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Double Standards and Peer Review Suppression

Ruesch (1989) compiled many scholarly opinions and observations relating to animal experimentation in medical research, including the following:

- "The experiments performed on animals in order to determine the effects of medicaments offer a very insecure basis for drawing conclusions as to the effects on humans ... 1879" (p. 251).
- "Medicants do not function the same way in humans as in animals ... they can not possibly be dosed appropriately for such a function ... 1891" (p. 246).
- "The effect of drugs upon animals is so entirely different from their effect upon man that no safe conclusions can be drawn from such investigations ... 1895" (p. 242).
- "An experiment on an animal gives no certain indication of the result of the same experiment on a human being. 1906" (p. 216).
- "The attempts to establish the effectiveness of antitoxins on humans by means of animals are frankly ludicrous ... 1926" (p. 183).
- "My own conviction is that the study of human physiology by way of experiment on animals is the most grotesque and fantastic error ever committed in the whole range of human intellectual activity ... 1933" (p. 151).
- "We must face the fact that the most careful tests of a new drug's effect on animals

may tell us little of its effects on humans. 1962" (p. 106).
- "The animal and human organs show striking differences in their sensitivity to chemical combinations ... 1978" (p. 80).
- "A drug that is tested on animals will have a completely different effect in man. 1985" (p. 62).
- "There are things that work in mice that do not work in people. 1986" (p. 47).

More recently, Lang (1995) came across a similar opinion that was expressed in 1994: "Dosing mice chronically with compounds does not prove that the same conditions will be found during 'normal' human usage" (p. 18). In an editorial announcing a forthcoming conference on peer review in Prague in 1997, Rennie and Flanagin (1995) wrote that "the only true peer review is the universal assessment that work receives after publication" (p. 987). There has been a continual assessment for more than a century, as the items cited at the beginning of the chapter illustrate, that animal experiments used in testing drugs for medical research are misleading, unreliable, and therefore also dangerous. (The thalidomide disaster, for instance, serves as a reminder.) Ruesch (1989) pointed out that Irwin Bross, former head of research at Sloan-Kettering Institute (one of the world's leading cancer research centers), came to believe that animal experimentation in cancer research was "worse than useless" because it was "consistently misleading" (p. 24). Bross (1994) elaborated on the theme of such unproductive research in *Fifty Years of Folly and Fraud "in the Name of Science."* Henry Bigelow of Harvard University was quoted as going so far as to say, "A day will come when the world will look upon today's vivisection in the name of science the way we look today upon witch hunts in the name of religion" (Ruesch, 1978/1991, unnumbered). Despite such extensive, long-term, negative assessment, animal experimentation for the testing of drugs and for other tests has been a paradigm in medical research for a long time (Ruesch, 1992).

DOUBLE STANDARD FOR REJECTION OF A RESEARCH PROPOSAL

Perhaps there is no more effective and revealing way to document double standards in peer review than to discuss peer review that involves animal experimentation in the testing of drugs. The author of the opinion from 1994 listed earlier that reads "dosing mice chronically with compounds does not prove that the same conditions will be found during 'normal' human usage" is none other than an establishment scholar recommending that Duesberg's proposal for a research grant be rejected. In the midst of this negative judgment—a judgment that repeats many similar opinions of the past about the unreliability of animal experiments—grants for large sums have been awarded often and consistently, both before and after 1994, for drug-testing experiments using mice and other animals.

In this case, the double standard used to deny Duesberg a grant went even further, as Lang (1995) pointed out: "The objection states: 'Twenty-four months is a very large part of the total life span of laboratory mice. Many mice might not survive the experiment'" (p. 17). In the face of this objection, Lang countered that "cancer studies on mice which routinely study mice for periods up to 24 months are routinely funded" (p. 17). In effect, Duesberg's proposal in this case involved research into a form of cancer.

Not long after Duesberg's request for funding was rejected, it was reported that the Food and Drug Administration (FDA) approved a new drug used to combat obesity. Approval was given despite "side effects seen in animal studies." Some experts mentioned "studies in which high doses of the drug caused brain damage in laboratory animals." Then, the comment was made that "there is no proof that people are similarly affected," and it is also reported that the drug has been in use in Europe and that none of the side effects found in the animals have been reported in persons living there who used the drug (Hellmich, 1996, pp. 1A–2A).

