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ESSAY

Shamans of Scientism: Conjuring Certainty Where There Is None

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Abstract—Some vociferous proponents of established science assert that it should always be believed when there are controversies over issues of public importance. That assertion rests on three assumptions, none of which are usually made explicit: 1) that only science is capable of arriving at truths about the natural world and that it actually does so; 2) that "science" is identical to the views propounded by the contemporary prevailing establishment of science, its mainstream institutions; and 3) that science can be distinguished unequivocally, with certainty, from everything else. None of those presumptions has been demonstrated to be correct, and indeed there are excellent historical and logical reasons to regard all of them as wrong. Since these underlying assumptions cannot be proven, self-styled "skeptics" and other activists who seek hegemony for contemporaneous scientific consensuses engage in intellectual and rhetorical misdirection to give actual uncertainty the appearance of "practical" certainty. In doing so, activists fail to acknowledge the crucial distinction between a point of view that appropriately guides scholarly discourse or scientific research and views (expert opinions or assertions or conclusions) that "everyone" should accept and that offer appropriate guidance to public policies and actions.

Introduction

Mainstream science typically resists unorthodox claims, even when they turn out later to have presaged a revolutionary advance (Barber 1961). In an increasing range of fields, mainstream science has become increasingly dogmatic (Bauer 2012a). Challenges to established views are resisted by those seeking to discredit them as mistaken, wrong. When the stakes are particularly high, the seemingly objective and technical pejorative "pseudo-science" is being superseded by the emotionally more evocative "junk science" and "denialism" (Bauer 2013).

Relatively minor deviations from orthodoxy are resisted via the normal routines of science: Methods and theoretical approaches are subjected to critical scrutiny, the soundness of the evidence is questioned, and the unorthodox conclusions drawn are then said to be shown to be incompetently arrived at and wrong.

Radical challenges to mainstream views, however, like those of interest to the Society for Scientific Exploration and the *Journal of Scientific Exploration*, typically encounter a different mode of resistance: Instead of confronting the presented evidence and its provenance, such challenges are dismissed as pseudo-science, at times because they are declared to belong to a whole intellectual territory that has already been pronounced pseudo-science (Gardner 1957), say psychic phenomena, homeopathy, or cryptozoology (Loxton & Prothero 2013).

Such a shortcut that avoids grappling with the specifics of methods and data relies on unstated assumptions: that only science is capable of arriving at correct descriptions of the real world, that one can definitively recognize something to be science as opposed to not-science or pseudo-science, and that contemporary institutions of science and their members or representatives can speak with authority about what science is and knows.

The belief that only science is capable of arriving at truths about the natural world, generally described as scientism, has not gained general acceptance in the intellectual world. Similarly, the belief that science can be definitively distinguished from not-science or from pseudo-science is not generally accepted in the intellectual world. Further, equating "science," which enjoys enormous public prestige and status, with the contemporary consensus flies in the face of the historical fact that science has progressed by modifying or superseding successive sets of mainstream consensus, demonstrating that they had *not* been sound knowledge (Barber 1961, Kuhn 1970).

The philosophy of science has a long record of discussing the so-called "demarcation problem" of distinguishing proper science from not-science, but there has not emerged anything like general approval of any principles or a set of criteria by which science could be indubitably recognized as such, and by means of which a particular case could therefore be indubitably diagnosed as pseudo-science. Indeed, several decades ago Larry Laudan (1983) had already declared "The demise of the demarcation problem."

Relatively sophisticated critics of radical unorthodoxies disclaim belief in scientism,[1] and they ignore or reject the points made by Laudan (and others). For instance, their criticisms of challenges to mainstream views may still invoke purported demarcation criteria like "the scientific method,"[2] or falsifiability, or science's supposed automatic self-correction, none of

which have had any significant traction for decades within the philosophy of science or other informed discourse. A variety of more-or-less ad hoc sets of criteria have also been proposed at various times to characterize pseudo-science, but none have stood the test of being applicable only to "pseudo" and not to instances of "proper" science (Bauer 1984:142–148).

The purpose of this Essay is to illustrate the intellectual weaknesses and misleading contentions that seem inevitable when scientistic belief and faith in the possibility of demarcation underlie critiques of unorthodox claims. Such critiques amount to attempts to conjure certainty where there is actually none to be had. Since any such attempt contravenes sound thinking, the arguments made are perforce deceptive and misleading.

