Authorizing Knowledge in Science and Anthropology

This essay examines an incident in the history of geometry in an effort to articulate what is at stake in the science and culture wars that are currently being waged in academia. The incident is a mid-19th-century “science war” that erupted around a challenge to Euclid’s fifth postulate, a challenge that has since transformed our understanding of the circumference of a unit circle (or π) and our concepts of time and space. The historical outcome of this challenge is the now-accepted non-Euclidean geometry, and the events surrounding its genesis raise issues of central concern to us at the millennium.

The issue I address by discussing this early science war in the context of current debates is that of scientific authority. I argue that the science wars then and now are not about science versus antiscience, not about objectivity versus subjectivity, but about authority in science: What kind of science should be practiced, and who gets to define it? This war for authority has raged and currently rages between disciplines but also within disciplines and fields. There are similarities and differences between the warring camps that defy the often used dichotomous categories of “science” versus “humanism” into which many different positions and arguments between positions have been lumped. For example, within anthropology the debates for authority to define the “correct” approach for studying human communities take place within sociocultural anthropology, as well as between sociocultural and biological anthropology.1 The language of “canons,” “universals,” and “generalized explanations” is often ascribed to the natural sciences, and the language of “variation,” “specificities,” and “contingencies” is used to describe humanistic studies.2 Some anthropologists are currently authorizing themselves as scientists who speak for science and against other kinds of scholarly work that they denigrate as antiscience, art, moralizing, or politics.3 For another example, some physicists and mathematicians are claiming the authority to denounce work in the humanities, as well as in the social and cultural studies of the sciences, mathematics, technologies, and medicines specifically (often abbreviated to “science studies”). Other physicists and mathematicians want no part of this offensive.

My effort here is to expand the terms of the discussion by asking several questions: What are the boundaries of science? Who qualifies as a scientist, and who does not? What is at stake in the wars? I address these questions through an exploration of the complexities of science and mathematics that is intended to move the discussion away from simplistic categories and dichotomies. I hope that my central example from the history of geometry will add to the understanding of what science is and can be, in order to further discussion across camps. My argument is that science is more complex, diverse, and multiple than the understanding of science that is held as an ideal type model (or rhetorically used) by some anthropologists and scientists involved in the debates. This latter model of science has been found lacking by science studies writers. Rather than join either side of this debate constructed around polar extremes that represent nothing that any of us actually do, I want to discuss what science studies is attempting to accomplish through its analyses and critiques.

The “Science Wars”

The movement often called the “science wars” has taken an active role in criticizing science studies, using
textual, conference, corridor, and virtual (World Wide Web and e-mail) sites to advance its offensive. An early conspicuous salvo in the North American science wars was the book Higher Superstition, by Paul Gross and Norman Levitt (1994), which explicitly attacked social and cultural studies of science and technology as heretical. A round of discussions followed, including the conference “The Flight from Science and Reason,” organized in part by biochemist Gross and mathematician Levitt and hosted by the New York Academy of Science (May 31–June 2, 1995). In 1996 a few literary theorists, sociologists, anthropologists, historians, philosophers, biologists, and population geneticists who are engaged in science studies published a set of responses to Gross and Levitt’s work in a theme issue of the collectively run, non refereed journal Social Text. The issue was subsequently republished by Duke University Press in the volume Science Wars (Ross 1996).

Included in the Social Text issue was an article by Alan Sokal, a physicist who teaches at New York University, entitled “Transgressing the Boundaries: Toward a Transformative Hermeneutics of Quantum Gravity” (1996b). Sokal’s essay presented itself as a sincere, albeit naive, attempt at deconstructing physics using the conceptually transfiguring work of physicists Heisenberg and Bohr, “revisionist studies in the history and philosophy of science,” “feminist and poststructuralist critiques,” and cultural studies to try to develop a “future postmodern and liberatory science” (Sokal 1996b: 217–218). But Sokal (1996a) later revealed in a Lingua Franca article that his essay was a hoax, an attempt to parody what he considered to be incorrect constructivist and postmodernist arguments about science based on his interpretation of authors such as Jacques Derrida. Sokal’s article engendered more discussion and debate, including workshops around the country.

In this article I consider a passage about π (π) in Sokal’s hoax article that actually makes considerable mathematical sense. I use the history behind this passage to draw an analogy between today’s “defenders” of “science” and 19th-century “defenders” of premodern geometry and number theory who reacted to new developments in mathematics with indignant polemics and exclusionary tactics.

The texts written by Gross, Levitt, and Sokal primarily aim their criticisms at “social and cultural studies of science” and “postmodernism,” with “the academic Left” as an additional target for Gross and Levitt. (I place quotes around these terms because Gross and Levitt are imprecise in their use of them. For example, in each category they include diverse writings in the humanities and social sciences that would be parsed into many different categories by the authors of the writings and their other readers.) The major arguments made by Gross, Levitt, and Sokal are, in the barest terms, as follows:

1. Authors of major texts in the science studies are antiscientific, antiobjectivist, and antirealist.
2. These authors are nonscientists who have pretensions to know and criticize science when they do not understand science, except perhaps at the level of popularizations.
3. The argument by science studies writers that scientific knowledge is socially constructed is absurd and speaks of science as if it produces false claims.
4. Science is good, science is neutral, and science has the best available methods (for example, the testing of hypotheses) for producing credible claims.
5. While social science and feminist epistemologist writings may be wrong about science, postmodernist writings are worse. They are empty of any real substantive content and are merely decorative in comprehensible language masquerading as knowledge. (This last claim belongs more to Sokal than to Gross and Levitt.)

Some of these points have already been refuted. For example, Roger Hart (1996) has focused on Gross and Levitt’s misrepresentations of the science studies literature through misquotes and quoting out of context. I want to discuss other issues raised in the work of Gross, Levitt, and Sokal. First is their misunderstanding of the notion of construction.

