BOOK REVIEWS

Paradigms and Barriers: How Habits of Mind Govern Scientific Paradigms. Howard Margolis. 1993. The University of Chicago Press, Chicago, IL. 267 p. $40.00 cloth, $15.95 paper.

In a previous book, *Patterns, Thinking, and Cognition* (University of Chicago Press, 1987), Howard Margolis outlined a general account of belief, persuasion, and judgment; in the present book, he applies the theory to a set of case studies in the history of science. It is the capstone of a long effort, with the goal of establishing a plausible psychological account of Thomas Kuhn's notion of paradigms and paradigm shifts, a psychological account rooted in the workings of human cognition. Since Kuhn has been seen as warranting only a social account of scientific change, Margolis's book provides a welcome slant on an old issue, even if (as he acknowledges) the slant is not entirely new—Kuhn himself, in *The Structure of Scientific Revolutions* (University of Chicago Press, 1962/1970) seemed disposed to account for paradigms and paradigm shifts using psychological as well as sociological principles.

Margolis's cognitive account begins with a reprise of his earlier book (the first part of which is reprinted in an appendix). He uses pattern recognition, the cued response to some state of affairs within a context, as the foundation of all cognition, the "atoms" or "genes" of his system. These are construed broadly, so as to encompass cued actions, mental or physical, as well as perceptual recognitions. He then argues that sequences of such recognitions constitute the "habits of mind" that underlie all thought. The parallel with physical habits (walking, speaking) is made explicit, and the habits of mind that matter for the present book are just those that add up to a Kuhnian paradigm. To "see" the sun as stationary and the earth as moving requires a collection of habits of mind that, once learned, are resistant to change.

Disavowing recent computational models of cognition, which generally use one or another variety of symbolic rewrite rule as a foundational principle, Margolis allies himself with William James. In a sense, James "peripheralized" cognition; his famous account of the stream of thought as "like a river," rather than as composed of discrete ideas and concepts, is close to Margolis' view, as is James' view of "habit" as a learned mode of thinking and doing. Both James and Margolis displace thought from an autonomous "center" of mind, locating it instead closer to the everyday sensory and motor world. We think by doing, even in science, and that can mean that we are as resistant (sometimes!) to new habits of thought as we would be to new habits of, say, walking. Like James, Margolis argues that existing habits of mind can constitute actual barriers to new habits of mind. Explanation of the Kuhnian paradigm resides here; paradigms are maintained by habits of mind shared by a community. Paradigm shifts occur when some unique barrier (perhaps in only one or a few people) has such strength that it motivates search for a way around an anomaly. If found, these ways become new habits of mind, and a new paradigm is in the making.

The present book seeks to apply this view by showing that case studies of scientific controversy can yield to analysis in terms of habits of mind. Four such cases are covered: the overthrow of phlogiston by the oxygen theory (25 pp.), the emergence of the concept of probability (18 pp.), the overthrow of Ptolemaic astronomy by Copernicus (76 pp.), and a brief account of Hobbes' differences with Boyle (17 pp.). Margolis uses the phlogiston story to illustrate his method, arguing that the notion of barriers can better account for resistance to the oxygen theory than previous historical accounts. The probability story exemplifies the converse point, that new insights can be blocked by the *absence* of a unique habit, in this case, the notion of "counting" things that haven't yet occurred, e.g., the chance that a pair of dice will come up with a seven. Each of these two cases is presented with great depth but little elaboration; they will be clear to the general reader but hard to read critically unless you are already quite knowledgeable about the specific histories in question. As for the Hobbes and Boyle chapter, this will be the least satisfactory for the general reader, because here Margolis is really aiming at another target; the social constructivist account of science. I'll say more about that later, because it is first necessary to take account of the heart of the book, which deals with the overthrow of Ptolemaic astronomy during the Copernican Revolution.

For the Ptolemaic theory, Margolis first provides a tutorial chapter that serious readers will appreciate; it is nothing less than a short course in the qualitative basics of pre-Copernican astronomy. Shorter and less comprehensive than Kuhn's magisterial (and uncited!) *Copernican Revolution* (Harvard University Press, 1957), Margolis' precis is, nonetheless, remarkable—concise, clear, and capable of evoking the reader's wonder at the richness of the old astronomy. This chapter will cost time, but it will be time well-spent! Ptolemy's earth-centered cosmology was accompanied by an elegant analysis of planetary motion into deferents (orbits around the sun, which is itself orbiting the earth) and epicycles (orbits of the planets themselves around points on the deferents). Epicycles thus ride around on deferents, like small clocks carried on the hands of a larger clock. The scheme accounts for the otherwise baffling retrograde motion of the planets and is amazingly close to the observational data. Margolis lets the non-specialist reader gain some rough entry into the elegance of the solution.

