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ORGANIZED SKEPTICISM, NAÏVE METHODISM, AND OTHER -ISMS

ABSTRACT. The Science Wars have pitted defenders of science against those accused of attacking it with the weapons of constructivism and relativism. I argue that this defensive stance is in large part a consequence of two other -isms, organized skepticism and naïve methodism, that play a significant, if mostly unconscious, role in how scientists tend to think about science, and suggest that increased awareness of these -isms may help dissipate the perceptions of hostility.

"Isms" seem to play an important role in many, if not all, of the conflicts that plague modern times, and the Science Wars are no exception. The opponents can be roughly characterized as "defenders of science," a body that encompasses many historians and philosophers as well as practicing scientists, and "critics of science," by which I do *not* mean those opposed to science (although *some* indeed may be), any more than drama critics are opposed to the theatre, but rather those who are engaged in a critique of scientific practice. Many of the former group (see, e.g., Gross and Levitt, 1994), including the editor of this journal (Scerri, 2003), have specifically assailed the latter group for promulgating constructivism and relativism, philosophical stances the defenders consider extremely dubious at best and antiscientific at worst. Other commentators, including myself (Labinger, 1995, 1997; Labinger and Collins, 2001) have tried to re-present these stances in less bellicose terms, that do not necessarily lead to pitting one side against another. But I will not repeat that debate here. Instead, I would like to examine a couple of other -isms that have not been discussed in this context, which may help illuminate why scientists find it so easy to view these issues as polarizing, and even (dare I hope?) to start dissolving some of the differences. The discussion is not

Foundations of Chemistry (2006) © Springer 2006
DOI 10.1007/s10698-004-3531-2

specific to chemistry, although I have tried to select illustrative cases from chemistry.

The first is a term introduced by Robert Merton, usually considered to be the founder of the field of sociology of science. In his essay "The Normative Structure of Science" (Merton, 1973, pp. 267–278) Merton identified four central components that make science special: universalism, communism,[1] disinterestedness, and organized skepticism. There are several -isms there, but I focus only on the last. It is a commonplace that skepticism is central to the scientific endeavor; think back to Boyle's *The Sceptical Chymist* and the Royal Society's motto "*Nullus in verba*." But why did Merton choose refer to "*organized* skepticism" rather than simply "skepticism?" I've consulted with several currently practicing sociologists of science – Merton's intellectual descendants – and nobody has provided an explanation.

A possible clue may be found in Merton's suggestion, that the hostility of other institutions (such as religion) towards science has much to do with this norm of skepticism, because people find it difficult to distinguish between a rigorous demand for scrutiny of reasons for belief, and an indiscriminate, iconoclastic attack on belief in general (Merton, 1973, pp. 264–266). Hence Merton may have felt it particularly important here to make an explicit distinction between the methodological and institutional nature of this mandate, that all claims be examined and justified empirically and logically, as opposed to a purely personal skeptical attitude. In a recent book Ziman offers much the same interpretation; he points out, however, that aside from peer review, which is indeed (relatively) systematic, science practices skepticism in a mostly *dis*organized, unsystematic fashion (Ziman, 2000, pp. 42–44). So then, in what sense is scientific skepticism organized, any more so than universalism or the other normative characteristics? Wouldn't "institutionalized skepticism" or some other term be much more appropriate?

This may seem a fairly trivial point – what difference does it really make exactly what diction Merton settled on? – but the phrase "organized skepticism" has always struck me as a bit jarring. Perhaps that is because it evokes (for me, at least) the more familiar "organized religion." I find it intriguing that in several places Merton uses these two phrases – ''organized skepticism'' and ''organized religion'' – in close proximity (Merton, pp. 264–265, 277–278). By juxtaposing parallel verbal constructions I suppose he intended – if there was any particular intention – to emphasize the opposition between science and religion. But is it possible that there is something more subtle going on here – that he might also have meant it ironically, to call attention to some degree, however slight, of similarity between the two realms?

Admittedly, this interpretation may be far-fetched; but the thought of such a connection was inspired by my experience with *literally* organized skepticism – or, to be more precise, with a self-proclaimed skeptical organization. There are at least two such: one is the Skeptics Society, an organization founded by Michael Shermer (author of *Why People Believe Weird Things*, among other books), which publishes the journal *Skeptic*; the other is CSICOP, the Committee for the Scientific Investigation of Claims of the Paranormal, publisher of *Skeptical Inquirer*. Both are devoted to countering the prevalence of credulity in contemporary society; some of their favorite targets include paranormal phenomena, UFOlogy, astrology, and scientific creationism.

