

***Silencing Scientists and Scholars in
Other Fields: Power, Paradigm Controls,
Peer Review, and Scholarly Communication***

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Trust, Skepticism, and Whistleblowing

Among the most treasured and essential ideals of science are collegial relations based on common pursuits and mutual trust. ... Interfering with collegiality is perilous for science. ...” It is uncomfortable to live with error but important to remember that correcting a specific error at the expense of collegial trust will not and cannot restore that comfort. (P. Woolf, personal communication, July 23, 1987)

This quotation is from an informal peer review of a proposed rebuttal article that purported to correct alleged errors found in a published article in the field of biomedical research. The peer-review judgment advised against publication of the rebuttal article, and the concept of “collegial trust” was invoked in support of a recommendation not to publish. On the other hand, Woolf (1986) is also on record as stating that scholars should accept criticism:

Scholars ought to be able to tolerate a good deal of mutual criticism. ... Although such professional criticism is uncomfortable, even painful for all the parties concerned, scholars cannot afford to be too thin-skinned if they are to maintain scholarly standards and professional autonomy. (p. 8)

Woolf’s apparently contradictory statements indicate that trust, skepticism, and whistleblowing are important factors in discussions of silencing of scholars. These

factors often intertwine and overlap. Whenever trust, skepticism, and whistleblowing are discussed in this context, however, they should also be analyzed within the parameters of rhetoric versus reality, double standards, and paradigm dependency. The word *mutual*, as used by Woolf in "mutual trust" and "mutual criticism" implies a common and universal application rather than one based on favoritism and special privilege. *Collegiality* is an academic term for politeness and courtesy, which might also include benefit of the doubt relating to questions about honesty and good faith.

The word *trust* abounds in the literature about science. As an example, Nigg and Radulescu (1994) wrote that scholarly publishing in science takes place "in an environment ... fundamentally based on trust" (p. 168). When it is stated that a person can be trusted, it often means that the person is honest and acts with integrity. Sometimes it is felt that a person or groups of persons can be trusted based on past behavior. At the same time, trust might be tied in with a sense of dependency, as in situations in which persons have no other choice than to place their trust in experts or leaders. In such cases, trust might become more a matter of hope than of belief or conviction. In any case, in terms of scholarship and scholarly communication, the word *trust* is usually associated with an assumption of honesty and integrity on the part of scholars, and this assumption is mutual (at least according to the rhetoric).

On the other hand, trust is also associated with plausible possibility and logical expectation, based on specific and particular circumstances. There is often a negative connotation involved, such as the feeling that persons or institutions cannot really be trusted to make certain types of decisions in cases involving conflicts of interest, special interests, vested interests, and paradigm challenges. In the midst of all the rhetoric about mutual trust, just what can academics be expected to do (i.e., trusted to do) within the reality of conflict of interest situations and vested interest situations?

Skepticism is another word commonly used in the literature and rhetoric of academia and science. It is associated with intellectual curiosity, critical inquiry, critical analysis, and basic scientific methodology itself. In this respect, it is an essential element in scholarly research and scholarly communication. Schmaus (1987) stated that "The growth of scientific knowledge, then, does not depend so much on the honesty as on the skepticism of scientists" (p. 4). However, because skepticism might also conjure up negative impressions along the lines of suspicion, doubt, and cynicism, it is sometimes given a clarifying, positive, and uplifting adjective, such as "healthy" skepticism ("enlightened" skepticism might be another example). Skepticism is not an adversary that places doubt on trust and honesty. On the contrary, it is an ally and colleague of trust and honesty, all involved together in the advancement of knowledge.

At this point, along come some whistleblowers. If they are traffic police, they might blow the whistle to halt traffic to allow schoolchildren to cross the street safely. Or, they might blow the whistle to stop a car that committed some traffic violation that endangered pedestrians or other drivers in traffic. Such whistleblowers are usually looked on favorably as protectors of society.

How are academic whistleblowers looked on? Do such whistleblowers enjoy the

“collegial relations based on common pursuits and mutual trust” that Woolf claimed are among “the most treasured and essential ideals of science?” Do academic whistleblowers do good or do harm in academia, in terms of “growth of scientific knowledge” (or growth of knowledge in fields other than science)?

In a sense, academic whistleblowers are scholars who have been successful in the exercise of their skepticism, to the extent that such skepticism has resulted in the discovery of error (ranging from so-called minor error to the more spectacular error consisting of a false paradigm that became entrenched in the scholarly literature). But when is such a whistleblower regarded as a successful skeptic and a discoverer?

Harvey is now regarded as the scientist who in the 17th century discovered the circulation of blood. This discovery is now acknowledged as important enough to be cited in textbooks and encyclopedias. According to words attributed to him, Harvey himself did not believe that he would be hailed (collegially or otherwise) as a great discoverer by his colleagues:

But what remains to be said about the quantity and source of the blood which thus passes, is of so novel and unheard-of character that I not only fear injury to myself from the envy of the few, but I tremble lest I have mankind at large for my enemies, so much doth wont and custom ... and doctrine ... and respect for antiquity, influence all men. (Nissani, 1995, p. 168)

WOOLF, THE U.S. CONGRESS TASK FORCE ON SCIENCE POLICY, AND BALTIMORE

In effect, Woolf's quotes at the beginning of this chapter can perhaps be understood best in terms of rhetoric versus reality situations. Her idea that “scholars ought to be able to tolerate a good deal of mutual criticism” serves as effective rhetoric of a generalized nature for members of Congress who heard her testimony. It seems obvious that powerful academics might not appreciate her words as much as the Congressmen serving on the Task Force on Science Policy might, if these words were spoken in specific relation to alleged errors in the works of academic authorities.

In fact, the informal peer-review remarks, “It is uncomfortable to live with error but important to remember that correcting a specific error at the expense of collegial trust will not and cannot restore that comfort,” were related to whether or not Stewart and Feder should publish a rebuttal article in the journal *Cell*. (The rebuttal article discussed errors in the *Cell* article for which Baltimore was a coauthor.) The key phrase seems to be “correcting a specific error at the expense of collegial trust.” This is a long way—in a brief period of time from when it was written—from the belief that “scholars ought to be able to tolerate a good deal of mutual criticism.” It is also a long way from the concept of considering collegial trust in terms of honesty and integrity versus fraud and misconduct. (In their rebuttal article, Stewart and Feder discussed some alleged errors, but they did not speculate about any possible misconduct or fraud.)

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By associating specific error with collegial trust, as Woolf did, it is no longer a mat-

ter of a scholar's honesty that is being trusted or distrusted, but the scholar's infallibility as an expert. No matter how Woolf's words are interpreted, the impression remains that she is saying that the rebuttal article, with the exposure and discussion of errors in the *Cell* article, should not be published because the article indicates that the authors of the rebuttal article do not trust the authors of the *Cell* article. (Otherwise, the question of trust would not have been raised by Woolf.)

However, Sarasohn's (1993) book and Lang's (1993) article in *Ethics and Behavior*, which deal with the *Cell*-Baltimore case in detail, both emphasize that the rebuttal article, as well as O'Toole's original activity in this matter, were concerned with the correction of error. O'Toole, Stewart, and Feder were not showing any lack of collegial trust for anyone's honesty or integrity. They merely wanted to see the errors corrected. It was the data that were not trusted in this case.

By contrast, how did mutual trust or collegial trust fare, in terms of the way some coauthors of the *Cell* article (and the leaders of the science establishment who rallied around them) treated Stewart, Feder, and O'Toole? Sarasohn (1993) reported that "the talk around Cambridge and Boston was that O'Toole's documents might not be genuine data. . . . Baltimore attacked O'Toole's motives" (pp. 36-37). Sarasohn then cited a memo from NIH official Joseph E. Rall to Stewart and Feder that affirmed that "it could equally well be that the seventeen pages you have are falsified" (p. 40). Moreover, Sarasohn wrote, "During one phone call, Stewart claimed, Baltimore said that O'Toole had stolen the documents and called Stewart a vicious person" (p. 41).

At this point, it seems clear that, on the one hand, an appeal was made to collegial trust as a reason to silence scholars (and the ideas they are expressing in a rebuttal article), whereas on the other hand, as part of an effort to silence the same scholars, collegial trust and mutual trust were abandoned, as evidenced by use of words such as "attacked O'Toole's motives," or "stolen" and "falsified." Thus, in this case, mutual trust comes into the picture as part of the rhetoric of science, but in reality the concept of trust was invoked in a one-way-only manner that resulted in a strenuous attempt to silence scientists.

In the famous peer-review experiment by Peters and Ceci (1982), articles published by famous scholars from prestigious institutions were resubmitted to the same journals, with the names of the authors and institutions changed to ones that were less prestigious or fictitious. Several of the articles that were previously published were rejected the second time around because of defects in methodology or for other reasons. It seems that some prestigious names were trusted more than less prestigious names. The trust was not mutual in this case.

TRUST, SKEPTICISM, AND VESTED INTERESTS

It would seem that a similar experiment (hypothetical or real) could be envisioned to test the concepts of collegial trust, mutual trust, and skepticism, in terms of what scholars might be expected to do (i.e., trusted to do) in vested interest situations and

conflict of interest situations. Such an experiment could also test how what they might be expected to do compares to the rhetoric about trust, skepticism, and the advancement of knowledge.

Suppose that two lengthy, detailed studies were drawn up, with one study concluding that there is no definite proof that tobacco causes lung cancer or any other serious illness, and with the other study concluding that not only has nicotine in tobacco been proven to be addictive, but also that damage to human health caused by tobacco is, in fact, much greater than has previously been reported or suggested. Both studies are sent to the same two scholars for their peer-review recommendations.

Now suppose, in this hypothetical case, that one of these scholars is a long-term senior research consultant for a major tobacco company and owns shares of that company's common stock (and also has the opportunity to exercise options to acquire many more shares). Furthermore, the scholar is a well-paid consultant to tobacco industry associations, and is a professor of agriculture whose chair is endowed by a tobacco growers' association.

The other peer-review referee in this hypothetical situation is an environmentalist activist on campus, and a leader of a lobbyist group that is trying to get the government to place tobacco on a list of addictive drugs that includes heroin and cocaine and is trying to have cigarettes, cigars, snuff, chewing tobacco, and pipe tobacco banned. Would any sociologist, or other scholar writing about higher education or about the sociology of science or peer review, expect both peer-review referees to exercise the same amount of trust and skepticism in reviewing both articles for scientific rigor, error, methodology, and accuracy?

WHISTLEBLOWING

The fact is that there are vested interests and conflict of interest situations, ranging from paradigm dependency to specific individual and institutional financial and commercial interests, that exist in academia. The potential magnitude of such financial and commercial interests is enormous. For example, Levin (1996) reported that the School of Medicine at Yale University has become "a major enterprise of biomedical research and clinical practice that generates more than 40 percent of the University's revenues and expenses" (p. 24). If whistleblowers were afforded collegial trust and given appreciation for the correction of errors that their skepticism produces, they might have excellent opportunities in many academic disciplines.

A major problem, however, that whistleblowers face is the fact that scholars, like other persons, do not appreciate being proven wrong. There might also be a connection or correlation between a scholar's power, prestige, and reputation and the scholar's lack of toleration for correction of the scholar's errors.

Minor errors are usually allowed to be corrected, but they do not necessarily involve whistleblowers and whistleblowing. If Baltimore and the authorities at MIT and Tufts University had allowed some errors to be corrected in the infa-

mous *Cell* article, the corrections might have been considered part of the so-called self-correction of science, with specialists adjusting their future research accordingly. O'Toole would not necessarily have been known as a whistleblower in this case, but instead, as another one of many scientists who exercised healthy skepticism. She became known as a whistleblower only after she and Stewart and Ned Feder tried to have corrections made after: (a) some authors and authorities refused to admit that serious errors were made, and (b) obstacles prevented the correction of error.

The *Cell*-Baltimore case, in this sense, illustrates some aspects of academic whistleblowing. Major breakthroughs in medical research by establishment scientists are not usually regarded as whistleblowing, but the major breakthrough arrived at independently by Semmelweis relating to the cause of childbirth fever is regarded as a classic case of whistleblowing. The perfection of miracle drugs and miracle cures has become part of the research paradigm that might involve specific vested interests and conflicts of interest. If a scholar discovers, however, that a miracle drug does not really work, after all, or is causing tragedy and death and suffering (e.g., DES, thalidomide, and SMON), scholars who discover the defects of the medicines become known as whistleblowers.

