

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

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## **Paradigm Dependency in Academia and Its Effect on Peer Review**

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Our very belief that we have found out most of the answers ... could be considered a superstition as great as any hope entertained by a simple tribesman that dancing will bring rain in the dry season. Equally primitive is the idea that we are prepared to tolerate radically new ideas. (Brookesmith, 1984, p. 6)

How dare you question me? Who are you to question me? Just who do you think you are to question me?

The ideas expressed in the second item partly summarize the title of this chapter. The first item, from *Thinking the Unthinkable: Ideas Which Have Upset Conventional Thought*, is a reflection on the phenomenon described as paradigm dependency.

The “How dare you...” item approximates the exact words of former Yale President A. Whitney Griswold, in three separate replies to a student who questioned him three times. The setting was the dining room of Calhoun College on the Yale campus. Griswold had called a special meeting of Calhoun students in the wake of a campus disturbance that had marred Yale’s reputation somewhat (in the wake of some negative media coverage), and that had strained relations with some

elements of the New Haven community (so-called “town-gown” relations, which have historically been a cause of some friction). Griswold blamed the students and scolded them verbally. On three separate occasions, a student questioned Griswold. Rather than engage in dialogue, Griswold stated that the student should not be asking any of these questions in the first place. The student walked out of the room. (The student’s identity is unknown to the author. It is not even certain if the student was a resident of Calhoun.)

At first glance, Griswold’s replies should have been a revelation and a stark warning that the erudite, open discussions of the Fellows of Pierson College (described at the beginning of this book) in 1956–1957 were not necessarily the rule at Yale, nor throughout academia. However, because Griswold was not speaking as a scholar, but rather as a high administrative official involved in a sensitive administrative problem, his reaction was more or less written off as a ploy of a powerful official pulling rank on a hapless student.

Looking back, however, and reflecting on the situation many years later, Griswold’s behavior might well be seen as a characteristic academic action within a bigger academic picture. Griswold most likely did not speak off the cuff. Given the sensitive nature of the situation, he most likely had given careful consideration to what he would say, and believed that what he decided to say was appropriate, right, and correct—so much so that he would not allow alternative thoughts or questioning of his position. His reaction was essentially a reaction of power to suppress unwanted ideas. On a larger scale, this situation essentially resembles that involving peer-review authorities when they are presented with intellectual ideas, hypotheses, and evidence that question academic paradigms.

## PARADIGMS IN ACADEMIA

Various scholars have discussed, in different contexts, the nature of paradigms and the difficulty involved in changing the paradigms when evidence shows they are false. Kuhn’s (1970) *The Structure of Scientific Revolutions* has come to be regarded as a classic. He observed that academics are educated and trained within the confines of paradigms of a specific discipline. This training might begin in secondary school or undergraduate years in college, and might solidify during study for a graduate degree, with continued reinforcement as academic careers progress. As scholars attain tenure, and as they continue career enhancement, they become part of a group of peers who have been trained in a paradigm and who collectively teach the paradigm to their own students.

The process of being introduced to a paradigm, being brought up—academically speaking—within it, and then repeating (teaching) it to others, has similarities to the Big Lie technique, in the sense that something is repeated often enough, without question or rebuttal, until it is believed to be true. Once it becomes engrained as truth, evidence that comes forth indicating it is false is not easily accepted. In this sense,

academic rhetoric—or at least part of it—has attained what amounts to paradigm status, based on Nissani's (1995) observation, "We have all been raised with the stereotype of the scientist as 'the open-minded man' ... and we tend to view any allegation or evidence to the contrary with incredulity" (p. 177).

The histories of science, medicine, and other academic disciplines are filled with examples of revolutions; that is, situations in which paradigms turned out to be false and were eventually overturned and replaced with new ones. In many of these cases—if not the vast majority of them—there was long-term, fierce resistance to the new paradigm, and bitter controversy before the false paradigm was abandoned. In this case, how can anyone really have been raised with the stereotype of the scientist as an open-minded person?