This Essay will first give a summary of the argument against the possibility of a definitive, indubitable demarcation of science from everything else. Then, there follow illustrations of the rhetorical trickery by which self-styled "skeptics," here an activist philosopher and an activist lawyer, deploy sleight of words to give the impression that science can be unequivocally distinguished from imposters and that science should always be unequivocally trusted to be right.

The Demise of the Demarcation Problem

The search for criteria to distinguish science unequivocally from non-science—the demarcation problem—is a priori unlikely to be successful. Sought is a rigorous set of criteria for defining a human activity that has changed over time and whose acknowledged components are diverse. Everyone agrees that chemistry, physics, geology, astronomy, biology—the physical sciences, sometimes called the "hard" sciences—constitute "science." Yet commonalities cannot be identified within all the activities regarded as "proper" in those fields. Scientists do a variety of different things; they observe, experiment, theorize, etc. Even the conventional wisdom that science judges a theory's validity empirically and pragmatically, by the evidence offered from the natural world, is not true of actual scientific activity, where theorists are likely to disbelieve experimental results that do not conform to contemporary paradigms (Bauer 1992:Chapter 2).

Science has grown and diversified explosively, especially during the last century or so, and it was not at all obvious earlier, say toward the latter part of the 19th century (Knight 1986), that it would be impossible to define science with logical accuracy. Indeed, several generations of philosophers of science, some of whom were also active scientists, worked at the problem. The definitive summary, cited by all who agree and also by those who disagree, was given by Larry Laudan (1983). Here is a synopsis of the argument:

At least since ancient Greece, "Western philosophers thought it important to distinguish knowledge (episteme) from mere opinion (doxa), reality from appearance, truth from error." Over the centuries, and particularly in the Western world in the last half-millennium or so, "science" became equated with "knowledge,"[3] yet by the middle of the 19th century, it had also become clear that "science"—empirically derived knowledge about the world—is fallible: Theories (understanding) had to be modified every now and again. Since certainty did not work as the criterion for being "science," efforts then focused on showing that science has a method that is better than all other approaches; however, from the beginning it was acknowledged not to be infallible. Moreover, no agreement could be reached about what the method was—naturally enough, given that scientific activity includes such a variety of different approaches.

Demarcation criteria must be both necessary and sufficient. If they are only necessary, then they cannot definitively pronounce something to be a science. For example, if it is claimed—as it often is by self-styled "scientific skeptics" and other pundits—that scientific results should be reproducible, that would be merely a necessary condition and would not even exclude mistakes, which are often reproducible. If criteria are merely sufficient, then they do not make it possible to designate something (say, astrology) as not-science or as pseudo-science: A sufficient condition for an activity being a game is that two teams try to outscore each other, but that alone would not suffice to account for why solitaire, singles tennis, or bingo are games.

Laudan then demonstrates that none of the suggested candidates for demarcation actually work: not "the scientific method," not falsifiability, not the abhorrence of ad hoc adjustments to theory, and not the attempt to distinguish "progressive" from non-progressive research programs.

The upshot is a lack of definitive, indisputable, universally applicable criteria by which genuine science can be distinguished unequivocally from pseudo-science. In each specific case, detailed scrutiny is called for to assess the appropriateness of methods, the soundness of obtained data, and the validity of inferences and conclusions (Bauer 2001). Six-thousand-year creationism can be challenged legitimately on the grounds of fossil evidence, radioactive dating techniques, etc.; but simply pronouncing it "pseudo-science" is not legitimate unless one has shown why that label applies, and for that there are no available shortcuts, only the aforementioned route through fossils and dating, etc.

Another way to look at the issue is to note that "science" has the pre-existing meaning of biology, chemistry, physics, etc. Identifying the criteria or principles common to all of those is a problem of *induction*, and philosophy has long understood that induction cannot lead to an

unequivocally and universally applicable principle or criterion. To dismiss something as pseudo-science without examining its individual claims and the associated evidence would call for *deduction* from some universally applicable principles or criteria that define science, and induction cannot yield such criteria or principles.

All that can be achieved validly is to note, on every specific issue, how faithful to evidence and logic are all aspects of the investigation and of the conclusions drawn. One may then legitimately have grounds for concluding that the activity and its claims are more or less worthy of serious attention, but that remains a significant cry from true or not true: It remains a matter of probabilities, in other words fallible—*doxa* and not *episteme*.

