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Standards in Science and Technology Studies

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In the 1660's living in Altdorf, Gottfried von Wilhem Leibniz, later credited as the co-inventor of the calculus with Issac Newton, was a newly minted Doctor of Laws. Seeking intellectual stimulation, he went to visit some scholars in Nuremburg, who told him about a secret society of alchemists who were seeking the Philosopher's Stone.[1]

> Leibniz decided to profit form this opportunity and learn alchemy, but it was difficult to become initiated into its mysteries. He proceeded to read some alchemical books and put together the more obscure expressions — those he understood the least. He then composed a letter that was unintelligible to himself and addressed it to the director of the secret society, asking that he be admitted on the basis of his great knowledge, of which the letter was proof. According to the story, no one doubted that the author of the letter was an adept alchemist or almost one; he was received with honor into the laboratory and was asked to take over the functions of secretary. He was even offered a pension (Ariew 1995, 21).

It may be a bit of a stretch to see Leibniz as the 17th century Sokal — but the parallels are worth pointing out, even if the situation is reversed. Although Leibniz is known for the mathematical, philosophical and scientific work he produced later, at this point in time he is a humanist playing a hoax on the alchemists who can, with a gentle nod, be viewed as proto-scientists. Sokal played a hoax on the social scientists/literary theorists. What kind of a hoax? In 1992, Alan Sokal the physicist submitted a paper entitled "Transgressing the Boundaries: Toward a Transformative Hermeneutics of Quantum Gravity" to *Social Text,* a journal for social and political scholarship. Using the trendy jargon of literary postmodernism and social constructivism he allegedly analyzed recent developments in quantum gravity showing that

> In quantum gravity ,..., the space-time manifold ceases to exist as an objective physical reality; geometry becomes relational and contextual; and the foundational conceptual categories of a prior science — among them, existence itself — become problematized and relativized (Sokal 1996a, 218).

Later, he published a letter in *Lingua Franca* admitting that "Transgressing the Boundaries" was a hoax.

The Sokal Affair elicited a far more intense reaction than one might have expected. Hoaxes are generally the source of amusement when they are part of the history of science, lets see how much fun we can have with this one. However, there was little public laughter. The mood quickly turned ugly. This otherwise minor episode has had far reaching consequences, causing many people to worry about the development of a

[1] I thank Roger Ariew for bringing this delightful tale to my attention.

widening breach between the world of science studies and the world of science. There is in fact something of a breach developing, and to the extent that there *is* a growing breach between these two communities, it is the fault of individuals in both groups.

We need to acknowledge from the start that the STS community does contain practitioners who display a certain ideological bent that, on the surface, fails to meet the standards of objective scholarship, at least the standards some physical scientists would like to advance. In addition, these STSers produce arguments that constitute attacks on the epistemically privileged status of scientific knowledge; hence, on the epistemically privileged status of science, and thus they are perceived as anti-science, which some are. On the other hand, the scientific community has been, and continues to be, notoriously thin-skinned when it comes to critical scrutiny by non-scientists. While the scientific community may very well be reasonable when reacting this way in days of decreasing public support for scientific research, it surely cannot expect to be completely immune from criticism. Further, while it is also the case that there is no monolithic STS community, I will argue, the research standards within at least one STS community do in fact not only meet the criteria most scientists endorse, but, seen from a slight distance, resemble the research process that science itself seems to exhibit. Below I will give examples of this kind of work.[2]

I will start with the Sokal Affair and offer a diagnosis of the underlying cause of the bitterness of the debate. I will then draw a distinction between two different types of STS agendas, Science and Technology Studies versus Science, Technology and Society. Finally, I want to make the case for doing Science and Technology Studies in a particular way - that is, to seek to understand science in its historical context as a social process whose domain is the real world. The goal is understand science, not to offer adulation or condemnation. The criterion for success in doing STS this way is to produce a story that meets the standard of explanatory coherence. But first we seek some understanding of the nature of the debate that has lead to the current view of STS as anti-science.

