1-1-1997

New Economics and Its History: A Pickeringian View

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Until fairly recently, the history of economic thought as generally taught and researched tended to end around 1940, more than a half century ago. Some textbooks, of course, went further with a few chapters on "recent developments," but more often than not, I suspect these latter chapters were rarely taught or were compressed into one or two, hurried, end-of-the-term lectures. At the same time, until a few years ago contributions to journals and conference programs on the history of economic thought almost never left the terrain of past thought. Indeed, since the *History of Political Economy* appeared nearly three decades ago, it seems as if most historians of economic thought have concluded that they no longer speak to other economists, and might accordingly focus entirely on thought that is no longer actively pursued by contemporary economists and on which history has closed the door. Certainly, for a time, historians tried to keep feet in both domains, arguing to their colleagues that new ideas often recapitulated old ones and that a study of past thought improved current theorizing. I doubt, however, that many people today find this theme of eternal recurrence still persuasive. Our colleagues in graduate programs don't seem to have given it so much as even a passing thought, as they have systematically eliminated the history of economic thought from the

Thanks to Roger Backhouse, Zohreh Emami, Wade Hands, and Philip Mirowski for helpful comments on earlier versions of this article.
Ph.D. curriculum in economics. Indeed, were undergraduate education in many colleges and universities not tied to some vision of the liberal arts, the history of economic thought might cease to be taught altogether in most places.

In the last several years, a small minority of historians have begun to make recent, ongoing economics a primary subject of research. Roy Weintraub's *Stabilizing Dynamics* (1991) brought his story of general equilibrium theory right up to the temporal backdoor of current economics. Philip Mirowski's recent work, for example, on scientific constants and the Santa Fe Institute (1995, 1996), even anticipates the direction of ongoing research. At the same time, however, nonhistorians have not hesitated to chart recent Whig history in survey article after survey article across a variety of generalist journals. Indeed, the American Economic Association's *Journal of Economic Perspectives* might well be seen as a history of recent economic thought journal. Most articles describe the recent development of economic ideas, the task at which historians generally excel. Yet historians, who are capable of bringing deeper insights to such surveys, rarely even appear in the journal, and when they do it is usually in the “Features” section. How is it, then, that historians have come to focus almost exclusively on past thought and do not expect and are not expected to contribute the history of current economics?

Clearly, one reason historians of thought focus on past thought is that they have seen their charge as explicating the texts of the “greats”—Smith, Ricardo, Marx, Keynes, and the others. This view of the history of ideas is surprisingly resilient in light of literally centuries of critical thinking about the complexity of the historical process, and it may be that attachment to the view on the part of so many historians of economic thought is a function of something more contemporary—namely, their isolation in the economics profession. The rise of formalism in economics in the last half century (predicated on disdain for the concrete, the qualitative, the normative, and history) no doubt reinforces the notion that great texts are the historians’ exclusive domain. Thus, on the one hand, in response to the lack of engagement with historical thought on the part of other economists, historians may sniff defensively that there are no “greats” in recent thought and that they keep better company with their nonliving colleagues. On the other hand, caught up themselves in the profession’s modernist obsession with the demonstrable and certain, historians may find it most acceptable to turn to their own, private
well-sealed realm—namely, a handful of texts that possess a logic to be investigated apart from attention to the complicated histories of their producers and their times. Yet an attachment to the “greats” and their texts, while it may have temporary defensive advantages in an increasingly narrow and hostile profession, is no excuse for poor historiography. Historians of economic thought, as argued by Margaret Schabas (1992), simply ought to be better historians.

The matter might be put this way: How are historians of economics to avoid becoming keepers of an increasingly dusty museum of economic thought rarely visited except by undergraduates? Giving credit where credit is due, we might start by noting that the spirit behind the myth of eternal recurrence, if not the myth itself, seems deserving of rehabilitation. That is, if historians focus on what’s living in past economic thought, why not also explain what’s living in current economics and attempt to understand the present from a historical perspective? Presumably, some will allow this might be a good tactic in a battle with colleagues for resources and respect, but they will still deny it is good strategy. Strategy implicates deeper goals, and the seeming absence of “greats” on the contemporary scene will make the recommendation intellectually suspect and opportunistic to some. Such a reply, it seems fair to say, presupposes an artificial conception of the historical process and the contributions individuals make to the history of ideas. But perhaps the most effective way of making this obvious to doubters is for historians of thought to produce histories of current, ongoing economics in which it cannot be ignored that economists and their texts (or their models) are embedded in concrete historical circumstances. This is not to say that historians should give up work on Smith, Ricardo, and the others. Rather, their work on those individuals and their times should proceed with the same attention to historical context that we would more naturally (I believe) adopt toward our own time. In short, I recommend a more contextually informed history of economic thought that pursues the past and the present on essentially the same historiographic basis, where the goals are, first, to expand the space in which historians operate to include the present and, second, to produce better histories of past economics.

