

# I know what you mean!

Against all odds, I had been hoping that *Impostures Intellectuelles*, published by physicists Alan Sokal and Jean Bricmont in France in the fall of 1997, was a hoax. I had enjoyed the hoax that made Sokal famous outside his area of quantum field theory<sup>1</sup>, and my motivations were much the same as his. Like Sokal, I had worked with science departments in Nicaraguan universities.<sup>2</sup> That experience had left me skeptical of the facile view of objectivity in science as an ideological smokescreen for domination and oppression, a position apparently exemplified by the following quotation from Andrew Ross, co-editor of the special issue of *Social Text* that published Sokal's parody:

"While the original story of science is still being told in opposition to the humanist tradition of rhetoric, in recent years critics have come to see science itself as just another form of rhetoric; one with particularly aggressive claims on objectivity." (*Strange Weather*, p. 113)<sup>3</sup>

To Stanley Fish's complaint about Sokal's "bad joke,"<sup>4</sup> one might respond that academia takes itself far too seriously, some universities going so far as to charge upwards of \$100,000 for a four-year education. And Ken Hirschkop's characterization of the hoax as a "childish gesture"<sup>5</sup> presupposes that academics, including science warriors, are models of adult behavior, a claim I would find hard to defend.

If *Impostures Intellectuelles* had been a hoax, its target would have been those scientists who, in recent years, have set out to condemn apparently disturbing attitudes toward science on the part of social scientists and philosophers, in the process displaying an often irritating misunderstanding of the texts they criticize. Unfortunately, with the publication of *Fashionable Nonsense* (FN), the North American translation of *Impostures Intellectuelles*, it appears Sokal and Bricmont are on the level. But then, by selecting ten French authors<sup>6</sup> influential in literary and artistic circles in the U.S., assembling a series of lengthy quotations in which these authors discuss science, and annotating them with the invariable comment "meaningless," what have they accomplished? Certainly not what sympathetic critics have claimed. Sokal and Bricmont have not established, nor even coherently argued, T. Nagel's suggestion that "Clearly all this name-dropping is intended to bolster [the] reputations [of the French authors] as deep thinkers."<sup>7</sup> Nor have they exhibited sufficient scholarship to justify R. Dawkins' assertion that "As for the harder task of reclaiming US literary departments for genuine scholars, Sokal and Bricmont have joined Gross and Levitt<sup>8</sup> in giving a friendly and

sympathetic lead from the world of science”<sup>9</sup>. In fact, FN fails to justify its principal accusations by giving a clear characterization of the phenomenon it purports to study, or even to establish its existence. Much less does FN shed light on the questions their selected authors address, or the contexts in which they are being addressed. The book is rather less than an intellectual imposture; it is an intellectual non-event.<sup>10</sup>

It's important to insist on this point, because most of the book's supporters — including the reviewers quoted above — have used its appearance as an excuse to call into question the legitimacy of the book's targets, following Sokal and Bricmont's explicit invitation to do so<sup>11</sup>. In the name of science, as we will see, much has been said about mathematics — in particular a facile assimilation of mathematics to the sciences — that many mathematicians would find hard to swallow. But mathematicians do share with experimental scientists the sense that what we do should be directed to a precise object. Instead of trying to determine what their object might be, Sokal and Bricmont present a list of silly things some famous French authors have written, or appear to have written, about science and mathematics. Absent a serious attempt to find these quotations' least common denominator<sup>12</sup>, this list is no more edifying or consequential than "NFL Football's Funniest Moments," or "The collected Errata of Michael Harris."<sup>13</sup> The preface to the American edition admits that FN has two distinct targets: “abuse” of scientific concepts and terminology — i.e., the silly quotations — and “epistemic relativism,” the notion that science is just one version of the truth among many. Among the French authors only Irigaray and (in part) Latour are criticized for relativism, but they are joined by Quine, Kuhn, and Feyerabend, as well as the Edinburgh school of Social Studies of Science in an “Intermezzo” on the philosophy of science. It's all just “Fashionable Nonsense” as far as the authors, their publisher, and their sympathetic critics are concerned. Since the first target is unmotivated and the second target belongs to an extensive philosophical tradition which FN — as the authors admit — addresses only superficially, the book is inevitably shallow as well as incoherent.<sup>14</sup>

But it has certainly been some kind of event. A year before publication, when it was still just a manuscript on a random walk among publishers and colleagues, this book was already a Paris scandal. Bruno Latour launched a preemptive strike in *Le Monde*, accusing the authors of anti-French protectionism and nostalgia for cold war research budgets. The scandal gained momentum in the weeks before the book hit the stands. “Are our intellectuals impostors?” screamed *Le Nouvel Observateur's* headline. The answer was not very encouraging. Julia Kristeva called the authors “francophobes” and diagnosed the book as a symptom of “economic and diplomatic competition between Europe and America...”; in an interview with *Corriere della Sera* she prescribed psychoanalysis. Physicist Jean-Marc Levy-Leblond compared Sokal to a cowboy who is “undoubtedly” fond of Engels (and Bricmont

to...a druggist!). It wasn't even necessary to read anything: *Libération* published a vicious attack by Vincent Fleury and Yun Sun Limet entitled "The Sokal-Bricmont Swindle," accusing the authors of intellectual dishonesty on the basis of quotations which were nowhere to be found in the book.<sup>15</sup> By contrast, most reviewers of the British and U.S. editions were happy to laugh along with the authors—the reviews by Dawkins and Nagel quoted above were typical — feeding the suspicion that the science wars, like the wider culture wars, are in large part a continuation of the Hundred Years' War by other means.<sup>16</sup>

The arrogance and offensiveness of certain French intellectuals, recalling the defensiveness of *Social Text*'s editors in the wake of the initial hoax, is perhaps the strongest argument in favor of the book, just as the arrogance of the book's supporters<sup>17</sup> is the best caution against joining the bandwagon. The best conclusion is that the science wars are being driven by the mass media — not a hospitable place for careful argument. Those in the U.S. who have celebrated the revival of the tradition of the public intellectual should bear this in mind, since the result is generally not more enlightening than the science wars.

Among the mass media I include blockbuster books like the original French edition of the Sokal-Bricmont book, a major media event of the fall 1997 literary season, with a massive press campaign orchestrated by publisher Odile Jacob.<sup>18</sup> Almost exactly one year later, two responses were published to the general indifference of the media<sup>19</sup> — testimony to the affair's short half-life and to the publishers' relative lack of resources for promotion. Jeanneret's book "L'affaire Sokal ou la querelle des impostures" is an extended analysis of the "affaire" in its rhetorical and mass media dimensions or, more properly, what he calls its "trivial dimension", "the way various ideas circulate, legitimate, meet, or avoid one another..." [p. 10]. Whether or not one shares the author's perspective, Jeanneret's book makes for interesting intellectual history, and has the additional merit of avoiding the cycle of accusations of imposture and stupidity that make the Science Wars so tiresome and predictable. The disadvantage of such an approach is that it sheds little light on the substantial matters at issue. For these, one has to turn to *Impostures Scientifiques* (IS), a collection of articles compiled or commissioned by Baudouin Jurdant and published simultaneously as an issue of the journal *Alliage* and as a book by La Découverte press. Jurdant presents IS as a "very broad defense of thought, in natural sciences as well as the humanities...insofar as [that thought] is at the origin of an original vision of the world that surrounds us and the manner in which that world can have a sense."<sup>20</sup> As we will see, the articles in IS are at their strongest when they explain how Sokal and Bricmont — and their supporters in the press — failed to address the "original vision[s]" of the authors they criticize. But IS also contains more than its share of distortion, settling of scores, and appeal to crude stereotypes. In particular, the authors are too often content to criticize *FN*'s weakest points -- to play the sort of fool's game best exemplified by *FN* itself -- and, having failed to

engage the deeper concerns on the part of the scientific community, can do no better than question Sokal's and Bricmont's motives — again, just as *FN*'s supporters do with the authors it criticizes. In the end, *IS* is too contaminated by the science wars agenda, as defined by the media, to help bring about the rigorous and critical dialogue between the polarized camps that alone can replace the science wars by something more constructive.<sup>21</sup>

## A FOOL'S GAME

The most appropriate way to explain what I mean by "a fool's game" is to play it myself. To illustrate the technique I have chosen three weak points in *FN* for criticism. I am aware that Sokal and Bricmont have addressed some of these issues more carefully elsewhere, even elsewhere in the book, but since they never really manage a synthesis of their purpose, and since they have frequently complained (with some justice) that their critics impute to them attentions that go beyond what they have written, their actual text is as good a place to start as any. Readers who object to the tone can treat this section as a parody of a traditional fool's game mathematicians play at the expense of physicists.

Sokal and Bricmont have not ceased to complain that

"In all the reviews, debates and private correspondence that have followed the publication of our book in France, no one has given even the slightest argument explaining how that relevance [of the scientific terminology to the subject supposedly under investigation] could be established." [FN, p. 13]

Taken literally, this claim is simply false. Several articles in *IS* take pains to defend some of the cited quotations from Lacan, Latour, and Deleuze, in particular.<sup>22</sup> At least two of these articles — physicist David Mermin's attempt at a generous interpretation of Latour and the first version of Jean-Michel Salanskis' discussion of Lacan and Deleuze — were published in 1997, well before the appearance of *FN*, which even contains a brief response to Mermin's arguments (pp. 131-133). What Sokal and Bricmont mean, presumably, is that no one has ever given an argument they find convincing. Rather than try to refute such circular reasoning, I will concentrate on Salanskis' alternative reading of what at first sight is a bizarre and obsessive rehash of basic differential calculus in Deleuze's "Difference et Repetition," and leave it to the reader to decide who is guilty of imposture.<sup>23</sup>

The case of Deleuze is crucial for Sokal and Bricmont. His quotations in FN, alone and with Guattari, are particularly extensive, and he comes across as the most apparently consistent abuser of mathematics after Lacan. Let me say at the outset that I haven't read Deleuze closely and don't pretend to understand the overall purpose of his books — a necessary starting point, one would think, for understanding the importance of the details. But at least some of the passages quoted in FN are not meaningless abuses of science. Salanskis argues in IS that the chapter “Synthèse idéelle de la différence” in Deleuze's *Différence et Répétition*, the source of the quotations on pp. 163-164 of FN, is concerned with how the Platonic idea, in its Kantian reinterpretation, “should and must be reinterpreted as a game of the difference with itself in the virtual, from which all individual things are supposed to arise” (p. 171). This kind of philosophy is not in favor in English-speaking countries, but one can recognize its affinity with stages in the Western tradition; Deleuze is particularly close to Spinoza and Leibniz, who are still taught in universities, even in the United States. Once the question of difference is introduced, it seems to me perfectly natural to consider the philosophical bases of the mathematical treatment of difference, not least because Leibniz was a philosopher as well as developer of differential calculus, and Salanskis develops this theme with regard to Deleuze's entire chapter.<sup>24</sup> But one doesn't have to be a philosopher to spot an inexcusable omission in FN. The first quotation on p. 163 of FN contains an ellipsis. If you go to the source you will find that Deleuze writes “...in the old interpretations of differential calculus, called barbarian or prescientific, there is a treasure that needs to be released from its infinitesimal straitjacket.” He then refers to three texts from the 18<sup>th</sup> and early 19<sup>th</sup> centuries — one by Hoëné Wronski, whose name should be familiar to readers — which draw philosophical conclusions from calculus, and the ellipsis concludes “Many philosophical riches, here, should not be sacrificed to modern scientific technique: a Leibniz, a Kant, a Plato of the calculus.”<sup>25</sup>