In theory, academic whistleblowers are scholars who believe they have detected serious error, and they try to correct it. In reality, however, the term *whistleblower* is usually reserved for scholars whose detection of errors and their attempts to correct them have hit a raw nerve in terms of paradigms, vested interests, or conflicts of interest.

Along this line, in the case of the Guido Riccio controversy, in 1907, Venturi detected what he thought was an error in attribution for part of a Sienese painting, and he published it (in passing, in a footnote) in an authoritative and monumental history of Italian art. Although, by 1907, the attribution to Simone Martini for the Guido Riccio fresco had become a paradigm, the discipline of art history was not yet developed enough, and the vested interests built up around the painting were not yet strong enough, to bring Venturi's comments to the level of whistleblowing. Instead, his insight did not lead to correction or even to discussion. It was ignored by scholars for a long time. In this sense, Venturi's opinion, as revolutionary as it was, did not draw the attention necessary to qualify it as an act of whistleblowing.

Although Venturi's publication about Guido Riccio did not differ much in content from that that was written about the same painting in 1977, it was the different reaction of the establishment from 1977 onward that turned the situation into one of academic whistleblowing.

When, in the 17th century, Harvey tried to convince the medical profession about the circulation of blood, he became a whistleblower. At the present time, medical researchers who routinely agree with and follow Harvey's findings in their development of miracle drugs are not known as whistleblowers or followers of a whistleblower, yet they are working within the paradigm that a whistleblower introduced.

SILENCING OF WHISTLEBLOWERS

Obviously, there are attempts to silence scholars who are whistleblowers, and these attempts have been taking place in academia for centuries. In fact, to the extent that progress in science and in medicine, and the advancement of knowledge in other disciplines as well, have taken giant steps forward as a result of paradigm changes brought about by discoveries made by scholars (or groups of scholars), the history of science and of medicine and intellectual history are histories of whistleblowing. As discussed earlier, establishment scholars have a vested interest in paradigms in their disciplines based on their own sense of reputation, prestige, expertise, and authority, all of which might be placed in doubt and question if the paradigm were suddenly proven wrong.

With the rise of the industrial–governmental–academic complex, vested interests have expanded greatly into financial as well as intellectual areas, leading, in turn, to some conflicts of interest. Under these circumstances, the areas in which academic whistleblowers operate become more complex and more expansive in dimension. In any case, similar knee-jerk reactions to whistleblowers seem to prevail at present as they did in the past. The overriding reaction seems to be that the ideas of the whistleblower must be silenced.

In order to silence the whistleblowers' ideas, there are often attempts to discredit the whistleblowers. Among the first casualties of the discrediting process is the sense of trust, mutual trust, or collegial trust that the scholar who is being discredited might have enjoyed previously. Strohman (1995) described the “before and after” whistleblowing situation experienced by Duesberg:

In 1987 Peter Duesberg was at the top of his career and the future was promising. ... He was the recipient of a seven-year Outstanding Investigator Award Grant from the National Institutes of Health that provided him with hundreds of thousands of dollars to conduct his research. ... His brilliance as an experimental virologist was acknowledged around the world and his prizes for leadership are many. Now, in 1995, he has no grants from the National Institutes of Health. (p. viii)

Furthermore, Strohman (1995) stated that the editor of *Nature* asked Duesberg to “quit his role as critic,” and that the editor has “used his power as editor to enforce Duesberg’s silence in the journal” (p. xi). Likewise, Horton (1996) wrote that Duesberg is

a brilliant virologist, and the former recipient of an award for outstanding investigative research from the National Institutes of Health. ... Yet, he is now perhaps the most vilified scientist alive. His work inspires excoriating attacks. ... What extraordinary course of events has led him to be dismissed by his peers and ridiculed by his colleagues? (p. 14)

In this case, and in many other cases, it seems that the “extraordinary course of events” is all part of a common phenomenon in the history of academia in which a

scholar becomes known as a whistleblower. In the *Cell*–Baltimore case, when O’Toole attempted to correct errors in the published article in question, the coauthors of the dubious article neither received excoriating attacks, or were they dismissed nor ridiculed by their peers. Nor, for that matter, was there an attempt to silence the authors. To the contrary, they were merely asked to publish a correction. However, according to Woolf, such an attempt to publish such a correction of another scholar’s published work was itself an example of withholding, or denying, “collegial trust.”

SILENCING BY MEANS OF ISOLATION

In contrast to mere suggestions to publish corrections of error, it would seem that excoriating attacks and ridicule by colleagues are certainly more evident manifestations of loss of collegial trust in academia. Whistleblowers are not only denied mutual trust, for the most part, but attempts to silence them are also linked with attempts to isolate them. This isolation process might take place even if the attempt at isolation has to be propped up by falsifications and misrepresentations. In such cases, factors of double standards, rhetoric versus reality gaps, and toleration for falsification can all come into play.

A glaring case of falsification and misrepresentation in an attempt at isolation of a whistleblower took place in the Guido Riccio controversy. Giovanni Previtali was quoted (in a Sieneese newspaper) as claiming that doubts about the establishment view about Guido Riccio were “only the invention of a non-expert who has not found anyone who agrees with him” (Mallory & Moran, 1996, p. 142). Previtali continued, “This American was mistaken from the beginning ... and he is spending all his life trying to demonstrate that the fresco is not by Simone. Poor man, by now he has taken on the form of a monomaniac” (p. 142). Some scholars, including Previtali himself, knew that his ideas contained falsifications in this case. Aside from the fact that Venturi expressed similar doubts about the painting in 1907, by 1984 other scholars in the United States and Italy, and also in England, had already cast some doubts of their own on the traditional establishment viewpoint (Falcone, 1991).

Complete isolation means that a scholar is alone, a single voice in the wilderness. No other scholar shares the same ideas. Previtali was trying to convey such an impression at a time when other art historians were actually joining in to voice what was becoming at least a small chorus of doubts. In addition to claiming, falsely—as Previtali did—that a scholar and whistleblower is actually isolated, attempts might be made to misrepresent what a scholar said or wrote.

Along with misrepresentations, attempts might also be made at intimidating other scholars so that they will not support the whistleblower’s ideas, or even associate with the whistleblower. Once again, a very clamorous example of such a misrepresentation occurred during the Guido Riccio controversy. When the destroyed part of the lower border of the Guido Riccio fresco became a focal point of discussion that was becoming embarrassing for the authorities, Roberto Barzanti (1988), former

mayor of Siena, Vice President of the government of Europe, and head of the Department of Culture of the government of Europe stepped in. He described as a sensationalistic shot (“sparate sensazionalistiche”) the observation that a part of the fresco had been destroyed in 1980–1981, and he implied that the observation was merely pure fantasy. Yet, if Barzanti had taken a good look at the fresco either before or after he published his statement, he would have clearly seen that the observation that part of the painting had been destroyed was an accurate description rather than fantasy. For the purposes of discussion of whistleblowers and their treatment by authorities, Barzanti’s words can serve as an illustration of how mutual trust can become practically nonexistent.

Rose (1978) described a variation on the theme of isolating and silencing whistleblowers. According to her account, at one point in the Velikovsky controversy, there was going to be a debate sponsored by the AAAS:

Originally, Dr. Velikovsky was promised that the panel would consist of three opponents of his views and three (including himself) defenders of his views. This promise was flagrantly broken, and the panel was rigged in such a way that only Velikovsky was allowed to speak in his defense ... there were four-to-one odds. ... When the Cornell volume was published David Morrison and Isaac Asimov were added to the negative side, and Goldsmith wrote a long “Introduction.” Velikovsky’s lecture was dropped. (The four-to-one odds became seven-to-zero odds). (p. 68)

In this case, the specific silencing of scientists and their ideas from the published scholarly literature might be described as a fabricated isolation.

LAFOLLETTE AND THE WHISTLEBLOWER AS NEMESIS

In her description of whistleblowers, LaFollette (1992) wrote:

As their efforts continue, they attract vigorous criticism, especially if their targets are senior scientists. They may even be accused themselves of hidden motives, of secretly harboring revenge. If their allegations later prove to be correct, society will probably applaud their courage and persistence—but it will be a bittersweet victory. (p. 137)

LaFollette (1992) compared whistleblowers to the concept of *nemesis*, in a negative setting, rather than to scholars who try to correct alleged error in spite of all the obstacles placed in their way. Although LaFollette made her own distinction between whistleblowers and “nemesis figures,” she described Stewart and Feder as nemesis figures although they are, in fact, better known in scientific and academic circles as whistleblowers. She also indulged in a sort of pop psychology, in explaining that “the nemesis cannot stay quiet” (p. 147) and “the nemesis may also feel more vindictiveness toward the transgressor and all he represents than empathy for the party wronged” (p. 147). She also asked why “scientists ... so often criticize the truth-seeking nemesis?” (p. 147).

One explanation she cited is that the "nemesis disrupts the tidy order of science and must be removed either temporarily or permanently from the scientific mainstream" (p. 148). Needless to say, in such cases "removed" in effect means silenced.

SILENCE AND UNRESPONSIVE INSTITUTIONS AND INDIVIDUALS

Relations among trust, skepticism, and whistleblowing can be discerned in LaFollette's (1992) further comments about nemesis figures and whistleblowers:

We may simply have to tolerate their presence until universities and research organizations find ways to treat well-intentioned questions about research conduct fairly and seriously. Concerned scientists have stepped into the nemesis role to force ethical responsiveness in seemingly unresponsive institutions. (p. 154)

In this passage, "unresponsive" can be associated with stonewalling, silence, or cover up. From the standpoint of silencing of scholars, the situation proceeds as follows: Whistleblowers raise questions and try to correct errors. The whistleblowers and their ideas are suppressed, and the silence that ensues is continued on the part of the institutions in the forms of stonewalling and cover up.

In this context, the word *unresponsive* in relation to the correction of error can refer to individuals as well as to institutions, and LaFollette and her book provided an interesting and paradoxical example relating to the term. LaFollette devoted much of the discussion under the subchapter "Protecting Individual Rights" to the case of David Abraham, a scholar of German Weimar history whose work was allegedly found to be marked by the falsification, if not fabrication, of some documentary evidence. According to LaFollette (1992), the American Historical Association (AHA) refused to "initiate a formal investigation" (p. 172). She then stated that Abraham "could only write his own letters for publication in subsequent issues of the same journals his opponents had already exploited. By the time Abraham replied in a later issue to one accusing letter, another letter with new allegations was published elsewhere" (p. 172). She also referred to the "nebulous nature of the charges" (p. 173) and stated that "gossip and innuendo ruled the process" (p. 173) and that Abraham was driven out of the history profession of academia.

Even a cursory reading of LaFollette's (1992) account by someone who was not familiar with the details of the case might conclude that Abraham was mistreated and was silenced unfairly. However, to anyone who had been following the case closely, LaFollette's narration might have caused the red flag that warns of falsification to start waving furiously. Instead of being forced to write "letters for publication in subsequent issues of the same journals his opponents had already exploited" (p. 172), in some cases Abraham was allowed much more space in the same issue of a journal than his opponents used, and in other cases his opponents were shut out of journals that took Abraham's side. For instance, the AHA's Executive Director, Samuel

Gammon (personal communication, December 19, 1983), pointed out that the AHA had published an "exchange" between Abraham and his critics. Gammon added that "The Professional Division and the Executive Committee of the Council reluctantly agreed to publish the Turner letter, provided Abraham had a right of response," and also that the AHA leadership "believed that essentially scholarly debate belonged in the pages of scholarly journals of record." Furthermore, in the June–September 1984 issue of *Central European History*, Abraham (1984a, 1984b) had about 88 pages to present his case, whereas his critic, Feldman (1984a, 1984b), expressed his views in less than half (about 40 pages) that number. Abraham (1985) was also allowed to publish a rebuttal article of more than 20 pages in *Vierteljahrschrift für Sozial- und Wirtschaftsgeschichte*. Instead of comprising gossip and innuendo, these exchanges among specialists in history journals seemed to be an actual working out of the self-correcting process that is often referred to in the academic rhetoric. In effect, in this specific case, that which Rennie and Flanagin (1995) described as part of the only real peer review is described by LaFollette (1992) as essentially gossip and innuendo.

It seemed obvious that LaFollette's (1992) description amounted to a blatant falsification of what actually took place. She was asked (G. Moran, personal communication, September 19, 1993) how she could have made such clamorous error in a book devoted to the discussion of falsification and the correction of error. She was also asked if she had trusted someone unwisely, instead of actually checking the record first. In any case, she was asked how she made the error, and a request was made of her to correct the error. More than 3 years have passed, and no reply has been received from her. The answer remains a secret, and this secrecy is a result of LaFollette's silence based on unresponsiveness.

