

Current Comments®

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Self-Promotion in Science: Too Much, Too Soon for "Cold Fusion"

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In science it has always been taboo for scientists to go beyond a certain point in calling attention to their own accomplishments. This stigma seems to be related to the self-perpetuating myth about science and scholarship—an implicit "mystique"—that recognition is supposed to happen by itself or through "natural causes." But we know that's not really true. The idea that all deserving persons will inevitably receive recognition is, unfortunately, also not true. Therefore, we ought to establish procedures—anonymous or otherwise—that would make it possible for worthy candidates to gain formal recognition by nominating themselves for awards. Even though the self-effacing personality is part of the mystique, nominating oneself for an award should not be a stigma. The embarrassment should be reserved for those who have not earned the recognition. And we know that such individuals are rarely embarrassed about discussing themselves.

But I must stress that, although self-promotion can be associated with deviant behavior in science, and especially with fraud, the activity does not necessarily indicate deviant behavior or fraud in the scientific community. I have discussed deviant scientific behavior in previous essays,^{1,2} and I will return to this topic in the near future—as recent events have focused attention on these issues once again. But I probably should add here that self-promotion is not a new phenomenon—it has a long history, as Dorothy Nelkin, Cornell University, Ithaca, New

York, observed in a recent piece in *THE SCIENTIST*.³

Self-promotion should not be viewed as negative. However, the practice shouldn't be abused either. And clearly, there is a very fine line between blatant, egocentric promotion and a science professional who is trying to reach a wider audience through accepted channels of protocol. According to Nobelist Baruch S. Blumberg (physiology or medicine, 1976), Fox Chase Cancer Center, Philadelphia, "An important—and vital—point is the volatility of the data and scientific results that are being 'promoted.' Promotion or self-promotion may be appropriate if the data is very sound, but would not be appropriate if the data has not been validated."⁴

A case in point is the recent intense media scrutiny of the alleged discovery of atomic fusion "in a glass jar" at room temperature using benchtop apparatus. A recent front-page item in *THE SCIENTIST*, for example, was one of many stories to deal with the non-scientific issues involved.⁵ There are several interrelated issues on this topic that bear closer examination.

Perhaps the most contentious aspect was the decision by chemists B. Stanley Pons and Martin Fleischmann and officials at the University of Utah to hold a press conference (on March 23, 1989) on the alleged discovery before publication of the results in an appropriate journal.⁶ This seems highly irregular, as eliciting public attention without providing data needed for technical appraisal

is not practicing good science; but there was pressure on the two scientists from a number of quarters.

First of all, there was the urge to beat out the competition (in particular, a group at Brigham Young University, Provo, Utah [BYU]). The University of Utah dearly wanted to be the first to present the alleged discovery to the public. However, things went a bit far when hundreds of preprint copies of varying generations of Pons and Fleischmann's work (some of them missing essential data such as an errata page and graphs) crisscrossed the US and even appeared in Europe via the telefacsimile ("fax") machine in a matter of days. This is a severe breach of the standard publishing protocol. A paper goes through the peer review process first, to be published second. The BYU group followed the standard procedure and is no doubt the better regarded for it.⁷

To be fair, it should be said that Pons and Fleischmann's work did eventually appear in the *Journal of Electroanalytical Chemistry and Interfacial Electrochemistry*,⁸ an English-language journal published in Amsterdam, The Netherlands. However, it is one thing to fax nonrefereed preprints to large numbers of people, a common practice in physics and other fields. (The most recent example is the temperature breakthroughs associated with superconductivity.⁹) But it is quite another matter to hold a press conference where the technical details of an experiment are not as fully defined, as well as less accountable, than a hard copy of a technical paper. Even the University of Utah's press release lacked any definite details.⁶ Perhaps one good thing to come out of this instant-publication phenomenon is that it took less time for scientists all over the world to begin their own investigations of the cold fusion claims.

Second, as was reported in *THE SCIENTIST* and elsewhere, there were considerable financial dividends (from patents) to be had if cold fusion actually had been found—for the researchers, the chemistry

department at the university, and the state of Utah.¹⁰ In a piece we recently reprinted, Columbia University sociologist Harriet A. Zuckerman discussed the emergence of "new claimants to scientific property."¹¹ The Utah case, with so many parties maneuvering to ensure their commercial stake, demonstrates how complex the matter of intellectual ownership in science has become. Full publication with all details might have saved many the time and energy it took to verify or repeat the experiments.