Conjuring Certainty

The ambition to label certain matters as pseudo-science stems from a wish to prove to everyone that those matters are misguided or downright mistaken in some manner, at any rate not to be taken seriously, let alone believed. Invoking the authority of science to that end, instead of arguing the detailed, specific validity of evidentiary claims, amounts to asserting the untruths that science is always trustworthy, that the contemporary consensus equals science, and that what science is can always be recognized unequivocally. Therefore, activists and vigilantes who seek to root out "pseudo-science" wherever they see it have to somehow show that probabilities of less than one can be legitimately equated with certainty. Since that is not logically possible, the pertinent literature is replete with sleight of words, rhetorical tricks, and polemic devices. The next section contains examples of this from a representative of "skeptics" groups who happens to be a philosopher, and from a lawyer concerned over the influence of unsound science in court cases.

A general tactic is to acknowledge the general fallibility of science and to follow that with the assertion that, however, *in this particular case and for all practical purposes*, established science should be granted unquestionable authority.

The Philosophy of Pseudo-Science

The *Encyclopedia of Philosophy and the Social Sciences* has an entry for "Pseudoscience" (Pigliucci 2013) that illustrates the intellectual contortions of those who wish to dismiss subjects they regard as spurious by labeling them pseudo-science. The author is a philosopher by present profession as well as a card-carrying "skeptic"[4] who has published a flawed compendium of alleged pseudo-science (Pigliucci 2010).

Pigliucci admits Laudan's critique to be "important," but calls "much too restrictive and impractical" Laudan's conclusion that "epistemic warrant should be attached to specific claims, not to broad endeavors." Note that Pigliucci does not contradict the logical soundness of what Laudan says; but he evidently has practical aims that could not be pursued if Laudan's conclusions, unquestioned on philosophical or logical grounds, are accepted. Like other self-styled "skeptics," Pigliucci wishes to be able to dismiss whole fields as pseudo-science without taking the trouble to argue specific cases:

> When a field like astrology has repeatedly, and for a long time, demonstrated its inability to make progress—due to the incoherence of its theoretical constructs (e.g., "constellations" are actually optical illusions) and its failure on empirical grounds—it seems the time has arrived to archive the whole thing as not warranting any more serious investigative efforts.

Note how many assertions here lack explicit support, how many critical points are left indefinite. Does "astrology" include the empirical data in which there seem to be correlations between positions or movements of the planets, sun, and moon on the one hand, and birth dates or times and personal characteristics on the other (Gauquelin 1991, Ertel & Irving 1996)? Why do these empirical correlations not constitute progress beyond the chart-drawing of centuries past? What is unscientific about using rigorous methods to study such correlations?[5] How long constitues "a long time" in fields "like" astrology—and "like" in what respects? And when someone like Ertel or Gauquelin publishes an empirical set of data on astronomical entities and human behavior, can that be automatically categorized as "astrology"?

Grant that traditional astrology has failed on empirical grounds, but how can Pigliucci be sure that the fault lies in the "incoherence" of its theoretical constructs? Which constructs exactly? And what constitutes "the whole thing"?

Who is Pigliucci addressing here? No one is asking him to put "serious investigative efforts" into "astrology." But also, no one has given him a warrant to tell other people on what they should or should not spend their investigative efforts. Does he propose that the study of "astrology" be declared a socially abhorrent or even criminal activity?

> Labeling something as pseudoscience—if called for—serves the same practical shortcut function of throwing an obviously frivolous lawsuit out of court before one invests money and time in something that has no chance of succeeding.

Again, the devil is in the details. What criteria are available to determine whether that label is "called for"? The analogy with "obviously frivolous" would only hold if it were shown—demonstrated, not merely asserted—that the alleged pseudo-science is "obviously" so unfounded as to have "no chance of succeeding." For that matter, are "obviously frivolous" lawsuits so easily classifiable as "frivolous"?

Pigliucci here admits explicitly that the "practical shortcut function" is just an attempt to avoid having to bother with proving the case through attending to the specific details of the claims. If those details are "obviously" unsound in some manner, why not simply list them and say why they are "obviously" faulty? If something is obvious, why is a shortcut called for?

It gets worse, if that seems possible:

> The current philosophical literature on pseudoscience is exploring some of the alternatives to the classical demarcation approach briefly mentioned above such as solutions based on fuzzy logic or on making more precise the notion of Wittgensteinian family resemblance concepts . . . The abandonment of the quest of necessary and jointly sufficient criteria to define science and pseudoscience in favor of, for instance, Wittgenstein-type family resemblance ("fuzzy") concepts, constitutes progress, not failure.

