metaphors, after all, are what science and culture are about—or so it seems to us today.

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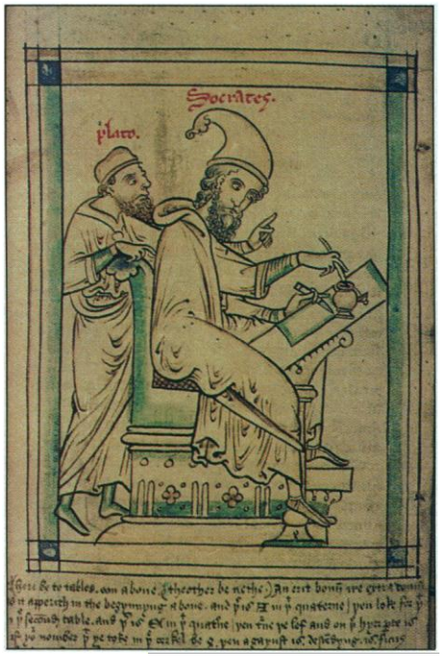
Underlaborers for Science

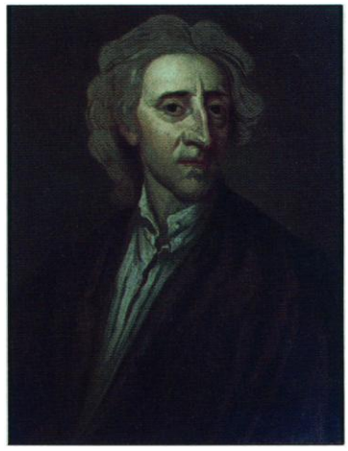
Taking the Naturalistic Turn. Or, How Real Philosophy of Science Is Done. WERNER CALLEBAUT, organizer and moderator. University of Chicago Press, Chicago, 1993. xxii, 553 pp., illus. $85 or £67.95; paper, $29.95 or £23.95. Science and Its Conceptual Foundations.

Werner Callebaut, Belgian evolutionary epistemologist and accomplished radio interviewer, has produced the best introduction one can imagine to contemporary philosophy of science. From 1985 to 1990, Callebaut interviewed 24 philosophically minded biologists and biologically oriented philosophers and sociologists of science. He then spliced together the interviews to make it seem as though the 24 were talking to each other about a series of common topics. The result is much lively and generally accessible conversation on some of the most pressing issues in both philosophy and biology today. An added treat is the biographical and historical material that Callebaut includes on the cast of characters, who include Donald Campbell, Patricia Churchland, Ronald Giere, David Hull, Philip Kitcher, Bruno Latour, Richard Lewontin, Helen Longino, and Michael Ruse.



Two early philosophers. [Bodleian Library MS Ashmole 304]

However, the book's informality and Callebaut's sympathetic interviewing style belie darker worries about the current state of the philosophy of science. These clouds emerge only occasionally in the interviews themselves but become very clear in Callebaut's Postscript. Readers may already suspect that trouble is afoot when Callebaut justifies his format by suggesting that the philosophical projects of his interlocutors make sense only if they are seen as talking to each other. Whatever else "the naturalistic turn" in philosophy may have accomplished, it seems to have diminished philosophical aspirations. Specifically, philosophy *of* science has become philosophy *for* science. Whether this subtle metamorphosis is to the benefit of philosophy or science remains to be seen.



John Locke [The Granger Collection]

Philosophers through the ages have not been known for their modesty. However, Callebaut allows a rather humble image of the philosopher to emerge from his interviews. It is that of "underlaborer," a term that John Locke coined in 1690 to describe the relationship between "master-builders" such as "the incomparable Mr. Newton" and others, such as Locke himself, who rest content with "removing some of the rubbish that lies in the way to knowledge" (quoted on p. 200, footnote 7). Locke assumed his custodial duties after Newton instructed him on how to write a persuasive review of *Principia Mathematica* without having to introduce much of its arcane mathematical structure.

How different is the situation today? According to Callebaut (p. 450), "Two-thirds of the more visible philosophers of biology today have spent time in the very same lab (Lewontin's at Harvard)." There are many ways of interpreting this observation. On the surface, it indicates that philosophers have finally become interested in the nitty-gritty aspects of scientific research. This is certainly a prominent way of "taking the naturalistic turn." However, philosophers whose politics are somewhat to the right of Lewontin's Marxism are more cynical. For example, Alexander Rosenberg periodically suggests that this singular reliance on Lewontin's charity may compromise the philosopher's critical independence.

Although I share Lewontin's political sympathies, I must confess that the conservative critics may have a point. Many of the philosophers speak of Lewontin—and, to be fair, E. O. Wilson as well—as genuine "natural philosophers." We are led to believe that they are no mere technicians but true visionaries, much as Newton was regarded by Locke. Unfortunately, the philosophers of science never describe each other in such edifying terms. Indeed, some make a virtue of their reliance on science to provide the entire basis for their inquiries. Philosophy of biology simply blurs into theoretical biology. Thus, when asked if he worried about whether philosophy had lost its point, Robert Brandon (p. 446) replied, "We philosophers of science are doing very good work now, and we don't know what the hell we are doing!" Clearly, Brandon is not setting his own agenda.

Understandably, the sociologists find the current state of the philosophy of science rather puzzling. To underscore their perplexity, Callebaut juxtaposes interlocutors so as to produce a suitably jarring effect. In a section on "the unfathomable goals of inquiry," he first has Philip Kitcher explain that science aims for ever greater unification of the phenomena of nature. Philosophers presumably aid in the task by showing how various phenomena logically fit together. Sociologist Bruno Latour is then brought in to question whether this is the sort of thing a philosopher ought to be doing (p. 218): "But philosophy is not about unifying factors; philosophy is about protecting against one factor's hegemony." When Latour thinks of a philosopher, he seems to have Socrates, not Locke, in mind.

