A PEACE PLAN FOR THE SCIENCE WARS

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ABSTRACT: In what has become known as the 'Science Wars,' two sides have emerged. Some philosophers of science have claimed that, because science is a social practice, it is hopelessly infected with political bias. Others have claimed that science is a special kind of practice, structurally immune to bias. They are both right, because they are referring to different things when they use the word 'science.' The second group is referring the method of theory selection, as practiced by scientists in the laboratory, while the first group is referring to the ongoing social practice of science, of which theory choice is a part. The scientific method of theory choice, when practiced correctly, is resistant to bias, while the socially embedded practice is particularly sensitive to political forces, and so is subject to bias.

KEYWORDS: science, scientific method, science studies, theory, explanation

I.

Most wars are about territory, and a peace plan usually consists of some way to give both sides as much of the desired territory as is possible consistent with other claims of justice. The two (or more) sides never all get all of what they want. The science wars are a bizarre variation on this theme, and my proposed peace plan will have to be correspondingly bizarre. First of all, the territory at issue here is territory in logical space; each party to the science wars seeks to stake out a position, defend a thesis, that appears to be inconsistent with the other side's position and thesis, driving the enemy into retreat. But things are not always as they seem. My claim is that the two sides are occupying different territory, which they mistakenly believe to be the same territory. It is as if, in response to Hitler's invasion of Paris, the inhabitants of Paris, Texas rose up in revolt. What follows is my story of how these people could be so mistaken, and what they need to realize to come to armistice. This is one case in which peace talks are all that is necessary; talk can actually settle the matter, if the combatants can be made to listen.

There seems to be no sharp date when the science wars began, but this much is clear: when anthropologists and sociologists, encouraged by works like Thomas Kuhn's *Structure of Scientific Revolutions*,[1] realized that scientists have a culture to

[1] Thomas S. Kuhn, *The Structure of Scientific Revolutions*, 3rd ed. (Chicago, IL: University of Chicago Press, 1996).

which sociological and anthropological methods apply, and began to apply them, there was a panicked reaction. People like Paul R. Gross and Norman Levitt, authors of *Higher Superstition*,[2] began to describe the new study of science as an attack on science. Some 15 years ago, the hostilities reached a boiling point with the infamous Sokal affair. Alan Sokal, a practicing physicist, decided to embarrass the enemy by infiltrating their ranks. He wrote an essay that was, as physics, manifest nonsense, but espoused many of the ideas of his ideological opponents in science studies. He then submitted it for inclusion in an interdisciplinary journal, *Social Text,*[3] to demonstrate the intellectual bankruptcy of the field of Science Studies. The journal accepted his paper for publication, at which point he announced his imposture, and the Science Wars erupted in frenzy. Naturally, those who wished to study science as a social institution tried to answer these charges, and so the fight was joined. Tensions have died down somewhat, but not because any resolution was reached.

The two sides of the conflict can be generally characterized this way: Some, especially scientists themselves, claim that science is, at least in the long run, immune to bias and self-correcting; others claim that science is a tool of political power, and therefore has biases built into it in favor of the powerful. Consider the following passages extolling the unbiased nature of science, one from philosopher of science Richard Rudner, one from scientist Carl Sagan:

> The books, so to speak, are never closed on any hypothesis in the precise sense that evidence relevant to the confirmation or disconfirmation of it can never be exhausted. Accordingly, it is fair to say that if a hypothesis we accept is false, the continued application of scientific method to its investigation will increase the likelihood that we will be able to correct our error by coming upon evidence that disconfirms it. It is in this sense, of a systematically built-in mechanism of corrigibility, that the intellectual history of the species has presented man with no more reliable ... method of inquiry than that of science.[4]

> Scientists, like all human beings, have their hopes and fears, their passions and despondencies – and their strong emotions may sometimes interrupt the course of clear thinking and sound practice. But science is also self-correcting. The most fundamental axioms and conclusions may be challenged. The prevailing hypotheses must survive confrontation with observation. Appeals to authority are

[2] Paul R. Gross and Norman Levitt, *Higher Superstition: the Academic Left and its Quarrels with Science* (Baltimore: Johns Hopkins University Press, 1994).

[3] Alan D. Sokal, "Transgressing the Boundaries: Towards a Transformative Hermeneutics of Quantum Gravity," *Social Text* 46/47 (1996): 217-252.

[4] Richard S. Rudner, *Philosophy of Social Science* (Englewood Cliffs, NJ: Prentice-Hall, 1966), 77.

