

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

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Toleration of Falsification

The hierarchies define ethical practices. ... They accept or reject men and material, and inflict sanctions all according to their own interests. (de Grazia, 1978, p. 188)

In a section of one of his file studies (National Institutes of Health [NIH], and Department of Health and Human Services [HHS], September 1, 1993), Lang reported that, as part of an investigation into allegations of improper conduct relating to AIDS research in the NIH laboratory of Robert Gallo, the Appeals Board of the U.S. Department of Health and Human Services (HHS) discussed a definition of scientific misconduct: "This definition cannot reasonably be read as encompassing falsification or any other conduct which does not *seriously deviate* from commonly accepted practices within the scientific community" (p. 5).

Lang commented on this definition as follows (in the September 1, 1993, section of the file study):

The Board has ruled that scientific misconduct does not include "falsification or any other conduct which does not seriously deviate from commonly accepted practices within the scientific community. ... "According to the Board's logic, if falsification becomes a universal practice among scientists, then it receives the legal approval of government agencies which are supposed to overview the maintenance of scientific standards for government grants and government laboratories. (p. 1)

De Marchi and Franchi (1996) also commented on this situation: They observed

that the definition of misconduct was changed after Gallo and others had been found to have engaged in questionable practices. The new definition, according to De Marchi and Franchi, served the interests of the science and pharmaceutical power structure.

IS FALSIFICATION A COMMONLY ACCEPTED PRACTICE?

Prior to the advancement of the Appeals Board definition, many codes and guidelines of academic and professional ethics included "falsification" as an example of misconduct or fraud. For example, LaFollette (1992) noted that the National Science Foundation considers falsification to be a part of its definition of misconduct. Along a similar line, Windom (1988) observed that the Public Health Service's definition of misconduct includes "fabrication, falsification, plagiarism, and deception" (p. 1). In fact, the association of falsification with misconduct had become so strong that Chalmers (1990) wrote, "Scientific misconduct is commonly conceptualized as deliberate falsification of data" (p. 1405).

In the wake of the Felig-Soman scandal at Yale's Medical School, the president and fellows of Yale University approved a policy statement that included these ideas: "Academic fraud is more than error; it may take the form of falsification or fabrication of data, plagiarism. ... It is hardly possible to exaggerate the damage that can result from such a breach of the academic commitment to truth" (*Yale Weekly...*, 1982).

Similarly, at Harvard the discussion of the John Darsee case—which is by now mentioned often among case studies of academic fraud and misconduct—included references to falsification. In one instance, the case is discussed with the title "Medical School Dean Releases Report on Falsification of Research Data," and the text includes references to the "discovery and admission of data falsification by Dr. Darsee," and to "all investigations and deliberations dealing with falsification of results" (*Harvard University Gazette*, 1982, pp. 1, 12). If the definition of misconduct cannot encompass falsification, Darsee's falsifications might not have constituted misconduct. At the same time, it is obvious that many persons in the scientific community did not approve of how Darsee conducted his research.

At this point, it should be obvious that the term *falsification* is similar to *national security* in the sense that both are very elastic terms that can be defined and interpreted in different ways at different times by different persons and organizations. Invoking the term falsification can lead to rhetoric versus reality gaps, and to double standards relating to toleration of falsification on the part of scholars and organizations. The greater the toleration of falsification that exists, the greater the proclivity might be to attempt to silence scholars who detect, expose, and try to correct the falsifications.

In fact, in the Appeals Board definition, the reference to "commonly accepted practices" is something of a recognition of a double standard. If something is commonly accepted, it might be generally accepted but not necessarily universally accept-

ed by all scholars and institutions all of the time. Commonly accepted can easily be associated with commonly tolerated.

Falsification can be deliberate, or it can occur in an unwitting and unintentional manner. A false paradigm in history, for instance, that is believed to be true, amounts to a falsification of history. The persons who were involved in the original formulation of such falsification of history might have known that it was not true. Later generations of scholars who come across the falsified version might believe it is true, however, and include it in textbooks and classroom lectures. To the extent that scholars perpetuate a false paradigm of history believed to be true, they are engaging in a falsification of history, but they are doing so unwittingly, in good faith, and within the context of honest error. This type of falsification would be a component of paradigm dependency in academia.