Kuhn (1970) provided what appears to be at least a partial answer. First of all, he observed that textbooks have to be "rewritten in the aftermath of each scientific revolution" (p. 137). (The same would be true for reference works and classroom lectures.) Then Kuhn stated that the revised texts do not tell the full story:

Textbooks ... once rewritten ... inevitably disguise not only the role but the very existence of the revolutions that produced them. ... Textbooks then begin by truncating the scientist's sense of his discipline's history and then proceed to supply a substitute for what they have eliminated. ... The depreciation of historical fact is deeply, and probably functionally, engrained in the ideology of the scientific profession. (pp. 137–138)

Persons who read textbooks that "disguise not only the role but the very existence of the revolutions that produced them" might well develop a sense of the stereotype of scientists as being open minded. This stereotype might be further reinforced by hearing or reading other ideas such as the following: "It is extremely rare for any scientist to publish an epoch-making discovery and have it ignored. As a rule, the knowledge of the discovery spreads rapidly and the scientist is honored promptly and copiously" (Nissani, 1995, p. 166). Or, in a similar vein, "Scientists are sensitive to history. Their reaction has been to become exceedingly receptive to new ideas" (p. 166).

Would a person who reads the revised textbooks get the same impression, as far as scientists being open minded is concerned, as a person who reads the actual source material? It is a rather widely known fact that William Harvey is regarded as having made an important discovery relating to the circulation of blood, and as a result he is regarded as a hero and a gigantic figure in the history of medicine and science. This appears to be something of a revised textbook version of events, rather than a description of what actually took place. One account states that "William Harvey 'was subjected to derision and abuse and his practice suffered badly. Only after a struggle of over twenty years did the circulation of the blood become generally accepted'" (Nissani, 1995, p. 173). Harvey himself seemed to have been quite aware that his discovery would meet strong disapproval and resistance from his fellow scientist peers, as evidenced by his own alleged premonitions:

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 a few, but I tremble lest I have mankind at large for my enemies, so much doth wont and custom, that become as another nature, and doctrine once sown and that hath struck deep root, and respect for antiquity, influence all men. (Nissani, 1995, p. 168)

## PARADIGMS AND PEER-REVIEW REFEREES

Campanario published studies involving resistance to new ideas, one of which is "Have Referees Rejected Some of the Most-Cited Papers of All Times?" (1996b). He listed many examples, including about a dozen cases where new findings that received harsh peer-review rejection were later honored with Nobel prizes. Excerpts from this article include:

The scientific community often finds it difficult to accept new ideas. ... The greatest and most harmful source of resistance from scientists to scientific discovery comes precisely from those peers whose mission is to preserve the quality of scientific work. ... Some papers were rejected because the referees felt the findings ... clashed with existing ideas or methods ... article on alpha and beta receptors was initially resisted and ignored because the concepts developed in that paper did not fit with ideas developed since 1980. (pp. 302, 306)

In another article, Campanario (1995) wrote that, "in other instances referees' and editors' negative evaluations demonstrate that they did fail to appreciate the importance of a potentially influential manuscript. Sometimes authors are 'guilty' of challenging in their papers the current views or paradigms of a given discipline" (p. 320)

Horrobin (1990), in "The Philosophical Basis of Peer Review and the Suppression of Innovation," lists 18 cases of exclusion of innovative ideas from journals, scholarly conferences, and grants. He stated that this is "by no means a complete list" (p. 1441). Horrobin added that:

Examples of the total suppression of an innovation ... are by definition nonexistent. How can one know about them if they have been suppressed? ... Editors must be conscious that, despite public protestations to the contrary, many scientists-reviewers are against innovation unless it is *their* innovation. Innovation from others may be a threat because it diminishes the importance of the scientist's own work. (pp. 1440-1441)

Cude (1987a), who discussed the themes of "by definition nonexistent" and "How can one know about them if they have been suppressed?," observed that the tendency toward suppression of innovation (by default, so to speak) is part of graduate students' experiences: "Representative of many other topics requiring originality that are avoided like the plague by doctoral candidates. ... Collegiality and tenure have combined to ensure a conspiracy of silence among our academics on the delicate topic of variant methodologies" (pp. 89, 124). Thus, in order to get their PhD degrees, according

to Cude, students must pretty much restrict their research to subject matter and conclusions within accepted paradigms.

