughts on these matters. Admittedly, this is going to be highly subjective and selective.

Let me begin with one general observation that I suspect will be of no surprise to anybody who has attended any portion of this conference: there is a great deal of emotion associated with plagiarism, whether it is an allegation of plagiarism or a definitive finding of plagiarism. In part, I suspect this is due to what Ms. C.K. Gunsalus observed yesterday when she said that plagiarism touches a central nerve of all authors. Authors, of course, feel intellectually violated by acts of plagiarism. But I also believe that some of the emotion is a reaction to the way that the "system" has responded to allegations of plagiarism.

While we can acknowledge the fact that emotionalism surrounds the notion of plagiarism and the way we deal with it, I think we also have to acknowledge that we need some good empirical research on all aspects of plagiarism. What is going on out there? How effectively are systems responding to it? That is a theme that has been repeated throughout the Conference, the need for more empirical study, and I want to endorse that as well.

We have also learned, again primarily anecdotally, but also through the compilation of figures, that of the three major violations of scientific misconduct--fabrication, falsification, and plagiarism--plagiarism seems to be the most common of the three at research institutions, acknowledging the possibility of bias and the nature of this Conference and its participants. I also noted in my welcoming remarks, as Dr. Nelson Kiang did just a moment ago, that the incidence of plagiarism on the part of students is too high. Clearly, this is a matter that really needs to be dealt with seriously, for moral as well as practical reasons.

Morally speaking, plagiarism is an intellectual equivalent of shoplifting or thievery. It also constitutes a deception of readers of the particular work, whether or not any intent was present. It also wastes resources, which has both a practical and a moral dimension to it, as does its effect on the evaluation of credentials. A bloated curriculum vitae may lead one to presume that a particular investigator is more productive than he or she really is, and it may give that investigator a comparative advantage over the honest investigator when it comes to decisions on promotion and tenure. It is important, then, to respond effectively to cases of plagiarism, not only to redress individual grievances, but also to protect the integrity of science and to fulfill a central responsibility on the part of all those who would attempt to seek some self-governance for their work; that is, of course, accountability to the larger society in which they operate.

This leads me to make a few direct observations about some very specific matters raised during the last day and a half. I begin by looking at the notion of sanctions. There seemed to be a consensus that sanctions ought to be connected to the degree of severity and seriousness of the plagiarism, or to put it another way, that "the punishment should fit the crime."

I thought the presentation by Dr. Wiser from Tulane got us off to a good start in thinking about the seriousness of plagiarism. He had five criteria. Let me just briefly repeat those: (1) the extent and frequency of the plagiarism in the work under consideration; (2) the intent of the act of plagiarism (I assume that if one really plagiarized with an intent to deceive and it could be so demonstrated, then it might require a more severe punishment than for one who innocently plagiarized); (3) there may have been previous evidence of plagiarism in earlier work; (4) the rank and level and the training of the plagiarizer; and (5) the nature of the source material.

You will notice that four of those five criteria focus on the plagiarizer. I suggest that a sixth criterion for determining the seriousness of the violation is its consequences for the person who is plagiarized, as well as other colleagues, post-docs, or students whose reputations may have been tainted or whose careers may have been adversely affected. In this regard, we can take our cues from the criminal justice system, where sentencing is based in part at least on the impact of the crime on its victim.

It is also worth noting here the observations made by Mr. James Meeks from Ohio State about the range of sanctions available to institutions, which he thought were extremely limited. He gave the example of his own institution having the options to reprimand somebody or to dismiss and retract tenure. These were the polar extremes of the range of sanctions available for faculty at his institution.

I am not sure whether the experience at your institutions matches his, but if it does, it seems to me that we need to be more creative in thinking about sanctions. If you have some ideas about this or experience with regard to your own institution, I hope you will share them during the discussion period.

The second major topic I want to address is the publicizing of findings of plagiarism. I agree with those who think such findings ought to be disclosed, both within and outside the institution where they occur, or by the institution that has done the investigation, whether it is a university, the Office of Research Integrity, or the National Science Foundation.
Such publicity not only alerts others to these violations by the perpetrator, so that they might take certain steps to protect themselves from being subject to a violation at some subsequent date or perhaps to clean up the mess left behind, but it also sends a message about the integrity of the system used to detect and respond to allegations of plagiarism. The accountability that is required of the institutions of science requires not only that they can document that they are behaving responsibly, but also that their practitioners as well as outsiders believe that they are behaving responsibly. These are very difficult requirements to meet, it seems to me, if the process is hidden behind a veil of non-disclosure.

Now, while on the subject of publicity, I was intrigued by the planned effort by the American Historical Association to raise this issue with its members in the fall. It seems to me that the publication of findings of plagiarism is an issue on which association members should have a very loud voice, especially on the matter of whether or not the membership is prepared to deal with the threat of lawsuits. You will recall that this was one excuse for that particular professional society for not making its findings public, a concern that is not unique to that professional society. It seems to me that if professional associations are to be major actors in all of this, they need to be prepared to take some risks. All of us should be alert and sensitive to that and see how AHA attempts to deal with that particular issue.