The debate between the scientific community and the STS community is a debate about standards.[3] While the debate actually began in earnest with the publication of *Higher Superstition* by Gross and Levitt, let us take the Sokal Affair as our starting point. The Spring/Summer issue of *Social Text*, which was devoted to a discussion of what had come to be called by its editors "the Science Wars" (referring to the furor raging around *Higher Superstition*), contained an unsolicited, but refereed article by the physicist Alan Sokal. It was conceived, in the author's words, as

[2] Now for post-modernists these claims already sound rather old fashioned - or to use the favorite pejorative, positivistic. But before the thesis advanced here is dismissed are anachronistic, allow me to muster an array of rhetorical strategies designed to enlist you into my network. The goal of doing so is to increase our understanding of the contents of the black box called science.

[3] Clearly it is inappropriate to major universal claims of this sort. I am, of course, only referring to small groups within each community. However, for simplicity's sake, I will refer to the "scientific community" and the "STS community" with the understanding that I am not intending global condemnations or approvals.

> a modest (though admittedly uncontrolled) experiment: Would a leading North American journal of cultural studies — whose editorial collective includes such luminaries as Fredric Jameson and Andrew Ross — publish an article liberally salted with nonsense if (a) it sounded good and (b) it flattered the editors' ideological preconceptions (Sokal 1996b, 62).

In the *Lingua Franca* piece cited above, Sokal confessed that "the article was written as a parody" and he proceeded to point out the bad reasoning and the "silliness" (his word) of his own contribution. He goes on to claim that: "*Social Text's* acceptance of my article exemplifies the intellectual arrogance of Theory — postmodernist *literary* theory, that is — carried to its logical extreme" (63). *Lingua Franca* not only printed Sokal's "confession" but invited responses, one of which was from the editors of *Social Text* with a response to them by Sokal. This was followed by much written fury on both sides, including letters in the *New York Times*, a letter to the *New York Review* by Steven Weinburg and responses to that. In 1998, special sessions have been held on the topic at the Philosophy of Science meetings in Cleveland and in Atlanta at the History of Science meetings.

So what exactly is this all about? The antagonism and polarization brought about by the publication of Sokal's parody cannot be explained merely by way of the fact that the paper was a hoax. Nor does the heat in the discussion come from Sokal's charge of intellectual incompetence directed at post-modernist practitioners, strong as it was. No, I think the intensity of this discussion derives from something deeper. The source of the argument in the first place, and the rancor it has produced, in the second place, comes not so from the charge of incompetence, for one can be serious and incompetent and sustain a charge of this sort and not generate a battle of this kind. Sokal's most critical charge here is that there is a lack of *seriousness* on the part of literary theory types and anyone else who endorses relativism and subjectivity. Let us refer to the whole gang as the postmodernists. [4] Despite the fact that Sokal tends to lump together everyone who rejects realism as the enemy, we can understand some of the depth of his concern when we realize that he isn't just worried about lack of argument and standards and rigor. He seems to be most concerned about the real world consequences of adopting a constructvist or subjectivist, or deconstructivist or anyone of these positions. Hear him:

> Theorizing about "the social construction of reality" won't help us find an effective treatment for AIDS or devise strategies for preventing global warming. Nor can we combat false ideas in history, sociology, economics, and politics if we reject the notions of truth and falsity (64).

Contra Sokal, it is important to distinguish among the various different positions that are the target of his wrath. However, it is not necessary for our purposes today. Even without doing so we can still understand the source of his anger.

[4] Once again it would be irresponsible to assume that all post-modernists fit into this picture.