Catt (1978) also believed it is possible to cite many instances in which scientific institutions have suppressed innovative ideas. He asserted that referees are censors who operate within "a system of censorship, the censor having no training in how to differentiate between 'wrong' and 'heretical'" (p. 138). And, in line with Kuhn's ideas, Catt added, "What is *not* permissible is to write or say something which contradicts the shared paradigm, and expect it to be tolerated by the accepted journals, conferences and faculties" (pp. 138-139).

### HOW MANY GENERATIONS OF SCHOLARS DOES IT TAKE TO CHANGE A PARADIGM?

The suppression of new ideas has also been seen in terms of refusing to admit errors that are embarrassing to those who enjoy academic fame and power. Along this line, Ruesch (1978/1991) described some studies of Vesalius that contested a paradigm that had been followed and adhered to since the time of Galen, and Ruesch observed that in this case "the university teachers would not admit that they had perpetuated a millennium error" (p. 154). According to Ruesch (1993), after Vesalius showed that Galen had been badly mistaken, it took another two centuries "before the textbooks were corrected" (p. 1).

From the standpoint of silencing scholars within the framework of resistance to paradigm changes, an interesting situation has developed recently in the Guido Riccio controversy. In this case, it seems that a rather subtle use of, or appeal to, Kuhn's observations has been made as a reason, excuse, or justification for not doing further research on the Guido Riccio problem. It sometimes take generations after paradigms are discovered to be false before these false paradigms are replaced by true ones. Why not let another generation resolve the Guido Riccio problem?

In fact, Falcone (1991) wrote an honors thesis, *Is Knowledge Constituted by Power? The Politics of Knowledge in the Art History Community: A Case Study of "The Guidoriccio Controversy."* He placed this specific academic controversy within Kuhn's ideas of paradigms:

I will utilize Thomas S. Kuhn's book *The Structure of Scientific Revolutions* ... in this thesis, drawing similarities. ... The Guidoriccio debate serves as my case study, illustrating how the normative model for the art history community limits the art historians in it, and when their modes of debate fail to resolve a dispute, political power ultimately equals knowledge. (p. 3)

At various points in his thesis, Falcone quoted and cited Kuhn and related his ideas to specific situations in the Guido Riccio controversy.

A part of Falcone's work is based on a taped interview with Professor Samuel Ripston (Williams College, Graduate Program in the History of Art), a famous

scholar of Italian Renaissance art, particularly on the relation of science to art. Perhaps due to his strong interest in science, Edgerton suggested that Falcone write his thesis from the standpoint of Kuhn. As Falcone (1991) himself stated, "In fact, it was Professor Edgerton who first gave me the idea to put Kuhn's conception of the paradigm, as it pertains to anomaly in the normative science community, together with the idea that the Guidoriccio case represents a parallel, as an anomaly being resisted by 'normal' art historians" (p. 47).

More specifically, regarding the importance of the traditional view about the Guido Riccio painting for art history specialists, Edgerton is quoted as follows: "This issue is central to their scholarship. ... All these art historians have their intellectual life. It has been based on accepting this and working in it ... they would have to sit and write something else to get themselves out of their holes" (Falcone, 1991, p. 48).