In the discussion that follows, I attempt to motivate these conclusions by applying Andrew Pickering’s view of scientific practice to the analysis of recent economics and the writing of its history. In my view, this generates a number of intriguing insights, some of which were introduced in Wade Hand’s recent (1994) reexamination of Weintraub’s *Stabilizing*
Dynamics. But it also creates a set of issues that derive from the fact that Pickering’s thinking about scientific practice is developed almost entirely in relation to natural science. The most important of these, in my estimation, concerns the historians’ participation in overlapping intellectual or interpretive communities, and what this implies regarding appraisal in the history of economics. Three sections thus follow: a rather compact summary of the principal themes and innovations in Pickering’s The Mangle of Practice; a brief illustration of these ideas in connection with one thread of contemporary economics, Robert Lucas and New Classical macroeconomics; and a look at what would be involved in thinking about the history of economic thought from Pickering’s perspective.

1. Pickering’s Mangle

Pickering’s The Mangle of Practice (1995b) offers a general analysis of scientific practice, which he calls the mangle. He charts his own development in the field of science studies as (1) beginning with the idea that science is more than a body of knowledge or collection of empirical and theoretical propositions about the world and (2) building upon documentation in the area of the sociology of scientific knowledge (SSK) on the importance of human production and use of scientific knowledge. In The Mangle this thinking culminates in a philosophical perspective alternative and complementary to SSK—suggested previously by Ian Hacking (1983)—that emphasizes what Pickering terms the “machinic aspects” of science and their implications for understanding the day-to-day business of doing science. As compared to a history of science rooted in epistemological concerns, Pickering takes scientific culture as his domain—a heterogeneous multiplicity of “made things” of science, including skills, social relations, machines and instruments, facts and theories—and then makes scientific practice, or the work of cultural extension, his chief focus (1995b, 3).

It is important to appreciate the distinctiveness of Pickering’s methodological thinking about science. He follows Richard Rorty’s (1979) dismissal of the project of philosophy as mirroring nature, though without adopting the metaphor of science as conversation or suggesting that its methods are those of literary criticism. His ideas have a strong social constructivist emphasis in regarding science as a human artifact, yet he

1. See Uskali Mäki 1993 for a summary discussion of SSK.
retains a commitment to realism, albeit what he terms a pragmatic realism. In his own view, Pickering principally distinguishes his work by its commitment to a performative image of science and by its rejection of the science-as-knowledge representational idiom. Scientists operate in communities, then, though not in interpretive communities with strategies for constructing meanings from and for texts, but rather in performative or practice-oriented communities with strategies for constructing machines and instruments to manipulate nature.

Scientists, he thus emphasizes, are not disembodied intellects, but agents doing things. Moreover, they are agents doing things in a world in which the world operated upon itself possesses agency, or "fights back," as it were, when scientists attempt to control it. This latter emphasis draws out an important implication of seeing scientists as agents rather than intellects, and seems to me to be at the core of Pickering's distinctive vision. Adopting a performative image of science, he reasons, involves not just investigating human agency in science, but investigating nonhuman, material agency, or, better, investigating the dance of agency in its human and material forms. Stretching and challenging our inclination to restrict agency to human beings, and brushing aside philosophers' qualms about the dependence of agency upon intentionality (6 n.), Pickering simply argues that there is much to learn about scientific practice by supposing that objects do things just as people do. Here he also parts paths with many in SSK, who, he charges, do not recognize how scientists' social interests and constraints themselves get transformed and mangled in scientific work when the world does unexpected things.