But we need to issue some cautions as well. Sokal needs to expand on the charge of lack of seriousness on the part of those he attacks. Not everyone he lumps together takes the trendy literary criticism attitude that we must all be cute and that the point of everything is to upstage someone else and to make in-jokes to humiliate your opponents. Most constructivist sociologists are very serious about their work. However, maybe Sokal is not concerned about how serious the postmodernists are about their own work, but rather, how serious are they about his work? If that is his worry, then he should reconsider. While it is true that an attack on the objectivity of science in general tends to put the work of individual scientists in jeopardy, one cannot read every such piece of analysis as a personal insult. To impugn the integrity of those who seek to produce an argument which you dislike is itself to reject the very cannons of objectivity and rationality Sokal purports to uphold. If the motives of the postmodernists are suspect, it must be proven; it is not enough to throw out insults. Furthermore, Sokal's offer of the paper in *Social Text* as evidence for the sloppiness of all literary theory types fails to meet his own criteria for adequate support for a hypothesis. How many reputable scientists would accept the viability of a hypothesis on the basis of one experiment?

However, to suggest that Sokal has inadequate evidence to support his charge is not to excuse the postmodernists. Whatever their shortcomings, Gross and Levitt have amassed enough data to at least justify an inquiry. But just as I object to Sokal's use of one "experiment" to support his outrage, I object to Gross and Levitt's selective use of the data. I will return to this point later.

Just as we can understand, if not approve of Sokal's anger, we can also appreciate the intensity of the response from the other side. Through the various articles and letters, one general attitude seemed to stand out by those offended by the scientists, represented by Sokal. It goes something like this — "You scientists are preaching to us about the standards of good science and the value of truth and falsity and you don't seem to realize that you have lost that battle, those ideas have been undermined, discredited and rejected. That being the case, on the basis of what authority do you attack us? The epistemic authority of science is no longer acknowledged. You express and defend just one point of view among others, all equally valid — see Kuhn, Bloor, Bijker, Harding, etc."

The heat is generated on one side by scientists who are defending truth, beauty and the American way, and, on the other side, by a group of scholars and intellectuals who are convinced that they have already demonstrated the lack of epistemic privilege for science, or any other form of inquiry for that matter, and who deeply resent the assumption by scientists that they have the right to lecture anyone. Thus the battle is between the modernists - who continue to take science not only to be the model for rational inquiry, but as the only form of inquiry capable of producing knowledge, and the post-modernists, who reject the epistemic authority of science and endorse a form of pluralism where any form of inquiry is equally valid.

At this point my questions are two:

(1) Are the postmodernists correct in their claim that the objectivity of science has been undermined?

(2) Where did the postmodernist position come from?

Let's take the second question first. [5] The issue comes in two parts, one part centers on the alleged breakdown of the claim of epistemic privilege for science. The second centers around the proliferation of so-called contexts within which science has been critiqued, i.e., feminist, economic, political, rhetorical, religious, sociological, etc. I will elaborate, but first one thing needs to be put out on the table. For me, the following is undeniable, the process of scientific inquiry has produced and continues to produce the best and most successful methods we have for understanding the world and universe around us. However, from this fact it doesn't follow that science has produced only truths and that only science can produce knowledge. I will return to these two points later.

One of the strongest features of scientific inquiry is its self-correcting nature. Faulty assumptions are exposed and rejected, new procedures are tested and new instruments are calibrated and retested, theories are proposed, explored, elaborated, and tested, only to be finally rejected or replaced by a new set of conjectures and methods. And despite this dynamic constant reassessment and reconfiguration, science continues to produce results which give us greater and greater control over our lives, giving us the ability to improve our life styles and our understanding of how it all hangs together. It is this mysterious fact about science that makes it the object of inquiry itself. How, despite all the chaos that the history of science reveals, does science manage to produce so much that is useful? This question - the modernist fascination with the ability of science to produce - is what has given rise to such disciplines as the philosophy of, history of, and sociology of science and technology.