> impermissible. The steps in a reasoned argument must be set out for all to see. Experiments must be reproducible.[5]

Similar expressions of confidence in the objectivity of science can be found in the writings of many scientists and philosophers of science. If science is guided by its very structure toward truth, then it cannot also be guided toward some other end (political domination), unless those ends coincide. I take it as obvious that they do not, or at least they usually do not; the set of propositions acceptance of which is useful to the powerful is not the same as the set of true propositions, or even the set of propositions most likely to be true given what else we now know. Consider now these passages, arguing for the politically biased nature of science, one from Marxist scholar Louis Althusser, one from philosopher of science Helen Longino:

> There is no perfectly transparent science, which, throughout its history as a science will always be preserved ... from ... the ideologies that besiege it; we know that a 'pure' science only exists on condition that it continually frees itself from the ideology which occupies it, haunts it or lies in wait for it. The inevitable price of this purification and liberation is a continued struggle against ideology itself.[6]

> The contextualist approach also makes it clear that the question of whether social values can play a positive role in the sciences is really the wrong question. Social and contextual values do play a role, and whether it is positive or negative depends on our orientation to the particular values in question. The feminist scientist, or the radical scientist, cannot simply try to be sensitive to the politically noxious values embedded in some research programs or try to avoid ideology by sticking to the data. 'Letting the data suggest' is a recipe for replicating the mainstream values and ideology that feminist and radical scientists reject.[7]

In other words, science is conditioned by ideology, and so does not correct itself with respect to that ideology. Similar expressions of doubt about the objectivity of science can be found in Bruno Latour,[8] Sandra Harding,[9] and a host of others.

These claims cannot both be true, and yet both seem plausible. It does seem that the scientific method (insofar as there is a single method) is designed precisely to root out error and tend toward truer and truer pictures of the world. On the

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[5] Carl Sagan, *Broca's Brain* (New York: Ballantine, 1980), 96.

[6] Louis Althusser, *For Marx* (London: Allen Lane, 1969), 170.

[7] Helen Longino, *Science as Social Knowledge* (Princeton: Princeton University Press, 1990), 218.

[8] See for example, Bruno Latour, *Science in action: How to follow scientists and engineers through society* (Cambridge, MA: Harvard University Press, 1987), and Bruno Latour and Steve Woolgar, *Laboratory life: The construction of scientific facts* (Princeton: Princeton University Press, 1986).

[9] For example, Sandra Harding, *The Science Question in Feminism* (Ithaca, N.Y.: Cornell University Press, 1986), and *Whose science? Whose knowledge?* (Ithaca, N.Y.: Cornell University Press, 1991).

other hand, scientists are people, and scientific investigation is done by people in societies, and it would be amazing if they didn't bring their biases into the laboratory with them. We have two choices: we can take one side or the other, or we can find a way to reconcile the two views. I will argue that the two views are indeed compatible. When the proponents of the self-correcting nature of science say "Science is objective" and the proponents of science as an ideologically driven enterprise say, "Science is biased," they are not disagreeing, because they are talking at cross-purposes; they mean different things by the word 'science.' The former are talking about a method employed in theory choice, abstractly conceived; the latter are talking about a socially instantiated practice that has theory choice as a component. Consequently, it is possible for the abstractly characterized method of theory selection to be self-correcting, and yet be embedded in a larger practice that to some extent undermines, or even defeats, self-correction. In the end, the two sides can each have what they want.

This distinction between science as theory-choice procedure and science as social practice is easily confused with another, related distinction. For example, many scientists would admit that particular scientists may have let bias creep into their work, but that when they were doing so, they were doing bad science. In other words, it is ideal science, or good science, that corrects itself. But both sides to the debate can agree that there is good and bad science. The believers in science may admit that some scientists are biased, but they want to assert that it is not merely in the ideal that science corrects itself, but also in real practice. They want to claim that science as we actually do it has a tendency toward truth, which would be unwarranted if it were only science as ideally practiced that has that feature. Also, many of the political critics of science want to claim that even when science approaches the ideal of objectivity, it still serves political power. So the distinction between real and ideal science does not illuminate the problem.

