

Phil 581  
w/ Alan Richardson & Judy Segal

Keith Douglas  
13714993

Philosophy, History, Sociology, Rhetoric:  
Avoiding "Nothing But-ism" in Science Studies

Introduction

This paper has three sections. First, it will make the case that some work in science studies suffers from a perceived mistake of what will be called "nothing but-ism". Second, it will present six families of examples of this mistake from various works in "science studies" and an example of how it is avoided. Third, it will propose a solution to this problem drawing upon the four disciplines which appear in the title of this paper. The latter will build on the author's earlier work (Douglas 1999) in which an attempt was made to explicate strategies of credibility used by science studies scholars in an attempt to suggest successful strategies for avoiding the mistake discussed in the current paper.

As the works surveyed have considerable merits outside of the flaws under discussion, this critical examination is meant to complement the scholarship of each. Intellectual flaws in scholarship are the target here, not a "social group". Hence the present paper should not be read as implying that the articles under consideration share content or that the authors have any sort of goal in common. Further, the merits of each investigation should be taken on its own for the previous reason.

Section 1 - Definition of "Nothing But-ism" and its defense

The name "nothing but" is proposed as the name of a mistake that is committed when one aspect of science studies is focused on too much, (whether it be history, philosophy, sociology or rhetoric<sup>1</sup>),

<sup>1</sup> Conceivably other disciplines could be fit into this schema as well, but I am considering these four in the interests of manageability.

with result that claims are made that go beyond evidence presented. Examples will follow in the next section; for now let us look what this mistake would involve and some possible criticisms that could be leveled against it.

The mistake involves thinking that having investigated one "reason" for a scientific result, one has thus given the reason for it.

For instance, if one is making the case that a particular style of writing convinced the scientific community that a particular hypothesis was justified, then one commits the "nothing but" mistake if one only does a rhetorical analysis of texts involved. One must further show that other factors did not play a role, or played less of a role than the rhetorical factors. Merely investigating the rhetorical factors alone can never tell one this. Philosophical<sup>2</sup>factors, say, may be important, or sociological ones.

Five general objections to this account shall be dealt with here some other, more specific, objections to the account shall be postponed until some concrete examples are dealt with in the next section of the present paper.

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<sup>2</sup> Philosophical factors for the present paper will include the factual basis of a scientific production and its conceptualization. Nothing hinges on this "running together" - the analysis and suggestions are applicable if philosophy and science are disjoint (assuming that there is such a creature as philosophy of science). Note that this applies even if one holds the view that scientific results have no factual basis - the set of factual bases in the following explanation just becomes the empty set. One would have to either show that the world did not matter at all in one specific case, or somehow come up with an argument that it never does. But see later where the legitimacy of this strong form of constructivism will be rejected somewhat.

The first objection concerns an attitude that could be phrased as follows: "I am interested in X. My work is thus about X, and I could not care less about Y, Z and W. Hence I will not concern myself with them."

This is fine - people are still welcome to investigate from whatever perspective they like on this account, however, trouble begins as soon as one starts claiming that one's account is the most important in a given case. See footnote 1 and in section three, below for issues concerned with ranking of importance. This claiming of importance can be done blatantly or, as is usual, subtly, and one has to be careful with certain claims as they do implicitly make "nothing-but" mistakes.

Another possible objection that should be dealt with concerns what might be called the "narrative objection". This is similar to the above worry. It can be phrased as follows: "In my discipline (or, my style of doing my discipline), we are not interested in 'the truth about X' (e.g. Einstein's motivations for working on special relativity), but instead want to give a story or a 'narrative' about X. Your claim that 'nothing-but' is a problem implicitly presupposes a 'scientistic' model of science studies that I may not want to adopt."

Two possible responses are appropriate here. One: if one is genuinely interested in what some might pejoratively call "telling a yarn", then one must then not claim conclusions of what might be deemed an explanatory sort. It will of course be rejoined "Why?" Here I turn to Robin Fox's (1996) paper. He writes (pg. 342, italics in original):

*"In short, If you wish to be believed, you must accept the burden of falsifiability. You*

must accept that your statements are hypotheses that are in principle subject to refutation. If you refuse to accept this burden, on any grounds whatsoever, then there is no reason why we should pay any further attention to anything you say, since you could just as well utter complete nonsense or gibberish; it would make no difference.”

