

Three Kinds of Political Engagement for Philosophy of Science

George Reisch

Published online: 20 June 2007
© Springer Science+Business Media B.V. 2007

Abstract In responding to critics and reviewers of my book, *How the Cold War Transformed Philosophy of Science*, I attempt to identify some misleading conventional wisdom about the place of values in philosophy of science and then offer three distinct ways in which philosophers of science can engage their work with ongoing social and political currents.

Keywords Cold war · Philosophy of science · Values · Value-free · Value-neutral

Critics of my book, *How the Cold War Transformed Philosophy of Science*, correctly point out several shortcomings. For example, the book does not provide a comprehensive survey of offer anything close to a survey of philosophy of science in the United States during and before the 1930s. My focus was on the unity of science movement and its reception in New York and Chicago; and, as Thomas Uebel points out, it concerns logical empiricism more narrowly than philosophy of science, broadly understood. I admit, in addition, these narrow foci create a difficulty, because the story I tell about this warm reception and the subsequent (and relatively chilly) postwar depoliticization of logical empiricism raises obvious questions about how far and wide this process really was. Does one find the same or a similar story in California and Minnesota, Philadelphia and Boston? Besides that, the book does not begin to cover Dewey, Reichenbach, Hempel and many others in sufficient depth to provide a broad picture of the profession and its development in these decades.

The fact is, I found the interactions and correspondence between the New York Intellectuals and the Vienna Circle emigres in New York and Chicago, and the FBI investigations of William Malisoff, Philipp Frank, Rudolf Carnap, and Albert Blumberg so surprising and, but for the documents in front of me, unbelievable, it seemed that once a coherent story emerged, however narrow, it needed to be told. No matter what a broader

G. Reisch (✉)

Open Court Publishing Company, Carus Publishing Company, Chicago, IL 60603, USA
e-mail: reischg@georgereisch.com

and more complete account might look like, I reasoned, these more or less direct connections between the profession's history and powerful political and cultural forces in European and American history almost certainly have some role to play in our understanding of the profession.

1 Value-Free and Value-Neutral

One of the striking things I've learned from reviews and criticisms of the book is how varied, unsettled, and sometimes unclear our intuitions are about the nature and roles of values in science and, in turn, philosophy of science.¹ Little has changed, I am tempted to conclude, since philosophers of science debated the questions posed by Kuhn's appeal to values in paradigms (in the 1960s and 70s) or even the 30s and 40s when figures such as Charles Morris attempted to find a place for the study of values in science by synthesizing American pragmatism and logical positivism.

Scott Edgar and David Stump both take politically *disengaged* philosophy of science to be one that, one way or another, keeps values at arm's length from science. Carnap's philosophy remained "value free," Stump notes, while Edgar takes the rise of politically "neutral" philosophy of science (as opposed to the anticommunist philosophy of science of Hook and others) to require explanation that he finds missing, or incomplete, in my book. Yet this vague notion of a discipline disengaged from values misleadingly structures our conception of political engagement as some opposite or inverted posture. Instead of eschewing values, that is, any politically engaged philosophy that we might conceive or attempt to take up, we suppose, must fill in the gap and equip itself with what has been missing. Instead of remaining aloof from values, such a philosophy of science must embrace values. Almost inevitably, during the cold war as well as today, our ideas about politically engaged philosophy of science arrive at something like *partisanship* or advocacy for particular values, causes, or political parties.

This is the mistake that Uebel suggests was made by the most vocal intellectual critics of Neurath's and Frank's unity of science movement, namely Horace Kallen and, by way of his aggressive anticommunism, Sidney Hook. They seemed to think that collective, public, intellectual projects such as those the movement hoped would thrive (and which included the *International Encyclopedia of Unified Science*, the international congresses, various study groups, journals and later, under Frank, essay contests) smacked of ideological alliance with centralized planning, thought-control and "totalitarianism." Totalitarianism opposed liberalism in a precise sense during these cold war polemics and was taken to be an extreme and pernicious kind of partisanship. It involved *imposing* thoughts or values (those of the party, the church, the cult, or what have you) on individuals or societies instead of nurturing free, unprejudiced inquiry and allowing it to reign.