To be sure, Western philosophy over the last 300 years has been strongly aligned with the natural sciences. Even philosophers who are normally presented as "science critics," such as Husserl and Heidegger, typically limit their complaints to the extension of natural scientific methods into the human domain. However, since Locke's day, there has also been active debate over the exact source of the natural sciences' allure, and even over

whether scientists themselves understand "the nature of science" very well. Indeed, ours is only the first generation of philosophers to embrace Locke's underlaborer role so openly.

Many of Callebaut's interlocutors seem to think that, in following Locke's lead, they are also continuing the project of the logical positivists and their Popperian cousins. The positivists are credited with good intentions in wanting to model philosophical practices on scientific ones. But they are faulted for their failure to master the details of particular sciences, with the partial exception of physics. Yet Callebaut's interlocutors are at a loss to explain how it was that the positivists, for all their technical deficiencies, managed to exert so much influence over scientific methodology and the public image of science.

One hypothesis is that the positivists were not trying to be underlaborers at all but were in fact using science to promote certain philosophical ends of greater societal import. From this angle, we may be able to see the point of Latour's perplexity. Consider the symbolic function of the natural sciences in the project of "Enlightenment" promoted in our own time by Karl Popper. The idea here is not one of philosophers paving the way for a mounting body of esoteric knowledge. Rather, it is one of extending to all spheres of life the critical attitude that had motivated scientists to challenge traditional beliefs in the first place.

As science becomes more expensive and more technical, it is increasingly difficult to sustain the spirit of criticism. Some followers of Popper, such as Paul Feyerabend, have gone so far as to suggest that scientific research programs need to be cut down to a size that enables criticism to flourish. Thus, when Feyerabend argues—against Kitcher and Ruse—that creationism should be taught alongside evolutionary theory in the public schools, he is not offering an opinion on the probative value of creationism *per se* but rather an opinion on the social contexts in which its probative value should be determined. This distinction is subtle but crucial for understanding the politics of science implied by the underlaborer model and why such politics might puzzle Latour and disturb Callebaut.

For his part, Feyerabend intervened in the creationist controversy as someone who wanted to square the imperatives of science and democracy. This is a classically philosophical interest that requires sustained thinking about science, but without being beholden to particular scientific research programs. In contrast, as he recalls to Callebaut, Kitcher became involved in the controversy as a partisan for the evolutionary cause. His first impulse was to forge a "new consensus" in the philosophy of science, one whose united front would keep the creationists out of the classroom. Philosophers have always been known to slip into ideology as their political ends overtake their intellectual means. However, today's philosopher-underlaborers come dangerously close to being apologists for the masters whose houses they so dutifully clean.

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Pipers and Tunes in Science

Prometheus Bound. Science in a Dynamic Steady State. JOHN ZIMAN. Cambridge University Press, New York, 1994. x, 289 pp. $24.95 or £16.95.

Across the industrialized world, strategic planning for basic science is the rage. From Tokyo to Brussels and Washington, the common purpose is to use scarcer funds more productively in order to serve national economic goals. But there's a catch: no one knows confidently how to plan science more reliably now than in the past. In the United States we can see this in the consternation elicited by recent calls for the National Science Foundation and the National Institutes of Health to chart "strategic goals." And in the European community, governments are stewing about the rationale for and expectations from university-based research funding.

Prometheus Bound is a masterly contribution to our thinking on these issues. In an elegantly woven appraisal, John Ziman speaks about and for the research community, proud of its past while finding its aspirations and resources under siege. Ziman substantiates the "radical, pervasive, and permanent structural change" in the institutions of the scientific enterprise. And he asks, "Can the research system be reshaped without losing many features that have made science so productive?" His prognosis is guarded. If Ziman were to pen in advance an epitaph for our spectacularly successful 20th-century science, my hunch is that he would write, "Died of extreme accountability."

Beginning ruefully by documenting the evident limits on growth in funding for all of research and development—an asymptote of 2 to 3 percent of national income in the most advanced nations—the book moves relentlessly to explore the consequences of this new "steady state" for basic science. What are the effects, asks Ziman, of "knowledge creation, the acme of individual enterprise, being collectivized" in the interest of national prosperity? The prose is clear and the tone consistently realistic ("nostalgia is a fruitless sentiment"). Ziman does not indulge in any superficial advocacy of addressing science's problems by applying merely a bit of money, here and there, just this year, nor does he toy with the possibility of science's returning to a period of double-digit growth.

In the heyday of the 1960s, a few perceptive leaders in science mapped the trends and estimated the consequences now emerging. After 30 years of policy analysis, we know a great deal about the linkages of education and fundamental research with the enormous range of activities essential to higher economic productivity. Yet past experiments with "targeting" science have yielded mixed results, and some long-range research driven only by curiosity has yielded unexpected economic benefits. Thus hopes for better research planning are undercut by the fear (as of a hex) that any fix will be worse than the present pattern. This concern is familiar to everyone immersed in public policy: "you want results and you get consequences."

Put another way, if science were managed more tightly, would the system leave enough freedom for investigators to follow their intuitions in ways that are not immediately "relevant" to explicit goals but might produce results widely applicable in meeting public needs and enhancing markets? To illustrate the point, Ziman reminds us of Faraday's legendary reply to a parliamentarian skeptical about the uses of electricity—"Someday you will tax it." Accordingly, science policy-makers are stuck with the problem of how to balance features of a bottom-up, pluralistic system in which investigators compete on criteria of quality measured against goals charted broadly with features of a top-down system in which allocations are made to scientists among subfields for quite specific purposes. Each nation employs some features of both models.

Ziman, a distinguished British physicist, demonstrates how well he knows the inner