

A Pall along the Watchtower: On Leaving the *HOPE* Conference

Philip Mirowski

All things that live long are gradually so saturated with reason that their origin in unreason becomes improbable. . . . Does the good historian not, at bottom, *contradict*?

—Friedrich Nietzsche, *Daybreak*

*We changed
“extract” in the
first page of the
article to
“epigraph.”
Please check.*

Precisely because the experience of the 2001 *HOPE* conference was a bit unnerving, I want to start out with a vote of thanks to Roy Weintraub and Craufurd Goodwin for coming up with the idea. Rarely have we enjoyed the opportunity to talk seriously and openly about the future prospects and pitfalls of our chosen field. So occasions such as this, where we collectively stare our predicament squarely in the face, are exceedingly scarce. It is even more rare to be confronted by the possibility that the history of economics as a disciplinary identity is on its last legs.

Now, the one capacity that you might suspect is congenitally rare in a collocation of historians is a well-developed ability to engage in futurology. Economists as a group have never enjoyed high lifetime completion averages in the realm of prediction; and I suspect people who have specialized in history are more inclined than most to avoid what can only be considered a high-risk, low-return activity like prognostication. True to form, few attendees at this conference engaged in any concerted reading of the tea leaves; more to the point, they tended to avert their gaze when it threatened to precipitate. Rather, the overriding impression I got from the papers and the conference was of a wistful sadness and a vague premonition of disaster; maybe nothing so portentous

as shifting deck chairs on the *Titanic*, but perhaps something more like a fire sale before the Wal-Mart opens up down the block. Of course, for the younger participants at the conference, with more to fear and less commitment to the cause, glancing nervously in the direction of the lifeboats was an understandable reaction.

It never hurts to entertain the idea that the inexorable tide of history may not be raising all boats; but what I might have expected from self-identified historians located primarily in economics departments (although that is changing, too) would have been adoption of a more analytical approach to the predicament of the history of economic thought (HET), perhaps accompanied by a greater attempt to situate the prognosis in a larger context, be it that of the economics profession, or the larger university setting, or perhaps even larger cultural attitudes toward forms of legitimate research at the turn of the millennium. We did get some of that, especially from our European counterparts—and the contrasting health of the European and Japanese professions was one of the surprises of the conference—but I can't shake the feeling we did not really yet manage to get down to brass tacks. It doesn't help matters to suggest that the field has "always" been held in contempt (Blaug 2001); that's a prescription for paralysis and, anyway, is historically inaccurate, as I argue below.

As if it weren't enough to own up to the snubs and slights that are a daily penance for the historian of economics, I think we should also face up to the fact that there are in place some imposing structural obstacles in North America to the prospect of a flourishing program of research into the history of economics in the next millennium. Margaret Schabas (this volume) alludes to the first obstacle in her essay, when she complains that the field is saddled with the designation *history of economic thought*. Most of us at some point in our career have had to endure some philistine calling what we do "economic history," but I doubt that the situation is ameliorated by opting for the opposite extreme of limiting the ambit of research to some disembodied "thought" thinking itself. Taking "thought" as our primary area expertise is perhaps our first major stumbling block. The implied distinction between "thought" and everything else really only serves to reify and validate a sharp separation between "pure" and "applied" economics, a separation that only became firmly established in the economics profession in the 1930s to 1940s. Yet I have been vexed to discover at this late date how very little work has been done on the history of various practices of economic empiricism. (The

recent rash of work on the history of econometrics, with few exceptions, has been the history of econometric *theory*.) Worse, this locution presupposes the existence of some Platonic form called “economic thought,” nicely partitioned off from “sociological thought” or “culinary thought” or “religious thought” or “biological thought.” Hence the very concept of an identity as a “historian of thought” has had real consequences in terms of the types of work previously attempted and the forms in which it is promulgated. As HET is now pushed out of economics departments, the very ontology of the subject matter suddenly grows more wobbly.

But our problems are not simply philosophical. Among historians in general, for instance, I think it is fair to say that practitioners of HET are frequently regarded as not quite cutting the mustard as *historians*. Most professional intellectual historians would frown upon someone reading a handful of superceded texts, ignoring the various citation clues deposited by the author, eschewing archival or contextual research, and then proceeding to write an essay expounding on the “true” meaning of the work as though it were transparently accessible to anyone who could read beyond a certain rudimentary level.¹ In some quarters, this is not considered history at all, but rote schoolroom explication de texte. Thus I think it is significant that while we had participants at this conference located in various departments outside of economics, we have not heard from a single one who resides in a history department. Opting to resemble orthodox economists far more than they seek to imitate historians in this fashion, with their economic “thought” traversing an abstract trajectory in a vacuum, the proponents of HET have alienated one of their natural core constituencies among the historians, without particularly gaining the respect or gratitude of economists.

