

Transición From Academic Discussion and Debate to Controversy and Scandal

Academic discussion, unless it is a monologue, implies that a group of two or more scholars, rather than one scholar only, are talking about a scholarly subject. The scholars might generally be in agreement, and discussion would involve elaboration of the main points that are agreed on. If during academic discussion there is major disagreement among scholars, the discussion might be called a debate. Both sides—or more than two sides—air their opinions and present their evidence, and everyone involved tries to evaluate and analyze the evidence.

Discussions and debate can be carried out in the classroom, during informal gatherings, in scholarly publications, or at meetings and conferences devoted to specific subjects. A real debate cannot take place unless opposing sides are involved. The concept of “debating the empty chair” does not involve a debate as much as it does an observation that the chair is empty.

SILENCE AND SILENCING

Discussions and debates can lead to controversies and scandals. As far as the silenc-

ing of scholars is concerned, it might be assumed that such silencing would take place more during controversies and scandals than during discussions and debates. Yet, scholars become silent on a subject when a discussion ends before the discussion becomes a debate. Similarly, scholars become silent on a subject when the debate on the subject ends. Even though there may be strong and unpleasant attempts to silence and censor scholars during controversies and scandals, the scholars who persist and who dare to try to overcome such attempts to silence them may end up—in totality, in the long run—writing and publishing more on the subject than they would have done if no one had attempted to silence them in the first place.

It may seem ironic and paradoxical, but, in some cases, authoritative scholars who decided to try to silence other scholars would have done better to let these other scholars have their say. In fact, sometimes the very attempts to silence scholars can be included among the reasons discussions and debates escalate into controversies and scandals.

DIFFERENT TYPES OF CONTROVERSIES

A comparison of various controversies indicates that some ideas are very likely to become controversial very quickly, whereas others may take a long time—even more than several decades—before they spark a controversy in academia. At the same time, some ideas might become highly controversial almost immediately, and quickly silenced, only to resurface at a later time and become the subject of avid interest that remains within the confines of polite academic discussion and debate, without any repetition of the harsh attempts to silence the idea that took place when the idea was first presented by a scholar.

ADOLFO VENTURI AND GUIDO RICCIO: NO DEBATE, NO CONTROVERSY

In 1907, a famous Italian art historian, Adolfo Venturi, wrote (in a monumental multivolume study of the history of Italian art) that the famous portrait of Guido Riccio on horseback was not painted by Simone Martini. He suggested the figure on horseback was an illustrative part of the large map painted below on the same wall. This map was believed to have been painted in 1345, a year after the death of Simone Martini. Venturi (1907) also doubted that similar large representations of a horse and rider would have been painted in the midst of other castles that comprised the 14th-century cycle of painted castles in the Palazzo Pubblico in Siena.

Venturi's comments were startling, if not revolutionary, in the face of what had become a paradigm in the literature. His observations seemed quite logical. As far as can be determined, his doubts about the attribution of the Guido Riccio portrait did not provoke a negative reaction (perhaps, in part, because these doubts were written

in a footnote). But neither were they accepted. In fact, it does not seem that these ideas of Venturi were taken up or even cited, in the subsequent literature on Simone Martini in general and discussion of the Guido Riccio painting in particular. Venturi's comments, as startling as they were, were apparently the beginning and the end of the discussion. His comments in this case neither provoked discussion nor led to any debate or controversy. Instead, the result was silence on that particular aspect of the subject of Guido Riccio, and on that aspect of the painted map (Mappamondo) on the same wall in the Palazzo Pubblico in Siena. There does not seem to have been any specific attempt to silence Venturi by preventing him from publishing his revolutionary idea, but his idea was, in effect, silenced because other scholars ignored it.

A question remains whether his idea was ignored on purpose because of its revolutionary nature, or because scholars who were his contemporaries and immediate followers did not think, at that time, that his idea was very important. Another question that remains is whether or not his idea was silenced simply because scholars did not read his multivolume work attentively, with the result that the idea remained lost and buried in the footnote.