Rudner, Sagan, and Sokal, along with the other defenders of the objectivity and self-correcting nature of science think of science as a method, structurally designed to weed out error. In particular, it is meant to weed out error due to the personal perspectives of scientists. The scientific method, as described in innumerable science textbooks, is something like this: a hypothesis is conceived, it doesn't matter how; logical consequences of that hypothesis are deduced; experiments are designed to see if those consequences are true; if not, the hypothesis is proven wrong, and the process returns to the beginning, with a revised or completely new hypothesis. If the consequences are correctly deduced, and the experiments are well-designed and well-performed, then the original hypothesis is refuted, even if it was the pet hypothesis of a well-beloved and authoritative scientist. Richard Feynman describes the method this way:

> In general we look for a new law [of physics] by the following process. First we guess it. Then we compute the consequences of that guess to see what would be implied if this law that we guessed is right. Then we compare the result of the computation to nature, with experiment or experience, compare it directly with observation, to see if it works. If it disagrees with experiment it is wrong. In that simple statement is the key to science. It does not make any difference how beautiful your guess is. It does not make any difference how smart you are, who made the guess, or what his name is – if it disagrees with experiment it is wrong. That is all there is to it. It is true that one has to check a little to make sure that it is wrong, because whoever did the experiment may have reported incorrectly, or there may have been some feature in the experiment that was not noticed, some dirt or something; or the man who computed the consequences, even though it may have been the one who made the guesses, could have made some mistake in the analysis.[10]

Feynman goes on to say that this picture is a bit oversimplified, but his further remarks only serve to add details to the three-part structure: hypothesis, deduction, experiment (see Figure 1).

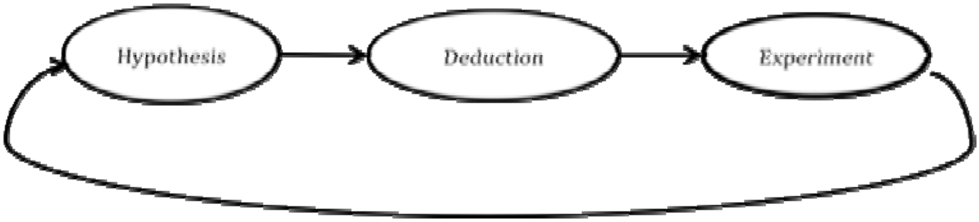

Figure 1

The results of the experiment then have an effect on what hypotheses get proposed, so the process is a self-correcting spiral, homing in on accurate representation of the world. It is easy to see how this understanding of what science is would lead one to think that it could not possibly be biased. If a biased scientist presents a faulty hypothesis, it will not be borne out by experiment, and so bias is rooted out, at least in the long run, by the structure of science itself.

II.

The critics of science as we now practice it do not see science as this idealized and highly abstract method of theory choice. The classical 'scientific method' is a component of science, but it is not the whole thing. They are thinking of science as a social practice that starts well before hypothesis with background information, distribution of resources and opportunities, and ends with publication and

[10] Richard Feynman, *The Character of Physical Law* (Boston: MIT Press, 1990), 156.

discussion of theories. What theories are accepted, published, and discussed forms the new background information out of which new hypotheses arise, so on this picture, too, science spirals, but the spiral is guided by more than just observation and experiment. It is because of these additional forces on scientific inquiry that science (in the 'practice' sense) can be biased, even if science (in the 'method' sense) is immune to bias.

Science as a social practice can be broken down into three stages: hypothesis selection, theory choice, and theory uptake. Theory choice has been the focus of much discussion of science, and so has become science itself for so many people, because it is amenable to abstract treatment. In particular, it is amenable to a normative understanding; understanding science as theory selection allows us to develop logics of science, and interpret particular cases of theory selection in terms of how well they achieve the goals of science, including an accurate picture of the world. But obviously there is more to how science gets done, and more to what scientific theories we accept, than the logic of theory choice alone. The scientific practice, as actually undertaken by real, working scientists, is better represented as a three stage structure, with theory choice taking place in a context of hypothesis selection and public uptake (see Figure 2).