Here are two peer-review situations. In one, a negative judgment of Duesberg's research proposal is based on the statement that "dosing mice . . . with compounds does not prove that the same conditions will be found in 'normal' human usage." In the other, peer-review approval is given amidst statements to the effect that "there is no proof that people are similarly affected" (i.e., affected with brain damage and perhaps other side effects), and that in Europe the people who have taken the drug so far have not been reported to have suffered the same effects as the animals have. Thus, in one case, rejection is recommended because humans and animals might not react in the same way to a drug, and in the other case approval is given in the very hope that humans and animals will not react in the same way to a drug. Why was there rejection in one case, but not in the other? If research in one case was accepted, why was the research proposal not likewise accepted in the other case?

DOUBLE STANDARDS IN PEER REVIEW IN THE THALIDOMIDE CASE

The thalidomide tragedy of a few decades ago provides examples of more double standards. The drug, produced by Chemie Grunenthal and hailed as something of a wonder drug after years of experiments with animals in which no birth defects were reported, caused about 10,000 babies to be born with horrible birth defects. Apparently there were also serious nervous system disorders among the women who took the drug. Ruesch (1978/1991) reported that animals used included "dogs, cats, mice, rats, and as many as 150 different strains and substrains of rabbits, with negative results. Only when the white New Zealand rabbit was tested, a few malformed rabbits were obtained" (p. 361). Ruesch described further what took place:

> In December 1970, the longest criminal trial in Germany's judicial history … ended with the acquittal of Chemie Grunenthal, *after a long line of medical authorities had tes-*

tified that the generally accepted animal tests could never be conclusive for human beings. This was unprecedented, for the testimonies came from an impressive array of individuals whose careers and reputations were practically built on animal experimentation, including the 1945 Nobel laureate biochemist Ernst Boris Chain. ... Even Prof Widukind Lenz ... testified at the trial that "there is no animal test capable of indicating beforehand that human beings, subjected to similar experimental conditions, will react in identical or similar fashion." (pp. 361–362)

Despite the details of this tragic thalidomide story, which comprise a sad chapter in the history of medicine, in 1988, Varaut (1990) presented a paper at a conference that included these ideas:

> The tragic thalidomide case, which is still cited, would never have happened if the drug had been administered to other species of animals than rats, which unfortunately were not affected by this product. Thalidomide is the best example of the absolute necessity for some experimentation on animals. (p. 36)

The implicit double standard for peer review in Varaut's (1990) statement becomes obvious with his reference to "other species of animals." The fact is that, as Ruesch (1978/1991) pointed out, many animals were tested for thalidomide (with negative results), whereas in other cases one type of animal might be used: monkeys, rats, dogs, mice, and so on (but not necessarily all in the testing for the same drug). Besides, if all drugs were tested on all species of animals, most likely at least one animal would show severe side effects for large doses of the drug being tested. It is obvious that not all species are tested for each drug. So where would Varaut, or anyone else, draw the line regarding the total number of species used before experiments with a drug are terminated?

The difficulty with Varaut's logic is illustrated by what happened in the case of the drug diethylstilboestrol (DES). Ruesch (1992) described the situation:

> DES ... was developed in 1939, tested without adverse effects on animals for years, but then it was suddenly discovered to have caused cancer in girls whose mothers had been prescribed this 'miracle drug' by their doctors during pregnancy. ... After DES had turned out to be the first drug that the medical confraternity itself had recognized as being responsible for creating a new type of cancer in human beings, animal tests with DES were started all over again, and again with no results; the test animals did not develop cancer. (p. 22)

Peer-review double standards for animal experimentation can have disastrous effects. In the case of the disease subacute myelo-optico-neuropathy (SMON), Inuoe claimed that a virus he discovered caused the disease (which had grown to epidemic proportions). Duesberg (1996b) stated that Inuoe "insisted he had caused SMON-like symptoms in mice ... either by injecting the virus into their brains or feeding the virus to other immune-suppressed mice" (p. 25). This type of research and the peer-review acceptance it receives, in the face of other peer-review judgments that point out that results of animal experiments are unreliable when applied to humans, tended to put scholars off the track in relation to the real cause, which turned out to be the "miracle" drug Oxychinol that Ciba-Geigy had developed. Regarding this situation, Ruesch (1992) noted:

