

# NEITHER ECONOMIST NOR HISTORIAN

BY  
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I defended my doctoral dissertation on stochastic stability of general equilibrium systems in Penn's Applied Mathematics program in fall 1968. That year I began teaching math for economists, mathematical economics, microeconomics, and even econometrics at Rutgers College, where I remained for a couple of years before moving to Duke. At Rutgers I saw that graduate students took required courses in micro, macro, statistics, math, and econometrics, and there were electives in other fields like public finance and economic history. I didn't know that there was any subdiscipline, or field, called the history of economic thought.

In writing my dissertation I had relied heavily on an excellent survey article on stability of equilibrium, by Takashi Negishi, that had been published in *Econometrica*. Negishi was a theorist, and he was writing about papers published in the past, so I assumed that his survey was good history since it brought current ideas and analyses into focus by tracing the development of those ideas. Such articles were in those years being published in solid mainstream journals like the *Economic Journal*, the *Southern Economic Journal*, and so on.

I also knew that my father lectured on what John Maynard Keynes really meant using Keynes's *General Theory*, his single required text, in his graduate macro class at Penn in the 1960s. Scanning mainstream journals, I saw that there were some economists, elder statesmen as it were, who occasionally wrote about past economists and their theories: Martin Bronfenbrenner wrote on Karl Marx, George Stigler wrote on Adam Smith and Alfred Marshall, Paul Samuelson wrote on everyone. These individuals had learned their own economics, and what was important for economists to know, in the 1930s. They self-identified as economists, not historians: the distinction made no sense at the time. They were comfortable employing arguments by canonical figures to make or support their own analyses and arguments. I thus assumed that any well-trained economist could

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Email: [erw@duke.edu](mailto:erw@duke.edu). As will be clear in what follows, this essay is mostly autobiographical. Asking someone in their ninth decade to reflect on past events that helped shape their personal history makes recollection inevitable. As a corrective to my account, perhaps a rereading of my 2003 HES Presidential Address, "Autobiographical Memory and the Historiography of Economics," reprinted as (Weintraub 2018), will provide a cautionary note. Some suggestions for intellectual renewal of the society appear in the [appendix](#).

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teach a history of economic thought course to undergraduates in the same way the mathematics community believed that any mathematician could teach calculus and linear algebra.

When we teach economics students, we often say that it is not so much the specific concepts that we want to convey, but rather we want to teach students how to think like an economist. Just as a mathematician speaks of students working to increase their mathematical sophistication, economics teachers want to help the student become adept in thinking in a disciplinary-appropriate fashion. From my own education, I had no idea that there was a community of historians. I would not have said that there was a way to think like an historian. From my own history classes, I assumed that historians simply knew lots of historical stuff like dates, names, battles, kings, and the like. Since I could never keep straight who or why anyone was conquering Malta, I thought that historians mostly had to have great memories. In any event they certainly were not scientists.

Like most economists, I knew nothing about the History of Economics Society (HES). I knew that many pre-war trained economists, like my father, were having difficulty maintaining their own ways of doing economics in the face of the postwar transformation of the economics discipline.<sup>1</sup> I knew that there was an Econometric Society because I was a member of it, just as I was a member of the American Economic Association (AEA), but these organizations were hardly welcoming. By the early 1970s it was getting more difficult to get any training in the history of economics (HE) as graduate programs began dropping the field.<sup>2</sup> There were only a few places in North America that encouraged graduate students to study the history of economics. I knew that at the University of Pennsylvania the history of economic thought had once been part of the economics PhD graduate requirements, but by the 1960s Arthur Bloomfield was the only one offering the non-required course.

As I was beginning my career trying to find my place in an economics department, I began publishing in very mainstream journals. I was also trying to figure out how I might prepare myself to work on one or another unsolved problem in economic theory. I recognized that the microfoundations of macroeconomics was a contested theoretical puzzle. Following what I saw as standard practice, and to teach myself about the literature (absent knowledgeable colleagues), I began writing a survey. Since “macro” to me meant Keynes’s theory, and “micro” to me meant neoclassical analysis, I began teaching in both areas. Through the 1970s I taught a graduate seminar on Keynes’s theory and its development using the emerging *Collected Writings* volumes.<sup>3</sup>

Mark Perlman, on a visit to Duke to give a lecture and to talk with his friends Martin Bronfenbrenner and Craufurd Goodwin, encouraged me to revise an early draft of my survey on microfoundations of macroeconomics and submit it to his AEA-sponsored *Journal of Economic Literature* (JEL). After its acceptance, he asked me to do an

<sup>1</sup> See, for instance, Morgan and Rutherford (1998). And for a deeply personal account, see Weintraub (2002a, ch. 7).