But let us descend from this rather high level of abstraction and briefly consider a particular case in the history of science that Pickering uses to illustrate his approach. Any scientific culture, he begins, includes machines and instruments that may display superhuman capacities but that are also enveloped in sets of human practices associated with their employment by scientists in research and experimentation. For example, physics Nobel laureate Donald Glaser, who was the inventor of the bubble chamber—the principal tool of experimental elementary-particle research in the 1960s and 1970s—was able to revolutionize scientific practice in subatomic physics because there already existed a community of scientific practitioners in physics that could be transformed into a new form of social organization (a big-science team) competent to carry out sustained investigation with his new machine. Their expertise, Pickering explains, derived from their being able to exercise a routinized,
disciplined, indeed machinelike behavior in the laboratory, and consequently collaborate with Glaser's machine in generating new particle theory results. On one level, then, what is involved in such cases is a process of tuning, whereby "just as the material contours and performativity of new machines have to be found out in the real time of practice, so too do the human skills, gestures, and practices that will envelop them" (16). But on another level, "disciplined human agency and captured material agency are ... constitutionally intertwined; they are interactively stabilized" (17; cf. Pickering 1989). Or, following Donna Haraway's apt metaphor (1991), Pickering characterizes "scientific culture . . . as itself a wild kind of machine built from radically heterogeneous parts, a supercyborg, harnessing material and disciplinary agency in material and human performances, some of which lead out into the world of representation, of facts and theories" (Pickering 1995, 145; emphasis added).

In one important respect, however, the symmetry of reciprocal effects worked by human and material agency upon each other breaks down. Scientists form intentions, aims, goals, plans; machines do not. Though machines are agents, human intentionality still lacks a counterpart in the material realm, and this implies, Pickering will ultimately go on to argue, that the modeling process in which scientists engage is an essentially open-ended process having in advance no determinate destination. How are we to reconcile this commitment to open-endedness with the idea that material agency also disciplines human agency, such that human aims and goals are bound up in and intertwined with past captures of material agency? Pickering distinguishes three stages or elements within the scientific modeling process, according to whether they reflect scientists' constraints or free discretion: bridging, "the construction of a bridgehead, that tentatively fixes a vector of cultural extension to be explored"; transcription, "the copying of established moves from the old system into the new space fixed by the bridgehead"; and filling, "completing the new system in the absence of any clear guidance from the base model" (116). Acts of bridging and filling are what he terms free moves, and they are the source of the open-endedness in scientific practice; transcriptions, by contrast, are forced moves that reveal how a scientific culture directs scientists' plans and intentions. This combination, Pickering argues in connection with a number of case studies, justifies our treating scientific practice as emergent upon scientific culture; it also tells us that human agency must have a temporal structure. The pathway scientific practice
follows, then, is not predetermined, is not free of the imprint of material agency, and is located in a real-time history.

Scientific practice, Pickering thus suggests, is a dance of agency that may be described as a process of resistance and accommodation. Scientists’ research goals and experimental plans, because of free moves, are emergent upon past captures of material agency. The scientist’s procedure is to “tentatively construct some new machine . . . then adopt a passive role, monitoring the performance of the machine to see whatever capture of material agency it might effect” (21). Does the machine perform as planned? As the story of quark-hunter Giacomo Morpurgo illustrates, the instruments with which a research program begins—here the magnetic levitation electrometer (MLE)—invariably undergo a whole series of modifications and reconstructions, as resistance in the form of unexpected results, no results, and counterintuitive results forces both a tinkering and re-tinkering with the investigative apparatus, and a parallel transformation of the scientist’s conceptualization of theoretical entities involved. The process comes to something of a resting point, not when original goals and plans are fulfilled, but when something that seems novel and substantial to the community of scientists involved in like research emerges:

In advance we have no idea what precise collection of parts will constitute a working machine, nor do we have any idea of what its precise powers will be. . . . We just have to find out, in practice, by passing through the mangle, how the next capture of material agency is to be made and what it will look like. Captures and their properties in this sense just happen. (24)

The continual reconfiguration of material setups in laboratories, interspersed with episodes of observing inactivity as the material world exercises its agency, shows that scientists move forward by making accommodations to the resistance the world throws up to their efforts. It is this dimension of the dialectic of resistance and accommodation that reveals the open-endedness of a scientific practice whose destination always remains to be determined. Indeed, it is the world’s resistance to our efforts, combined with space for free moves, that explains how scientific practice is open-ended.