As Uebel usefully reconstructs it, our understanding of this mistake goes a long way toward clarifying and defending my claims against Scott Edgar's query as to why a politically neutral kind of philosophy of science flourished when its cousin, the liberal, anticommunist pragmatism of Hook and Kallen did not flourish. The premise, I think, is either misleading or false, for it is not clear to me, at least, that Hook and Kallen did not

¹ At least one otherwise positive review mentions unclarity about values and their relation to science as a weakness in my book. See Jonathan Tsou, *The British Journal for the History of Science*, Volume 40, Issue (1), pp. 153–155.

succeed professionally and politically as much as Reichenbach, Feigl, Hempel, Brodbeck and others in establishing disciplinary borders that excluded the analysis of ideologies and social values in science or epistemology from the core of philosophy of science and, as Hook would have insisted, disciplines throughout the university. The fact that some philosophers, like Hook and Kallen, were vociferous anticommunists and the fact that others, most postwar logical empiricists, refrained from partisanship seems incidental to the fact that other kinds of philosophy of science that aimed to integrate critical understandings of science, society, and economy had become taboo and marginalized. The partisanship model of political engagement, in other words, is misleading. The question should not concern who does and who does not rely on or espouse values (as if that distinction mapped onto which program is and is not politically engaged) but rather what is the relationship of any philosophical project to the values that, inevitably, frames its methods and goals.

I agree with Hook on this point (and only this point) that the important question is how an individual or a discipline positions itself with respect to values. The problem with communist faculty, he regularly complained, was that they adopted values without examining them; they said and believed (he alleged) what the party told them to say and believe (Hook 1953). They were turncoats to liberalism in so far as they had given up their freedom to examine the world and themselves from top-to-bottom without doctrinal straightjackets or social or political force or intimidation.

This may seem confusing, I admit. For I claim in my book that Hook and Kallen who, along with other anticommunists, helped kill the last century's best hope for a serious, politically engaged philosophy of science in North America, were *themselves* authoritarian bullies who forced their colleagues to toe the popular anticommunist line. They most likely supported inquisitions like J. Edgar Hoover's and they spoke ill of colleagues who seemed one way or another "soft on communism." Why should I appeal to them for clarity about what it means to be a politically engaged philosophical program?

Stump sees this apparent twist in the logic of my story and calls me on it. He suggests that, if you want a politically engaged philosophy of science, you're going to have to deal with people like Sidney Hook and their bullying and intimidation. I disagree, however, for exactly this reason: Hook had a decent understanding of the obligations of intellectual liberalism, but he utterly failed to connect them coherently to his personal political campaigns against faculty who he argued, because of their interest or membership in the communist party, were unfit to hold university teaching jobs. At least one important philosopher of science who lost a teaching job at this time (though not, so far as I know, because of Hook, specifically) confided to me that he lost his job because he refused to yield on this point: If this were truly a free, liberal society, he reasoned, then as long as one does not break laws one should be free to associate politically and intellectually with others as one wishes. But the most vocal defenders of "freedom" during the cold war—Hook, Hoover, Buckley are those that figure in my story—nonetheless supported loyalty oaths, doctrinal recantations, and corrosive investigations of suspicious Americans in ways that would have made proud the very institutions—Rome, Moscow, the CPUSA—they claimed to oppose and urged us all to fear.

So, to Stump's point about Hook, and also to Edgar's claim that Hook represented a viable kind of politically engaged philosophy, I reply that this kind of anticommunist activity on the part of these philosophers is just that—political anticommunism that has very little connection to, and seems in fact contradicted by, their professed liberal intellectual ideals. This separation of philosophy and politics in Hook's career, at least, is further indicated by his political reversal, in the late 1930s from ardent Trotskyite to ardent

cold warrior; from defender of Marxism and socialism to virulent anticommunist. So far as I am aware, there was no coincident change or reorientation taking place in his conceptions of naturalism or pragmatism.

2 Values as Axioms

One way to help further specify the “minimal political conception of philosophy” that Uebel provides is to look at the mature, depoliticized conception of scientific theory that took hold in the 1950s and 60s and was later dubbed “the received view” in Suppe’s (1977): that a scientific theory can be specified as a formal system resting on axioms. On this conception, the task of contemporary “philosophical analysis,” as Feigl and Sellars put it in their compendium (which Edgar cites), and the task of “scientific philosophy” as Reichenbach popularized it in his *The Rise of Scientific Philosophy* (which I cite in my book) was to address the logical validity of interconnections within the system and deduction or inferences made on its basis (such as empirical predictions). As Edgar’s helpful discussion of the nuts and bolts of professionalization suggests, this axiomatic model of scientific theory enabled philosophy of science to be a profession with an abstract theory and method. Because particular scientific theories (*per* Reichenbach’s “context of discovery”) are free, unpredictable creations of the human imagination, this abstract theory and method must respond to history and be tailored for individual applications. Philosophy of science, in other words, is a non-standardizable practice.