While on the subject of publicity, our deliberations for the most part have emphasized publicity about findings of "guilt." We have heard very little about publicity that can help to restore the reputations of those who have been falsely accused. My sense is that we are not very accomplished at doing this, and I would welcome some ideas from all of you during the discussion period on this matter.

Now turning to a third subject area, which is education, I think we make a mistake if we assume that researchers, students, and postdoctoral fellows come to the scientific arena properly prepared and informed about the norms of science and the regulations that govern research in the United States. It seems to me that education is a very critical and needed tool to deal with this, and it is education that I think can be provided by the universities as well as by the professional associations.

But while I think we need to focus on education, we need to keep in mind that to some extent the

norms are in flux; there is not always agreement on what the articulated standards ought to be. I think the session regarding computers did us all a service identifying potential concerns with which we need to begin to grapple.   In our educational efforts, we need to be very careful, not only to point out where there is a consensus on the norms, but also to be honest where there is disagreement and to wrestle with this along with our students.

One of the advantages of education with regard to ethics and research is that it does allow for the discussion of the role of personal responsibility in making decisions on these matters and how the system affects our ability to make such decisions. I think people ought to be more sensitive to this. We need to acknowledge that the system creates pressures on all of us in the research arena, but we also need to disabuse people of the notion that "the system made me do it." It seems to me education is one route toward achieving that.

But reinforcing the importance of personal responsibility in such matters as research ethics will ring hollow if individuals do not believe that they have the authority to take action on that responsibility. This means that those who bring allegations of plagiarism have to be convinced that the system is prepared to follow up on those allegations conscientiously and seriously and that their efforts to blow the whistle on such allegations will be received as an attempt to contribute to the quality of science, not as an act of betrayal or an uncollegial act.

Finally, a few words about education in research ethics with regard to a multi-cultural environment. For a variety of reasons United States' research institutions have been a very attractive haven for foreign students and scientists and engineers. Although science claims to be an international enterprise, the fact is that norms and practices are always subject to interpretation. The problem is compounded when people come with different ideas about the way things ought to be done in science, and I think Dr. Nelson Kiang has sensitized us to that in his presentation. The point I wish to leave you is a plea to be more sensitive to these cultural differences and to conduct some good empirical research on the nature of these kinds of differences--where they occur, how they occur, why they occur, how often they occur. There is very little empirical work on this, but I suspect that it is more of an issue than we have yet to recognize. We need to do such research, it seems to me, if we are going to design some effective educational strategies. I was impressed with Dr. Kiang's description of how MIT attempts to deal with this matter by working with its Dean of International Students. I personally would like to hear how some of your institutions have attempted to deal with this matter.

CLOSING REMARKS: ORI

Dr. Alan Price
ORI

I will not attempt to add to the eloquent remarks made by my colleague, Dr. Mark Frankel of AAAS. Let me first thank the Conference Planning Committee, which included Dr. Lyle Bivens, Dr. Clyde Watkins, and Dr. Larry Rhoades of ORI; Ms. Deborah Parrish from our Office of General Counsel; and Dr. Al Teich and Dr. Mark Frankel from AAAS. I also wish to add a disclaimer, because many people have given me credit for a number of things at this Conference that were actually Dr. Frankel's ideas; in particular he contributed the ideas for the first sessions to add the historical association context and the last sessions on the computer era, forward looking, and multicultural diversity. We have been equal partners in the planning of this Conference, and ORI is very much grateful to AAAS and Dr. Frankel for their efforts. We also thank Ms. Karen Gorirossi and Ms. Tracy Sumner of ORI who have done all the staff work for the participants in the Conference.

In not adding to what Dr. Frankel said, let me just remind you that the responsibilities for integrity in science lay with a variety of people within the system. The system is not one person or one organization. It is everyone in science. First, there are the individuals that conduct research: the principal investigators, mentors, students, postdoctoral fellows, and others involved in the research program. Then there are the institutions that are responsible for their activities and submit their grant applications. There are the professional associations of which the scientists are a part and which help oversee their ethical responsibilities. There are also the granting agencies that give money for research, particularly those in the Federal Government. And last, we have the investigative offices, such as the Office of Research Integrity in the Public Health Service and the Office of Inspector General at the National Science Foundation or other agencies of the Federal Government.

Many of you have suggested that ORI might "do something" that would exceed the scope of ORI's Federal authority; ORI is only responsible for PHS-funded research or applications to PHS. Other things may well not fall within the PHS definition of scientific misconduct, even though they are unprofessional, unethical, inappropriate, and make people very upset and angry. But we in ORI do not have authority to deal with them, so we plead for some sense of the community's responsibility in handling these issues.

DISCUSSION SESSION #5

Mr. Leonard Minsky (N.C.U.P.I.): It seems to me to be part of the student culture that over 80 percent of students admitted to cheating. Would you comment on that?

Dr. Nelson Kiang (MIT): Yes, apparently this is not unusual. Rutgers did a questionnaire, which came up with somewhat the same results; I think a number of campuses across the country all came up with about the same, so this is not an "MIT problem." Everyone thinks of MIT as the place where there is so much stress on the students; the competition and the level of work is much higher, for instance, than Harvard, which is the other institution that I know thoroughly. It has often been said that it is because of this stress that students respond to it by cheating. I do not believe that is the case. I think that it is a generational problem. The longitudinal studies or questionnaires seem to indicate that this is not generational, that it was always this way. But every senior professor that I know of who has been around for four decades or so has a different feel for it, that in the last 15 to 20 years this has gotten much worse. But we have nothing to back that up, because no one really was interested back then.