BLIND TRUST AND EXCESSIVE TRUST

It might be that LaFollette's (1992) falsifications resulted from her placing what amounts to blind trust, untempered by skepticism, in sources that turned out to be unclear, misleading, or unreliable. Baltimore's reflections on the controversy in which he was a leading character include comments about his "trust in the efficacy of the peer review process," and observations that he might have been "blinded to the full implications of the mounting evidence by an excess of trust" (Lang, 1993, p. 69). His reference to "excess of trust" would seem to imply that trust itself should be viewed with a certain amount of skepticism, as a sort of brake or harness on excessive trust. An excess of an excess of trust might amount to blind trust.

In some cases in which persons (nonspecialists, journalists, and the general public) do not understand the technical terminology and in which persons are dependent on the actions of experts, blind trust is all that people can have, if they have any trust at all. On the other hand, conflict of interest situations might arouse skepticism to such a degree that trust, if not negated, is placed in doubt. It might well have been specific conflict of interest situations in medical research that led Pradal to ask, "Whence

comes this blind trust, when intelligence should in fact lead us rather toward mistrust?" (Ruesch, 1989, p. 89).

A more generalized skepticism about trust and error is found in Brian Martin's (1996) analysis. He observed that experts have often been mistaken in the past, and, as a result, he asked, "Why should the public trust them now?" (p. 3). Trust in this sense refers to infallibility, of trusting experts not to make serious mistakes. If the term *cover up* were substituted for *mistakes* in this instance, skepticism about trust would refer to honesty and integrity of the investigators and the investigation process. Martin's ideas find an echo in Wade's (1995) observation that "scientific establishments have been wrong in the past," and that "in cases of scientific fraud, universities have proved as zealous as the Pentagon in discouraging whistle-blowers" (Wade, 1995, p. 14).

In these contexts, trust can also refer to reliance on. Lang, who recently wrote a letter (personal communication, January 11, 1996) to the Council of the NAS relating to AIDS research, stated, "As I have written before, it is left for individual scientists to propagate correct information, since scientists cannot rely on the official media such as the *New York Times* and *Science*."

TRUST AND FEAR

Fear, and particularly fear of the unknown or a fear of catastrophes, can elicit or provoke trust in political leaders and scientific leaders. It can be warranted and justified in some cases, but it can also represent a source of exploitation, or simply colossal blunders. The so-called swine flu scare is such an example, according to Duesberg (1996b), who recalled that "the naive legislators easily could be manipulated by the CDC's alarmist rhetoric" (p. 142). Duesberg expanded on this theme:

The CDC has nevertheless continued to exploit public trust by transforming seasonal flus and other minor epidemics into monstrous crises and by manufacturing contagious plagues out of noninfectious medical conditions. The fear of infectious disease had now been revived on a mass level for the first time in decades, and the lay public had no choice but to trust their appointed experts for answers. (pp. 137, 158)

If fear provokes dependence and blind trust, or inevitable trust, in experts, the opposite might be said for secrecy. Although appeals to secrecy might work, in relation to trust, in the name of national security (but not all the time, as Relyea [1994] showed), secrecy in peer review and scholarly communication breeds skepticism and suspicion. In Sharp's (1990) analysis of "publication bias," no less than nine proposals are listed to improve fairness in peer review. He observed that "implicit in many of them is a lack of trust in the ability and integrity of the reviewer" (p. 1391). One of the nine proposals is to abolish secrecy. Secrecy in peer review (in its broader definition) is one of the most effective weapons for silencing scholars in a democratic society.

Peer-Review Secrecy

There is not a crime, there is not a dodge, there is not a trick, there is not a swindle, there is not a vice which does not live by secrecy. Get these things out into the open. (Ruesch, 1995, p. 3)

If peer review, in its broader definition, consists of the total assessment that a work receives, referees' reports and editorial decisions are a small but crucial part of the entire peer-review process (which, in theory, is a continuous, open-ended process that proceeds as long as the work is discussed by peers). Within the total process, secrecy can assume various forms in relation to various audiences. The most common forms of secrecy are described as anonymous refereeing and double-blind review.

At the same time, the very concepts of suppression, censorship, and cover up all imply secrecy, in the sense that there are attempts to keep certain persons or groups from having knowledge of specific facts and ideas. In terms of silencing of scholars, secrecy and peer review would go hand in hand.

As discussed earlier, important knowledge remains secret (i.e., hidden) when false paradigms act as impediments to discovery. In this sense, secrecy has something of a passive nature, in contrast to intentional suppression and censorship. The common phrase "unlocking the secrets of nature" implies that the secrecy was not caused by peer-review suppression, but rather that peer-review approval of original research allowed, or helped, the secrecy to be overcome.

In some cases, secrecy imposed by peer-review suppression might have a specific impact and might appear significant to a small number of specialists in a given acad-

emic discipline (and might also be of interest to sociologists studying academic behavior), but might not be of any special concern to other scholars. In other cases, such as studies of infectious diseases or highly toxic poisons in consumer goods (or in the air or the water supply), secrecy might affect the public at large, and thus all scholars regardless of their disciplines.

It would seem that in the halls of academia, where the rhetoric praises and underlines the importance of mutual trust, free exchange of ideas, free flow of information, and open debate and discussion, secrecy would be most unwelcome. Yet, it is obvious that secrecy is entrenched in academia, with terms like *anonymity*, *blind review*, *double-blind review*, and *confidentiality* used, instead of the word *secrecy*, in a manner that deflects attention away from the direct contradiction between secrecy and mutual trust and open discussion.

WHY SECRECY?

Perhaps a hypothetical situation can help in understanding some aspects of the nature of secret peer review. Suppose that a scholar is asked by an academic publisher to be a peer-review referee for the manuscript of a book that the publisher is considering. The policy of secrecy is used, on the basis that secrecy will protect the referee from retaliation in the case of a negative judgment about the manuscript, thus allowing the referee to give "honest" opinions. Suppose that the referee gives a scathing negative review, and as a result the publisher informs the author that the manuscript is rejected. The author goes to another academic publisher, and the book is eventually published.

Now suppose that the referee who gave the secret scathing negative judgment of the manuscript is asked to write a book review now that the manuscript is published. What does the reviewer do in this case? There would seem to be several possibilities: (a) write another scathing review, perhaps even more negative, in order to keep sales, distribution, and access to the author's ideas as low and as limited as possible; (b) insist that the published book review is anonymous, or published under a false name; (c) write a less negative review this time, or a balanced review, or even a favorable one, in order to avert retaliation; or (d) decline the invitation to write the review, out of fear that negative comments will result in retaliation.

The last of these possibilities would seem to be the most logical. The result would be that another scholar would be sought to review the book, and the scholar who declined to review the book would end up with one less entry on his or her curriculum vitae. The third possibility, if enacted, would indicate that published peer review (e.g., book reviews) is not as forthright and "honest" as secret peer review. The second possibility (i.e., anonymous book review) might conflict with the editorial policy of the journal or newspaper, and therefore amount to a result that is the same as a decision to decline to write the review.

The most interesting possibility is the first one; that is, writing of a signed negative

review after writing a secret negative review of the same work. It would demonstrate that there really was no need for secrecy in the first place. In this case, anonymity would not have affected the contents of the review, which would have been very negative whether it was signed or whether it was secret. The overriding factor was the negative nature of the review, not whether it was written openly or in secret.

The fact is that scholarly journals do publish signed reviews (book reviews) that are strongly negative. This fact might lead to at least two questions. First, was the favorable secret peer review that recommended publication erroneous (in the sense that the work should never have been published if it was as bad as the negative review claimed it is)? Second, if the editors of these journals can find competent reviewers who are willing to write negative reviews openly (in the form of book reviews), why cannot the editors find reviewers for unpublished manuscripts without resorting to secrecy? In other words, why is there really any need, at all, for secrecy? If these editors believe in mutual trust and open communication, why do they not abolish secrecy in their own editorial process? Certainly fear of retaliation would not really be a reason after all, as demonstrated by the published negative reviews that are signed.

HIDDEN RESPONSIBILITY

Other factors, however, might benefit from secrecy. One of the most obvious of these is conflict of interest of a personal, institutional, collegial, financial, or other type of nature, particularly if members of the editorial leadership are themselves directly involved. Conflicts of interest can also overlap with responsibility for publication or rejection. Who was responsible, for example, for approval of all the articles of Darsee, Breuning, Felig, and so on (and their many coauthors) that were published and then subsequently retracted because of the clamorous errors in them? Editors might be able to claim, with justification, that the material was highly specialized and that it was necessary to rely on (i.e., trust) the judgment of specialists. The editors can thus avoid personal responsibility for having published clamorous errors. Responsibility would then be shifted to hand-picked peer reviewers. The hands of these referees may well also be clean, without washing, if they made honest miscalculation of judgment. However, if their hands have been dirtied as a result of some conflict of interest that prompted a favorable recommendation, there is no need to wash the hands, because the dirty hands remain a secret.

Editors who can find scholars who do not hesitate to write signed negative reviews, and editors who, at the same time, allow scholars to write secret reviews, are at best really paving the way for the exploitation of conflicts of interest, and paving the way for referees to avoid personal responsibility. At worst, such editors are enabling scholars to hide potential or actual conflict of interest situations.

In contrast to the responsibility for secret peer-review approval of articles that end up being retracted, there is also a question of responsibility for the secret peer-review rejection recommendations for works that later became so-called citation classics, or

rejection of works that lead directly to high honors such as Nobel prizes. In "Have Referees Rejected Some of the Most-Cited Papers of All Times?" Campanario (1996b) discussed some rejected papers that were eventually published and led to Nobel prizes, and he also discussed other rejected works that eventually became among the most highly-cited articles in their fields. He stated that the present peer review system "often allows excellent manuscripts to be criticized by referees with vested interests" (pp. 302), and he added, "We could even speculate on the fact that some good papers that would have received many citations were never published because of the delays due to reviewing and initial rejection" (p. 308). Once again, editors can claim lack of expertise and necessity to rely on the judgments of referees who are specialists. The referees might have been involved in what amounts to variations of the paradigm-dependency phenomenon. They just could not fathom the importance of the new material because of their conditioned ways of thinking within the paradigm. In this case, their rejections would remain within the realm of honest error.

On the other hand, reasons for rejection in the cases discussed by Campanario (1996b) might have been motivated by, and based on, conflict of interest situations or vested interests. Whatever the reasons for such rejections—conflicts of interest, paradigm dependency, or honest errors of judgment or miscalculation—it is important to keep in mind Dalton's (1995) observation that "secrecy permits referees to get away with slipshod work" (p. 235).

DISTRUST AND THE ORIGINS OF SECRECY

It seems obvious that, in effect, peer-review secrecy originates with the editor's distrust of the author, or perhaps of the referee. If an author asks the editor for the name(s) of the referee(s), and the editor refuses to give the answer, the author might ask why. If, in this situation, editors reply that they are simply carrying out editorial policy, the author might ask what the basis is for such a policy. If editors reply that they can expect more honest opinions from referees if their names remain secret, the author might ask the editors why they do not choose referees they can trust to give honest opinions for which the referees will take personal responsibility. The author at this point might also make a specific request to have such referees review the manuscript in question.

At this point, if editors trusted the author, they would either agree to use such referees (e.g., referees whose names are known to the author and who will assume personal intellectual responsibility for their peer-review judgments), or else admit that they cannot find any such referees. If they claim the latter, it would mean that referees would not review manuscripts unless their identities remain secret. In such a case, editors could claim that the referees, not the editors, are responsible for peer-review secrecy.

If, on the other hand, editors admit that they use secrecy in their peer review in order to protect their referees from retaliation from the authors, then it is obvious that

the secrecy is based on the fact that the referees do not trust the author. In such a case, it would be the alleged untrustworthy nature of the author that is responsible for peer-review secrecy.

Another alternative is for editors to say that they trust both authors and referees, but despite this trust they prefer to conduct their peer review in secret. In this case, however, they would then be admitting that they themselves are responsible for conducting secret scholarly communication operations that directly contradict their own professional rhetoric about open discussion and free exchange of ideas.

In any case, editors, referees, and authors are scholars and colleagues in their disciplines. Editors most likely have been, and continue to be, authors. The same is true for referees. Thus, when secrecy exists within the peer-review process of the literature of a scholarly discipline, no matter how the responsibility for secrecy is analyzed, it shows that scholars in that discipline tend to show a distrust, instead of mutual trust, toward each other, despite all the rhetoric to the contrary.