The Skeptics Society sponsors regular Sunday afternoon meetings, which are held on the Caltech campus, about 100 yards from my office. A few years ago I attended one of them. The scheduled speaker was someone I was very interested in hearing, and I'm generally in agreement with the Society's goals and positions, as well as with much of what I've read in their publications, so I expected to enjoy a pleasant hour or two. My expectations, alas, were not fulfilled.

To begin, Shermer ''warmed up'' the crowd, telling stories about particularly outrageous examples of what people are ready to believe in. It was entertaining, to be sure, and the audience was highly responsive. But I soon became rather uncomfortable. It seemed to me that he was essentially telling us how stupid others are, and how pleased we should be with ourselves for not being like them. After ten or fifteen minutes of that, he introduced the speaker, UCLA professor Jared Diamond, author of the best-seller *Guns, Germs and Steel* – a book I had found interesting, even provocative, but certainly controversial. Shermer acknowledged the controversy, but suggested that it primarily reflects resentment of Diamond for doing history the "right" way, as opposed to the traditional, non-scientific methodologies employed by most professional historians. He went on in this vein for some minutes, and then Diamond took the stage. I don't recall his precise topic, but basically he continued in the same posture: that his was the (only) correct way to go about doing history, which he proceeded to develop with specific illustrations. Again, most of the audience received this enthusiastically. But after fifteen minutes I couldn't take any more, and fled.

At the time I didn't really know why I found the whole affair so unpleasant. After all, most scientists make strong claims for their work, and most chairpersons introduce their speakers in the most positive terms; why was this any different? I now see why I felt so alienated, and what the connection is to Merton's ideas (which I read subsequently). This was a meeting of an organization whose mission is to promote skepticism – and yet we were essentially being encouraged to suspend all disbelief for an hour or so while receiving the officially approved Word. Shermer's introduction said, in effect, "This man is one of us. Yes, we're skeptics – but you don't need to be skeptical about *him*. If his work is controversial, it's because it offends those who don't think the same way we do."

In fact, I later realized, my discomfort was much the same as that I have felt, occasionally, in other situations – at meetings of political advocacy groups and, most significantly, at some religious services. There was the same feeling of enforced solidarity, of implicitly signing on to a body of belief by one's mere presence. What could be more foreign to Merton's concept of the role of skepticism in the scientific community? And yet, this example of skepticism organized in the literal sense seemed to take on some of the hallmarks of organized religion, suggesting that it is very difficult for a skeptical organization to turn inward and permit, let alone encourage, skepticism about itself and its own core practices and beliefs.

It seems to me that there is a clear connection between these observations and the hostility that many scientists have expressed towards science studies. Skepticism is a proper scientific attitude, they would surely agree with Merton; but they see any expression of skepticism *about* science as inherently antiscientific, a subversive assault on its fundamental methodology, that opens the door to all the irrational forces that are the implacable enemy of science. Earlier I noted Merton's point, that hostility *towards* science may be a consequence of equating the examination of grounds for belief with the desire to undermine those grounds. I would suggest that a parallel argument may well apply to hostility *from* science towards science studies.

If this argument has any validity, we would expect to find these skeptics' organizations to be particularly hostile to science studies, and indeed a number of attacks can be found in the journals they published. One such, an article by physicist Robert L. Park (adapted from his recent book *Voodoo Science*) mainly goes after some of the usual suspects, such as Transcendental Meditation; but it also contains a particularly revealing comment, wrapped inside a brief discussion of the global warming debate (more on which later). Park notes that there have been, and continue to be, substantial disagreements between experts on whether global warming is a serious problem, and comments:

> If scientists all claim to believe in the scientific method, and if they all have access to the same data, how can there be such deep disagreements among them? What separates the two sides in the climate controversy, however, is not so much an argument over the scientific facts, scientific laws, or even the scientific method ... What separates them are profoundly different political and religious world views ... the antagonists believe sincerely that they are engaged in a purely scientific debate ... but earlier world views "learned at their mother's knee" tend to occupy the gaps in scientific understanding. (Park, 2000, pp. 26–27)

That sounds almost like something from a science studies piece, doesn't it? But Park goes on:

> This sort of dispute is seized upon by postmodern critics of science as proof that science is merely a reflection of cultural bias, not a means of reaching objective truth. They portray scientific consensus as scientists voting on the truth. That scientists are influenced by their beliefs is undeniable, but to the

> *frustration* of the postmodernists, science is enormously successful. (Park, 2000, p. 27; my italics)

Admittedly, it isn't completely clear just who he means by "the postmodernists;" but since most scientists who have spoken out on this theme seem to treat "postmodern" and "critics of science" as inseparable phrases, it probably isn't unfair to take this as a slap at science studies. But where does it come from? It appears totally gratuitous in the context of the previous quote – especially the word "frustration."