<sup>2</sup> These observations were documented in Ted Gayer’s (2002) “Graduate Studies in the History of Economic Thought.”

<sup>3</sup> It was in that period of the 1970s that Mark Perlman, in his role as editor of the *Journal of Economic Literature*, reclassified all articles and books on Keynes from “macro theory” to “history of economic thought,” leading some later bibliometric scholars to believe wrongly that the history of economics had, in the 1970s, become a more active field.

extended version of it for his Cambridge University Press book series called the Cambridge Surveys of Economic Literature. I was simply an economist writing about how a current literature in economics developed.

I was, though, beginning to obsess about how economists could appraise work in economics. That interest was shaped on the one hand by my having studied Karl Popper, Thomas Kuhn, and Imre Lakatos as a member of a reading group (when I visited the University of Bristol in 1971–72), and on the other hand by my being close to the founders of the Post Keynesian community (my father, Paul Davidson, Jan Kregel, et al.). I then naturally began to ask questions about the development of the contemporary theories I was trying to appraise. This was how I was dealing with these matters in the 1970s. I was not in any way connected to the older men who had come together to start the History of Economics Society. They began holding meetings, but I was unaware of them.<sup>4</sup> Yet, as my own interest in appraisal grew, I came to believe that theories of appraisal of subject areas had to be preceded by detailed contextualized histories of those areas, and this led me to think more seriously about the history of modern general equilibrium theory as the gateway to the microfoundations problem.

I need to be very clear here that there were no such histories of modern economic theories, there were only surveys of those theories for researchers, like graduate students, to get their bearings in a current literature. Interest in a contextualized history of contemporary theories was alien to individuals who were creating the History of Economics Society. They had their own projects writing about long-gone individuals like David Ricardo, Marx, Smith, Keynes, Marshall, Léon Walras, Thorstein Veblen, and the two Mills, James and John Stuart. Although some of them were performing serious scholarship based on prodigious (often biographical) research, others were continuing to produce exegetical or interpretative essays on old books, much as English literature scholars pored over *Paradise Lost* or the *Canterbury Tales*. Still others produced narratives of how economic ideas, economic thought, had developed: how, for instance, did the idea of economic rent develop in the works of A, B, and C? Such scholars performed the history of economic thought as intellectual history. I was not yet able to distinguish between individuals who wrote primarily on canonical texts and individuals who were concerned with the development of economics as a discipline. I was thus ignorant of the work of my colleague Craufurd Goodwin, as well as that of scholars like Joseph Dorfman, A. W. “Bob” Coats, Warren Samuels, and William Barber.

Since the history of economic thought textbook chapters stopped before discussing economics in the post-World War Two period, there were no textbook end-of-chapter questions like “Discuss the effect of Haavelmo’s treatment of probability in structuring econometric arguments.” This was the mirror image of mainstream economics textbooks that by the 1970s reified the comment Phoebe Dymmes once made to me: “There is no reason for any economist to read anything written before Samuelson’s *Foundations*.”

Put bluntly, in the 1970s there was no such subject area as the history of *recent* economics. The diminishing community of historians of economics was increasingly joined by heterodox economists who, too, were marginalized by the postwar neoclassical mainstream. As the mainstream insisted that economics was in fact a scientific

<sup>4</sup> John B. Davis wrote an exceptionally fine history of those meetings in his (2002) “The History of Economics as a Subdiscipline: The Role of the History of Economics Society Meetings.”