This has some bearing on Craufurd Goodwin’s claim that there is or was something like “HET for all the disciplines.” Perhaps we move in different circles, but I don’t detect that historians even acknowledge the existence of such a category. Look, for instance, at Novick 1988, which is an attempt to survey the twentieth-century American waterfront, with a stress on intellectual history. There is no mention of Joseph

1. Perhaps I will be allowed one supporting quotation: “We historians, as a result of our training and inclinations, are professionally sensitized to the historicity of intellectual life: the extent to which the emergence of ideas and their reception are decisively shaped by surrounding cultural assumptions, social setting, and other elements of the total social context” (Novick 1988, 6). Economists, by contrast, are taught *not* to do this by their training. See, for instance, Myerson 1999.

Schumpeter, or Joseph Dorfman, or Blaug, or anyone else recognizable as an HET author (although there is a passage on Robert Fogel and Stanley Engerman). Or contemplate how in their published work Dorothy Ross or Mary Poovey or Yuval Yonay or Emma Rothschild seem to go out of their way to avoid acknowledging the writings of our serried ranks. Or consult the *Journal of the History of Ideas* or *Journal of the History of the Behavioral Sciences* or *History of the Human Sciences* to savor the silence. The suggestion is that the supposed “classics” of HET may have been profoundly out of sync with historiographic trends for perhaps a century now, largely due to subordination to the economics discipline, and that the attempt to draw upon our fund of interdisciplinary credentials for respect or legitimacy in our hour of need is essentially bankrupt.

For a while, caught between the disdain of the historians and the contempt of their economist colleagues, historians of economics sought succor from their colleagues in the philosophy of science. That, for a while, seemed a more promising alliance. People who wandered into the history of economics probably did so out of a curiosity about the intellectual background, epistemic status, and ontological quiddity of economics, and in the 1960s it may have appeared that their concerns were being addressed by a working alliance being forged between the history and philosophy of science, which held out the enticing prospect of serious cross-fertilization. The third daunting obstacle to the future success of the history of economics is that these hopes have been sorely dashed in the interim (Hands 2001), with disenchantment with the Kuhnian version of the alliance having turned especially bitter of late (Mirowski 2001). The erstwhile Science Wars are merely symptomatic of this recent turn in academic culture.

But these are all eminently conceptual obstacles to a bright future for HET; I should be violating my own precepts if I neglected to point out that the material context of our activities also throws up a fourth obstacle to the continued flourishing of HET. This problem of the place of HET in the universities, and especially in the curriculum, has been raised by a number of papers at the conference; but parsed most explicitly by Brad Bateman and Roger Backhouse. People may make various claims for the salutary virtues of the study of history, but these are unavailing if we ignore the fact that the American and European university education is undergoing wrenching transformation as we speak. The way in which disciplines are organized, and the ways in which research is funded and conducted, and the relative status of pedagogy are being

radically restructured from the older “Cold War” regime into the newer situation of competitive, globalized privatization (Mirowski and Sent 2002). Whatever pedagogical functions one may have believed a course in HET could have served in the older framework, historians of economics have yet to even begin to forge an accommodation with the newer regime. In some ways, this may be the most daunting obstacle facing historians in the new millennium. I shall therefore devote the rest of the essay to asking: Who might want the history of economics in the future?

History of Economics in the Modern Academic Landscape

How Disciplines Have Shed Their Histories

It would seem imperative to get clear on the changing role of self-histories in the evolving state of the sciences and the academic professions in general, to begin to address the question at hand. Historians of science have pointed the way with careful consideration of their uneasy relationships with the fields they study (Reingold 1991, Thackray 1980, Dennis 1997). In short, and restricting our summary temporarily to the American context, the sciences only began to professionalize around the turn of the last century, and courses in the history of the individual disciplines (history of physics in physics; chemistry in chemistry departments; and so forth) were taught by practitioners within those disciplines with an amateur interest in intellectual history. These were offered explicitly to meet a demand to “humanize” what was often disparaged as a narrowly technocratic and vocational education by their opponents, the partisans of a “liberal education.” In the first two decades of the century, history of science rested uneasily in the curriculum, often depending on the largesse of the odd amateur or bibliophile; it had “great difficulty in acquiring the cognitive and professional identities which mark an academic discipline” (Thackray 1980, 461). The career of George Sarton, the founder of the journal *Isis*, illustrates not the success of the discipline, but rather its shaky status within higher education and its lack of support from within the history profession.