The Ptolemaic system is, on his account, dependent upon habits of mind; the epicycles were "seen" as real by its adherents. Since it worked so well, how did Copernicus' account overturn it? As Kuhn had shown, the usual argument, that the heliocentric solution is simpler, does not account for the overthrow of the Ptolemaic system; until Kepler's innovation (*elliptical* orbits), either solution, sun-centered or earth-centered, still required epicyclic motion to account for the observations. Why switch, particularly when the great Polish astronomer's alternative demands a counter-intuitive earth motion to make it work? Margolis's claim is that this can be understood if we examine the habits of mind that became part of astronomy's way of "seeing" the Ptolemaic system *and* the barriers that these

imply. To make his claim work, Margolis describes some variants of the Ptolemaic account, one based on a different scaling of the sizes of the orbits, and one based on an inversion move in which the epicycles become larger than the deferents (like a large clock riding the hands of a small clock). By contrast, Copernicus carried out a *sliding* move, sliding the center of the system to the sun position, rather than the earth position. Here Margolis appeals to the reader's own ability to "see" such motions and to see that the small-on-large motion of the traditional Ptolemaic account, the large-on-small motion of the "inverted" Ptolemaic account, and the shifted sun position with epicycles and earth motion produce equivalent observed motions. For Ptolemaic "seers," of course, "seeing" the earth motion would have been exceptionally hard.

Tycho Brahe tried a "compromise" between Ptolemy and Copernicus, a partially inverted system, by putting the earth back in the center, with the sun and moon in orbits around the earth and the planets fixed on large epicycles whose center was the sun (Ironically, Margolis does not describe this system, though he mentions it frequently—the reader is supposed to already know it, but will need to reference at least Kuhn's earlier book to get the point). The Tychonic solution was a temporary partial step that even Tycho abandoned; Margolis raises the issue of why no one in the traditional camp worked out a *completely* inverted system. The reason no one tried this, he says, is the relative complexity of the moves for those whose habits of mind were already partly Copernican. In fact, the Copernican move was already in the works by the time of Tycho's innovation; those whose habits of mind had started to lead them in Copernicus' direction had no inclination to try a different Ptolemaic solution and hence no motive to develop a completely inverted Ptolemaic system.

It is hard not to be impressed by the cleverness with which Margolis approaches these issues and the arguments are compelling indeed. Even so, one gets the odd feeling here that positing "what-ifs" is a dangerous way to approach historical analysis—and Margolis relies very heavily on his invented Ptolemaic system to make his case. This is a key to the major weakness of the book: the problem with positing barriers to thought is that one must then provide evidence showing that something exists because something else *doesn't* exist. And to then invoke the barrier as an *explanation* for the absence of something is awkward and possibly circular—rather like arguing that people can't remember events from infancy because of repression and then using the mechanism of repression to explain why people don't remember events from infancy!

Even so, the case at hand does exemplify the positive side of Margolis' argument, the claim that different habits of mind are manifested in Ptolemaic and Copernican world views. Indeed they are, and the reader who takes the time to follow the argument will gain something like a direct experiential verification of the claim. If it is harder to assent to the account of barriers, we can readily accept and applaud the notion of paradigms as cognitive psychological in character.

Among recent scholars, however, there are those who would disagree. In particular, recent sociological accounts of science (and especially the now-notorious "Strong Programme" associated with Edinburgh University) have at times denied that anything psychological is needed for *any* level of description in science studies. Margolis preemptively attacks this position with his account of the differences between Hobbes and Boyle on the "spring of the air." Boyle, of course, is justly famous for his air pump experiments confirming the weight of the air and the inverse relationship between volume and pressure. At the time, however, philosopher Thomas Hobbes disagreed. For him, the experimental effects found by Boyle were the consequence of the movement of weightless particles of air–and the volume of air was fixed, like that of water.

Margolis does not cover the case in great detail because his target is not Boyle (or Hobbes) but a more recent work, one of the "classics" of recent social constructivism, Shapin and Schaffer's *Leviathan and the Air Pump* (Princeton University Press, 1985), a book which has received much critical acclaim for its adroit account of the emergence of experimentation as a social activity. For Shapin and Schaffer, truth has a social history, one that, in this instance, "yields a rich harvest of political and social maneuvering" (Margolis, p. 162). But for Margolis their argument is suspect, because they have mischaracterized Hobbes' true position. Margolis demonstrates effectively that the position attributed to Hobbes was in fact only lightly held by him, for only a brief time, and was not fundamental to his larger concerns with the nature of natural science. Alas, this hardly does the job, since, on Margolis' own admission, Shapin and Schaffer really were not concerned with Hobbes' own position, but rather with the position attributed to him by Boyle and his circle. So how can Margolis' argument that Hobbes has been misread undercut or limit the force of their argument? He is right on the historical actuality of Hobbes, but being right on the point does nothing for his larger claim, namely, that the social constructivist story is too loose and too congenial with modern interests. Margolis is on the side of rational construction as the principal force in science, not social construction. But his case is not helped by this attack on Shapin and Schaffer, nor is their case hurt by it.

In the end, then, Margolis is arguing for a rationally constructed Kuhnianism. The argument, in spite of my cavils, is on the whole marvelously well-done and based on a deep reading of original sources that will delight the reader on many levels. If it fails to fully refute its self-defined major opponents, well, we can forgive it that for its virtues. In the end, it is a serious and scholarly attempt to restore a psychological dimension to the reality of scientific thinking, a middle course between the excesses of the "Strong Programme" and the excesses of a rationalism that would attribute scientific change solely to the logical force of observation and reason. It is not the whole story (nor does it claim to be), but it is an important and valuable part.

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