Without putting too fine an edge to it, consider the following truncated accounts of selected events that can be seen as setting the stage for our current debate. The major modernist program to both defend science and to provide it with a kind of intellectual justification was that of the Logical Positivists. Long before Kuhn (1962), the positivist program was under attack from within by the likes of Quine (1953/1971) and Sellars (1956). And long before Kuhn, the historicist movement in the philosophy of science was already underway. For example, Norway Russell Hanson founded the first North American History and Philosophy of Science department in 1956 at Indiana. So while I cannot credit Kuhn with being the Great Positivist Slayer or even the first person to rub the positivists' noses in the history of science, he can perhaps be seen as the first popular Anglo-American postmodernist. For whatever else his *Structure of Scientific Revolutions* argued for, it was that paradigm change was not a rational process and, this was interpreted as showing that science had no better claim to knowledge production than any other activity. Incommensurably and gestalt switches did not leave much room for reasoned debates between partisans of different viewpoints. Likewise, his emphasis on

[5] My thinking on this has been helped, if not sharpened, by numerous discussions with Mike Seltzer and Jim Collier

the social context of scientific change, acknowledging that few positions are ever overturned by reason (you just have to wait around until the old guys chairing the departments and editing the journals die), opened the door to further scrutiny of the social domain of scientific practice and placed a greater emphasis on its importance.

The impact of *Structure* cannot be denied. Today talk of paradigm shifts can be found everywhere. Explaining the popularity of *Structure* is something else again. Consider the following suggestions: first, the book was clearly written and easily accessible. Second, it dealt with a suggestive new idea, the concept of a paradigm, in such a loose way that it allowed for any number of possible interpretations. Finally, the arguments over the proper analysis of such Kuhnian ideas as paradigms and anomalies themselves artificially inflated the significance of the book's thesis. But because *Structure* could be read not only as an attack on the positivists' reconstruction of the logic of scientific concepts, but also as an attack on the universality of scientific method, it opened the door for others who, for a variety of reasons, relished the opportunity to take science down off its pedestal. Now it ought not to come as a surprise that often the most vicious attackers are those who have sought and who have been denied entry to the temple. Given a foothold, they will attack orthodoxy in the name of upholding its very principles.

Enter the Edinburgh school of sociology of science with its so-called Strong Programme. Their idea was to produce thoroughgoing empiricist sociology. There were to be no more Mertonian norms. The sociological study of science, concentrating as it does on the social processes within the scientific community no longer will find in that aspect of its domain room for a consideration of what the scientist is concerned about, namely nature. Since the Strong Programme could not operationalize nature in terms of interests, power plays, and paranoia, it simply denied that nature had anything to do with the conclusions scientists arrived at. Science is here presented as merely a social process with the results of scientists being negotiated among themselves. Science is social all the way down, so we are told.

The importance of the Strong Programme was not what it said, which was extreme, which is my answer to (1) above. To the question "are the postmodernist claims true" the answer is "of course not". (Truth is not something with which they are concerned. They are concerned with waging a war for power in the academy.) Strong Programme advocates were part of this attack on science, not because of the truth of what they had to say, but because their methodology could be used to convince those who are easily confused by sophistry that science has no special claim on our credibility. The importance of the Strong Programme was the general position it opened up, which was that science should be considered a social process. For now the question became which social process? Many social processes which emerged from the woodwork, each claiming to have equal legitimacy as sociology for its own critique of science. Thus we get economic critiques, Marxist critiques, feminist critiques, political critiques, and literary critiques, each designed to show that the scientific process is motivated not by its subject matter, but by these various social forces. How are we to adjudicate among them? Pandora's box has been opened, and now we have this problem on our hands. The "we"

by the way is those of us who study, as opposed to do, science. The problem is how to identify the appropriate context in which to explain how science works.

Scientists also have a legitimate worry. If the arguments for a plurality of legitimate epistemic contexts are accepted, and then the argument for the privileged position of science in the knowledge generating game is undermined. This can translate into serious issues when the topic is funding. While scientists are right to insist that there are standards for inquiry and argument and that they make a difference, how can they make their case without being accused of pleading special interest?

However, it is not their responsibility to make the case for the privileged status of scientific knowledge. And, Sokal and Gross and Levitt not withstanding, there are non-scientists who believe in and defend the notion of an objective reality and the possibility of knowledge, who have standards of argument and proof that rival and, nay, even exceed that of the scientific community. The scientific community will find that their strongest position lies in having their case made by someone who does not profit from it. That is the job of the STS community. But which STS community?