> At least a thousand deaths had to be counted in Japan and 30,000 cases of blindness and/or paralysis of the lower limbs before it was realized that heretofore unexplained similar cases of death, blindness, and paralysis in Holland, Denmark, Germany, France, Great Britain, Belgium, Italy, Sweden, etc. had also been caused by Oxychinol-containing drugs. (p. 20)

In this case, Ruesch (1992) related that according to the studies of Hansson, the researchers discovered (but kept secret) severe side effects on the experimental animals "who were seized by violent convulsions and respiratory difficulties as soon as they were made to swallow Oxychinol" (p. 20). The drug was marketed anyway, with a warning, as Ruesch noted, not to give it to "*house pets*." (p. 20) He cites this case as evidence "that the researchers themselves do not believe in the validity of animal tests in respect to human beings" (p. 21).

A double standard in peer review is based on an oscillating viewpoint, as the situation suits the peer-review authorities. On the one hand, results of animal experiments are not applicable to humans (as in the rejection of Duesberg's research grant proposal, and in extensive expert testimony during the thalidomide–Chemie Grunenthal trial in Germany). On the other hand, animals are valid models for humans in drug testing (as evidenced by the large number of peer-review acceptances for grants and publications of such studies using animals as models for humans). It is possible to accept or reject a grant proposal or a manuscript by suddenly and arbitrarily changing the rules (or the criteria, as the case may be).

IDENTIFYING DOUBLE STANDARDS

Distinctions should be made among different standards, different criteria, and double standards. Some journals may have more stringent, stricter (higher) criteria for acceptance than other journals have. Some referees for the same journal may be generally considered harsh and strict in comparison with other referees who are considered more lenient in their judgment. These are subjective differences, but not necessarily double standards. Criteria for hiring, promotion, and tenure at some universities are more stringent than at others. These are different standards, but not necessarily double standards. By contrast, double standards usually involve a sudden, arbitrary shift in judgment or policy that is contrary to usual policy.

The subjective nature involved in different standards and criteria sometimes makes it difficult to identify a double standard. The distinction might be blurred and unclear. The famous study published by Peters and Ceci (1982) can be regarded as a classic case of double standards in peer review. Dalton (1995) summarized what took place:

> The authors took 12 published research articles by investigators from prestigious and highly productive American psychological departments in 12 different highly regarded American psychological journals. ... After replacing the authors' original names and institutions with fictitious names and affiliations that had neither reality nor prestige, they resubmitted the articles to the same journals. ... Only 3 of the 12 articles were recognized. The remaining nine proceeded through the refereeing process ... eight of the nine were rejected. Sixteen of the eighteen referees recommended against publication, often on the grounds of serious methodological flaws. (p. 215)

It is obvious that the eight rejected articles were treated completely differently the second time around. If they had the same referees both times, it was obviously a case of double standards. However, the fact that only 3 of the 12 articles were recognized indicates a certain laxity, or a certain policy, on the part of the editors, who seem to have glanced at the papers (rather than read them carefully) and then relied heavily on referee judgments.

It is possible that different referees have sincerely different ideas and standards about methodology, and the embarrassing rejections of the same articles reflect these differences. On the other hand, if the referees and the editors rejected the articles because they were too long or too short in length, or because the format, genre, or style were declared inappropriate and unacceptable, then obvious double standards have been used.

One referee might have actually detected methodological flaws and inaccuracies of various types that slipped by another referee. In that case, sloppy evaluation, rather than deliberate double standards, might have caused the embarrassing discrepancies in peer-review decisions. If this were the case, the rejection recommendation would be the equivalent of a rebuttal article or a letter to the editor pointing out defects in the published article. (For this reason, among others, peer review should not be secret.)