Now observe how easily Park's polemic can be transformed into an argument *for* science studies, by just eliminating some of the more pejorative words and rearranging a bit:

> This sort of dispute is of great interest to those who seek to understand how it can be that while science is to some degree a reflection of cultural bias, and scientists are influenced by their beliefs, nonetheless science is enormously successful.

Isn't that the sort of question that a true skeptical inquirer might ask here? But that would require acknowledging that critics of science – those outside the organization – might also be guided by the skeptical spirit. Apparently Park's status as organization man blinds him to that possibility, even though it would seem to follow reasonably from his previous points. Instead, he attacks – but there are no details offered in support of the offhand attack. Presumably Park, like so many others, feels that the sins of the postmodernists speak so loudly for themselves that justification would be entirely superfluous.

That stance is, alas, all too common. Again, I could provide many examples, but will limit myself to one of the most egregious manifestations, which was provided by Richard Dawkins a few years ago. He sneers at what he calls the confused state of postmodernist thinking, and offers the following as an illustration thereof:

> The figure/ground distinction prevalent in *Gravity's Rainbow* is also evident in *Vineland*, although in a more self-supporting sense. Thus Derrida uses the term "subsemioticist cultural theory" to denote the role of the reader

as poet. Thus, the subject is contextualized into a postcultural capitalist theory that includes language as a paradox. (Dawkins, 1998, p. 41)

Just what *is* Dawkins illustrating here? That each of those statements is nonsensical? Maybe; but it's far from obviously so. Take the first: even though I've read both of those books (has Dawkins?) I don't know whether it might be a valid and insightful comment on Pynchon. But if I ran across it in an essay I would have no *a priori* reason to do anything but accept it as meaningful, just as I would if it were instead an article about some scientific topic in which I lack expertise.

As an experiment, after reading Dawkins I picked up the topmost journal from a pile on my desk, turned more-or-less at random to an article in a field I am only somewhat familiar with, and about halfway through ran across the following sentence:

On the other hand, doping with an aliovalent cation perturbs the periodic potential of the oxide-ion array so as to trap the vacancies at the aliovalent ions that introduce themselves, thereby increasing E_a. (Goodenough, 2000, p. 823)

On its face, in isolation, does that seem any less arcane than one of Dawkins' examples? (Especially if one doesn't have any more idea of the meaning of "aliovalent" than of "subsemioticist" – as I didn't, until I read the entire piece.) But surely if Dawkins happened to read it, he would assume the author knew what he was talking about. He would never dream of criticizing it, even as a piece of relatively impenetrable writing, let alone as meaningless nonsense, though it *would* be mostly meaningless to him. Why won't he grant the same to the likes of Derrida?

Alternatively, perhaps Dawkins merely means that the set of three sentences above are nonsensical taken together. I concede that's true, but it proves nothing: the passage was *intended* to be nonsense. It comes from a website with the following self-description: "The essay you have probably just seen is completely meaningless and was randomly generated by the Postmodernism Generator." (Bulhak) So what we have here is Dawkins citing a deliberately nonsensical parody as supposed evidence of the nonsensical character of that which is being parodied. No doubt he would be (justifiably) outraged if one of his critics cast all logic to the winds and turned such a rhetorical weapon against him; why does he blithely do so himself?

I believe that this and the other examples cited are closely connected to the somewhat paradoxical aspect of organized skepticism discussed above: members of the organization are given all benefit of the doubt, while outsiders are allowed none. They are not even considered to be engaged in the same form of intellectual activity, and therefore do not need to be treated according to the implicit rules and conventions that tradition requires. And it is all too easy to go on to conclude, like Parks, that skepticism outside the organization arises from the desire to see the organization brought low.

Of course, the majority of scientists do not belong to any skeptics organization – nor for that matter are they particularly aware of science studies or even the Science Wars over them – and yet often appear to be predisposed to statements more or less supporting the side of the defenders of science. I suggest this can be understood as the consequence of another prevalent -ism, which I will call "naïve methodism."[2] "Methodism" here has nothing to do with religion, just as Merton's "communism" had nothing to do with politics. It refers rather to the fact that, as a number of commentators have pointed out, scientists appear implicitly and unquestioningly to accept the notion that there is a single, well-defined, universally valid scientific method. Not that they have any coherent conception of what it actually *is*, as Nobel-winning-biologist-turned-philosopher Peter Medawar long ago observed:

> Ask a scientist what he conceives the scientific method to be and he will adopt an expression that is at once solemn and shifty-eyed: solemn, because he feels he ought to declare an opinion; shifty-eyed, because he is wondering how to conceal the fact that he has no opinion to declare. (Medawar, 1969, p. 11)

but they are confident that there is one, that can be appealed to when needed. (Hence my choice of "naïve" as a modifier.) Philosopher of science Philip Kitcher, himself arguably more a defender than a critic (see, e.g., Kitcher, 1998) has described this state of mind as "Legend" (Kitcher, 1993).