This omission is already enough to disqualify Sokal and Bricmont as intellectual historians. I interpret this to mean that the philosophical questions raised by these texts are of interest independently of the subsequent development of the calculus. If this interpretation is correct, the comment in FN that “It is hard to see why a philosopher would choose to ignore [a rigorous exposition of differential calculus, based on limits rather than infinitesimals],”<sup>26</sup> is completely irrelevant. Besides, Deleuze makes it clear on p. 229 that he knows the modern definition of limits, and says explicitly that “When one speaks of the ‘metaphysics’ of calculus, it is precisely this alternative between the infinite representation and the finite representation.” Here infinite presumably refers to Leibniz and finite refers to the modern definition using limits. The IS authors are particularly sensitive to this kind of weakness in FN; J.-L. Gautero makes the point about Deleuze's reading of Leibniz quite clearly in IS (p. 61). But take another look at these remarks. How can Sokal and Bricmont

claim that Cauchy's theory of limits placed the definition of the derivative on firm foundations when an acceptable definition of real numbers -- the set to which the limit belongs -- was not proposed until 50 years later? The two physicists seem to be more "realist" about real numbers than most mathematicians. We will return to this point.

Salanskis also points out a blunder in FN's reading of Deleuze. Sokal and Bricmont give the "correct" translation of Deleuze's phrase "La limite est la puissance du continu" as "the limit is the power [i.e., the cardinality] of the continuum", and call this "exceedingly confused." Salanskis argues, reasonably, that "puissance" here is the (standard) translation of Aristotle's  $\mu$ , and that the whole phrase says something like "individuals are generated in the continuous [i.e., the flow of experience] by specification of their borders". An alternative reading might be that "the concept of the continuous [central to metaphysics from Aristotle to Kant] is actualized in the concept of the [mathematical] limit." Both readings are, at worst, "banal", to use FN's favored term for the residual truth value in the texts criticized, after the nonsense has been leached away; the last reading has the added advantage of being compatible with FN's correction.<sup>27</sup> Again, I'm not particularly a fan of Deleuze's writing, but at least FN's reading doesn't prove he was a dunce.<sup>28</sup> One wonders how retrofitting concepts from 19<sup>th</sup> century mathematics to the metaphysical tradition resolves the question of whether the class of differentiable functions is an adequate representation of physical reality. University teaching would be an entirely different experience if calculus students were in the habit of raising this question.<sup>29</sup>

Does Deleuze's reputation as a deep thinker really depend on the "superficial erudition" (FN, p. 155) displayed in these and other quotations cited by Sokal and Bricmont? Whatever his intentions may have been, Deleuze disarmed the implicit accusation of bad faith long before Sokal showed up: "How can write otherwise than about what one doesn't know, or knows poorly? It's necessarily on that subject that one thinks one has something to say... We have thus spoken about science in a way of which we are well aware, unfortunately, that it is not scientific."<sup>30</sup> If Deleuze's intention was nevertheless to shore up a shaky reputation with pseudo-scientific quotations, it was apparently unnecessary: *Deleuze: une vie philosophique*, a collection of articles on his work published shortly after his death, had very little to say about his theory of calculus and still managed to fill 568 pages.<sup>31</sup>

Sokal and Bricmont do not claim any grand philosophical ambitions: "We are aware that we will be dealing with difficult problems concerning the nature of knowledge and objectivity, which have worried philosophers for centuries" (FN, p. 50). Their intermezzo on philosophy and "cognitive relativism," the longest chapter in the book, is

just long enough to establish that the authors' are not guilty of the most naive errors (of which they have been accused nevertheless, this being much easier than reading what they actually write). From a scientist's standpoint, it is predictably "banal" in the Sokal-Bricmont sense: they agree with skeptics that Popper's epistemology does not capture the fundamental rationality of science, see much that is valid in Kuhn and Feyerabend, and quote approvingly Einstein's remark that "The scientist...must appear to the systematic epistemologist as an unscrupulous opportunist." Hints of a more problematic philosophy emerge nonetheless. Their philosophy of mathematics, for instance, is summarized in the sentence "A mathematical constant like  $\pi$  doesn't change, even if the idea one has about it may change." ( p. 263). This claim, referring to a "crescendo of absurdity" in Sokal's original hoax in *Social Text*, is criticized by anthropologist Joan Fujimura, in an article translated for IS. Most of Fujimura's article consists of an astonishingly bland account of the history of non-euclidean geometry, in which she points out that the ratio of the circumference to the diameter depends on the metric. Sokal and Bricmont know this, and Fujimura's remarks are about as helpful as FN's referral of Quine's readers to Hume (p. 70). Anyway, Sokal explicitly referred to "Euclid's pi", presumably to avoid trivial objections like Fujimura's -- wasted effort on both sides.<sup>32</sup> If one insists on making trivial objections, one might recall that the theorem that  $e$  is transcendental can be stated as follows: the homomorphism  $Q[X] \rightarrow R$  taking  $X$  to  $e$  is injective. In other words,  $e$  can be identified algebraically with  $X$ , the variable par excellence.<sup>33</sup>



More interestingly, one can ask what kind of object  $e$  was before the formal definition of real numbers. To assume the real numbers were there all along, waiting to be defined, is to adhere to a form of Platonism.<sup>34</sup> Dedekind wouldn't have agreed.<sup>35</sup> In a debate marked by the accusation that postmodern writers deny the reality of the external world, it is a peculiar move, to say the least, to make mathematical Platonism a litmus test for rationality.<sup>36</sup> Not that it makes any more sense simply to declare Platonism out of bounds, like Lévy-Leblond, who calls Stephen Weinberg's gloss on Sokal's comment "une absurdité, tant il est clair que la signification d'un concept quelconque est évidemment affectée par sa mise en œuvre dans un contexte nouveau!"<sup>37</sup> Now I find it hard to defend Platonism with a straight face, and I prefer to regard the formula  $e = \sqrt{6\zeta(2)}$  as a creation rather than a discovery. But Platonism does correspond to the familiar experience that there is something about mathematics, and not just about other mathematicians, that precisely doesn't let us get away with saying "évidemment!"<sup>38</sup> This experience is clearly captured by Alain Connes, a self-avowed Platonist, in his dialogue with neurobiologist J.-P. Changeux, who (to

oversimplify) expects to find mathematical structures in the brain.<sup>39</sup> I don't think Connes (or Roger Penrose, another prominent Platonist) is confused about reality, and I have a hard time imagining a neuronal representation that does justice to the concept of  $\pi$ . But the ontological issues are far from settled, and while there is no reason to assume they will ever be settled, the important point is that this situation is not an obstacle to mathematics, much less to rationality.<sup>40</sup> The real absurdity is to claim otherwise.

FN devotes a separate Intermezzo to “Some abuses of Gödel's theorem and set theory.” Even without having read Régis Debray's attempt to use Gödel's incompleteness theorem to show that self-government is logically inconsistent (what Michel Serres calls “the Gödel-Debray principle”), we know what this chapter is about. Nothing particularly French is going on here. Use of Gödel as a cipher for undecidability has long been a staple of general, not to say popular, culture, especially for my generation, which grew up with Hofstadter's *Gödel, Escher, Bach*.. The temptation to draw worrisome conclusions is irresistible<sup>41</sup>, and Sokal and Bricmont should have asked themselves why this is so, rather than attempting to squeeze Gödel's theorem back into its logical straitjacket.

Historically, the theorem symbolizes the failure of a program to ground knowledge unshakably on the model of mathematical reason. Postwar analytic philosophy was born from the ashes of this program, whose sources include that very Enlightenment Sokal and Bricmont claim to be defending.<sup>42</sup> Writers may be unaware of the content of the theorem, not to mention its proof, and yet understand it perfectly as a watershed in western culture.<sup>43</sup> The obsession with Gödel's theorem is proof that the culture cares about mathematics. What is incomprehensible is that anyone concerned with the status of mathematics should complain about this.

But Sokal and Bricmont make a curious claim: “metatheorems in mathematical logic, like Gödel's theorem... are rarefied branches of the foundations of mathematics [that] have very little impact on the bulk of mathematical research and almost no impact on the natural sciences.” Ignore the abusive metaphor, absent in the French original, that likens metatheorems to billiard balls. Ignore the surprising suggestion that the Turing machine has been of little importance for the natural sciences (in theory and practice), or the historical importance of Gödel's theorem in paving the way for present-day agnosticism regarding foundations.<sup>44</sup> Morris Kline devoted an entire book to the thesis that Gödel's theorem was a “disaster”, and called “the loss of truth... a tragedy of the first magnitude.” “The present state of mathematics is a mockery of the hitherto deep-rooted and widely reputed truth and logical perfection of mathematics.”<sup>45</sup> Sokal and Bricmont are observing, correctly, that there was no tragedy; that mathematics, not to



mention the natural sciences, have lost none of their creative force in spite of the failure of the foundationalist program. In this sense perhaps it's true, as a graduate student in Performance Studies once tried to convince me, that mathematics was the first field to go postmodern.<sup>46</sup>

## MATHEMATICS

Attempting to capture the scope of the Science Wars, one soon realizes that it practically coincides with academia as a whole. Proceeding by a sort of Brownian motion, I discovered abelian groups of exponent 2, popularized among structuralists by André Weil's appendix to Lévi-Strauss' *Elementary Structures of Kinship*, used as an organizing principle in articles by Rosalind Krauss and James Clifford on "post-modern" sculpture or the boundary between ethnography and art.<sup>47</sup> This unorthodox use of technical mathematics, while non-standard, seems perfectly unobjectionable as well as intellectually fruitful; there is no reason to assume its purpose is "no doubt, to impress and, above all, to intimidate the non-scientist reader" (*FN*, p. 5). By the same token, Nathalie Charraud's article in (*IS*, p. 243 ff.) makes it at least plausible that Lacanian psychoanalysts found non-orientable surfaces a helpful representation of the structures they claim to have discovered.<sup>48</sup>

Sokal and Bricmont are troubled by the apparent willingness of Roger Anyon, a British anthropologist interviewed by the New York Times, to grant the same epistemic status to scientific theories of Native American migration from Asia and Zuni claims to have originated in the spirit world beneath America: "We are faced with two theories that *contradict* each other. How can they both be equally valid?"<sup>49</sup> FN wisely allows for the possibility that Anyon was misquoted, but the question is better answered by looking at the incident in its proper context, that of contemporary relations between anthropology and the belief systems of the groups being studied. A grant proposal to the National Endowment for the Humanities, regarding the reinstallation of Native American objects in an art museum, promised that "the reinstallation 'will present *both the academic interpretation of an object or objects and the interpretation of the same material as viewed and understood by Native elders and artists* '"<sup>50</sup> Rather than accuse anthropologists of attempting to repeal the law of the excluded middle,<sup>51</sup> Sokal and Bricmont need to recognize that Anyon is taking part in a [dialogue], in which truth is only one issue among many.