Having placed the Guido Riccio problem within a paradigm setting, Edgerton then related how he would approach the problem: "Like I would do with it; shrug my shoulders and say, O.K., let someone else handle it in another generation. Let's put it aside..." (Falcone, 1991, p. 49). Later Edgerton (personal communication, January 30, 1992) asserted that "the truth about Guido Riccio will be revealed" but he repeated the "another generation" aspect, claiming that the Guido Riccio problem is "an issue which may well need a couple of generations to finally work itself out. ... Other advocates ... need *time* to salve their own egos. It's my opinion that things might move faster if you would just—pardon my bluntness—*shut up*."

At this point, this question might be asked: On what grounds, or on what basis, should a scholar let scholars of a future generation handle a paradigm-busting issue, rather than tackle the issue head on (perhaps with an intensive graduate program seminar, or more than one seminar, devoted to it)? It would seem that there might be two possible answers. Either the present generation of scholars lacks the necessary critical ability (in the case of art historians this would mean connoisseurship, stylistic analysis, critical analysis, historical analysis, etc.), or else there is a specific volition not to allow the new evidence to be discussed and published during the present generation.

Having a scholar shut up is one of the most effective ways of silencing a scholar. If in the near future Edgerton were to be chosen as an anonymous referee for a paper on Guido Riccio that contained additional paradigm-busting evidence, would he suggest to the editor that discussion should be put off until a future generation can look into it? If he were chosen as a referee for an article that tried to shore up the traditional establishment view, would he suggest the topic should be left for discussion by a future generation? What would editors think of such a recommendation?

### FALSE PARADIGMS CAN BE HARMFUL AND DANGEROUS

Duesberg (1996b) wrote that during 1914 the pellagra epidemic "reached the two hundred thousand-victim mark" (p. 51). During the epidemic, official research was

conducted along the lines that the disease was infectious, based on the so-called germ theory. At one point, Joseph Goldberger was named by the Public Health Service to head the government's pellagra research effort. He discovered that the disease was not infectious and that it was caused by a nutritional deficiency. His ideas contrasted sharply with the official establishment hypothesis of an infectious agent. In fact, research relating to infectious agents had become a paradigm in pellagra studies. As a result, Goldberger "stirred up intense anger and controversy," and he was harshly dealt with in medical journals and newspapers. (Duesberg, 1996b, pp. 51-52) Goldberger was right, but there was much suffering and death taking place during the time he was being mistreated in the newspapers and medical journals.

Philip Semmelweis confronted a similar situation when he found the cause of childbirth fever and proposed the precautions to prevent it. In the midst of the anger, embarrassment, and controversy that Goldberger and Semmelweis (and who knows how many other medical researchers like them throughout the history of medicine) provoked, should they and their colleagues and adversaries have let another generation try to solve the scholarly problems involved?

In the case of pellagra, and of childbirth fever, to mention just two documented examples, clinging to a false paradigm in the face of evidence that the paradigm is wrong led to much death and suffering. Sometimes the harm to health as a result of scholars perpetuating false paradigms can last for centuries, as in the case of research into the cause of scurvy, for instance.

If scholars involved in the Guido Riccio case "need *time* to save their own egos," it would seem that scholars who were clamorously wrong in the pellagra and childbirth fever cases would also have needed time to save their egos. It is true that research relating to disease, with all of the suffering, death, and grief that are involved in the subject matter, has a greater urgency than vexing problems of attribution in art history. However, if search for the truth is part of the academic mission and the academic rhetoric, there is no reason why the truth should be purposely delayed and, thus, withheld from a generation or two of fellow scholars, from students whose families pay tuition, from tourists who pay admission fees to museums, buy guidebooks, and pay for audio-video explanations, and so on.