SEVERAL DECADES LATER: A LONG, BITTER CONTROVERSY ENSUES

In 1977, the following appeared in a brief article in an art history journal:

Iconographical, documentary, and historical evidence, and the lack of spatial harmony between the equestrian figure and the landscape, all suggest, however, that the portrait of Guidoriccio was not painted at the same time as the scenes representing Montemassi and Sassoforte ... a date of 1352 or later would help explain several facts. ... The document of May 1330, which contains a payment for painting Montemassi and Sassoforte, makes no mention of the central figure. ... There is no known evidence that any of these other castles had a Sienese Capitano General painted in the foreground. ... This hypothesis for two separate programs for the fresco ... (Moran, 1977, pp. 82, 84-85)

Both the 1907 and 1977 items propose very much the same thing, namely, that the portion of the painting that represents the horse and rider was painted a couple of decades or so later than the rest of the painting. The 1907 hypothesis considers the horse and rider not as originally part of the scene painted in 1330 with the castle of Montemassi, but instead, as a symbolic figure painted over a large map depicted on the same wall (believed to have been painted in 1345). The 1977 hypothesis considers the horse and rider not as originally part of the scene painted in 1330 with the castle of Montemassi, but instead, as a memorial portrait painted in 1352 (or slightly later). Because it had been ignored by scholars and left out of the pertinent subsequent mainstream literature, the 1907 passage was unknown to the author of the 1977 article until several years after that article had been published.

Even before it was published in a scholarly journal, the hypothesis of the 1977 arti-

cle touched off a strong negative reaction in the city of Siena and in the art history community. It eventually escalated into a controversy described as the "case of the century," "the hottest issue in art history today," "one of the great art historical questions of the century," "one of the most intense and acrimonious battles in the annals of art history," (Moran, 1991, pp. 164–5) and so on. If the 1977 article had been ignored the way the 1907 item had been, or if the 1977 article had been noted without an academic fuss made over it, perhaps things might have ended there, with the hypothesis returning to silence after 1977, the way the similar hypothesis was treated with silence after it appeared in 1907.

The Simone Martini attribution for the Guido Riccio fresco was a paradigm in both 1907 and 1977. There must be some reason, or more than one, to account for the greatly different reactions. One possible explanation is that in 1907 art history was not a fully established academic discipline in many universities, whereas it is at the present time. By 1977, however many professors had taught, to countless students in classrooms in the United States and Europe, that Simone Martini painted Guido Riccio. In fact, scholars waxed eloquently about it, in classroom lectures and scholarly publications, as an example of Simone's genius and originality. (In reality, such originality was so exceptional for a painting of the 14th century that it should have raised doubts about the attribution, rather than serving as a reason for giving Simone Martini credit for having more originality than he actually possessed.) Meanwhile, generations of Sieneese school children had been brought into their city's Palazzo Pubblico (the city hall, which is also a famous museum and tourist attraction) and told by their grade school or junior high school teachers, in front of the painting, that Simone Martini painted the portrait of Guido Riccio for the glory of the Sieneese republic of the 14th century. Besides, in 1907 tourism in Siena was a very small activity compared to the booming industry that it has become since the end of the World War II. After the war, the famous image of Guido Riccio on horseback began to appear often, in Siena and elsewhere, on tourist agency posters, brochures, postcards, covers of guidebooks, plates, ashtrays, cookie box covers, wine bottle labels, calendars, lampshades, bathroom tile, T-shirts, compartments of passenger trains throughout Italy, and so on.

The 1907 hypothesis fell into silence perhaps because of a lack of subsequent adherents to it, combined with a general lack of specific interest about it. What is certain is that it did not cause a fuss or a controversy in Siena or in the art history community. By contrast, when a very similar hypothesis was proposed several decades later, the keen interest and curiosity—if not stupor and shock—that it aroused made it more difficult to be passed off with silence. It was not merely a fine point of attribution and dating to be discussed by a handful of specialists in Sieneese 14th-century painting with their colleagues and students, but it was a matter of great curiosity and concern among the citizens of Siena and the press. Under these circumstances, attempts to silence views that contested the paradigm-dependent views of the establishment scholars merely added to the curiosity and interest among segments of the public and the press.