discipline, heterodox economists rehashed and reinvigorated old arguments that economics was not and could never be a real science. They began to argue that philosophers of science supported their beliefs. That is, there was a turn to discussions of how philosophers of science understood the growth of scientific knowledge and how those ideas could be applied to the growth (or lack of growth) of modern economics. The focus of such work was whether contemporary theories were or were not progressive, whether they were or were not exemplars of good science. Philosophers of economics, now called “methodologists,” engaged with these matters, as did some historians of economics. Other self-identified historians mostly pushed criticisms of modern economic models.<sup>5</sup> Their papers argued that a particular dead (or old) economist (e.g., Veblen, Keynes, Friedrich Hayek, Ludwig von Mises, Marx, Piero Sraffa, Michał Kalecki, etc.) worth a chapter in a history of thought textbook had been correct and modern economists were all wrong. This trope was fertilized by poor training in mathematics and statistics, and thus weak understanding of modern economic theory and econometrics. And since such history scholars could no longer publish in mainstream economics journals, new journals and societies proliferated under the banner “Pluralism Is a Good Thing.” This led the diminished history of economic thought community to welcome methodologists and heterodox economists. Certainly Marx was an historical figure, as was Keynes, as was Sraffa and (the undead) von Mises and Hayek. In this fashion heterodox economists who did economics along the lines developed by their (mostly) dead historical heroes self-identified as historians. They subsequently joined the History of Economics Society, and later the European Society for the History of Economic Thought, each of which quite welcomed them.

It wasn’t until I began to construct a history of modern general equilibrium theory around 1981–82 that I could say to myself that I was potentially interested in the history of economics.<sup>6</sup> The first time that I went to a meeting of the History of Economics Society was the 1982 meeting at Duke when Martin Bronfenbrenner was HES president, and he suggested that I present some of the material I was then developing. I scanned the published program to see about attending other sessions, and I recall being rather nonplussed at how few sessions were even remotely connected to postwar economics. I was also startled at the number of socially maladroit individuals who would stand up in various sessions and make speeches about why modern economics was all wrong, and why their own theory of XYZ did not receive the recognition it so obviously deserved. Many of the sessions were given over to long-dead X’s Theory of Y, or X as a neglected Precursor of Z. I don’t recall very many individuals my age or younger, and out of maybe 100 individuals present there may have been one or two women. It was certainly not like any Econometric Society meeting I had ever attended.

One of the reasons for such weird “contributions,” or what I was coming to think of as non-historical histories, was that there was a deliberate HES policy to accept all submitted conference papers (see Davis 2002), a policy that seems still to be in effect. Since most of the society’s North American members were at non-research universities and small colleges, they had no research budgets. Instead, these teachers had to justify

<sup>5</sup> This turn is evident in Mark Blaug’s (1980) *The Methodology of Economics*.

<sup>6</sup> The most interesting class I took as a mathematics undergraduate student was “American Intellectual History (After 1865)” with Robert Bannister, though three semester courses in the philosophy of science with Lawrence Sklar came in a close second.

their fees and expenses for attending HES to their dean or department chair. Approval of their request then was based on their participation—they had to present a paper or serve as an HES officer. Giving a paper meant receiving travel and lodging reimbursement. A healthy HES meeting needed lots of fee-paying attendees, so the number of papers submitted equaled the number of papers accepted. This was a far cry from usual practice for scientific conferences: a paper arguing that the physical world was created by God in 4004 BC would not be accepted for presentation at a twentieth-century geology conference. And when unrefereed papers from the HES conference began populating the pages of the *HES Bulletin* at the editor's discretion, that journal published some extremely weak papers.

One of the features of this quality unevenness can be traced to academic tastes developed in undergraduate courses in the history of economic thought. Such survey courses were structured like English literature survey courses of the “From Beowulf to Virginia Woolf” variety. Textbooks might begin with the Greeks, then amble thorough the Scholastics, the Physiocrats, then on to Smith, Thomas Malthus, Ricardo, et al. The mind-numbing article genre of “What economist X really said about topic Y” has its roots in such pedagogy. Longer undergraduate student essays, honors papers, and even master's theses were structured as recapitulations, exclusively based on the secondary literature, of what older economists and historians A, B, C, and so on had said about what X said about Y. They were rational, not historical, reconstructions. They concerned ideas, not people. They were interpretations and reinterpretations of passages from canonical texts: it was exegesis all the way down. Much of the published history of thought literature followed this rubric of the undergraduate syllabi.

I still did not think of myself as an historian. In those days I considered myself to be an economic theorist who was additionally interested in methodology, and I published articles in both areas (e.g., in volume 1, number 1, of the journal *Economics and Philosophy*). It was several years before I attended another HES meeting, in Boston in 1987.<sup>7</sup> I did attend the Philosophy of Science Association meeting in Pittsburgh in 1986 and the History of Science Society meeting in Raleigh in 1987. In those years I formed friendships with several individuals, particularly Mary Morgan, Ted Porter, Deirdre McCloskey, and Margaret Schabas, whose work I had come to admire.