Since "family resemblance" needs to be made "more precise," it evidently isn't precise as it stands, and yet it is exactly precision that is called for before asserting unequivocally that something is or is not science or is or is not true. "Family resemblance" may not amount just to "I know it when I see it," but it is no answer to the demarcation problem. Implying that it offers hope toward that is misdirection. "Fuzzy logic" is itself already semantic misdirection, because it is nothing like the common understanding of "logic" as precise and infallible. In what way could having only a "fuzzy" way of identifying pseudo-science constitute progress over definitive criteria?

It does so only in the sense of being faithful to the reality that definitive criteria are not to be found, which is not Pigliucci's intended meaning.

There are no grounds on which to quarrel with philosophical discourse aimed at better defining what can be done with the concepts of family resemblance and fuzzy logic. But Pigliucci's aim is to be able to pronounce something as *unequivocally* pseudo-science, *unequivocally* not worth paying any attention, *unequivocally* without truth value, in order that the conventional wisdom and public policies should be shaped accordingly. To that end, asserting as progress the replacement of precise criteria by imprecise resemblances is just another rhetorical trick to inveigle the reader into forgetting that the warrant for unequivocal categorizing is lacking.

Junk Science in the Courtroom

Peter Huber (1991) expresses amply justified disgust at some of the results of court cases in which sound, scientifically based knowledge was brushed aside. A fine example is Charlie Chaplin being forced to pay a settlement for fathering a child who could not have been his, as demonstrated by blood typing. Such junk-science verdicts were becoming common (in 1991), according to Huber.

Unfortunately, Huber equates sound knowledge with science, and science with what the consensus happens to be at any give time. So, like philosopher Pigliucci, lawyer Huber engages in rhetorical excesses, unwarranted analogies, and generalities instead of specifics as he tries to argue that keeping junk science out of the courtroom means always accepting the contemporaneous consensus in the pertinent scientific discipline. Perhaps that is why he seems to accept as proven (Biello 2009) the unproven (Bauer 2012b) hypothesis that carbon dioxide is causing global warming.

Certainly there have been court cases, cited in detail by Huber, where plaintiffs collected damages and corporations paid additional punitive fines just because it *might* be that, for instance, environmental pollution could cause cancer and other ailments. To my mind, the appropriate solution would be that the lawyers in each such case be sufficiently knowledgeable as to be able to find the right expert witnesses and to cross-examine the plaintiff's witnesses in ways that would expose the dubiousness of their testimony. Instead, Huber wants to take shortcuts, like declaring as junk science all such claims of environmental harm: "Take the serious sciences of allergy and immunology, brush away the detail and rigor, and you have the junk science of clinical ecology" (Huber 1991:2). Might Huber rethink his dismissal of environmental chemicals as a cause of cancer now that the World Health Organization has asserted unequivocally that they are (Brumfield 2013)? If not, why reject the official mainstream view now when he accepted it earlier?

Huber (1991:3) also contrasts properly scientific physical therapy with the junk science of chiropractic, and the properly scientific orthopedic surgery with the junk science of osteopathy, yet osteopathic doctors are fully licensed to practice medicine and to dispense drugs, and chiropractic has fared at least as well as mainstream therapies in clinical trials about coping with lower-back pain (Rubinstein, van Middelkoop, Assendelft, de Boer, & van Tulder 2013).

These examples illustrate that Huber regards as unproblematic the issue of distinguishing proper, sound science from junk or pseudo. As I have already pointed out, this is a serious error. Huber commits it consistently by taking the established, accepted, mainstream view as authoritative. For

instance, he approves "the hope that, with the help of determined judges, the legal consensus would in time converge with the scientific one" (Huber 1991:14).

Huber's (1991:194 ff.) section on "Science as consensus" argues at length for equating consensus with science. Michael Crichton's (2003) take on this cannot be cited too often:

> Whenever you hear the consensus of scientists agrees on something or other, reach for your wallet, because you're being had . . . If it's consensus, it isn't science. If it's science, it isn't consensus. Period . . . Consensus is invoked only in situations where the science is not solid enough. Nobody says the consensus of scientists agrees that $E = mc^2$. Nobody says the consensus is that the sun is 93 million miles away. It would never occur to anyone to speak that way.