In my introduction, I distinguished between two types of STS, Science and Technology Studies (STS -1) versus Science, Technology and Society (STS - 2). The difference may perhaps be put in the following way: STS-1's agenda is not political. Under my construal, it has an epistemic agenda. Its objective is to understand - in a manner to be cashed out soon - how science works. But, it will be objected, isn't having an epistemic agenda itself a political agenda. If every agenda is a political agenda, then the use of "political" to characterize an agenda loses its force. By asserting that all human action is an exercise of power, we ignore the truth, and we trivialize human action.

STS-2, by concentrating on explaining how science and technology function in society, more readily opens itself up for exploitation by those pursuing a political program. STS-2 goes by a number of names - science, technology and society, science, technology and values, science and technology in society, science policy, social studies of science, etc. It has as its focus the legitimate job of understanding the impact of science and technology on individuals, governments, cultures, religion, values, or anything social. And while it can produces studies which are neutral in their claims about the benefits or negative effects of, say the introduction of a given technological process, it is more common to see such studies aiming toward a normative conclusion, although this is not necessary. It is, however, what many people want.

For example, when we began the undergraduate program in Humanities, Science and Technology, at Virginia Tech, our aim was to try and produce a coherent set of courses which gave students the tools to make up their own minds about the merits or demerits of a given scientific discovery or technological innovation. After some discussion, we considered the best approach to this end was to teach them enough of the specific science or technology to put them in the position of being able to evaluate different claims being made by advocates or detractors of the discovery or innovation in question. A number of the students complained at first because they wanted a class in what was wrong with this

or that. My own belief is that we ought not to be advocates of that sort. We can give our students theories of right and wrong, pointing out their strengths and weaknesses, and we can explain the science and the technology with its possible ramifications. But we must leave it to them to draw out and defend their own conclusions about value.

STS-2 pushes a different line. It builds on the claims of no special privilege for science to argue for, among other things the democratization of science — which includes having non-scientists on review panels making decisions about what kind of science ought to be done.

Democracy is a fine thing. In politics, democracy often is a good thing.[6] When it comes to running a prison, it is not clear that it is a good thing; likewise when it comes to running an army. When it comes to the creation of new knowledge, it is not at all clear that democracy is the appropriate. Having a different point of view is not a justification for giving that point of view a voice in every circumstance. If we take the lack-of-privilege argument and turn it back on itself, there can be no selection of discreet voices to be heard, since every voice is equally valid, therefore everyone must have a say in everything. Not only is this not possible, but the obvious drawbacks should deter any reflective defender of the democratization of science program — for I am sure he or she would not want the general public to have a voice in what he or she teaches or publishes.

Finally there is a phrase that has developed currency with some unfortunate consequences. Today we hear a lot about "science studies". This phrase began to be used as a substitute for "science and technology studies" and "science, technology and society" several years back, probably as short hand for these longer expressions. The unfortunate consequence is that by speaking of "science studies" you conceal your ideological disposition. Are you an STS-1er or an STS-2er?

So what is STS-1 and how can it help resolve the growing conflict between the STS community and the scientific community? First, we need a fuller (note the lower case "f") understanding of what STS-1 practitioners actually try to do. STS-1ers view science as a historically contingent social process whose major objective is the understanding of nature. Any adequate account of the activity and findings of science must not only take into account the people involved, but how their interactions with nature influenced and affected their behavior and their thinking. In short, the view that reality is socially constructed is rejected. If it wasn't obvious before, let me make it clear now that I am proposing that we must study science in context and that that context must, by necessity, first and foremost be historical.