ANOTHER CONTROVERSY, BUT WITH A DIFFERENT SCENARIO

By comparison, another situation in the history of Sieneese painting might seem very surprising, based on the different types of reactions that occurred. As early as the 17th century, a variety of secondary sources led scholars to believe that an artist named Barna painted a cycle of frescoes in the main church (Collegiata) of the hill town of San Gimignano in the province of Siena. There is even a legend that Barna slipped and fell from the scaffolding while painting in the church, and that this accident led to his death. (There is a plaque in the church, near the paintings, recounting this legend.) Like Siena, San Gimignano has become a thriving center for tourism, and in San Gimignano the large cycle of paintings attributed to Barna is one of the main drawing cards for tourists.

In 1927, the archivist and scholar Peleo Bacci made the startling proposal that the artist Barna never existed, and that the fresco cycle (representing scenes from the New Testament) was painted, instead, by Lippo Memmi, Simone Martini's brother-in-law and collaborator as a painter (Bacci, 1927). A swift, severe, and bitter rebuttal by a powerful art historian, Cesare Brandi (1928), was quick to appear, and the subsequent literature in art history followed the traditional view, by now an entrenched paradigm, that Barna painted the famous New Testament scenes. In effect, Bacci and his ideas on the subject were effectively silenced as a result of the stinging rebuttal. There was no debate and no controversy. In fact, it was certainly more revolutionary and startling to suggest that an artist, to whom a corpus of important works was attributed, did not ever exist, than to suggest that a part of a work (Guido Riccio fresco) was not painted by the same artist who was believed to have painted the rest of the work. In this sense, it was not a surprise that Brandi tried to silence Bacci in such a severe manner, or that other scholars did not attempt to rebut Brandi and support Bacci on this subject.

The hypothesis was thus nipped in the bud, and no debate evolved. In a sense, Bacci's idea was silenced, to the extent that no scholar gave it any backing or support in a scholarly publication. In this regard, it resembled Venturi's hypothesis about Guido Riccio. Both Venturi's idea, however, and Bacci's hypothesis still remained as part of the scholarly record.

Then, almost 50 years later, in 1976, two scholars from two different countries in two different articles independently revived the hypothesis that the famous paintings in San Gimignano were not painted by an artist named Barna. One article (Moran, 1976) stated:

If this hypothesis is correct, it serves two major purposes. Most importantly, it vindicates Peleo Bacci, who was severely, unfairly, and unjustly attacked in print for suggesting that an artist named "Barna" might *not* have painted the Collegiata New Testament Scenes. Secondly, it clears the way for new, fresh investigations to determine just who did paint these frescoes. (p. 79)

Both of the 1976 articles closely followed Bacci (1927) in his belief that the frescoes were painted by Lippo Memmi. One article (Caleca, 1976) repeated the Lippo Memmi attribution, whereas the other suggested that Lippo's brother, Federico Memmi, might be the artist, in collaboration with Lippo.

This time around, Brandi did not respond negatively the way he did in 1928. Perhaps he was no longer keenly interested in this aspect of art history, and it seems he no longer had the clout and power he once had. But neither, for the most part, did other scholars respond in a harsh, negative manner that attempted to silence the idea. To the contrary, lively discussion took place in the scholarly literature (and presumably in classrooms as well), all within the parameters of polite academic debate. Some scholars have tried to hold onto the traditional Barna attribution, but a rather large segment of the specialists in the field, in Italy and elsewhere, have supported the view that members of the Memmi family were the artists who painted the work (Freuler, 1986).

In a sense, this situation was the opposite of the Guido Riccio case. For Guido Riccio, the paradigm-busting hypothesis was at first neither contested nor considered and it fell into silence and oblivion for several decades. When a similar hypothesis was presented decades later, it touched off a storm of strong negative reactions and developed into a controversy described as the case of the century. For the Barna case, a paradigm-busting hypothesis met quick, harsh criticism, so harsh that the hypothesis was in effect silenced in deference to the traditional view. When a nearly identical paradigm-busting hypothesis was proposed ("revived" might also be an accurate term) decades later, it provoked polite and lively academic discussion and debate, with much support for the paradigm-busting hypothesis. The bitter controversy that might have been expected to break out did not take place.