SECRECY AND VESTED INTERESTS

Once secrecy is established by means of editorial policy, this secrecy confers power to editors and referees to promote their own vested interests and special interests that remain hidden from other scholars and from the public. If they feel they have to falsify in order to promote these interests, secrecy allows them to avoid responsibility for falsifications.

In an academic climate in which editors and their institutions, friends, and colleagues and referees and their institutions, friends, and colleagues, are in keen competition with other scholars for grants, promotions, prizes, consultantships, and financial gains from research results (e.g., patents and contracts), the question arises whether anyone should be trusted with the peer-review secrecy that can conceal vested interests. Other questions might also arise. For example, if editors do not trust referees to give honest opinions if their names are revealed, why should editors trust them to reveal their conflicts of interest if these conflicts can be hidden by the secrecy of the referees' identities?

No matter whether editors trust neither referees nor authors, or whether they trust both groups, or trust one group but not the other, the questions just asked lead back to the question of why editors really want to operate within the realm of secrecy in the first place. It would seem that the best way to dispel any distrust on the part of any of the parties involved (authors, editors, and referees) would be to have all of their peer review actions and decisions out in the open so that they could be judged by their peers.

It may well be that the two-part article "Imbroglia at Yale: Emergence of a Fraud" would never have been written if peer review had been open instead of secret. As mentioned earlier, Broad (1980) related that Arnold Relman, editor of the *New England Journal of Medicine*, sent Helene Wachslicht-Rodbard's manuscript to Philip Felig for

review. Felig showed the paper to his colleague Vijay Soman, and Felig then wrote Relman that the paper should be rejected. Soon after the rejection, Felig and Soman were coauthors of a very similar paper, containing some material from the rejected paper, that was sent to *American Journal of Medicine*, where Felig had an editorial leadership position.

If Felig and Soman had known beforehand that Felig's rejection advice and Felig's name as the author of the rejection advice were both to be communicated to Rodbard, maybe it would have seemed too risky for Felig and Soman to send their specific article, as it was written, to any journal. On the other hand, if the rejection advice could not be traced back to Felig, then Felig and Soman could have claimed they arrived at the same conclusions first. In this case, the conflict of interest would have remained secret if Relman had not been led to reveal to Rodbard (in the wake of her suspicions of plagiarism) that it was, indeed, Felig who recommended to him that Rodbard's paper be rejected (Broad, 1980)

As long as the names of referees remain secret, it is impossible to know how many other cases of similar referee activity have taken place. It is also impossible to tell precisely how many attempts at plagiarism by means of peer-review secrecy have been successful. It is obvious, however, that open peer review in the place of secret peer review would allow easier detection, with the result that fewer attempts at plagiarism of this type would take place.

Secrecy can cause doubts and problems at various other levels in addition to those involving referee reports. In the *Cell*-Baltimore case, it was reported that the NIH "decided to protect its five-member investigating panel from outside scrutiny, going so far as to keep their names secret" (Mervis, 1989, p. 1). It was also stated that Paul Friedman, who had previously handled important investigations in academia similar to the one involving Baltimore, "scoffs at the need to keep secret the members of the investigative panel ... anonymity is hardly the answer. It just isn't right to hide their names from everyone else who's involved in the investigation" (Mervis, 1989, pp. 1, 4). In such a case, it would not be possible to know if the secrecy was actually protecting the investigative panel members from the persons being investigated (and from their friendly colleagues), or if it was hiding evidence of a possible conflict of interest, based on collegial cover up, with the intention of absolving the persons under investigation. In any case, the decision to have scholars serving on a secret investigating panel was made at about the same time as an editorial appeared in *Science*, stating that "today's scientists need to realize that errors must be handled ... in full view of an anxious public" (Koshland, 1988, p. 637). Rhetoric and reality emerge again.

Problems of peer-review secrecy have been discussed in relation to doubts about the effectiveness or safety of the Salk polio vaccine:

In answer to these charges there has been a curious silence. ... The charges of secrecy and restriction of pertinent data to a small handful of selected physicians is not a new one. It has been made twice by Dr. Wendell M. Stanley, Nobel Prize winner ... Dr. E. M. Krumbiegel ... Prof. Dr. Redeker ... Dr. Sven Gard ... have also made the same charge. (Ratner, 1988, p. 329)

Situations like this one might lead to the inference that as the commitment and stakes get higher, the perceived need for strategic secrecy and silence becomes greater.

COVER UP

One form of secrecy involved in high-stakes situations in which financial rewards and reputations are on the line is cover up. Problems associated with cover up came up during various phases of the *Cell-Baltimore* case. In this regard, Lang (1993) stated that "in some cases panels have actually contributed to intimidation or to covering up" (p. 44). Lang also pointed out that Imanishi-Kari herself was quoted as saying, "If OSI reaches the conclusion that there was misconduct on my part, then you have to conclude that MIT covered up and Tufts covered up" (p. 44). Lang then observed that "The OSI has now reached that conclusion" (p. 44).

Di Trocchio (1993) discussed the Stephen E. Bruening case at length, with considerable attention to the activities at the University of Pittsburgh and the NIH that resulted in long delays in investigations concerning misconduct. Sprague (1987) mentioned that the investigations dragged on for a long time, with the final report appearing "three years and five months after the investigation started" (p. 12)! Secrecy in peer review goes hand in hand with cover up of conflict of interest situations.

Secrecy in peer review (in its broad definition) also occurs when review of data is not allowed because of confidentiality, or when source material of one form or another is kept hidden from researchers. Hammerschmidt and Gross (1995), in their recent study of biomedical fraud, made some pertinent remarks on the subject:

The author ... declined to produce the primary data for independent review. ... Even after fraud is suspected, the confidentiality that rules in peer review favours the perpetrator by inhibiting the investigation. ... We suggest that potential authors routinely give biomedical journals the right to scrutinize research records on demand. (pp. 3, 6, 10)

In his review of *Confronting the Experts*, Gratzer (1996) described similar problems involving secrecy: "So the evidence that sewage would not always sink and disperse ... was suppressed. Beder gained access to forbidden documents and fought her way through a morass of evasion and obfuscation by politicians and others" (p. 35). In *Intellectual Suppression*, Pugh (1986) stated that, in some cases involving university investigations, "elected representatives of academic staff" become "caught up in a conflict of interest situation" and, as a result, they might "prefer silence and secrecy" (p. 238).

Peer-review secrecy might often be involved in situations dealing with toxic, poisonous, and harmful substances. In "Hidden Hazards," Kasperson and Kasperson (1991) asked, "How is it, then, that certain hazards pass unnoticed ... growing in size until they have taken a serious toll? How is it that asbestos pervaded the American workplace and schools when its respiratory dangers had been known for decades?"

(p. 9). A partial answer might be found in a closed circle comprised of vested interests, conflicts of interest, and peer-review secrecy.

It would seem that if peer review were open rather than secret, conflicts of interest could be detected more easily, and the healthy skepticism that would be stimulated by such detection might result, in turn, in a greater possibility for the detection of error, or even for the accurate prediction of error. If peer review were based on trust and free of conflict of interest, there should be nothing to fear in terms of retaliation, and nothing to hide, and there would not seem to be any reason to prevent openness from replacing secrecy.

According to Walker (1994), there were gag clauses in experiments for testing AZT in treatment of AIDS patients. Scholars and doctors participating in the so-called Concorde Trials had to agree to have approval from Wellcome before publishing material based on the trials. The gag rule, it seems, extended to requiring doctors not to complain or object if they were not granted permission to publish their material. It would seem obvious that such gag rules impose a type of peer-review secrecy on certain findings and ideas.

In the case of AZT, negative criticism seems to have been muted at an early and crucial stage. When more vociferous negative criticisms were made, such as in Lauritsen's (1993) *Poison by Prescription: The AZT Story*, in Duesberg's (1996b) *Inventing the AIDS Virus*, Martin Walker's (1994) *Dirty Medicine*, and in *AIDS: La Grande Truffa*, by De Marchi and Franchi (1996), it appears that AZT had become an entrenched paradigm as a medicine for AIDS, if not the medicine. These later doubts seemed to have been brushed aside as AZT continued to be used widely. Some of Duesberg's criticisms of AZT have been taken up by the Italian press. An article in a leading national newspaper, the English title of which would be "Why We are Losing the War against AIDS," repeated Duesberg's claim that AZT is not only ineffective, but also dangerous. This article is, in effect, a discussion of Horton's (1996) review, which was translated into Italian in *La Rivista dei Libri* (Prattico, 1996). It is obvious, however, that a challenge to an entrenched paradigm is more difficult than a challenge, presented during open debate among scholars, that reveals evidence before the medicine attains paradigm status that the medicine is useless and dangerous.

SECRECY IMPEDES THE STUDY OF PEER REVIEW

Wenneras and Wold (1997) emphasized that an "in-depth analysis" of peer review "can be achieved only if the policy of secrecy is abandoned" (p. 343). Along this line, it is stated that the "policy of secrecy in evaluation must be abandoned" (p. 341). It would seem logical that one of the best forums for discussion of secrecy in peer review would be a conference or congress specifically organized to discuss peer review. A recent attempt, however, to engage in such a discussion at the International Congress on Biomedical Peer Review and Global Communications in Prague was rejected. The identities of the referees in this case remained secret, and the Congress

Director, Drummond Rennie, and the Congress Coordinator, Annette Flanagin, did not give the slightest hint to the author about what the referees said in their secret reports. The closest thing to a specific reason given (D. Rennie and A. Flanagin, personal communication, March 7, 1997) for the rejection of the paper, "The Peer Review Process and Questions of Trust, Openness, and Correction of Error," was that "priority" was given to "new and original research" and to abstracts that are "most relevant to and critical of the processes of peer review and scientific publication."

In their studies of peer-review practices at the Swedish Medical Research Council (MRC), Wenneras and Wold (1997) were able, by means of the Freedom of the Press Act, to gain access to much material that would otherwise have remained secret. Based on this material, Wenneras and Wold concluded that peer review at the MRC was characterized by sexism and nepotism. In terms of nepotism, they observed that "applicants who were affiliated with a committee member" received more favorable treatment than applicants "who lacked such ties" (pp. 342-343).

Regarding the influence of, and affiliation with, "committee" members, it would be interesting to know what Wenneras and Wold, or other scholars, would find if they could gain access to the secret peer-review material relating to the 1997 Prague peer-review congress. The program of this congress reveals that 3 days of this congress are devoted to peer review, with a fourth day devoted to the effects of electronic publishing on peer review. The program for the 3-day program on peer review includes 47 papers. No fewer than 11 members of the committee (i.e., advisory board) that organized (and that controls) this congress, including Rennie and Flanagin, had their papers receive peer review acceptance. In fact, four of Rennie's papers were accepted, as were four of another advisory board member, Richard Smith. (In addition to Rennie and Smith, three other advisory board members had more than one paper accepted.) In fact, 22 of the 47 papers that were accepted are authored by members of the advisory board or by their coauthors (or coauthors of their coauthors). These facts indicate that nepotism in peer review is not limited to the practices of the MRC (Campanario, 1996a). Under these circumstances, it is easy to understand that a paper dealing with questions of "openness" in peer review was rejected.

It is obvious that the secrecy of peer review makes it difficult, if not impossible, to study the secrecy of peer review, or to make a comprehensive study of peer review itself. A rather strange and somewhat inexplicable episode illustrates this point. In 1990, *Library Journal* sent out a request to a scholar to write a review of Noble's (1990) book *Bookbanning in America*. The review was written, as requested, and sent to the book review editor of *Library Journal*. Some time later (a few years or so), the author of the review wrote to the publisher, inquiring about where reviews of the book had been published, because it was not possible to trace or track down any published reviews. The publisher replied with a list of several publications, including *Library Journal* and *Wilson Library Bulletin*.

By the middle of 1996, it was possible to trace, through *Library Literature's* review sections from 1989 through 1994, notices of reviews of Noble's book published in *Choice* (January 1991), *ALA Booklist* (October 1990), and the *Newsletter on*

Intellectual Freedom (March 1991), but not the other journals that the publisher mentioned. This represented a very strange and confusing situation and another letter of inquiry was sent to the publisher (G. Moran, personal communication, May 9, 1996), which containing the following:

I am very interested in the situation at *Library Journal* and *Wilson Library Bulletin*. Do you know if these two journals actually published reviews of *Bookbanning in America*? If so, could you please kindly send me copies of the reviews, or at least let me know which issues contain the reviews? If these two journals did not publish reviews, despite your listing them as reviewers of the book, do you have any idea why they did not?