Mr. Minsky: Right. The culture is a non-ethical or an anti-ethical culture, and that is what we are looking at. Related to this and as a matter of curiosity, on Dr. Frazier, he seemed to confess easily, he provided you with the documents from his garage, and he resigned. This stands in stark contrast to the Leonard Freeman situation at Albert Einstein, where Dr. Freeman has not resigned and the institution continues to protect him. Could you comment: Dr. Frazier did not seem to think he was doing wrong.

Dr. Kiang: Well, I cannot speak to his state of mind. But when the charges were originally brought, he offered us hypotheses, such as his secretary might have left off the quotation marks and all the usual stories. We had to work through that before we came to the final compelling evidence. So I cannot speak for how other institutions handled it, but in this case it was pursued rather vigorously by our committee.

Dr. Heidi Weissmann (NY): I was wondering if you can comment about the difference in cultural diversity, particularly in the excuse put forth by the father of a student from an oriental culture. While there are, I am sure, valid differences in the way different cultures look at this, I was struck by my own personal experience. I actually had to go to the Republic of China when Freeman first denied that a book existed; when we confronted him with the existence of a book, he then just did not remember how my chapter got into that book with my name off and his name on it, and maybe the people in the Republic of China did it. So I ended up going to the Republic of China to bring back the evidence that, in fact, he had sent it to them that way.

When the editor of the book read Freeman's testimony in court about maybe the people in China did it, taking my name off and putting his name on, he said maybe he just sent them a batch of reprints. I am paraphrasing, but that was the gist of what he was saying. The editor was appalled that an American medical professor would impugn the Chinese editor's reputation or that of his staff, to suggest that he or anyone on his staff would do such a thing. I remember that his language was very graphic to me. He said, "Please forgive me, because we consider this a serious breach in our culture what Freeman has done, and we consider it like intellectual rape; Freeman is saying that I, Professor, chopped off Dr. Weissman's head and replaced it with Dr. Freeman's--we do not do that here."

So to me the excuse put forward by the father--while I think there is validity to a difference in how cultures view it--is just another variation of "the secretary did it" or whatever, because in fact there are cultures that consider it as serious an offense, or even more serious an offense, as well as maybe less serious.

Dr. Kiang: Yes, I did not mean to imply in any way that other cultures would in fact condone plagiarism. I only presented to you what the father represented as his own personal attitude. I consider that to be actually a "bogus argument." However, there is a broader consideration, which is that of the role of credit in our society and the problem of plagiarism are intimately bound with that, because in societies where the individual's contribution is not highlighted so much, the problem of plagiarism is a little less important.

If you view the totality of intellectual work as arising out of society, in that no one individual should be credited with anything more than any other, then the assignment of credit automatically becomes of less interest. In many of the ancient societies, like the Egyptian priesthood and many of the older Chinese or certainly the Sufi philosophers, you make a practice not to write anything down, because you do not want the distilled knowledge from the past to seem as if it is coming from you as an individual. Knowledge is submerged in that of the entire culture, and the wise man simply learned this and repeated it to the next generation.

But we are living in this particular society, and there is no way that you can condone the plagiarism by people who are in the system and are old enough to know better. Many of the students actually do not know what plagiarism is. I had one student who was an engineer come to me. He was accused of plagiarizing something that he had written, and he genuinely did not know that you had to attribute anything to anyone. He said that he had worked one summer at a very large company, where the company brochures were simply written by groups, and there was no authorship attached to these. All he did was to take some of those brochures and put them together in writing his paper, and there was nobody to whom he could attribute them. He thought this was, in fact, the standard engineering practice. I had to agree that, in the small sample that he had, this may even have been true.

When I started to describe to him what the values of the academic culture, he at that point said, well, it is a good thing he is not going to become an academician, because he found these ideas too arcane for him, and he became an engineer, of course.

Dr. Jane Rosen (NY): I want to bring up the whole concept of exploitation. In graduate education, it is classic for mentors to take the credit for their graduate students' work; in postdoctoral fellowships, this is rampant. Traditionally, universities take the position of protecting the professor against the student's position of intellectual property rights. I have been the actual victim of this sort of thing, where my Ph.D. dissertation was appropriated by my mentor without permission or knowledge from me, and it was actually published. The university basically exonerated the professor of any transgression. I was just wondering what position the MIT committee might have taken or is taking in situations where a student's work is appropriated without his or her knowledge.

Dr. Kiang: I cannot speak for anybody else, but I can give you my personal opinion about it, one that I have expressed many times at both MIT and at Harvard. I believe that to be a very common situation. The problem at MIT is that there is no standing committee where a grievance from a student can be brought against a professor. You have to bring it up the administrative line, which automatically has conflicts of interest, since the chairman of the department knows he has to deal with this faculty member if he is tenured, for life, and the student will go away after a while. There is a built-in conflict of interest as well for the dean of the school, in that he does not want to do anything that his chairman does not support. So I have been arguing for a standing committee at MIT for quite a while. Just before this meeting, I talked to the Chairman of the Faculty to get the sense of the faculty view at this moment; he said that there is no support for this at the moment among the faculty.