Why was there no attempt to silence the non-Barna hypothesis the second time around, the way the non-Simone Martini hypothesis for Guido Riccio was? Why, in this case, did the situation remain in the realm of polite debate instead of developing into bitter controversy? It certainly was not because the specialists in the Barna debate are a kinder, gentler, and more courteous group than the scholars involved in the Guido Riccio controversy, because they are, to a large degree, the very same scholars.

Perhaps there is a plausible reason, based on the specific evolution of the published studies relating to Barna and to Lippo Memmi. The large New Testament fresco cycle in San Gimignano was practically the only stylistic basis for attributing other works (wall paintings and wood panel paintings) to an artist named Barna. As scholarship on Barna and Lippo Memmi intensified between 1928 and 1976, scholars noticed the strong stylistic affinity between the known works of Lippo Memmi and the frescoes in San Gimignano attributed to Barna. This stylistic affinity was so strong, in fact, that other paintings attributed to Barna by some scholars were attributed to Lippo Memmi by other scholars.

The result was an overlapping of attributions published in the scholarly literature that seemed to be leading to an impasse, if not to a hopeless morass. By the mid-1970s, the time was ripe for Bacci's (1927) silenced hypothesis to get a new hearing, as a logical explanation for the impasse, and also as a means for breaking the attribu-

tion logjam, so to speak. In contrast to the Guido Riccio fresco, which since 1907 was becoming more and more entrenched as a Simone Martini paradigm as a result of rhetoric and hype from scholars and tourist agencies and tourist guides, the Collegiata New Testament fresco cycle was experiencing, in the scholarly literature since the late 1920s, something of a merger between the styles of Lippo Memmi and an artistic personality known by the name of Barna. If Bacci had written his ideas on the subject for the first time during the 1970s instead of the 1920s, he most likely would not have received any reaction similar to the negative one written by Brandi (1928).

As a result of this comparison of these two cases, Guido Riccio and Barna, a case can be made that there is a certain evolution of the scholarly record that can help determine whether a paradigm-busting idea or hypothesis will provoke discussion and debate or whether it will create a bitter controversy that might even develop into scandal. From 1907 to 1977, the Simone Martini attribution for Guido Riccio became solidified and entrenched by extensive repetition, eloquence, and even hype in the classroom and in the scholarly literature. The attribution was also reinforced by means of the painting becoming one of the major tourist attractions of Siena. Meanwhile, between 1928 and the mid-1970s, the traditional Barna paradigm was becoming undermined by increasing overlapping attributions to Barna and Lippo Memmi made by scholars who were working within the paradigm. The paradigm-busting hypothesis, abruptly silenced when it appeared in 1927, found a favorable "state of the research" situation when it was proposed again in 1976. In a sense, the timing was right, even though the hypothesis remained essentially the same. In 1927, the idea appeared to be a bold paradigm buster. By the 1970s, the same idea appeared as something of a logical adjustment to the paradigm.

LANG AND A SOCIOLOGICAL SURVEY

Sometimes a scholarly controversy originates by chance, and develops and escalates, at least in part, because of reactions to attempts to silence scholars and their ideas. A "second reminder notice" relating to a sociological survey did not trigger a controversy in 1969, but it did in 1977. In fact, the second notice of 1977 led to a controversy that involved the sociological establishment and, among other institutions, the AAAS, AAUP, American Council of Education (ACE), NAS, National Science Foundation (NSF), the *Chronicle of Higher Education*, Sloan Commission on Government and Higher Education, and the Carnegie Council on Policy Studies in Higher Education. The controversy lasted from about 1977 to 1980, dealt with defective studies in the field of sociology, and attempted to silence exposure and discussion of these defective studies. What started out as what might seem to be an innocent, bureaucratic, business-as-usual "second reminder notice" escalated into a controversy which is recorded in a book of 700 pages that documents "a major public controversy that involved a good part of the education network" (Lang, 1981, p. 1).

In 1969, Lang threw away a sociological survey sent to him. After the second

reminder notice, he wrote to Clark Kerr, Director of the Carnegie Council on Policy Studies in Higher Education, to let Kerr know of the defects of the survey and of Lang's "contempt" for what Kerr "was doing." No reply from Kerr was received and that was the end of it. In this sense, Lang was silenced because Kerr preferred silence. It is not known if Kerr felt too busy to respond to Lang, if he thought Lang's criticism did not merit a reply, or if he actually agreed, to one degree or another with Lang, but decided not to discuss the issue further. In any case, discussion was nipped in the bud. No debate and no controversy emerged.