This began to change around the 1930s, with historians recruited to take sides in the debate over whether science could be subject to planning, and entered a new and more stable academic phase in the 1940s. Most historians now trace the professionalization of the history of

science, marked by its separation from “practitioner’s history” and amateur incursions, to Harvard president James Conant’s imposition of the General Education requirements at that university (Dennis 1997, Fuller 2000, Hershberg 1993). The issue that exercised Conant was the need to reconcile the apocalyptic products and authoritarian processes of “Big Science” with the political commitment to the superiority of democratic and egalitarian political structures in the Cold War. Conant and his lieutenant in “Nat. Sci. 4,” Thomas Kuhn (that’s right—the very same), started out from the premise that big science, like the atomic bomb, was here to stay; somehow ordinary citizens had to be brought to acquiesce in the wisdom of its existence even though they would never be able (due to incapacity or Cold War secrecy) to really understand how it worked. The pedagogical solution was to develop one image of science for the layperson and another, distinctly separate pedagogy for the tyro scientist. The layperson was to be cajoled into accepting an oversocialized conception of scientists so that they might believe (however wrongly) that dissent and disagreement were absent from legitimate science, and science, therefore, exhibited a self-organizing structure and unified valuation schema no matter what the cultural set-up. Further, it became imperative to reify a strong pure/applied distinction in order to deny that scientists bore any role or responsibility for the uses to which their discoveries were put—no more of Oppenheimer’s hand-wringing about “discovering sin.” Henceforward, grateful Harvard products would be satisfied that their tax dollars were being spent for who-knows-what arcane classified research, which might eventually result in such boons to mankind as Teflon and the Internet. While the Harvard general education course sought to inculcate the idea that all scientists deployed essentially the same “strategy and tactics” (Conant’s terminology) from the seventeenth century to the present (the famous unique “scientific method”), with the newly professionalized historian of science running the drills, the alternative pedagogy for the budding tyro scientist would dispense with any history altogether.

I want to stress that Conant (and Kuhn) really did believe the doctrines they were promulgating and were not just cynically concocting convenient Cold War ideology. If science really was fully self-organizing and autonomous from the uses to which it was put, then there really was no need for the “humanizing” balm of history for apprentice scientists: they should just work the problem sets, fill in the lab books, and soak up the

ineffable “tacit knowledge” of the scientific profession from their mentors. (Does this sound familiar? See Klamer and Colander 1990.) It is not frequently noticed that the Cold War reorganization of the university thus sought to *strengthen* boundaries between disciplines. Hence, in this program, history of science should be rescued from those prewar amateur practitioners (Kuhn never really liked “scientist’s history” anyway) and plopped down in a new profession whose mandate was to acquaint non-scientists with the true nature of generic science and to convince them that it was best for a democratic polity to just fund scientists to the hilt but otherwise leave them alone. (The relationship of this description to the actual situation on the ground of military funding and organization of many sciences is best left for another time.) Practicing scientists may, as a consequence, end up with a warped view of what *really* happened in history—this was the “wicked” or Orwellian side of Kuhn’s *Structure of Scientific Revolutions*—but in the final analysis, it didn’t really matter. (This has some bearing on the opinion often expressed at the conference that famous economists would “appreciate” the labors of historians of economics, if only *historians* were a bit more subservient.)

So Kuhn really did turn out to be one of the most important figures in the postwar history of science, although not in the way most people originally imagined. By the 1960s, most natural sciences had shed their self-historians (modulo the odd Grand Old Man pontificating on the historical significance of his own work), gladly devolving that function to a newly professionalized history community. This was facilitated by all sorts of funding arrangements: the NSF developing separate programs in history of science and science education, foundation support, agencies such as the Smithsonian and NASA hiring their own historians, the Alfred Sloan Foundation commissioning biographies of Nobelists, and so forth. What is germane to the present issue is that the social sciences slowly imitated this model with a pronounced lag.

