

## **Readings**

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### **Misconduct in Science – How to Avoid Common Traps**

- 1. Scientific Fraud, Not New, Not Rare, but also Not Common**
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#### **1. Scientific Fraud, Not New, Not Rare, but also Not Common**

The posting below looks at issues regarding scientific fraud. It is by Amy Adams on a speech given by Donald Kennedy, editor-in-chief of Science and president emeritus of Stanford University. It appeared in the January 25, 2006 issue of the Stanford Report. <http://news-service.stanford.edu/news/medical/index.html> Copyright © Stanford University. All Rights Reserved.

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Donald Kennedy, editor-in-chief of Science, explains in a Jan. 20 lecture why research fraud is so hard to detect. The journal recently learned that it had published faked stem cell findings.

No journal has an infallible mechanism for detecting scientific fraud, according to Science editor-in-chief and Stanford University president, emeritus, Donald Kennedy, PhD. "Scientific fraud is not new and is not rare," he said during his talk at a stem cell symposium held Jan. 20. "Luckily it's not common either."

The remarks were prompted by Science's recent decision to retract two papers it had published from South Korean researcher Hwang Woo Suk after the data in them was found to be fabricated. In the first, which appeared in the magazine in 2004, the scientist and coauthors claimed to have cloned a human embryo and extracted stem cells. In the second, which ran last year, Hwang and his colleagues reported that they had honed the technique so that it would require fewer human eggs to produce a line of stem cells. These advances, if true, would have laid the groundwork for making genetically matched stem cells for human therapies.

The symposium, "Beyond the Embryo," was hosted by the Stanford Center for Biomedical Ethics' Program on Stem Cells and Society with the intention of reviewing some of the ethical and societal issues raised by both embryonic and adult stem cell research.

On Jan. 10, Science released a statement from Kennedy explaining that the journal would evaluate how the papers were reviewed and seek new ways to improve its procedures.

In the symposium's opening address, Kennedy elaborated on his initial statement by discussing the questions he's been asked over the past few weeks. The most prevalent one from journalists and the public was whether Science editors had any forewarning of the misconduct.

"We couldn't find anything," Kennedy said. None of the scientists who reviewed the papers raised questions and no journalist working for Science spoke with sources who suggested fraud. He said he expects in the future many people will come forward saying that they knew it all along, but none of those people spoke up in advance of the scandal.

If the journal didn't see the fraud coming, Kennedy said he is often asked whether this means the review process is flawed. He remains confident that it is not. "I don't think any reviewer could detect fraud if it was carried out by a capable scientist who knows how to walk the walk," he said.

Particularly in a journal such as Science, which sets a high bar for publishing only major scientific advancements, reviewers often have no way of assessing experiments that have never before been carried out. Instead, they must rely on authors to provide accurate data to support their conclusions. Kennedy said that most researchers do provide accurate data and those who don't are eventually discovered when colleagues aren't able to replicate the work.

Another question Kennedy said he is frequently asked is one that is not of interest to Science—that's the question of which of the many authors were involved in perpetrating the fraud. He added that some journals are considering requiring all authors on a paper to report what role they played in the research. He said that might eliminate some authors who are on the paper for political reasons but who didn't contribute to the research.

In the wake of the revelations about the stem cell findings, people have questioned, in particular, the role of one senior author on the 2005 retracted paper, Gerald Schatten, PhD, of the University of Pittsburgh, as he was not based in South Korea where the experiments were allegedly being conducted. In response to a comment from the audience, Kennedy said he thought this author did, in fact, deserve to be on the paper. However, he thought that requiring a written notice of participation might increase an author's accountability and therefore decrease the chance of fraud.

Moving forward, Kennedy said he didn't think the scandal would have fallout for researchers trying to treat disease using stem cells. "I think people will work as hard as ever to achieve their goals," he said. He does have concerns that some politicians may turn the scandal into ammunition against the entire field. If they do, it could result in less funding or tighter restrictions on future stem cell work.

"I hope devoutly that does not happen," he said.

Although the South Korean episode is the most prominent case of scientific fraud in recent years, Kennedy said that he doesn't think the field is any more prone to such incidents than any other areas of research. He said journals, including Science, should do what they can to look out for and prevent fraud in all areas of science to maintain the public trust.

Kennedy's lecture led off an afternoon of talks by scholars from Stanford and elsewhere. Topics included the ethics of egg donation, the realities of cord blood banking, the creation of chimeric animals and differences in stem cell policy between the United States and other countries.

## **2. Ethically Problematic Behaviors in Science**

All of us want to follow the highest ethical standards in our our roles as professors, and in most instances doing so is not be a problem. Yet, there are times, particularly in our teaching and research, when knowing and doing the "right things" are not as simple as they sound. In such situations, it is helpful if we can share out experiences in making the "right calls" when confronted with ethically problematic situations.