So scientists’ plans, machines, and concepts are mangled in practice. But Pickering sees the mangling that the dance of agency causes as extending beyond the “inner” social organization of the laboratory
and the work of individual scientists to the wider, "outer" organization of science—namely, that network of technical-institutional-social relations that both define the tasks and place of science in society in its different forms, and which also puts the results of science to work for a variety of social purposes. One example is how the weapons laboratories in World War II influenced the postwar reorganization of academic physics. The most extended discussion of this wider ambit of the dialectic of resistance and accommodation in *The Mangle of Practice*, however, comes in an account of how numerically controlled machine tools were introduced at the General Electric Aero Engine Group plant in Lynn, Massachusetts, in the early 1960s. Lathes, milling machines, and other metalworking tools are for Pickering a prototypical capture of material agency that creates superhuman capacities. But they also require skilled operators to channel this agency; rather, they require a human-machine couple, a metalworking cyborg. And writ large, "we find sociocyborgs: arrays of lathes and milling machines in a corporate machine shop, operated by wage labor within a classic Taylorite disciplinary apparatus of specified social roles and relations—a hierarchical command structure, precise job descriptions, production targets, rewards and penalties, and so forth" (159).

At the outset, however, management at General Electric looked upon the introduction of numerically controlled machine tools into production in technological-determinist terms, supposing that "the innate properties of the new technology would themselves guarantee increased production of high-quality engine parts" (160). When production rates fell and worker protest developed, management moved to accommodate the resistance encountered by initiating a pilot program in which operators were encouraged to take over management functions, orchestrate production, assume responsibilities for quality control, monitor materials and safe functioning, and so on. Just as important, management recognized that its concept of a push-button human-machine couple on the shop floor had to be abandoned, and that it could only learn how to employ numerically controlled machine tools by letting operators learn how to operate them. The goals, theories, and social control strategies of GE management, then, were mangled and transformed in an open-ended process of accommodation to the resistance thrown up by the Aero Engine sociocyborg inseparably composed of workers and machines. Thus, while most of *The Mangle of Practice* concerns scientific practice in the narrower
sense of the laboratory, Pickering clearly believes the dance of agency extends fully across science, technology, and society.

One last theme deserves attention. Though Pickering believes himself much indebted to SSK, he nonetheless distinguishes his own approach from SSK as specifically posthumanist in its treatment of human agency. SSK studies from David Bloor, Barry Barnes, Steven Shapin, and others are social constructivist in supposing scientific practice always reflects the interests of individual scientists and the social constraints on scientific practice, as if we need only understand these social causes to understand the development of science. "The tendency," Pickering comments, "is to write as if the substantive interests of actors were present and identifiable in advance of particular passages of practice, setting them in motion and structuring outcomes without being themselves at stake" (64). Yet the interests and goals of scientists "cannot be regarded as unmoved movers, as causal principles lying outside (behind, above) and explaining the extension of scientific culture" (64), since they and the social identities of scientific actors are mangled and transformed in practice just as are the concepts and machines of science. SSK, Pickering tells us, helps us understand the role of human agency in science, but it remains wedded to a humanism that is reluctant to see that human agency as "inextricably entangled with the nonhuman, no longer at the center of the action and calling the shots" (26).

A posthumanism such as this may not flatter us in our desire to see ourselves at the source of science, but, Pickering believes, it is the logical consequence of supposing that scientific practice is truly open-ended. Thus, imagine that in experimentation there are "three disparate cultural elements: a material procedure (assembling and running a piece of apparatus), an interpretive model (a theoretical understanding of how the apparatus functioned), and a phenomenal model (a theoretical understanding of the phenomenon under investigation)" (1995a, 48). These heterogeneous elements constitute for Pickering the topology of experimental practice, in that they characterize different points on the surface of scientific culture scientists must combine in the modeling process. But as they are disparate in nature, they may only be linked together through a process of association, the exact nature of which the experimenter largely chances to hit upon. Accordingly, though constraints will always play a role in defining the setting in which science operates, this part of the story fails to capture the real-time dynamic of scientific practice.
2. Lucas Mangled

