BOOK REVIEWS

Paul Hoyningen-Huene, ***Systematicity: The Nature of Science***. Oxford: Oxford University Press (2013), xiii+287 pp., $65.00.

Paul Hoyningen-Huene, deservedly known for his careful analysis of the thinking of Thomas Kuhn (Paul Hoyningen-Huene, *Reconstructing Scientific Revolutions: Thomas S. Kuhn's Philosophy of Science* [Chicago: University of Chicago Press, 1993]), now takes on Kuhn's contemporary Paul Feyerabend. Notoriously, Feyerabend argued that there can be no way of deciding between science and nonscience, demarcation criteria such as Karl Popper's principle of falsifiability are doomed to failure, and ultimately at best science is what people call "science." Hoyningen-Huene will have none of this, and his carefully argued, rather dense book is devoted to showing that Feyerabend is wrong.

The notion that Hoyningen-Huene seizes on is that of *systematicity*. "Scientific knowledge differs from other kinds of knowledge, in particular from everyday knowledge, by being more systematic" (25). What exactly does he mean by this, for after all something like baseball is pretty systematic when it comes to following rules, yet—for all that, the late Stephen Jay Gould often turned to baseball to illustrate points about paleontology—we would not generally think of baseball or its rules as being particularly scientific. It turns out that we sail quickly into fairly familiar waters, for Hoyningen-Huene's notion of a systematic body of knowledge is basically what people like Kuhn and others (notably the late Ernan McMullin) highlighted as epistemic values, the sorts of things that they argued lead to reliable scientific understanding (Thomas Kuhn, *The Essential Tension: Selected Studies in Scientific Tradition and Change* [Chicago: University of Chicago Press, 1977]; Ernan McMullin, "Values in Science," in *PSA 1982: Proceedings of the 1982 Biennial Meeting of the Philosophy of Science Association*, vol. 1, ed. Peter D. Asquith and Thomas Nickles [East Lansing, MI: Philosophy of Science Association, 1983], 3–28).

Hoyningen-Huene takes us through no less than nine marks (or what he calls dimensions) where science shows its superior systematization: "descriptions, explanations, predictions, the defense of knowledge claims, critical discourse, epistemic connectedness, an ideal of completeness, knowledge generation, and the representation of knowledge" (27). So, for example, when it comes to prediction, ancient science showed its mettle when the Babylonian astronomers were able to predict the future occurrences of eclipses. Somewhat more complex is a notion like "epistemic connectedness," something we learn "means the existence of manifest connections of knowledge to other pieces

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of knowledge" (118). I do not think it totally unfair to say that Hoyningen-Huene runs into rather heavy weather as he tries to unpack exactly what one means by "manifest connections" in this context. It means things like equivalence, deductive consequence, consistent with, and more. He himself admits that such a "variety of possibilities may convey the impression that on an abstract level, the concept of epistemic connectedness covers almost anything and is therefore almost empty—and indeed this is the case" (119). Candid, but not entirely hopeful, if you know what I mean.

Let us look at another dimension, knowledge generation, but before we do so let me say that although there is very much I admire in this book—if you want a primer on what people like Popper and Kuhn were saying 40 or 50 years ago, then this is the book for you, since the material is covered with Germanic thoroughness and fairness—it does rather give off Qthe odor of the past. If not oil lamps and antimacassars, then certainly typewriters and travel agents. In a funny sort of way, it was these people like Kuhn and Popper who pointed us away from the kind of philosophy of science that Hoyningen-Huene practices, namely, philosophy of science that is basically theoretical and divorced from the real science of the day (or the past). You cannot do ravens and emeralds anymore but must look at the work in the laboratories and the journals and so forth.