If this is regarded as being “scientistic”, the point is conceded. But all that shows is that without this, science studies disciplines are bound to produce people dogmatically or pointlessly talking past each other and through each other endlessly. Claiming that one does not wish to be believed or that one wants to write gibberish, etc. thus dismisses scholarship altogether.

Further, what would make the above mentioned sort of activity a study? Unconstrained by evidence even in the weakest way, why would one wish to call what one was doing a study of any sort? Their branch of science studies would become “Science-related storytelling”. The present paper will concede to anyone who wishes to do this that she is welcome to do so, with the proviso that she has to be wary of slipping into the purview of the first answer to this objection. Thus this branch of science studies, if it can be still called that, is not under consideration in the present paper. It appears that none of the works under consideration are such things, but of course the author would change his views on them should contrary evidence arise.

A third possible objection might come from the movements within science studies which regard their investigation as being primarily “political” (e.g. [perhaps] Harding 1986; Haraway 1991). The objection runs as follows: “Since we are doing politics, and

trying to change the way the world is, not "describe it" as the mistake seems to presuppose. Of course our accounts are incomplete, we're showing the negative side of something that we want changed or replaced!" This response can produce the usual objections one can raise to doing politics in the clothing of scholarship (for which see, e.g., Patai & Koertege 1994). But in the context of the present paper one can raise a further counterargument: namely, if one wants to show that (say) some aspect of science is sexist, one has to consider how the other aspects might (or not, as the investigation proceeds) mute this criticism. For instance, if one wants to claim that Bacon was a sexist<sup>3</sup> and that this influenced development of modern science, one cannot merely show that Bacon wrote using sexist language, and that this sexist language continues to this day and from there jump to the conclusion that science must be reformed. One has to show that the corruption extends beyond the language, and further, that the corruption cannot be cured nondestructively. In other words, one can easily commit the very mistake being warned against if one jumps too easily into a "political mode".

If it is then claimed that the goal of a given paper (book, lecture, etc.) is a merely a polemic or "consciousness raising" with little or no substantive content desired, and so one ought not to judge the paper on the standard of scholarship being proposed, the answer is similar to the one given to the second objection above. It should also be noted that clearly announcing one's intentions for the document should be done in this case.

This avoids the case of charges of "nothing but-ism" being laid

and then defended against by a statement along the lines of "but I

<sup>3</sup> It should be noted in passing that this particular example (one of Harding's) has received a rather critical investigation in Soble's In Defense of Bacon (1998).

didn't really mean that!".

The fourth objection concerns perception. To some extent, the diagnosis of this mistake relies on some subjectivity of the reader. It is thus likely that some readers of the present text will not agree with the diagnosis in all case, or perhaps may be aware of better cases within the same works presented. This possibility does not trouble the present author too greatly, as it is felt that enough examples are provided to show that the mistake does in fact occur. He makes no claims at actually how prevalent this mistaken is in the broad interdiscipline of science studies.

A fifth and final objection concerns the issue of difficulty in avoiding this mistake. "Surely", it could be claimed, "what you propose requires doing a larger amount of work than we are used to. Will that not reduce our productivity?" In terms of output of number of papers, monographs and books completed, it may very well do that. (The possibility that, due to the collaboration suggested in section three, productivity in amount of work will increase is not too far fetched, however.) But even if the amount went down, is this a good tradeoff for increased quality of work? The present author thinks so and hopes that his colleagues will have similar sentiments, though he is quite aware of the difficulty that this brings.

### Section 2 - Examples

There are two main ways in which "nothing but-ism" can play itself out. First, it can overlook certain factors from other science studies disciplines and thus overemphasize factors from one discipline. Second, the mistake can also occur in a stronger form when it is claimed that one discipline has explained a given "result" in science. In this section we shall see six examples of

the mistake and one example that avoids it. (This ratio is not meant to be an indicator of ratios in the literature or anything of the sort.)