To say one intends to make new economics one’s subject may appear foolhardy when one considers the discipline’s propensity to dispose of all but the most recently published ideas. Though I haven’t attempted a systematic analysis of the matter, I suspect a study of the dates of citations in articles published in the top dozen Social Science Citation Index-ranked economics journals for any of the last few years would show dates largely concentrated over the last ten years. New economics resembles the ever-withdrawing Western frontier of the last century; as soon as the settlers seem to have reached it, it has moved on again beyond their reach. But I think one can nonetheless draw rough boundaries around our domain by distinguishing what practicing economists think (or hope) is not yet a subject for historians of economic thought. Supposing that most practicing economists hold to a Schumpeterian view of steady analytical progress in economics, new economics is the domain of breakthroughs that recasts or replaces all past ideas. Thus, though a figure such as Robert Lucas has in some respects been cast aside by later developments in macroeconomics, his contributions of the Lucas critique and development of rational expectations analysis still stand as breakthrough achievements in the eyes of even his critics.

What may we learn about Lucas from Pickering? Lucas, along with Thomas Sargent and Neil Wallace, dominated macroeconomics discussion throughout the 1970s, particularly in the United States. Though this analysis has been cast as a theoretical conceptualization of pre-Keynesian classical economic thinking, from a Pickeringian perspective it can be argued that it was rather the practical application of New Classical models, especially in the form of the Sargent-Wallace policy ineffectiveness proposition and the Lucas critique, that were the chief source of New Classicism’s impact. That is, Lucas and the others in the eyes of their followers had invented a model/machine which could be used to determine which actions policy makers could and could not pursue. Strictly speaking, Pickering does not regard models as machines. But it is interesting to treat them as such to re-read the recent history of macroeconomics. Thus, while traditional Keynesians and even monetarists complained that rational expectations and continuous market clearing (e.g., Fischer, 1977) do not adequately represent the true nature of the macroeconomy, and though there was not much empirical evidence that supported Lucas’s
"surprise" aggregate supply view, these representationalist difficulties were of secondary importance to the followers of New Classicism, who were chiefly interested in having a model/tool, even if initially crudely crafted, that would replace the policy interventionist model/tool wielded so long by traditional Keynesians.

From a Pickeringian perspective, economic culture, like scientific culture, is a heterogeneous assemblage of "made" things: tools, concepts, posited entities, and their associated social-technical relations. Economic practice, accordingly, consists in the extension of that culture through the invention of new tools of economic analysis: new mathematical models; new econometric techniques; and new procedures for data mining, experiments, calibration exercises, and so on. The physicists Glaser and Morpurgo modified and developed existing laboratory tools to construct new machines that made possible the "discovery" of quarks and various other subatomic particles. Similarly, Lucas and the other New Classicals refined and redeveloped existing mathematical and econometric models to construct new models/machines that made possible the "discovery" of economic agents with rational expectations! Indeed, their sudden repopulation of the world with a new type of economic agent simultaneously made extinct an older type of being: the naive victim of money illusion, whose expectations adapted but gradually to changing circumstances.

That Lucas and his coinvestigators were able to redirect macroeconomics as they did, we should then suppose, was due to the successful combination of free and forced moves they adopted. The structure of their New Classical economics basically involves three principal tenets: the rational expectations hypothesis, the assumption of continuous market clearing, and Lucas's "surprise" supply function which has output deviate from its "natural" level in response to the deviation of actual prices from expected prices, generally as a result of monetary disturbances. The last element was largely taken over from Milton Friedman's natural rate of unemployment analysis, and thus can be said to have constituted transcription—a forced move. The rational expectations hypothesis might be regarded as an act of bridging, a free move, "that tentatively fixes a vector of cultural extension to be explored" because it involved imagining a new phenomenon of expectations without adjustment lags. The continuous market clearing idea, finally, might be thought an act of filling, a free move where "guidance from the base model" is lacking.
and where traditional, neoclassical-Keynesian models had operated with money illusion.

Thus, in modern macroeconomics human agency and material agency combine in a dialectic of free and forced moves we may distinguish to understand economic practice. Or, economists enter into a process of resistance and accommodation, where free moves emergent on past captures of material agency lead to construction of new models, whose performance is then passively monitored to see what combination of resistances and effects results. In fact, the resistances Lucas encountered to the intended use of his model were more significant than initially believed, so that by the early 1980s New Classical models had reached an important theoretical and empirical impasse. Attempts to explain business cycles in terms of money-to-output causality had turned out not to be persuasive (Sims 1980), and evidence in support of the proposition that anticipated money was neutral also did not appear robust (Barro 1993). Yet if monetary shocks were not the cause of business cycles, some reasoned, then perhaps aggregate instability could be explained in terms of models that allowed for real shocks. Real business cycle theory of Finn Kydland and Edward Prescott (1982) and John Long and Charles Plosser (1983), we might here argue, threw over the monetary disturbance impulse mechanism of New Classical models—a free move, or an act of bridging—while yet retaining the endowments-tastes-technologies propagation mechanism of those models—a forced move, or an act of transcription.

