The Devil is in the (Historical) Details: Continental Drift as a Case of Normatively Appropriate Consensus?

Naomi Oreskes
University of California, San Diego

In Social Empiricism, *Miriam Solomon proposes a via media between traditional philosophical realism and social construction of scientific knowledge, but ignores a large body of historical literature that has attempted to plough just that path. She also proposes a standard for normatively appropriate consensus that, arguably, no theory in the history of science has ever achieved, including her own ideal type—plate tectonics. And while valorizing dissent, she fails to consider how dissent has been used in recent decades as a political tool to challenge scientific evidence on diverse issues, including the link between tobacco and cancer and the reality of anthropogenic global warming.*

Introduction

Miriam Solomon presents *Social Empiricism* as a Hegelian synthesis emerging from the dialectic between the traditional philosophical thesis of individual-based rationality and the SSK antithesis of social construction. I concur wholeheartedly that a new synthesis is in order, but I find three difficulties with her claims. First, she exaggerates her novelty, neglecting two decades of historical scholarship that has tracked the middle way she advocates. Second, she proposes a standard that no theory in the history of science has ever met, including her own ideal case. Third, she ignores the difficulty that dissent can serve undemocratic political ends and can counter the goal of scientific understanding of the natural world.

I. Previous attempts to find a middle ground

Thomas Kuhn famously argued nearly half a century ago that scientific consensus is forged in communities of like-minded practitioners with shared values, and agreement as to what constitutes both appropriate questions and appropriate types of answers. Scientists who read Kuhn in

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the 1960s and '70s were often pleased with his account, which resonated with their own experience of community preferences and group loyalties, but they mostly ignored the aspect that most excited (or exercised) historians, sociologists, and philosophers: incommensurability. How could science be rational if it was characterized by leaps of faith? How could a rational result emerge from an irrational or at best a rational process?

Since then, many sociologists and historians have argued that it is in the very sociability of science—the give and take of arguments, the push and pull of replication and its discontents—that scientific knowledge emerges. Bruno Latour emphasized in his early work, *Laboratory Life,* that science was an agonistic process resulting in a socially accepted product.

While Solomon cites Latour's later work, she strangely ignores this seminal text, which inspired historians of science to consider more deeply the role of social processes in producing rational scientific outcomes. Chief among them was Martin Rudwick, who in his path-breaking 1985 work, *The Great Devonian Controversy,* recounted in fine detail how geologists established a fact of nature—the existence of the Devonian period of geological time—through a protracted process of *negotiation* in the socially articulated arena of the Geological Society of London (Rudwick 1985).

Rudwick's most recent book, *Bursting the Limits of Time*—an analysis of the emergence of modern geology in the late 18th century and early 19th centuries—emphasizes that despite the diverse ways in which savants generated scientific ideas and information—alone or in pairs, in museums or in the field—scientific *knowledge* was always finally "consolidated in social interaction" (Rudwick 2005, 640, emphasis added).

Recent historical studies have also explicitly rejected the view of science that Solomon alleges to be widespread: generality, individualism, and the pure/applied science distinction (Solomon 2001, Chapter 1, pp. 1–13). Solomon claims, for example, that it is "generally assumed that all scientifically rational work has in common the same scientific method," but this claim has been refuted at length in the work of historians such as Peter Galison, Jane Maienschein, and Andrew Warwick, as well as philosophers such as Alexander Rosenberg, John Dupré, and Alison Wylie (Maienschein 1985; Galison 1987; Rainger et al. 1991; Dupre 1993; Galison and Stump 1995; Galison 1997; Wylie 2002; Warwick 2003). She similarly claims that scholars have remained fixated on the idea of scientific rationality as an individual process, ignoring the many important recent works that track the ways and means in which social engagements among communities of investigators conclude in the stabilization of empirical facts about the natural world. Besides Rudwick's work, she might consider Peter Galison's *Image and Logic,* which explicitly considers the interplay between communities of practitioners, Andrew Warwick's

Masters of Theory, which stresses the creation of a culture of physical and mental competition that trained "wranglers" at Cambridge to solve vexing problems in mathematical physics, and Mary Terrall's, *The Man Who Flattened the Earth,* a refined account of a man whose sociability was crucial to his capacity for scientific demonstration and persuasion.

The point is not that Solomon necessarily needs to engage with any one of these books particularly, but rather that the role of the social in producing the rational has been a central focus of history of science over the past two decades. These are not minor books by obscure authors, but prize-winning books by leading academicians.