Our first example comes from Campbell (2000). His presentation title and indeed, introduction, includes phrases like "why was Darwin believed?". The lecture includes an admirable job of showing the cleverness of Darwin's language and why it would be persuasive to his audience. However, this does not answer the question posed in the title and introduction, at least by itself. One would have to pursue one of two interrelated further investigations to settle that question. Namely, either Campbell meant by the title that this and this alone (points to rhetorical strategies) was why Darwin was believed, or this was part of the reason. Either way, one must confront the other factors involved, whatever they may be in this case.

Our second example is from Evelyn Fox Keller's recent (1995) book. Let us examine the second chapter of this enlightening collection in the light of the current proposal.

In this chapter, Keller is investigating Schrödinger's foray into biology. She writes of Schrödinger's discussion of how organisms maintain homeostasis (1995, pg. 68, italics in original):

Let me underscore some of Schroedinger's words: The marvelous faculty of a living organism - that which guarantees its existence - is a *device*. What sort of device *really*, as he puts it? This he did not say - only that it 'really consists in continually sucking orderliness from its environment.' Is it possible that the silent ego behind the cogtio, and hence behind the sum, is no more (or less than an infant sucking life from the breast of its world? Not quite. *Really*, we have been instructed, this is not an

infant homunculus; it is a *device*.

However, closer scrutiny seems to reveal it is not an infant at all but an old man; perhaps it is Mr. Schroedinger himself. Certainly there is an oddity to this text - one that might have gone unnoticed, were it not for the surfacing of the image of the infant.

Keller's passage above contains an instance of the mistake that is the concern on the present paper. It builds an interpretation of Schrödinger around what might be a misreading of a word in his passage. She quotes him as saying "'The device by which [it] maintains itself ... really consists in continually sucking orderliness from its environment.'" (pg. 68) This seems to misread "device". "Device" also has "means" as a meaning, and this sentence of Schrödinger's becomes less awkward grammatically if it is read this way. It is thus suggested that reading Schrödinger the way Keller does requires additional support, in light of the principle of charity. This possible error is pointed out as it is the main support for her Lacanian interpretation of the passage - without it, the interpretation that the device is (represents) Schrödinger himself is left less supported. This commits the nothing-but mistake, as there are other interpretations of Schrödinger's concern over life and thermodynamics. To claim that he has "tried to find himself" in the work, or the like, as a psychoanalyst would have it, requires further evidence that other factors were not at work here. This extends to Keller's later (pg. 70 ff.) remarks about the context of the second world war, etc.

It might be rejoined that some or all of the features of this text that have been criticized are in fact to be taken as metaphors. This objection does not quite do justice to the passage, because she clearly is trying to understand Schrödinger's motivations. We



can thus play the card of Fox's point, quoted above in section 1 of the present paper. Metaphors aren't easily testable (or interrelatable with other explanations), especially when it is not clear what the metaphor is supposed to be a metaphor is about.

Our third case draws from Thomas Gieryn's 1999 well researched work, *Cultural boundaries of science: credibility on the line*, and his earlier work on similar subjects (1983).

The preface of Gieryn (1999, pg. ix-x) states:

Why is science so widely trusted? Why do we turn so often to scientists for help in reaching personal or policy or corporate decisions? Why do we provide copious public patronage to support more scientific research? Why is science conferred the legitimate power to define and explain nature and other realities?

The answer will not be found upstream, I suggest, but down. Nothing in the practices of scientists at their benches, nothing in their skillful mangle of gadgets or critters, nothing in the literary machinery that translates inquiry into facts on a page can alone explain why science is trusted (in so many and varied situations) to provide credible and useful accounts of nature. Or, more precisely, upstream science substantially underdetermines the epistemic authority that marks its consumption downstream.

This contains a subtle instance of the "nothing but" mistake, a correction of it, and a reforming of the mistake (as we shall see). The first mistake occurs when Gieryn tells us that the answer to the question he is investigating "will not be found upstream". This is then modified by "can alone" (emphasis added) later in this passage. Finally, in the last sentence we have a possible case for the mistake, with Gieryn's claims of underdetermination.