Lucas’s reaction to his being supplanted as the leader of classically oriented theory shows a mangling of his original intentions. Objecting that an exclusive focus on real as opposed to monetary disturbances seemed a mistake, he allowed a hybrid model of both sorts of disturbances might be desirable. At the same time, as if to say that explaining business cycles had always been of secondary concern, he turned his attentions to analysis of the microfoundations of growth, thus minimizing the importance of fluctuations around trend growth (1987). Thus, though his model/machine did not function quite as expected, it did generate results that others could generalize upon in further modeling/machine-building, and it did create a platform for further model/machine construction on Lucas’s part. As a Nobel laureate, Lucas was lauded for his contributions to economic knowledge. But economists will rather recognize his accomplishments as a sophisticated toolmaker, just as they have credited
past prize winners—not with developing a better understanding of the economic world, but with advancing new models/machines with which one could operate on that world in novel ways.

Pickering, then, would have us take a second look at Lucas by going beyond the “inner” social organization of the economics profession within the academy to the “outer” organization of the science—namely, that network of social-institutional relations that both defines the ways in which the work of economics can be done and identifies the ways in which its results can be brought to bear on economic life. If we recall his account of the adoption of numerically controlled machine tools by GE management at the Lynn, Massachusetts, Aero Engine plant, we may similarly imagine how those anxious to employ New Classical economic models hoped to “manage” policy orientation in the macroeconomy by undermining traditional interventionist strategies. Indeed, the image of a self-correcting, rational expectations-laissez-faire economy offered by Lucas was an ideal sociocyborg apparatus writ large: a vast, economy-wide collection of machine-human parts, all smoothly adjusting to the discipline of the marketplace, never requiring maintenance or repair. Needless to say, Lucas’s new push-button technology was almost immediately mangled by real-world individuals who were either not rational or had not heard about the “true” model, and by markets that inexplicably failed to clear instantaneously. It was left to real business cycle theorists to logically conclude that real shocks must thus be pervasive!

Can we explain the history of this brief episode in recent economic thought in terms of SSK thinking? What Pickering’s mangle view specifically brings to the table is an emphasis on the transformation of human agency—goals and intentions—in the interaction with material agency. Perhaps some of his language about the active powers of the latter goes a bit far, but his dialectic of resistance and accommodation is valuable for the idea that the work of the scientist/economist is imperfect and incomplete, because it gets carried out in a medium whose properties are largely unknown. This means that ongoing work must emerge in conception and results as it is pursued, and that the interests driving modeling/machine-making are relatively indeterminate at the outset. It may be a fine point to distinguish Pickering’s mangle and SSK thought, but it provides a strong assist in helping one to see that pathways of investigation are relatively open. Let us thus turn to what we may understand about the history of economic thought when pursued in a Pickeringian manner.
3. Writing Recent (and Not-so-recent) History of Economics

This short sketch of Lucas and New Classical economics is meant merely as an illustration of what might be attempted from a Pickeringian perspective. Its point is to show how one might begin to look at the development of economic ideas as a process of resistance and accommodation, in which models function as invented machines. But notice that having historians of economics briefly step into Pickering’s shoes in this way does not actually apply Pickering’s lessons to writing the history of economic thought itself, which would involve treating historians of thought as scientists, tuning and being tuned in turn by their machines of historical analysis in the same manner as Pickering’s physicists. Indeed, in my sketch of Lucas, I stand outside my portrayal, offering you a new mapping grid without much comment upon how well this tool will respond to use or how it will affect the goals and intentions of historians of thought. But, for that matter, Pickering himself is not particularly good at stepping into his own shoes in this respect, since in his *Mangle of Practice* he also is almost always an outside observer of the dialectic of resistance and accommodation in which scientists are engaged. Though he does make comments on the problem of reflexivity in connection with SSK (or the stones-and-glass-houses problem), he never really asks how his own machine of analysis mangles his own intentions, professional behavior, ideas, and so on, and he even ventures rather carelessly at the end of the book that the mangle is possibly a TOE, or the holy lost grail Theory-Of-Everything.