Constructivism in Science Studies

The term construction, or social construction, has been a point of contention in science wars rhetoric. Constructivism is generally employed in science studies to argue that scientific representations and objects are constructed by scientists engaged in particular kinds of scientific work. These representations can take the form of theories, diagrams, drawings, photographs, autoradiographs, and plant and animal models. Scientific representations are created, mediated, and judged by sets of rules and procedures, often called protocols. Each community of scientists claims the license and mandate to construct and enforce such rules and procedures. Thus scientific work includes the methodological development of rules by which representations are constructed and confirmed, as well as the production of representations. Constructivist studies have provided an extensive body of historical and contemporary examples of how locally practiced sciences produce knowledge, how standardized practices and tools are constructed in particular scientific fields, how standards of judgment and verification are established, how specific controversies are resolved, and how social and
cultural locations circumscribe scientific problem selections and constructions.\textsuperscript{10}

Biologists, computer scientists, theoretical statisticians, mathematicians, physicists, and engineers are, in a different sense, constructivists. With particular desires, dreams, cultural resources, and technical competencies in their toolkits, they construct new realities, as well as new representations (e.g., Fujimura 1996; Haraway 1997). It is their job. I refer not only to their construction of theoretical statements about nature. Scientists also construct and create new entities. They create novel creatures, new life, new ways for atoms to exist and move, and new problems that create new solutions that are new entities. These, in turn, change our societies and cultural practices. Scientific constructions defy the human-nature binary, the social-natural binary, in their production and their effects.

In some cases scientists create new objects and new organisms in order to study a particular problem. These objects and organisms are meant to represent particular phenomena. For example, the “OncoMouse” is a living and breathing mouse genetically engineered by DuPont to produce a particular type of tumor consisting of cells containing \textit{ras} oncogenes.\textsuperscript{11} This mouse is an experimental model of cancer. Before the OncoMouse, biology and genetics had a long history of constructing experimental animal strains and cell lines according to specifications designed by scientists interested in particular causal models of cancer. For instance, they crossbred and recrossbred mice to create strains that would reliably produce particular tumors for study by geneticists and biologists. Theories of cancer were based in part on the experiments conducted on these models, which in turn were designed with particular causal theories in mind. Biologists have been producing research tools of these types since at least the beginning of this century (e.g., Clarke and Fujimura 1992; Fujimura 1996).

Other examples of objects produced through science include the new creatures created by agricultural biologists and breeders for commercial purposes. Dolly, the cloned sheep, is only the most recent of these efforts. Dolly was the result of painstaking efforts, 434 attempts. A nonbiological example that comes to mind is the splitting of atoms in a linear accelerator. The amount of money, time, materials, and work (building and managing) that goes into splitting atoms by humans is phenomenal.

Science studies has taken as one of its tasks the analysis of how these productions and others (for example, theories of cancer causation, theories of organismal growth and development, and theories of time and space) are generated through the practices, instruments, and materials of scientific work. Alan Sokal has expressed a concern that constructivist studies of science imply a lack of faith in objective realities.

What concerns me is the proliferation . . . of a particular kind of nonsense and sloppy thinking: one that denies the existence of objective realities, or (when challenged) admits their existence but downplays their practical relevance. . . . My concern about the spread of subjectivist thinking is intellectual and political. Intellectually, the problem with such doctrines is that they are false (when not simply meaningless). There is a real world; its properties are not merely social constructions: facts and evidence do matter. What sane person would contend otherwise? And yet, much contemporary academic theorizing consists precisely of attempts to blur these obvious truths. [Sokal 1996a:63]

Whether or not the authors Sokal criticizes deny the existence of objective realities is a question for empirical investigation, that is, at least a careful reading of the literature. If Sokal refers to the reality of the objects or properties represented through scientific work, the science studies I have read are quite varied in their positions on this issue. But such analyses do not assume that theories and other abstract representations are mere ephemera, as Sokal implies in his crudest representation of science studies. "Anyone who believes that the laws of physics are mere social conventions is invited to try transgressing those conventions from the windows of my apartment. (I live on the twenty-first floor)" (Sokal 1996a:62).\textsuperscript{12}

Some science studies authors have argued that theories are real in their consequences.\textsuperscript{13} They are interested in how people use abstract representations to organize their actions in particular ways. For example, people allow their bodies to be irradiated or injected with chemicals because they and their doctors subscribe to particular theories of cancer. Similarly, scientific theories of racial differences had real consequences in the history of the United States as justifications for particular forms of social organization, with slavery as just one example. For a less venal example, Einstein argued that space and time are not simply mediated and represented by rulers and clocks but, instead, are generated by these instruments. Indeed, notions of space and time represent “the humble and hidden practice of superimposing notches, hands and coordinates” (Mermin 1997b:13). Those who take Einstein’s view still can study how these notions generated from the work of rulers, clocks, and their makers have been used to organize human lives in many cultures.

Sokal states that when science studies does admit to the existence of objective realities, it “downplays their practical relevance” (1996a:63). He perhaps intends to say here that social and cultural studies of science deny that the reality of natural phenomena have anything to do with how scientific controversies are
resolved. Whether representations reign because they best represent the properties of the real world is something scientists and philosophers argue about constantly. In philosophy of science, this discussion would be parsed into, at the very least, discussions of realism and positivism, which are not the same. In science studies there is no consensus on issues of realism, positivism, or relativism. I would argue that some are realists, some are positivists, some are both, some are neither, and others take no positions on these two issues; and the term relativism houses many different views. For example, in contrast to Sokal's assumption, physicist Karen Barad (1996) sees no contradiction between realism and social constructivism, as the subtitle of her article attests. Using Niels Bohr's explication of Heisenberg's uncertainty principle, she discusses the production of the natural object through the "intra-action" of the instrument and the object it measures. For her, the natural object is both real and constructed.

Some science studies take relativist positions on the role of evidence in ending controversies and instead focus on examining the ways through which reliability and stability are produced in scientific and engineering work. There has been much interest in how evidence is constructed, argued for, ignored, marshaled, and so forth. It is also possible for those in science studies to examine the ways in which scientists argue for one position or another without taking positions ourselves. There are often good reasons for not taking a position. For example, given that mathematicians and physicists are engaged in endless and fascinating discussions about the "nature" of space, it would be presumptuous of science studies researchers to assume that we understand the reality of space enough to adjudicate among competing representations of space and to then use that "reality" to explain the state of geometry and physics. As physicist N. David Mermin reminds us,

It was indeed a convention among scientists, buried so deep in their culture to be unrecognizable as such, that space and time were real objective entities measured by clocks and meter sticks. Einstein's profound insight was that, on the contrary, space and time are abstractions, serving to coordinate the results of such measurements. This is what sociologists of scientific knowledge have been saying for over two decades about all kinds of entities that scientists view as objectively real. [1997b:13]

Instead of appealing to "realities," some science studies researchers observe the practices of scientists and the minutiae of their debates and controversies. They try to understand how scientists come to their representations of the world. And they want to understand how this work takes place within the world, within society, within history. Scientific work is not disassociated and detached from the world. Understanding it requires that we examine it in context. Some science studies writers argue that it is not productive to assume that we understand what nature is and then use that nature to explain science or society. There are also debates about whether we understand society and can use those understandings to explain science. Sokal's implication that the protocols in science studies imply a lack of faith in objective realities and his lumping of the diversity of protocols are simplistic and nonproductive.