Sokal and Bricmont have a hard time imagining that anthropologists have anything more interesting to discuss than the truth. It will therefore come as a surprise to them, and to many philosophers, that truth is also a secondary issue in mathematics. Of course we want to prove true theorems, but this is hardly an adequate or even useful description of our objective.

Mathematicians, and scientists for that matter, judge our peers not by the truth of their work but by how interesting it is<sup>52</sup>. The difference between the true and the interesting even has a market value. Professor Ed Fredkin of Carnegie-Mellon will give you \$100,000 — almost enough to buy a bachelor's degree — if you are the first to make a computer prove an interesting theorem in mathematics.<sup>53</sup> Whereas any beginner can program the computer to prove a true theorem after a single lesson.

This point is hardly novel; Lévy-Leblond says something similar in *IS* (p. 39), and Dieudonné distinguishes further between “mathématiques vides” and “mathématiques significatives.”<sup>54</sup> But it is surprising to see just how little we seem to be concerned with “truth” these days. Mathematicians rarely discuss foundational issues any more<sup>55</sup>, so it was significant that an article by Arthur Jaffe and Frank Quinn, reaffirming the importance of rigorous proof in the current context of strong interaction between physics and mathematics, provoked no fewer than 16 responses by eminent mathematicians, physicists, and historians. No two of the positions expressed were identical, which already should suggest caution in laying down the law on rationality, as Sokal and Bricmont (and Lévy-Leblond, see note 37) seem inclined to do. But for our purposes here, what is remarkable is that almost none of the responses had much to say about “truth.”<sup>56</sup> “Truth” was central, predictably, only to the responses of Chaitin and Glimm. Chaitin's branch of mathematics treats “truth” as a technical term, without metaphysical connotations, and Chaitin's claim to have “found mathematical truths that are true for no reason at all” suggests that it may be harder than Fredkin suspects to know just when to award his prize. Glimm's brand of truth is quite the opposite: it “lies not in the eye of the beholder, but in objective reality... It is thus reproducible across barriers of distance, political boundaries and time.”<sup>57</sup> Turning to the introduction to the book *Quantum Physics*, by Glimm and Jaffe, one finds the unusual assertion that “mathematical analysis must be included in the list of appropriate methods in the search for truth in theoretical physics.” Generally speaking, the mathematics department may be the only spot on campus where belief in the reality of the external world is not only optional but frequently an annoying distraction. But this patently does not apply to mathematical physicists, and I can't help thinking it's not a coincidence that both Bricmont and Sokal are amply represented in the Glimm-Jaffe bibliography.

Philosophers and philosophically-minded sociologists concerned with mathematics seem to think their job is to explain mathematical truth. Edinburgh sociologist David Bloor and philosopher Philip Kitcher, cast for science wars purposes as an irresponsible relativist and a moderate realist, respectively,<sup>58</sup> have both attempted to develop empiricist accounts of mathematical knowledge<sup>59</sup>. (Knowledge and truth are not synonyms but they are on the same wavelength.<sup>60</sup>) They have their own (very different)

reasons, but in so doing I'm convinced they have missed the point of mathematics. As is typical in such discussions, their examples are drawn either from mathematical logic or from mathematics no more recent than the 19<sup>th</sup> century. If the sociologist, at least, had done some field work, he couldn't have helped observing that what mathematicians seem to value most are "ideas" (not necessarily of the Platonic variety); the most respected mathematicians are those with strong "intuition." Now intuition, the philosopher assures us, is philosophically indefensible; Sokal and Bricmont add that "intuition cannot play an explicit role in the reasoning leading to the *verification* (or falsification) of these theories, since this process must remain independent of the subjectivity of individual scientists."<sup>61</sup> Fredkin's theorem-proving machine may see things that way, but what are we to make of Thurston's emphasis on the "continuing desire for *human understanding* of a proof, in addition to knowledge that the theorem is true"?<sup>62</sup> We know what he means, as we know what Robert Coleman means, when, having discovered a gap in Manin's proof of Mordell's conjecture over function fields, he nevertheless writes "I believe that all this is testimony to the power and depth of Manin's intuition."<sup>63</sup> Is Coleman trying to slip a counterfeit coin between the context of discovery and the context of justification? Do these offhand comments touch on something genuine and profound about mathematics? Or is it just my indoctrination that makes me think so?

Let's try a thought experiment. How do we know Wiles' proof of Fermat's Last Theorem, completed by Taylor and Wiles, is correct? Although this particular theorem, better publicized than any in history, has been treated with unusual care by the mathematical community, whose "verdict" is developed at length in a graduate textbook of exceptionally high quality, I'd guess that no more than 5% of mathematicians have made a real effort to work through the proof<sup>64</sup>. Some scientists (and some mathematicians as well) apparently view Wiles and his proof as an "anachronism."<sup>65</sup> The general public is not entirely convinced. Why are we? Can a sociologist study this question without knowing the proof? Can mathematicians pose the question in terms sociologists would find meaningful? Knowing the truth of the matter is obviously of no help, and relativism is not the issue: it's not clear what kind of "reality" would be relevant to settling the question, but the fact that no one has found a counterexample is certainly not a good candidate. The question is rather more subtle than the example Sokal and Bricmont propose in their philosophical intermezzo, how to decide whether a herd of elephants is stampeding inside a lecture room.<sup>66</sup> We will conclude the thought experiment at the end of the essay.

CONTEXT

As an occasional geometer, I was delighted to read recently that “La géométrie nous enseigne la toute-puissance de la forme. Dans sa logique, la matière importe peu, n’existe pas. D’ailleurs, il n’y a pas de matière dans l’Univers. La physique ne reconnaît que des formes et des masses.” The source of this information was the December/January issue of Paris Vogue magazine entitled “Fashion and Science,” which featured 100 pages of highlights from the history of science, from Euclid and Archimedes to cloning and wormholes. Schrödinger’s cat, the Mandelbrot set, and the butterfly effect make their expected appearances, as do Galileo, Darwin, Einstein, Heisenberg, and the rest of the gang, in a running series of captions below fashion photos vaguely arranged by scientific themes.

At about the same time, Givenchy introduced a new men’s cologne called ; the ads showed a cosmonaut and the slogan “un peu plus loin que l’infini.” A few months earlier, at the seventh annual “Cité de la réussite” at the Sorbonne, 100 assorted big names from all fields — Nobel prize winners, a Cardinal, three former prime ministers and a constellation of ministers, Yehudi Menuhin, fashion designer Sonia Rykiel, Jean-Marc Lévy-Leblond — gathered to talk about time. The program looked like something straight out of Virilio: Boutros Boutros-Ghali and Javier Solana talked about “Human rights beyond time and space” while (former Mitterand adviser) Jacques Attali and communications professor Daniel Bounoux spoke on “infinite space” and the global village.

Has nothing changed in Paris since the ‘60s, when Lacan’s *Ecrits* and books about philosophy of mathematics were best-sellers, available in drugstores.<sup>67</sup> Does intellectual imposture on scientific themes really sell cologne and haute couture<sup>68</sup> and public policy<sup>69</sup>? These are the wrong questions. Science plays a peculiar role in every culture, and maybe the peculiarities are more visible in France than elsewhere. Picador, Sokal and Bricmont’s U.S. publisher, finds this an occasion for levity; hence the title’s allusion to the stereotypically Parisian industry and the picture of the Eiffel Tower on the cover. The original title in France and Britain was sterner: somebody is guilty of imposture. The reader is offered the implausible choice: the French authors are either fools or frauds, and finally someone is willing to say so. This explains the recurring reference to *The Emperor’s New Clothes*. But why did these authors turn to science? Why these particular branches of science? And why have their readers been so dumb for so long?

The failure of FN as an intellectual event is its authors’ failure to ask these questions, the ones that should naturally occur to a scientist. Sokal and Bricmont write:

In some cases we have quoted rather long passages, at the risk of boring the reader, in order to show that we have not misrepresented the meaning of the text by pulling sentences out of context. (FN, p. 17)

This may satisfy those who imagine that the context of page 50 is pages 48-52, say, but if the context is an ongoing literary debate or an entire culture's orientation to mathematics and science, then the length of the quotations is irrelevant. To paraphrase remarks made by David Bloor, Sokal and Bricmont are "as it were, coming into the middle of a conversation that has been going on for some time."<sup>70</sup> In what is perhaps the most cogent observation in *IS*, Dahan-Dalmedico and Pestre point out that, while they are prepared to laugh along with Sokal and Bricmont at the "extraordinary scientific mimicry that overcame most intellectuals in France in the 1960's," the two physicists' reading of the French authors concentrates on "what is badly expressed or ridiculous — while *never* asking what is at stake intellectually, *never* speaking of the manner in which these authors...work and propose to think the world."<sup>71</sup>

Perhaps the biggest scandal is that the answers to the questions Sokal and Bricmont neglected to ask are probably not that hard to find. From Oulipo to Althusser, postwar France was obsessed with the search for scientific regularity, preferably mathematical, of the Bourbaki variety.<sup>72</sup> Dahan-Dalmedico and Pestre assert that this "scientism" was "desired and sustained by the scientists themselves"<sup>73</sup> Unfortunately, the examples they cite are too special, and much more historical work needs to be done to reconstruct all the interactions between philosophers and scientists of the "post May '68" generation.<sup>74</sup> What's certain is that some of the variables hidden from Sokal and Bricmont can be found literally around the corner from the offices of Odile Jacob, at the Ecole Normale Supérieure, where scientists in good standing were among those attending Lacan's and Althusser's seminars. Some credentialed mathematicians talked topology with Lacan; others worked with Althusserian philosophers on some of the same texts of Leibniz and Hegel discussed by Deleuze<sup>75</sup>; still others tried to develop mathematics adequate to structuralism and post-structuralism. Salanskis refers to his own experience as well as that of Gilles Châtelet and Jean Petitot, both of whom found inspiration in Deleuze's *Différence et Répétition*.<sup>76</sup> Sokal and Bricmont replace this history by an H.C. Andersen fairy tale.<sup>77</sup>

Interactions between the most famous French mathematicians and literary intellectuals of the '60s and '70s have not been documented, as far as I know, and perhaps Sokal and Bricmont fault the latter for learning their mathematics from unrepresentative figures. They certainly expect the intellectuals to make the distinction between mainstream physics and "the most subjectivist writings of Heisenberg and

Bohr.”<sup>78</sup> The collaboration between scientists and philosophers initiated by Althusser was paralleled by the continuing seminar on philosophy and mathematics, where Jean Bricmont spoke in this in December 1997, on the problem of hidden variables in quantum mechanics. This topic is by no means irrelevant to the science wars. Hidden variables theories as an alternative to “quantum orthodoxy” are defended by Gross and Levitt in *Higher Superstition*, by S. Goldstein in an article in *The Flight from Science and Reason*, and by Bricmont in a separate publication.<sup>79</sup> As a mathematician I can only be cheered by the news that Dürr, Goldstein, *et al.* may have constructed a consistent deterministic account of quantum mechanics, and the fact that it comes at the cost of adding variables that can never be measured is the least of my concerns. As a mathematician, furthermore, the question is strictly none of my business, but it does seem to me that (1) sociologists of science are going to be hard-pressed to figure out who is the mainstream and who is the margin in this debate and (2) that the primary motivation for hidden variables theories seems to be the desire to preserve determinism at all costs is probably the strongest argument I’ve yet seen in favor of the social constructivist view of science.