But it would be incorrect to say that this picture of philosophy of science professionalized operates in any sense *without* values. What is correct, I think, is to say that it operated without *addressing* or *analyzing* values that are now understood to reside in, or near, the axiomatic bases of a theory—in, say, the reasons why empirically accurate theories are desired, at all; or in the factors that led to the development of one kind of physics (with particular axioms) over another. Reichenbach’s axiomatic model admits that values are embedded in any theory because the axioms, the starting points for the logical presentation (or philosophical reconstruction), must be volitionally and freely chosen. The axioms may determine the deductions or corollaries to be drawn from the system, but nothing determines what the axioms may be save for the free choice of the scientist who built the theory (or the philosopher descriptively reconstructing it). This is the framework that Reichenbach uses to *explain* why philosophy of science is “value-neutral” and altogether divorced from ethics and political advocacy: Were ethical knowledge and beliefs to become the target of philosophy of science, there could be no way to justify the axioms that prop up that system of belief: “The scientific philosopher distinguishes between ethical axioms, or premises, and ethical implications, and he regards only the implications as capable of logical proof” (Reichenbach 1951, p. 319). When it comes to ethics, just as when it comes to knowledge of nature, the philosopher of science is an analyst, only, and never an advocate.

3 Three Types of Engagement

The fact that so many commentators persist in taking logical empiricism to be “value free” in important ways, I figure, is just further testament to how effectively philosophy of science was able to remove itself from the vexing questions about “values,” “ideologies” and alternate value-systems that many took to sound suspiciously Soviet-friendly or

“pink” and which led many academics to change fields, transform their methods, or just find something else to study during the cold war. I take it to help explain why our ideas of political philosophy of science devolve to mere partisanship.

Even partisanship, however, seems to some reviewers a distant and obscure goal for the profession to consider pursuing. What could it mean, both David Stump and Alan Richardson ask, to be politically engaged as a philosopher of science?² Yet the issues they themselves place before question marks strike me as ready for contemporary students of philosophy of science and, even, logical empiricism to grapple with. Intelligent design, for instance, is (I am nearly certain) theology masquerading as science. But to make that case clear and cogent, one would need some conception of the sciences as an interrelated whole (i.e., unity of science) and an account (based on some reconstruction and logical analysis, perhaps) of why the claims of ID fail to connect in the right ways to those of other, well-accepted sciences. Government (or corporate) meddling in scientific research? We need some account of scientific objectivity in order to frame claims that corporate funding or government censors detract from that objectivity and, in turn, the integrity of science. But objectivity, Richardson and others have taught us, is a core concern of early neo-Kantian philosophy of science of the sort that Carnap and other logical empiricists imbibed before inventing scientific philosophy. This is not to say that philosophers of science today should read up on their Carnap and hire publicists. Only that materials and issues within the profession are available to take up these issues, should one choose to do so.

We can call this “Type-1” political engagement: a philosopher chooses an axe to grind with public or social relevance and then brings to bear his or her expertise and research to make a case for one side or the other. Examples include the feminist philosophers Stump mentions, James Fetzer’s writings about 9–11, and the use of Kuhnian models of science by ID advocates (again) to persuade us that creationism is an unfairly overlooked scientific paradigm just bursting power and light that could revolutionize our tired, anomaly-ridden understanding of Darwinism and natural history. In earlier decades, it includes Feigl’s writings for *The Humanist* magazine and Carnap’s signatures in support of disarmament and dialogue between the superpowers (informed as those were, he told Hook, by his contempt for “gross exaggerations” (in Reisch 2005, 282).

If there is any lesson to be had from the unity of science movement, however, it is that this is not the only type of engagement available. Neurath and Frank, for example, sought to build consciousness of science as a tool in the world and to create institutions (such as scientific philosophy itself and the *International Encyclopedia*) to facilitate responsible, collective development of the sciences and their use in modern life. This picture owes much to Uebel here, who has helped articulate the connections between logical empiricism and the ideals of Enlightenment that circulated in Europe and that connect Neurath’s *Encyclopedia* to Diderot’s. The essential point is that this kind of engagement is at one remove from these Type-1 engagements as partisan or advocate. A sophisticated philosophical understanding of science would help *equip* individuals to be advocates if they wished. More importantly, it would *equip them to be to be critical and discerning* about, say, social and economic claims made on the basis of “modern science” or “philosophy of science” made by those who were advocates or partisans. Call these “Type-2” engage-