Meanwhile, more than one letter had been written to the book review editor of *Library Journal*, asking about the publication of the review of *Bookbanning in America*. No replies have been received from *Library Journal*, nor has a reply been received from the publisher in response to the last letter of inquiry. It seems possible that reviews of the book were suppressed after they had been requested, written, and sent in. In any case, it does not seem that the book review department officials at *Library Journal* or the head of the publishing company are willing to discuss what took place. Secrecy seems to reign, and authors of the unpublished reviews are silenced in this case.

All this secrecy about suppression of reviews that the head of the publishing house thought would be published is baffling. Somebody, or a group of persons, might have been upset or uncomfortable about some aspect of the book, or about the specific reviews that were written but remain unpublished. Perhaps the following passages from the book have some bearing on the situation:

Usually, challenges to books galvanized from the Right. ... the bookbanning had seemed to materialize from the conservative side. ... Now all of a sudden the bookbanning came from a different direction. ... Here is where bookbanners on the left have joined bookbanners on the right in lockstep. ... The consequence has to follow: no one is safe from assuming that what didn't offend in one generation might continue to not offend in another generation. (Noble, 1990, pp. 270, 273-274)

It might be that, somewhere along the line, these passages (and others related to them) hit a raw nerve relating to political correctness that led to some peer-review secrecy and silencing of scholars as far as the unpublished reviews are concerned. At any rate, political correctness itself has become one of the most acute problems of all concerning the silencing of scholars.

Politically Correct

As the religion professor delivers a lecture ... at the Chicago Theological Seminary, a school official sits nearby with a tape recorder, a kind of word cop, in case the professor says anything sexually offensive. ... Professor Snyder recited a story from the Talmud, the writings that make up Jewish civil and religious law. ... A woman in the class was offended ... because she believed the story justified brutality toward women. She filed a complaint ... Mr. Snyder, an ordained minister ... has used the Talmudic lesson in the classroom for more than 30 years. The university issued a formal reprimand and notified every student and teacher at the school. (Johnson, 1994, p. 1)

This situation at Chicago Theological Seminary shows that silencing of scholars by means of application of so-called political correctness pressures is a complicated subject, filled with ambiguity, contradictions, trendiness, and changing academic attitudes based on the political fashion of the time. In this case, a passage from a sacred scripture that had been used for decades by a professor, Graydon Snyder, to illustrate questions of ethics and morality to his students was suddenly declared politically incorrect. Furthermore, the professor was punished for using the passage as part of classroom discussions.

It would seem that the term *politically correct*, as it has been applied on campuses, is itself inherently self-contradictory in a democracy, where it is not politically correct, by nature of democracy itself, to impose a specific politically correct stance that curtails freedom of expression and academic freedom. As discussed in Chapter 1, dictatorships impose political correctness within their territorial jurisdiction.

INTELLECTUAL FREEDOM AND THE DELETION OF POLITICALLY INCORRECT LANGUAGE

A recent publication of the Office for Intellectual Freedom (OIF) of the ALA reveals some problems involving intellectual freedom and the ambiguous, complicated, and contradictory nature of political correctness. It would seem that officials of OIF would be allies of Snyder and other scholars whose writings or teaching materials have clashed with political correctness on campus at a given time. In fact, the January 1991 *Memorandum* (with its *Attachments*) by Krug and Levinson, sent to "Chapter Intellectual Freedom Committees," included the following ideas:

Everyone has the right to freedom of opinion and expression. ... We know that censorship, ignorance, and limitations on the free flow of information are the tools of tyranny and oppression. ... Any effort to restrict free expression and the free flow of information aids the oppressor. ... We believe that ... pressures towards conformity present the danger of limiting the range and variety of inquiry and expression on which our democracy and our culture depend. ... It is wrong that what one can read should be confined to what another thinks proper. ... Freedom is no freedom if it is accorded only to the accepted and the inoffensive. ... We realize that the application of these propositions may mean the dissemination of ideas and manners of expression that are repugnant to many persons. (Krug & Levinson, 1991, Attachment I, pp. 1-2; Attachment VII, pp. 1-2, 4-6)

These are strong words. There would seem to be little doubt that the ALA OIF leadership affirmed, with this issue of *Memorandum*, that a truly politically correct stance allows freedom of expression and freedom to read. Professor Snyder, after reading these words and ideas, might feel he was justified, after all, to cite and discuss passages from sacred scripture (e.g., the Talmud) that some persons considered to be sexist and, therefore, politically incorrect.

In this same issue of *Memorandum*, however, one also read, "The Intellectual Freedom Committee, and the Freedom to Read Committee of the Association of American Publishers ... recently completed a review of the document. ... Editorial changes have been made to delete sexist language" (Krug & Levinson, 1991, p. 3). After reading about this combined action of these two committees, authorities at Chicago Theological Seminary might feel justified in deleting sexist language from classroom discussions, including sexist language and ideas in sacred texts that constitute religious law. After all, if the OIF itself felt that expressions that are sexist should not be allowed in their own literature, why should academic authorities allow similar expression (i.e., sexist) in their classrooms or elsewhere on campus?

A CASE STUDY OF POLITICAL CORRECTNESS AT ITHACA COLLEGE

Officials at Ithaca College in Ithaca, New York instituted a "remedial action" pro-

gram for a tenured professor who was found to have used "sexual innuendo" and "sexual allusion" during teaching. Such innuendo and illusion were considered unacceptable, based on principles of political correctness that held sway at the time. A Memo by the Provost, dated November 13, 1992, indicated that unacceptable "examples of language" included "a climaxing moment," and the professor's suggestion to cello students to create "a tone that is transparent like a negligee." The remedial actions included the instructions to "stop immediately the use of sexual innuendo in any and all ... teaching settings," and also "to purge all sexual allusions from ... presentations." Classes of the professor were monitored by an Ithaca College authority to observe compliance. The "transparent like a negligee" phrase had apparently been used for years by the professor as a teaching device, similar to the use of the passage from the Talmud by the scholar at Chicago Theological Seminary.

Monitors (or other persons) at Ithaca College discovered that subsequently there was "continued use" of sexual allusions and innuendo in the classroom, including phrases such as "tuning is an intimate experience," "we only leave the womb once," and "follow through with an olympic stroke." The President of Ithaca College then wrote, in a communication to the professor, dated August 17, 1993, "your behavior cannot and will not be tolerated by Ithaca College. Accordingly, you are hereby dismissed from your employment with Ithaca College, effective immediately." As far as can be determined, the Board of Trustees and the leadership of alumni groups of Ithaca College approved of this firing of a tenured professor, or at least gave their consent by silence in the face of letters of inquiry about whether they approved of the action. This case at Ithaca College leads to the question of whether or not it is possible to eliminate and "purge" all sexual allusions and innuendo from all "teaching settings" without removing books from the college library that contain such allusions and innuendo. Students in all disciplines might go to the library, in the wake of a class lecture or discussion, to look for material that discusses the subject matter in more detail, or to look for background material on the subject. Such consultation of material could easily be described and defined as being within teaching settings.

Suppose that the Ithaca College professor had refrained, once remedial actions had been put into effect, from making any more sexual allusions and innuendo in the classroom (and in the studio or concert rooms where the students practiced and rehearsed their music). Suppose also that students, after attending classes that were free of sexual allusions and innuendo, went to the library in search of background material relating to their courses. Such background material is filled with what college authorities might regard as sexual allusion and innuendo. For example, students might come across the following passages (among many similar ones) in material relating to their courses: "frankly sensuous passion ... purely sensuous effect" (Grant, 1965, pp. 414-415). Students doing research on the music of Richard Strauss might read a reference to "a physique suitably voluptuous for shedding the seven veils" (Kupferberg, 1975, p. 99), and other music students might find this passage in a book

by the famous musician Yehudi Menuhin (1981): "Like love which requires two to become one, so violin playing becomes alive with the complete integration and coordination of both hands" (p. 126).

Now suppose that a student checked out from the library one or more books with such allusions and innuendos, brought the books into class (while the political correctness monitors were present), and started to discuss the contents of the books with classmates and with the professor. Would the monitor (perhaps a dean, in this case) and authorities punish the student as well as the professor? Would the monitor confiscate the books?

Cornog and Perper (1992) wrote an article "For Sex, See Librarian," in which they invoked the ethical "framework" of the "Freedom to Read ideal" (p. 12). They explained that the Freedom to Read ideal was formulated by the ALA, with its "watchword" of "All points of view on all questions" being "surely known to all librarians" (p. 9). They stated further that "the Freedom to Read ethic" has become "the official ... stance of professional librarianship" (p. 9). According to this ideal, in the view of Cornog and Perper, the librarian "accepts the *totality* of outpourings of the human mind and soul concerning sexuality and all its moral issues" (p. 12). The Freedom to Read ideal, it would seem, also allows access to readers of this totality, and not only to librarians (unless the ALA, in formulating the ideal, and Cornog and Perper, in invoking it, were speaking for themselves only, rather than for the public at large, which would include professors and their students).

The Cornog and Perper (1992) article appeared at about the time of the political correctness episode at Ithaca College. Inquiries were made (G. Moran, personal communication, October 28, 1993) of Cornog and Perper in relation to their article, to the Freedom to Read ideal, and to the Ithaca College situation, based on the following: A hypothetical situation is described in which a powerful right-wing political leader, known for his outspoken views against pornography, after leaving his job with the government, became the president of a university or college, and, once in office, sent a written message to all faculty members. The message, in this hypothetical case, instructed faculty members to purge all sexual allusions and innuendo from all teaching and classroom settings. Further, the message would state that monitors would be sent into classrooms where there were reported violations. Professors who continued to use sexual allusions, or what the monitors interpreted as sexual innuendo, would be fired.

Cornog and Perper were asked: "If a powerful group of academics said such things, is it somehow different" than if a powerful political leader, now turned academic administrator, said the same things? They were also asked if "the same words would have an ethical justification *if* or *because* powerful academics wrote them?" Another pertinent question was asked: "And from the standpoint of the 'Freedom to Read ideal,' do you think it is really possible to 'purge all sexual allusions' from 'instructional presentations' and ... teaching settings without 'creating' an ethical basis to purge such material from the academic libraries also?" No reply has been received, and silence reigns.

It is not known how many scholars have actually been silenced, to one degree or another (ranging from deletion and suppression of specific teaching material, to being fired), as a result of political correctness censorship. Limbaugh (1993) described a case at the University of Alaska where a professor described a situation in which, in Limbaugh's words, "the faculty was under pressure to graduate some native students who had not yet mastered the required skills" (p. 235). It was reported that the professor was declared a racist, was not allowed to teach courses in the education department, and was investigated by the university authorities and also by the U.S. Office of Civil Rights. The professor apparently was not intimidated, and she defended herself from the charges. Limbaugh wrote, "She fought back and she won. We don't hear much about the thousands of political correctness victims who roll over and lose. But their numbers must be legion" (p. 235). In one era and in one place there was Kristallnacht, and in other times and other places political correctness knocked.

In some states such as Michigan and Wisconsin, codes restricting speech that have been drawn up by university authorities have been overturned by the courts or by specific laws (Johnson, 1994) However, formal codes are not needed for powerful academic authorities to equate unwanted speech with intolerable behavior. There is no reference, for example, in the August 17, 1993 communication (in which the Ithaca College President fired a professor) to the violation of a specific speech code, but rather to "behavior" that "cannot and will not be tolerated."

Ambiguity regarding expression and behavior can work in favor of the censors and suppressors. Burning something might be an evil act or a protected means of expression. Burning a flag might be considered a means of expression, whereas waving a flag or merely displaying one might be considered an intolerable act amid the ambiguity that pervades the political correctness scene.

It would seem that the concept of labeling as a form of censorship is also fraught with ambiguity. In *Memorandum*, Krug and Levinson (1991) included an attachment, *THE FREEDOM TO READ* (Attachment VII), which speaks out against the "prejudgment of a label," on the basis that "labeling presupposes the existence of individuals or groups with wisdom to determine by authority what is good or bad for the citizen" (pp. 4-5). In this case, reference is made to the labels "subversive" and "dangerous" (p. 4). (Other potential labels might include "immoral," "pornographic," "sexist," "racist," and so on.) It is not specified in the attachment whether labels are written or verbal, nor is it specified whether the concept of labeling applies only to books already in a library collection, or also extends to books that are being considered for library acquisitions. In any case, in the midst of these negative comments about labeling, it seems that Krug and Levinson (1991) and their colleagues decided to label (verbally) parts of one of their own attachments (e.g., Attachment VII) as "sexist" and, as a result, decided to make deletions in the text: "Minor editorial changes have been made to delete sexist language" (p. 3).