Nonetheless, this does not prevent our reexamining the practice of the history of economic thought as one important extension of an economics culture of made things. And it may be that historians of thought can even do Pickering one better in this regard by being quicker to implicate themselves as practitioners when it comes to their relation to recent economics. That is, in regard to recent economics in particular, historians are already aware that their past practice leaves them somewhat ill prepared to develop new tools of analysis for recent thought, and that it raises questions about their goals and intentions as producers of tools of historical analysis per se. They should thus be quick to grasp the dialectic of resistance and accommodation in economics practice on the terrain of recent thought; should this result in a broad discussion of the

2. For a more systematic application of Pickering’s resistance and accommodation thinking to another of the New Classicals, Neil Sargent, see Sent, forthcoming.
goals and strategies of analysis using the history of economic thought, it could also lead to changes in historians’ practices in connection with past economics.

What is involved, then, in doing a Pickeringian history of recent economics? The basic idea of the mangle is that human and nonhuman agency are intertwined in cyborg-like fashion, such that human practice tunes and is tuned by the machines it builds. What sorts of models/machines would historians of recent economics construct? Other economists build models/machines which only include themselves as noneconomists—that is, as ordinary individuals to whom the models might apply—so that the dialectic of resistance and accommodation in economic practice takes place, as it were, behind their backs. Historians, however, whose subject is the development of economic ideas, build models/machines that comprehend as one unit models and their economist modelers—the economics cyborg—and, indeed, since most historians are also economists, these models also include themselves, albeit at one remove. This difference implies that historians of economics are in a position to understand economic practice, the modeler/model-building process in economics, while the practitioners/model-builders who only reflect upon their machines are predisposed to misunderstand it. Though economists act in and influence the world they study, their models occlude this central fact.

A first implication of this is that just as model-builders misunderstand economic practice, they also misunderstand the nature of their machines—their models. Indeed, though they are aware that tinkering is part of model construction, they see this rather in a Schumpeterian manner as searching for the “right” model. The idea of innocent search for the right model draws on the representationalist rather than performative idiom, and it fails to see how tools are constructed in historical contexts to do specific kinds of things. Further, these right models are thought to correspond to reality in and by themselves, and their construction is then thought a matter of developing “pure” theory, a sort of immaculate conception understanding of what economists produce that makes reference to a model’s author only in the most legendary, heroic, and artificial terms. Historians of thought, it must be admitted, have tended to accept this imagery in their own treatment of the “greats.” But their treatment in the last decades at the hands of their colleagues has surely disabused most historians of the idea that model-builders are dispassionate searchers for truth. Given this, it would appear to follow that a turn to
the history of recent economics from a Pickeringian perspective would make a dethroning of pure theory a key item on historians’ agenda.

Pure theory, we may just begin, is only one “made” thing among many extensions of economics culture: applied studies, policy analyses, proprietary business documents, economics teaching, economic journalism, surveys, think tank reports, peer reviews, statistics gathering, curriculum proposals, and so on and so on. Pure theory, it then might be argued, is hierarchically privileged over these other forms of economic practice in the eyes of its producers’ on the basis of a false causal theory of the effectiveness of pure theory. Formal, abstract relationships—the domain of pure theory—are thought to correspond directly to reality by outlining real world relations, or, more archaically, the eternal Platonic forms. Other, lesser, products of economics practice involve distorting admixtures of social interest, values, and irrelevant information. These more ordinary products may be seen as hybrids, if the complaintant believes “pure” theoretical relations are merely obscured (an epistemological view). Or, more neurotic theorists may hold that economic practice with social content results in bastardized products, perhaps on ancient philosophical-spiritualist grounds that human affective states are incomplete and imperfect by comparison with the fully ideational. In either case, only pure theory may capture the underlying or transcendent real world (depending on the metaphor preferred for this alienation), and only it may consequently provide knowledge of the world’s causal mechanisms.