Similarly, Solomon argues that there has been "no recent change" in the accepted demarcation of pure and applied science, ignoring the celebrated debate between Dan Kevles and Paul Forman in the mid 1980s over the impact of military funding on the direction of "basic" research in physics. Forman in particular argued that the very definition of "basic" science was altered by military priorities and its culture of "gadgeteering" (Forman 1987; Kevles 1990). Since then, numerous authors, myself included, have examined the intricate interconnections between the scientific, technological, and political goals of the Cold War and how they blurred the "pure" and "applied" distinction.[1] None of this is to say that these works necessarily refute her philosophical position, but rather simply to say that it would be reasonable for her to acknowledge their existence and explain how her work differs from theirs, if it does.

II. Continental drift and plate tectonics: A normatively appropriate consensus?

Solomon's neglect of existing historical literature comes to bear most importantly in the central claim of her work: that consensus in science is normatively appropriate if and only if one theory has all the available empirical successes. Her type example is continental drift and plate tectonics: she claims that the consensus over plate tectonics in the 1960s was normatively appropriate because it met the proposed standard. Here, I raise two issues: one, that Solomon mis-represents the continental drift debate, and two, that plate tectonics does not meet the standard she has set.

Solomon's starting point is the work of philosopher Henry Frankel, who in the 1970s and '80s used continental drift/plate tectonics to evaluate reigning philosophical theories, and saw no significant social dimen-

1. For continuing debate on this issue, see Dennis 1987, 1991, 1994, and 1997; DeVorkin 1992, 1996; Doel 1997; Forman 1996; Galison and Hevly 1992; Hounshell 1997; Hacker 2000; Kaiser 2002; Krige 2003; Leslie 1992, 1993; Mukerji 1989; Oreskes and Doel 2002; Oreskes 2003; Rainger 2001; Solovey 2001.

sion to that scientific debate. Solomon quotes Frankel: "continental drift was not . . . a scientific controversy having significant political, economic, or social aspects" (Solomon 2001, 86). Unlike evolution or relativity, drift was not interpreted through the lens of religion, politics, or metaphysics, and there is little or no evidence that "external factors" such as concern for offending religious sensibilities, played a significant role in the debate. Solomon asserts that others who have analyzed the history, including me, "have come to similar conclusions" (Solomon 2001, 86).

While I agree with both Frankel and Solomon that continental drift was not interpreted to have broad political, metaphysical, or religious ramifications, it does not follow from this that the debate had no significant social dimensions, and to suggest that I think so is to misread my work.

The Rejection of Continental Drift shows how scientists' evaluation of the theory of drift and the evidence invoked to confirm or deny it was pervasively interwoven in a fabric of epistemic, cultural, and social considerations. One cannot understand any individual scientist's epistemic position vis à vis drift without understanding the cultural and social vectors that defined that position. This theme is explicit in my discussion of what I call "epistemological affinities."

In the core of the book, I argue that the problem of continental drift was not under-determination, as might appear on first examination, given the various competing hypotheses that flourished at the time, but rather over-determination: no one theory was consistent with all the evidence. There was, in effect, too much evidence, and different individuals and groups of scientists preferred to emphasize certain sets of data over others. I call these preferences "epistemological affinities," because they "expressed themselves epistemologically"—in terms of differential weightings of evidence—but I stress that "their sources were largely *social*" (Oreskes 1999, 53). And I explicitly declined to describe such factors as "external," because the point of the argument is to say that social factors are *not* external—imposed on or imported by scientists from outside their expert communities—but integral to their scientific practice.

I further argued that a major obstacle to American acceptance of continental drift was a commitment to the method of multiple working hypotheses—the idea that good scientific practice requires resistance to singular, over-arching theories—and that American scientists viewed this method as *an instantiation of American democracy*—a social and political commitment if ever there were one! It was characteristically American to be open and fair-minded, or so they believed, and this is why Americans would ultimately produce a better science than their autocratic European counterparts (they believed).

Through detailed analysis of published papers, unpublished letters, course notes and field notebooks, I showed that the method of multiple working hypotheses was really used, and that commitment to the ideals it represented led many Americans to reject continental drift. Nevertheless, I concluded, the process was rational insofar as it was explicit and consistent; it involved weighing of evidence and serious argument; and the outcome was acceptable to nearly all of the parties involved (even Alfred Wegener himself). Most important, the door was left open for reconsideration of the question when new evidence and arguments developed.

Fast forward to the 1960s, when the idea of moving continents was accepted with revisions under the new title of plate tectonics. Solomon enshrines plate tectonics as her ideal type for a normatively appropriate consensus, because "plate tectonics had all the empirical successes."