March and April 1989 was a time of extreme pressure on elements of the scientific community. A promise of unlimited, cheap energy, a method to tap that energy, as well as the associated monetary rewards were definitely appealing lures to inform the public through nonstandard channels. Pons and Fleischmann were faced with the proverbial Gordian knot. Like Alexander the Great, they took a shortcut—Alexander, according to legend, simply cut the knot in two with a sword; the electrochemists held a press conference. On the other hand, probably no product of Pons and Fleischmann's experiment could live up to the initial positive media reaction to the information released at the late-March press conference. The other side of the coin is that, had the experiment worked out, Pons and Fleischmann would have been forgiven the self-promotion!

This recent controversy on cold fusion only highlights a continuing debate on how much scientists should promote themselves, their place of employment, and even their technical specialty. These recent events have influenced me to reprint an editorial that originally appeared in *THE SCIENTIST*.¹² The points that I discussed in that piece are highly relevant to the issues raised by the current topic of cold fusion.

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REFERENCES

1. Garfield E. What do we know about fraud and other forms of intellectual dishonesty in science? Parts 1 & 2. *Current Contents* (14):3-7, 6 April 1987; (15):3-10, 13 April 1987.
2. ———. Some deviant behavior in science has nothing at all to do with fraud. *Current Contents* (49):3-5, 7 December 1987.
3. Nelkin D. Hyped science: researchers are hurting their own cause. *THE SCIENTIST* 3(10):11, 15 May 1989.
4. Blumberg B S. Personal communication. 25 May 1989.
5. Anderson G C. Utah's "fusion" fuels heated debate. *THE SCIENTIST* 3(9):1-3, 8, 1 May 1989.
6. University of Utah. "Simple experiment" results in sustained N-fusion at room temperature for first time: breakthrough process has potential to provide inexhaustible source of energy. 24 March 1989. 5 p. (Press release.)
7. Jones S E, Palmer E P, Czirr J B, Decker D L, Jensen G L, Thorne J M, Taylor S F & Rafelski J. Observation of cold nuclear fusion in condensed matter. *Nature* 338:737-41, 27 April 1989.
8. Fleischmann M, Pons S & Hawkins M. Electrochemically induced nuclear fusion of deuterium. *J. Electroanal. Chem. Interfac.* 261:301-8, 10 April 1989.
9. Garfield E. The 1987 Nobel Prize in physics: citations to K.A. Müller and J.G. Bednorz's seminal work mirror developments in superconductivity. *Current Contents* (18):3-11, 2 May 1988.
10. Pool R. How cold fusion happened—twice! *Science* 244:420-3, 28 April 1989.
11. Garfield E. Ownership rights in science: Harriet A. Zuckerman on evolving norms of intellectual property. *Current Contents* (26):3-9, 26 June 1989.
12. ———. Is there room in science for self-promotion? *THE SCIENTIST* 1(27):9, 14 December 1987.

Is There Room in Science for Self-Promotion?

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The topic of self-promotion is discussed. Its relationship to deviant scientific behavior and possible boundaries of acceptable self-promotion are examined.

Scientific fraud has received much attention lately, both within the scientific community and increasingly beyond it.

Unfortunately, some journalists with a taste for the sensational have exaggerated its frequency. The obvious example is William Broad and Nicholas Wade's *Betrayers of Truth* (Simon & Schuster, 1982). (On the other hand, careful science journalists have detected genuine instances of fraud—for example, Oliver Gillie's 1976 exposé in the London *Sunday Times* of Sir Cyril Burt's misdeeds.) Plainly some have mistakenly taken "scientific fraud" as a virtual synonym for "scientific misconduct." This practice was pointed out to me recently by Robert K. Merton of Columbia University, who has long examined the spectrum of activities that, in different degree, violate norms of the scientific community.

Thirty years ago Merton described some of the colored bands in that spectrum: fraud, both the concoction of false data and the fudging of data to have them support a hypothesis; plagiarism, and correlatively, in priority disputes, falsely imputing plagiarism to others who have independently come upon the same findings; and "secretiveness lest one be forestalled" ("Priorities in Scientific Discovery," *American Sociological Review*, vol. 22, 1957, pp. 635-59).

Harriet Zuckerman, also of Columbia, has more recently described other hues of scientific misconduct. They include disreputable or negligent errors (as distinct from reputable or state-of-the-art errors) and breaches of "the etiquette of science," such as eponymizing oneself; the under-acknowledgment of collaborators; ad hominem attacks; and publicity seeking ("Deviant Behavior and Social Control in Science," in E. Sagarin, ed., *Deviance and Social Change*, Sage Publications, 1977, pp. 87-138; "Norms and Deviant Behavior in Science," *Science, Technology, & Human Values*, vol. 9, Winter 1984, pp. 7-13). To these can be added issuing research results in least publishable units to increase one's publications, adding gratuitous co-authors to a paper, failing to acknowledge intellectual predecessors, and irresponsibility with research funds.