So to, knowledge generation. At least part of what is going on here is getting the empirical material—"Just the facts, ma'am, just the facts." (This is my interpolation, not Hoyningen-Huene's, and for those who delight in such trivia I am aware that Joe Friday on *Dragnet* actually said, "All we want are the facts.") The trouble is that they never are just facts, as Norwood Russell Hanson (somewhat surprisingly not mentioned by Hoyningen-Huene) showed so clearly. Facts always come surrounded by theory (Norwood Russell Hanson, *Patterns of Discovery* [Cambridge: Cambridge University Press, 1958]). Charles Darwin famously made this point. "How profoundly ignorant B. must be of the very soul of observation! About thirty years ago there was much talk that geologists ought only to observe and not theorise; and I well remember some one saying that at this rate a man might as well go into a gravel-pit and count the pebbles and describe the colours. How odd it is that anyone should not see that all observation must be for or against some view if it is to be of any service!" (Letter to Henry Fawcett, September 18, 1861, in *Charles Darwin, Collected Correspondence*, 21 vols. [Cambridge: Cambridge University Press, 1985–], 9: 269).

It seems somewhat inappropriate, cruel almost, that Hoyningen-Huene actually cites Darwin as an example of one who collected without theory. "Charles Darwin's voyage with the Beagle from 1831 to 1836 was largely devoted to the description of plants and animals and also included the description of geological formations" (134). However, as anyone who knows

anything about the voyage will tell you, mere or pure description was just about the furthest thing from the mind of Charles Darwin. As he scampered across South America looking at the mountains and vales, chipping away at the rocks, searching out the fossils, the whole time he was comparing Lyellian uniformitarianism with Cuverian catastrophism, to the benefit of the former and the detriment of the latter (Sandra Herbert, *Charles Darwin, Geologist* [Ithaca, NY: Cornell University Press, 2005]). It all looks so simple in theory and is so complex in reality.

I am sounding a bit more negative than I intend, because I think Hoyningen-Huene is really onto something in giving a list of factors for separating out science from nonscience. It is not just one thing that makes for something being a science but a cluster of things. Where Feyerabend went wrong is in thinking that any cluster will do, so long as people agree to it or are pushed into accepting it. Where I think Hoyningen-Huene goes wrong is in refusing to accept Feyerabend's insight that the personal factor does count. You cannot do it all on formal, epistemological grounds. You have to bring in the psychology or the sociology of the situation as well. Let me try to show what I mean by picking on a topic that Hoyningen-Huene brings up at the end of his book, namely, "pseudoscience."

Hoyningen-Huene wants to distinguish pseudoscience formally. Something is a pseudoscience if and only if:

1. It has been less progressive than alternative theories over a long period of time and faces many unsolved problems; but
2. The community of practitioners makes little attempt to develop the theory toward solution of the problems, shows no concern for attempts to evaluate theory in relations to others, and is selective in considering confirmations and disconfirmations. (203–4)

It does look as though Hoyningen-Huene is bringing in sociology in talking about the community, but he is not really. He is interested in what rational beings would or should do, not living, breathing, striving, liking, hating beings. And the trouble is that, as people like Larry Laudan showed 30 years ago in the debate over evolution versus Creation Science, judged by these criteria many sciences fail the test and many pseudosciences pass with flying colors (Michael Ruse, ed., *But Is It Science? The Philosophical Question in the Creation/Evolution Controversy* [Buffalo, NY: Prometheus, 1988]). I have just completed an intensive study of the Gaia hypothesis, the claim that the earth is a living organism (Michael Ruse, *The Gaia Hypothesis: Science on a Pagan Planet* [Chicago: University of Chicago Press, 2013]). All over the scientific community are charges that it is a pseudoscience. (Google "Gaia" and "pseudoscience" if you doubt me.) Yet the inventor of Gaia, English chemist James Lovelock, went out of

his way to speak to the criticisms. For instance, to the charge that he was unduly holistic and had an illicit group conception of the working of natural selection, he invented the Daisyworld model, showing how homeostasis on a planet could be achieved by stringent individual selection processes. And still the charges of "pseudoscience" haunted him.