DOUBLE STANDARDS AND BIAS

The issue is further blurred by the concept of *bias*, which is often identified with double standards. Referees who are usually somewhat lenient might become quite harsh in judgment of scholars or ideas they do not like. Or, on the contrary, harsh referees might suddenly become lenient if the referees' personal or professional vested interests are better served by leniency in specific cases. However, the mixture of subjective factors and critical evaluation often makes precise distinctions difficult. For example, a manuscript that was rejected by referees and editors who had a strong bias against the author of the manuscript might also have been rejected by referees and editors who had no bias at all against the author. (As long as referees' identities remain secret, and reasons for rejection remain secret, some clear cases of double standards might escape detection.)

In any case, many scholars seem to feel that they and their ideas are being silenced because of bias against them. Some years ago, the Office of Scholarly

Communication of the American Council of Learned Societies (ACLS) conducted a survey of 5,385 scholars, of whom about 71% responded. Morton and Price (1989) discussed the results, including views about bias:

> About three out of four respondents think the editorial peer review system is biased. ... About 40% think bias is so prevalent in their disciplines that it merits reform. ... The question is, therefore, not whether bias exists in the peer review system, but whether it is prevalent and whether it systematically interferes with the free exchange of information and ideas by discriminating against particular subjects, opinions, and classes of authors. ... The survey shows that suspicions of bias appear to be held by scholars in all types of universities and among all the disciplines sampled ... the unease is pervasive, not an occasional outcropping of discontent. (pp. 7–9)

EXCUSES AND "DIRTY TRICKS"

In addition to being reflections of bias against specific persons and their ideas, double standards in peer review can have other characteristics. Sometimes they are excuses rather than reasons for rejection. A good example is rejection because of length when articles recently published in the same journal were longer and shorter than the rejected article. Another example involves "catch 22" type reasons, such as, on the one hand, rejection for not citing and discussing other publications on the subject, and, on the other hand, rejection for repeating information and ideas that have already been published elsewhere.

In one way or another, double standards might be considered "dirty tricks." Remus (1980) stated that one such trick is to criticize the work negatively "for vices it does not have" (p. 89), and then recommend rejection based on the alleged vices, or defects, as the case may be. It seems something of a dirty trick to recommend rejection of a grant proposal on the basis that animals may not react to drugs the same way humans do at the same time that grants for such animal experimentation are, in fact, an integral part of the research paradigm. (On the other hand, rather than considering the rejection opinion in this case to be a peer-review dirty trick, some scholars might consider it instead to be a forthright peer-review admission—if not confirmation—that the prevailing paradigm is a false one.)

"Almost everyone who has ever submitted anything to a journal has a horror story or two to tell." If this harsh judgment by Leslie (1989, p. 125) is true, it would suggest that double standards abound in the peer-review process. A close scrutiny of referees' files in the editorial offices of journals might give some hint as to what extent Leslie's views are accurate, but Campanario (1995) found that an attempt to engage in such scrutiny "encounters resistance from most editors, even if it is possible to keep referees' names anonymous" (p. 320). In his discussion of peer-review rejections by *Science* and *Nature*, Campanario referred to a double-standard incident at *Nature* in which "one of the reviewers tried to change the requirements he had laid down for acceptance in the first place" (p. 313).

DOUBLE STANDARDS FOR EXCLUSION AND INCLUSION

Such an attempt to "change the requirements" might be considered a form of double standard by means of "changing the rules as you go along." Something quite similar to this situation took place at the editorial offices at *Art Bulletin*, the major publication of the College Art Association, which is is member organization of the ACLS. An article was submitted by Donna Baker for publication. A main part of the subject matter dealt with the Mappamondo, a large map that was painted in the Palazzo Publico in Siena, Italy, where the Guido Riccio fresco is also located. In this article, the author disagreed quite strongly with the establishment point of view on some issues that involved the Mappamondo and its relation to the Guido Riccio controversy. The editor, Richard Brilliant, rejected the article. In his rejection letter to Baker of May 19, 1991, Brilliant stated that he read the article "several times" (there is no indication that he ever sent it to a referee before rejecting it).

In the editor's view, Baker made "allegations" and, according to the editor, such allegations "would require the *Bulletin's* provision of an opportunity to be heard to all the contestants." Provision for this opportunity was based on "fairness,"according to Brilliant. He then expressed reservations about presenting the views of all the contestants because discussion "could well be endless." The article was rejected and that seemed to be the end of the matter.