This is just plain wrong. In the 1960s, plate tectonics *did* have a very large number of empirical successes. It explained the homologies across continents that the advocates of continental drift had pointed to in the 1920s, as well as new data from the sea floor, continental magnetism, and seismology. These were enormously significant. But it is simply incorrect to say that plate tectonics had all the empirical successes. One specific difficulty was widely discussed by scientists at the time, and has been further discussed in retrospect: the problem of the Earth's internal structure and strength.

Throughout the 1930s and '40s, seismologists argued that continental drift was impossible because the Earth's interior was too rigid to permit large-scale motions. In the 1960s, this point was taken up by one of the most brilliant geophysicists of his generation, Gordon J. F. MacDonald. In 1957, MacDonald, working with colleague Walter Munk, analyzed the available empirical data regarding seismic propagation, and the available experimental data concerning the Earth's internal composition. These data suggested that the Earth would not yield by viscous flow as theories of crustal mobility required.

In the early 1960s, a dramatically increased quantity and quality of seismic data became available through the World Wide Standard Seismograph Network. In 1963 and 1964, MacDonald published two articles analyzing these data, in particular, the Earth's "free oscillations"—vibrations produced in response to earthquakes. He concluded that the planet was too rigid to support large-scale crustal movements. Moreover, early satellites had produced better measures of regional gravity, which indicated that the density differences between continents and oceans extended to very great depths—MacDonald concluded 300 miles—inconsistent with the flat plate model. MacDonald's objections were a major obstacle to many people's acceptance of plate tectonics; several scientists have recounted that they accepted the new theory not because his objections had been answered, but because the theory performed so well in other respects (Oreskes and Le Grand, 2001).

Numerous other anomalies were also noted at the time (MacDonald 2001). Most of these had to do with how the theory explained (or failed to explain) persistent continental features, such as the Rio Grande Valley, the mid-continent rift, or the Colorado mineral belt. If mountains, volcanoes, and rifts formed at plate boundaries, then how to explain the Rockies, a thousand miles from the western boundary of the North American plate, or the mid-continent rift, whose very named revealed the problem?

And then there was Hawaii: volcanic islands smack dab in the middle of the Pacific plate. The new theory had no explanation for this, and so Jason Morgan introduced the concept of a "hot spot"—a locus of intra-plate volcanic activity. Here was an ad hoc adjustment if ever there was one: there was not a shred of independent evidence to support it. Yet, the empirical failure that Hawaii represented did not impede consensus formation. Scientists just worked around it.

Solomon may respond that empirical anomalies do not matter if *no* theory can explain them, but in the cases described here, competing theories did explain at least some of them. The free oscillation data were consistent with reigning notions of crustal mobility, including the geosyncline theory previously dominant in the United States, and whole Earth expansion theory, promoted particularly by scientists at Columbia University, could explain intra-continental rifts. These other theories did not explain most of the other data, but that is precisely the point: *no one theory had all the empirical successes.*

How can good scientists in good conscience ignore empirical anomalies? I suggest the answer lies in what I call "productive indifference." Historical studies show that scientists frequently ignore small failures in favor of bigger successes. Scientists saw the promise of plate tectonics and concluded that it made sense to accept it and worry about the details later. Grasping what the theory could do, they considered it productive to be indifferent to what it could not. Since no one theory could explain everything, it made sense to go with the theory that explained the most.

Does any theory in the history of science meet Solomon's standard?

If plate tectonics fails to provide Solomon with an ideal type, perhaps she has simply chosen a bad example. I think not. From a distance, a highly successful theory may appear to have all the empirical successes, particularly insofar as scientists crowing in the wake of a scientific revolution will rarely highlight the problems that have been swept under the rug. But on closer examination, I suggest, all theories look like plate tectonics.

Consider Darwin's theory of the origin of species by natural selection. We all know that it engendered ontological outrage in certain religious and cultural circles, but it prevailed among scientists, at least in its gradualistic account of evolution if not in its mechanistic explanation. So, did Darwin have all the empirical successes? No. Historians have described at length how Darwin unsuccessfully grappled with the awkward fact that the fossil record did not reveal gradual change at all. As known in 1859, it recorded long intervals of stasis, punctuated by apparently instantaneous change. Life suddenly appeared at the base of the Cambrian; dinosaurs and ammonites suddenly disappeared at the end of the Cretaceous. Darwin's response that the fossil record was "incomplete" was to many contemporaries altogether too convenient. Darwin did not *explain* the fossil record, he explained it *away.*

Similar conclusions can be drawn about the Copernican universe, the atomic theory of matter, and quantum mechanics. No theory in the history of science has ever explained everything that was on the table; difficulties have always been explained away, pushed aside, or left to be solved another day. Scientists have always been productively indifferent.