Then, in 1977, Lang received another survey. He threw that one away also, as well as the first reminder notice. At about the same time, Lang read an article about the work of Seymour Lipset, who directed the 1977 survey. A second reminder notice arrived, and Lang wrote a letter to "the surveyors," expressing his "exasperation." The letter was eventually reproduced, with Lang's permission, in the *Harvard Crimson*, which also reported that Lipset said "Lang sounds like a crackpot to me" (Lang, 1981, pp. 6-7). The controversy was off and running. Perhaps without a second reminder notice there would not have been any real controversy, much less one that lasted 3 years and filled a 700-page book.

In this case, a consistent attempt to silence Lang was a major cause of the escalation. According to Lang (1981), the controversy could have ended in January 1978, soon after it started. In a reference to Edith Uunila, Assistant Editor of the *Chronicle of Higher Education*, he wrote:

I said she should ask the editors when they talk about me and the file (and they do) just to imagine what would have happened in January 1978 if instead of writing a tendentious article which caused Provost Garfinkel to link me with McCarthyism, *The Chronicle* had printed my 15 pages of comments. It is staggering in retrospect to think how differently things would have turned out: no *New York Review* article; no problem with AAUP; NO FILE!! (p. 588)

It also seems possible that the controversy could have ended even sooner if Lipset had not decided to insult Lang in print in a Harvard publication. Perhaps a more prudent reaction to Lang's disapproval of, and negative reaction to, the survey (e.g., stating something along the line that he would take Lang's observations into consideration in the compilation of future surveys) would have saved Lipset a lot of time and embarrassment.

THE CELL-BALTIMORE CONTROVERSY: DID IT HAVE TO HAPPEN?

Something similar seems to have happened in the Cell-Baltimore case, which turned into one of the most bitter controversies involving modern biomedical research (Because, in this case, the situation escalated into more than one government investigation, and because disciplinary actions were taken, a case can be made that the con

trovsky actually became a scandal.) In "Rockefeller U. Faculty Cool to President Baltimore," the following observations were made:

The melee at the prestigious "Rock" thus further lengthens a grotesquely unpleasant affair that could have been nipped in the bud three years ago if Baltimore and his colleagues hadn't brushed off a young postdoctoral fellow who expressed solid reservations about portions of a paper they co-authored. ... Dingell would not have held hearings on the case. (Greenberg, 1987, pp. 5-6)

This passage sounds very similar to Lang's comments about how the controversy about the 1977 survey could have ended much sooner than it did.

Various observations that describe what took place arrive at similar conclusions:

At the outset, the substance of the dispute was not unlike others that occur regularly in biology labs. It was simply a disagreement over scientific matters between two scientists. ... *Nature* was preventing the scientific community from learning of certain issues. ... Hence *Nature* has a substantial (but of course not exclusive) responsibility for the escalation of the whole case. (Lang, 1993, pp. 11, 31)

Just as the second reminder notice touched off events that led to publication of a 700-page book about defective sociological research, the refusal to publish a mere correction letter regarding specific biological research results touched off events that led to publication of *Science on Trial: The Whistle-blower, the Accused, and the Nobel Laureate*. In this book, the author, Sarasohn (1993), observed that the disagreement could have ended before it escalated into a controversy: "Baltimore ... truly could have stopped the dispute at many different points. ... The damage to his reputation would have been nil" (p. 266).

A letter of correction published in *Cell* in 1986, or soon thereafter, might well have been noticed by specialists, who could have taken note of the correction in their future research. Few, if any, other scientists might have given a second thought to the correction after they had read it. Instead, there was an adamant refusal to publish a correction, or to allow a correction to be published by other scholars. Events escalated to the extent that Baltimore resigned as President of Rockefeller University (Sarasohn, 1993).

From the very outset of the *Cell*-Baltimore controversy, there were attempts to silence scholars who tried to correct what they perceived to be serious error. In one sense, the attempts at silencing succeeded, as Margot O'Toole lost her job in academia, and Stewart and Feder have been punished by means of transfers within NIH. On the other hand, all three of these scholars were heard from extensively during the controversy, despite the attempts to silence them. If a correction letter had been published, Stewart and Feder probably would not have been heard from at all in this case, and it seems unlikely that any more than a few persons would ever have been aware that O'Toole was involved in the correction of the specific errors involved.