Then, in the June 1996 issue of *Art Bulletin*, a long article appeared by a different author (Kupfer, 1996) on the same subject (i.e., the Mappamondo in the Siena Palazzo Publico). The hypotheses and conclusions were in sharp disagreement with those of the article that had been rejected. Besides, in the published article the following allegations appear: "But holding the *Mappamondo* hostage to the *Guidoriccio* ... will not help advance understanding of either work ... a desperate attempt at obfuscation for the sake of sustaining an attack on the traditional attribution of the *Guidoriccio* ... interpretation of the ambiguously worded inventory seems gratuitous" (pp. 288–289).

The double standard in this case should be easy for all to grasp. One article was rejected because, in the name of "fairness" all sides of the controversial issue should be heard, particularly because the article contained "allegations." But discussion might be "endless" if all sides are heard. Under these terms, publication was dependent on all sides being heard, not just one side, but that was not practical for reasons of space.

After the article was rejected in this manner, another article on the same subject was published in the same journal. The published article disagreed with the contents of the rejected article, and similar to the rejected article, the published article contained allegations. Apparently the "fairness" reference did not apply to scholars against whom the published allegations were made, or to scholars who disagreed with the contents of the published article. This seems to be a rather ironic double standard case, in which the fairness concept is invoked in a manner that results in the establishment view being published and the dissenting view being silenced and shut out from the pages of the journal. (Usually, fairness is invoked to allow a point of view to be heard, not to suppress it.)

Another glaring example of peer-review double standards regarding inclusion and exclusion took place during the Guido Riccio controversy in 1985, when a conference devoted to the art of Simone Martini was held in Siena. At that time, the controversy was in full swing. Some new evidence that further contested the official attribution came to light, and a request was made to the Organizing Committee (Comitato Scientifico) to be allowed to present the new material in a paper as part of the program of the conference. The request was denied. Inquiries were made to seek the reason for rejection.

What followed is described in *Confronting the Experts*:

> The reason for our rejection was explained by Professor Bellosi ... of the Organizing Committee ... to another member of the Organizing Committee, Professor Miklos Boskovits. ... According to Boskovits, Bellosi stated that by then scholars knew where each side stood on the issue of Guido Riccio, that the subject had been worked over in detail recently, and that there should be a pause for reflection. Boskovits said he agreed with the reasoning behind the decision to exclude us from the program. (Mallory & Moran, 1996, p. 144)

Up to this point, this decision resembled the *Art Bulletin* rejection of the Mappamondo article, in the sense that no discussion on the subject from any side or point of view would be held. So far, no discussion at all, and therefore no double standard, but then the following took place: "After having kept us off the program because Guido Riccio was not to be discussed, the ... Committee included one of their own members ... Torriti, on the program to give a long talk on the Guido Riccio situation in which he attempted to refute our views" (Mallory & Moran, 1996, p. 144).

Such a situation is not an isolated case in the attempt to silence scholars. Velikovsky faced similar treatment after he came forth with evidence and theories that upset authorities and experts: "The ... settings provided for the discussion ... were mostly arranged ... by hostile critics or intimidated moderators. He was excluded from discussion of his own work and his works were not subsequently published" (de Grazia, 1978, p. 173).

THE RIGHT TO REPLY

The right to reply seems to be a major problem involving double standards in peer review.

According to the rhetoric of academia, all scholars have the right to reply, because there is free and open discussion and debate in which all points of view are given a hearing. In reality, however, apparently some scholars have a right to reply, and others do not. As far as can be determined, this right is determined by arbitrary editorial decision. Apparently from a legal standpoint, in many cases at least, editors have the legal power to decide what will be excluded from their journals. In this legal sense, scholars do not have a right to publish a reply in the pages of a journal. Instead, they are granted the opportunity (the privilege, honor, or however it may be described) by the editor. On the other hand, when an editorial official or a scholar refers to a right to reply, the reference is usually to an ethical or moral right based on the rhetoric about free and open discussion and debate, or else based on the ethical concept of fair play, in which scholars who have been verbally "attacked" in the pages of a journal have the right to defend themselves, rebut, explain, and so on.