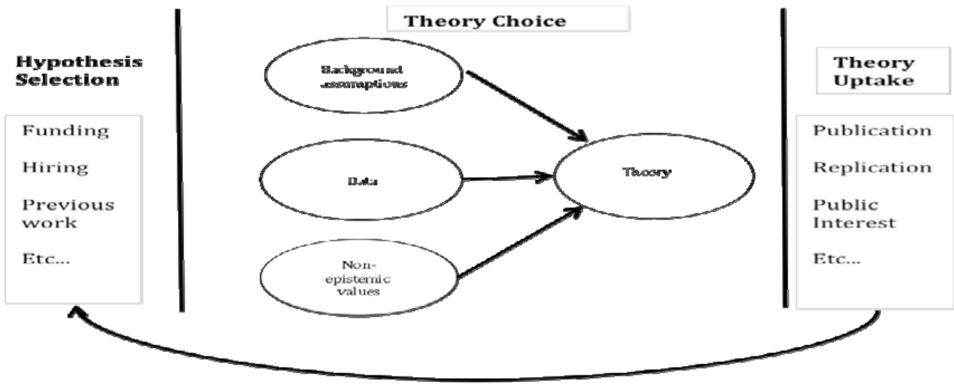

Figure 2

At the stage of hypothesis selection science gets its direction. To begin with, what science gets done is partly a function of what previous scientists have already done and what presently employed scientists would like to see done. Scientists are hired, promoted, and otherwise evaluated partly on the strength of how interesting the problems are that they are pursuing, so what we find out about the world is in part a function of what presently employed scientists find interesting. Hypotheses that no-one respects will have trouble finding funding and support; hypotheses that are very radical will be difficult even to formulate, for lack of a history. So, what theories we accept is constrained by what hypotheses get tested. At the theory uptake stage there are similar constraints. If no scientific society or journal finds your work important or interesting, it will not get published, and so other scientists will not try to replicate the results, and the general public will never find out about it. Evolutionary biology had to wait decades for Gregor Mendel's groundbreaking work because it languished in a second-rate journal that nobody was reading. Even if a paper on a problem considered marginal by the majority makes it to publication, if the scientific community doesn't pick up on it, discuss it, and expand on it, it vanishes into obscurity. So when we confine ourselves to consideration of the scientific method, it is true that any hypothesis, no matter what it is or who brings it up, is treated equally; however, when we turn to the social practice of science, we see that only hypotheses that can attract enough interest to get resources, publication, and discussion really have a chance to be accepted.

To show that bias is possible, due to the social nature of science, is not to show that it is biased in favor of the powerful. That requires more argument. But that further argument is readily available. To put it simply, if our community has a political preference, it can and will express it in what science it supports with funding for research. The federal government, in the hands of conservatives, is much less likely to spend money on research in drug treatment as an alternative to punishment, studies of the harmful effects of religion on communities, fetal tissue research, birth control device trials, and the like. If our society considers the ailments of women uninteresting, then scientists who insist on studying women's ailments will lose funding, respect, promotions, and employment opportunities. They will also find it hard to publish their results, no matter how well-designed their studies are. In all these ways, the communities of which we are part can help some projects and hinder others, all because we need those communities' help to do our research. If science has been biased in the past, then the research projects against which it is biased will have less of a history. In other words, there will be less previous work to draw on, so the work will be correspondingly more difficult. Research tools, like indices, can also be skewed against some research projects. If there is no category in *Psychological Abstracts* for Abused Woman Syndrome, then people who want to do research on that have a harder job tracking down what previous work there is.

The defenders of science might at this point concede that science-the-social-practice can be and probably is politically biased, but that the bias is safely confined to the stages before and after theory-selection. Consequently, they may affect the direction that science takes, but the structure of the scientific method will insure that, at least in the long run, we will be protected from accepting false theories. So we may overlook some interesting avenues of research, but we will not accept bad theories. There's something right about this. There are two kinds of bias, from two parts of inquiry: directional bias, that is, bias in what problems science pursues and what problems it ignores, and experimental bias, which is bias in how theories are chosen on the basis of the scientific method. Directional bias by itself will not lead us into error; the most it will do is lead us to miss some of the truth. Experimental bias is the kind of bias that causes trouble, because it will lead us to accept false theories. But the scientific method is supposed to correct for that, so we are only in danger insofar as we do science badly. Careful attention to experimental design and interpretation of results will at least keep us moving toward truer and truer theories. Unfortunately, this is not quite right. The picture of the scientific method schematized in Figure 1 is inaccurate, even as an idealization.

III.

Bias causes two different kinds of undesirable result: error and ignorance. In other words, bias can lead you to accept something false because you find it congenial, or it may lead you to ignore some truths because you find them uncongenial. To say that science is self-correcting and therefore structurally immune (in the long run) to bias may mean only that it is protected from error in the long run, not from ignorance. Experimental bias leads to error, directional bias to ignorance. It seems that the critics of science are right about the biases that occur in hypothesis selection and theory uptake. The defenders of science as an abstract method are right to point out that the scientific method is particularly well designed for avoiding error, but this does not mean that they are right about the immunity of the method from error, even in the long run. As nice as it would be to say that both sides are right, each in its own way, once the ambiguity of the term 'science' is pointed out, the fact is that even theory selection abstractly conceived is not immune to bias. The picture of the scientific method schematized in figure 1 is oversimplified.