I encounter one manifestation of this attitude frequently: I receive on the order of 20–30 papers a year to referee, many or most having to do with problems in mechanism; and every year I find one or two that cite one particular methodological principle: that of Ockham's Razor. Usually this takes the form of "well, we can't really say how this reaction works, but there's another reaction that looks similar, so Ockham's Razor tells us that it is correct to conclude that this one works the same way." The fallacy of such argumentation has been well expounded by Hoffmann and coauthors (Hoffmann et al., 1996), so I will not repeat it here.

I may be particularly sensitive to this issue as a residue of my first research problem, as a beginning graduate student, which was to investigate the mechanism of the oxidative addition of alkyl halides to an iridium(I) complex (Eq. (1)).

$$(1)\quad IrCl(CO)(PR'_3)_2 + RX \longrightarrow (R)(X)IrCl(CO)(PR'_3)_2$$

There was substantial evidence at the time that when $R = $ methyl the reaction proceeds via the common S_N2 mechanism, with the Ir center acting as nucleophile; the classic "definitive" proof of S_N2 is inversion of configuration at carbon, which cannot be readily tested using methyl halides. Accordingly we designed a more complex alkyl halide whereby the stereochemical consequence of oxidative addition could be determined by NMR, and obtained (and published) results which seemed quite consistent with inversion (Labinger et al., 1970). We then went on to design further, improved test molecules, and found results completely *in*compatible with inversion. Further study revealed that a combination of misleading coincidences had led us to misinterpret the earlier result: all our probe molecules react via a radical-chain mechanism, not S_N2, although the latter apparently *does* operate for methyl halides, as had been previously thought. In fact there are two completely different mechanisms at work here, for the same class of reactants RX, and relatively small changes in the nature of R can effect the switch from one to the other (Bradley et al., 1972).

In retrospect, it seems very likely to me that our hasty publication of the first (erroneous) conclusion had more than a little to do with a (mostly subconscious) application of Ockham's Razor. Most of its appearances in the papers I've refereed seem fairly harmless, at worst offering a questionable conclusion on an issue where total agnosticism might have been more appropriate. More than a few times, though, I was able to suggest further experimentation that could shed light on the question; the investigators missed those possibilities and/or felt additional work was unnecessary. That was due in part (I feel sure) to their unreflective obeisance to an illusory methodological principle.

Although this story shows naïve methodism having a (negative) impact on the *practice* of science, a more common (and more relevant to the theme of this essay) version may be seen in how scientists *talk about* science, rather than how they actually *do* it. A telling example arose in an exchange in the Letters to the Editor section of *Chemical & Engineering News* (the weekly publication of the American Chemical Society) a few years ago. An editorial on the danger of global warming (Baum, 1999) elicited a number of responses, some quite hostile. One of the more temperate correspondents criticized the research supporting global warming as inconsistent with proper scientific method:

> Like many others, I was taught to believe that the secrets of our physical world can be discovered through the scientific method. As scientists, we daily must evaluate our work to see if it meets scientific standards. Let us for a moment evaluate the global warming hypothesis without forecasting or alarmist thinking: Suppose our task was to determine the role of electron transfer in DNA, which is controversial in its own right. If we were given data from 80 separate experiments, each performed under unknown conditions, with different detectors, measured on an inappropriate timescale, would we feel confident in plotting an activity versus concentration graph and drawing a line through the data? No doubt we would not. Neither would we spend our time second-guessing the results, adjusting them to agree with our way of thinking. Science is not undertaken in this manner. Unfortunately, as Baum correctly points out, the global warming debate has become "political" and based on "incomplete science." (Barden, 1999)

My (admittedly fanciful) response:

I commend Christopher Barden not only for acknowledging the civil tone of Rudy Baum's piece on global warming, but also for responding in kind (unlike one or two of the other correspondents) in his letter. But it seems to me that his invocation of "the" scientific method and scientific standards is founded on an idealized picture of science that is difficult to sustain in the real world.

He offers as analogy the controversy over electron transfer in DNA, and notes that there, as with global warming, it would be unsound to combine the results of 80 disparate experiments, made under different and perhaps incommensurate conditions, and reach any confident conclusion. True enough. But "confident" is the key word, and it may mean different things in different contexts. Let's push this analogy a bit further.