One of the most extraordinary cases of challenge to academic paradigms in recent times involves the studies of Immanuel Velikovsky. This case is exceptional both for the breadth of the nature of his studies and the number of paradigms that were simultaneously challenged, and for the intensity and viciousness of the reactions of scholars who were angered by his ideas. The totality of Velikovsky's interdisciplinary studies "ravages established doctrines in disciplines from astronomy to psychology" (de Grazia, 1978, p. 19). In *The Velikovsky Affair*, which gives a vivid account of the reactions against Velikovsky, de Grazia (1978) wrote that "the uproar against Velikovsky resulted from his trying to relate ... historical memories and documents to astronomical and physical research" (de Grazia, 1978, p. 158). From groups ranging from scholars at Harvard and editors at the American Philosophical Society (APS) to officials at the AAAS, Velikovsky faced censorship and verbal attacks. Velikovsky



(1978) himself was well aware that his startling ideas would not be accepted by the contemporary academic establishment, when he wrote, "it is almost impossible to change views acquired in the course of decades of reading, writing, and teaching" (p. 6), and also when he observed that scholars "will express their disbelief that a truth could have remained undiscovered so long" (pp. 6-7).

### THE HIV-AIDS PARADIGM

Some paradigms have been around for centuries, whereas others are relatively new. There are also nearly instant paradigms. One example of the latter is the hypothesis that a virus called HIV is the cause of what has variously (i.e., changing with time) been defined as AIDS. If the Velikovsky case is an example of a widespread interdisciplinary reaction of perhaps unprecedented fury in an attempt to prevent several paradigms from being proven false and overturned in one fell swoop, the HIV-AIDS hypothesis, and the challenges to it, seem to represent an excellent case study of how an increasingly monolithic institutionalization of research can make a challenge to a paradigm very difficult and problematic (Lang, 1994, 1995).

The HIV-AIDS hypothesis gained virtually instant paradigm status in the wake of a 1984 press conference. Apparently about 100,000 articles have been written on the subject since then, all of them based on this paradigm. Many scholars have doubts about the HIV-AIDS hypothesis, however, despite the fact that it is official establishment dogma. One of the most outspoken and persistent of them is Duesberg, who has worked and written extensively on the subject. If Duesberg is right, his book *Inventing the AIDS Virus* (Duesberg, 1996b) might well become one of the all-time classics in the history of medicine and science.

In a review of Duesberg's work, Horton (1996) asked, "How could so many scientists have gotten it all so badly wrong?" (p. 15). Such a question might be called an appeal to the paradigm. However, the fact is that history shows that many establishment scientists have been "so badly wrong" on many occasions in the past on many subjects, and for various reasons. Horton's question in this case might have been formulated better by not asking "How could they...?" but by asking, instead, "Have they done it again?"

In fact, Duesberg (1996b) traced several cases, from centuries ago to the present century, in which so many scientists did, in effect, get it all so badly wrong in trying to establish an infectious cause for diseases such as scurvy, beriberi, pellagra, SMON, and kuru. If Duesberg is right, the fact that scientists might have gotten it all so badly wrong in this case would not be as much of a very exceptional event as it would be just one more example that constitutes part of a centuries-old pattern. Along this line, Di Trocchio (1997) described many cases in which the scientific establishment was not able to understand revolutionary ideas, much less accept them. For example, in 1901, when Marconi attempted to send a transmission from Great Britain to Canada, the invention of the radio was impossible in the establish-

ment's view. Likewise, at a meeting of the Royal Society of London in 1878, it was declared that Edison's idea of electric illumination was idiotic ("idiota") and that the problems Edison faced were impossible to resolve from a technical standpoint. Shortly before the Wright Brothers made their brief historic first airplane flight, the scientific establishment believed that it was mathematically impossible for an airplane to fly. Di Trocchio also related how, from about 1917 to 1937, leaders of the physics establishment were on record, at the British Association for the Advancement of Science and elsewhere, with their beliefs that the atom could not be split and that nuclear energy could not be harnessed.