Scientific Authority and the Posteuclidean Revolution in Mathematics

A second issue raised by the work of Gross, Levitt, and Sokal is that of authority. In their criticisms of social and cultural studies, Gross, Levitt, and Sokal assume their right to determine who gets to speak for and about science and whose work gets to be considered scientific and therefore credible. They claim to be able to determine what is true and false in the statements made not only in the sciences but also in social and cultural studies of science, and even more generally in the social science and humanities literatures. Either they do not recognize that "humanistic studies" have their own standards for determining the quality of scholarship, or they disapprove of those standards. Another topic eschewed in the Gross, Levitt, and Sokal critiques is the question of what scientific standards are. When one studies the actual practices of science, one sees many different standards practiced at different times for different stages of a research project as well as in different kinds of scientific research. Gross, Levitt, and Sokal assume that we all know what scientific standards are and that they apply equally in all sciences, social sciences, and humanities.

In this section I want to compare Sokal's rhetoric with that of historical antecedents who made similar authoritative statements about how science should be done. Sokal's hoax article referred me to the history I discuss here.

Historicizing Pi

I begin with the story of \( \pi \) (pi). Pi is the circumference of a circle with unit diameter, that is, a circle with diameter of length one (of whichever unit of measurement one wants to use). I will refer to such a circle as a unit circle. What is a circle? It is a collection of points equidistant from a given point, such as an origin point. In euclidean geometry, \( \pi \) is a constant, a transcendental number having a value (to five decimal places) of 3.14159.

In Social Text Sokal concocts what he thought would pass as a postmodernist (but nonsensical) emulation of a passage on mathematics by French philosopher
Jacques Derrida. Derrida’s statement, as translated into English and quoted by Sokal, follows:

The Einsteinian constant is not a constant, is not a center. It is the very concept of variability—it is, finally, the concept of the game (jeu). In other words, it is not the concept of something—of a center starting from which an observer could master the field—but the very concept of the game which, after all, I was trying to elaborate. [Derrida 1970:267, in Sokal 1996b:221]

In an abstruse interpretation of this already abstruse passage, Sokal states that

the π of Euclid and the G of Newton, formerly thought to be constant and universal, are now perceived in their ineluctable historicity; the putative observer becomes fatally de-centered, disconnected from any epistemic link to a space-time point that can no longer be defined by geometry alone. [1996b:222]

In addition to his play on Derrida’s passage, Sokal added a statement that one can construct multiple π’s, all of which could be valid in particular situations.

In subsequent discussions of his hoax, Sokal stated that “throughout the article, I employ scientific and mathematical concepts in ways that few scientists or mathematicians could possibly take seriously... I intentionally wrote the article so that any competent physicist or mathematician (or undergraduate physics or math major) would realize that it is a spoof” (Sokal 1996a:63). In a subsequent essay Sokal credits these “meaningless or absurd statements” to Derrida and other “postmodern Masters.”

The most hilarious parts of my article were not written by me. Rather, they’re direct quotes from the postmodern Masters, whom I shower with mock praise. In fact, the article is structured around the silliest quotations I could find about mathematics and physics (and the philosophy of mathematics and physics) from some of the most prominent French and American intellectuals; my only contribution was to invent a nonsensical argument linking these quotations together and praising them. . . .

Now, what precisely do I mean by “silliness”? Here’s a very rough categorization: First of all, one has meaningless or absurd statements, name dropping, and the display of false erudition. Secondly, one has sloppy thinking and poor philosophy, which come together notably (though not always) in the form of glib relativism. [Sokal in press, emphasis in original]

Since Sokal’s article is in hoax mode and his revelation of the hoax in Lingua Franca was not specific about π, we cannot know whether he intended readers to believe that π is “constant and universal.” Therefore, it is important to point out that “the π of Euclid, . . . formerly thought to be constant and universal,” is “now perceived in [its] ineluctable historicity.” The view of a “constant and universal” unit circle circumference (π) was refuted 170 years ago by noneuclidean geometry, as is well documented in the history of mathematics. In 20th-century mathematics and physics, the value of the noneuclidean π now depends on motion, space-time, and gravity (Feynman 1989; Wolfe 1945). Thus this mathematical statement that Sokal attributes to the “postmodern Masters” is well accepted by mathematicians, physicists, and engineers.

The reconstruction of geometry was part of major changes in mathematics and science that began in the 1820s. As part of the revolution in geometry, the value of the circumference of a unit circle (or π) changed. Until that time, euclidean geometry had reigned over the world of mathematics for 2,000 years. Then, in the 1820s, three mathematicians—Carl Friedrich Gauss in Germany, János Bolyai in Hungary, and Nikolai Ivanovich Lobachevsky in Russia—indeed invented new ways of doing mathematics by challenging Euclid’s fifth postulate, on which euclidean plane geometry is based.

I want to demonstrate the reconstruction of π in two ways: first, via a discussion of noneuclidean geometry and, second, through a discussion of noneuclidean distances.

Noneuclidean Geometry

Out of nothing, I have created a strange new universe.

—János Bolyai, letter to his father, November 1823.

How could it be that the circumference of a unit circle is not 3.14159...? According to high-school geometry, it seems obvious that this is the only possible value for this circumference. To answer this question, we must consider the alternative geometries invented by mathematicians when they questioned Euclid’s fifth postulate.

Euclid’s fifth postulate for geometry in the plane states that “if a straight line falling on two straight lines makes the interior angles on the same side less than two right angles, the two straight lines, if produced indefinitely, meet on that side on which are the angles less than the two right angles” (see Figure 1). The lines, according to euclidean geometry, must intersect. Gauss, Bolyai, and Lobachevsky challenged this postulate by inventing alternative plane geometries where the lines do not necessarily meet (see Figure 2). Others considered the possibility that all lines intersect.