Most interwar social sciences treated their self-histories not as humanizing supplements to technical education, but rather as integral parts of their “theory” sequences. Graduate students in political theory read Plato, Machiavelli, and John Locke; graduate students in sociology read Émile Durkheim, Herbert Spencer, and Marx; graduate students in anthropology read Marx and James Frazer and Franz Boas and Bronislaw Malinowski. One of the weaknesses of our own profession is that we do not adequately highlight that this was equally true in interwar economics, although Ross Emmett (1998, 144) has begun to rectify this situation

with his work on the Chicago economics department. However, the ubiquitous science envy of the social sciences was another prominent feature of the postwar period, with many funding agencies and military patrons seeking to encourage what they perceived as the more “solid” scientific tendencies within many of the social sciences. For instance, the Ford Foundation’s initiative in “behavioral science” in the 1950s was an umbrella for encouraging a melange of quantification, formalization, statistical hypothesis testing, and research focused intently on individuals as “part of an effort to dispel those notions of vagueness and reformism which so many laymen, including members of Congress, associated with social science” (Lyons 1969, 279). In effect, the patrons of social science sought to intensify a trend for the social sciences to imitate the natural sciences, both in research methods and in pedagogy. One seemingly minor aspect of this Cold War reengineering project was a notable movement in the newfangled “behavioral sciences” to jettison the older historical component of the graduate theory training sequence.² There were no such things as “hallowed classics” in the Conant-inspired construction of scientific pedagogy; only instrumentalist techniques (usually conflated with mathematics *tout court*): students don’t need to *think* so much as subordinate themselves to their mentors and then go forth and *do* science. This trend has proceeded to a greater or lesser degree in all the American social sciences down to the present day.

But the situation for the historians of social science did not turn out to be completely symmetrical with that of historians of the natural sciences. The latter were professionalized with a particular pedagogical function in view, albeit one aimed not so much at scientists but rather at the general populace. In some cases, although not all, they even merited a separate department within the university, a sure sign of having ascended to postwar legitimacy. When economics and the other social sciences jettisoned their history component, there existed no commensurate alternative role for them to occupy, and therefore no structural encouragement for a commensurate process of professionalization. This goes some distance in explaining the relative distance still maintained between the historians of science and historians of economics, as well as

2. As an example, the disdain of Cowles members Jacob Marschak and Tjalling Koopmans for those attempting to include a doctrinal historical component to research is covered in Mirowski 2002 (chap. 5). Perhaps some future historian will more directly explore how history was banished from individual major economics departments. When that happens, some of the more “liberal” reputations of famous twentieth-century economists will have to be revised.

the impressions of the field of HET being stranded between history and economics. It also provides a contrasting diagnosis of the possibility of an alliance of history of economics with the history of science to that offered by Margaret Schabas in this volume.

Why Be a Historian?

I should like to briefly address the challenge that Roy Weintraub threw out at the conference, namely, to entertain the idea that our economist colleagues are right: specifically, that we do not “belong” in an American economics department. I shall respond to it by turning to a suggestive article written by a historian of science.

Paul Forman (1991) has a nice article where he suggests that historians of science must have different ideals and standards than the scientists they write about. He writes that science (and, I would add, mathematics and economics) holds out to its members the prospect of personal transcendence: they submit to the moral authority and autonomy of their professional identity, and in return, they get to see their lives as one part of a quest or world-historical process: the search for their bequeathed notion of truth, or perhaps the improvement of the welfare of mankind. “The critical historian—understanding that scientific knowledge is socially constructed, partly within and partly outside the scientific discipline—must instead focus either on social problems of science or on science as a social problem. . . . Both are characterized by an implicit repudiation of transcendence” (83). The best they can aspire to is independence from the value system of the objects of their study.

This has direct consequences for what appears to be the dominant model of doctoral theses that still aspire to some historical content, namely, three essays, of which two contain “good orthodox economics,” while perhaps a third comprises a historical essay. If Forman is correct, then doesn’t this hybrid attempt to help the student get a job actually stifle the intellectual development of the historian of economics?

Liberal Education, Cold War Education, and Privatized Distance Education

Brad Bateman has stood out as someone who has been willing not only to publicly defend the place of HET in the pedagogy of undergraduates, but also to question the “research university ideal” that has come to

dominate graduate economics training. Further, he has also explored the coincidence of the decline of moral education with the rise to dominance of neoclassical economics in the American context (Bateman 1998). I am sympathetic to all these themes but would like to suggest that the problem is not specific to economics; rather, it is a function of the two great waves of innovation in American higher education: one dating from just after World War II, and the other really only getting underway in the 1990s.