## ETHICS

The publication of *IS* in France was celebrated by a discussion in a small bookstore near Jussieu, in which one author after another expressed regret at the distortions and bitterness that make real dialogue impossible in the shadow of the science wars. François Gèze, the director of *La Découverte*, made clear his sincere hope that publishing *IS* would contribute to raising the intellectual and ethical level of the debate. More than one of the *IS* authors expresses a similar concern for ethics; Salanskis’ call for an “epistemology of reading” is equally a plea for an ethics of reading. Toward the end of *IS*, Isabelle Stengers makes the point, which someone surely ought to have made before, that the negative reaction of many scientists to science studies is not necessarily a sign that the sociologists are on the right track; that the feeling of scientists that their work is being treated with contempt should be taken seriously.<sup>80</sup>

Stengers’ article proposes to imagine a possible “peace” in place of the science wars, and she writes of ethics explicitly. There are reasons to be skeptical: her article is often rather nasty in its description of the reaction of scientists, who “have never read [Lacan, Kristeva, Deleuze, or Bergson] and certainly never plan to read them.”<sup>81</sup> And in a September 1998 meeting organized by the association “Femmes et mathématiques,” Stengers announced baldly that, “of course,” Sokal is being “piloted” by Weinberg. This is not only offensive; it also shows a profound ignorance of and contempt for Sokal’s political motivations, with which the North American left broadly identified, for reasons having nothing to do with Weinberg or the superconducting supercollider. Disregard for the U.S. political context is all

too frequent in the French reaction to Sokal, among supporters as well as critics. At the bookstore meeting near Jussieu, Lévy-Leblond saw the Gross-Levitt book (see note 8) as an expression of the extreme right (!), and even François Gèze, whose good faith seems beyond question, read the Sokal affair as part of the campaign against political correctness. More subtly, Patrick Petitjean's generally excellent article in *IS* on the historical tendency of the French left to identify science uncritically with social progress (and socialism) slips into the error of suggesting that Sokal and Bricmont want to restore this identification<sup>82</sup>; if *FN* makes anything clear, it is that this is not the case.

There is no intellectual excuse for these mistakes. *IS* itself contains an accurate account by Michel Pierssens of some elements of the political background of cultural studies and the academic scene in North America.<sup>83</sup> But this is a war. Thus Michel Callon, echoing Latour's comments about cultural protectionism, writes "there is an increasingly strong and direct link between forms of competition in the markets and strategies of cooperation and rivalry in the academic world"<sup>84</sup>; Jon Henley wrote in the *Guardian* that "modern French philosophy is a load of old tosh"<sup>85</sup>; and, inevitably, someone (in this case Gautero) talked about MacDonald's and Coca-Cola.<sup>86</sup>

The martial mood is based on a perception of the urgency of mounting a defense of one's epistemic perspective in the face of an enemy with whom negotiation is impossible. Ethical considerations go by the board; as in the Balkans, yesterday's aggrieved becomes today's aggressor.<sup>87</sup> Paul Gross, speaking of the 1995 New York meeting on which *The Flight from Science and Reason* was based, claimed that "The strong constructivists have been in charge, in control of departments of sociology, anthropology, and, to a very significant extent, history of science nationwide [in the U.S.] and in Western Europe for 15 years... We are the oppressed. We have to find a voice, and so this meeting is our voice."<sup>88</sup> It is unsettling to compare this with quotations from the other side. Jon Agar, expressing misgivings about the prospects of scientists adopting the STS perspective, wrote "The resources open to scientists to construct their authority are greater than those open to science studies - and they will do so to reproduce science."<sup>89</sup> And in *IS*, Jurdant reports some scientists' second thoughts about Sokal's hoax: "Il n'est pas toujours facile d'être du côté des plus forts quand il s'agit de rire avec eux aux dépens des plus faibles"<sup>90</sup>.

The sense of urgency can only be proportional<sup>91</sup> to the danger, which has been described in outlandish terms on both sides. Once you accuse your opponent of being a charlatan, or an idiot, or a power-hungry authoritarian, or a dupe, the usual rules of argument no longer apply. The prize has to go to Mario Bunge, whose call to "expel the charlatans from the university" warns that the alternative is to "put modern culture at risk... undermine modern civilization... prepare for a new Dark Age."<sup>92</sup> On the other side, my (least) favorite is

Bruce Robbins' claim that "what is really expressed by the angry tirades against cultural politics that have accompanied the Sokal affair is a longing for the days when women were back in the kitchen and it was respectable to joke about faggots and other natural objects of humor."<sup>93</sup>

Most science warriors express themselves more moderately, but the distrust of their colleagues at the other end of campus is palpable. Speculations as to why this may be the case have ranged from the pop psychology of Gross and Levitt's *Higher Superstition*<sup>94</sup> to Dorothy Nelkin's unsubstantiated claim that the science wars are "really about" declining government funding.<sup>95</sup> It seems most scientists — the ones too busy to look into the matter — have swallowed the notion that French philosophy (or cultural studies, or science studies) really is the work of cretins and poseurs on the one hand and their intimidated acolytes on the other. The symmetric bad faith accusation — for instance, that the scientists in the science wars are acting to defend their undeserved authority or their military budgets or their secular religion — apparently circulates equally unchallenged.

In the introduction, Sokal and Bricmont claim that they "do not find...of very great interest" the question whether the abuses they believe they have identified "arise from conscious fraud, self-deception, or perhaps a combination of the two." [p. 6]. Apart from a few remarks of this kind, and rather to their credit, the two physicists do not speculate much about ulterior motives.<sup>96</sup> The same cannot be said for *IS*. Dahan-Dalmedico and Pestre ask whether Sokal and Bricmont are not motivated by "a more irrepressible desire to control the proliferation [foisonnement] of the social and of thought at work, a desire, exorbitant in its pretention that would like to see "science" (?) control words and their proliferation..." Fujimura's article is entitled "The authority of knowledge in question" and summarizes Sokal's hoax as "a tool of social control, a form of discipline."<sup>97</sup> Treating motivations at this level wouldn't work as a plot device in a grade B thriller. But because a war's on, a number of *IS* authors, like Robbins and Nelkin, favor explanations that place Sokal and Bricmont and their supporters in a bad light. Thus, *FN*'s explicit distancing itself from the "boundary transgressing" of sociobiology and neurobiology is passed over in silence.<sup>98</sup> Indeed, if it weren't for the war, the scientists' ulterior motives in the science wars might more charitably be sought in Weinberg's commitment, shared with Stephen Jay Gould, to keeping creationism out of the schools; in Gould's work on the IQ controversy, in Richard Lewontin's exposure of the role of the "genetico-industrial complex" in the most recent developments in agriculture, and in Serge Lang's successful campaign, largely based on Neal Koblitz' research, to keep Samuel P. Huntington out of the National Academy of Sciences.<sup>99</sup>



## HOPEFUL SIGNS

Sokal and Bricmont express the sincere wish to create “a real dialogue between the ‘Two Cultures’” (p. 183), to “put aside the ‘science war’” (p. 185). *IS*’s dust jacket concludes in almost identical terms: “pour en finir enfin avec la ‘guerre des sciences’”. And yet both books are pure science wars products. The reason is not hard to find: *FN*’s goal is to discredit a certain list of authors, at least insofar as they use the vocabulary of science; *IS*’s goal is to discredit *FN*. Here and there both books give the occasional nod to the legitimate concerns of the other side, and Andrée Bergeron admits in *IS* to having some sympathy, as a scientist, with *FN*’s goals. But, Stengers’ talk of peace notwithstanding, negotiation is not on either book’s agenda. *A House Built on Sand*, another recent entry on the pro-reality side, edited by Noretta Koertge, and featuring articles by Sokal, Gross, Levitt, and Kitcher, among many others, is in some ways even more disappointing. Though many (by no means all) of the contributions are substantial, as a whole the collection reads like a monologue, like “Science Wars Minus One.” As in *The Flight from Science and Reason*, only one camp gets to present its position, and an open-minded reader can’t help thinking this is a distortion of the debate, that the other side can’t possibly be guilty of such gross errors. Indeed, Kitcher objects to the book’s title and calls his contribution “A Plea for Science Studies,” making himself the advocate for the position that “The work done in [certain branches of science studies is] an important part of scientific activity and often continuous with science itself.” And Sokal adds, “I agree with nearly everything in” Kitcher’s article.<sup>100</sup> But the practitioners of science studies were not invited, and one gets no real sense of what the debate is about.

Fortunately, there are venues, far from the noise of the science wars, where real differences in perspective as well as problems stemming from the difficulty of translation, particularly between disciplines, are being discussed civilly. At the very least, the simultaneous presence of specialists holding different perspectives is a guarantee against the crudest distortions. If you have learned about science studies from Gross and Levitt’s *Higher Superstition*, I urge you to look at the recent collection *The Disunity of Science*, edited by Peter Galison and David J. Stump. The first thing you will notice is that the debate over relativism and realism is a central theme within science studies, which in no way resembles the abridged version criticized in *FN*’s philosophical intermezzo.<sup>101</sup> Stump’s article is largely a call to “end the tiresome battles over realism and relativism.” The second thing you will notice is that science studies is by no means monolithic, that there really are differences of opinion. Arthur Fine, a philosopher sympathetic to constructivist sociology of science, nevertheless asserts that “constructivists write a great deal of nonsense” on the question of relativism, and

attributes this in part to their “romantic and apocalyptic...rhetorical style”. Stump’s Afterword calls for “a continuing debate on [realism, the rationality of science, and relativism], but a positive and consensus-building debate, not a hostile and divisive debate such as has occurred in the past.”<sup>102</sup>

“Romantic rhetoric” serves better than “higher superstition” or “fashionable nonsense” to describe the sentiment expressed in the Andrew Ross quotation with which this article began, and unlike the other two is compatible with dialogue, of which *The Disunity of Science* provides a prime example. Stump notes that “Philosophers, historians, and sociologists all have something to add to the new science-studies philosophy, and what has been a hostile debate is being replaced by signs of cooperation.” [p. 256] All three disciplines were well represented in the 1991 workshop on which *The Disunity of Science* was based but, with the hindsight provided by the science wars, the absence of scientists seems, or should seem, a gross oversight. Neither scientists nor sociologists were invited to the discussion recently published in *Le Débat* under the heading, “How to write the History of Science?.” But historian Dominique Pestre represented sociologists in explaining the new approaches in the history of science, while philosophers Catherine Chevalley, Pierre Jacob, and Gérard Jorland raised objections similar to those expressed by scientists in the wake of the Sokal affair.