This challenge to Euclid’s fifth postulate has had dramatic consequences for geometry. For instance, in euclidean plane geometry, there is exactly one line through a point P parallel to a line L (see Figure 3). This is the familiar world of euclidean plane geometry. But in one example of noneuclidean plane geometry, there can be infinitely many lines through a point P parallel to a
Euclid's fifth postulate. If angles $A$ and $B$ add to less than 180 degrees, then the lines $L$ and $l$ when produced indefinitely meet at some point $P$.

line $L$ (see Figure 4). This geometry is called hyperbolic plane geometry.

At first glance (as well as second, third, fourth glance) Figure 4 seems nonsensical, and for 2,000 years mathematicians and logicians attempted to prove this. Gauss, Bolyai, and Lobachevsky proved that Figure 4 did make sense, by imagining and inventing alternative plane geometries, thus challenging Euclid's fifth postulate. Mathematicians gradually came to accept these new geometries in part by invoking mathematical logic to show that they were just as logically consistent as euclidean geometry was within its system of postulates. In these new geometries, the circumference of a unit circle (that is, $\pi$) is no longer constant and universal. It can be a value different from 3.14159... and is one of the interesting topics of study in posteuclidean geometry.

Those of us who are used to discussing only euclidean geometry intuitively assume that there can be only one line drawn through point $P$ that lies parallel to line $L$. But this intuition does not apply, for example, when we consider distant galaxies and the curvature of space.\textsuperscript{22} Resemblances between non-euclidean geometry and geometry on spheres and other curved surfaces are useful in understanding non-euclidean geometries. For instance, imagine the lines through $P$ in Figure 4 as small circles on a ping-pong ball. Each "line" will meet up with itself before it intersects with the line $L$. Thus there are infinitely many "parallels" through the point $P$ to the line $L$. This highly simplified example is useful for imagining infinitely many parallels to a line, but it is not equivalent to the more subtle noneuclidean geometry in a plane, for which I do not have adequate space here to explain fully (Wolfe 1945:155, 178).

Mathematician Marvin Jay Greenberg explains that Einstein had to use noneuclidean geometry to develop his ideas about distances, the curvature of space-time, and mass, all of which were crucial to his work on the theory of relativity.\textsuperscript{23}

We must question the nature of our instruments—aren't they designed on the basis of Euclidean assumptions? We must question our interpretation of "lines"—couldn't light rays travel on curved paths? We must question whether space, especially space of cosmic dimensions, cannot be described by geometries other than these two (Euclidean and hyperbolic).

The latter is in fact our present scientific attitude. According to Einstein, space and time are inseparable and the geometry of space-time is affected by matter, so that light rays are indeed curved by the gravitational attraction of masses. Einstein said, "To this (non-Euclidean) interpretation of geometry I attach great importance, for should I not have been acquainted with it, I never would have been able to develop the theory of relativity." (Greenberg 1974:249)

Euclidean geometry, then, was not sufficient to allow the development of Einstein's theory of relativity.\textsuperscript{24}

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**Figure 1**
Euclidean geometry. Exactly one line parallel to line $L$ through point $P$.

**Figure 2**
The negation of Euclid's fifth postulate. If angles $A$ and $B$ add to less than 180 degrees, then the lines $L$ and $l$ when produced indefinitely do not necessarily meet.

**Figure 3**
Hyperbolic geometry. Infinitely many lines parallel to line $L$ through point $P$. 

**Figure 4**
Hyperbolic geometry. Infinitely many lines parallel to line $L$ through point $P$. 
Sokal’s postmodern master is on the mark when he states that “mathematically, Einstein breaks with the tradition dating back to Euclid, . . . and employs instead the non-Euclidean geometry developed by Riemann” (1996a:221).

Noneuclidean Distances

For noneuclidean geometry, the circumference of a unit circle can have many values, but it is too complicated to demonstrate here how one computes these values. Instead, I provide another example, that of noneuclidean distances, in which computing alternative values is easier to explain.

A circle is a collection of points equidistant from a given point, such as an origin point. What is distance? For a long time, distance meant euclidean distance (see Figure 5). Since the posteuclidean revolution, however, mathematicians have constructed infinitely many distances, many of which are used daily in scientific research and applications. For each one of these distances, there will be a different π.

For example, Figure 5 shows a euclidean unit circle with a circumference of 3.14159 . . . The euclidean distance d_e between a point with coordinates (x, y) and the origin point with the coordinates of (0,0) is defined as the square root of the sum of x squared and y squared, that is, \( d_e = \sqrt{x^2 + y^2} \). For instance, the euclidean distance from the origin to (5,1) is \( \sqrt{5^2 + 1^2} = \sqrt{25 + 1} = \sqrt{26} = 1.58118 . . . \) Figure 6 presents an example of a unit circle based on a noneuclidean distance called the absolute distance d_a, symbolized as \( d_a = |x| + |y| \), where the straight brackets denote absolute value. For instance, the absolute distance from the origin to (5,1) is 5 + 1 = 6. Figure 7 presents an example of a noneuclidean distance called the maximum distance d_m, or \( d_m = \max(|x|, |y|) \), where maximum stands for the larger of the two numbers in the parenthesis. For instance, the maximum distance from the origin to (5,1) is the larger of the two numbers 5 and 1, that is, 5. For these two distances, the values for the circumference of a unit circle are 2π and 4, respectively. Which distance one uses depends on the application. Figures 6 and 7 do not look like the circles we are used to, since many of us have our concept of circle from euclidean geometry. But they are circles according to the definition of a circle. Often, rather than using just two values x and y, mathematicians and scientists employ several values x, y, z, . . . of interest. In such cases, \( \sqrt{x^2 + y^2 + z^2 + \ldots}, |x| + |y| + |z| + \ldots, \) and \( \max\{|x|, |y|, |z|, \ldots\} \) are called, respectively, the euclidean, absolute, and maximum metrics.