The basic theme is this: to study science in context is to seek out and weave together in a coherent fashion the relevant factors for explaining what happened. There are several criteria for an adequate coherent explanation. (1) It must account for all the relevant

[6] Although it is interesting that when Austria recently acknowledged the legitimate demands of the elected right wing to a role in the government, the rest of the European Union ostracized it. So, for some, democracy is good only if it gives you the "correct" results.

factors that contributed to the event (shorthand for anything you want to explain) at that time. (2) It must cohere with what happened prior to the event in question and *with what happened afterwards* (Pitt 1992). The story we tell is an historical story and it must locate the event in question historically, which is to know its antecedents and to follow its consequences, where possible.

Thus, we can ask why a scientist worked on the problems he or she did when he or she did. To understand that is get a hold of the problematic in question. But to identify a problematic requires that you know where it comes from — i.e., what sorts of problems were people working on prior to and contemporary with the person we are studying now? How do the problems our author/scientist found worthwhile relate to the problems his predecessors and contemporaries found worthwhile? What happened to those problems and the solutions that were developed afterwards? To explain the problematic is to contextualize it in terms of its antecedents and its subsequent history. Yes, this means that a historical account of some relatively contemporary event like the demise of the Super Colliding Super Conductor will not be explanatory until there is enough distance from it to see what the fallout was. Thus, if we tried to tell the story of the SSC now, it would be incomplete and in need of constant revision as the consequences of its cancellation for the physics community unfolds. But every historical explanation is in need of revision as we find out new things. The difference, I propose, is in its pragmatic dimension.

Now in what sense does this approach produce explanations and in what sense is it a pragmatic account? It makes it possible to develop an explanation in the sense that it helps us tell a story in which the events in question are seen in a coherent historical context — i.e., it must include all relevant factors for accounting for why those problems were being addressed, why the solutions that were proposed came to the forefront, and why the subsequent history of those problems developed the way it did, and how the solutions came to the fate they did. This last point is what makes this a pragmatic account — the focus on consequences. In many ways this approach is the opposite of doing Whig history. Instead of importing contemporary categories backwards into the past, the emphasis here is on following the consequences of some particular set of actions into the future.

A lot of pressure is being placed here on the story being coherent. That means that it must handle in a systematic fashion all factors that can be shown to make a difference to what happened. That is why studying science in context must take into account how the scientists interacted with that part of the world they were investigating, i.e., nature. For how the world reacted to their efforts to undercover its secrets is part of the tale, for it bears on what they did next. And so, just to make this a bit contentious, if we are studying Galileo's discovery of the law of free fall, then it seems important to figure out how Galileo timed the rate of descent when the kind of time pieces we are familiar with were not available to him. Further, it does not appear relevant to worry if he was doing this to come up with a new discovery to flatter his patron Duke Cosimo, *pace* Biagioli (1992).

Placing the emphasis on historical context clearly gives history some sort of privileged place. For one cannot talk about any other kind of context without placing it historically. And here, perhaps, it would help to say a few words about different kinds of history. Larry Laudan, in *Progress and its Problems,* introduced a distinction between history of science 1 and 2 - HOS 1 is "the actual past of science" and HOS 2 is "the writings of historians about that past" (Laudan, 1976, p. 158.) It is a good idea, but not as it is phrased, since we cannot isolate the history of science *per se*. The history of science is embedded in the past and cannot as such be separated from a lot of other things that were going on at the same time. So let us try a slightly different approach.

The Past is what happened, the whole thing, all of it, every minute, second and detail in the non-stop flow of time. History is the story we tell when we decide to select out certain items from The Past and credit them with some sort of importance.[7] It is like stopping a movie and snipping some frames and then splicing them together and trying to make that make sense. There is another important ingredient to be considered and that involves the choice of a historiography.[8] Hence, understanding the historiography an historian employs to write a history helps to explain why it is this history rather than that one we end up telling.

We must admit that in any telling of a historical story, History, will be, to some extent, arbitrary in its selection of the facts and other factors deemed by the historian to be relevant. Further, History, i.e., the story, will change as we uncover new facts which we can show to be relevant and as we import new considerations into the story to make it hang together better with our understanding of happened before and after. So, to the extent that we can never tell the whole story of what actually happened, and tot he extent that the story we tell will be influenced by the selection of facts, the uncovering of new data, the realization that a different perspective needs to be added, History will constantly be changing as we attempt to tell the best story possible.