Pestre, a contributor to *IS*, restates some of the themes developed in his article with Dahan-Dalmedico. Away from the Science Wars, though, the arguments have much more room to develop. The exchange in *Le débat* makes it clear that, once one gets past the “romantic rhetoric” and the rather arbitrary assignment of political labels on the basis of philosophical positions, the intellectual conflict underlying the science wars hinges on the fact that historians and philosophers (and sociologists and scientists) simply don’t choose to “cut up” [découper] the phenomena in the same way (Pestre, p. 100). The philosophers want to know, as Chevalley asks, “Peut-on dire que la [nouvelle historiographie scientifique] décrit correctement l’activité scientifique en général?” Jorland asserts that “C’est parce que les propositions scientifiques s’affirment universelles qu’elles intéressent la philosophie.” And for Jacob: “la question soulevée par le développement scientifique n’est pas tant de comprendre les désaccords entre les spécialistes que leurs convergences.”<sup>103</sup> As it happens, this is the question I find most interesting, but it’s certainly not the only interesting question, it’s not a question to which anyone has ever proposed a credible answer<sup>104</sup>, and I would never dream of assigning it to the sociologists or historians as their sole legitimate task.<sup>105</sup> In his response to the philosophers, Pestre defends the historians’ perspective in the following terms: “Le choix de ne pas placer comme préalable la question épistémologique (qui, du social ou de

la nature, détermine le savoir?) ou, plus exactement, de la juger indécidable, est une renonciation à l'absolu et vaut acceptation que l'homme est à jamais un être limité."<sup>106</sup>

There are even signs of dialogue between scientists and sociologists of the SSK perspective. Physicist David Mermin, a contributor to *IS*, was invited by *Social Studies of Science* (SSS) to review *Scientific Knowledge* by Barnes, Bloor, and Henry (abbreviated BBH), the most recent general expression of the Edinburgh school. Mermin took this invitation as an opportunity "to set out, as a basis for subsequent discussion, several examples where I find [the BBH] picture of scientific knowledge or the process of creation to be deficient or misleading." Bloor and Barnes were given the chance to respond to Mermin's criticisms, and this was followed by Mermin's reply to their response.<sup>107</sup> As in *Le Debat*, the exchange is cordial and respectful; Mermin and Bloor go out of their way to adapt their arguments to each other's languages. The resulting dialogue is deeply frustrating, in that it reveals how difficult it is to eliminate misunderstanding — somehow questions of methodology or disciplinary perspective keep being mistaken for questions of truth claims, in spite of the best efforts on both sides to avoid such slippage. Bloor writes that "when ideas are expressed in a slightly unfamiliar idiom they can become... completely unrecognizable." But the very existence of the dialogue, despite the science wars' distortions, is an immensely hopeful sign, and Bloor continues, "Perhaps this is the strongest testimony possible to the importance, in the present circumstances, of learning to switch back and forth between the different perspectives of the natural and social scientist."<sup>108</sup>

The BBH book begins by regretting that "some scientists and philosophers have assumed that since we neither praise nor defend science our objective must be to subvert it," an implicit acknowledgment of the need for more dialogue of the kind described in the last paragraph. Nevertheless, there has been rhetorical backsliding by members of the SSK community. In a second review of BBH published in the same issue of SSS, Jon Agar, criticizing what he perceives as the book's defensiveness, claims "their stance of disinterestedness makes the authors very shy of suggesting that their arguments could be used to criticize science.... The idea that theory is neutral is reminiscent of the American gun lobby slogan: 'guns don't kill people, people do.'" Why would anyone want to criticize "science" as such, if not out of a taste for romantic and apocalyptic rhetoric? And Bloor himself, reviewing Barbara Herrnstein Smith's *Belief and Resistance*, wonders "What if scientists need to believe a mythical and false ideology, because they would lose motivation without it?" He then goes on to praise Herrnstein Smith as an example "of persons who can reason cogently and clearly without the prop of myth."<sup>109</sup> Here we see how adherence to a specific epistemological position is elevated to the level of a moral imperative. Much science wars rhetoric on both sides is really about conflicting views of

the good, giving the lie to Sokal and Bricmont's *a priori* isolation of "cognitive" from moral and aesthetic factors.<sup>110</sup> Though I wouldn't go so far as to write a book about it, I happen to find rationalism much more ethically compelling than skepticism. This is why I do not secretly loathe myself for doing mathematics. When Borel expresses a core belief of mathematicians, writing "I do believe in the self-correcting power of mathematics" in the BAMS exchange, it would not only be crude and condescending (from what privileged vantage point?) to treat this as an instance of "myth" or "false ideology;" a sociology that feels the need to do so is suspect on intellectual as well as moral grounds.

Few of us would choose to treat our belief that Wiles proved Fermat's last theorem as "a mythical and false ideology," but is it possible that our attempts to justify this belief always involve an element of self-delusion? And how are we to convince a skeptical outsider that this is not the case? The only reasonable answers that come to mind are empirical in nature, and specifically historical and sociological, rather than philosophical.<sup>111</sup> We would have to pay attention to the question of how knowledge is transmitted among mathematicians. Fermat's last theorem provides a particularly good test case. Wiles' proof generated an unprecedented<sup>112</sup> number of reports, survey articles, colloquium talks, working seminars, graduate courses, and mini-conferences, as well as books, newspaper and magazine articles, television reports, and other forms of communication with non-mathematicians. Not to mention the spate of announcements, designed to impress public policy-makers and the public at large, citing Wiles' work as proof that mathematics "has never been healthier." Has anyone been keeping track of all these incitements to belief formation, checking them for contamination by myth and false ideology?

Studying questions like these provides a second answer to the thought experiment proposed above, complementary to the answer we would naturally provide based on our experience as mathematicians, and potentially just as interesting. Leaving aside romantic rhetoric, these two answers are not in competition, much less on opposite sides of a battlefield. Arriving at the second answer would be the work of sociologists. For this, full cooperation with mathematicians would be necessary. The examples just cited provide hope that such cooperation may be possible.

With this in mind, and in the spirit of Paul Erdős rather than Professor Fredkin, I offer \$100<sup>113</sup> to the first person who programs a computer to compose recognizable contributions to the Science Wars. The genre is now sufficiently extensive that this should be scarcely more difficult than the Postmodernism Generator so often cited by science warriors,<sup>114</sup> and should provide at least as much fun. Human combatants will be

less tempted to take up arms when the battlefield is fully automated. Our energies will then be free for more interesting matters.

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<sup>1</sup> In 1996, Sokal published an article entitled “Transgressing the Boundaries: Toward a Transformative Hermeneutics of Quantum Gravity,” in a special issue of the journal *Social Text* entitled “Science Wars.” A few weeks later, Sokal revealed that the article was a parody of an intellectual style he and others call “postmodernism.”

<sup>2</sup> Unlike Sokal, most of my work was based in the U.S.

<sup>3</sup> Given the media distortions surrounding the so-called Science Wars, it is difficult to gauge the prevalence of this position among practitioners of Science Studies, or even whether Andrew Ross takes it seriously. Compare this to an earlier statement by Cornel West: “If science is, as Rorty notes, a ‘value-laden enterprise,’ is there an ideological character intrinsic to the very methods of the natural sciences owing to an agreed-upon conception of and disposition toward nature which may promote the domination not only of our environment but also those people subsumed under the rubric ‘nature’ such as women, non-Europeans, and even ‘earthy’ workers?” (J. Rajchman and C. West, “Post-Analytic Philosophy,” 1985, p. 268). Nicaraguan educators in the mid 1980s answered this question with a sophisticated pragmatic approach to basic science as a means to escape dependence on expensive imported technology. Some of their debates were recorded in *Nexo*, a theoretical journal published by the Universidad Nacional de Ingeniería, with the support of Italian NGOs. As far as I know, *Nexo* has never been cited in the science wars debate, not even by those claiming a “post-colonial” perspective.

<sup>4</sup> *New York Times*, 5/21/96.

<sup>5</sup> K. Hirschkop, “Cultural Studies and Its Discontents, in *Social Text*, **50**, 131-133 (1997).

<sup>6</sup> Lacan, Kristeva, Irigaray, Latour, Baudrillard, Deleuze, Guattari, Virilio, and secondarily Lyotard and Debray.

<sup>7</sup> (Nagel, *New Republic*)

<sup>8</sup> Authors most notably of *Higher Superstition*, the *locus classicus* of science wars confusion on the science side, published in 1994.

<sup>9</sup> ." (Dawkins, *Nature*, 7/9/98)

<sup>10</sup> This dubious categorical judgment has been placed near the beginning of the article for the convenience of journalists too lazy to read through to the end. Not that I really expect any journalists to read this article, on a topic whose time may have passed.

<sup>11</sup> “[I]f these writers have become international stars primarily for sociological rather than intellectual reasons, and in part because they are masters of language and can impress their audience with a clever abuse of sophisticated terminology--non-scientific as well as scientific—then the revelations contained in this essay may indeed have significant repercussions.” *FN*, p. 8. Elsewhere in *FN* and in published interviews, Sokal and Bricmont insist that their objective is the more modest one of pointing out abuses of science, and claim not to judge the substantial works of the authors in question. As Yves Jeanneret points out in *L’affaire Sokal ou la querelle des impostures*, (PUF, 1998), discussed below, this only adds to the confusion (pp. 133-136). Jeanneret’s book also contains the most sustained attempt to theorize the unity of the Sokal affair, including *FN*. It should be obvious to everyone that the book would not have attracted such attention on the basis of the more modest goal alone.

<sup>12</sup> Sokal and Bricmont actually “warn the reader against the temptation to lump [the authors] together in a single category.” Of course, the book’s success is largely conditioned on the reader’s ignoring the warning, and indeed calling into question the whole of “French thought.”

<sup>13</sup> A prolific contributor to the genre, unfortunately.

<sup>14</sup> Incredibly, the only explanation offered for combining these two targets in a single book is that “Sokal stumbled on most of these texts in the course of writing his parody” (p. 15).

<sup>15</sup> Latour: *Le Monde*, 18 janvier 1997. Kristeva: Réponse à Alan Sokal et Jean Bricmont: Une désinformation, *Le Nouvel Observateur*, 23 septembre 1997 (the issue with the screaming cover); *Occhione della Sera* [date]. Lévy-Leblond: Le Cow-boy et l’apothicaire: L’alliance du revolver épistémologique et du discours scientifique, *La Recherche*, décembre 1997. Fleury and Limet: *Libération*,

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<sup>16</sup> Of course the “affaire” doesn’t break down neatly along national lines: the French reaction to Sokal’s initial hoax was evenly divided, and a few intellectuals, most notably J. Bouveresse and J.-F. Revel, were pleased at the opportunity Sokal and Bricmont provided to denounce what they saw as the “impostures” of certain of their compatriots. And the London Review of Books published a quite hostile review of the British edition, provoking an unusually angry reaction on the part of readers.