Sometimes falsification is associated with intent to deceive, and sometimes it is regarded as honest error. A problem in such cases is that sometimes only the persons engaged in the falsification know for sure whether or not it was done deliberately. In such cases, a sense of democratic due process or fair play might result in giving someone the benefit of the doubt, based on the concept of being innocent until proven guilty. Until it is proven that falsification was deliberate, it might be considered, believed, or assumed that it was the result of honest error. Proof might be in the form of an outright admission, but short of such an admission, some doubt might linger. This sense of due process and fair play can therefore be a factor in the toleration of falsification, a factor viewed in a positive sense. In its broader definition, falsification can include fabrication of data, misrepresentation, plagiarism, obfuscation, prevarication, cover up, and so on. In one sense, any activity that actively obstructs knowledge of the truth might be considered part of a falsification process. Thus, secrecy and cover up can be components of falsification.

On the other hand, a relativist position that denies the existence of absolute truth might regard falsification as a relative, or even a fictitious, notion. If there is no truth, there cannot really be a falsification of the truth. A somewhat similar situation occurred in discussion of rules for the Olympic Games. For a long time, competition and participation were reserved for nonprofessional athletes. Then there was discussion of what really constituted a professional athlete and what really constituted violations in this regard. Finally, professional athletes were allowed to compete in some sports. At one point, someone commented to the effect that if there are no rules there can be no violations.

FALSIFICATION TREATED AS A SUBJECTIVE MATTER

Along a similar line, toleration of falsification can take place by regarding falsification as a subjective matter. Such a position is similar to a relativist one. What is considered to be a falsification by one person or institution might not be regarded as falsification by others. This was essentially the response of the president and the

Board of the College Art Association (CAA) in a letter written by CAA President Paul Arnold to Gordon Moran (personal communication, December 11, 1987; also known as The Arnold Letter). As the Guido Riccio controversy was unfolding and developing, certain practices were detected that seemed to be examples of misrepresentation, destruction of evidence, falsification, peer-review conflict of interest, and censorship. As a result, the CAA was asked to include these practices in its published ethics code as examples of unethical practices in art history. The Arnold Letter stated that the CAA "cannot" include such practices in the ethics code as examples of unethical conduct because the CAA was being asked to include "subjective matters," such as "fairness," and "respect."

In effect, the specific use of the terms *fairness* and *respect* in this case is itself an example of falsification, as in the specific requests to the CAA that led to the writing of The Arnold Letter, the CAA was not asked to include fairness and respect. Why these terms were brought up in this context remains something of a mystery.

In reply to The Arnold Letter, the CAA was asked to reconsider its position relating to unethical practices (falsification, etc.), and it was pointed out that the CAA had not been asked to include fairness and respect, and that inclusion of those terms in the letter written by Arnold amounted to a falsification. At a meeting of the CAA Board of Directors of April 16, 1988, the "board voted unanimously to reaffirm the position taken in Professor Arnold's letter" (G. Edelson, personal communication, May 8, 1988). After further correspondence and discussion on the specific subject, on August 9, 1988, it was stated that "the Board considers the matter closed and sees no reason to depart from its reaffirmance of the positions stated in the Arnold letter" (B. Hoffman, personal communication, August 9, 1988).

Because the CAA is a member of the ACLS, the President of ACLS, Stanley Katz, was asked (personal communication, February 26, 1988):

Do you agree with the CAA Board that our requests for practices to be included in the CAA ethical code involved "subjective matters"? In other words, do you believe, in your role as a scholar, and in your role as President of ACLS, that, in terms of academic ethics, practices such as false statements, falsification, misrepresentation, censorship, conflicts of interest ... represent examples of "subjective matters"?

Several years have passed, and still no answers to these specific inquiries have been received from Katz, or, for that matter, from any other officer of the ACLS.