Perhaps one of the most revealing cases of double standards involving right to reply took place with *Burlington Magazine* and the manner in which it dealt with aspects of the Guido Riccio controversy. In response to an article and also to an editorial published in that source, a letter to the editor was published. It begins, "Sir, this is not the appropriate place for us to discuss the evidence presented in Professor Andrew Martindale's article on the Guido *Riccio* controversy" (Mallory & Moran, 1987, p. 187). In the original submission, the letter began, "Letters to the Editor are customarily brief and therefore not the appropriate place for us to discuss the many ambiguities, discrepancies, and errors that we feel we have detected so far in Professor Andrew Martindale's article," but the text was changed based on the suggestion of the editor in a letter to the authors (N. MacGregor, personal communication, July 2, 1986). The specific intention of this opening to the letter in its original text was to alert scholars who had read Martindale's article that errors had been detected in his article and that these alleged errors would be discussed in publications planned for the future.

The editor, Neil MacGregor, was receptive to the publication of the letter, but he stated in his letter that he considered the reference to "many ambiguities, discrepancies, and errors" as being "unnecessarily discourteous." In this case, an attempt to alert scholars to error was silenced, but the silence would be broken in planned future publications, and there did not seem to be the same urgency as there might have been if the errors had involved medical research, atomic energy, or political science dealing with sensitive issues that might lead to war.

The published letter provoked a reply in the form of a letter to the editor by Piero Torriti, published in the July 1989 issue of *Burlington Magazine* (p. 485). The letter begins, "I should like to take the opportunity to refute once and for all the absurd and defamatory accusations ... contained in the letter published ... in the March 1987 issue" (by the time this letter was published, Caroline Elam had replaced MacGregor as editor). A case could already be made that an obvious double standard has been used, as charges of having made "absurd and defamatory accusations" would seem to be more "discourteous" than charges of "ambiguities, discrepancies, and errors." On the other hand, maybe it was a case of different standards of different editors, rather than a double standard per se.

In any case, a reply seemed warranted to rebut the charges (deemed completely false) of having made "absurd and defamatory accusations." By simple logic, these charges would be false. If an editor would not allow references in a letter to "ambiguities, discrepancies, and errors" because they were "unnecessarily discourteous," would the same editor allow the publication of absurd and defamatory accusations, which are much more discourteous? Besides, publishing "defamatory" material would have placed the journal itself, and those persons who have statutory responsibility for the journal's contents, in potential legal trouble. Therefore, it was unlikely that the editor would have allowed "absurd and defamatory accusations" to be published.

In a desire to rebut the untrue and unfounded charges that absurd and defamatory accusations had been made and published, a letter to the editor was submitted. However, the editor, Caroline Elam, rejected it, stating that she considered *Burlington Magazine's* discussion on the subject closed. Obviously, the editor had the legal power to deny publication of the letter, so the right to reply was not a legal issue. Instead, the question was placed within the framework of fair play; that is, scholars who have been falsely accused in print should have the opportunity to defend themselves and to set the record straight.

Over the course of about a year, Elam wrote several rejection letters, despite appeals to fair play. In the face of the rejections, the appeal was taken to other members of *Burlington Magazine*'s leadership, including Sir Brinsely Ford, a trustee, who supported Elam by responding quickly (personal communication, October 23, 1989) with, "You have made accusations to which Professor Torriti had the right to reply, and that, in my opinion, should be the end of the matter as far as the *Burlington* is concerned" (Mallory & Moran, 1996, p. 148).

In Sir Brinsely's response, the issue of right to reply was brought up directly. Torriti had the right to reply but that "should be the end of the matter," even though Torriti made accusations. In this case, the double standard is undeniable. On what basis (legal, fair play, or otherwise) would Torriti have a right to reply to other scholars, but no other scholar would have the right to reply to Torriti, except the basis of power that arbitrarily decides to employ a double standard? By use of power and double standards in peer review, scholars can be silenced. (As it turned out, MacGregor became Chairman of the Board, and at a Board meeting, Elam's rejection decisions were reversed and a reply to Torriti was allowed, and eventually published in the January 1991 issue.)

In addition to taking the appeal, based on fair play within the *Burlington* leadership, various scholars outside of art history who were specialists in scholarly communication, were consulted, and their opinions were requested. One of these scholars is Ralph Eubanks of the University of West Florida. Eubanks (personal communication, February 6, 1990) stated:

> I do indeed believe that scholars who have been charged with having made a "defamatory" statement should be given the chance to reply to such a charge in the pages of the scholarly journal. ... Simply in the interest of justice, one should be allowed the opportunity to refute this kind of allegation.