By the late 1980s I began to lose confidence that methodology, and a focus on appraising economic analyses, could help me understand the development of those analyses. I also became allergic to the methodologists' facile assumption that “better” methodology would produce “better” economics. I came to believe that the actual history was obscured by the methodological presuppositions. This realization led me to the science studies literature, and I turned more consciously to historical reconstruction, and thick contextualization, of economic theories.<sup>8</sup> Following a fellowship year's residency

<sup>7</sup> Since I was chair of the Duke economics department from 1983 through 1987, I did go to some HES/methodology-related sessions at the ASSA (Allied Social Science Associations) in-between junior recruiting interviews. At Hyman Minsky's invitation, I even gave a paper appraising Joan Robinson's confusion about “equilibrium” at a 1984 AEA session. The paper was received by a remarkably hostile audience—Minsky had failed to tell me that his was a session honoring the recently deceased Robinson.

<sup>8</sup> Yann Giraud (2022) was able, in an extended conversation, to help me reconstruct how my scholarly tastes were changing in those years.

(1988–89) at the National Humanities Center, I began to think of myself more as an historian and less as an economist.

By the end of the 1980s I began to attend HES meetings more frequently, since some members of the HES community were getting interested in what I was trying to do, and I thought that by going to HES I could interest even more members of that community. I talked extensively with Don Patinkin about narrative strategies at the HES meeting in Richmond in 1989 as well as with Phil Mirowski about what we each were trying to work through at that time, pushing hard to contextualize how neoclassical economic theory developed in the postwar period. Some similar work was being done in Rome by Giorgio Israel and Bruna Ingrao. I began to hope that under the large HES umbrella, there was some room for me. And this led naturally to stronger engagement with that community as I attended HES meetings regularly from 1992 through 2016. In time I served as an executive committee member, and eventually an officer, of HES in the early 2000s, and I tried to open HES to both historians of science and science studies scholars.

I was unsuccessful in that effort. Meetings still were primarily constructed around sessions on individual past figures, since the presidents-elect could hardly find participants otherwise. Over those years HES as an organization concerned itself almost exclusively with putting on the annual meeting and hoping that the *Journal of the History of Economic Thought* was running on time. Providing grants to young scholars doing the same kind of work as their mentors was not any way to produce more research-rich histories. HES's scholarly values were displayed in the prizes awarded by its several committees. But what can be inferred about a society's self-understanding as it honored non-historians George L. S. Shackle, Edward A. G. Robinson, Israel Kirzner, and Geoff Harcourt as Distinguished Fellows? As a history society, but unlike the American Historical Association, it stood mute on intellectual controversies and historiography. The HES community through the society systematically ignored entire areas of scholarly work that were being produced outside that community and that could have been of interest to members of the community.

Even worse, HES as an organization maintained a view that historians of economics were economists. It ignored the marginalization of the HE community within the larger community of economists. Its members continued to argue that if economists only knew more history, they would be better economists, a view repudiated by mainstream economists. Replying "a pox on mainstream economists" was not helpful as faculty positions in North America and the UK continued to be lost and graduate students in economics with historical interests were discouraged, if not actively rebuked, by their economics teachers.<sup>9</sup> The closing of history of economics programs at the University of Notre Dame, Michigan State University, the University of Toronto, and the University of Amsterdam produced no response from HES. The HES band played on.

Following Neil De Marchi's and my retirements in 2016, and Goodwin's death in 2017, I've not been involved in HES. Thus, to contribute to this fiftieth anniversary, I looked back over my 2002b edited *HOPE (History of Political Economy)* conference volume *The Future of the History of Economics*, and its expression of my then pessimism about HES's intellectual health. Today there are only a few arguments I

<sup>9</sup> When my father died in 1983, my mother, brother, and I endowed a graduate fellowship in economics at Penn in his name. It was to be awarded to a student concentrating in either the history of economics or economic theory. To date, it has never been awarded to a non-theorist.



would revise. Yes, the subdiscipline is now more gender-diverse, and more international in its scope and membership, but the difficulties economists have in writing, and writing about, history remain. While the collapse of institutional support for training in the history of economics continues in North America, support for feminist scholars recovering contributions of women economists of the past appears to be robust. This literature, though, has often been shaped as a critique of an economics profession in which only men's voices were heard and men's writings were studied. But feminist economics is not history. There is still no widely shared understanding that one can write well about the history of modern economics without denigrating its corpus.