However, appropriating the work of students has a very interesting twist on it. If a faculty member signs off as supervisor on a Ph.D. thesis, he is really testifying that this work was done independently by the student, which is the condition for the Ph.D. So you cannot appropriate the work of that student and say that it was not original because you did it and at the same time sign off as a supervisor. So it seems to me the logic is irrefutable; one way or the other, the faculty member is on the horns of a dilemma.

Dr. Charles McCutchen (NIH): But he does not get credit, he does not get punctured by it. On this business of, "You cannot have it both ways," his answer is "Yes, but I just did."

Dr. Kiang: This is our problem. Our university systems and probably all of our institutions have not faced up to these issues. I would not be optimistic about the timetable. You are talking about a culture that has been there for a long time.

Dr. Rosen: It is time that they do. The student is suspended, and the professor can keep his career.

Dr. Edward Huth (OJCCC): I would like to come up with a remedy possibly for this situation. My understanding of copyright law is that you possess copyright as soon as you complete the work, but that as a matter of legal defense, you may have to register the copyright. But at least in legal theory, you own copyrights. So that I would raise the question as to why should students not put a copyright notice on each piece of paper completed?

Dr. Rosen: My thesis is copyrighted.

Mr. Walter Stewart (NIH): But it is not a legal question. It is a scholarly question of academic values. I wanted to ask Nelson Kiang why some institutions, like Harvard Medical School, apparently seem to take this sort of stuff seriously, and in other institutions, the cultural differences we are dealing with, in places like Albert Einstein and NYU, "sanction plagiarism" and defend the senior person. How are we going to address these many institutions that will condone and do not censor or do anything about the most flagrant cases. What are we going to do about that?

Dr. Kiang: Well, they will not respond until the legal heat is turned up.

Mr. Stewart: But Dr. Heidi Weissman won her case. She won it all the way up through the Supreme Court, and Einstein says that has nothing to do with academic values.

Dr. Kiang: The legal path is not the way to go. It is a cultural matter, and that means publicity, education, and sensitizing people as to what the real issues are. The reason why the Harvard Medical School, I think, handles these things better than most institutions is because you "clobbered them."

Mr. Stewart: Maybe you could help out with Albert Einstein here. In other words, if the leaders in the community, like Dr. Drummond Rennie, Dr. Edward Huth, and a whole bunch of other people, would say what Albert Einstein is doing is simply not right--never mind if it is legal or anything else; I mean, Heidi Weissman won her suit. If you look at that and say, is this the right academic approach, maybe they would climb down off their "soap box" and stop hurting whistleblowers and defending the faculty who have done this egregious plagiarism.

Dr. Kiang: I am inclined to think that the pressure ought to be turned on the organizations that publicly regard themselves as being the umbrellas for academic institutions, like the American Association of University Professors, American Association of Medical Colleges, institutions of that type.

Mr. Stewart: No, institutions do not ever do anything. It is individuals, who are committed to something within those institutions, that change the institutions.

Dr. Weissmann: Excuse me. These same institutions that will "thumb their noses" at court decisions and Congressional investigations will also thumb their noses at American Association of University Professors (AAUP). Yeshiva University has been on the censure list of the AAUP for more than a decade, long before I came along, and they have been reported each year. There are years where it says the administration has not been responsive to our requests to try and discuss the problems, a violation of faculty rights and due process, etc.

So it is almost as though whatever works, we then say, well, you need to do something else. If the peer review does not, if the court works well, we really need to have AAUP look at it. Well, AAUP has a censure list of which Yeshiva University has also been on. If that does not do it, then a Congressional investigation. Well, Congressman Weiss did a three year investigation and had a report. Where does it stop? When do the institutions finally get a message that these kinds of behaviors are not to be promoted, defended and protected, but are to be dealt with openly, honestly, honorably, and ethically? Where does it stop? They have a limited amount of time and money compared to individuals to keep it going; as long as they have an institution, they want to keep it going. That can be that if that institution is such that they will outlive an individual.

So how do we make the milieu change, so that we do not have the "dead bodies" of people who stand up? Now I like, instead of whistleblower, Roger Beaujolaise's term, truthteller, or the Glazier's term, ethical resister. How do we do enough to help people who are standing up for what we all espouse, that words are important and to be valued. But how do we support the people who actually take action to try and promote that, so that we do not wind up with all these "dead bodies littering the field," whistleblowers who are just trying to do the right thing and believe the graduation speeches, the handbooks of policies, and these society guidelines, only to find there is a huge chasm between the words that are published by the institutions, societies, and associations versus their actions, when you try and just fulfill what you are told and tend to believe it is your professional responsibility.

Dr. Kiang: Ultimately you have to get to the trustees, and they will respond to economic arguments, if nothing else.

Dr. Drummond Rennie (U.C.S.F.): What has to happen is that our professions think it is a good idea to behave enormously appropriately. The profession is not the deans nor the trustees and so on. It is all the academics and all the scientists out there. Going to law (as has been demonstrated many times in using copyright law) does not do it. It is a professional, ethical matter, not a legal matter.