Consensus in the scientific community also governs peer review, which is often cited as a guarantee of the soundness of science. As an editor of *Lancet* has pointed out, however (Horton 2003:306), "Peer review . . . is simply a way to collect opinions from experts in the field. Peer review tells us about the acceptability, not the credibility, of a new finding."

Accepting the mainstream consensus as indisputably true is an unwarranted shortcut to evade having to prove a specific case by employing general categories (Huber 1991:214–215): "Are there then any real differences between astronomy and astrology, chemistry and alchemy, immunology and clinical ecology, pharmacology and homeopathy, mathematics and numerology?"

Of course there are; they are built into our definitions of those names or activities. But disputes about specific claims are not about such distinctions. To discredit a particular claim, it should not be enough to label it "clinical ecology" or "alchemy;" the hard work ought to be undertaken to show how and why this specific claim belongs in the discredited category. That hard work is what Pigliucci, Huber, and other "skeptics" and pundits seek to evade by applying labels that have not been individually argued and justified. Thus the pejorative "alchemy" has been directed at individuals who offer evidence of the transformation of one element into another under conditions of "cold fusion"[6] that employ electrical or sonic energy; whereas no objections are raised to evidence of the transformation of one element into another in nuclear reactors, bombs, and particle accelerators, the latter of which also depends on the application of electrical energy. What then is the criterion for labeling something alchemy, if it isn't the transformation of one element into another? Clearly enough, Huber is just accepting the contemporaneous consensual judgment of the established mainstream, not

bothering about how any given claim could be classed as "alchemy" in contrast to "chemistry" or "(nuclear) science."

Perhaps Huber should apply to scientific disputes the same logic that applies to the use of prejudicial evidence in court. It is not usually permitted to cite a defendant's prior record of charges or convictions, so that only matters directly pertinent to the present, new charge should be taken into account. In other words, people should not be found guilty just because they are "the sort of person who commits this sort of crime;" their guilt should be judged specifically in relation to the present particular crime. By analogy, one ought not to label something as junk science just because it seems like "that sort of thing" in the eyes of some people, even if that is the consensus of an established community; every unorthodox claim or minority view in science ought to be judged purely by the specific evidence pertinent to the specific claim.

This point becomes even more important because there are such widespread misconceptions about what science actually is. Huber's book illustrates some of these, e.g., that there are unproblematically objective facts whose significance is independent of any theoretical framework or mode of interpretation (Huber 1991:218–219). A judge is cited approvingly for denying that mainstream scientists are prejudiced against important new ideas: It would be "inconceivable that such a looseknit group of independent thinkers in all the varied fields of science could, or would . . . effectively censor new scientific thought" (Huber 1991:219). "Modern" scientists are not dogmatic about their beliefs, according to Huber (1991:221). That judge, Huber, and all too many pundits, "skeptics," and others would benefit from familiarity with Bernard Barber's (1961) classic article, "Resistance by scientists to scientific discovery": The mainstream consensus is always dogmatically sure, a priori, that contrarian claims are wrong (see also Hook 2002).

Like Pigliucci, Huber (1991:223) insists that "lines can and must be drawn . . . . [s]haggy edges notwithstanding"—one can legitimately speak of "junk" science just as the term "junk bonds" is used in the absence of characteristics that clearly, objectively distinguish them from non-junk bonds. One can rely on the independence and wisdom of judges and scientists "to get the facts right" (Huber 1991:223–224). But whether lines can or must be drawn is a different question in scholarly discourse than in social action and the making of public policy. In academe and intellectual discourse in general, drawing the wrong lines causes no direct harm, whereas drawing the wrong lines could be catastrophic in matters of economic or environmental policies—or on going to war.

"The modern judge who defers to mainstream science will at the same

time defer to science's own, methodical acceptance of the possibility of error. To insist that things are more uncertain still is to deny things we know to be true" (Huber 1991:226). Again, typically, it is acknowledged that science is fallible, and because scientists know that therefore what scientists say can be trusted. One might legitimately call this turning of logic on its head casuistry or sophistry.

False analogies support this invidious rhetoric. "If we can't say what is fact and what is fantasy, how can we challenge the next demagogue who declares that the Jews are plotting against the Reich?" (Huber 1991:227). We can and should challenge such statements by looking at the specific evidence, not by calling them fantasy because we supposedly know fantasy when we see it.