In order to successfully show that the underdetermination thesis is correct, one cannot merely present sociological factors of the sorts the book presents. One must also show that other factors do not play an equivalent (or almost equivalent) role, regardless of how salient the sociological ones are. Let us examine one of the cases he presents in order to see if his strategy is successful.

Throughout his 1999 and 1983 texts, Gieryn is concerned with the "boundaries" of science - what distinguishes science from other fields of endeavour. He investigates the work of the (*inter alia*) science popularizer John Tyndall<sup>4</sup>. Gieryn's thesis in this section is that Tyndall "changed the boundaries" of what constitutes science in order to fit his audience. This fits into Gieryn's larger project of showing that the boundaries of science are not fixed within science *per se*, but are rhetorically constructed by social factors in social contexts, rather than, say, cognitively developed to better understand the world<sup>5</sup>.

Gieryn correctly points out how Tyndall mentioned different features of science to different audiences. Gieryn reports that he stressed the empiricism of science to the religious audiences; the rationalistic aspects to mechanics. Fine, as far as it goes. But does this support the thesis that science has flowing boundaries? Not by itself, it does not. One would have to show that Tyndall did not think of science as being ratio-empiricist, as many (e.g. Bunge 1996) have emphasized. An alternative explanation, which

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<sup>4</sup> It is not being suggested that Tyndall was only a popularizer, but engaged in this in addition to a practicing scientist. Since Gieryn discusses the popularizer "side" of his activities, this is being stressed in the analysis in the present paper.

<sup>5</sup> This alternative, a bit of a strawman of another extreme, is what might be called the "philosophic" viewpoint in the terminology of the present paper.

Gieryn does not rule out, is that Tyndall was emphasizing different aspects of science to better "play to his audience" rather than "constructing them" or "recreating them".

A similar situation applies to Gieryn's third case study, which concerns debate over phrenology and a chair in metaphysics in Scotland circa 1830. In this chapter, the claim is that science "got redrawn" to exclude a phrenologist from this chair by his detractors and the phrenologist's supports "drew the boundary" of what is considered science as to be inclusive of him. In order to make this case for sociological factors being most important, one has to show that one of the sides (or both) was not appealing to established scientific principles of the time. In other words, one has to show, using independent means, that one side engaged in pseudoscience. One cannot merely report that one side had one conception of science and one had another. One cannot simply assume that science has fluid boundaries without begging the question here - one must show that the "static" boundary conception is wrong independently. This does not entail that static boundaries of science in an extreme sense need apply. The boundaries may change as a result of cognitive demands or pressures. One must rule out the possibility that one side or other was legitimately crankish. One feature of pseudoscientists to this day (almost by definition) is that they claim that science ought to include their particular field, and they do this often by redefining the world "science" to include their field.

An objection to this account can be raised at this stage. "Aren't you", say the critics, "begging the question against Gieryn's thesis by assuming that there is (at any given historical time) at least one boundary between science and nonscience?" No, this

account does not beg the question, because it proposes that the investigation of how other (putative) pseudoscience(s) were dealt with at the time be done with a different case. With that on the table, one could see if either side was redrawing the boundaries of science rhetorically. If it turns out that there is constant recharacterization of science (keeping in mind the worries raised about Gieryn's remarks on Tyndall, above), then Gieryn's account would be vindicated. Gieryn's other admirable investigations (1999) might prove useful to provide means to repair the flaws in the sections discussed above, but that would require further investigation that in the interests of the present paper, as each chapter is somewhat stand-alone, as they are in the case of his (1983).

In that vein, let us now investigate another case of "nothing but", one from Haraway (1991).

Haraway (1991, ch. 9) uses "us" and "them", and similar language to emphasize what she takes to be a female or, perhaps, feminist viewpoint. She claims that the concept of "objectivity" has traditionally been regarded as masculine, and "subjectivity" as feminine. In order to show, however, that these concepts are through and through gendered, one cannot simply show their historical usage. Haraway is somewhat aware of this, but when she turns to redefining "objectivity" towards page 188, she has implicitly assumed that the current definitions are gendered or otherwise problematic. Thus in order to make a case for the "usefulness" of her redefinitions, one must show now that the use of "objectivity" is incurably sexist (etc.). This would involve refuting those (e.g. Bunge 1999, Radcliffe Richards 1996) who claim that objectivity as normally understood (by definition) is

sexless. She must show that accounts like theirs fall under her rubric (1991, pg. 190) of:

“All Western cultural narratives about objectivity are allegories of the ideologies of what we call mind and body, of distance and responsibility, embedded in the science question in feminism.”