Thus, the establishment has been very wrong in the past. In the case of the current HIV-AIDS hypothesis, even persons without training in biomedicine who have been following the controversy even casually might end up with some doubts about the official (or orthodox) view that HIV is the cause of what has been defined at various times as AIDS. Decades before AIDS was known or given an official name, a young man in Manchester, England, died from what was considered a strange malady. The case seemed so unusual that it was reported in *Lancet*, a leading medical journal (of which Richard Horton is editor). When AIDS was defined and given a name, another item appeared in *Lancet*, suggesting that the Manchester patient was an early AIDS victim, if not the first known victim. Later, it was claimed that HIV was found in his body (which had been preserved, in part, for future medical research). The announcement of this claim made a big hit in the medical research establishment, because it would nullify the theory that HIV jumped the species gap from monkeys to humans as a result of the polio vaccine experiments in Africa in the 1950s. (The Manchester patient would have been infected before those experiments took place.)

At that point, two scientists who specialized in the mutations of the HIV virus requested samples of the batch of virus found in the Manchester patient, as they did not have any samples of HIV from that early date. When they received the samples, they determined that the HIV was from the early 1990s and not from the 1950s. It seemed obvious that either the HIV had been contaminated with later samples, or else HIV from the 1990s had been used. The specialists then sought samples directly from the preserved tissues of the Manchester patient. They received the tissue samples, but not a trace of HIV was found (events were reported in this manner in various newspaper articles; Connor, 1995, p. 1).

If it is true that the Manchester patient did not have HIV, it is obvious that something other than HIV was the cause of AIDS in this case. In other words, HIV would not be necessary to cause the weakening of the immune system to the extent that one of more of the 29 or so AIDS-defining opportunistic diseases took hold in the patient. Nevertheless, in his review of Duesberg's published AIDS research, Horton (1996) wrote, "HIV has been shown to be a necessary factor for the occurrence of AIDS" (p. 10). Even though the journal of which he is editor, *Lancet*, was a protagonist in events that allegedly show that HIV is not necessary to cause AIDS, Horton continued to take the official view on this topic. His attitude would seem to be a further

reflection of how deeply the HIV–AIDS paradigm has taken hold in just a dozen years or so. In fact, now many in the mass media often refer to HIV as the virus that causes AIDS.

Some paradigms consolidate, survive, and become entrenched for long periods of time partly, if not entirely, because of a lack of serious intellectual challenge. Duesberg (1996b) attempted to demonstrate how the recent HIV–AIDS paradigm quickly became entrenched as a result of a monolithic government-controlled peer-review grant system that does not tolerate challenges. In this case, it does not seem to be the accumulation of evidence that consolidates the paradigm as much as the bureaucratic, administrative, and commercial interests that have been built up around the paradigm, and, in some cases, have become dependent on it. Excerpts from Duesberg's observations include:

No medical scientist could even hope to make a career without a research grant from the NIH. Grant allocation selects and rewards conformism with the establishment view. Non-conformists are eliminated. ... The growing number of researchers creates a herd effect, drowning out the voice of the lone scientist who questions official wisdom. ... Through peer review the federal government has attained a near-monopoly on science. ... By declaring the virus the cause of AIDS at a press conference sponsored by the Department of Health and Human Services, NIH researcher Robert Gallo swung the entire medical establishment, and even the rest of the world, behind his hypothesis. Once such a definitive statement is made, the difficulty of retracting it only increases with time. (pp. 452–454)

(In these passages, words like “eliminated” and “drowning out” are direct references to silencing of scholars and their ideas.)

### DIFFICULTIES IN DISLODGING FALSE PARADIGMS

A common theme of discussion of paradigms involves the difficulty in overturning them and dislodging them in the face of evidence that they are false. Why should this be so in academia, where the mission includes the correction of error in the pursuit of truth? Some scholars try to explain this difficulty in terms of psychology or human nature. Nissani (1989) defined one psychological aspect as “conceptual conservatism,” whether it includes child development or history of science: “Although we are all indisputably capable of changing our beliefs, everyday experience suggests that such changes are hard to make: we often cling to old and familiar conceptions of reality, disregarding or explaining away contradictory evidence” (p. 19).