In the same letter, Eubanks recalled a situation he was confronted with when he was editor of a scholarly journal. In a book review that was to appear in the book review section, the reviewer charged that the author plagiarized. Eubanks gave the author the chance to publish a rebuttal alongside the review itself. In explaining his editorial position, Eubanks affirmed that he "did not regard this editorial decision as an extraordinary one." Instead, he "thought of it as one *dictated* by the standard of fairness." (As far as known, the February 6, 1990 letter by Eubanks has not been published.)

Perhaps the majority, or even the vast majority, of scholarly journals follow the standards of fairness to which Eubanks adhered. The concept of fairness and justice in scholarly communication becomes a component of the rhetoric relating to open discussion, debate, and free exchange of ideas. In light of such standards, the questioning of a scholar's right to reply, statements that a scholar does not have the right to reply, or opinions to the effect that one scholar has the right to reply and "that should be the end of it," should lead to suspicion that the concept of right to reply is being invoked in order to silence scholars. Such invocation usually involves, by its very nature, a double standard.

Velikovsky had difficulty, on several occasions, getting his rebuttals into the scholarly literature. It seems that on one occasion "*The Proceedings of The American Philosophical Society,* which in 1952 carried extensive attacks upon him, would not suffer his reply" (de Grazia, 1978, p. 179). In this regard, Duesberg, with his dissident views on AIDS research, was in the same situation as Velikovsky as far as being refused the right to reply in establishment publications.

Among Lang's file studies, there is one called the "Journalistic Suppression and Manipulation File." In an item from February 19,1996, Lang wrote, "I have documented the way information and certain points of view are currently suppressed by the quartet constituted by *Science*, *Chemical and Engineering News*, the *Lancet*, and the *New York Times*. *Nature* is in a class of its own." (There are plans to have some of Lang's recent file studies published in the near future.) Within the context of these instances of suppression, the right to reply is a specific issue. For instance, in a letter dated January 15, 1996, to Duesberg, the editor of *Chemical & Engineering News* (*C & EN*), Madeleine Jacobs, wrote, "As I have stated before, you do not have a right to publish a letter in *C & EN*." (After considerable activism at the grass roots level in the scientific community, primarily including the large mailing list for Lang's file study, Duesberg's letter was published in *Chemical & Engineering News* on March 25, 1996, p. 4.)

Duesberg (1996b) described what happened at *Nature*: "The editor, John Maddox, not only refused to publish the letter, but advertised the censorship in a full-page editorial, entitled 'Has Duesberg a Right to Reply?' The answer, according to Maddox, was no" (p. 401). The very fact that such an editorial was published, proclaiming a denial of the right to reply, might in itself place *Nature* "in a class of its own."

DOUBLE STANDARDS SET BY LEARNED SOCIETIES

Double standards that might result in the silencing of scholars can take place on levels other than those of editors' and referees' peer-review decisions. A rather unusual case took place at the NAS. Once again, it is NAS member Lang, in a file study piece entitled "Comments on the Meaning of Membership in the National Academy of Sciences," dated October 7, 1992, who documented and described the situation. (Until the file studies are published, they may be obtained from Lang, care of the Mathematics Department at Yale University.)

According to Lang's account, several years ago, the NAS Council urged a Russian dissident mathematician, Igor Shafarevich, to resign as a member of NAS because Council members did not like the contents of some of the things he published after he gained NAS membership. Lang described this as a "spectacular and unprecedented action of the Academy." The Council claimed that the Russian scholar violated NAS's "principles." Lang's documentation and discussion compare Shafarevich's behavior (which allegedly violated NAS principles) with similar behavior by Yuval Ne'eman and William Shockley, who were treated differently by the NAS Council; that is, their behavior was tolerated and they were not urged to resign. Lang also raised questions about whether or not the famous scholars Samuel Huntington, David Baltimore, and Robert Gallo might also have violated one or more NAS principles, and he observed that "whatever 'principles' the Council of the Academy had in mind were left unspecified, except for the ones which they chose to mention to Shafarevich."