VELIKOVSKY: A FAR-RANGING, INTERDISCIPLINARY CONTROVERSY

Not all controversies can be nipped in the bud. In some cases, the nature of the subject matter is so crucial, or so vast and far-reaching, or the alleged mistakes on one side are of such great magnitude that even suppression of the issue for a long time will not put an end to the controversy in all its aspects. The controversies that Velikovsky touched off took place within several different academic disciplines. It is reported that at one point, in 1950, an issue of *Science News Letter* "printed denunciations of Velikovsky's ideas by five authorities in as many fields" (de Grazia, 1978, p. 29). Velikovsky's most famous work, *Worlds in Collision*, became a best-seller despite (or perhaps also because of) the fact that "Commentators ranging from the British Astronomer Royal to the American science writer Martin Gardiner denounced the book" (Cude, 1987a, p. 62). There seems little doubt that the massive attempts at silencing Velikovsky and his ideas were aimed at nipping scholarly discussion in the bud by preventing academic debate, thus hoping to prevent escalation to controversy. Nevertheless, "In spite of the clamour against the heretic, his books have found an enthusiastic following. ... A German edition went through five printings" (de Grazia, 1978, pp. 51-52). The September 1963 issue of *American Behavioral Scientist* "chronicles more than a decade of controversy" (de Grazia, 1978, p. 54).

Velikovsky (1978) himself made some interesting observations relating to the nature of academic controversies:

And many of those who look to acknowledged authorities for guidance will express their disbelief that a truth could have remained undiscovered for so long. ... Never in the history of science has a spurious book aroused a storm of anger among members of scientific bodies. But there has been a storm every time a leaf in the book of knowledge has been turned over. (p. 7)

Along a similar line, he quoted the philosopher Butterfield:

But the supreme paradox of the scientific revolution is in the fact that things which we find it easy to instill into the boys at school ... things which would strike us as the ordinary natural way of looking at the universe ... defeated the greatest intellects for centuries. (Velikovsky, 1980, p. 9)

It seems that the current "ordinary natural way of looking at the universe" consists of a series of confirmations of Velikovsky's hypotheses and predictions concerning the planet Venus. The Mariner probes of Venus have apparently confirmed his startling (i.e., startling to the establishment, but logical and natural to Velikovsky) predictions that the surface of Venus was very hot, that its atmosphere was filled with hydrocarbons, and that it rotated slowly and retrogradely. These were all crucial aspects that were foundations for his more comprehensive theories. "What was unbelievable and heretical in 1950 is making great inroads into the sci-

ence that claimed dogmatic completeness and infallibility as recently as then" (Velikovsky, 1980, pp. 8-9).

Velikovsky's critics in the science establishment were not particularly quick to give him credit for these confirmations of his specific predictions, but it appears possible that in some areas other discoveries and observations will dovetail with his ideas, creating a situation somewhat similar to that of the Barna studies in art history, in which a theory that was not tolerated in the past eventually became the logical conclusion of subsequent studies that evolved. Velikovsky's studies, wide in scope, encompass various academic disciplines. For this reason, his revolutionary ideas might not become incorporated into mainstream orthodox studies quite as quickly as the revolutionary Barna theory did, but the trend might be heading in that direction after the discoveries about Venus revealed by the Mariner probes.

The Velikovsky case, from the beginning, was quite different from the Guido Riccio case, the survey of 1977 case, and the *Cell*-Baltimore case. If scholars ignored Velikovsky, instead of trying so strenuously to silence him, they would have given Velikovsky free reign to expand on and attempt to buttress his revolutionary ideas with further evidence. A letter of correction could have nipped the *Cell*-Baltimore case in the bud, but no letter of rebuttal or correction could have prevented Velikovsky's ideas from stirring strong controversy. If Lipset had not insulted Lang in a Harvard publication, there would most likely not have been any controversy, but an absence of insults against Velikovsky could not have prevented a controversy. If the 1977 Guido Riccio article was ignored or shrugged off (e.g., as being interesting but not important) by scholars, there probably would not have been a controversy that is still intensifying two decades later, but such a tactic would not have worked in the Velikovsky case, which would have continued nevertheless.