According to Solomon's standard, therefore, no consensus in the history of science has ever been appropriate. She may be willing to allow that the set of theories that meets her standard is an empty one, but then her claim to naturalism collapses.

III. The political dimensions of dissent

An important component of Solomon's position is her argument that funding agencies should support dissenting voices. Following feminist and standpoint epistemologists, she holds that "members of marginalized groups often have special abilities, deriving from their social locations, to identify particular kinds of decision vectors" (Solomon 2001, 147). Like many in science studies, she makes the default assumption that minority voices are under-privileged, with highly constrained access to social and material resources. Therefore, giving them pride of place (or at least more pride than they have had in the past) will lead to a fairer, more just outcome. "Social empiricism," she concludes, "is primarily interested in bringing about democratic science" (Solomon 2001, 149).

I applaud any effort that makes the world a fairer place, but the presumption that dissenting voices in science are underprivileged is demonstrably false. In recent years in the United States, minority scientific opinions have been trumpeted by powerful interests group to advance political and economic agenda. These groups include major industrial interests, such as the tobacco, fossil fuel, and pharmaceutical industries, who have supported research to create dissenting opinions where they did not previously exist, or to amplify and expand what were previously peripheral or minority views (Micheals 2005; Michaels and Monforton, 2005; Michaels, 2008; Yach and Bialons, 2001). The most obvious example is tobacco.

In the mid 1970s, the U.S. tobacco industry launched a biomedical research program intended to highlight the uncertainties surrounding the causal mechanisms of cancer and other degenerative diseases, and thus confound the link between tobacco use and ill health. Between 1975 and 1989, R.J. Reynolds Corporation spent $45 million dollars on this program.[2] To oversee the distribution of grants, Reynolds hired a retired distinguished physicist, Frederick Seitz, a former President of the U.S. National Academy of Sciences and former President of the Rockefeller University. In 1979, Seitz explained how he become connected with R.J. Reynolds:

> About a year ago, when my period as President of the Rockefeller University was nearing its end, [I was] asked if I would be willing to serve as advisor to the Board of Directors of R.J. Reynolds Industries, Inc., as it developed its program on the support of biomedical research related to degenerative diseases in man—a program which would enlarge upon the work supported through the consortium of tobacco industries. . . . Since . . . R.J. Reynolds had provided very generous support for the biomedical work at The Rockefeller University, I was more than glad to accept (Seitz 1979).

Directly challenging the existing consensus of scientific opinion, Reynolds Executive Colin Stokes explained that the charges that tobacco was linked to cancer, hardening of arteries, and carbon monoxide poisoning were "tenuous," and that the Reynolds Corporation was reacting to the "these scientifically unproven claims by intensifying our funding of objective research into these matters" (Stokes 1979).

While most epidemiologists and oncologists in the 1979 would have said that the link between cancer and smoking was demonstrable, Stokes insisted that ". . . science really knows little about the causes or development mechanisms of chronic degenerative diseases imputed to cigarettes, including lung cancer, emphysema, and cardiovascular disorders." Many of the studies that linked smoking to these diseases were either "incomplete or . . . relied on dubious methods or hypotheses and faulty interpre-

2. For comparison, the entire NSF budget for systematic biology in 1977 was $6.9 million.

tations." The program strategy would be "to identify . . . highly promising young investigators who are underfunded at present . . ." (Seitz 1978).

No doubt many aspects of chronic degenerative diseases were (and remain) poorly understood, but the context of the Reynolds program was not to build a better, more democratic science. The context was the value of conflicting or complicating scientific evidence in creating "reasonable doubt"—which could be used in court. Stokes explained:

> Due to favorable scientific testimony, no plaintiff has ever collected a penny from any tobacco company in lawsuits claiming that smoking causes lung cancer or cardiovascular illness—even though one hundred and seventeen such cases have been brought since 1954 [sic] (Stokes 1979).

In recent years, similar tactics have been applied to political campaigns to challenge the scientific evidence that sulfate emissions cause acid rain, that chlorinated fluorocarbons destroy stratospheric ozone, and that greenhouse gases are causing global warming. In each of these campaigns, "minority" scientific views have been trumpeted in the name of fairness, equal time, and other attractive virtues, but the goal has not been to advance understanding of the natural world. The goal has been to protect the financial and ideological interests of the patrons (Rampton and Stauber, 2002; Oreskes and Conway, 2008). In short, dissent can be a potent political tool, and nurturing minority opinions does not necessarily advance science, democracy, or rationality. One needs to consider the political context in which dissent is formulated. For while there are certainly contexts in which promotion of minority voices advances democracy, there can also be contexts in which it does the reverse.

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