<sup>17</sup> To be fair, Sokal and Bricmont do distance themselves from their “more enthusiastic supporters” on the very first page of their preface. But Dawkins’ review appears on the dust jacket and the publisher sent Nagel’s article along with my review copy.

<sup>18</sup> Sokal’s hoax had already taken on national political significance the previous spring, with an article by Régis Debray using it as a pretext for attacking Pierre Bourdieu’s book *Sur la télévision* (*Le Monde*, March 18, 1997). Kristina Zarlengo’s review of FN in *Salon* (November 2, 1998) indicates that the publishers had run out of galleys four months ahead of publication.

<sup>19</sup> The major daily newspapers and a few magazines reviewed these books, but the Sokal-Bricmont book generated a press packet is easily 20 times thicker than the competition. What’s more, the reviews of the responses were mostly taken up with a review of the Sokal affair and the Sokal-Bricmont book; hardly any space was devoted to the new books.

A third response, Marc Richelle’s *Défense des Sciences Humaines*, was largely favorable to Sokal and Bricmont.

<sup>20</sup> IS, p. 13.

<sup>21</sup> As Jeanneret puts it, “la provocation se vend bien, en édition et en recherche.” (p. 32). Jeanneret’s suggestion for a constructive alternative is worth quoting in full: “Il faut demander à cette querelle ce qu’elle peut nous donner. Le procès en culpabilité, de part et d’autre, ne donnera matière qu’à esquives et paradoxes; mal engagé, il ne saurait déboucher sur aucune certitude. En revanche, le d’ebat qu’il provoque concerne des enjeux très importants, dès qu’on accepte de considérer les accusations mutuelles entre les protagonistes successifs, non comme des cas à trancher, mais comme des points de vue à comprendre. Car, discutant du sens de la pseudo-expérience, les uns et les autres écrivent, ou défont, une conception de ce que sont des influences intellectuelles. Les centaines de pages écrites au fil de cette querelle pourraient ainsi ne pas avoir \’et\’e produites en pure perte.” Jeanneret, p. 65. Other hopeful avenues of dialogue will be discussed at the end of the article.

Jurdant also refers to the media “phenomenon that makes all intelligent reflection improbable or, at any rate, difficult” (p. 11), which he identifies as one of the main obstacles to putting IS together. The collection concludes on an interesting article by Allain Glykos, comparing the Sokal affair (as media event) with a rather different media scandal called “The crisis of contemporary art,” which goes back to the early ‘90s. In the process, Glykos asks whether the media “are able to found normatively the validity and legitimacy of debates in our modern democracies?” The reviewer in *Sciences et Avenir* (October 1998) didn’t much care for this question, and attacked Glykos with a paragraph of self-righteous and irrelevant abuse, in the process proving Glykos’ point.

<sup>22</sup> By contrast, Baudrillard, Virilio, Irigaray, and Kristeva are all but ignored in IS. Michel Pierssens, in his contribution to IS on the North American context of Sokal’s hoax, sees Baudrillard as more influential in the U.S. than in France, “his articles in *Libération* having a perfect predictability that removes all their surprise and impact.” (p. 107) When I spoke with Sokal in 1997 I tried to convince him that Baudrillard’s scientific metaphors are seen as purely stylistic devices; this is presumably why I am listed among the 60-odd names on the acknowledgment page.

<sup>23</sup> I find Nathalie Charraud’s explanation of Lacanian topology in “Mathématiques avec Lacan” less convincing; more on this below.

<sup>24</sup> Referring to Leibniz, G. Châtelet wrote “c’est bien le concept métaphysique de ‘monade’ qui permet de comprendre le calcul différentiel.” *Les Enjeux du Mobile*, p. 24.

<sup>25</sup> *Différence et Répétition*, p. 221, my translation.

<sup>26</sup> Footnote 184, p. 148. Several colleagues have pointed out that the same comments applies to Abraham Robinson’s non-standard analysis.

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<sup>27</sup> Deleuze, *Différence et répétition*, p. 67; FN, note 211, p. 162; Salanskis, *op. Cit.*, p. 176.

<sup>28</sup> Nor a “mixture” of “charlatan” and “idiot”, according to Nagel; see note 7. I don’t pretend to know the correct reading of this sentence; I freely admit to being out of my depth here, being unfamiliar with the history of the concept of continuity in the western tradition even in translation, much less in the Greek and Latin originals. As for *puissance*, this is at worst ...relatively technical;  $\mu$  is the 12<sup>th</sup> entry in the glossary of Aristotle’s *Metaphysics*. Of course, the word has other meanings, and even granting Salanskis’ reading one might find Deleuze’s casual tossing around of such terms tiresome, or snobbish; but these objections would be on literary, not intellectual grounds.

<sup>29</sup> The philosophical reductionism for which Weinberg gives “two cheers”, in *Dreams of a Final Theory*, begs precisely this question, insofar as reducing molecular biology to physics, say, depends on heuristic extrapolations from the existence and uniqueness theorems of ordinary differential equations.

<sup>30</sup> Deleuze, *op. Cit.*, p. 4. Needless to say, these remarks are *not* quoted in FN.

<sup>31</sup> Several contributors to this volume refer to Deleuze’s use of the term “singularity,” which they seem to read as a metaphor. See also note 76, below.

<sup>32</sup> There are many circles in Euclid, but no pi, so I can’t think of any other reason for Sokal to have written “Euclid’s pi,” unless this anachronism was an intentional part of the hoax. Sokal’s full quotation was “the of Euclid and the G of Newton, formerly thought to be constant and universal, are now perceived in their ineluctable historicity. “ But there is no need to invoke non-Euclidean geometry to perceive the historicity of the circle, or of pi: see Catherine Goldstein’s “L’ un est l’ autre: pour une histoire du cercle,” in M. Serres, *Elements d’histoire des sciences*, Bordas, 1989, pp. 129-149.

<sup>33</sup> This is not mere sophistry: the construction of models over number fields actually uses arguments of this kind. A careless construction of the equations defining modular curves may make it appear that pi is included in their field of scalars.

<sup>34</sup> Unless you claim, like the present French Minister of Education [at the time of writing, i.e. 1999], that real numbers exist in nature, while imaginary numbers were invented by mathematicians. Thus  $\pi$  would be a physical constant, like the mass of the electron, that can be determined experimentally with increasing accuracy, say by measuring physical circles with ever more sensitive rulers. This sort of position has not been welcomed by most French mathematicians.

<sup>35</sup> Cf. M. Kline, *Mathematics The Loss of Certainty*, p. 324.

<sup>36</sup> Compare Morris Hirsch’s remarks in BAMS April 94.

<sup>37</sup> IS, p. 38, footnote 26. Weinberg’s remarks are contained in his article “Sokal’s Hoax,” in the *New York Review of Books*, August 8, 1996.

<sup>38</sup> Metaphors from virtual reality may help here.

<sup>39</sup> *Matière à penser*. Odile Jacob, 1989. For example, Connes writes, “Je pense que le mathématicien développe un “sens”, irréductible à la vue, à l’ouïe et au toucher, qui lui permet de percevoir une réalité tout aussi contraignante mais beaucoup plus stable que la réalité physique, car non localisée dans l’espace-temps” (p. 49). In fact, the debate is even more complex, because Changeux often comes across as a social constructivist, though one who sees society as materialized in the human brain; thus he sees mathematical objects as “‘représentations culturelles’ susceptibles de se propager, se fructifier et de proliférer et d’être transmises de cerveau à cerveau.

<sup>40</sup> See Barry Mazur’s astonishing “Imagining Numbers (particularly  $-15$ )” for (among other things) an attempt to get beyond sterile ontological debates.

<sup>41</sup> A recent example: “...as the mathematician Gödel showed, inconsistency is an inevitable trait of any self-sustaining system built up out of consistent parts.” K. Kelly, *Out of Control*, (1994), p. 394. Kevin Kelly, executive editor of *Wired*, presumably has understood Gödel’s proof, as did molecular biologist François Jacob, when he used Gödel’s theorem to argue that the human mind will always resist science (*La logique du vivant*, 1970, quoted in *Matière à Penser*, p. 205). “Gödelizing,” as a repentant Debray puts it, may legitimately be criticized as a cliché, but this is primarily a literary judgment.

<sup>42</sup> This point is made by Amy Dahan-Dalmedico and Dominique Pestre in their contribution to IS, p. 87.

<sup>43</sup> See Salanskis’ comments on this point, IS, p. 184 ff.

<sup>44</sup> I recently made explicit use of the axiom of choice for the first time in my professional life, in an article with Richard Taylor in which we identify  $\mathbf{C}$  with  $\overline{\mathbf{Q}_\ell}$ . In “La Conjecture de Weil. II,” Deligne writes “Je ne prétends pas croire à

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l'existence d'isomorphismes entre  $\overline{\mathbb{Q}}$  et  $\mathbb{C}$ , et ceux-ci ne sont qu'une commodité d'expression." (*Publ. Math. IHES*, **52**, (1981) p. 332). I suspect Deligne's position is widely shared, but not being a sociologist I wouldn't know how to go about determining whether this is the case.

<sup>45</sup> Kline, *op. Cit.*, pp. 5, 278, 6.

<sup>46</sup> There is no room here to discuss FN's third intermezzo, on "Chaos Theory and 'Postmodern Science'" — again, the reader can probably guess its contents from the title — except to say that, if the philosophical significance of chaos theory has been exaggerated, this is largely the fault of (parts of) the scientific community itself. This point is made clearly by Dahan-Dalmedico and Pestre in *IS*, pp. 95-96. See also Note 74, below.

<sup>47</sup> R. Krauss, "Sculpture in the Expanded Field," in H. Foster, ed. *The Anti-Aesthetic* (1983), p. 37; J. Clifford, "Collecting Art and Culture," in R. Ferguson et al., eds., *Out There* (1990), p. 146 ff. From Krauss's article I learned that the Klein group is also called the Piaget group.

<sup>48</sup> In other words, if there is a problem with Lacan's work, it's pointless to look for it in his topology.

<sup>49</sup> FN, p. 195. See also P. Boghossian, "What the Sokal Hoax Ought to Teach Us," in N. Koertge, ed., *A House Built on Sand*, (1998), p. 27 ff. Sokal mentioned this example in a debate with Andrew Ross, who (according to Boghossian) didn't help matters by using it as an excuse for demagogically accusing Sokal of putting Native Americans "on trial."

<sup>50</sup> Clifford, *loc. cit.*, p. 169, footnote 69.

<sup>51</sup> Or their audience of having redefined truth: FN, p. 195, note 258.