Experimental psychology provides evidence for conceptual conservatism, according to Nissani (1994): “Experimental psychology strongly supports the notion of conceptual conservatism—the human tendency to cling to strongly-held beliefs long after these beliefs have been decisively discredited” (p. 307).

Margolis (1993) discussed the issue in terms of mental habits, in *Paradigms and*

*Barriers: How Habits of Mind Govern Scientific Beliefs:* "It is the robustness of the habits of mind that block the path to the new idea" (p. 31). In other words, mental habits affect thinking in a manner similar to the effects of physical habits on behavior. Just as persons, including scholars, are subject to physical chemical dependencies (e.g., tobacco, alcohol, recreational drugs, and pharmaceuticals), it would appear that scholars are subject to paradigm dependencies (mentally and intellectually). Such paradigm dependency might be seen as a reflexive, "knee-jerk," negative reaction to ideas that challenge academic paradigms.

If a paradigm that was believed to be true turns out to be false, the situation would be a case of what is commonly called *honest error*. (No matter how clamorous the error, if it was made unwittingly it would be considered honest error.) In such cases, scholars sincerely believed they were adhering to the truth. This honest error concept would also extend to the reflexive action (or knee-jerk reaction) of assuming that specific evidence to the contrary is not sufficient to invalidate the paradigm that is being challenged. In such cases, scholars were brought up, intellectually, to believe the false paradigm to be true (in which case the paradigm might be considered an unwitting Big Lie).

## PARADIGM DEPENDENCY AND PARADIGM PROTECTION

Within academia, particularly in light of recent research fraud investigations, there have been attempts to distinguish among honest error, misconduct, and fraud. These three terms are elastic in the sense that they can be interpreted in different ways, meaning different things to different persons and institutions. Besides, what might seem to some scholars to be a case of obvious intellectual fraud might not be regarded as such by administrators in government agencies and universities, and it might not constitute fraud in the sense of legal terminology and the courts.

One common way to judge the difference between honest error and cheating (e.g., misconduct and fraud) is to view the situation from the standpoint of intent. As long as scholars seek the truth and strive for accuracy, the errors they commit are considered honest errors. Only when there is deliberate intent to deceive does the situation involve fraud or misconduct, according to one point of view. Of course, only the specific scholars themselves knew at the outset if there was intent to deceive, and if there was such intent, there might be a strong tendency not to admit it.

The notion of intent to deceive is important for the discussion of academic paradigms and the silencing of scholars. Obviously, if scholars teach their students false paradigms they believe to be true, there is no intent to deceive, even though students end up being deceived. Even when contrary evidence that contests the validity of the paradigm is presented for the first time, the initial knee-jerk incredulity concerning the new evidence can be understood in terms of sincere belief, based on an inherent paradigm dependency.

At certain points, however, in the face of persistent paradigm challenges that are

marked by increasing evidence that shows the paradigm to be false, peer-review authorities begin to suspect, fear, or even realize that the paradigm might be false. At this point, if there is further resistance to critical inquiry, paradigm dependency is no longer entirely based on honest error, conceptual conservatism, or mental habits, but rather on vested interests of one type or another. It would seem that at certain points, adhering to a false paradigm becomes more a case of deliberate paradigm protection instead of inherent, basic paradigm dependency. Peer review and academic rhetoric are then put to severe tests and scholars, their ideas, and the truth are all silenced to the extent that critical analysis and debate are not allowed in regular academic forums (e.g., scholarly publications, conferences, and workshops) to test whether the paradigm is true or not. At a certain point, suppression of evidence for the purpose of paradigm protection amounts to an intent to deceive.