By contrast, Lang, who had been following the case, wrote, with specific reference to the CAA response in The Arnold Letter, "I do wish the CAA and similar organizations took cognizance of the facts of obstructions, evasiveness, falsifications, misrepresentation, which occur on a much more widespread basis than is usually recognized" (Moran & Mallory, 1991a, p. 63).

The legalistic position regarding falsification taken by the HHS Appeals Board, and the subjective position taken by the CAA (with the apparent consent, based on silence in the face of inquiry, of the ACLS president), contrast with the academic and professional policies, codes and guidelines in which falsification is

considered an example of unethical behavior (even to the extent of being considered misconduct or fraud). Such strongly contrasting attitudes can lead to ambiguity. This ambiguity, in turn, can lead to arbitrary double standards, based on power, special interests, and vested interests, relating to whether falsification will be tolerated or punished.

A CASE AT HARVARD: DELIBERATE FALSIFICATION OR HONEST ERROR?

In *Le Bugie della Scienza* (the title of which in English would be *The Lies of Science*), Di Trocchio (1993) described a situation in scientific research in which falsification was both tolerated and punished during the development of the case. A summary of Di Trocchio's account is as follows. In March 1986, researchers led by Ellis Reinherz at Dana Farber Cancer Institute at Harvard University announced the discovery of Interleukin 4-A. Articles in *Science* and the *Journal of Experimental Medicine* gave details of the discovery. It turned out that the articles were erroneous, and a retraction was published in *Science*. Di Trocchio asked why the retraction took place so quickly, when in the Breuning and Baltimore cases it took a long time to ascertain that errors were serious enough to warrant a retraction.

Di Trocchio (1993) explained that the experiments that were crucial to the "discovery" were conducted by Claudio Milanese, a young Italian scholar sent to do research in the United States. In fact, Milanese was coauthor with Neil Richardson and Ellis Reinherz for one of the two articles, whereas the second article listed six coauthors. Before the retraction, two other articles on the subject were in the works for publication in *Science* and the *Proceedings of the National Academy of Sciences*.

At one point, according to Di Trocchio (1993), Reinherz held a press conference (to which Milanese was not invited). He also applied for a patent for the discovery and was negotiating a deal with a pharmaceutical company. Milanese was left out of the picture, and he returned to Italy.

However, then it turned out that further experiments were not working as expected, and Milanese was called back to the United States. Di Trocchio (1993) related that Milanese reminded Reinherz that even before the publication of the article in *Science* he had explained that the experiments were yielding some negative results. The experiments were repeated again, with negative results. Milanese returned to Italy once more. That left Reinherz with the problem of how to get out of the situation (i.e., how to make an about face and still save face).

According to Di Trocchio (1993), it was decided that because Milanese carried out the experiments, he must assume the blame for the failed (i.e., negative) results. Milanese was asked to sign what amounted to a confession. Milanese claimed he did not really know the cause of the errors involved. The head of Dana Farber appointed a committee to investigate the case. The committee concluded that Milanese was sole-

ly responsible. Letters were sent and phone calls were made to Italy, and Milanese lost his university research job and abandoned a career in academia.

In Di Trocchio's (1993) account of what happened to Milanese, it seems clear that a scientist was silenced on two occasions. First, at the press conference and in negotiations with a pharmaceutical company, Milanese was left out, even though his role as the conductor of the experiments would qualify him as a protagonist in these events. It would seem, in light of how things ended in this case, that the honorary coauthorships, and the credit for the discovery that went to Reinherz and others, would amount to a falsification of the scholarly record in terms of whose discovery it really was.

If scholars did not do the experiments, how could they be the coauthors who were the discoverers? Taking credit for the results of the experiments they did not do is a type of falsification that has become commonly accepted and tolerated in the form of so-called honorary authorship. To the extent that a scholar is not given due credit (at a press conference or in dealings with a drug company), the scholar is silenced.