The slow eclipse of liberal education is indeed one of the main determinants of the fortune of the history of economics in America. Liberal or moral education was the premier model of pedagogy in America up through the first third of the twentieth century. Various educational reformers like James Conant and Karl Compton of MIT, as well as the Ford and Rockefeller Foundations, wanted to thoroughly revamp education in the direction of meritocracy, technical expertise, and the closer integration of teaching and research roles through stratification of graduate and undergraduate curricula. Many innovations growing out of wartime, like overhead charges on research grants, research assistantships, the SATs (another Conant brainchild), and much else, transformed the way universities were run, or at least universities that wanted to hop aboard the government gravy train. As mentioned before, the effect of these Cold War innovations was to raise the barriers between disciplines and further encourage specialization. These institutions became vast assembly lines, offering students an impressive but ill-organized smorgasbord of “credit hours” often taught by graduate students, and with “requirements” largely structured around instrumental technique taught to large sections. By definition, those colleges and universities that hewed to the liberal education ideal, the Grinnells and Bowdoin and Oberlins and Williamses of the world, in effect opted out of the rankings race and could not expand at the rate of the newer “megaversities.” (No wonder your average student at a “top 5” graduate program looks upon a job in a liberal arts college with disdain!) It was outlandishly costly to “sit on a log with Adam Smith,” and in any event, it got in the way of the real output of the modern university, which was research. In the Cold War model, HET was just another elective, like public finance or the novels of Jane Austen. This model did not spread immediately to Europe but now is slowly becoming more and more the norm.

Yet I might suggest it would be misguided to pine after the older liberal model, because in turn the Cold War model has been concertedly

dismantled in the more recent past. The Cold War model depended heavily on regular and substantial government subsidies and subventions, which started to melt away with the cessation of the Cold War. Instead, politicians began to promote universities as growth poles in economic development, and incentives too numerous to enumerate were put in place to encourage privatization, first of research and now of the education process itself. Parenthetically, the Internet innovations surveyed by Ross Emmett in this volume are a big part of this incipient model of pedagogy. Suppose someone does eventually manage to automate a course in the history of economics, complete with streaming videos and flashy graphics and full-text searchable databases of “the classics,” as indeed many forward-looking educational institutions are presently encouraging their faculty to do.³ It will then rapidly become *de rigueur* to (1) quickly nail it down as intellectual property; (2) license the software out to other universities, and (3) hire gypsy unfaculty to “teach” the course for three grand a pop. I leave it to the imagination of my audience what effect this will have on the future demand for the history of economics.

I fear that all three models will continue to coexist in the near future: a few liberal arts colleges will survive as niche markets for children of the rich, a few big research universities will find their way to live off private research contracts and wealthy alumni, but the vast majority of students will enjoy privatized virtual universities via (terrible euphemism) “distance education.” Perhaps the role of HET in the liberal education model could not then be the really pressing issue.

Who Pays, and What Constitutes Value for Money?

Nevertheless, following up on the theme of this conference, the germane questions are who has paid for economics, and who might pay for history of economics? There is much less research by historians into this issue than one might hope, given that it would seem a natural inquiry for trained economists. The person with his finger on the pulse of this issue is indeed Craufurd Goodwin, both in Goodwin 1998 and in his article for this volume. In the former, he surveyed four postwar patrons: higher education, government, business, and foundations. I might slice his lineup a little differently, given, as above, that I think the structure of higher

3. For a glimpse of the future, visit the business school Web site of Michael Rappa: ecommerce.ncsu.edu/topics/index.html.

education was itself a Cold War phenomenon of government provenance; that “government” was not a monolithic player, but really should be subdivided into the military and the rest; and that the shift from foundations as purveyors of research initiatives to the military is the major theme in the postwar history of science policy in America. Nevertheless, what is really needed in the history of economics are studies of how much smaller but significant units of economics were and are funded: say, who paid for the NBER and why; the relative role of the Ford Foundation and the Air Force at RAND; the way in which the Cowles Commission was weaned off near-total dependence upon Alfred Cowles; who paid for the founding of CORE at the University of Louvain in Belgium; the role of various think tanks (Smith 1991); and so forth. Once we have a better handle on which patrons fertilized which sorts of economics, we might be better equipped to make realistic assessments of who might want to subsidize future versions of the history of economics.

For the nonce, the conference left us with some suggestions as to client groups who might still wish to devote a modicum of resources to promote HET as a component of their own agendas; but the stark truth is that no one could manage enthusiasm for the idea that the history of economics will be supported for its own sake in the foreseeable future. Indeed, as this appears, it seems likely that the popularity of the movie *A Beautiful Mind* based on Sylvia Nasar’s life of John Nash (1998) will have raised this issue in even more stark terms, with journalists and infotainment content providers making a play to displace any serious academic history of the modern discipline. Perhaps the lesson we should take from this is that the discussion of the banishment of history from economics should be widened out beyond the narrow circle of attendees at the Duke conference.

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