With this tri-fold distinction in hand, let us now draw a parallel set of distinctions that should help us understand the roles of science studies - S&TS - STS-1.

Parallel to The Past there is Nature. Parallel to History we have Science. Finally, parallel to Historiography we have STS-1, S&TS; thus:

Nature	The Past
Science	History
STS-1, S&TS	Historiography

[7] I thank Barbara Reeves for helping me articulate this difference between history and the past.
[8] Michael Seltzer forced me to consider this point.

Just as Science is about Nature, Science Studies is about Science, and to be about Science you must understand in what ways Science is about Nature. Further, doing serious science studies means the story is always revisable in the light of new data. In principle then, doing serious science studies is very similar to doing serious science. While we may have a good hypothesis with strong evidential support, it is all up for reevaluation in the light of new data. Furthermore, we must consider all the data, not just what will confirm our hypotheses and make us look good, cute, witty, or wise.

Titillating as Gross and Levitt's attack on the postmodernists was, it was bad humanistic scholarship and bad science. Neither scientist nor humanist should be allowed to pick and choose which data he or she can use and ignore the rest. If the quality of their science were to be judged on the basis of their book, Gross and Levitt would not get any more NSF funding. Good scientific practice does not allow you to throw away the stuff that makes your hypothesis look dubious, which is just what they did. There is a lot of good solid science studies research that does not buy into the post-modernist game. For Gross and Levitt to ignore that scholarship would be similar to a humanist citing the Piltdown Man and cold fusion as reasons for rejecting science and ignoring the rest.

Earlier Science was characterized as a self-correcting enterprise. While there is one universal scientific method, the willingness to review any theory in the light of new evidence is a crucial presupposition of what it means to be scientific, and, it is what it means to do good science studies. Consider now the following case for the respectability of science studies based on new evidence. The claim here is straightforward: good science studies, like good science, is a self-correcting enterprise.

There are those of us in the science studies community who do not merely accept the latest fad and let it slip by. We may take a bit longer than scientists do, but we too police our own, which is what makes what we do self-correcting. Within the last several years at least five separate studies have come out which take various authors to task for playing fast and loose with the facts.

(1) In a review in *Journal for the History of Astronomy* Michael Shank challenged Mario Biagioli's historical claims in defense of his (Biagioli's) claim that Galileo sought to ingratiate himself to Cosimo II by connecting the moons of Jupiter to the astronomical evidence adduced to support the Medici political dynasty. Biagioli replied in an article in *Early Science and Medicine.* Shank responded in even greater detail, making an irrefutable case. This exchange took place first in *Journal for the History of Astronomy* and then continued in.

(2) Alan Shapiro, in an article in *Perspectives on Science* charged Simon Schaffer with historical inaccuracies claiming Schaffer:

> has already attempted to study the acceptance of Newton's theory, by using a constructivist approach and implicitly adopting the model of a modern laboratory science. His account, however, must be judged a failure when weighed again the historical evidence. Applying his approach to the acceptance of Newton's theory means focusing on Newton's instruments,

> especially prisms, the difficulty of replication, the opaqueness of instrumentation and experimental procedures, the uniqueness of local practices, and Newton's efforts to establish "authority" and "transparency" for them. By reducing the issue of acceptance to one of power and authority, Schaffer argues that Newton established his theory by means of a virtual conspiracy among his acolytes. Newton's power to get his theory accepted, he tells us, "lay in control over the social institutions of experimental philosophy. In the 1670s, Newton had exercised no such power. After 1710 his authority among London experimenters was overwhelming" (p.100). Not only does this explanation not satisfy the chronology of the acceptance of his theory, which occurred in Britain well before 1710, but it does not account for its acceptance on the Continent. Schaffer situations Newton's experiment and use of prisms in such a local situation that, he argues, Newton and his conspirators held that the conspirators will succeed only with prisms made of British glass. This odd claim is what initially led me to distrust his account, especially when I found that the sources he cites to establish his argument said nothing of the kind." (Shapiro 1996, 60).