Here the case is of socio-historical accounts being at odds with philosophical ones.

Another likely instance of the “nothing but” mistake occurs in Haraway’s use of “us” and “them” within this chapter. What groups are these meant to pick out? Either her groups are banal or she commits the mistake. Her account relies on assuming that the “us” is sufficiently narrow that her criticisms ring apparently true with some group. Of course, use of “us” then makes her claims banal. If on the other hand, she is suggesting that one should want membership in this group (for instance, to other women) she has committed the mistake, for she must thus argue for broadening her circle. Her implicit isolationism suggested by the previous remarks thus leads to the mistake.

Another example can be drawn from the works of Richard Lewontin. As noted in previous works (Douglas 1999), Lewontin has asserted that those who hold that biological explanations of human social features are possible are conservatives. Let us examine *de nouveau* these claims in the light of the discussion of the concern of the present paper.

The suggestion that those who believe in the possibility of human sociobiology are all reactionaries can commit the “nothing but”

mistake quite handily. If one shows that human sociobiology<sup>6</sup> would lead to unfortunate (for leftists) political consequences, and further shows that current sociobiological accounts are scientifically flawed (which Lewontin 1991 does very admirably), and finally also shows continuity in the content of accounts between current sociobiologists and Social Darwinists, racists, etc. one has not shown that human sociobiologists are reactionaries. Here the "nothing but" mistake shows up in overdoing historical explanation at the expense of philosophical explanation.

Philosophical explanation could (conceivably) show that there is a necessary connection between belief in the possibility of human sociobiology and conservative political views, without having to investigate the politics of all the human sociobiologists. There is an easy way in which this could be done, but ultimately a question-begging one. This would be the thesis that one is by definition a political conservative if one believes in the possibility of human sociobiology.

This way out is question-begging for it amounts to a persuasive definition. Furthermore, there seems to be no intrinsic reason why sociobiological findings need produce findings that bolster conservatives. (It may very well be that all putative findings to date have been such, but that is something else entirely, even ignoring the "ought-is" gap problem involved in these cases. See also Konner 1999 for a forceful left-liberal defense of sociobiology.)

Our final example will come from Kuhn's influential *The Structure*

<sup>6</sup> Human sociobiology is stressed as there has (as Wilson [1995] noted) never been any virulent opposition to sociobiology of other animals.

of *Scientific Revolutions* (1996 [1962])). In this case, let us examine his account of Lavoisier's advances in chemistry. First let us rehearse what Kuhn says about revolutions. On page 93, he explains what happens during a political revolution, and on the next page, explains that he is going to be explaining that science proceeds in a similar fashion. He writes (pg. 93, [1996 {1962}]) concerning political revolutions (*italics in original*):

Then, as the crisis deepens, many of these individuals commit themselves to some concrete proposal for the reconstruction of society in a new institutional framework. At that point the society is divided into competing camps or parties, one seeking to defend the old institutional constellation, the others seeking to institute some new one. And once that polarization, *political recourse fails*. Because they differ about the institutional matrix within which political change is to be achieved and evaluated, because they acknowledge no supra-institutional framework for the adjudication of revolutionary difference, the parties to a revolutionary conflict must finally resort to the techniques of mass persuasion, often including force.