² Richardson, politely attributing confusion and unclarity to Neurath and not me, says he is “not certain...what alternative future for the philosophy of science Reisch would like to promote” and asks, specifically, “What specific advice, for example, would Neurath (or Reisch) provide regarding the inter-relations of science and politics in, say, the teaching of Intelligent Design, or the alleged interference in science by the current U.S. administration?” (Richardson, [forthcoming](#).)

ments, designed to equip philosophers (and citizens) to engage in, or criticize, “Type-1” engagements.³

There is, finally, a third type of engagement, though it may not be of the sort that Richardson and Stump are asking me to specify. It comes from Frank, who wrote in an essay published in 1951 of the important difference between “active positivism” and “humble positivism” (Frank 1951). Any decent scientist or philosopher, Frank noted, is aware of the many things about the world that we do not know or practically do not know how to do. The difference between active and humble positivism concerns whether and how philosophers treat this awareness in their relations with others—personal, professional, or political. Active positivists will question or criticize those projects or pronouncements coming from cultural leaders or politicians that mistakenly assume knowledge or scientific consensus that is not in fact available. Active positivists believe, according to Frank, that “where science has no answer, no one else has any answer, either.” The “humble” positivist, on the other hand, either ignores feigned insights of politicians or pseudoscientists or, more interestingly, rationalizes and excuses such behavior on the ground that society needs, as Frank puts it, “such educators and rulers of men” to organize and structure society for a “good life” (in Reisch (2005), 298).

Frank’s distinction nods to these Type-2 engagements, since those would produce, he hoped, confident, clear-thinking active positivists that would be less susceptible to what, following Harry Frankfurt, we can now professionally refer to as bullshit in the public sphere. But it points to a third kind of engagement in the way that Frank called for active positivism in opposition to the declining fortunes of his unity of science movement. The clue is this notion, then bound up with the poor reputations of once Stalinist or fellow-traveling intellectuals in the popular and political magazines after 1939, roughly, that social stability required “rulers of men” that were *not* intellectuals. Intellectuals, the liberal, anticommunist press announced, had been very, very wrong about the humanist credentials of Moscow. So they were in no position to be leading national debates about how “the good life” was to be achieved in North America.

One common professional self-image for philosophers (and other kinds of intellectuals) was being forged into a new, politically disengaged profile. And this new and prominent distinction between intellectuals and statesmen or “rulers of men” functioned as a handy, possibly Straussian, rationalization that this transformation or demotion of intellectuals was all for the good. Frank resisted by calling for philosophers to stand actively, not humbly, in the way of this cultural shift. In this sense, Frank was taking up a Type-1 engagement, arguing for the importance of philosophy of science in society in much the same way that feminist philosophy of science engages public discourse about gender, as Stump reminds us.

But this rationalization also points to the third kind of engagement. Call it “Type-0” to suggest that it is a default engagement that every member of a profession participates in, if only by accepting the parameters and possibilities he or she inherits from professors and professional organizations upon joining. It corrects both of these prejudices we have about political engagement in philosophy—that this is a remote, and difficult to imagine posture to take as a philosopher; that something (values, or advocacy of particular values?) would

³ Philipp Frank’s last book was a survey of philosophical and political schools (Marxism, Thomism, and Pragmatism) and the ways they appealed to science for authority in making social, political, or economic arguments. The fact that Frank never published this book, however, and the broader decline of the unity of science movement in the 1950s, perhaps helps explain why this type of political philosophy of science has today a much lower profile than Type-1.

have to be injected anew into the profession, as if a paradigm would have to shift or new attitudes and ambitions would have to be cultivated. For according to Type-0 political engagement, the very history of the profession, and therefore its current range of projects and posture in the world, has been shaped by social and political trends in ways that remain, unfortunately, invisible to many. What Frank was hinting, I think, to those “humble” positivists is just this: while they may believe that they eschew political engagement either for matters of personal taste or because of certain professional ideals that they uphold, it may be more correct to say that they are, in fact, engaged in political struggles that they simply keep losing.

References

- Frank P (1951) The logical and sociological aspects of science. Contributions to the analysis and synthesis and knowledge, *Proceedings of the American Academy of Arts and Sciences* 80:16–30
- Hook S (1953) *Heresy, yes—conspiracy, no*. John Day, New York
- Reichenbach H (1951) *The rise of scientific philosophy*. University of California Press, Berkeley and Los Angeles
- Reisch G (2005) *How the cold war transformed philosophy of science: to the icy slopes of logic*. Cambridge, New York
- Richardson A (forthcoming) review of Reisch (2005), *Minerva* – an internet journal of philosophy
- Suppe F (1977) *The structure of scientific theories*. University of Illinois press, Chicago