(3) In several recent issues an interesting set of exchanges took place in *Isis* between Moti Feingold and Steve Shapin. Feingold disagreed with Shapin's historical story as told in *The Social History of Truth.* Shapin tried a rebuttal and Feingold responded in detail.

(4) At the 1996 History of Science meetings a younger scholar challenged in a quite conclusive manner Shapin's characterization of the 17th century British gentry, showing that were not disinterested figures and could not be the models for objective evaluation. Her point was that the historical context couldn't be ignored. The British gentry of the 17th century, having recovered from a brutal civil war had to take over management of their own estates. They had a significant stake in the advancement of agricultural science and that is why the British Society placed such emphasis on agricultural experiments.

(5) And, finally, in a review essay in *Physis,* if I might modestly add, I argued against Mario Biagioli's (1992) that Galileo was in Cosimo d'Medici's court because he was THE Galileo and not because Galileo was only trying to curry favor with his patron.

We may be slow, and that can in part be explained by the length of time it takes to get papers published in the humanities, but we do correct our own when they make mistakes. We do so in the same way scientists do: we check the data, we explore the relation between the facts and the hypothesis and we attack the argument, not the person giving it. Would it be rash to suggest that scientists might learn something from the methodology of good humanistic science studies scholarship, just as we have learned from science? I urge scientists to learn to recognize and to appreciate good science studies, for one of its jobs is to clarify just what is so special about science. This does not mean that everything we say will be positive — call it tough love.

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Science and technology studies in one easy lesson. STS in one lesson? Not really.Â A standard history of STS might start with Thomas Kuhnâ€™s Structure of Scientiï¬c Rev-. olutions (1962), which emphasized the communal basis of the solidity of scientiï¬c. knowledge, the perspectival nature of that knowledge, and the hands-on work needed.Â from other disciplines that study science and technology is an activist interest. For the Low Church, key questions are tied to reform, to promoting science and. technology that beneï¬t the widest populations. Science & Technology Studies is an international peer-reviewed journal dedicated to the advancement of scholarly studies of science and technology as socio-material phenomena, including their historical and contemporary production and their associated forms of knowledge, expertise, social organization and controversy. This includes interest in developing Science and Technology Studies' own knowledge production techniques, methodology and interventions. The journal welcomes high quality contributions that are based on substantial theoretical or empirical engagement with the multidiscip Standard and Collaborative Research, Scholars, Professional Development, Research Community Development, Conference and DDRIG Proposals. Important information and revision notes. The program name has been changed from "Science, Technology, and Society" to "Science and Technology Studies" to best connect in an inclusive manner with the research communities that are served by the program. Eligibility requirements for Scholars Awards have been changed; these grants are to be made to U.S. Institutions of Higher Education and to U.S. Non-profit, Non-academic Organizations. As the National Science Education Standards emerged in 1996, the middle grades were defined as grades five through eight. Middle school philosophy calls for teams of teachers (from all facets of the curriculum) to work with a given set of middle school students and to unify and relate all study for those students. Project 2061, formulated in the late twentieth century, is a reform project that ties the curriculum together, especially science, mathematics, technology, and social studies.Â 1990. Science for All Americans: A Project 2061 Report on Literacy Goals in Science, Mathematics, and Technology. New York: Oxford University Press. Science Education for Public Understanding Program (SEPUP). Science and technology studies or science, technology and society studies (both abbreviated STS) are the study of how society, politics, and culture affect scientific research and technological innovation, and how these, in turn, affect society, politics and culture. Like most interdisciplinary fields of study, STS emerged from the confluence of a variety of disciplines and disciplinary subfields, all of which had developed an interestâ€”typically, during the 1960s or 1970sâ€”in viewing science and