<sup>52</sup> As Barry Mazur reminded me, "interest" in this context is generally used as shorthand for an intellectual criterion, such as "enhancement of understanding" (see Thurston's comments, quoted below). Such a criterion is by nature not well-defined, yet we have the sense that we know what it means.

<sup>53</sup> Actually a "major mathematical discovery" [Fredkin's website]

<sup>54</sup> Quoted in Dominique Lambert, "L'incroyable efficacité des mathématiques," *La Recherche*, janvier 1999, p. 50. This article also includes a detailed comparison among various contending theories of the nature of mathematics. Truth is also not what interests Deleuze and Guattari: "ce n'est pas la vérité qui inspire la philosophie, mais des catégories comme celles d'Intéressant, de Remarquable ou d'Important qui décident de la réussite ou de l'échec." *Qu'est-ce que la philosophie*, (1991) p. 80.

<sup>55</sup> A course on foundations of mathematics is not a core requirement in any university with which I am familiar. One would think this fact would be of interest to sociologists of science, but I have not seen it addressed in the literature. As a graduate student at Harvard, I only saw foundations actively discussed in the graffiti in the men's room on the second floor of the science library.

<sup>56</sup> The debate provoked by the Jaffe-Quinn article is taken up in a recent article by Leo Corry, "The Origins of Eternal Truth in Modern Mathematics," *Science in Context* **10** (1997), 297-342. Corry is so intent on developing his theme (that "the idea of eternal mathematical truth...has not itself been eternal") that he completely misses the near absence of the word "truth" from the debate, claiming that "the eternal character of mathematical truth" was "implicit[] at the very least" in the Jaffe-Quinn proposal."

<sup>57</sup> Glimm goes on to say that mathematical truth is to be compared with the stronger standard of truth in science, "the agreement between theory and data." The whole discussion can be found in *Bulletin of the AMS*, July 1993 and April 1994.

<sup>58</sup> What they really think hardly matters. The "strong program" of Bloor and Barry Barnes is criticized at length in the philosophical "intermezzo" of FN; but Sokal "agrees with nearly everything" in Kitcher's attempt to "occupy middle ground" in Koertge, *op. Cit.* We return to Bloor and Kitcher at the end of this essay.

<sup>59</sup> D. Bloor, *Knowledge and Social Imagery* (U. of Chicago press, 1991), chapters 5-8; P. Kitcher, *The Nature of Mathematical Knowledge*, (Oxford University Press, 1984).

<sup>60</sup> Thurston's extended response to the Jaffe-Quinn article does refer to truth but he seems more interested in knowledge and especially on understanding; see below.

<sup>61</sup> Chapter 3 of Kitcher's *op. Cit.* is devoted to a refutation of Kantian or Platonist intuition as a means to mathematical knowledge, and what we mean when we use the word informally is presumably even less defensible. The quotation is from FN, pp. 143-44, note 183.

<sup>62</sup> Thurston, *BAMS*, April 1994. Thurston's comment referred to the computer-assisted proof of the Four Color Theorem, and echo Deligne's remarks on the same topic, quoted by Ruelle in *Chance and Chaos*, pp. 3-4.



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<sup>63</sup> Coleman, "Manin's proof of the Mordell conjecture over function fields," *L'Enseignement Math.*, **36** (1990), p. 393.

<sup>64</sup> The text book is G. Cornell, J. H. Silverman, G. Stevens, eds.: *Modular Forms and Fermat's Last Theorem*, Springer-Verlag (1997). If I believe, or understand, or have some meaningful relation to the proof, it's mainly because I have been collaborating with Richard Taylor to generalize parts of the argument to automorphic forms of higher dimension.

<sup>65</sup> Horgan, *Scientific American*.

<sup>66</sup> FN, p. 91.

<sup>67</sup> François Vosse, *Histoire du Structuralisme*, Eds. La Découverte (1992), p. 273.

<sup>68</sup> I called Caroline van der Voort, the *Vogue* editor who produced the special issue on science, and asked for an explanation. The motivations were apparently quite simple and unpretentious: she needed an original theme, she found the graphic qualities of numbers appealing, etc.

<sup>69</sup> Some French management theorists do seem to like the work of Latour and his collaborator M. Callon: see H. Amblard, P. Bernoux, G. Herreros, and Y-F. Livian, *Les nouvelles approches sociologiques des organisations*, Eds Seuil (1996), pp. 127-185. And the feeling appears to be mutual: see Callon's comments in *IS*, pp. 264-266, e.g. "il existe un couplage de plus en plus fort et direct entre les modalités de la compétition sur les marchés et les stratégies de concurrence et de coopération dans le monde académique."

<sup>70</sup> In "Changing Axes", *Social Studies of Science*, **28/4** (August 1998), p. 635.

<sup>71</sup> *IS*, p. 92.

<sup>72</sup> "Le réflexion épistémologique dans le domaine des sciences humaines est tributaire des mutations en cours dans les sciences "dures", et sur ce plan on constate la même inflexion formaliste." And "[L]'idéologie bourbakiste a certainement fortement contribué à forger la mentalité et l'activité structuraliste..." Vosse, *op. Cit.*, p. 111, p. 272.

<sup>73</sup> *IS*, p. 93.

<sup>74</sup> A reasonable start is made by David Aubin in "The Withering immortality of Nicolas Bourbaki. A cultural connector at the confluence of mathematics, structuralism and the Oulipo in France", *Science in Context* **10** (1997), 297-342. Aubin presents a good review of the interaction between Bourbaki and Levi-Strauss, Piaget, and Oulipo, pointedly refusing to claim that the latter were a consequence of the former. For my taste, Aubin follows too closely Jean-François Lyotard's evaluation (in *La Condition Postmoderne*) of fractals, chaos, and Thom's catastrophe theory as "symptomatic of a 'postmodern science'". It is symptomatic that Aubin thanks no mathematicians in his acknowledgments, though his article is based on a Princeton Ph.D. thesis. Nevertheless, it is interesting to contrast Aubin's reading of Lyotard with that of Sokal and Bricmont in their "intermezzo" on Chaos, and even more interesting to compare Aubin's account of Bourbaki's decline (as "cultural connector") with Cartier's reminiscences in the *Intelligencer* [date??].

<sup>75</sup> Two of my French colleagues, who would probably prefer not to be identified here, were involved in this project and seem none the worse for the experience. The project, on the history of the rigorous development of calculus, was published as *Philosophie et calcul de l'infini* (Maspero, 1978). Pierre Raymond, director of the project, wrote, in response to unspecified texts by Deleuze, that "les sciences sont le moyen fondamental et particulier du combat révolutionnaire", a comment that would Sokal and Bricmont would probably view as an abuse of science.

<sup>76</sup> Deleuze's discussion of singularities in *Différence et Répétition*, as well as in the passages from *La Logique du Sens* ridiculed in FN, pp. 165-166, is based almost word for word on Albert Lautman's *Essai sur l'Unité des Mathématiques*, as Salanskis points out (and Deleuze gives the reference on p. 230 of *Différence et Répétition*). Lautman's essays, written in the '30s and '40s, were reissued in 1977 by the publisher 10/18 with a forward by Dieudonné. Lautman's discussion of the role of singularities of systems of differential equations in organizing structure, the point of departure for Deleuze's "lucubrations," as Sokal and Bricmont call them, should look familiar to readers of René Thom's theory of catastrophes, or for that matter to anyone who has studied Morse theory.

<sup>77</sup> More precisely, they devote one sentence to the history, on the bottom of p. 191 of FN, referring to Lyotard's postmodernism as a post '68 "reaction against the scientism of certain rather dogmatic strains of structuralism and Marxism" (FN, p. 191) but this effectively erases the 1970s. Incidentally, Levi-Strauss was already criticized for his mathematical "impostures" in the late 60s. See the discussion in Vosse, p. ? In this sense, FN is nothing new.

<sup>78</sup> FN, p 261. Dahan-Dalmedico and Pestre devote several interesting pages to the history of Bohr's complementarity and the "felt necessity of *speaking* of atomic phenomena outside of formalism, to speak in the words of ordinary

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language.” In response to Weinberg’s rejection of Bohr’s attempt to make complementarity into a general principle, they conclude: “...saying that things could have happened otherwise never runs the risk of contradiction” but for the historian “it is without interest to take the position of those who know better than their predecessors what they should have done.”

<sup>79</sup> *Higher Superstition*, p. 261, note 9; Goldstein, “Quantum Philosophy: the Flight from Reason in Science” in Gross, Levitt, Lewis, *op. Cit.*, pp. 119-125; Levitt, “Monsieur Jourdain and Uncle Vanya,” in Koertge, *op. Cit.*, p. 280.

<sup>80</sup> Stengers, in *IS*, p. 277. Explicitly, Stengers wrote “Je proposerais aux constructivistes sociaux, sociologues, et penseurs postmodernes de ne pas ressentir un sentiment de triomphe facile face aux réactions, certes quelques peu stupides, que leur travail a suscitées de la part de ceux qu’ils décrivaient,” p. 288.

<sup>81</sup> *IS* P. 271. Though this is completely beside the point, I can’t resist mentioning that Stengers, taking her inspiration from Stanley Fish, devotes several pages to baseball, in which she seems to have found room for a referee with a whistle (p. 282). And this in the middle of a long discussion of an article by Stephen Jay Gould!

<sup>82</sup> *IS*, p. 133.

<sup>83</sup> By contrast, the brief review of this background by Dahan-Dalmedico and Pestre in *IS* is mainly useful as an illustration of how *misinformed* many French authors are regarding North American cultural politics (pp. 102-104). Among their many confusions, the most unforgivable is blaming “Sokal’s friends” for the term “science wars,” which seems to have been coined by Andrew Ross. Pierssens’ piece, unlike most of *IS*, is basically sympathetic to Sokal and Bricmont, or at least hostile to much of cultural studies; he even calls Andrew Ross a “typical product of the star system.” Jeanneret is also well-informed about the political content of the culture wars. On the other hand, I have yet to see a French reference to earlier critiques of science coming from within the English-speaking science communities themselves, as in the group Science for the People and its British counterparts. Three of the contributors to the *Science Wars* issue of *Social Text* come from this tradition, and at least two of them are respected scientists. I have seen no mention of this fact anywhere. This “New Left” critique of science from within was addressed in my remarks, prepared in collaboration with Jim Ritter, at the afternoon meeting mentioned in note \*.

<sup>84</sup> *IS*, p. 264.

<sup>85</sup> The Guardian Weekly, October 1997.

<sup>86</sup> *IS*, p. 67. Michel Callon also gets in a dig at Californians on p. 261.

<sup>87</sup> Jeanneret’s analysis of the language used by both sides is one of the best features of his book. Recurrent motifs identified by Jeanneret include *war*, *religion*, (judicial) *trials*, *fashion*, and *disease*. (p. 222 ff.).