But what about real problems in the world, say, driving to the grocery store or building bridges over water? Surely euclidean distance must be used. Theory is fine in research, but in the practical world there can be only one metric. Not always so. For an example of absolute distance, consider an automobile that makes two trips of lengths l and L. The odometer will record the...
absolute distance. For an example of the maximum metric, consider the problem of the reliability of the materials that go into building bridges. These materials are tested for qualities such as the maximum stress that they can withstand without breaking. That is, reliability engineers who test the materials that go into building bridges use the maximum metric, since it is the maximum stress to which a bridge cable will be subjected that matters. If the cable cannot withstand that degree of stress, the bridge will collapse.

**Canons and “Purity” Control in 19th-Century Geometry**

Sokal's “postmodern Masters” claim that π is no longer constant and universal, then, is now accepted. This so-called postmodern claim was first made in the early 1800s and presented what was then considered to be a heretical challenge to Euclid’s firmly established fifth postulate. Indeed, in 1829 Gauss predicted resistance to his noneuclidean geometry in the form of the “howl from the Boeotians” and would not allow his findings to be published until after his death.26

It is amazing that despite his great reputation, Gauss was actually afraid to make public his discoveries in non-Euclidean geometry. He wrote to F. W. Bessel in 1829 that he feared “the howl from the Boeotians” if he were to publish his revolutionary discoveries. He told H. C. Schumacher that he had “a great antipathy against being drawn into any sort of polemic.” [Greenberg 1974:146]

In the 1820s Gauss was probably correct in his evaluation of the potential reactions to his ideas, should he have published them. In a footnote to Gauss's reference to the Boeotians, Greenberg explains that the critique that Gauss expected actually came in response to Riemann, Helmholtz, and Hilbert’s work on noneuclidean geometry published in the 1860s and, a century later again, in response to Einstein’s ideas published in the 1920s.

[The term Boeotians was] an allusion to dull, obtuse individuals. Actually, the “Boeotian” critics of non-Euclidean geometry—conceived people who claimed to have proved that Gauss, Riemann, and Helmholtz were blockheads—did not show up before the middle of the 1870s. "If you witnessed the struggle against Einstein in the Twenties, you may have some idea of (the) amusing kind of literature (produced by these critics). . . . Frege, rebuking Hilbert like a schoolboy, also joined the Boeotians. . . . Your system of axioms, he said to Hilbert, is like a system of equations you cannot solve (Freudenthal, 1962)." [Greenberg 1974:146]

Referring to a potential source of resistance in a letter to F. A. Taurinus, who had also investigated Euclid’s fifth postulate, Gauss wrote, “But it seems to me that we know, despite the say-nothing word-wisdom of the metaphysicists, too little, or too nearly nothing at all, about the true nature of space, to consider as absolutely impossible that which appears to us unnatural” (Wolfe 1945:47). As Greenberg explains,

The “metaphysicists” referred to by Gauss in his letter to Taurinus were followers of Immanuel Kant, the supreme European philosopher of the late eighteenth century and much of the nineteenth century. Gauss’ discovery of non-Euclidean geometry refuted Kant’s position that Euclidean space is inherent in the structure of our mind. In his Critique of Pure Reason (1781) Kant declared that “the concept of (Euclidean) space is by no means of empirical origin, but is an inevitable necessity of thought.” [1974:146]

Admittedly, accepting noneuclidean geometry required a transformation in thinking. Kant treated space as a transcendental (synthetic a priori) category that existed neither “in” the mind nor “in” the world alone. For Kant, humans experience the world through sense perception, but its form is determined by a priori categories. According to mathematician Harold E. Wolfe, the transformation in mathematical thinking that allowed Gauss to determine that Euclid’s fifth postulate could not be proven was a transformation of mathematics from a metaphysical study to an experimental science.

In that day, it required not only perspicacity, but courage, to recognize that geometry becomes an experimental science, once it is applied to physical space, and that its postulates and their consequences need only be accepted if convenient and if they agree reasonably well with experimental data. . . . The discovery of Non-Euclidean Geometry led eventually to the complete destruction of the Kantian space conception and at least revealed not only the true distinction between concept and experience but, what is even more important, their interrelation. [Wolfe 1945:44; emphasis added]

Wolfe perhaps too generously notes that Gauss had more to lose at the hands of the “smug and shallow-minded geometers” who could easily engineer his fall from grace than did Bolyai and Lobachevsky, who had not yet achieved Gauss’s eminence.

By his day many prominent mathematicians, dominated by the philosophy of Kant, had come to the conclusion that the mystery of the Fifth Postulate could never be solved. There were still those who continued their investigations, but they were likely to be regarded as cranks. It was probably the derision of smug and shallow-minded geometers that Gauss feared. Nor can one safely say that he had less courage than those who made public their results. Compared to him, they were obscure, with no reputations to uphold and nothing much to lose. Gauss, on the other hand, had climbed high. If he fell, he had much farther to fall. [Wolfe 1945:48]

So great was Gauss’s fear of his critics that, upon receiving correspondence from Wolfgang Bolyai, János’s father, urging him to support the junior Bolyai’s work
on noneuclidean geometry, Gauss responded that his intention was to withhold publication of his work until after his death.

My intention was, with regard to my own work, of which very little up to the present has been published, not to allow it to become known during my lifetime. Most people have not the insight to understand our conclusions and I have encountered only a few who received with any particular interest what I communicated to them. In order to understand these things, one must first have a keen perception of what is needed, and upon this point the majority are quite confused. On the other hand, it was my plan to put all down on paper eventually, so that at least it would not finally perish with me. [Gauss’s letter to Wolfgang Bolyai, quoted in Greenberg 1974:144]27

Gauss so disappointed János Bolyai that, after his initial publication of a 26-page appendix to a book published by his father, Bolyai never again published any more of his work on noneuclidean geometry.

Lobachewsky was much bolder than either Bolyai or Gauss in his attempts to promote noneuclidean geometry. He published the first account of this new work in Russian in 1829 and another account in German in 1840. But his attempts to have this paper reviewed met with the ridicule and rejection that Gauss had feared. In 1832 the paper “On the Principles of Geometry,” sent to the Petersburg Academy by the university council of Kazan University, where Lobachewsky was then serving as the rector, met with the following negative review by Mihail Vasil’evic Ostrogradskii.