Dr. Kiang: It is very hard to get unanimity in the entire profession.

Dr. Rennie: You do not get unanimity, but you certainly get a majority, and it should be a vocal one.

Participant (Unidentified): What would work is if journal editors did not accept articles from those institutions. That would put pressure there. (Applause)

Mr. Stewart: If they simply printed a list of things they do not approve of each month: in other words, just like the AAUP, say we still censor Albert Einstein for still supporting Freeman and still denying Weissman, or Rosen and Polsby elsewhere. If these institutions were held up to public scrutiny, I think that would help a lot. If we could get the rest of the journals interested too, I think this thing would go away real quick, the most flagrant abuses anyway.

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Dr. Marcel LaFollette (G.W.U.): To pick up on something that Dr. Mark Frankel of AAAS said, that it might be useful to find sanctions that would enable us to have "the punishment fit the crime," I think one of the difficulties with plagiarism as opposed to many of the other types of misconduct that we discussed is that it is almost "impossible" to have the punishment fit the crime. That is, the plagiarism itself continues to exist in the literature and in the library. We do not rip the page out of the journal, and even a retraction does not really truly bring it back. Thomas Mallon said it very well about books: the plagiarized book and the plagiarism itself will sit side-by-side on the library shelf for as long as that library is there. Is there some way we can turn this to our advantage in an educational setting? I know that in the history of technology, where there was a rather notorious plagiarism 20 years ago, many of the professors so touched by the episode themselves used that book in their graduate courses for the decade following as a way to instill in their students both a "horror" or repulsion and a determination never to do such a thing themselves. Is there some way in which we can use the examples of plagiarism in our educational setting?

Dr. Mark Frankel (AAAS): I think teaching by example, good or bad, is a good methodology, and I suspect that there is a way, although I have no precise way of defining that for you. I do not know whether it is impossible to devise an appropriate scheme of sanctions to deal with ranges of plagiarism; I do not believe we have tried hard enough to think about that in the contemporary environment. I do think, however, that it does present institutional officials with some difficulties, when the only two options they have is the private slap-on-the-hand reprimand versus throwing someone out of the university or denying tenure, whatever might be the case. I do not want to see them have this as an excuse for not
dealing effectively in applying sanctions. I think what happens, although I do not have the data, is that they end up dealing with it more gently than they probably like to. But the fact is they would be hitting someone over the head with a "bloody shovel" if they went the other route, and they do not want to put themselves in that position. I think we need to find alternatives to those two and work with them to see if there is some way of designing some sort of reasonable, fair, and effective sanctioning system. Although again, as I hope I emphasized in my talk, I think in the

long term that education is the way to deal with these things, both by example and in the context of the classroom. We need to work on more effective strategies for dealing with this issue, and I would certainly support and would like to see it receive more emphasis than it has in discussions about plagiarism.

Dr. Drummond Rennie (U.C.S.F.): Dr. Frankel has answered very well on a problem that runs throughout this, which I think must cause violent difficulty. For example, if an institution does not behave properly, the Government sanction options are to do nothing or to withdraw Federal funds. Now, in my own institution, the University of California San Francisco, which I believe has behaved pretty well, withdrawal of Federal funds would shut down San Francisco. You just cannot do it. What we are talking about here is lesser than the "hydrogen bomb," but a very big "bomb." I think we should really think very hard in the wider sense and also specifically with plagiarism about putting in sanctions. I think it is very important, because otherwise people are always going to walk with nothing done at all.

Dr. Michael Burlingame (Conn. Coll.): I would like to address the question that was raised by Dr. LaFollette on how we can make use of plagiarized materials to help educate our students. Earlier this month I was at Gettysburg College, and the historian and chairman said that he teaches the course in historical methods to his seniors. He assigns each one 50 pages from Steven Oates's biography of Abraham Lincoln and 50 pages from Benjamin Thomas's biography, and then he has them draw a conclusion whether Dr. Oates has committed plagiarism. So that helps: use a plagiarized book.

Somebody suggested that an umbrella organization, like the American Association of University Professors (AAUP), might be the most appropriate forum for dealing with these issues. I asked a spokesman for the AAUP a couple of years ago whether they would be willing to serve that function. He said we are interested in protecting faculty members from administrators, not in adjudicating disputes between faculty members.

Dr. Rosen: I believe that the Office of Research Integrity could play a very significant role in adjudicating in their own way these issues. I feel very badly because whistleblowers have called me up and asked me whether I suggest that they throw out their complaints to ORI. Unfortunately I cannot recommend that, because I do not feel that justice was done in my particular case. The professor who appropriated my dissertation without permission, and basically allegedly "plagiarized" it, was not sanctioned by ORI, and the case was closed in a very preliminary inquiry stage by the institution.

I feel that ORI is not really facing the issues properly. But I do believe that the ORI can make corrections in their ways of thinking on an academic scale, and they can make a statement. Unfortunately there are a lot of whistleblowers, who are even present in this audience, who are afraid to approach them because they feel it is a "whitewash." I think that is a very sad statement because the ORI does represent an organization which can sanction universities and can remove

grant funding from these perpetrators of fraud, and I feel that there is some problem in their ability to act and make things happen in that direction.