Suppose a scientist is sitting in his lab, poring over the above-mentioned pile of data, and is on the verge of coming up with the killer experiment that should reconcile all the discrepancies. Suddenly, Maxwell's Demon appears (a *deus ex micromachina*, perhaps?), and says, "Give me the answer to this problem right now. Of course, I know what it really is and if you're wrong, you'll be eternally exiled to the realm of infinite entropy." Does our hero respond, "No, I won't. My data are incomplete and it would be unscientific to do so. Bring on your $\triangle$S!" ? Or does he take his best shot, maybe even doing the meta-analysis that Barden dismisses?

Wait a minute, I hear you cry, didn't I just appeal to the real world a paragraph back? How can I offer such a frivolous fairy tale as a serious argument? But I don't think the analogy is all that far-fetched. I am certainly no expert on global warming, but it doesn't seem unlikely to me that we will reach a point (some say we're already there) where we don't have enough data to reach what Barden would consider a confident conclusion, but can say that a particular model has some chance of being right, and that if it is right, failing to take action promptly would have unacceptable consequences. What does the scientific method tell scientists to do in such a situation? Do we recommend waiting until we can be sure? If (Demon forbid) such a worst-case scenario does come to pass, can we absolve ourselves from responsibility – after all, in recommending inaction, weren't we acting according to proper scientific rules? (And, if we were to offer such a defense, would anybody be much impressed?)

Barden believes it is unfortunate that the global warming debate has become politicized, and that the "sense of advocacy" is potentially damaging to science. I can certainly share his longing for clear-cut scientific answers, but the model that he was "taught to believe" in (as was I) is just not realistic, as I think we all come to recognize, to some degree, when we leave the classroom and start actually practicing science.

Back in the real world, we cannot separate science from politics, particularly in such a complex and significant case as global warming. But we continue to display to society a scientific puritanism, insisting that a clean separation is possible, and that we can always make decisions based on judging our work against some context-independent set of absolute scientific standards. By doing so, I fear we risk damaging science and its public image at least as much as the advocacy of which Barden complains. (Labinger, 1999)

My point (leaving out Maxwell's Demon etc.) is that there is much less at stake in the DNA problem, and nothing much to lose by waiting for more and/or better data, a luxury we may well not have when it comes to global warming. A DNA researcher faced with a demand for an immediate "best" answer to the problem, under the threat of a severe penalty for failure, might well decide to resort to the sort of meta-analysis dismissed as unscientific by Barden. More generally, what constitutes "proper scientific method" cannot be absolute and context-independent. I would have been most interested to see responses to my argument but, unfortunately, the exchange ended there.

I suspect, though, that the majority of scientists who line up with Barden do so more or less automatically. It's not that they have considered and reject alternative viewpoints such as those often espoused by science studies; probably they have not even been exposed to them. Instead, they have been trained (at their teachers' knees, if not their mothers') according to the traditional pure science viewpoint: scientists should (and can!) resolve the scientific issues, using "the" proper scientific method; then and only then should their findings be handed over to politicians and policy makers to decide how to act upon them. But faced with a real-world problem to solve, with temporal, political and all sorts of other constraints, and seeing an approach that *might* offer some help, would they really be deterred by considerations of an idealized method? Perhaps, under the right circumstances, even Feyerabend's (in)famous dictum "Anything goes" might no longer seem so outrageous?

Granted, all of this discussion has been mostly anecdotal, but I believe that two implications have some general validity. First, that the two -isms, naïve methodism and organized skepticism, can color (often unconsciously) the way scientists view science. And second, that the combination influences scientists' attitude towards those whose science studies are informed by varying degrees of relativism and constructivism. It may be particularly responsible for the tendency to conflate the latter with an antiscientific stance. In the light of a little more self-awareness about the scientific -isms, one might be more sympathetic to the idea that those science studies are akin, not diametrically opposed, to the activities the scientists they are studying. I do not propose instituting a new scholarly discipline of "comparative ismology;" but I would hope that people might examine their own -isms, conscious or otherwise, before coming down too hard on those of others.

NOTES

1. Of course by communism he meant nothing political, but rather that scientific knowledge is held to belong to all; communalism is perhaps the better term.
2. This echoes the phrase "naïve realism", which is generally used non-pejoratively, even by those often taken to be opponents of that philosophical stance. For example: "Natural scientists, working at the bench, should be naïve realists – that is what will get the work done." (Collins and Yearly, 1992, p. 308) Here I take "naïve to be perhaps a bit less innocuous.

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