While North America and the Antipodes remain nearly history-free, that is not entirely the case in Europe and South America. Despite numerous national "Research Assessment" resource allocation schemes, a few younger HE scholars have managed to at least begin scholarly careers. While few so far have found long-term or permanent academic appointments in economics or history departments, some have joined interdisciplinary programs.

Unlike what I saw in 2002, today there might be some alternative paths for a subdiscipline drowning in the strait between the islands of Critique and Interpretation. A small group of European and American historians has contextualized postwar economics as one discipline among the several social sciences and recognizes that writing history requires a sensibility apart from "thinking like an economist." Those new historians of economics are connected to several younger historians and philosophers of the social sciences who themselves are in history or philosophy departments or interdisciplinary academic programs. Such individuals were trained in historical research methods in scholarly cultures that assume that writing the history of contemporary economics is a craft that can be taught.<sup>10</sup> The result is the emergence of new historiographic approaches involving social network analysis, prosopography, oral history, witness seminars, and exploration of syllabi and textbooks, artifacts, popular histories, and narrative strategies. The intellectual and socialization processes that help shape a disciplinary ethos in the philosophy of science, the sociology of science, history of science and history proper, and science and technology studies frequently produce complex, interesting, and thick histories. These historiographic developments, though, have not always led to interesting historical narratives, and neither does any particular technique pique the interest of oblivious economists.

Economics was becoming a mathematical science in the 1940s. Trygve Haavelmo, John von Neumann and Oskar Morgenstern, Paul Samuelson, Jan Tinbergen, Lawrence Klein, and others helped redefine economics. That was eighty years ago. Historians of economics were not participants in that postwar stabilization of economic evidence and argument. Fifty years ago, the History of Economics Society was created to support the work and teaching of those historians of economics. And now fifty years later the activities of economics and historians of economics have become nearly incomprehensible, one to the other. It is long past time for HES to reconsider its place in the present intellectual universe. It is long past that time for HES as it is presently organized to reconsider its purpose and alternative futures. For as Keynes once wrote:

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<sup>10</sup> See, for example, Weintraub and Düppe (2018).

There is no reason why we should not feel ourselves free to be bold, to be open, to experiment, to take action, to try the possibilities of things. And over against us, standing in the path, there is nothing but a few old gentlemen [sic] tightly buttoned-up in their frock coats, who only need to be treated with a little friendly disrespect and bowled over like ninepins. Quite likely they will enjoy it themselves, when once they have got over the shock. (Keynes [1929] 1972, p. 125)

## COMPETING INTERESTS

The author declares no competing interests exist.

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## Appendix: HES Renewal

Initially, the history of a scientific discipline or an intellectual movement is usually written by the practitioners of that discipline, with a polemical intent—that is, as a weapon in the internal struggle for the appropriation of symbolic and institutional



capital. The extensive use of documents and archival data is often a good sign of the emergence of a second wave of “true” historical scholarship, one which challenges received views and brings about a more nuanced and complex narrative of the rise (and often the fall) of disciplines and movements. (Bortolini 2013, p. 1)

The formation of the History of Economics Society (HES) in the early 1970s was associated with the desire to bring together those few members of the economics profession who were concerned with, and wrote about, the history of economics. The society’s founders sought to create a scholarly community. The founders of HES were Robert Eagly, Frank Fetter, Craufurd Goodwin, Warren Samuels, Joseph Spengler, and Vincent Tarascio. They were all economists, and none of them were hostile to mainstream economics. They were not heterodox economists. HES saw itself as a field in economics, like public finance, and wished both to define that separate field and connect it to the larger economics profession. The HES annual meeting was to collect like-minded individuals to provide a personal sense of being a member of a well-defined subcommunity. Eventually HES established a presence at American Economic Association meetings, which themselves were folded into the larger Allied Social Science Associations annual meetings. In those days members of the HES community considered themselves to be, and with a few exceptions were, North American economists interested in the history of economic thought.

HES considered that its job, its singular mission as a scholarly society, was to promote high-quality research and teaching in the history of economics distinguished by a strong historical sensibility.