LONG-TERM CONTROVERSY ABOUT ANIMAL EXPERIMENTATION IN MEDICAL RESEARCH

In contrast to controversies that suddenly burst open into the academic science, such as those provoked by Velikovsky's studies, other controversies may simmer beneath the surface (as far as scholarship is concerned) for long periods of time, and have great difficulty breaking through certain silence barriers within academia and the mass media. Similar to the Adolfo Venturi hypothesis that doubted the traditional Simone attribution for Guido Riccio, some controversial ideas remain ignored in the mainstream scholarly literature for a long time.

From at least as early as 1873 to the present time, many scholars (more than 1,000) have been claiming and warning that animal experimentation in medical research (particularly drug research) can lead to serious error when the results are used as models for humans. In 1899, Wilson stated that such animal experiments are "inherently misleading in their application to man and therefore unreliable" (Ruesch, 1989, p. 230). Although there has been some recent rhetoric about refining, reduc-

ing, and replacing animal experiments in medical research, it is obviously a long way between the 19th century and the day that Bigelow envisioned, cited earlier, 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, p.). Despite eye-opening titles that occasionally creep into the scholarly literature like *Fifty Years of Folly and Fraud 'in the Name of Science,'* by Bross (1994), and despite rather widespread international sales and distribution of books such as Ruesch's (1978/1991) *Slaughter of the Innocent*, animal experimentation for testing of drugs continues to be a methodological paradigm of the powerful academic-governmental-pharmaceutical-industrial complex.

One reason that a type of silence barrier has prevented the debate—long simmering beneath the surface—from breaking out into the open in academia and among the wider public the way the Velikovsky controversy did, is that the scientific problems involved have been deflected into discussions involving cruelty to animals. Various so-called animal protection organizations have been vociferous, but their efforts are geared more toward "humane" treatment of animals, rather than scientific discussions about tragic errors caused by misleading and unreliable results obtained from animal experimentation (e.g., thalidomide, DES, and SMON). Besides, Ruesch pointed out that infiltration into animal protection groups has been responsible for the fact that among these societies, issues of scientific methodology are not prominent. In fact, the Foreword of the published acts of a 1988 conference on animal experimentation begins, "Scientists are coming under increasing pressure from activist groups to stop animal experimentation branded as cruel and unnecessary for improving human health" (Garattini & van Bekkum, 1990, p. vii). The discussion is directed toward the public activists, not toward rebutting the many scholars who have questioned the scientific validity of using animals as models for humans in drug testing.

The 1988 conference was not a two-sided debate but a repetition of the orthodox view. The views of scholars like Ruesch and Croce were silenced. Croce (1991) wrote that animal experimentation is a "methodological error," and he elaborated:

Which animal? There are millions of species of animal on the earth. So, which should we use? ... No experimentation carried out on one species can be extrapolated to any other, including man. To suppose that such extrapolation could be legitimate is the main reason for the failure and sometimes for the catastrophes which are inflicted on us ... especially in the area of drugs. Too little is spoken or written about certain facts. ... For example, in August 1978 only Japanese newspapers reported the appearance in Tokyo of 30,000 people paralyzed and blinded by Clioquinol. (pp. 13-14)

This situation might have reached proportions of a scandal in Japanese newspapers, but it did not provoke very much discussion, debate, or controversy in the scholarly literature.

Croce (1991) also related that, from 1972 to June 1983, the sale of more than 22,000 "medicinal preparations" had been prohibited because of harmful effects, and he noted that all those preparations "had passed with flying colors the animal experiments

imposed by law" (p. 14). Croce then asked, "how many years must pass before it is realized that a medicine is dangerous and how many have fallen victim to it in the meantime?" (p. 14). As a partial answer to these questions, he cited a study discussed in Germany in 1976: "'6% of fatal illnesses and 25% of all illnesses are due to medicines'" (p. 14). More recently, a brief article in *USA Today* (1995), "Costly Treatment," cited a study that claims that "prescription drug-related medical problems ... cost the nation \$76 billion annually. ... Drug-related problems can result from unforeseeable complications or side effects. ... Hospitalizations are the biggest part of the cost" (p. 9A). The term *unforeseeable* could refer to misleading conclusions derived from results of animal experiments. (The recall, or the prohibition of use, of drugs and medicines would be the medical equivalent of a retraction of a published scholarly article. It would be interesting to know if, when the thousands of medicines were recalled, the scholarly articles that announced their discovery and supported their use as treatments and cures were subsequently retracted. A study of this nature would involve service quality of academic libraries, a subject discussed later in chapter 13.)