Robert E. McGinn has taught a number of courses on technology and society and on ethical issues in science and engineering at Stanford University. He has generated a list (see below) of fifteen "ethically problematic behaviors in science." The list focuses on research related conduct and as you can see, with the exception of a few items (#1, #2, #5 and #8 for example), these situations are not simple black and white matters with easily prescribed courses of action. Here is the list:

Ethically Problematic Behaviors in Science,"

1. falsifying (e.g., "cooking" or "trimming") data obtained from a genuine experiment;
2. fabricating experiments to "obtain" or "generate" data;
3. misrepresentation in funding requests (e.g., hyperbole regarding previous accomplishments or future value of research);
4. giving undue credit or failing to give due credit to someone regarding authorship of research work;
5. deliberately misleading research competitors to "throw them off the trail" in order to improve one's chances of "getting there first";
6. failure to secure bona fide "informed consent" from experimental subjects (for example the Tuskegee experiment involving subjects with syphilis, or recent Department of Energy revelations regarding testing of civilians with radioactive substances);
7. failure to take steps to insure "fair play" in one's laboratory (e.g., discrimination against or sabotage of the work by one or another party or group);
8. plagiarism;
9. demeaning a competitor's work to boost one's own;
10. allowing one's research findings to be used in a misleading or potentially harmful way for personal or group political or economic gain;

11. publishing one's work in LPUs (Least Publishable Units) to increase the number of one's publications;
12. failure to "blow the whistle" on someone whose work is known to be defective where failure to do so may endanger the public interest or put a private party at risk of incurring unjustifiable harm;
13. failure to conduct a fair-minded and scrupulous review of a scientific paper for which one is a referee;
14. providing a biased or facile evaluation of a proposal for research funding for which one is a reviewer, and
15. influencing scientific research projects of one's subordinates (e.g., graduate students) in order to advance research in which one has a vested economic interest (e.g., because of owning stock in a company which stands to benefit from the skewed research).

The first step in avoiding many of these behaviors is to acknowledge their existence and by so doing bring them out into the open for discussion. In discussing these matters it helps to be aware of the pressures leading some faculty, in spite of their best intentions to the contrary, to engage in such conduct. McGinn has looked at this issue in some detail and has postulated a dozen "factors conducive to misconduct in contemporary science."

They are:

1. the institutionalization of contemporary science (with all that this implies regarding the indispensability of obtaining substantial, ongoing funding);
2. the concept of an obsession with "success" in U.S. society, something which translates into great value being placed on obtaining desired results and which tends to devalue the importance and integrity of the process by which the results are obtained;
3. the difficulties that stand in the way of replicating previous experiments (e.g., difficulty of obtaining funding to replicate someone else's experiment);
4. the time that must be spent writing and marketing proposals to obtain funding for one's laboratory or institution, resulting in less time being available for transmitting "integrity values" to one's students "at the bench";
5. fear of being hit with a lawsuit if one blows the whistle on a colleague or superior;
6. fear of ostracism by colleagues if one blows the whistle;
7. the highly competitive nature of contemporary science regarding obtaining funding, being first in print, and obtaining one's own laboratory or a coveted endowed chair;
8. the high prestige attached by institutions and departments to having colleagues who publish prolifically and the related reward system;
9. the unprecedented degree of specialization in contemporary science (resulting in the prevalence of "a vulgar quantitative mentality" regarding publications);
10. the huge (about 40,000) number of scientific journals extant (resulting in the publication of much work of dubious scientific value and the difficulty of detecting fraud);
11. the lack of will and absence of an effective mechanism in science to root out fraud; and
12. the pressure on young scientists to obtain significant funding and publish a lot to get

tenure.

### **3. Double Dipping in Conference Papers**

The posting below looks at touchy subject of giving the same presentation more than once. It is by Scott Jaschik and is from the May 20, 2008 issue of INSIDE HIGHER ED, an excellent - and free - online source for news, opinion and jobs for all of higher education. You can subscribe by going to: <http://insidehighered.com/>. Also for a free daily update from Inside Higher Ed, e-mail [[scott.jaschik@insidehighered.com](mailto:scott.jaschik@insidehighered.com)]. Copyright © 2008 Inside Higher Ed. Reprinted with permission.

If you are going to give a talk at a scholarly meeting, do you need new material?

That's the question being debated in political science - as evidenced by a series of articles in the new edition of the journal PS: Political Science and Politics. While the journal finds a range of views on whether the trend is understandable or regrettable, the authors agree that it is real, and that attitudes appear generational.

As Nelson C. Dometrius, a professor of political science at Texas Tech University, writes in his introduction to the journal's debate, when he raised the question with senior faculty members, he received mixed reactions, with people quickly outlining special cases where they viewed such "double dipping" as justified. When he posed the same question to graduate students, Dometrius relates, "the modal reply was a blank stare - a lack of comprehension that presenting the same paper as many times as you wished would be viewed by anyone as an unusual or questionable practice."