<sup>88</sup> Quoted by Franklin Hoke in *The Scientist*, Vol:9, #14, pg.1, July 10, 1995. Elsewhere, Gross writes: “Among the best-known science critics are some well-known feminist philosophers, many of the new multiculturalists, postmodernists, eco-radicals, and purveyors of identity politics. I stress again that this description applies to a subset of sociologists, historians, anthropologists, and philosophers of science, probably to a minority among them. *Nevertheless, their views have risen to dominance*. And within the last five years this has led to a few books and one conference in explicit opposition—a drop in the bucket by the frenetic standard of science studies, which is an international movement well-supported by universities, foundations, and government agencies such as the National Science Foundation.” (emphasis added).

<sup>89</sup> Posted June 2, 1997 on sts@CCTR.UMKC.EDU, [h-sci-med-tech@h-net.msu.edu](mailto:h-sci-med-tech@h-net.msu.edu). Agar continued: “...therefore, underlying their account is the familiar grand narrative of enlightenment emancipation from superstition. There is an unacknowledged contradiction between this narrative and the direction of argument of science studies. *And who has the resources and authority to make interpretations of science studies stick?*” (emphasis added).

<sup>90</sup> *IS*, p. 13.

<sup>91</sup> This formulation is a typical ordinary language metaphorical abuse of a precise mathematical concept, which gives the lie to Sokal and Bricmont’s much-quoted theory that “a metaphor is usually employed to clarify an unfamiliar concept by relating it to a more familiar one, not the reverse” (*FN*, p. 11), a claim that would mystify analysts of Greek tragedy, for example. At least ten such abusive metaphors have been planted in the present text.

<sup>92</sup> “Charlatanism in Academia”, in P. R. Gross, N. Levitt, and M.W. Lewis, eds., *The Flight from Science and Reason*, p. 110. Charlatans, for Bunge, include radical feminists, “scientific” racists, Latour-style sociologists, pseudoquantitative economists, phenomenologists, existentialists, and ethnomethodologists.

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<sup>93</sup>“Anatomy of a Hoax,” *Tikkun*, September-October 1996. In a March 1997 interview with a Rutgers newspaper, Robbins, one of the editors of the issue of *Social Text* containing Sokal’s hoax, shows how little time he has spent in mathematics departments when he asks “Where does one find the strongest anti-nationalist sentiment: among scientists who are funded by the military and the government, or among multicultural critics?” Robbins’s resort to cross-campus stereotypes is particularly disappointing because his book *Secular Vocations* (Verso, 1993) shows keen sensitivity to mass media stereotyping of literary intellectuals.

<sup>94</sup> More recently, in an article entitled “Monsieur Jourdain and Uncle Vanya,” Levitt offered this explanation: “If the general culture seems headed into a stagnant slough [he had previously asked whether there is a contemporary composer we would compare with Brahms or Debussy or Mahler or Strauss, and likewise for novelists, poets, etc.] where irony infuses everything and all values are transient or self-negating, why shouldn’t science be drawn into the overall disaster?” Levitt, without irony, credits John Searle with this particular insight. (In Koertge, *op. Cit.*, p. 280).

<sup>95</sup> D. Nelkin, “What are the science wars really about?” *The Chronicle of Higher Education*, July 26, 1996. The claim, already present in Nelkin’s contribution to the *Social Text* issue on the Science Wars, may be unsubstantiated, but that doesn’t mean it wasn’t repeated, by Latour and Robbins, among others; cf. Jeanneret, *op. Cit.*, p. 157. The claim is echoed in the article by Dahan-Dalmedico and Pestre in *IS*, where it is claimed that “Mathematical and physical sciences [...] contribute more than ever to the cold war (think of President Reagan’s Strategic Defense Initiative)...” (p. 103). At the very least, this statement is tendentious and one-sided; recall that the majority of U.S. physicists and mathematicians voted *against* funding for SDI through their professional organizations.

<sup>96</sup> They do have their own theories about the “intellectual sources of postmodernism and relativism.” Sokal and Bricmont admit the “conjectural nature” of these theories. One might add that the theories derive from no systematic study, and that moreover the authors suggest no methodology on which such a study might be based.

<sup>97</sup> Sokal and Bricmont: *FN*, p. 6; Dahan-Dalmedico and Pestre: *IS*, p. 98; Fujimura: *IS*, p. 233. It’s beyond me how a literature that identifies itself as post-colonial -- Fujimori explicitly compares Sokal to “the missionaries and anthropologists ... who brought back to their home countries information about the ‘Others’” [*IS*, p. 232] -- can identify with Latour and Woolgar when they compare their rejection of “the uncritical acceptance of the concepts and terminology used by some scientists” to “an anthropologist’s refusal to bow before the knowledge of a primitive sorcerer.” [p. 29]

<sup>98</sup> See *FN*, p. 184. Sokal and Bricmont are here in agreement with Bruce Robbins, who, in the Rutgers interview already quoted, saw the science wars deriving in part from “a backlash against cultural explanations, in favor of explanations of social and cultural phenomena in terms of the physical sciences--a sort of imperialism in which physical science models claimed the right to explain human or sociopolitical phenomena.” See also the comments by J. Treiner, one of Sokal’s most energetic French supporters, in *Le Monde*, 11 octobre 1997.

<sup>99</sup> Weinberg, *Dreams of a Final Theory*, pp. 248-249; Gould, *The Mismeasure of Man*; J.-P. Berlan and R. C. Lewontin, “La menace du complexe génético-industriel,” *Le Monde Diplomatique*, décembre 1998, p. 1; *Math. Intelligencer* (Koblitz/Simon). The value of the distinction between good and bad science is described in similar terms by Peter Galison, where in his introduction to *The Disunity of Science* he expands on a point made in J. Dupré’s article (p. 13; Dupré’s comments are on p. 106).

Possibly in a reference to *FN*’s French title, and speaking about how the ideology of scientific progress is used to mask private interests, Berlan and Lewontin write that “Gloser sur le progrès en général, en ignorant comment les choses se passent en pratique, relève de l’imposture.”

<sup>100</sup> *A House Built on Sand*, N. Koertge, ed., Oxford University Press (1998). Kitcher: p. 33; Sokal: p. 17.

<sup>101</sup> Sokal and Bricmont distinguish between general philosophical relativism and methodological relativism. The former leads to sentences like “science is a social process, not a mode of inquiry” (Gross, in *The Flight from Science and Reason*, *op. Cit.*, p. 6) or even “physical ‘reality’ ... is at bottom a social and linguistic construct” (from Sokal’s parody, reprinted in *FN*, p. 213), of which Dominique Pestre, responding to accusations of relativism, writes “Ce genre de propositions est sans intérêt et je les pense idiots,” (in *Le débat*, **102**, nov.-déc. 1998, p. 103) and David Bloor writes “Every sane person knows [that human interaction with the material world] is central to scientific activity.” (in *Social Studies of Science*, **28/4**, 1998, p. 624). No contributor to *The Disunity of Science* defends such a perspective, and the two physicists admit that the symmetry principle in the “strong programme” in sociology of science, associated

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with Bloor and Barry Barnes, is a form of methodological relativism. But they go on to say “this position is untenable if one abandons philosophical relativism, because it ignores an essential element of the desired explanations, namely Nature itself.” (*FN*, p. 92). The “because” in this sentence is misplaced, unless you assume that the goal of sociologists is to produce the kind of explanations physicists “desire”. The selections discussed below demonstrate that sociologists (and some historians) have other motivations entirely, deriving from their own discipline, and the failure of certain scientists and philosophers to understand this point is the main reason the science wars are not going away.<sup>102</sup> Stump, p. 286; Fine, p. 232; Stump, p. 447. I choose these quotations not because I necessarily endorse them, nor because they are the most interesting material in the book, but for their relevance to *FN*. The third thing you will notice, reading *The Disunity of Science*, is that there is much more to science studies than the debate on realism vs. relativism. But the margins of the science wars are not wide enough to explain what else there is.

The book *Science as Practice and Culture*, edited by A. Pickering (Chicago, 1992), contains some heated debates on some of these issues, including an exchange between British and French schools in sociology of science. A low point in the science wars came with Paul Gross’s description of these debates as “internecine wars...deadly and amusing.” (Gross, *loc. cit.*). One might as well ridicule the mathematicians for their disagreements in the BAMS exchange.

<sup>103</sup> *Le Débat, op. Cit.* Chevalley, p. 71; Jorland, p. 93; Jacob (emphasis added), p. 85.

<sup>104</sup> When I was a graduate student, a distinguished philosopher once confided to me his opinion that philosophy of mathematics was impossible. He had written many papers on the subject and it would be presumptuous of me to disagree with him.

<sup>105</sup> See note 101.

<sup>106</sup> *Le Débat, op. Cit.* P. 104. Pestre’s response, entitled “Science des philosophes, science des historiens,” is an extremely well-written account of the misunderstandings that arise when one discipline (in this case, philosophy) attempts to judge the productions of a second discipline (in this case, history) without understanding its specific goals. But as the quotation indicates, there is a difference in philosophical perspective, which may really reflect a difference in taste. Thus when Jacob defends the distinction between the fact that a proof is valid and the fact that the experts consider it to be so, Pestre replies “I would not know where to place myself, nor on what authority to base my judgments to hold such a position, and therefore judge it *sufficient* to describe how each one comes to the conviction of a fact or a proof, how a collective governed intersubjectively (...) comes to agree that a representation it defines is true, correct, interesting, or practical and useful...with regard to certain constraints and approximations. I would then have the impression that my statements are very solid and...of having attained all that it is honest to say.”

Pestre’s comments have to be qualified, however, according to one local historian of science, who notes a “general tendency in science studies” to “slide irresistably along the philosophical slope” when they attempt to theorize their work [private communication]. This tendency, which overlaps with the “great deal of nonsense” Fine sees in constructivist writings on philosophy, is obviously in direct contradiction with Pestre’s quotation.

<sup>107</sup> N. David Mermin, “The Science of Science: A Physicist reads Barnes, Bloor, and Henry,” *Social Studies of Science*, 28/4 (August 1998), 603-623, followed by David Bloor, “Changing Axes”, pp. 624-635; Barry Barnes, “Oversimplification and the Desire for Truth,” pp. 636-640; N. David Mermin, “Abandoning Preconceptions”, pp. 641-647.

<sup>108</sup> *Op. Cit.*, p. 634. There is no space here to develop any of the questions raised by Mermin, or the sociologists’ responses, and I can only encourage those interested to read these articles before tackling the science wars literature.

<sup>109</sup> Agar: SSS, p. 651. Bloor: SSS, p. 658.

<sup>110</sup> *FN*, p. 52.

<sup>111</sup> As Thurston wrote, Wiles’ proof — still incomplete at the time — “helps illustrate how mathematics evolves by rather organic psychological and social processes.” Thurston, *op. Cit.*

<sup>112</sup> Or so I would assume.

<sup>113</sup> Or my royalties from this article, whichever is greater.

<sup>114</sup> For instance, in Dawkins, *Op. Cit.*, and on Alan Sokal’s web site. The Postmodernism Generator produces random paragraphs that look like some people’s idea of postmodern literary/cultural criticism.

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