CAMPUS PUBLICATIONS AND POLITICAL CORRECTNESS

In fact, much of recent political correctness censorship has been directed against expression that has been given the label of "sexist" or "racist." Campus publications have been among the targets for such censorship. For example, an editorial in *USA Today* with the title, "Keep Our Speech Free" (*USA Today*, 1993) affirmed that "At 29 campuses in 14 months, PC thieves swiped thousands ... of newspapers they deemed offensive. ... University of Maryland's *Diamondback* lost 10,000 copies to snatchers who labeled it 'racist'" (p. 8A). And at Pennsylvania State University "women ... stole 4,000 copies of the *Lionhearted*" (p. 8A).

The suppression of publications on campus has been a long-term aspect of political correctness. What is being censored, suppressed, and silenced at the time depends on what the specific political trends or fashions are that determine the specific politically correct stance of the day. Schrecker (1986) related that in 1947, "the dean of students refused to let the organization put out its magazine, *The New Student*, as an official Harvard publication. Claiming that the format was too polished to be the work of undergraduates, the dean ... called in *The New Student's* editors and grilled them about their politics" (p. 87). The student magazine soon was forced to cease publication. The editors and their magazine were leftist. McQueen (1988) reported that, a few decades later, Dartmouth officials punished student editors of *Dartmouth Review*: "Dartmouth spokesman ... says ... *Review* editors were suspended after they provoked a verbal battle ... and Dartmouth English professor ... Jeffrey Hart ... testified that university officials abhorred the *Review's* call for greater emphasis on traditional college humanities courses" (p. 3). The student editors and their publication in this case were conservative.

THE CHANGING WINDS AND TIDES OF POLITICAL CORRECTNESS

What is consistent in these cases is the effort of powerful academic authorities to silence campus publications that the authorities feel are not politically correct. What changes is the notion, on the part of academic authorities, of what is politically correct or incorrect. At one point, Harvard authorities silenced leftist publications, and at another point Dartmouth authorities acted to silence conservative publications. At certain times, academic freedom, intellectual freedom, and freedom of expression might be considered the most politically correct values, whereas at other times academic authorities might consider academic freedom to be less politically correct than other forms of political correctness that are in vogue at the moment.

This last statement can be illustrated by whether or not scholars will be invited to speak, or whether they will be banned (silenced) from speaking on campus. It can be further illustrated by whether or not scholars, once they are invited to speak, will actually be allowed to speak or will be drowned out (silenced) by one heckling tactic or

another. It can be illustrated even further by whether or not they will be allowed to speak and at the same time be protected from criticism by means of drowning out (silencing) the criticism.

Many Communist or Marxist scholars were banned (silenced) from speaking on U.S. campuses from 1939 to 1941, and later during the Cold War and McCarthy era. Schrecker (1986) observed:

As the Cold War intensified, academic authorities became increasingly reluctant to let Communist, and later merely controversial, speakers address their students. Speaker bans were hardly new. In the previous red scare of 1939–1941, Harvard, Dartmouth, Brooklyn, Cornell, Vassar, New York University, Princeton, Oberlin, Swarthmore, and Smith all barred Party leader Earl Browder from speaking. During the late forties and early fifties, college restrictions against outside speakers intensified. Faculties were often as repressive as administrations. The Faculty Lecture Committee of the University of Michigan routinely refused to let left-wing student or faculty groups bring speakers onto the campus. (pp. 89–90)

It was not considered politically correct to allow them to speak.

As the Cold War cooled down, it came to be considered politically correct to allow controversial speakers to speak on campus, in the name of academic freedom. In effect, political correctness on campus became associated, to one degree or another, with academic freedom. However, this does not mean that in specific cases various means were not employed to favor one ideology over another. Sometimes, conflicting notions of what is politically correct exist at the same time, and attempts are made to enforce them. For instance, the following appeared in an editorial entitled “Yale’s Beastly Behavior” (Wall Street Journal, 1986): “Free speech and academic freedom have not got much respect from liberals these last few years. Reagan officials have to avoid certain campuses.” Meanwhile, Relyea (1994) and Pell (1984) gave detailed accounts of how President Ronald Reagan and leaders in his administration tried to enforce their own ideas about what was politically correct.

Schrecker (1986) documented many cases of leftist scholars who were not allowed to speak on campus because their ideas were not considered politically correct at the time. By contrast, Lang showed how, in the mid-1980s, it was deemed politically incorrect to point out and discuss defective scholarship of the Marxist scholar Jon Wiener, who had been invited by the American Studies Department to speak at Yale. Lang related that he had become aware of defective scholarship and “defamatory journalism” practiced by Wiener, and, as a result, he asked the “Chairman of American Studies for the opportunity to speak.” This request was denied. Lang stated that he “had trouble” delivering a critical statement about Jon Wiener’s scholarship during the public discussion of this forum organized by the American Studies Department, because of restrictions imposed by Jean-Christophe Agnew (who was then Director of Undergraduate Studies of the department), and because, as Lang described it, “there were catcalls and shouts of ‘throw him out.’” In addition, Lang related that he had “asked American Studies for a copy of Mr. Wiener’s remarks so

that he could analyze what was said, but that no text has been provided." (Lang's documentation for this event, and for events stemming from it, are found in his "file" study "The Teaching and Learning File," a copy of which was given to the main library at Yale University.)

A case can be made that in this episode it seems that Agnew and others present at the meeting felt that academic freedom was not as politically correct as the perceived need to protect a Marxist scholar from adverse criticism relating to defective scholarship. A scholar's point of view was silenced at the meeting to protect another scholar. Thus, from a historical and chronological standpoint, factors relating to political correctness at one point silenced Communist-Marxist scholars, at another point tolerated them, and at still another point protected them from criticism of defective scholarship. Now that the Soviet empire has collapsed, the Cold War has been put in the deep freeze (or has already melted away), and Marxism has been discredited, perhaps sometime in the future notions of political correctness will not enter into suppression, promotion, or protection of Marxist studies or discussions. Instead, criticism of Marxists and of specific Marxist scholarship might be regarded as a normal aspect of specialized political science research. Promotion of Marxism might someday be revived on a large scale, or attempts to revive Marxism might be viewed as the equivalent of trying to bring a dead horse back to life.

It should be emphasized that historical trends regarding political correctness in relation to anti-Communism, pro-Marxism, or post-Marxism are not clear cut, but may be overlapping during a specific year or years. At about the same time that criticism of a Marxist scholar was being silenced at Yale, a Marxist scholar at McGill University, David Mandel, was apparently becoming a "victim of unprofessional" conduct on the part of university authorities. As Cude (1987b) described the case, Mandel's "publications were Marxist, he had cancelled classes to show solidarity with striking McGill maintenance workers and his sympathies were pro-Palestinian on the Mid-East situation. While his students admired him, some of his fellow professors detested him, and so they hired an American instead" (p. C7). That specific decision was seemingly "in defiance of Canadian immigration regulations" (p. C7), which state that non-Canadians can be hired only in the absence of Canadians who qualify.

The changing winds and fortunes of political correctness indicate that academic "politics" can be an important aspect of success or failure. Many leftist scholars lost out in the 1950s, and others struck it rich a few decades later on the same campuses where their predecessors lost out. Mandel lost out at McGill at a time that his fellow Marxist Wiener was being protected from criticism at Yale.

A VARIETY OF CASE STUDIES

The concept of political correctness can be extended far in more than one direction. Devastating charges of being subversive, racist, anti-American, anti-Semitic, sexist, and so on, can be exaggerated and falsified, but also made to stick (resulting in pun-

ishment). In hindsight, many such charges might be considered as serious violations of academic freedom, unfair and unjust, or tyrannical denials of due process.

Political correctness can also be related to intellectual paradigms in academia. There is an interesting case in which it seems that ideological and intellectual paradigms are intertwined. Greenhouse (1988) related that, about half a century ago, Bourgin wrote a dissertation at the University of Chicago, in which he challenged "a then-common notion that ... Roosevelt's social programs departed from the *laissez-faire* principles of the U.S. government in its early days" (p. 1). His dissertation was rejected and he did not get his degree. A few decades or so later, he showed his failed dissertation to Arthur Schlesinger, Jr., who "praised the work as pioneering, and who enthusiastically urged Chicago's political science department to take a fresh look" (p. 1). They did, and this time around the dissertation was deemed of sufficient quality (politically correct?) and the scholar was finally awarded his degree. Greenhouse mentioned that one member of the dissertation committee (the second time around) that accepted the work remarked, "The world is full of former graduate students who have failed to get their PhDs" (p. 1).

It might be that when Bourgin was writing his dissertation, Roosevelt's policies were about as politically correct to the University of Chicago political science department as Ronald Reagan's policies were at Yale in the 1980s. In any case, Bourgin and his ideas were silenced. Who knows how many of the many former graduate students who have failed to get their degrees might have been silenced for reasons of political correctness rather than for defective scholarship?

The silence imposed by political correctness can extend beyond the classroom or publications and dissertation committees. During a wedding reception in England around 1990, a Yale alumnus related that at a recent gathering where the famous Yale singing group, Whiffenpoofs, were performing, he requested one of the group's traditional and favorite old-time standby songs, "Daddy Is a Yale Man." He said that he was told that it had been banned and that they would not sing it (apparently because it was sexist).

My Fair Lady was a spectacular success as a Broadway musical in the 1950s, and became a multiple-Oscar winner as a movie. Who knows if the same play would have been boycotted, picketed, panned, or even banned from New Haven if it had opened there around 1990, instead of on Broadway in the 1950s. The lyrics and dialogue are at least on the same scale as "Daddy Is a Yale Man," as far as political correctness is concerned.

As has been shown, notions of political correctness change as political trends and fashions develop in varying directions. The pace and degree of these changes are not always predictable. This unpredictability itself can also lead to silencing of scholars who might become cautious and prudent to avoid getting into political correctness predicaments. Noble (1990) asserted that "once a restriction is placed upon free expression, it will be enforced in ways no one can truly predict ... one man's limitation will be another's unburdening, making it impossible to forecast what will develop" (p. 237). Such uncertainty is underscored in Kimball's (1990) remark that "it is a

sobering irony that what began as an appeal by the Left for free speech at Berkeley in the sixties has ended with an equally fervent appeal by the Left for the imposition of censorship" (p. 69).

Ladd and Lipset compiled "The 1977 Survey of The American Professoriate" and sent it to many scholars. Not all of them filled it out. One professor wrote that "the contents of that questionnaire, if answered correctly by any but milk-toast, middle-of-the-roader academics, could jeopardize the employment status of people even slightly off-center in either political direction if the political climate were only slightly less tolerant than at present" (Lang, 1981, p. 213). That scholar's reaction can give a good idea of how sensitivity to political correctness might lead scholars into silence. At what point does prudence and caution become transformed into intimidation that results in silence in certain subjects?

In addition to notions of political correctness dealing with sexism, racism, multiculturalism, and ethnic diversity on a general level, there are narrower aspects of political correctness that apply to specific academic disciplines, departments, or research centers. Sometimes they might be referred to as academic politics, departmental politics, politics of science, or similar names. As mentioned, Gallo and his AIDS research was involved in the politics of science, and he was silenced about public discussion of "Dingell ... or the patent for the HIV blood test." (Zimmerman, 1995, p. 1)

Gallo's politics of science in this case was conditioned by controversy over specific governmental subsidized research that was under investigation. By contrast, the case of Scott Nearing shows that academic politics can be broad-based as well as specific, as his scholarly ideas were considered politically incorrect by a variety of sources that had differing political orientation. He "was dismissed from the Wharton School of Economics in 1915 due to his opposition to child labour in Pennsylvania" (Martin, 1986a, p. 181). He was also "charged with sedition over his anti-war book *The Great Madness*" and he could not enter Britain "because of his outspoken opposition to colonialism" (p. 181). But Nearing's political views did not please the Communist Party either, which expelled him in 1929 "for refusing to change his views in his book on imperialism" (p. 181).