Theories are supported by more than mere observation. Any set of observations is consistent with an infinite variety of different theories. Consequently, when we decide between empirically equivalent theories, we are always deciding on the basis of more than observational data. One way we narrow down the number of contender theories is by invoking nonepistemic values, like simplicity, predictive fruitfulness, and elegance. When scientists are faced with mutually inconsistent but equally empirically adequate theories, they frequently choose which to pursue in further research on grounds other than likelihood of truth. It may be, as Willard Quine (in some moods) says, that two theories that are completely empirically equivalent – that is, entail all the same observations – are mere verbal variants on one another. But we are not at liberty to say that about theories that are equivalent only in that they entail the same evaluations for observation sentences we already know the truth-values of, or ones we are likely to know soon. This limited kind of empirical equivalence is the pressing problem, and equally intractable if we limit ourselves to differences between the theories that bear on how likely they are to be true. Similarly, and also because of the underdetermination of theory by data, scientists judge particular theories in the light of what they already believe. If a scientist earlier in this century had a firm belief that all the laws of nature must be deterministic in character, not statistical, then she would be inclined to reject the standard understandings of quantum theory and accept hidden-variable versions of the theory. Other scientists accept the standard understandings of quantum theory as superior to hidden-variable theories (on the grounds of simplicity) and therefore reject the view that all the laws of nature must be deterministic. It is probably right to reject the deterministic view of law rather than the value of simplicity, but the point is, whether any given instance is right or wrong, these kinds of considerations have a role to play in theory choice. This feature of science makes it possible for political biases to play a role even in theory selection. If our society considers one kind of person inferior to another kind of person, then scientists will bring that prejudice as an auxiliary assumption to their research. They will tend to accept theories that support that claim, in spite of flaws, and be skeptical of theories that challenge it, in spite of support by data.[11]

Another way we narrow down candidate theories is by appeal to non-epistemic theoretical values. It is hard to see how what values we use to judge theories could be employed to political ends. Nevertheless, it almost certainly happens that theories are chosen over other theories for reasons that have nothing to do with whether they are likely to be true, and people with different political aims would accept different theories. Suppose for example that the two theories under consideration are indistinguishable with respect to empirical consequences, simplicity, and whatever other uncontroversial theoretical virtue you like. If the first theory is true, then a commercial application that will lead to gigantic profits will follow. If the second theory is true, then there is no readily exploitable commercial result, but there is an easy application to some problem in the third

[11] For two examples of cases in which this sort of thing seems to have happened, see Nancy Tuana's "The Weaker Seed: The Sexist Bias of Reproductive Theory," in her edited book *Feminism and Science* (Bloomington: Indiana University Press, 1989), and Stephen Jay Gould, *The Mismeasure of Man* (New York: Norton and Co., 1981). Longino, *Science as Social Knowledge,* is also full of examples.

world. People in science for the sake of technological application and profit will tend to accept and pursue the first theory, while people who see the role of science as one of improving quality of life for people will tend to accept the second theory. The theoretical value in this kind of case is tied to what the scientist takes to be the primary role of science in society, what 'good science' is supposed to do. Of course, if those theories really are alternatives, then one or both will be wrong, but it may take quite a bit of time to find out which, and in the meantime, science will continue in the direction it has taken because of a non-epistemic value choice. The upshot of all this is that science, because of its essentially social nature, can be and frequently is bent to political purposes, in spite of its tendency to root out certain kinds of errors.

So the peace plan is simple. Those who stand with Rudner, Sagan, Feynman, and Sokal can have their territory in logical space – that the scientific method, if applied correctly and with honest intent, will tend to expose error – as long as they have no further territorial ambitions, for example, to the claim that this gives scientists special social authority. Those who stand with Latour, Longino, Althusser, and Harding can have their territory – that scientists are people in a social setting, susceptible to political pressures that can and often do invisibly affect how their results come out – provided they have no further territorial ambitions, say, to the claim that there is no objective fact to which scientific theory aims to approximate. It may well be that scientists are especially objective folk and should have a special social authority, but the mere utility of the science-as-method doesn't show that. It may be that reality is radically socially constituted, but the mere social nature of science-as-practice doesn't show that.[12]

[12] An earlier version of this paper was delivered at the North Texas Philosophical Association meeting in April of 1997, and at the conference *Webs of Discourse: the Intertextuality of Science Studies*, held at Texas Tech University in February, 1998.