Then, once the falsification (in the form of false and negative results of experiments) of the research was detected, the same scholar who was denied a part of his credit for the discovery was silenced by means of punishment meted out on the basis of intolerance for falsification. In this case, silencing took place in the form of abandoning a career in academia. So, on the one hand, falsification was tolerated, but, on the other hand, falsification resulted in punishment (although what might be considered falsification by the investigative committee in this case might also be considered honest error by the researcher who did the experiments).

There are some rather subtle aspects to the question of toleration of falsification in this case. In Di Trocchio's (1993) account, Milanese was quoted as saying that he did not have regrets about assuming most of the blame, but that he did regret that he did not prevent Reinherz from publishing the results that he (Milanese) knew were doubtful. In this specific instance, by remaining silent, Milanese was tolerant of potential falsifications that ended up being published in leading scholarly journals. At the same time, he allegedly had clearly explained, before publication of the articles, that the most recent experiments were not working according to the results obtained in the article submitted for publication. Therefore, the results about to be published were falsifications, or potential falsifications, in that they did not agree with the latest unpublished results. By not clarifying and confirming the results before publication (after being advised by Milanese that the latest experiments were showing some negative results), Reinherz would be, in these circumstances, showing a tolerance for falsification. Otherwise, he would have confirmed the results before publishing them. If confirmation could not be obtained, at least he could have decided not to submit the articles, or if he had already submitted them, he could have withdrawn them from publication, or perhaps he could have included a cautionary note along with the publication of the results. A silent withdrawing of the articles could have saved the embarrassment of a subsequent retraction letter.

SOME QUESTIONS ABOUT FALSIFICATION IN THE FELIG-SOMAN CASE

It seems that some double standards or ambiguities relating to toleration of falsification took place in the scandal at Yale involving Philip Felig. This case was reported in a two-part article in *Science*, with the first part entitled "Imbroglia at Yale (I): Emergence of a Fraud" (Broad, 1980). A summary of events reported in the article is as follows. A junior researcher at NIH, Helena Wachslicht-Rodbard, submitted an article to the *New England Journal of Medicine (NEJOM)*. Her supervisor, Jesse Roth, was a coauthor. An anonymous reviewer for *NEJOM*, Professor Philip Felig of Yale, recommended rejection. Before returning the paper with its negative recommendation to *NEJOM*, Felig had his associate, Vijay Soman, read and comment on it. Soman made a photocopy of the manuscript, which he used for an article of his own in the same area of research. Soman sent his manuscript to the *American Journal of Medicine*, where Soman's boss, Philip Felig, was an associate editor. Felig was also a coauthor of this article. The manuscript was sent out for peer review to Roth, who had his assistant, Rodbard, read it. She read it and spotted plagiarism, "complete with verbatim passages" (Broad, 1980, p. 39).

Rodbard sent a letter to *NEJOM* editor Arnold Relman, along with a photocopy of the Soman-Felig article. Relman was quoted as saying the plagiarism was "trivial," that it was "bad judgment" for Soman to have copied some of Rodbard's work, and that it was a "conflict of interest" for Soman and Felig to referee Rodbard's paper. (Broad, 1980, p. 39) Relman then called Felig, who said, according to Broad (1980), that peer-review judgment was based on the low quality of Rodbard's paper, and that the work on the Soman-Felig paper had been completed before Felig received the Rodbard manuscript. (Broad stated that this last statement by Felig was incorrect.)

Relman published the Rodbard paper, in revised form. Roth called Felig (a longtime friend from school days) and they met to discuss the two papers, for which they were either coauthors or reviewers. Broad (1980) stated that prior to the meeting "Felig had not compared the Soman manuscript to the Rodbard manuscript" (p. 39), even though Felig was coauthor of one article and referee for the other! When he returned to Yale, Felig questioned Soman, who admitted he used the Rodbard manuscript to write the Soman-Felig paper.