Aside from the fact that, in general, no one likes to be proven clamorously wrong, there is a basic vested interest that scholars have in terms of reputation, expertise, and authority. Schneider (1989) alluded to this vested interest when he wrote, "If the knowledge expounded by recognized scholars to their students should prove to be of dubious reliability, then their authority is open to question. Thus, scientific progress and changing theories are natural enemies of authoritarian tradition" (p. 137). As a result of this basic intellectual vested interest, paradigm protection might be seen as a predictable follow-up to, or extension of, paradigm dependency. Schneider further observed that during the controversy that Lavoisier's ideas provoked in the field of chemistry, "Those who were not only involved in research, but who also taught chemistry and needed to instruct their students and answer questions, were put into a difficult position" (pp. 141-142). At a certain point during a persistent challenge to a false paradigm, paradigm dependency and paradigm-protection are no longer identical. An embarrassing as it may be, authoritative scholars and experts can admit error and inform their students that a major breakthrough in the advancement of knowledge has possibly been made. In fact, Lavoisier's startling discovery about combustion is now taught in high school chemistry classes without the paradigm-related problems that the academic chemistry establishment faced in the 18th century.

Although Schneider's observations were made in relation to Lavoisier and studies in chemistry of the 18th century, vested interests in academia relating to authority were strong from well before the 18th century, and continue to the present time. Meanwhile, vested interests of a commercial, industrial, and financial nature have increased greatly since the era of Lavoisier's research in chemistry. Horton (1996) referred to the "gravy train that has become AIDS, Inc." (p. 19). From their beginnings less than two decades ago, vested interests in the HIV-AIDS hypothesis have mushroomed into a multibillion-dollar affair on an international level, involving diagnostic tests, research, expensive treatments, mass media advertising campaigns, and educational programs in schools. It almost seems as if the momentum of the totality of all this activity on behalf of the HIV-AIDS hypothesis does not even allow enough widespread attention to, or consideration of, the possibility (backed by considerable evidence, as Duesberg and others have shown) that the hypothesis might amount to a

false paradigm. It would seem that the larger the vested interests become, the greater the tendency to brush aside, ignore, or suppress and censor evidence that would endanger the vested interests that are based on the HIV–AIDS hypothesis. Under such circumstances, it is easy to imagine how peer-review authorities might be tempted to make decisions that are evasive or deceptive in nature.

According to Strohmman (1995), Duesberg's doubts about the HIV–AIDS theory were a follow-through and logical extension of Duesberg's research into retroviruses as a possible cause of cancer. Strohmman related that the cancer establishment was "committed to a virus hypothesis" (p. ix), but that Duesberg found serious flaws in the establishment view, and also that Duesberg's "case was a strong one" (p. ix). Strohmman added: "I remember discussing it with some of my own friends at the NIH who were quite surprised that someone of Peter's stature would basically declare obsolete one of the mainstream approaches to such an important disease" (p. ix).

The element of being surprised in this specific instance is important from the standpoint of paradigm dependency, paradigm protection, paradigm changes, and possible intent to deceive. If Duesberg's case was in fact a strong one, were the NIH scholars surprised because inherent paradigm dependency obstructed them from making the same paradigm leap that Duesberg made? Or, were they surprised because they really thought Duesberg was wrong and they wondered how so brilliant a scholar could go so far off the track? Or did they realize, or suspect, on the other hand, that Duesberg might be right, based on the merits of his "strong" case? If this were the case, were they surprised because Duesberg engaged in a form of noncollegiality, a form of betrayal of his colleagues by exposing a clamorous error on their part? There seems to be another possibility. Perhaps they were surprised because they felt Duesberg should have been aware of the retaliation that he would face from the establishment. If they, in fact, suspected that Duesberg was right, their surprise—instead of their support and praise—would represent an obstacle to correction of error, if the surprise was motivated by collegiality with the establishment scholars, or by fear of retaliation.

If these friends of Strohmman at NIH were chosen to be peer-review referees for manuscripts that included research results that "surprised" them in a similar manner, how could they be expected to react? Whether or not such surprise would lead to rejection or acceptance recommendations on the part of these colleagues of Strohmman at NIH, it seems obvious that challenges to paradigms pose serious problems for peer-review authorities, and it seems possible that such problems can lead to double standards in the evaluation of manuscripts.