Kuhn argues that one should regard each side of a revolutionary conflict in science as being similar to the factions in a politically revolutionary conflict because they each argue within their own framework (paradigm). Like the political revolutionaries who are at odds and cannot resolve their conflicts because there are no institutions to do so, Kuhn says the scientific revolutionary and his opponents cannot find common ground. Here is where we take up Lavoisier, in the light of Kuhn's remark (pg. 94, [1996 {1962}]):

To discover why this issue of paradigm choice can never be equivocally decided by logic and experiment alone, we must shortly examine the nature of the differences that separate the proponents of a traditional paradigm from their revolutionary

## SUCCESSORS.

In order for Kuhn to establish the above point, he has to make the case that there is a strong difference between, in our case, Lavoisier and his critics. As the next pages indicate, this considerations will lead to Kuhn suggesting that science is not cumulative. But here we have our danger of the "nothing but" mistake. To be sure, sociological factors do somewhat explain how Lavoisier's conceptions came to be accepted over his rivals. If there was a break, a place where experiment did not equivocally decide and yet Lavoisier "jumped at it", that would be a large part of our revolution. "Nothing but" comes in here because of the danger of reading the revolution at too coarse grained a historical level. One has to perhaps do microhistory to see if there is a point where Lavoisier "jumped". Note carefully what is being claimed here - Kuhn is committing the "nothing but" mistake because he has looked at "too broad a picture" (and curiously enough at the same time too narrow one) of revolutions. By explaining that there are no crucial experiments by themselves, he allows the "nothing but" to apply to the choice of paradigm. Since he claims to have eliminated logic and experiment as means of choice, he claims then that "conversion style experiences" for individuals, with certain social factors why the result spreads amongst groups, are (he says) what is left. "Nothing but"! What about coherence with the rest of science? What about slow build up and then the jump to something new? Here is where the examining "from too far away" comes in - one misses the ways in which the science is continuous and cumulative<sup>7</sup> in the way he denies. The

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<sup>7</sup> In other words, if the analysis herein is correct, Kuhn is right to say there are revolutions in knowledge, but wrong to say that they break totally with preceding accounts, even in the same field. In the case of Lavoisier, much chemistry's content remained constant "over" the revolution.



social factors Kuhn uses as cannot explain why individuals adopt a new paradigm, as by definition there is no one else to convince them of its merit. One should investigate whether the "radical change" actually happens - does this happen to Lavoisier? And if so, what does it say for the "nothing but"? If it does not occur at the "microlevel", (by repetition) it seems to follow that it could occur at a "macrolevel" as well. Here is where social forces may come in - Kuhn is perhaps right that the "conversion experience" applies to some of those who hear of new results second hand.

It is not the interest of the present paper to show if Lavoisier did in fact radically change his world view. If it is shown that he did, Kuhn has avoided the "nothing but" in this particular case. But then one must perform a similar analysis for his other cases, for doing either of the above in the case of Lavoisier does not refute or support Kuhn's thesis taken generally. One has to examine each and every case he discusses, and any that people have proposed since<sup>8</sup>. (See Kitcher 1993 for some investigation along these lines.)

Finally in this section, in order to provide contrast, an example of an attempt at avoidance of the "nothing-but" mistake. I take this example from Shapin's (1998) The Philosopher and the Chicken: On the Dietetics of Disembodied Knowledge. In this work, Shapin presents accounts of various philosophers and scientists and their attitudes towards food and drink. He is aware that the historical approach he is presenting need not be the only one (19xx, p.43):

Third, I want to acknowledge both the possibility and, within limits, the legitimacy of a  
<sup>8</sup> Note the great difficulty in identifying in a non-question begging manner a putative Kuhn-style revolution.

“realist” psychological and sociological way of talking about the disengagement and ascetic discipline of intellectuals.

Shapin thus acknowledges the possibility that his account may be supplemented by these additional viewpoints, and does well to recognize them.

### Section 3 - Proposed Remedies

In this section, three remedies are proposed. The first of these is banal, cooperation amongst science studies practitioners, both within their sphere and within the sphere of scientists. Sharing one’s sociology of science paper with (e.g.) the scientists one has studied (or whose work one has studied)<sup>9</sup>. would perhaps do wonders for avoiding the complaints that many critics of science studies have made. For instance, it has been remarked that some feminist critiques of biology ignore historical evidence (Gross 1998). If historical accounts were taken into consideration, the sociological explanations offered in, e.g., Martin (1996) are insufficient, and thus must be weighed against this evidence. Similarly for philosophical investigation (Gross 1998 performs this task as well).