Broad (1980) reported that Rodbard and Roth began to express disagreement about the extent of plagiarism involved. Rodbard wrote to the Dean of Yale's School of Medicine, Robert Berliner, who did not believe all that she wrote. He was quoted as writing back to her, "I hope you will consider the matter closed" (p. 38). NIH apparently put off (by dragging their feet or by stonewalling) an investigation. A subsequent audit of the records revealed, according to Broad, "gross misrepresentation" (p. 41). Soman admitted that he falsified, but claimed it was not "significantly different from what went on elsewhere" (p. 41). After further investigations, at least 11 papers were retracted. Soman was asked to resign from Yale University, which he did. Felig became Chairman of Medicine at the Columbia College of Physicians and Surgeons.

From the standpoint of silencing of scholars as a result of toleration of falsification, the Rodbard article was given a referee rejection by Felig, who then became a coauthor of essentially the same article, published under different authorship. If Rodbard had not persisted in order to gain what she considered to be academic justice, at least part of her work would have been published under the name of other scholars, and her own article would not have been published (after the negative referee report). It seems that Rodbard left NIH. Soman left Yale, and some of his articles (some coauthored with Felig) were retracted.

A VARIETY OF CASES OF TOLERATION OF FALSIFICATION

Indications of toleration of falsification can be noticed in a variety of situations and settings within academia and on its fringes. Several years ago, Lang made a successful challenge to the nomination of Samuel Huntington for membership in the NAS. At a chance meeting in the small Italian hill town of Montepulciano (in the province of Siena), a mathematics professor from Princeton, Enrico Bombieri, was drawn into a conversation about Lang's successful challenge of Huntington. As part of his challenge, Lang cited what he and other scholars considered to be Huntington's improper use of mathematics, resulting in falsifications of history (Lang, 1988). Bombieri defended Huntington during the conversation, which also touched on other aspects of falsification in academia. Part of the conversation with Bombieri was reported to Lang (G. Moran, personal communication, October 2, 1990) as follows: "What struck me most about the conversation was Bombieri's apparent lack of concern about falsification in academia ... and his apparent contempt for persons who try to expose and correct (you, Dingell, etc.) falsification."

For someone studying the phenomenon of toleration of falsification, the compilation of the *35th Reunion Directory* (Class of 1960, Yale University) was a real eye-opener. The compiler and editor was William Boardman, assisted by Peter Parsons and Huntley Davenport. The directory informs the reader that "The University has provided technical services to assist ... in producing this directory" (p.ii). During the production, Boardman, a County Superior Court Assistant Judge in Vermont, sent a letter to his classmates, dated September 20, 1994, stating:

We were serious—we still are serious—about our *WARNING* to those who submit nothing: "you risk seeing something we made up appearing under your name." The editorial staff is determined that something shall be said about everyone, regardless of truth, decency. ... Of course we will strive to avoid the libelous, but not necessarily the false, the misleading, the scurrilous, or the innuendo-laden.

As mentioned earlier, one of the high points in the Guido Riccio controversy was the 1985 conference held in Siena. This conference was reviewed by Gardner (1989) in *Burlington Magazine*. His review included a description that amounted to a clamorous falsification of the events that actually took place. Gardner wrote:

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Few can have attended a conference where the presence of the police was so apparent, and the presence of television and press so intrusive. *Frondeurs* simultaneously staged a lively counter-conference. The reason for this unwonted excitement was, of course, the debate over the authenticity of the Guidoriccio da Fogliano. (p. 489)

A journalist in Siena wrote that Gardner reported mendaciously, and another Siense, Giorgio Sacchi, asked, rather sarcastically, how many *Frondeurs* were involved, where was the counter-conference held, and where were all the police located (Mallory & Moran, 1989, p. 24)? The fact is that there was no counter-conference (much less a simultaneous and lively one), no *Frondeurs*, and no apparent presence of police.

After this false account was published, the editor of *Burlington Magazine*, Caroline Elam, was asked to make a published correction. In a rejection letter, she refused to do so (personal communication, October 20, 1989). In this case, the falsifications were tolerated and allowed to be perpetuated, and, at the same time, the true version of events was silenced.