Having pointed out that of the two definite integrals Mr. Lobachewsky claims to have computed by means of his new method one is already known and the other is false, Mr. Ostrogradskii notes that, in addition, the work has been carried out with so little care that most of it is incomprehensible. He therefore is of the opinion that the paper of Mr. Lobachewsky does not merit the attention of the Academy. [Kagan 1905–07:247, quoted in Rosenfeld 1988:209]

In 1834 another review of this work, “an insulting lampoon” entitled “On the Principles of Geometry, A Work of Mr. Lobachewsky,” was published in the Petersburg literary journals Svyat otechestva (Son of the fatherland) and Severnyi arhiv (Northern archive). The authors of the review were probably Ostrogradskii’s students S. A. Buracek and S. I. Zelenyi.

Glory to Mr. Lobachewsky who took upon himself the labor of revealing, on the one hand, the insolence and shamelessness of false new inventions, and on the other, the simple-minded ignorance of those who worship their new inventions.

However, while I realize the full value of Mr. Lobachewsky’s work, I cannot but hold it against him that, having failed to give his book an appropriate title, he forced us to think for a long time in vain. For instance, why not write, instead of On the principles of geometry, A satire on geometry or A caricature of geometry or a similar thing. [quoted in Rosenfeld 1988:209]

Gauss’s correspondence articulating his ideas was published after his death in 1855. It helped to persuade the mathematical world to take noneuclidean ideas seriously.

Some of the best mathematicians (Beltrami, Cayley, Klein, Poincaré, and Riemann) took up the subject, extending it, clarifying it, and applying it to other branches of mathematics, notably complex function theory. In 1868 the Italian mathematician Beltrami settled once and for all the question of a proof for the parallel postulate: he proved that no proof was possible! He did this by proving that non-Euclidean geometry is just as consistent as Euclidean geometry. [Greenberg 1974:150]

Even after Beltrami had “settled once and for all” that noneuclidean geometry is just as consistent as euclidean geometry, noneuclidean geometry still faced resistance. “As late as 1888, [mathematician] Lewis Carroll was [still] poking fun at non-Euclidean geometry” (Greenberg 1974:150). Even in 1945 mathematician Harold E. Wolfe described students entering a course on noneuclidean geometry as being “imbued with what is almost a reverence for Euclidean Geometry, . . . the one thing about which there can be no doubt or controversy”.

The study of Non-Euclidean Geometry is a fine, rare experience. The majority of the students entering a class in this subject come, like the geometers of old, thoroughly imbued with what is almost a reverence for Euclidean Geometry. In it they feel that they have found, in all their studies, one thing about which there can be no doubt or controversy. They have never considered the logic of its application to the interpretation of physical space; they have not even surmised that it might be a matter of logic at all. What they are told [upon entering this new course] is somewhat in the nature of a shock. But the startled discomposure of the first few days is rapidly replaced during the weeks which follow by renewed confidence, an eager enthusiasm for investigation, and a greater and more substantial respect for geometry for what it really is. [Wolfe 1945:vii–viii; emphasis added]

Discipline and Authority in Science

Mathematicians and scientists have changed the world by challenging canons, universals, and constants. The challenge to Euclid’s fifth postulate presented by Friedrich Gauss, János Bolyai, and Nikolai Ivanovich Lobachewsky is just one example from the history of science. They and their supporters transformed mathematics and produced a new world where the circumference of a unit circle does not have to be 3.14159. . . . In his statement that “the π of Euclid, . . . formerly thought to be constant and universal, [is] now perceived in [its] ineluctable historicity,” Sokal’s parodical author refers
(without citation) to the historical reconstruction of π that had occurred 170 years ago. To argue that π is 3.14159... in euclidean geometry is correct; to argue that 3.14159... is the only value possible for π, as the circumference of a unit circle, is to lack imagination and to ignore radically interesting possibilities. The challenge to euclidean mathematics that began in the 1820s changed the circle, mathematics, science, and the world in thoroughly radical and provocative ways.

Unfortunately, these debates did not have the best outcomes for the individuals involved in pushing the boundaries of what was acceptable science. Greenberg reports on the resistance met by another innovative 19th-century mathematician.

The most “fundamentalist” position on the philosophy of mathematics is that of Leopold Kronecker, who dominated the German mathematical world in the late nineteenth century. According to Kronecker, “God created the whole numbers—all else is man-made.” This position was later attacked by Hilbert, who asserted that “no one shall expel us from the paradise (of infinite cardinal and ordinal numbers) which Cantor has created for us.” The fundamental conceptual changes in Cantor’s work were so repulsive to Kronecker that he blocked Cantor from obtaining a professorship at the better German universities and even prevented him from having his papers published in any of the German mathematical journals. . . . Today, Cantor’s set theory has been accepted by most mathematicians as the foundation for all of mathematics. [Greenberg 1974:255]28

People who take “fundamentalist” positions on science can and often do attain positions of power in academia and ordain themselves as the arbiters and upholders of truth.

Parody as Social Control

Sokal specifically wrote his Social Text article as a hoax, an attempt to parody what he considered to be incorrect constructivist and postmodernist arguments about science by authors such as the philosopher Derrida. He also cited “postmodernist” and “cultural studies” writings as empty, inane, decorative, incomprehensible, nonsense, and “gibberish” strung together using politically correct keywords.

It is notable that euclidean geometry “metaphysicists” also used satire to ridicule and mock work that they did not understand. In his 1832 review comments on the incomprehensibility of Lobachevsky’s “On the Principles of Geometry,” Mihail Vasil’evic Ostrogradskii of the Petersburg Academy assumed that his inability to understand the paper was due to Lobachevsky’s writing with little care rather than to his own closed-mindedness. “Mr. Ostrogradskii notes that, in addition, the work has been carried out with so little care that most of it is incomprehensible” (quoted in Rosenfeld 1988:209).

In 1834 another review of Lobachevsky’s work, an insulting lampoon probably written by one or two of Ostrogradskii’s students was published in two Petersburg literary journals.

The similarity between the critical styles of 19th-century attacks on noneuclidean geometry and contemporary attacks by Sokal, Gross, Levitt, and others is striking. Why do these critics lampoon, parody, and satirize the works of their adversaries? Assuming that they are sincere in their desire to challenge bad scholarship, one would expect serious attempts to obliterate or nullify their adversaries’ points and not mockery and ridicule.