Dr. Alan Price (ORI): It was a university inquiry. After not being satisfied with it, Dr. Rosen brought her materials down to OSI, and we had a full day of further discussion on her case. We felt it was very unfortunate that she was not able to publish her Ph.D. thesis, because independent work was published by her mentor. However, the institution concluded that there was not evidence to warrant an investigation of plagiarism, given the long-standing collaboration between the mentor and the student, followed by the reproduction of the work and the use of new words by the professor. So no finding was made to conduct an investigation, and it was not scientific misconduct. It was a difficult situation, but the institutional inquiry finding was that it did not warrant further investigation as scientific misconduct.

Dr. Rosen: I think it sends out a very dangerous message when dissertation mentors can appropriate a dissertation without attribution. It sends out a dangerous message to the entire graduate education system in this country if dissertations are not protected as original documents.

Dr. Jerome Rosenberg (Univ. Pittsburgh): I was heartened by the growing experience of many of our academic institutions that were revealed at this conference to deal with the problems "on their own turf." This is something new for universities and colleges, and there is going to be some stumbling along the way. But I think that many institutions have risen to the challenge, and they have not all been represented on the program here. I think that some of the errors of the first trials have been corrected in subsequent iterations of the process or other cases. I think conferences like this are very useful for "strutting" the experience from those institutions that (unfortunately) have had problems to deal with, but have dealt with them, to inform institutions that are learning how to deal with their first case, which may come tomorrow.

Dr. Leonard Saxe (C.U.N.Y.): I have been struck, as Dr. Frankel was, at the emotion of people here, particularly those of us who have been victims of plagiarism. I think as we have talked. there has been this tension between the rights of an individual, either as a victim or as a perpetrator, in institutional issues. I think that Professor Rosenberg's comment about institutional responsibility for these problems is for me the critical outcome of this meeting. That is, if plagiarism continues to be just a conflict between two people, then nobody is going to win. Unless institutions take responsibility, at one end, for dealing with the problem and openly acknowledging it, and at the other end, for making sure that their promotion and reward practices and their decision making practices do not promote plagiarism, do not set up the conditions where people engage in this behavior, then we will not have had much success. I hope all the discussion here of institutional responsibility, whether it be ORI or individual academic institutions, is a very important outcome.

Dr. Kiang (MIT): Yes, I think it is important that we recognize that plagiarism is not just against an individual author, but it is against the system, and it is the system that has to correct it.

Dr. R. Douglas Wilkerson (Medical College of Ohio): As a scientist and one who has to deal with these issues through my institution, I would agree with Dr. Rosenberg that I do not want ORI to be "in charge" of all these things. It is an institutional responsibility. It should be handled at the institutional level, with appropriate support from ORI where required. I think our institution takes its assurance to ORI seriously, and it will do whatever it has to do to deal with individuals who are brought to us, either by ORI, a journal editor, or whomever. I would hope that all other institutions would do the same, so that we would not have a situation somewhere in the future where we would have a "Big Brother" that is in charge of all this.

Dr. Kiang: I think ORI is more in the role of a referee, or should be. It is the institutions that should really do it. The problem is that the institutions cannot really agree upon what "game" they are playing. Some think they are playing water polo, some that they are playing soccer, and some that they are playing football. It is really ORI that should say, "This is the game we are playing, and we are going to be the referee."

Participant (Unidentified): I am intrigued by the 80 percent incidence of cheating at MIT and other institutions, and Dr. Kiang's comment that he did not think that stress was a factor. With all due respect, I think that is going against the principle of Mr. LaChatliere, which says that if there is a stress placed on a system, the system is going to react in a way to relieve the stress. That is one, possibly simplistic, view. The plagiarism will undoubtedly increase with that 80 percent base that you have described.

Dr. Kiang: I did not say that stress was not a factor. What I said was that I do not think that MIT was "special" in having a "special stress." There is no question, in my own opinion, that it is the stress of success. What we are telling these young folks is that "you have to succeed." We held an MIT colloquium on the topic, success and/or honesty, simply to set up that dynamic tension for them, to consider whether doing anything is justified on the grounds that it will make you successful. If you want the success so much, what will you not go out and do?

Dr. S. Yoshikami (NIH): When I think of plagiarism, I think I know what it is; but the more I think about it, the more I get confused. What brought that to my mind more sharply was the poster exhibit here by Kell Julliard, which I hope everyone gets to see. It points out that some people will see plagiarism and it does not look like plagiarism. I was wondering, of Dr. Kiang's 80 percent of the students he said were cheating, what percent admitted to plagiarism and what percent did not really know what plagiarism was? I think knowing is a very important point.

Dr. Kiang: I do not think the technical act of plagiarism is the important thing. The important thing is whether you are claiming credit for work you did not do. That is the problem, and that will cover most forms of cheating; it will cover plagiarism. It is all right if you do not claim any credit for anything, but if you said, I did this, and you did not do it, that is the problem. That is why you should not be confused as to what plagiarism is. It is very simple; it is claiming credit for work not done.