This lawyer "doth protest too much, methinks,"[7] because he cannot stop insisting that lack of certainty is no barrier to being certain. "Let us concede one last time that the difference between dream and reality is itself uncertain [is it really?], that absolute certainty is always unattainable"; nevertheless in practice one should act as though one were absolutely certain:

> The best test of certainty we have is good science—the science of publication, replication, and verification, the science of consensus and peer review; the science of Newton, Galileo, and Gauss, Einstein, Feynman, Pasteur, and Sabin . . . the best test of certainty so far devised by the mind of man. (p. 228)

But the science of those eminent people was not accepted by contemporaneous peer review when they first proposed it (Barber 1961); moreover, replication hardly ever features in science because there are no rewards for it, and the philosophy of science has long acknowledged that verification is a logical impossibility.

The Media

The mass media are, by and large, acolytes of the shamans of scientism: They parrot whatever the consensus of an established scientific community happens to be. A fine illustration is the decision by *The Los Angeles Times* not to print any more letters questioning the mainstream dogma that global warming or climate change is being caused chiefly by human activity that generates carbon dioxide (Thornton 2013):

> [W]hen deciding which letters should run among hundreds on such weighty matters as climate change, I must rely on the experts—in other words, those scientists with advanced degrees who undertake tedious research and rigorous peer review. And those scientists have provided ample evidence that human activity is indeed linked to climate change. Just last

> month, the Intergovernmental Panel on Climate Change—a body made up of the world's top climate scientists—said it was 95% certain that we fossil-fuel-burning humans are driving global warming . . . Saying 'there's no sign humans have caused climate change' is not stating an opinion, it's asserting a factual inaccuracy.

I suspect Thornton does not take the same stance of deferring to the experts on matters of, say, economics. There he probably feels able to accept the expert advice of (say) liberal or progressive economists while dismissing the biased opinions of conservative economists (or vice versa). Since there are thousands of competent climate scientists who disagree with the mainstream consensus on global warming, on what basis does Thornton dismiss their opinions? Because they too can be labeled "conservatives" (Bauer 2012b), like for example Frederick Seitz, a former President of the National Academy of Sciences and a former President of Rockefeller University?

Sadly, all too many people stop trying to think when something "scientific" is asserted by "experts," especially when they use phrases like "95% certain." Yet it should take very little thought to ask just how such a probability could possibly be calculated. Merely asking the question brings an easy answer: There is no way to estimate the probability that there exists no presently unknown variable, and therefore there is no way to calculate a probability of being right as opposed to wrong, and no way to be sure that one is not wrong, even, perhaps, "100% wrong".

In a Nutshell

Determined advocates of certain policies and actions support their positions by invoking what "Science" says. As a surrogate for actual scientific evidence, the contemporaneous view of the visible majority of established mainstream experts is invoked. However, since it cannot be gainsaid that science is fallible and that contemporaneous consensuses were often later superseded or vitiated, the advocates have to resort to misdirection, employing technical jargon like "fuzzy logic" to induce the laity to accept their interpretations as definitive, to accept somehow that "uncertain" could mean "certain," that a probability could be converted into a certainty. Such misdirection seems particularly reprehensible when engaged in by philosophers or by lawyers, whose professional responsibility it is to know better, to know what an intellectually sound argument is and what is not one.

What is appropriate in academic discourse may be inappropriate in the sphere of public policy. Mistakes made in the trial-and-error processes of

scholarly and scientific research cause no immediate widespread damage, but injudicious public policies and actions may bring highly damaging social consequences in short order. The desire for a contemporary scientific consensus to be accepted as the universally valid guide to public actions does not originate in the intellectual environment of scientific activity, but rather from the wish of a few activists within and outside the scientific community to find backing for their desired social actions. In placing social activism before intellectual rigor, such activism does a disservice to both science and other intellectual disciplines as well as to public discourse and policymaking.

Notes

[1] However, the prominent "skeptic" Michael Shermer (2002) actually praises scientism and its proponents.

[2] "I would strongly recommend this book [Bauer 1992] to anyone who hasn't yet heard that the scientific method is a myth. Apparently there are still lots of those folks around" (Goodstein 1992).

[3] Indeed, in German, *Wissenschaft* means knowledge and *Naturwissenschaft* stands for science.

[4] http://www.platofootnote.org

[5] http://www.astrology-research.net/rgcsa.htm
http://www.astrology-and-science.com/hpage.htm

[6] Proponents now often call this putative phenomenon LENR, for "low energy nuclear reactions," or CMNS, for "condensed matter nuclear science."