Ostrogradskii and his students most likely did not understand the revolutionary ideas expressed in Lobachevsky’s paper and therefore could not argue the points presented in the paper. Similarly, Sokal may not understand the writings he criticizes in social and cultural studies of science and in cultural studies more generally. This misunderstanding may in part be due to his unfamiliarity with the language and context of discussion. Reading, especially across disciplines and across theoretical approaches (sometimes called “paradigms”), is not a transparently facile activity. Understanding the writing and reading conventions of another author, much less another discipline or approach, takes time, effort, and care. As in any field, not every writer in social and cultural studies of science is as careful as he or she should be, and some of the literature may be unclear or “rhetorically excessive.” But the best science studies take the time, effort, and care to explore the sciences before writing about them. These studies involve patient labor, scholarship, rigor, and careful argumentation. Language is the major instrument for conceptualization and argumentation; thus the best work calibrates language much as scientists calibrate instruments for measuring the phenomena they attempt to inscribe. Criticism, then, should be based on a serious effort to engage with the material on its own terms rather than on the assumption that the reader and writer employ the same tools of analysis, description, and representation. This statement applies to both science studies and science studies critics.

Arkady Plotnitsky, who writes on literature and mathematics, argues that Sokal misunderstands Jacques Derrida’s writings on Einstein’s ideas because he does not understand the language of argumentation in philosophy. Derrida is concerned with the philosophical interpretations and implications of Einstein’s theory of relativity. According to Plotnitsky, part of the reason for this misreading is Sokal’s ignorance of the literature in philosophy on relativity and quantum physics and on the conventions of reading and writing in philosophy. Plotnitsky contends that should Sokal want to engage in philosophical debates or critiques, it is incumbent on
him to read the literature within the conventions of practice in philosophy.

Neither Derrida’s more substantive discussions of mathematics and science . . . nor his caution in this respect, are considered by his recent critics in the scientific community. These critics instead appear to base their views of Derrida’s ideas . . . on indiscriminately extracted, isolated references to science or on snippets of his texts, without placing such statements in the context of his work . . . . At stake here are . . . the most elementary and most traditional norms of reading. Such norms would be routinely applied by scientists in reading scientific texts but are massively disregarded by most scientists who commented on Derrida and other authors mentioned. [Plotnitsky 1997:1]9

Rather than discuss or challenge his philosophical ideas, Sokal’s hoax distorts and parodies Derrida’s statements. But perhaps Sokal is applying the rules of physics to the study of Derrida. Here Plotnitsky employs a quote from Derrida to show that “competent” physicists would not argue in such a fashion, even in cross-disciplinary debates.

The most competent scientists and those most committed to research, inventors and discoverers, are in general, on the contrary, very sensitive to history and to processes which modify the frontiers and established norms of their own discipline, in this way prompting them to ask other questions, other types of questions. I have never seen scientists reject in advance what seemed to come from other areas of research or inquiry, other disciplines, even if that encouraged them to modify their grounds and to question the fundamental axioms of their discipline. [Plotnitsky 1997:27]

Science makes good use of skepticism, as Plotnitsky notes. Sokal is not acting as a scientist interested in arguing on the basis of evidence, informed by an awareness of potential errors in calibrating instrumentation, different interpretations of data, or, according to Plotnitsky, the fundamental principles of physics, biochemistry, or mathematics in any historical period.

What is Sokal doing then, if he is not arguing the merits of the science or of the philosophy? I suggest that Sokal uses ridicule and mockery for four interrelated reasons. First, he cannot engage with the specific arguments and empirical work of social and cultural studies of technoscience because he does not understand them. Second, again perhaps because he does not understand them, he constitutes cultural studies and social studies of science as the “Other,” as different, wrong, and even harmful, much as early Euro-American missionar­­ies and anthropologists viewed peoples of other cultures on their initial encounters. Third, in so judgmentally representing science studies as wrong, he constructs himself as the authority over all sciences, social sciences, and humanities. That is, he constructs his language, method, and set of understandings as the univer-

sally correct set of practices and procedures and then uses his epistemology to represent other ways of doing things as inferior and incorrect. In the pose of defender of universal truth, he becomes the arbiter of truth. Fourth, like early missionaries and anthropologists who brought news of the “Other” back to their home countries, Sokal passes his (mis)understandings and (mis)representations of these literatures on to those scientists who read only his expositions and incites their disapproval. In short, Sokal uses parody as a tool for social control, as a form of discipline.

**Conclusion: Crafting Knowledge in Science and Anthropology**

This essay addresses issues of authority and control in knowledge production. I examine the science wars in 19th-century geometry to create a comparative case for the current wars being generated by critiques of contemporary science studies. My aim is to introduce a historical perspective into the methodological and epistemological debates that have recently rent the academy.

Writings in the history, sociology, anthropology, and literary studies of science document and analyze the constellation of instruments, practices, logics, cultural arrangements, metaphors, and other tools through which complex objects like pi, oncogenes, neutrinos, gravity waves, and airplanes emerge, stabilize, and then produce a wide variety of real effects in our technosociocultural world. This work produces complex accounts of the objects of technoscientific knowledge and of scientific and technological cultures and practices rather than clichés, labels, and stereotypes.30

Contemporary attacks by Sokal, Gross, and Levitt on this work have their predecessors in the 1800s in Kronecker’s attacks on Cantor and in “the Boeotians” who frightened Gauss into withholding the publication of his novel noneuclidean geometry until after his death. Despite these attacks, mathematicians began to question canons and established practices and to reexamine and change the principles behind those canons and methods. We have expanded our conceptions from euclidean mathematics to noneuclidean mathematics; from Newtonian mechanics to quantum mechanics to Einstein’s theory of relativity (all of which are still used); from dimorphic sexual reproduction to Dolly, the cloned sheep. In comparison to all this, science studies, for all its spirited discussions and contentious debates, is relatively conventional.

The stakes in the authoritarian battles are high. They include institutional resources such as research funds, university faculty positions, promotions and tenure, and publication outlets.31 More than just personal
gains for individual academics, these institutional resources are the means for reproducing different intellectual positions through the education of undergraduate and graduate students.

But the battle is not just intellectual or "academic." It also has consequences for people's lives, for knowledge making, and for political actions. The power that science, engineering, and medicine have over people's bodies and lives and over the environment certainly extends beyond the bodies and environments of scientists. For that reason alone, it seems most appropriate that the study of science, technology, and medicine be of concern to a broad spectrum of society.