It should be obvious how the violation of unspecified principles and the punishment of NAS members for writing about ideas that violate the principles can lead to the silencing of scholars. Double standards of enforcement of unwritten or unspecified principles only add to the uncertainty that might serve as an inhibiting factor resulting in silence rather than expression.

If Duesberg's (1996b) account is accurate, it seems that the NAS was recently involved in another case of suppression by means of double standards. Supposedly, "Academy members such a Duesberg have an automatic right to publish papers without the standard peer review" (p. 397) in the NAS publication *Proceedings of the National Academy of Sciences*. Duesberg (1996b) related that he submitted a paper but the "editor promptly rejected it. ... Duesberg invoked his rights as an academy member and protested. The new editor took up the issue ... insisting he could not publish it without peer review" (p. 397). After some negative and hostile reviews, the paper finally appeared. Duesberg submitted another paper, and "again the editor promptly rejected the paper, arguing that it was too long" (p. 397). Duesberg redid the work and submitted two shorter articles. One was accepted, the other was rejected after several negative peer-review reports. It was reported that this decision "made Duesberg the second member in the 128-year history of the Academy to have a paper rejected from its journal; apparently, the other had been Linus Pauling, who had argued vitamin C might prevent cancer" (p. 398).

DIFFERENT SETS OF RULES

In a brief chapter, "For the Freedom to Comment by Scientists," in *Intellectual Suppression*, Springell (1986) concluded that "rank and file scientists operate under a different set of rules from the chiefs" (p. 75). He also wrote, "In the meantime, my attention had been drawn to an initiative of the U.S. National Academy of Sciences designed to guarantee the freedom of Inquiry and Expression for scientists" (p. 76). This NAS declaration was from 1976, or so, in the wake of Shafarevich's election to NAS.As a dissident scholar in Russia, Shafarevich battled with the Soviet authorities about the rights of scientists to speak out. The NAS makes an appeal for freedom of inquiry and expression, but then when one of its members says something the NAS Council does not like, attempts are made to punish the member. This double standard resembles somewhat the case at Yale where Schmidt's inaugural address with its fervent pitch for freedom of expression on the nation's campuses was delivered at the same time that a Yale student was being punished for having displayed a satirical poster that Yale authorities did not like.

Another case that seems to involve double standards based on different sets of rules took place at Yeshiva University medical school. (Albert Einstein College of Medicine and its teaching hospital, the Montefiore Medical Center). A researcher, Heidi Weissmann, charged a colleague, Leonard Freeman, with plagiarism. Lawsuits ensued. An article in *Science and Government Report* (Greenberg, 1990) relates that Weissmann had to pay her own legal expenses, but Freeman's were apparently reimbursed by Montefiore Hospital. A district court decision favored Freeman. In the wake of the court ruling, the president of Montefiore was quoted as saying, in a memo to the hospital staff, "The Federal District Court has completely vindicated our colleague, Leonard Freeman ... I know you are all as pleased as I am with the decision" (p. 4).

Then, after Weissmann appealed the case in court, a U.S. Court of Appeals ruled in her favor against Freeman, stating that Freeman had "attempted to pass off the work (by Weissmann) as his own" (Greenberg, 1990, p. 4). Meanwhile, an inquiry panel had been formed at Montefiore to investigate the case. In the wake of this court decision, the panel "expressed indifference to the court findings, stating that 'The committee did not feel bound by the decisions of the Appellate Court or the District Court'" (p. 4). It was reported that the "Court of Appeals decision favoring Weissmann did not inspire" the president of Montfiore "to a similar conclusion on her behalf" (Greenberg, 1990, p. 4) as the lower court decision inspired a conclusion on Freeman's behalf. It does not seem that there was a statement by the president that Weissmann had been "completely vindicated." In fact, it was reported that Weissmann lost her job, and Freeman received a promotion.

As in the Yeshiva case, and other cases discussed later, the use of double standards can lead to the problem of toleration of falsification. This toleration includes attempts to silence scholars who try to expose falsifications. Toleration of falsification within the scientific community is itself something of a double standard, because the rhetoric of science claims that science is self-correcting and that the pursuit of truth is a main part of science's mission.