Levitt (1999) has suggested that in accounts of the world at large science should have some sort of “trumping power” over alternative explanations. This is because of the success of science in explaining the world, or at the very least allowing the production of all matter of devices<sup>10</sup>. In some broad sense “science works”,

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<sup>9</sup> Needless to say, this is impossible literally in the case of historically oriented investigations. But nevertheless, there is often a community of existing scholars in the field’s descendants. (What counts as a descendant of a given extinct field can be a difficult question, though.)

<sup>10</sup> The possible claim that technology as commonly understood does not depend (in any way) on the development of science is too absurd to take seriously.

and so this fact should be taken into consideration during investigations of it. It has also been pointed out that there is a bizarre inconsistency involved in those who claim that science does not find out about the world and yet protest (with often good reason) technologies that depend on it (e.g. atomic bombs, bioengineering, etc.). (What would be the great physical danger of atomic bombs if nuclear physics were not at least approximately true?)

It is suggested here not that what I have labeled "philosophical" investigation should trump others, but merely that it should have priority in the following sense. If two competing explanations exist for "result about science A", then in the absence of decisive information, the philosophical one has priority. Note that the discussion here is continually at the metalevel. Adjudicating the specific content of science (e.g. "the hydrogen atom has one proton") is something else entirely, not on the subject of the current paper. We are instead concerned with how statements like "bromine reacts with potassium" get established by science (if they do), how they get checked, and so forth. Remember that the factual content of science is a potential explanation factor in many of these cases. (It may never be the whole one, but that is another story for another time.)

Let us see why this follows from Levitt's suggestion, and further why this should not be objectionable to all those involved in science studies. It does take as given that the world matters. It has been suggested that the strong constructivism "was never

meant"<sup>11</sup>, and so all are agreed that the world matters in terms of the outcomes of science.

Scientists at the very least say they are investigating the world, or some aspect of it. Now unless one has good reason to suppose they are not<sup>12</sup>, one should investigate science "with that in mind". This is much the same as one should investigate, say, musicians, with "they produce music" as the "guiding principle.

This should not be objectionable, as it gives us the domain of individuals, groups and their artifacts<sup>13</sup> to be studied in science studies, at least to a first approximation. One needs a starting point, even if it is one open to revision as new information comes in.

Another remedy that the considerations of the previous sections of the paper suggest is attention to careful language. Words like "only", "alone", etc. are dangerous in that they can lead to the "nothing but" mistake. Many of the criticisms that have been raised above surrounding the "nothing but" problem stem from what

<sup>11</sup> This is being most charitable to certain individuals within science studies. In the interests of the present author's own time, the present author will ignore the issue of whether this claim on the part of the constructivists is at all fair. See, however, Cole (1992, 1996) for some remarks on this.

<sup>12</sup> One immediately runs into the objection that if they are not, they are simply not acting qua scientists, for whatever reason. Investigating the duplicity in these cases is interesting its own right, but does presuppose that some scientists genuinely do what they say they are doing in some weak sense. Remember that one must agree scientists must do it in some weak sense at least sometimes in order to avoid becoming a strong constructivist of the kind that is claimed not to ever have existed.

<sup>13</sup> Ideas herein are counted as artifacts. If this is regarded as odd, simply add "ideas" to the list of things possibly studyable in science studies. Similarly, *mutatis mutandis*, for texts and speech, etc. that the rhetoricians might wish to consider.

might be called "overenthusiastic language". Related, each claim is a hypothesis, in the sense of Fox's quotation above, and thus should be stated as such. (This methodological suggestion extends to the present paper's own investigations.) The positive example of Shapin, above, falls into this category.

The final remedy to be suggested involves a proposed change in social practice. At least in the present author's home discipline (philosophy) teamwork and coauthored papers are unusual. Collaboration amongst different kinds of science studies scholars would perhaps reduce the frequency of the error of this sort. This includes collaboration with scientists being studied if such was germane to a particular problem.

It is suggested that these proposed solutions would increase credibility of science studies practitioners of all kinds and avoid some of the worries the present author and others have raised elsewhere (e.g.: Douglas 1999; Gross, Levitt and Lewis 1996; Koertege 1998). This is because any attempt to clear up what is felt to be a important error is regarded as a welcome improvement in scholarship, and it is hoped that the present paper has suggested that this error is serious and yet somewhat easily correctable.