In her studies of the effects of McCarthyism on academia, Schrecker (1986) stated that "medical schools ... seemed to care less about academic freedom than other types of educational institutions," and she pointed out that Jefferson Medical College "was particularly insensitive to such concerns" (p. 244). In light of her comments, it is interesting to note that some recent studies in the field of medical research actually use the term *politically correct* in discussion of suppression of research findings, theories, and ideas. In *Dirty Medicine*, Mumby (1992) said:

I feel that everyone ought to be warned what is in the wind. What is being attempted here is a serious hijacking of a statutory body, the General Medical Council, for the express purpose of suppressing freedom within medicine and eliminating people who practice medicine which is not politically correct. (Mumby, cited in Walker, 1994, p. 557)

Duesberg (1995) discussed his battles with the AIDS establishment by making use of the same term:

The AIDS virus also proved to be the politically correct cause of AIDS. No AIDS risk group could be blamed for being infected by a God-given egalitarian virus. ... Once accepted as the politically correct explanation of AIDS, the HIV hypothesis has become the central investment for a whole generation of AIDS scientists, AIDS companies, AIDS journalists, AIDS politicians and gay activists. ... Claiming this priority, the virus-AIDS orthodoxy justifies intolerance, even censorship, of all those who question infectious AIDS. ... (pp. 515-516)

He also wrote, "In this era of centralized, government-sponsored science, an article against politically correct science can be fatal for a journalist" (Duesberg, 1996b, p. 335).

In her discussion of women's studies and "feminist critique," Hannah (1986) observed that in Australia after World War II, universities expanded within "the English tertiary system with its classical traditions and conservative outlook. ... The almost total absence of women both as teachers and students ... was taken to be the natural order of things" (p. 201). This reference to the "natural order of things" perhaps could be the equivalent to the nature of political correctness at that time. More recently, some persons might consider it the natural order of things for a theology professor to be punished for discussing (as he had been doing for many years) a passage from the Talmud, or for a music professor to be punished for mentioning a "tone that is transparent like a negligee."

Barbash (1996) described an interesting case involving the concept of political correctness. Cambridge University Press asked a "panel of experts" to review a manuscript for a book that had been submitted by a scholar at the State University of New York at Stony Brook. The panel "enthusiastically endorsed" it. A Cambridge University Press official was also quoted as stating that "there was no doubt the manuscript was extremely high quality" (p. 9). Nevertheless, the publisher decided to cancel publication of Anastasia Karakasidou's book. It was not politically correct in some circles, so much so that the author, it seems, has even received death threats. In the wake of the decision not to publish the book, three peer reviewers and editorial board members of Cambridge University Press "have resigned or dissociated themselves from the publishing house" (p. 9). In this case, both the publisher and the publisher's peer reviewers agreed that the scholar's work was of high quality, but the scholar was silenced nevertheless as the publisher caved in to political forces that did not consider the work politically correct.

Why did the peer reviewers protest the cancellation of this book? One of them, Hertzfeld, claimed the publisher "was acting to protect commercial interests in Greece" (Barbash, 1996, p. 9). Yet, many publishers act, on an almost daily basis, to protect their commercial interests in one location or another. Was Hertzfeld concerned, instead, about the silencing of Karakasidou because such silencing is contrary to the academic rhetoric about open debate, free exchange of ideas, and academic

freedom? Or, is it possible that Hertzfeld felt that the author of the book stated the politically correct point of view in the book and that it should not be opposed? If the author's political adversaries had submitted a manuscript to Cambridge University Press, would the same peer reviewers have approved it, and would they have protested in the same manner if the publisher had canceled the book, under political pressure, despite peer-review approval?

A recent newspaper article by Carvajal (1996), with the headline "U.S. Holocaust Debate Is Silenced," begins, "At the urging of a German publisher and its controversial American author, New York University abruptly canceled a long-planned symposium of Holocaust scholars" (p. 2). The symposium was organized by Deutsches Haus and participants were to include scholars from Harvard, Rutgers, Bryn Mawr, Columbia, and Tel Aviv University, and attendance of about 200 persons was expected.

It is considered a violation of political correctness to attempt to deny that the Holocaust took place (not to mention a violation of the ethical requirement of a scholar's commitment to truth), or to give it a lesser than historical dimension. At the same time, it also seems that in some circles it is considered politically incorrect to claim that a large section of the German population itself was responsible, to a degree, for the Holocaust. In any case, the official reason given for the cancellation of the New York University symposium was that Goldhagen's (1996) book, *Hitler's Willing Executioners: Ordinary Germans and the Holocaust*, was to be discussed at the symposium, and because Goldhagen would not be present "the organizers decided to cancel the symposium because they considered it unfair to discuss his book without his participation" (Carvajal, 1996, p. 2).

By contrast, it seems that discussions of a book by Velikovsky used a different standard for fairness:

The ... settings provided for the discussion of Velikovsky's work were mostly arranged ... by hostile critics or intimidated moderators. He was excluded from discussion of his own work. ... The *Proceedings of the American Philosophical Society*, which in 1952 carried extensive attacks upon him, would not suffer his reply. (de Grazia, 1978, pp. 173-179)

Was it fairness, or questions of political correctness, that caused the New York University symposium to be silenced?

According to a recent article by Williams (1996), it has not been politically correct in Italy to discuss the use of poison gas by the Italians against the Ethiopians in 1935. In the article, "Italy Peers Reluctantly into 2 Faces of Its Wartime Past," Williams claimed that "60 tons of a poison ... were dropped on Ethiopian soldiers" (p. 6). An Italian historian, Angelo Del Boca, was quoted as saying, "There has not been a government that has favored a serious discussion of what we did in Africa" (p. 6). Because Italian universities are, for the most part, under direct central government control, it is understandable how "serious discussions" of a subject not politically correct had been silenced.

It was politically correct, it seems, to protect “Europe’s beef market,” but not politically correct to warn about health dangers. At least that is one of the themes of an article by Buerkle (1996) that begins “The European Commission sought to silence a German scientist who warned about a risk to human health from ‘mad cow’ disease more than one year before Britain admitted a probable hazard” (p. 1). At present, it seems that scientific evidence for mad cow disease and a direct relation to human disease is scanty. If strong evidence were to come to light in the future, demonstrating that mad cow disease was becoming rampant and also that there was a direct link to human diseases, then it might become much more politically correct in this case to warn about health dangers than it would be to protect a region’s beef industry.

A good idea of how political correctness tends to silence scholars might be conveyed by the following passage:

Ideas and activities once tolerated came under attack. Professors and administrators responded by revising the normally vague definition of academic freedom to exclude in a surprisingly explicit way the types of behavior the rest of the community did not like. (Schrecker, 1986, p. 14)

The same could be true for the observation that “what had been tolerated before becomes intolerable now” (Schrecker, 1986, p. 20).

The professor who was suddenly punished in the 1990s for describing a story from the Talmud, as he had been doing for decades before then, might regard these passages as pertinent references to his own case. And the tenured professor who was fired from Ithaca College for saying things like “a tone that is transparent like a negligee” (as he had been doing for years), might believe that these passages could refer to his own situation. However, neither of the two passages was intended to be prophetic. The first one describes academic freedom cases from the end of the 19th century to the 1950s. The second is from the commencement address that the President of Columbia University delivered in 1917.

The cases discussed in this chapter are merely a small sampling of the documentation that demonstrates how academic freedom can take a back seat to whatever politically correct stance holds sway and is being enforced according to the political climate of the day on campus. Intimidation leads to a chilling effect, which in turn leads to a silencing effect. Part of this intimidation results in the form of not citing works that might otherwise have been cited and discussed. This can lead to an “out of cite, out of mind” phenomenon that silences scholars and their ideas.

Out of Cite, Out of Mind

The figure of Guidoriccio da Fogliano is therefore the illustrated figure of the topographical map of the Sienese republic. (Venturi, 1907, p. 614)

In reality we have in the so-called Guidoriccio a remaining external part of the map. (Zeri, 1988, p. 52)

The first of these passages is from about seven decades before the Guido Riccio controversy broke out. The second is from about a decade or so into the controversy, when it was at an intensive phase. Both represent very similar ideas, and have been overlooked, ignored, and not specifically cited by scholars who have attempted to shore up the traditional paradigm that Simone Martini painted the figure of Guido Riccio on horseback. More revealing, a recent extensive study by Kupfer (1996) of the map in question does not even cite, much less discuss, the items from 1907 and 1988, even though the authors of both items, Venturi and Zeri, specifically claimed that the figure of Guido Riccio was an actual integral part of the very map that was the precise subject matter of Kupfer's study. This situation illustrates that scholars' ideas can fall into the silence of oblivion as a result of the out of cite, out of mind treatment.

WHO DISCOVERED THE CAUSE OF CHILDBIRTH FEVER?

Silencing of scholars and their ideas by means of the out of cite, out of mind treatment can have tragic consequences. The suffering and deaths caused by childbirth fever serve as a grim reminder. Semmelweis is often associated with the discovery of the cause of childbirth fever. However, according to Ruesch (1978/1991), the original discovery did not belong to Semmelweis:

Others before Semmelweis had suggested that puerperal fever might be a contagious disease, and that hygiene could prevent it; but they had been laughed at. ... In 1795 Scotsman Alexander Gordon gave ample proof that the disease was contagious, in a paper titled "Treatise of the Epidemic Puerperal Fever of Aberdeen," in which he stressed the need for disinfection. ... Although the evidence he offered was indisputable, it went under amid the general hilarity of the medical giants. ... In 1843 Oliver Wendell Holmes ... wrote "The Contagiousness of Puerperal Fever." It also met vigorous opposition from the leading obstetricians, and its facts began being acknowledged only after it had been enlarged and reprinted in 1855. ... English historian, Lord Moynihan, called it "one of the greatest essays ever written in the history of medicine." Semmelweis had not heard of the English works when he came to the same conclusions. ... Semmelweis returned to his native Budapest and published a book about his findings. But as his countrymen, too, derided him ... he died without witnessing the triumph of his ideas. ... Semmelweis and the few of a like mind obtained recognition a quarter of a century later. (pp. 166-168)

The out of cite, out of mind situation goes even further, in this case. de Grazia (1984) pointed out that Gordon, Holmes, and Semmelweis were all preceded by Charles White, who, in 1773, "had insisted upon absolute cleanliness in the lying-in hospital" (p. 28). As de Grazia observed, "It took about a century from White's obsessive insistence upon cleanliness in Manchester's lying-in wards to consensus about a matter that should have been simple enough to grasp" (p. 29). Who knows if other scholars did not precede White in relation to the cause and prevention of childbirth fever?

WARNINGS ABOUT POTENTIAL DANGERS AND DISASTERS

In 1895, Charles Bell Taylor wrote, "It is not true that we owe our knowledge of drugs to experiments upon animals. The effect of drugs upon animals is so entirely different from their effect on man that no safe conclusions can be drawn from such investigations" (Ruesch, 1989, p. 242). These themes, and variations of them, have been repeated often, as Ruesch (1989) related, by doctors and medical researchers through 1950. These were, in effect, decades-long warnings about potential medical disasters that might occur if animal experimentation was relied on for the safety and efficacy of pharmaceuticals. Ruesch (1978/1991) wrote that, between 1967 or so and the end

of 1970, "a long line of medical authorities had testified that the generally accepted animal tests could never be conclusive for human beings" (p. 361). These testimonies were, in effect, authoritative and expert explanations about why the thalidomide tragedy happened. The thalidomide case was not the last of its kind. Other similar tragedies have occurred in its wake, after approval for marketing of drugs was given following animal experiments with the drugs (Ruesch, 1992).

These examples illustrate different types of out of cite, out of mind situations. The startling 1907 observation about the painted image of Guido Riccio was not assimilated into the art history literature and classroom teaching relating to either the Guido Riccio fresco or to the painting of the map (Mappamondo) on the same wall. In 1977, a similar hypothesis was made, and it caused a violent reaction. It might be that the 1907 reference, found in a footnote in a large volume dealing with a survey of 14th-century art, went unnoticed. A recent monograph study of the Guido Riccio case, *Simone o non Simone*, by Ragionieri (1985), deals at length with a description of the painting written in 1916, but does not deal at all with the 1907 item.

It might be, in this case, that every scholar who has studied Simone Martini, in general, and Guido Riccio, in particular, since 1907 has conducted research that was, to some degree, incomplete, inattentive, and sloppy. It might be that everyone overlooked Venturi's 1907 observation and therefore missed its significance. If that was the case, the item was not cited by scholars because it was not known to them.

In the childbirth fever situation, it seems that some scholars were aware of the 1773 work of White. They were also aware of the ideas of Gordon written in 1795, but, instead of giving them wide circulation, scholars derided them. The same seems to be true for the 1843 work of Holmes (father of the famous jurist). Diffusion of the ideas of these three scholars remained, it seems, within the circle of scholars opposed to these ideas. As a result, these ideas did not get carried forward in the mainstream body of literature so that Semmelweis could become aware of them before he arrived at the same conclusions (or very similar conclusions). It was not sloppy research as much as a short circuit, or breakdown, in the scholarly communication process that prevented Semmelweis from having knowledge that was crucial for his work.