Rules of the Game

In drawing distinctions between types of deviance or misconduct, these and other sociologists of science have brought to light the complex and largely unspoken "rules of the game" that scientists honor and to which they usually adhere. In sociological parlance, these rules form the cognitive and the social or moral norms of science.

Infractions of different norms carry different penalties and are reflected in the varying degrees of condemnation which each evokes within the scientific community. Fraud is the legal equivalent of a felony, whereas breaches at the other extreme are akin to a lesser offense, such as jaywalking. While recent attention has understandably focused on heinous crimes of a few scientists, pedestrian violations have their own interest.

In *Selling Science: How the Press Covers Science and Technology* (W. H. Freeman, 1987), Dorothy Nelkin of Cornell University observed the rising tendency by scientists to seek favorable media attention for their research. "Increasingly dependent on corporate support of research or direct congressional appropriations, many scientists now believe that scholarly communication is no longer sufficient to maintain their enterprise," she wrote. "They see gaining national visibility through the mass media as crucial to securing the[ir] financial support...." (p. 133) Nelkin cited researchers working on interferon, DNA and artificial intelligence who have pursued the public spotlight for this purpose (pp. 7, 138) and others who have hired public relations firms to prepackage research results for distribution before their formal publication. (p. 174) Indeed, she noted that many newspaper editors are beginning to feel they are being used as "pawns for grantsmanship." (p. 141)

Zuckerman wrote that "going to the lay public for primary legitimation and recognition violates the norm of organized skepticism since it bypasses the primacy of qualified peer review." (p. 122) With an increase in such behavior, scientists are starting to worry about, in Nelkin's words, "the corruptive influence on science of self-promotion and the encouragement of scientists more skilled in public relations than in research." (p. 169)

Nelkin further observed that "unlike physicians and other licensed professionals with codes of ethics and standards of confidentiality, [scientists] share few norms to guide their relations with the public." (p. 160) One standard, enacted by some journal editors, is known especially in the medical sciences as the Ingelfinger Rule (named for the former editor of the *New England Journal of Medicine*, Franz J. Ingelfinger). It proscribes the publication of articles whose substance has first been reported in the press.

In order to ensure informed judgment on the value of research projects and an orderly dissemination of research findings, the scientific community should examine closely the actions of self-promoting scientists who use the media to gain support. Perhaps some of the unspoken norms regarding scientists' dealings with the public require specific and overt codification, as in the Ingelfinger Rule.

There's One at Every Conference

But what of those scientists who are self-promoting but who do not attempt to circumvent peer review in pursuit of research funds and other awards? (Here I leave aside the visible or continuously public scientists—the popularizers and the social and political activists.) Every field of science has those who promote their own research. Their motto seems to be "modesty is the opiate of the mediocre." I recently used the terms "charisma" and "chutzpah" to describe these characters ("Some Deviant Behavior in Science Has Nothing at All to Do with Fraud," *Current Contents*, no. 49, December 7, 1987, p. 3).

These self-promoting scientists, by their unbounded enthusiasm for their own projects and their exuberant personal style or flamboyance at conferences, often earn the derision and sometimes the censure of their peers. No matter that some of them are obviously brilliant or exceedingly creative. Even when festooned with medals for authentic accomplishments, these intellectual egotists find themselves somehow standing outside the society of science.

Just what standard do these scientists violate? It is the community's consensus that a good scientist—one who can be trusted—casts a cold eye on the data; any passion should be for the advancement of knowledge, not for the advancement of self. That is the ideal. But John Ziman, now chairman of the Science Policy Support Group, London, noted the reality (and paradox): "One must have sufficient confidence in one's own notions to carry conviction in argument. Yet one must not become so deeply committed that one cannot escape from them if they prove untenable." He added, "*The Double Helix* brings out the passion and anguish with which scientific research is really pursued" ("Some Pathologies of the Scientific Life," *Advancement of Science*, vol. 27, September 1970, p. 11).

Of course, most scientists have enthusiasm for their research. That some express it in the form of self-promoting language or behavior reflects more than anything else the diversity of personalities within the scientific community. I am far from unsympathetic with strong or colorful personalities who find it difficult to blend quietly into the background of their adopted milieu.

Is there room in science for self-promotion? Of certain innocuous varieties that are merely extensions of unconventional individuals, I surely hope so. Members of the scientific community can exhibit tolerance toward such colleagues. But of those varieties of self-promotion which attempt to skirt peer review, it is not tolerance but scrutiny and perhaps action that is required. ■