Another disaster similar to those caused by thalidomide, DES, and Clioquinol might force the issue out into the open as part of a controversy that includes questioning the scientific reliability of the methodology of animal experimentation. Another possibility is that a trend toward refining, reducing, and replacing animal experimentation might eventually lead to its disappearance from medical research. In that case, a long-running debate might become resolved without ever becoming an acknowledged academic controversy. In such a case, a paradigm would not be overturned by a revolutionary idea, but would, somewhat similar to the Barna case in art history, evolve or transform, on a long-term basis, into another paradigm.

In the May 23, 1996 issue of *New York Review*, a book publisher was quoted as saying, "If Duesberg is right in what he says about AIDS, and we think he is, he documents one of the great science scandals of the century" (Horton, 1996, p. 14). This statement would indicate that a debate has evolved into a major controversy, and could escalate into a major scandal (if it is not already in the process of doing so). One of Duesberg's (1996a) main hypotheses is that HIV is not the cause of AIDS. Several months after the review article in *New York Review*, readers of an article in the *International Herald Tribune* might not have been aware that there had ever been a debate on the issue, much less a controversy, as they read Roberts' (1996) words: "HIV, the virus that causes AIDS" (p. 19). A fierce controversy might be raging among scientists involved in a crucial issue concerning public health without a large portion of the public becoming aware of it. Scholars might be able to overcome strenuous attempts to silence them among their colleagues, as the Horton review of Duesberg's publication testifies, but the scholars may still come up against strong silence barriers among the mass media, even though the pertinent material is of vital importance to the general public.

In his lengthy review of Duesberg's published AIDS research, Horton (editor of the *Lancet*) acknowledged that a controversy was in full swing: "The standoff between Duesberg and the AIDS establishment has become increasingly embittered

and ugly. ... Parts of the lay press have also adopted a highly partisan position in the Duesberg controversy. ... An open debate with Duesberg could have grave commercial consequences" (Horton, 1996, p. 19).

This last observation is of utmost importance for the subject of silencing of scholars. De Marchi and Franchi (1996) stated that scholars who have attempted to debate the authorities on the subject of AIDS have come up against a wall of silence ("muro di silenzio"). Commercial interests are directly involved in much current scientific research. Duesberg (1996b) observed that the "commercialization of science" goes hand in hand with "consensus" of scholarly opinion:

As the NIH budget has increased, so has the subsidized market for biotechnology products. The pharmaceutical industry, likewise, has profited from monopolies granted by the FDA. ... Naturally, some of these federally provided corporate profits find their way back to scientists in the form of patent royalties, consultantships, paid board positions, and stock ownership. These same scientists often sit in judgment of their fellow researchers as peer reviewers. ... Such commercial conflicts of interest have almost totally permeated biomedical scientific institutions today. ... It would be economic suicide for a scientist to advance research that would render his established commercial products obsolete. (pp. 454-455)

If peer reviewers are also holders of commercial interests, it would be not only economic suicide, but possibly also academic suicide for scholars, particularly non tenured scholars, to try to publish ideas and findings that are not in line with the consensus that shores up the commercial interests. In such situations, if scholars were in need of grants to enhance their careers, would they challenge the consensus, or would they tend to remain silent as far as a challenge is concerned? This question leads to the problem of scholars not saying and writing, for various reasons, what they really believe. In its totality, the reticence of scholars to say what they believe is perhaps the most insidious and the most widespread factor responsible for the silencing of scholars in academia. There is no way to determine just how many scholars have been silenced in this manner, nor is there any way to know the total impact such silence has had on the amount of error that remains uncorrected in the scholarly literature.