In the case of the thalidomide tragedy, citations that were serious, insistent, and fervent warnings stretching over a long period of time were, in effect, ignored and not cited as animal experimentation continued as the prevailing paradigm. Then, in the wake of the tragedy, the ideas that had been ignored were, in a sense, transformed into alibis to explain what happened. Because animal experimentation is misleading and unreliable, the medical and academic authorities testified (echoing the chorus of many scholars who had previously said the same thing in hopes of avoiding medical disasters), and because such experimentation is required by law before drugs can be marketed, the company that did the animal experiments and marketed the drug based on the results of the animal experiments cannot be guilty of violating the law. (The logic, in any case, goes something to that effect.)

The thalidomide case involves selectivity in the use of citations. In his discussion of precursors of Velikovsky's ideas and theories, de Grazia (1984) equated citations

to a form of intellectual snobbery: "The recounting of one's precursors has in it an element of snobbery, like the genealogical research that discovers barons but not brigands, big shots rather than bums" (p. 335). Citations and intellectual snobbery can also affect service quality and access to material in libraries. A case was published by de Grazia (1984) in which a local library in Kansas "has refused to accept or acquire" a copy of *A Struggle With Titans*. The reason for such refusal, as reported by de Grazia, was that "the standard reviewing media have ignored it" (p. 264).

CITATION AND IMPACT

If ideas, discoveries, or hypotheses are not cited (or applied) at all after they are initially presented by scholars, they cannot have any impact. In other cases, scholars' works might be cited often, and even discussed at length, but with the express purpose of preventing any impact and of preventing future citations of a favorable nature.

Other ideas may enjoy very wide circulation and citation, but they do not obtain any meaningful impact because they are kept out of official policy and establishment thinking. As a result of his persistence (and of the logic of his evidence and argumentation), Duesberg's ideas about AIDS and HIV have attained wide circulation in articles, books, interviews, reviews of his books, conferences, and lectures. At the same time, a very large majority of the mass media have continued to repeat over and over again the official establishment view, as if Duesberg had not written any dissenting views on the subject at all. The mass media thus help create the impression that the HIV-AIDS hypothesis is a proven fact of paradigm status, instead of letting the public know that it is a hypothesis adhered to by the establishment, but also one that is keenly debated and contested by scholars. For example, on the ABC News broadcast of May 7, 1996 (and on the repeat on Italian TV on the morning of May 8), HIV was referred to as the virus that causes AIDS without a hint that such a hypothesis is the subject of intense scholarly debate. (Similar statements to that of the ABC statement have been repeated often in the media since then).

By contrast, sometimes ideas can have a widespread diffusion and a long-lasting, far-reaching impact, but scholars who were key figures in the formulation of those ideas are not cited. The theory of evolution is often associated with Darwin. However, Manwell and Baker (1986a) observed:

It is generally accepted that Alfred Russel Wallace independently discovered the theory of evolution by natural selection and published the outline in the same year as Darwin. Furthermore, it is known that there were important predecessors who had formulated the essential elements of the theory. (p. 284)

Perhaps there was some intellectual and social snobbery at play in this case, as Darwin came from a family that Manwell and Baker described as "wealthy and intellectually prominent" (p. 284). Darwin's publication on the subject is also more extensive and exhaustive, solidifying his position as originator of the theory.

Something similar apparently took place with the studies of the physicist Lise Meitner, who became a collaborator of Otto Hahn until she was forced to flee from Nazi Germany. She continued some collaboration from Sweden, and it seems a major breakthrough occurred in their research about this time. But according to Bartusiak (1996),

Hahn published the chemical evidence for fission without listing Meitner as a co-author. ... In the history of modern physics there are names that perpetually resonate. ... The name of Lise Meitner has diminished to a footnote. ... Fortunately, attention is gradually being refocused on this remarkable woman. (p. 12)

Like the studies of Wallace, Meitner's studies had a great impact in academia, but the spotlights of citation attention have been shining on other scholars who get the lion's share of the credit for such impact.

PEER REVIEW AND ITS EFFECT ON CITATIONS

Problems related to the out of cite, out of mind factor are obviously tied up with peer review when there is deliberate attempt at peer-review suppression. Not only are unwanted ideas rejected from publication, but citations are deleted from manuscripts that are accepted. There can also be tendencies toward self-citation and mutual citation of colleagues, with exclusion, or near exclusion, of citations of works of scholarly adversaries.

Peer-review rejection, however, might also take place because editors and referees are not able to fathom the current or future significance of the work. In such cases, rejection is not so much the result of deliberate suppression as it is the result of lack of knowledge and lack of expertise. It is often difficult, even with the benefit of hindsight, to determine where deliberate suppression ends and honest peer-review error in judgment begins. The result is the same, however, if important ideas are suppressed and false (and possibly dangerous) ideas prevail in the mainstream literature. In "Have Referees Rejected Some of the Most-Cited Papers of All Times?," Campanario (1996b) quoted Glenn:

Who Knows. ... how many papers which could have had an important impact on the discipline lie buried and neglected in obscure journals because of unfair rejections ... or how many incorrect conclusions published in major journals have found their way into textbooks to be taught to unwary undergraduates or have influenced personnel and policy decisions. (pp. 307-308)

Along a similar line, Gibbs (1995) referred to a "vicious circle of neglect" and "publishing barriers" that can "doom good science to oblivion" (p. 92). As the information explosion expands from a proliferation of journals published in the traditional manner to the apparently limitless boundaries of electronic publishing, the Internet, and so

on, there is a corresponding increase in the potential for important scholarly ideas to fall into the silence of out of cite, out of mind oblivion. Language barriers add to the problem, particularly in the case of languages that are rarely found in the literature of a scholarly discipline.

HYPOTHESES TRANSFORMED INTO PARADIGMS

As an idea is cited by various scholars over a period of time, the nature of the idea may be transformed, even to the extent that what was originally presented as a hypothesis eventually is placed in the scholarly record as a fact. At that point, scholars might not even bother to look for dissenting views published in the past, with these uncited dissenting views reduced to silence as a result. Rose (1978) reported that Velikovsky came across this problem in his studies of ancient history:

There are numerous examples of tentative proposals becoming canonical, simply because of the passage of time. Back in the nineteenth century, Boeckh *guessed* that the dates of Eudoxus might be 408–355. Eventually, all of the qualifications were dropped. Today, many dictionaries and encyclopedias and other reference books give those dates as if they were beyond question. In 1938 de Santillana discovered that it is far more likely that the dates were really 390–337. But the “traditional” dates ... have become so widely circulated and so well entrenched that Santillana’s better dates ... have so far not even been much noticed, let alone accepted. (p. 40)

In AIDS research, the famous Gallo–HHS news conference spoke of a “probable” cause, but the word “probable” soon got the out of cite, out of mind treatment in the official policy and among the mass media.

NONCITATION, FOLLOWED BY SURPRISE AND ASTONISHMENT

There are some situations that are presented in the mass media as if they were surprising and startling events and episodes to journalists, and to establishment researchers and administrators. They appear to be surprising and startling not because they are new, but rather because earlier discussions by scholars had been given the out of cite, out of mind treatment. A recent article by James (1996b) indicated that the world’s population is faced with a serious resurgence of tuberculosis, which might kill 30 million people in the next decade or so:

Specialists say that developed countries that thought tuberculosis was vanquished are facing a resurgence of the disease, often in a deadly new and virtually incurable form. ... Sir John Crofton ... called the Milan hospital outbreak ... “terrifying” because it involved a strain of the disease deadlier even than the Ebola virus or the bubonic

plague ... Italy dismantled its network of tuberculosis sanatoriums and clinics in the 1970s believing the disease to be a thing of the past. ... The Milan outbreak involved a strain of the disease resistant to seven of the nine drugs known to be effective. (p. 5)

Altman (1995) also reported on the situation, stating that "a worldwide surge in drug-resistant strains" of tuberculosis is taking place, and that "there are more cases of tuberculosis today than ever worldwide" (p. 3). Ruesch (1978/1991) related that, as early as 1962 and 1963, it was pointed out by Dr. Raiga that "the number of staphylococcal strains resistant to penicillin has been steadily growing" (p. 282). Ruesch also observed that, in 1972, during testimony before the U.S. Senate Monopoly Subcommittee, it was revealed that typhoid bacilli had become resistant to antibiotics (p. 283). He also stated:

When at the end of the forties the price of penicillin suddenly dropped owing to overproduction, the doctors began using it indiscriminately, even for minor flus or common colds, thus depriving the organism of the faculty to develop its own natural defenses. The doctors used the available antibiotics ... without realizing that they were not only weakening the human organisms, but at the same time strengthening the various strains of bacteria, to such an extent that some of them would eventually defy every type of antibiotics. So modern science had begun in the forties already producing stronger and stronger bacteria. (p. 280)

From these passages, it seems that the situation is complicated by the fact that either too light a dosage or too heavy a dosage of antibiotics can result in the survival of more resistant forms of bacteria. If that is true, perfecting the right dosage for specific treatments of specific patients might become something akin to a lottery or roulette, for who knows exactly what is precisely the right amount in each case? The lack of certainty, and the resulting risk, seem so great that a resurgence of diseases such as tuberculosis might well have been anticipated and expected, rather than appear as a sudden surprise. In any case, the out of cite, out of mind form of silencing of the works of scholars that took place in this situation could well have been a factor in the great surprise that was reported relating to the resurgence of tuberculosis.

Another similar example of out of cite, out of mind at work was illustrated by Ruesch (1993). He alluded to headlines in the *New York Times*, from 1993, that read "Many Say Lab-Animal Tests Fail to Measure Human Risk" and "Animal Tests as Risk Clues: The Best Data May Fall Short" (p. 1). If one did not already know that these headlines were from 1993, one could imagine that they were describing the defense of Chemie Grunenthal at the thalidomide trial in Germany a few decades earlier. Soon after these articles appeared in the newspaper, Ruesch observed:

All that the *New York Times* has reported with blue-eyed wonderment had been printed and disseminated 15 years ago by America's biggest publisher, when Bantam Books' Publicity Department sent out 3,500 review copies of *Slaughter of the Innocent* to the "science writers" on its list, and not one seemed to have glanced at it, or caught the message. The book disappeared under silence. (p. 1)

ON SITE AND IN SIGHT, BUT OUT OF CITE

A recent article in *Time* (Gray, 1996) stated that a statue was sighted each day by employees and visitors in a building in New York City, but no one paid very much attention to it until it was recently cited as a work by the famous artist Michelangelo. (On site, and in sight, but out of cite until there was more insight?) The same article pointed out that the poem, "A Funerall Elegie," is on the minds of Shakespeare scholars after it was recently cited as a work by Shakespeare. Gray (1996) observed, "What we could safely ignore or overlook before now commands our reverent attention" (p. 77).

By contrast, it can be risky and dangerous to ignore or overlook many ideas in some academic disciplines. It took centuries after the first observations that citrus fruits and other foods prevented or cured scurvy before it was accepted that scurvy was caused by a deficiency in diet rather than by other factors (Duesberg, 1996b) The earlier observations fell under a long out of cite, out of mind silence, rather than being continually discussed and tested.

Aronson (1986) related an out of cite, out of mind story about the Dutch scientist C. A. Pekelharing. He gave a talk in 1905, which was published in Dutch in 1907, but was apparently not included in an index of pertinent works:

No active nutrition researcher had apparently ever heard of or cited Pekelharing's remarkable statement until 1926. ... In his Nobel speech Hopkins counted Pekelharing among his predecessors ... commenting that "it is indeed astonishing that the results of such significant work ... should not have been rapidly broadcast." (p. 643)

Aronson (1996) mentioned that Pekelharing's work did not appear in an index. If scholars do not locate important scholarly works in indexes, catalogs, or on library shelves, the scholars might not be able to cite certain works or ideas. Indexes and library shelves are only part of the picture, however. The reference of Hopkins to "rapidly broadcast" invokes a broader sense of scholarly communication, one that might include scholarly conferences and informal contacts among scholars. In fact, Hopkins might have been referring to a type of informal communication among scholars known as *invisible colleges*.

At any rate, out of cite, out of mind situations in academia lead to discussion of the role of academic librarians in scholarly communication, and in problems relating to the silencing of scholars.