## References

- Bunge, M. 1996. *Finding Philosophy in Social Science*. New Haven: Yale University Press.
- Bunge, M. 1999. *Dictionary of Philosophy*. Amherst: Prometheus Books.
- Campbell, J. A. 2000. Why Was Darwin Believed? The Origin of Species and the Problem of Intellectual Revolution. Public Lecture as part of University of British Columbia Course PHIL 581 / ENGL 509, Winter 1999-2000 session.
- Cole, S. 1992. *Making Science: Between Nature and Society*. Cambridge: Harvard University Press.
- Cole, S. 1996. Voodoo Sociology: Recent Developments in the Sociology of Science. In Gross, Levitt & Lewis (eds.) 1996.
- Douglas, K. 1999. Credibility and Approaches in the Field of Science Studies: A Sketch of a Critical Appraisal. Unpublished paper for University of British Columbia course PHIL 581, Winter 1999-2000 session.
- Fox, R. 1996. State of the Art/Science in Anthropology. In Gross, Levitt & Lewis (eds.) 1996.
- Gross, P. 1998. Bashful Eggs, Macho Sperm and Tonypandy. In Koertege 1998.
- Gross, P. Levitt, N. & Lewis M. (eds.) 1996. *The Flight From Science and Reason*. Baltimore: Johns Hopkins University Press.
- Gieryn, T. 1983. Boundary-Work and the Demarcation of Science From Non-Science: Strains and Interests in Professional Ideologies of Scientists. *American Sociological Review*, Vol. 48; pgs. 781-795.
- Gieryn, T. 1999. *Cultural boundaries of science: credibility on the line*. Chicago: University of Chicago Press.
- Haraway, D. 1991. *Simians, cyborgs, and women: the reinvention of nature*. London: Free Association.
- Harding, S. 1986. *The Science Question in Feminism*. Ithaca: Cornell University Press.
- Keller, E. F. 1995. *Refiguring life: metaphors of twentieth-century biology*. New York: Columbia University Press.
- Kitcher, P. 1993. *The advancement of science: science without legend, objectivity without illusions*. New York: Oxford University Press.
- Koertege, N. (ed.) 1998. *A House Built on Sand: Exposing Postmodernist Myths About Science*. New York: Oxford University Press.
- Konner, M. 1999. Darwin's Truth, Jefferson's Vision: Sociobiology and the Politics of Human Nature. Online version of Jul-Aug. 1999 *American Prospect*, available at <http://www.prospect.org/archives/45/45konner.html>
- Kuhn, T. 1996 [1962]. *The Structure of Scientific Revolutions*. (3e.) Chicago: The University of Chicago Press.
- Levitt, N. 1999. *Prometheus Bedeviled: Science and the Contradictions of Contemporary Culture*. New Brunswick: Rutgers University Press.
- Lewontin, R. 1991. *Biology as Ideology: The Doctrine of DNA*.

- Concord: House of Anansi Press.
- Martin, E. 1996. The Egg and the Sperm: How Science Has Constructed a Romance Based on Stereotypical Male-Female Roles. In E. F. Keller & H. E. Longino (eds.), 1996. *Feminism and Science*. Oxford: Oxford University Press.
- Patai, D. & Koertege, N. 1994. *Professing Feminism: Cautionary Tales from the Strange World of Women's Studies*. New York: BasicBooks.
- Radcliffe Richards, J. Why Feminist Epistemology Isn't. In Gross, Levitt & Lewis (eds.) 1996.
- Shapin, S. 1998. The Philosopher and the Chicken: On the Dietetics of Disembodied Knowledge. In C. Lawrence and S. Shapin (eds.). 1998. *Science Incarnate: Historical Embodiments of Natural Knowledge*. Chicago: University of Chicago Press.
- Soble, A. 1998. In Defense of Bacon. In Koertege (ed.) 1998.
- Wilson, E. 1995. Science and ideology. Available on Internet at: <http://cycad.com/cgi-bin/Upstream/Issues/science/wilson-ideology.html>.