Notes

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1. As histories of anthropology demonstrate, debates about what anthropology should be have existed since its inception. The arguments, positions, and alliances have been complicated and have defined neat dichotomies throughout its history. And there has been no consensus about what is scientific anthropology and what is not.

2. This essay was first presented at the 119th Annual Meeting of the American Ethnological Society, Seattle, WA, March 6–9, 1997, whose theme was "Anthropology and the Canon."


4. See, e.g., Nader 1996 on the boundaries of science.

5. Science wars had erupted earlier in the United Kingdom with a critique of science studies by a science popularizer (see Lynch 1996).

6. The New York Academy of Sciences is not associated with the National Academy of Sciences. A volume of papers presented at the conferences was later published as The Fight from Reason (Gross et al. 1997). See science historian Paul Forman's (1997) review of this volume.

7. The Social Text editors are Stanley Aronowitz and Andrew Ross. See Robbins and Ross 1996 on why they published Sokal's article. They should not have published the manuscript, but mistakes are common in all human endeavors. That this mistake has become the standard bearer and rallying point of a strange alliance of political conservatives and supposedly progressive academics is fascinating.

8. Although I question Sokal's judgment of scholarship in what he calls postmodernism and science studies, I do not assume that all the writings that he lumps into these categories are examples of good scholarship. Neither do I assume that all writings in physics are examples of good scholarship. I do assume that each work should be evaluated on its merits and not on the category to which it is assigned.

9. There are many science studies writers who do not subscribe to constructivist ideas, but I focus on constructivism here because it is a major bone of contention for critics of science studies.

10. But it is important to realize that they represent in fact a diverse group of perspectives that share some commonalities and have many differences. These differences have been the source of creative debates in the field. See Fujimura 1996 and Pickering 1992 for more discussion.

11. Ras is a technical term that refers to a particular class of oncogenes, or cancer-causing genes.

12. It is distressing to see such misrepresentations applauded by other academics who usually demand careful investigation and argumentation.

13. But the relationship between a representation and its interpretation or use, and therefore its consequences, is not fixed. Representations change, depending on who is using and interpreting them, when, and for what purposes. Thus the study of representations should incorporate their diverse interpretations, renderings, and uses. This highlights the active role of audiences. The consequences of representations (that is, realities) are the products of cooperation, conflict, negotiation, and sometimes power struggles between audiences and producers, between readers and authors. To understand science as a way of producing realities, we also have to understand this interaction.


15. I do not have the space here to articulate the many positions and differences. See Smith 1997 for an excellent extended discussion of current controversies over truth, objectivity, realism, relativism, and constructivism in science studies.

16. There are also occasions when science studies authors choose to take positions in debates. For example, feminist science critics have argued that many biological theories about sex differences are more representative of social and cultural arrangements than of "nature" and have aimed their studies toward examining the social contexts within which such theories have been constructed. Many anthropologists, sociologists, and feminists across the disciplines have long questioned biological explanations for social behaviors and cultural arrangements. But even this could not be characterized as a realist position per se. One could argue that biological realities, whatever they are, are not at issue here and argue instead that social relations can and should be constructed on the basis of principles of equality of opportunity and so forth.

17. Michael Lynch (1996) similarly points out that, for Gross and Levitt, the battle is actually about how "Science" (that mythical, nonexistent entity) should be portrayed, about who controls its portrayal, and not about what scientists or social scientists actually do. See also Lynch 1997 on Sokal.
18. The notation π was introduced by William Jones in 1706 in his *Synopsis Palmariorum Matheseos; or, a New Introduction to the Mathematics*. According to Beckman, "Jones' introduction of the symbol in his book strongly suggests that he used the letter π as an abbreviation for the English word *periphery* (of a circle with unit diameter)" (1970:141). This usage of the symbol π was later popularized by Euler in his *Variae observationes circa series infinitas* (1737).

19. According to Goldhaber,

To Pythagoras and Euclid, π was . . . the ratio of perfect circles' circumferences to their diameters. . . . Now in Einstein-Riemann geometries, as you will find spelled out in Landau and Lifshitz's classic, *The Classical Theory of Fields*, a spinning perfect circle turns out to have a circumference which is not 3.14159 . . . times the diameter; but rather the ratio varies according to the velocity of rotation. Using the original definition of π, you find it is not a constant. [1997]

20. See especially chapter 3 of Wolfe 1945.

21. This statement was published in an appendix to Bolyai 1832 and quoted in Wolfe 1945:51.

22. For an illuminating discussion of this point, see chapter 6 of Feynman 1997.

23. Greenberg refers the reader to George Gamow's article "The Evolutionary Universe" (1956), "which tells how Einstein developed a non-Euclidean geometry appropriate to general relativity from the ideas of Georg Friedrich Bernhard Riemann (1826–1866)" (Greenberg 1974:249).

24. For a more detailed account, including a discussion of three-dimensional noneuclidean geometry, see Feynman 1997, chapter 6.


26. Joan Richards (1979) has argued that resistance in 19th-century England to Reimann's and Helmholtz's noneuclidean geometry also came from outside mathematics due to the use of euclidean geometry to shore up theological authority.

27. See also Rosenfeld 1988:216 for a slightly different translation of Gauss's letter in German to Wolfgang Bolyai.

28. Note that Hilbert battles Kronecker's fundamentalism with his own version of fundamentalism. See also Eglash 1997 on the relationship between Cantor's set theory and the Bamanian sand divination system.

29. The numbers in the Plotniksky quotation citations are the argument numbers in Plotniksky's Internet journal article.


31. At this time, scientists have much more power in the hiring and tenuring of humanities and humanities-oriented social science faculties than do humanists over scientists. A recent example was the refusal of physicists at the Princeton Institute for Advanced Study to allow social science chair Clifford Geertz to hire historian Norton Wise. This followed on the heels of the first refusal by physicists to allow the social science division to hire science studies writer Bruno Latour. In contrast, the social science division has not had the power to interfere in physics hires. Even more ironical and paradoxical is the rhetorical strategy used by Gross, Levitt, and Sokal, who present themselves as marginalized champions of science fighting a wave of critical attacks by cultural studies and postmodernism. Interestingly, this strategy uses the rhetoric of those they attack in the culture wars, especially that in the literature on marginality.

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