Mr. Minsky (NCUPI): One of the things that is a glaring omission in this discussion is the role that the institutions are playing in perpetuating the culture that the students share. If we take Dr. Kiang's observation that there is no standing committee at MIT that will accept student grievances against the faculty, we have to ask what that says to the students. I think that there is a problem here, that the institutions are not being addressed as a source of the problem, but instead are being trusted to resolve the problem. Our experience at the National Coalition has been that in every case brought to our attention, the institutions are complicit with the problem, and are the problems and the obstacles to resolving them. The people in this room who are the victims of those institutions do not see in this discussion the existence of another institution, ORI for example, as a resolution of their distrust of the system. So it is the system indeed that perpetuates the problem, the system that needs to be reformed. Part of the system are the institutions themselves, and there is a really inadequate attention to the role that the institutions are playing in protecting perpetrators.

Dr. Kiang: Perhaps you ask that as a rhetorical question: what signals are we sending to the students? I can tell you what the students think, because many of them tell me. Basically the message is that "might makes right." If you are powerful enough, you can get away with "anything."

Dr. Kay Fields (ORI): In a "previous life," I was at MIT. It seems unbelievable to me that the students at MIT could be "squelchable" enough to be taught about plagiarism, in various courses or perhaps at their mother's knee, and not feel that if they had a charge to bring against a professor, that there was no one at that institution who would hear them. Who is, according to their assurance to ORI, supposed to receive allegations? I think it is very important within the institutions, just from my personal observation, if a student will stand out enough to make an allegation, it is very important, then, that someone within the institution take over that role. Because it is a very unequal battle if you have a complainant who is a student and has noticed this and been told to come forward, and a professor who is resisting with all his might and power. Why cannot an institution see that they need to take over the role of the complainant in such a case? Why does it not happen, or does it?

Dr. Kiang: Of course. Because if you are one of the ones with power, you would be reluctant to relinquish it. This problem is not limited to just, say, plagiarism or cheating. You have a problem with sexual harassment, which is exactly the same issue. If there is an imbalance in the power that is held by adversaries, then you need some sort of neutral and disinterested forum, in which the facts of the case can be debated. In institutions which do not have that, you are going to see these problems arising again and again and again.

Dr. Harold Orlans: As an independent scholar, I found that the institutions have mechanisms for handling sexual harassment and charges of discrimination. Why do they have no mechanism for handling charges of intellectual plagiarism?

Dr. Kiang: We have the same mechanism for handling both. We go up through the administrative ladder.

Dr. Virginia Marcum (NIH, Editor, *Journal of Clinical Chemistry*): Those mechanisms work equally well, which means they do not work at all.

Dr. Kiang: Well, no, that is not fair.

Dr. Marcum: It does not matter if the complaint is sex discrimination, sexual harassment, censorship of work, misconduct of one sort or another. The senior people tend to be protected, and the junior person is considered the troublemaker.

Dr. Kiang: No, it is unfair to say that they do not work at all. The fact is that they work most of the time, but sometimes they do not work because of this, because you do not really have disinterested people who are deciding things.

Dr. Lawrence Rhoades (ORI): Let me say that a number of professors are found to have committed scientific misconduct. In some cases it does work. What the particular parameters of those cases are we do not know yet, but about half of the people who are found to commit scientific misconduct (at least for the first couple years of ORI) have been what you would call senior professors, either associate professors or full professors. So it is working in some instances; maybe it is not working in others.

Dr. Marcum: You talk about the difference between a hand slap, nothing, or the "hydrogen bomb." I think it is very important to have publicity about the people who have been found guilty of plagiarism or intellectual theft. That does not close down an institution; that does not end a whole department. But it does make sure that the people involved are identified. If they retire from the institutions or resign, or whatever, and go to another institution, I think it is very important to have a bulletin board or a data base of some sort, so that, for instance, a journal editor getting a manuscript can consult a list of names. It does not mean that the manuscript being submitted is plagiarized, but it would be viewed with special care. I would like your comments on that please.

Dr. Mark Frankel (AAAS): I would like to echo at least the main thrust of your suggestions. They certainly sound plausible. I do not think it is possible to truly be held accountable without disclosure and publicity. We have to be more open and devise mechanisms to help one another within the community and outside the community to be alert for these kinds of violations. If you poll a group of lawyers about this issue, in terms of threats of lawsuits, you will find about 50 percent (at least in my experience) will lead you to worry, and the other 50 percent will see such threats as very exaggerated. I think in reality it is probably exaggerated. But nevertheless, the notion of publicity is very, very important, and I am particularly glad to hear it from the journals.

Dr. Rhoades (ORI): I am sure there is going to be very wide publicity on people who are found by ORI to have committed scientific misconduct. As of yesterday (June 21, 1993), ORI started printing the names and cases in the *Federal Register*, *The NIH Guide To Grants and Contracts*, and the *ORI Newsletter*. It is going to be on the OASH electronic bulletin board. I am sure reporters from the *Chronicle of Higher Education*, *Science*, etc. will pick this up at some point. I think people who want to know are going to have access to the information about PHS cases.

Dr. Marcum: That is fine for ORI, but I am talking about the other institutions as well.

Dr. Kiang: I would like to make a comment on the publicity. We wrestled with this problem on the Harvard Faculty Conduct Committee for a long time, and it is a little complex. To begin with, I think everyone would agree that you do not want to publicize cases of "dismissal of the case," because for many people, even an allegation against them is a negative factor. So you are only going to publicize the cases that turn out the other way. But quite often there are appeal processes within the institution that you have to worry about. So you have to worry about at what stage is it going to be publicized. But I do agree that at some point, it should come to closure.