Several scholars have commented, in one way or another, about the tendency toward toleration of falsification in academia. Here are some examples. Hillman (1995), in "Honest Research," stated, "there are a large number of improper practices which are tolerated and accepted by the academic community, but hardly ever spoken about, and are often regarded as behavior acceptable to the research community ... they are so widespread and sometimes secret and not admitted" (pp. 56-57). Martin (1992) made similar observations in "Scientific Fraud and the Power Structure of Science": "A host of things go on in scientific research that could be open to suspicion. Some of these are accepted as standard practice, others are tolerated. ... Some individuals have tried to raise concern about these practices, but for the most part they are tolerated" (p. 84). Martin then described several specific case studies in which plagiarism and other falsifications were tolerated at some Australian universities.

In the view of DeFelice (1991), the widespread practice of "unearned authorship" (sometimes referred to as honorary authorship) itself creates a situation in which "researchers routinely accept a certain level of dishonesty and therefore defend larger transgressions that involve the same vice" (p. 104). He then elaborated:

By accepting or insisting upon unearned authorship, much of the scientific community has forfeited the right to bear witness. Thus when investigations reveal unbecoming conduct that involves the same crime, scientists close their ranks, because many are guilty of far less spectacular but similar infractions. (p. 104)

Hillman (1997) classified honorary or unearned authorship as "parafraud" (p. 126).

DeFelice's use of the words *routinely accept* finds something of an echo, if not at least a partial confirmation, in the sworn congressional testimony of Margot O'Toole during the *Cell-Baltimore* case:

Dr. Huber called me and told me that there was no doubt I was right scientifically.

However, she and Dr. Wortis were convinced that there was no fraudulent intent. She said a correction would have a devastating effect on Dr. Imanishi-Kari's career. They had therefore decided that no correction would be submitted. I was shocked. I said the paper had to be corrected because others were relying on it. Dr. Huber replied that there were so many faulty papers in the literature, that one more did not matter. (Lang, 1993, p. 7)

COVER UP AND TOLERATION OF FALSIFICATION

Another aspect of toleration of falsification is known as *cover up*, generally understood to mean the hiding of evidence of scholarly error or wrongdoing of one form or another. If falsification were openly tolerated, there would be no need to cover up the fact that falsification had been perpetrated and detected. If falsification were not tolerated, it would be punished, instead of having evidence of its existence covered up. It would seem, therefore, that toleration of falsification tends to exist in covert form, rather than openly and publicly. It might be widely tolerated, but it is usually not admitted publicly that it is widely tolerated.

Articles in *The Scientist* several years ago give firsthand accounts, told by Sprague (1987), Hollis (1987), and Jacobstein (1987), of cover-up activity at University of Pittsburgh, Case Western Reserve University, and Cornell University. In each case, these scholars tried to expose and correct serious errors. All three cases reveal similar patterns of institutional resistance to exposing and correcting the errors, and institutional retaliation against the scholars who tried to have the errors corrected.

In such cases, retaliation becomes a component of the phenomenon of toleration of falsification. A newspaper article describing the University of Pittsburgh case is entitled "Study Finds Reluctance to Expose Scientific Fraud." This article (Raeburn, 1987) discusses the same themes of resistance to correction of error, and of retaliation against scholars who try to correct error: "There is a generalized reluctance to take prompt corrective action in response to faulty, misleading, or fraudulent data ... 'Researchers may fear that if they raise such questions, they themselves will suffer.' They might be viewed by colleagues as troublemakers" (p. A-7).

