



CHICAGO JOURNALS



History
of
Science
Society

Isabelle Stengers; Robert Bononno. *Cosmopolitics I*.

Author(s): Jeff Kochan

Source: *Isis*, Vol. 102, No. 3 (September 2011), pp. 594-595

Published by: [The University of Chicago Press](#) on behalf of [The History of Science Society](#)

Stable URL: <http://www.jstor.org/stable/10.1086/663071>

Accessed: 13/10/2011 05:57

Your use of the JSTOR archive indicates your acceptance of the Terms & Conditions of Use, available at <http://www.jstor.org/page/info/about/policies/terms.jsp>

JSTOR is a not-for-profit service that helps scholars, researchers, and students discover, use, and build upon a wide range of content in a trusted digital archive. We use information technology and tools to increase productivity and facilitate new forms of scholarship. For more information about JSTOR, please contact support@jstor.org.



The University of Chicago Press and *The History of Science Society* are collaborating with JSTOR to digitize, preserve and extend access to *Isis*.

<http://www.jstor.org>

son's philosophy, clearing up misunderstandings, highlighting differences between Hanson's and Kuhn's and Feyerabend's views of theory-ladenness, and defending Hanson against the charge of subjectivity. In fact, one of the aims of Lund's book is to show that Hanson's account of observation is fully compatible with scientific objectivity.

Another focal point of Lund's reconstruction is Hanson's logic of discovery program. Critics have argued that Hanson was unable to provide a *logic* of discovery; all he was able to do was to offer a distinction between good and bad reasons for scientific hypotheses. Lund agrees with this criticism, adding that that an analysis of discovery must capture the actual *processes* of reasoning. He then sets out to salvage Hanson's position. He argues that if a principle can be provided that explains why particular forms of reasoning are good or bad, these forms of reasoning qualify as a logic. He claims that a justificatory principle can be provided for analogical reasoning. He does so by showing that analogies were crucial tools of reasoning for Ampère and Kepler and by specifying a set of conditions for successful analogical reasoning, thereby drawing on recent studies of human cognition.

Lund thus grounds his amendment and justification of Hanson's logic of discovery on episodes from the history of science as well as on information gathered from cognitive science. But is this an adequate justification for a philosophical position? Lund's move brings us to another thorny problem, the long-standing question of the relation between history and philosophy of science. As Lund notes, Hanson's view of this relation is not easy to pin down. Hanson was the cofounder of one of the oldest HPS departments in the United States, the Department of History and Philosophy of Science at Indiana University, Bloomington. He explicitly subscribed to the familiar dictum that history of science without philosophy of science is blind and philosophy of science without history of science is empty, while at the same time stating that the logical relevance of history of science to philosophy of science was nil. Lund does his best to disentangle and reconcile Hanson's diverse contributions to the problem of HPS. Hanson believed that while historical facts cannot be used to *justify* philosophical claims—this would mean committing the genetic fallacy—philosophical analysis must *begin* with the scientific theories and evidence available at a particular time; otherwise it would not even qualify as philosophy of science. In his later works, however, Hanson did allow for a mild form of the

genetic fallacy, although he did not spell out what exactly this meant.

Seeking to strengthen Hanson's account, Lund suggests that a normative theory of science may be derived from empirical data if one can show that there is a high probability for the theory to be true given the evidence available. According to Lund, the facts of past and present human cognition as we now know them should be a key ingredient of our philosophical accounts of knowledge generation—and this is of course precisely his own justificatory strategy for demonstrating the power of analogical reasoning. Lund's criticism is well put, but I do not think that his amendment can solve the “is-ought” problem that has troubled the relation between history and philosophy of science for so long. The problem as such cannot be solved; the only option we have is to move on to an alternative conception of philosophy of science.

Having finished reading *N. R. Hanson: Observation, Discovery, and Scientific Change*, I remain somewhat unsatisfied. Lund ends on a downbeat note, quoting John Ziman's verdict that Hanson's name will “never be a great one in philosophy.” By no means do I think that only “great” names deserve the attention of historians of philosophy. But Lund has not quite convinced me that Hanson's philosophical thought merits further scrutiny. I can't help thinking that Lund is not quite convinced either.

JUTTA SCHICKORE

Isabelle Stengers. *Cosmopolitics I*. Translated by **Robert Bononno**. (Posthumanities, 9.) viii + 299 pp., index. Minneapolis/London: University of Minnesota Press, 2010. \$25 (paper).