Now, with respect to students, you have a different problem. The Buckley Amendment basically says that a student's academic record has to be kept confidential and is available only to those who have to have that information. The MIT lawyers have interpreted the disciplinary record as being part of the academic record. So one of the problems is that a student who was disciplined at a departmental level could do this in 21 different departments, and no one will ever know that he or she has done anything wrong before, because the records are kept in the department. So this year we started a central repository for disciplinary records, so that when things are done at the departmental level, a copy of that will go to the Dean of Students.

Dr. Rhoades (ORI): In dismissed (no misconduct) cases, there is a provision in the PHS regulation which says that an institution should take steps to restore a reputation that has been besmirched by an investigation. However, I think that is given a wide parameter. And certainly if the person involved (the respondent) objects to it, I am sure it would not be done. It would only be if the respondent desired it.

Dr. Edward Huth (OJCCC): I think I should comment briefly on the matter of journals giving attention to misconduct. The climate is changing, and I think the excuse frequently used in the past, the question of legal action, is beginning to evaporate. I was involved in one episode when I was with the *Annals of Internal Medicine* in which we inadvertently published what was a duplicated report by persons at Johns Hopkins. I simply made a straightforward statement that this paper, when compared with a previous paper (and I gave the citation) appears to be exactly the same clinical study, and I briefly gave the evidence. That was the end of it.

The American Diabetes Association last year dealt with the republication of a review article by a prominent diabetologist on the West Coast. They set up an *ad hoc* committee, which reached the conclusion that the second version had been submitted without the author's attention to prior publication. The August 1992 issue of *Diabetes Care* had a two-page statement by the *ad hoc* committee. It had a response by the responsible author, and I wrote a short editorial about related matters. I think this one was accelerated.

It may not be satisfying in the short run. But I think for someone who has had a perspective over a number of years, it is changing. I mean, it was not too many years ago that the editor of the *Journal of Clinical Investigation* said "We don't publish retractions." That simply would not fly any more, so the climate is changing. I do think that the National Library of Medicine has been very helpful in this regard, by drawing attention to retraction statements and so forth to citations in the Medline data base. There may be other devices of this kind that will accelerate the change.

Dr. Del Bennett: In order for an institution to receive funds through one of the conduits like NIH, there has to be an investigative process in place in the institution. The question is what kind of criteria does NIH employ in making a decision to drop a case as one alternative to conclusion or to continue it? I am willing to concede the court involvement in the Abbs case. Has somebody made the point that years have gone by and the conditions have changed? That is another dimension in judgment.

Dr. Clyde Watkins (ORI): I would like to address this to a limited extent. That investigation was not dropped; it was interrupted for a long period of time by a District Court decision, which prohibited ORI from having any activity in Wisconsin's Western District. I assure you that the case is still active, and no one in ORI has yet said we give up or see no reason to pursue this.

Dr. Charles McCutchen (NIH): Dr. Kiang mentioned the idea that if an individual is found innocent, the case should be kept confidential. The problem with that is that you can never tell; the public has no chance to look at the investigation and the decision making process and see if it was a whitewash. So if whitewashes are occurring, they will be in that concealed file. I think we should all thank the gentlemen from ORI for being willing to "expose" themselves to this. [Audience applause]

Dr. Emanual Stadlan (NIH): The discussion has gone in many, many different directions. An issue that has been mentioned yesterday by Dr. Gunsalus, which Dr. Kiang reminded me of when he said truth is a valid defense against libel: the importance of first establishing facts. I think that is terribly, terribly important. One really cannot continue to discuss the pros and cons, the rights and wrongs, without knowing exactly what the facts are. A deliberative body can find the facts, show that the accuser is incorrect, or that the accuser is correct. A deliberative body can also find that they do not have the facts in order to make a decision. In such a case our legal system says that a person is innocent until proven guilty.

This does, however, create a problem, and I just wonder whether or not Dr. Kiang or others who have been on deliberative bodies can address the issue as to what kind of announcement do they contemplate making in a circumstance where they do not have the facts to allow them to make a decision?

Dr. Kiang: I can explain to you that in the Committee on Discipline at MIT, our decisions are made in two phases. The first is the fact finding: is the accusation true? The second involves the sanction. Those are separate processes, analogous to the judicial process of the finding and the sentencing. The finding is made on a criterion of "clear and convincing

evidence," rather than on the criminal criterion of "beyond reasonable doubt" or the civil criterion of "preponderance of the evidence." So there is always a decision. If there is insufficient evidence to provide a clear and convincing case that the accusation was true, then we dismiss the case.

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Dr. Lyle Bivens (ORI): As the person who had the first word in opening the conference, I thought I would have the last word. The last word is I thank you all very much for your attendance and participation. I would point out that OSI and ORI have always been in the midst of an interaction between scientists, academicians, academic institutions, and individuals who are scientists that is inherently adversarial. One of the criteria that I set up as a measure, whether ORI was doing a good job or not, is whether we are "catching it" from everybody. And judging on the basis of this Conference, we are doing well on both sides of this issue. So thank you again for your participation.