Over the years, and into the current era, that original vision of HES’s mission to encourage teaching and research in the history of economic thought has proved impossible to effect. Specifically, the continued reduction of North American faculty positions in academic institutions that encourage or even permit study of the history of economic thought has proceeded unabated. While some history of economics courses remain in some liberal arts colleges, there are no mainstream graduate programs training new scholars. Although Colorado State University, George Mason University, the University of Utah, the University of Massachusetts at Amherst, and the New School do have history of economic thought graduate concentrations, they are in fact part of a larger heterodox economics or political economy concentration and produce heterodox economics PhDs.

The continuous growth of the history of economics community outside North America has fostered the formation of history of economics societies in Europe (France, Italy, the UK, et al.), Central and South America, Japan, the Iberian Peninsula, and so on, with their own journals and meetings. While this would appear to be a healthy affirmation of HES’s original mission, in practice it has not been that. The various national societies, with officers drawn almost exclusively from those nations, primarily cultivate the several national traditions and national work in the history of economics. Some of those traditions are critical of mainstream economics. Much of that work is practitioner history. By accepting the idea that history is properly the servant of critique, they oppose the HES mission to promote high-quality historical research. That is, the historical scholarship that emerges is frequently not up to research standards required by history or history of science graduate programs in North America.

The important question that HES must answer now is: How can it re-establish a meaningful role in promoting historical scholarship about economics?

In terms of its founding mission, HES has been and is a failure. HES needs to reaffirm its original mission. Allow me to make some suggestions.

1) I submit that in recent years significant contributions to the history of economics have been produced by non-economists. Historians, historians of science, science studies scholars, philosophers and sociologists of economics, and journalists have reframed tired conversations and themes in the history of economics literature. It is long past time that the history of economics community welcomes such scholarship, and such scholars. HES needs to reach out to other scholarly societies whose members have overlapping interests with historians of economics. Today an individual HES scholar finds it difficult to cross social science borders. HES could help that scholar to cross the border by sponsoring joint activities with other societies to enable scholars outside the economics community to play a more significant role in the history of economics community and the HES.

2) With respect to the annual HES meeting, it needs to be recognized that ever since HES's founding, all *submitted* papers to the annual HES meeting are *approved to appear* somewhere on the annual meeting's program: this lack of selectivity degrades the meeting's intellectual value. This is not a secret. With the increased internationalization of the HES annual meeting, conferees now often witness the confused and incommensurable national ideas of the nature of research in history of economics. The HES annual meeting in its current form is an increasingly unsuitable means to exhibit and thus encourage serious historical scholarship and research. Its sessions do not feature, or recognize, high-quality research in the history of economics. To showcase the best research, half of the sessions should be plenaries. Other papers, like student papers and work in progress, can be prepared for poster sessions. The annual conference should be held in North America. Monies to support the two annual conferences and to pay expenses of participants would be provided through current revenues from HES annual dues and the publisher's payments to HES associated with the *Journal of the History of Economic Thought (JHET)*. The overarching focus of the sessions at the two conferences should be important *historical research of professional interest to historians of economics*.

3) With respect to *JHET*, writing book reviews does not attest to the research scholarship of members of the society: writing them is unpaid community service and publishing them has little or no value to a career. Instead, I propose that the *JHET* editor commission review essays of significant books, and these essays be treated as articles in *JHET*. In its new format *JHET* would thus offer, in addition to a) research articles that exhibit a significant historical sensibility; b) a section of review essays; and c) a notices section of one-paragraph notices of recent book publications (much as the *Journal of Economic Literature* treats most textbooks and edited volumes). On the History of Science Society model of the eminent journal *Isis*, there could also be, at the editor's discretion, letters to the editor and elegies.

4) This aggressive focus on serious historical research suggests that there needs to be a tighter discipline associated with the various prizes that HES awards. The award today least connected to the mission of HES is the Distinguished Fellow award, which has sometimes been granted to individuals with no connection whatsoever to the history of economics. On one occasion the unfortunate selection of a non-historian led to a

retaliatory granting of a Distinguished Fellowship to that individual's ideological and non-historian opponent. This kind of intellectual failure needs to end: all the award committees must be committed to the mission of HES, and not to their own idiosyncratic views with respect to what is or is not the history of economics. The award committees thus need to be constituted by individuals who have already won those awards, or who have won similar awards by related organizations in history, history of science, science studies, philosophy, sociology, etc.