In *Cosmopolitics I*, Isabelle Stengers pursues two distinct but interrelated goals. The first is to account for the historical emergence, in the modern era, of a “psychosocial” physicist, whose identity is exemplified by Planck's insistence, *contra* Mach, that the unobservable entities of modern physics are both historically fabricated *and* endowed with an autonomous, ahistorical existence. According to Stengers, the considerations and practices of the “psychosocial” physicist lend coherence to these two apparently contradictory characteristics. The second goal is to trace the vagaries of the concept of “state” through the history of modern dynamics. Stengers argues that the idea of “change of state” eclipsed the idea of “temporal change” as the prevailing means by which physicists came to understand the events satisfying the requirements of dynamics. Stengers largely focuses on

this second goal, guiding the reader on an impressively erudite, often fascinating, and occasionally suspenseful journey stretching from the seventeenth century, through the work of Leibniz, Lagrange, Carnot, and Hamilton, and ending in the late nineteenth century. She gives much less attention to the first goal, perhaps because she views the relation between the physicist and the objects of physics as one of “reciprocal capture”: these two *relata* “coincident” one another (p. 38). We are presumably meant to accept that a history of the concept of “state” is simultaneously a history of the modern “psychosocial” physicist. This strategy is, at best, unconvincing. At worst, it is a rhetorical dodge, a smokescreen meant to obscure the fact that Stengers is really offering only an eccentric, though still quite excellent, example of traditional intellectual, as opposed to social, history of science. The historiographic heat generated by such metaphysical firecrackers as “reciprocal capture” will already be well known to many readers, and Stengers’s own contribution provides, in this regard, little in the way of new light.

In reflecting on Stengers’s historiographic method, it may help to place her in sequence with Planck and his fellow travelers, who, Stengers tells us, “forge[d] increasingly audacious categories with great freedom” (p. 169). These gentlemen should not be criticized for their methodological errors, urges Stengers, but recognized as “poets” who relied on “faith” rather than “austere rationality” in their passionate pursuit of the truth (pp. 169, 6). Such recognition, she suggests, would provide us with an antidote against the cultural power of physics. We should see physicists not as rational masters of the cosmos, but as themselves held captive by the magisterial power of their own radical creations. The audacious freedom with which they leave behind observable phenomena springs from their passionate submission to the internal exigencies of their own historical enterprise.

According to Stengers, Planck promoted the passionate freedom of the physicist in order to safeguard the specificity of physics *vis-à-vis* the other sciences: indeed, “he literally ‘cries out’ against Mach the *fact* of that difference” (p. 7). Yet there was more at stake here than professional autonomy. Mach himself would go on to inspire the Vienna Circle, whose members sought to balance scientists’ autonomy with their public responsibility by emphasizing the intersubjective basis of scientific knowledge in shared rules and concepts. They rejected metaphysical statements as inimical to the public verifiability of scientific knowledge claims.

Kuhn followed suit, but he insisted that the intersubjective basis of scientific knowledge also includes irreducibly tacit elements. He took this idea from Polanyi, while rejecting the latter’s preoccupation with “scientific passions” as enablers of a “mystical contemplation of nature” (Michael Polanyi, *Personal Knowledge: Towards a Post-Critical Philosophy* [Routledge & Kegan Paul, 1958], p. 133). With his emphasis on the passions, Polanyi belongs in the historical sequence of Planck, and it is here that Stengers too belongs. She almost entirely ignores the intersubjective nature of the scientific activity whose history she otherwise so delightfully tells, focusing instead on the way in which specific concepts and signs accumulate power over time and thus drive scientists to ever more radical feats of intellectual ingenuity. Indeed, one of the great heroes of Stengers’s story turns out to be the concept of “equivalence,” represented by the diminutive but stunningly powerful = sign (pp. 170, 254). Despite her insistence on the specificity of her own historiographic method (p. 221), Stengers finds her proper place as a distinguished contributor to the French tradition of “the history of the concept,” which also includes such luminaries as Bachelard, Canguilhem, Cavaillès, and Foucault. And, as with these others, Stengers’s historiographic method, despite its indubitable virtues, is finally unable to illuminate how a concept or sign can apply, without the continuous guidance of busy human hands, the pressure needed to oblige scientists to act in one way rather than another.

JEFF KOCHAN

Andrew Robinson. *Sudden Genius? The Gradual Path to Creative Breakthroughs.* xxxv + 371 pp., illus., bibl., index. Oxford/New York: Oxford University Press, 2010. \$34.95 (cloth).

Andrew Robinson is a literary editor and the author of twenty books, several of which are biographies of talented individuals such as Einstein, Thomas Young, Satyajit Ray, and Rabindranath Tagore. He has personally known Nobel winners—for example, the physicist Subrahmanyam Chandrasekhar—and artists—for example, the photographer Henri Cartier-Bresson. These personal associations were sources of his interest in creativity and the idea of geniuses, from which he “began to see that their breakthroughs had key elements in common” (p. xiv).

The book is divided into three parts. Sandwiched between an initial essay on the “ingredients of creativity” and a final essay on “patterns of genius” are biographical studies of