There is a somewhat ambiguous or double-edged relation between cover up and toleration of falsification. The exposure of a false paradigm is embarrassing and uncomfortable to experts and specialists in a field of study, because as Schneider (1989) pointed out, "If the knowledge expounded by recognized scholars to their students should prove to be of dubious reliability, then their authority is open to question" (p. 137). As discussed earlier, much rhetoric states that a primary mission of the university is the pursuit of truth. The exposure of the perpetuation of clamorous falsifications (as in the case of false paradigms), or of massive falsifications of data in scientific experiments, would indicate failures in the pursuit of truth or commitment to truth, and might place the university's commitment to truth in question, just as unreliable knowledge propounded by an expert places the expert's authority in question. Cover up is prompted, at least in part, by an attempt to hide the fact that

serious falsifications have been taking place. On the one hand, by covering up—rather than punishing or correcting—the falsifications, it would seem that falsification is being tolerated. On the other hand, cover up is sometimes—if not often—resorted to in an attempt to give the impression that falsification is not tolerated and has not taken place. However, by hiding and silencing evidence of deliberate falsification, the institution involved ends up perpetuating the falsifications for some time, and, thus, tolerates them (at least for the duration of the time they are perpetuated as a result of the cover up).

It seems that cover up is, therefore, part of a cycle involving toleration of falsification. The rhetoric states that truth is pursued and deliberate falsification is not tolerated. However, unearned authorship, and allowing faulty papers to abound in the literature of a discipline without correction are examples of some types of falsification that are tolerated. But such toleration is usually not openly acknowledged beyond the circles of the persons who are falsifying or tolerating the falsification.

Thus, toleration of falsification is not officially approved, but the approval exists unofficially. Then, scholars such as Sprague, Hollis, Jacobstein, Stewart, Feder, O'Toole, and Lang try to expose and correct the falsifications they have detected (without necessarily knowing at the time if the falsifications came about unwittingly or deliberately). At this point, the authorities involved have basically two choices: (a) investigate, and if falsifications have taken place, admit it and make corrections, and (b) cover up. Sometimes, when evidence of falsification is too strong to hide or deny, or if it has already become too widely known to be covered up effectively, authorities might try to justify falsification, either on the basis that it is a subjective matter or that it is a commonly accepted practice. Such justifications are usually not widely promulgated, however. In fact, after Lang's negative criticisms of *The Arnold Letter* appeared in a scholarly journal (Moran & Mallory, 1991a) the CAA revised its published ethics code (*CAA News*, 1996). Instead of considering misrepresentation to be a subjective matter that the CAA could not include in its ethics code as an example of an unethical practice (which is, in effect, the position the CAA leadership took in the *Arnold Letter*), the new ethics code states that "Art historians ... must guard against misrepresenting evidence" (College Art Association, 1995, p. 1). At this point, misrepresentation is not officially tolerated. In one sense, a cycle has been completed. (It is not known if there is a direct connection between the appearance of Lang's remarks in a publication and the CAA's revision of its ethics code. The revisions might have been formulated before Lang's remarks appeared in print.)

Swan (1994) illustrated another situation in which the CAA showed tolerance for falsification (in the form of tolerance, by means of silence, for a specific case of blatant plagiarism of an art historian's work). Swan wrote, "there is such a thing as unmistakable verbal theft ... this clear-cut case of plagiarism resulted in no official action or public notice, despite the efforts of the victim of the larceny. ... The borrowing is unacknowledged, and it is verbatim and blatant" (p. 45–46). Swan then related that "a letter of complaint" was written "to the College Art Association," and that "no action was

taken" (p. 46). He mentioned further that "the CAA was arguably one relevant body to receive the complaint ... the rest was silence" (pp. 45–46).

A subtle form of toleration for falsification can occur when a professional society or association publishes ethics codes or guidelines, and then claims that it cannot enforce the codes, which serve as guidelines rather than laws. In fact, it seems that some organizations that issue ethical codes and guidelines do not publish, or do not have in place, mechanisms to deal with alleged violations that are brought to their attention.

When attempts to justify falsification are made, these attempts might indicate that academic discussion has escalated beyond the confines of the type of discussions held by the Fellows of Pierson College (as described in the Introduction), and of collegial and academic debate elsewhere in academia. At a certain point, debate becomes controversy and can escalate further to various levels of scandal. It is at the levels of controversy and scandal that attempts might be made to justify falsifications of one type or another. It is during the developments of controversies and scandals that some of the most strenuous efforts to silence scholars might take place.