Many senior faculty members say they first were discouraged from the practice in grad school - as often through subtle instruction as through any formal list of rules. In the PS articles, scholars consider the question of whether this shift in attitudes is one to fight or accept. Why, Dometrius wonders, is it now acceptable to do what was once "bad form"? (Via e-mail, he said that while he hasn't rechecked every paper he has given, he does not believe he has ever double dipped.)

While noting that the practice has become visible largely when reviewing job applications, Dometrius wanted to quantify it, so he assembled 114 vitas from political science departments at seven regional universities. The pool was made up of 87 faculty members and 27 graduate students, and departmental or institution-specific conferences were excluded, so the focus was only on conferences to which scholars regionally or nationally might apply to present or would travel to attend. He counted as "double" any paper with the same title or substantially the same title (although he notes from experiences that some who may be more ashamed of the practice try to hide it with substantially different titles for the same paper, so he may be undercounting).

In his sample, he found not a single case of double presentations prior to 1992. Then in the mid-1990's, he finds a paper or two a year, and by this decade it becomes fairly common - even if there is still a ton of new material out there. While double presentations

are pretty much a non-factor for those who earned Ph.D.'s through 1985, the attitude is quite different now.

Consider the following table showing double presentations by year doctorate was received. (The numbers for the most recent group may appear low, but that is primarily because these scholars have had less time to make presentations of any kind, let alone doubles, and the percentage suggests that their figures will rise considerably.)

#### Duplicate Presentations, by Year Doctorate Received

Year of Doctorate	% Who Have Double Dipped	Duplicates as % of All Papers Presented
1960-1985	14.8%	0.5%
1986-1995	46.7%	4.1%
1996-2005	52.6%	6.7%
2006 - present	25.9%	7.4%

The traditional reason given for double presentations - getting feedback and then revising - remains a strong justification, according to the articles in the journal. But many question whether in fact such revisions are taking place, as opposed to other motivations (such as CV padding). A variety of ethical issues are raised: Is this fair at a time that major conferences are turning away record number of paper proposals? Do those who fill resumes in this way gain an unfair edge over those who give fewer (but perhaps more original) papers? Do those who double dip have an obligation to flag the practice?

Lee Siegelman, a professor at George Washington University and immediate past editor of *American Political Science Review*, raises the question of whether such double presentations make some professors hypocrites, in light of the direction they provide students.

"Suppose that in a course you are teaching on the presidency during the spring semester, a student seeks your permission to submit, for full credit, a paper on the veto power for which or she already received credit in a course on Congress during the fall semester, or perhaps a somewhat reworked version of that paper. Would you grant the requested permission? I am betting that you wouldn't. Indeed if you 'caught' a student doing what this student has sought permission to do, you may even bring him or her up on plagiarism charges."

Others, however, defend the duplicate practice. For starters, defenders note that many conference sessions have remarkably small audiences - so if 2 of the 15 audience members at a regional meeting of the discipline heard the same paper at the national meeting, it's not like hundreds of scholars are being denied anything.

Two political scientists at Michigan State University - Sandra K. Schneider and William G. Jacoby - write jointly to "confess" to duplicate presentations and to defend them. They note several reasons: With more political scientists out there, "research productivity requirements" are growing, and graduate students are expected to present earlier in their

academic careers. These trends create "enormous pressure" to present at scholarly meetings when possible and it is "unrealistic and undesirable" to expect completely new work for each such event, they write.

Further, they say, papers do get better with feedback, but that sometimes you need multiple presentations before you get good feedback. If the end result is a paper to be sent to a journal or the start of a book, quality should count, and presenting multiple times encourages quality, they write.

In some other disciplines, the norms are different and there is no shame about duplicate presentations, although there are some issues related to how such papers are noted on CV's. Rosemary G. Feal, executive director of the Modern Language Association, said that it is fairly common for scholars to present a paper more than once. "The audience at MLA is going to be different from the audience at the 18th Century Studies Association," Feal said. In fact, she noted that scholars are so accepting of the practice that speakers will acknowledge what they are doing with remarks such as "when I last presented, I received a lot of questions about this point," she said.

And that shows the benefit of the practice, Feal said. Arguments are refined. Issues are clarified.

At the same time, Feal noted that ethical issues are raised if scholars try to imply that that a series of papers - essentially versions of the same work - are all original. The new edition of the MLA Style Manual notes in the plagiarism section the concept of "self plagiarism," in which a scholar repackages earlier work as if new.

Applying this to conference papers and CV's, Feal said that it should be clear - if one comes across a long list of papers on a resume - whether they were all original. Feal said that there is nothing wrong with telling a hiring committee that asks how many papers you gave in the last year that you gave two original papers, three times each at different conferences. But it would be wrong to represent that record as having presented six original papers.

The idea, she said, is "don't misrepresent what you've done."

Scott Jaschik

The original story and user comments can be viewed online at <http://insidehighered.com/news/2008/05/20/double>.