[7] After "The lady doth protest too much, methinks": Shakespeare, *Hamlet*, Act 3, Scene 2, p. 230.

References

Barber, B. (1961). Resistance by scientists to scientific discovery. *Science*, *134*, 596–602.

Bauer, H. H. (1984). *Beyond Velikovsky: The History of a Public Controversy*. Urbana & Chicago: University of Illinois Press.

Bauer, H. H. (1992). *Scientific Literacy and Myth of the Scientific Method*. Urbana & Chicago: University of Illinois Press.

Bauer, H. H. (2001). *Science or Pseudoscience: Magnetic Healing, Psychic Phenomena, and Other Heterodoxies*. Urbana & Chicago: University of Illinois Press.

Bauer, H. H. (2012a). *Dogmatism in Science and Medicine: How Dominant Theories Monopolize Research and Stifle the Search for Truth*. Jefferson, NC: McFarland.

Bauer, H. H. (2012b). A politically liberal global-warming skeptic? 25 November. http://scimedskeptic.wordpress.com/2012/11/25/a-politically-liberal-global-warming-skeptic/

Bauer, H. H. (2014). Anomalistics, pseudo-science, junk science, denialism: Corollaries of the role of science in society. *Journal of Scientific Exploration*, *28*(1), 95–111.

Biello, D. (2009). Is combating climate change worth the cost? *Scientific American blog.* 14 Jan. http://www.scientificamerican.com/blog/post/is-combating-climate-change-worth-t-2009-01-14/?id=is-combating-climate-change-worth-t-2009-01-14

Brumfield, B. (2013). Air pollution causes cancer, world health authority says. *CNN.com*. 17 Oct. http://www.cnn.com/2013/10/17/health/geneva-air-pollution-carcinogenic-who

Crichton, M. (2003). Aliens cause global warming. *Caltech Michelin Lecture*. 17 January.

Ertel, S., & Irving, K. (1996). *The Tenacious Mars Effect*. London: Urania Trust.

Gardner, M. (1957). *Fads & Fallacies In the Name of Science*, New York: Dover.

Gauquelin, M. (1991). *Neo-Astrology: A Copernican Revolution*. London: Arkana/Penguin.

Goodstein, D. L. (1992). Rangings of the mind. *Science*, *256*, 1034–1036.

Hook, E. B. (Editor) (2002). *Prematurity in Scientific Discovery: On Resistance and Neglect*. Berkeley: University of California Press.

Horton, R. (2003). *Health Wars: On the Global Front Lines of Modern Medicine.* New York: New York Review Books.

Huber, P. W. (1991). *Galileo's Revenge: Junk Science in the Courtroom*. New York: Basic Books.

Knight, D. (1986). *The Age of Science.* Oxford & NY: Basil Blackwell.

Kuhn, T. S. (1970). *The Structure of Scientific Revolutions.* Chicago: University of Chicago Press.

Laudan, L. (1983). The demise of the demarcation problem. In *Physics, Philosophy and Psychoanalysis*, edited by R. S. Cohen & L. Laudan, Dordrecht: D. Reidel, pp. 111–127.

Loxton, D., & Prothero, D. R. (2013*). Abominable Science*. New York: Columbia University Press.

Pigliucci, M. (2010). *Nonsense on Stilts: How to Tell Science from Bunk*. Chicago: University of Chicago Press. (See two less-than-enthusiastic reviews in *Journal of Scientific Exploration*, *25* (2011)—by Constantin Cranganu (pp. 385–390) and by Hannah Jenkins (pp. 390–394).

Pigliucci, M. (2013). Pseudo-science. In *Encyclopedia of Philosophy and the Social Sciences*, edited by Byron Kaldis, Thousand Oaks, CA: Sage Publications, pp. 766–769.

Rubinstein, S. M., van Middelkoop, M., Assendelft, W. W. J., de Boer, M. R., & van Tulder, M. W. (2013). Spinal manipulative therapy for chronic low-back pain. *Cochrane Summaries*, 31 Jan. http://summaries.cochrane.org/CD008112/spinal-manipulative-therapy-for-chronic-low-back-pain

Shermer, M. (2002). The shamans of scientism. *Scientific American*, June, p. 35.

Thornton, P. (2013). On letters from climate-change deniers. *The Los Angeles Times*, 8 October. http://www.latimes.com/opinion/opinion-la/la-ol-climate-change-letters-20131008-story.html