In a recent brilliant study of the thinking of Immanuel Velikovsky, who I heard for the one and only time in a symposium of the Philosophy of Science Association and who upset the staid and comfortable by arguing that most of conventional astronomy is wrong and that the Bible is a better guide—the Red Sea was parted by a comet and that sort of thing—Princeton historian of science Michael Gordin (*The Pseudoscience Wars: Immanuel Velikovsky and the Birth of the Modern Fringe* [Chicago: University of Chicago Press, 2012]) shows that the charges of "pseudoscience" were hurled at Velikovsky primarily because in the 1950s when he was active the scientists he challenged felt threatened for various reasons: money, status, credibility, and these sorts of things were at stake. It was not so much what Velikovsky said but what he represented, giving astronomers a bad image at a time when that was the last thing they needed.

I can bear this out—that the charge of "pseudoscience" is as much sociological as epistemological and a function of the insecurities of the "real" scientists—by a little tale of Florida State University just after I arrived on campus in 2000. The administration was eager to start a medical school, which it did a year or two after I arrived. A number of us were opposed to this on the grounds that it would drain resources from the rest of campus, which it has done in spades. Conscious of their unloved status, the new medics did much to control damage, claiming (truly) that they were going to focus on producing general practitioners who would work in rural areas. How could a good liberal like me object? Then disaster struck. An influential state legislator, a chiropractor, got $10 million appropriated to start a department of chiropractic. The administration loved the idea and even went so far as to put an ad in the *Chronicle of Higher Education* advertising for a new director. The campus medics nearly died and fought fiercely, and the proposal was dropped. Charge after charge claimed that chiropractic is pseudoscience. What was interesting and surely significant is that before and after this incident the medical profession in Tallahassee, the home of Florida State University, gets on well with the chiropractors and even recommends them on occasion. We know really what was happening. The new medical school was insecure—it could not afford to be "tainted" with the charge of being home to chiropractic—and so fought to reject it. "Pseudoscience" was an epithet flung in self-defense.

I doubt that this or like examples will change the mind of Paul Hoyningen-Huene. He wants an epistemologically based philosophy of science. I, who

spend as much time with the historians as with the philosophers, am happy to go all constructivist. So let me end by recognizing a worthy opponent across the divide and recommend his book, either as an example of what one should do or as an example of what one should not do.

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Tim Maudlin, ***Philosophy of Physics: Space and Time***. Princeton, NJ: Princeton University Press (2012), xiv+183 pp., $29.95 (cloth).

This volume by Tim Maudlin is part 1 of a two-part series dealing with philosophical issues raised by our foundational theories in physics. This volume deals with space and time, covering aspects of Newtonian physics, the special theory of relativity, and the general theory of relativity. The central theme is the way in which our views about space and time, either commonsense or philosophical, have undergone forced revisions in the light of experimental science and the theories constructed to deal systematically with those empirical results. Both scientific results and philosophical ruminations on them are treated rigorously and clearly. There is an issue, I think, about the level of students for whom the book is intended, since the chapters on Newtonian theory and on general relativity will be much more easily grasped by students without much background in the formal sciences than will the chapter on special relativity.

Chapter 1 outlines some crucial aspects of the Aristotelian worldview, with the earth at rest in the center of the universe as the standard of rest and motion, and then contrasts that with Newton's First Law and Newton's Absolute Space and Absolute Time. The levels of our formal characterization of space (topological, differential, affine, and metric) are noted. Then Newton's view is expounded with some of the wonderful parts of Newton's own "Scholium to the Definitions" in the *Principia* quoted at length.

Chapter 2 takes up the evidence for Newton's (astonishing to the relationists) view. The First and Second Laws are explored and the relevance of the spinning bucket and globes-on-a-rope thought experiments made clear. The history here seems to me a little thin. Galileo's (false) "circular inertia," grounded in his inclined plane experiments and useful to his attempt to rebut the dynamic arguments against the earth's rotation, is contrasted with Newton's "innovation" that true inertial motion is constant speed in a straight line. No mention is made of Benedetti or Descartes, who got much closer than Galileo to the final, correct straight line view of inertia in Newton. Well, to be sure, Newton in the "Scholium to the Laws" in the *Principia* credits Galileo with using the first two laws of motion in his study of projectile motion and never mentions Benedetti or Descartes.