The simple reply to this false causal picture is that economic practice in all its forms plays a role in extending economics culture. How this multiplicity of extensions links together and to the “outer” science-technology-society organization in which economics culture exists is a complicated matter that requires more than unicausal thinking in social science. Historians of thought, however, because their “made” machine is the modeler/model couple itself, are in a position to pursue this richer sort of investigatation. For them, economists are not disembodied intellects, and thus do not produce pure theory. That historians have largely failed to write this multicausal history in the past seems to be because they have generally restricted their vision to past, no longer ongoing economics. On that terrain it is easy to lose sight of the mangle and think that texts capture the development of economic ideas, whereas on the terrain of recent, ongoing economics the dialectic of resistance and accommodation is there for anyone to see who merely looks. Indeed, were historians to spend some time with the historical complexity of current
ideas, it is reasonable to think they would begin to find that same complexity in past ideas (as some of our more innovative colleagues have indeed begun to do).

A second implication of the difference in orientation between economists and historians concerns how we look upon the topic of appraisal. Roger Backhouse (1992a–d) and Roy Weintraub (1992a, 1992b) recently engaged in an interesting exchange on this subject, with Backhouse arguing that methodological appraisal is inseparable from history of economic thought, and Weintraub arguing that constructivist methodology makes no commitment to any particular view of truth. The exchange is especially valuable because it arose over Weintraub’s Stabilizing Dynamics when Backhouse commented that it was but “a short and defensible step from appraising contemporary economics to appraising past economics” (1992a, 28; also see Hands 1991). That is, Backhouse (whose own history of thought text does treat recent economics) sees an intimate connection between appraising ongoing economics and historiographic methods. Weintraub, on the other hand, believes methodologically or philosophically oriented history of thought is “thin” and typically uninteresting.

I think there are merits in both of these views and that there is a way we might balance them against one another in the framework at hand. Let me first attempt to take stock. In support of Weintraub, two things may be said. First, though there are difficulties in saying what “thick” history involves, methodologically motivated histories often tend in the opposite direction, especially when they simplify away the cross-cutting complexities of the historical process to make un-nuanced judgments. Second, historians of thought do not stand outside of the economics cyborg, the economist/model couple, so they, too, are “tuned” by the model/machine upon which they work, thereby losing an element of privileging that would support appraisal. In support of Backhouse, there are also two points to make. First, even thick history is perspectival and judgment-laden, so interesting history is not necessarily unslanted. Second, though historians of thought are themselves mangled in their production, because they still have the economics cyborg as their modeling object—unlike economists who simply think about their models/machines—they achieve a vantage point that permits better judgments about the whole than their colleagues can make.

I will try to focus these points on the identity of the historian in economics. Weintraub points out that historians, methodologists, and
economists belong to three, sometimes overlapping, interpretive communities which we must be careful not to conflate. Backhouse responds that the overlap between these communities is significant, and that it implies that appraisal is an inevitable part of historical work. What sort of overlap is there between these communities from Pickering’s perspective? Since both historians and methodologists take the economics cyborg as their model, they possess a more comprehensive understanding of economics practice than do economists, who generally think only in terms of their models/machines. This more comprehensive perspective creates a detachment of sorts for historians and methodologists, and thus permits appraising judgments regarding economists’ artificial conception of practice. But historians are also economists, and though this is generally an advantage in understanding the work of economists, it also imposes limits on historians’ detachment, because it tends to restrict historians’ vision to economists’ narrow concerns. To this we should add that on Pickering’s dialectic of resistance and accommodation, just as the goals and intentions of scientists are emergent in their work, so, too, are the goals and intentions of historians. Thus historians in their own work need to consider their judgments as provisional and subject to revision.

The upshot of this, it seems, is that historians have special opportunities for appraisal, but these opportunities are intertwined with specific episodes in the historical development of economic theory. Methodological prescriptivism as was sought in the form of ultimate criteria for theory choice must be given up for critical understanding of ongoing economics practice. This conclusion has been argued before (e.g., Caldwell 1989), but rather in light of shortfalls in the Popperian and Lakatosian frameworks. In contrast, Pickering offers a way to understand how the different communities in which historians operate structure historians’ practical identities and thereby create temporary platforms for the appraisal of economics practice, which are regularly dismantled and reconstructed. On this conception, description and prescription are closely related, and a constructivist history of economics is both thick and thin at the same time.

4. Concluding Remarks

It should be noted that no attempt was made here to investigate two important issues that arise in connection with Pickering’s approach. One is his understanding of pragmatic realism, which he regards as a departure from
scientific realism, but which he develops all too briefly in *The Mangle of Practice*. The other is the concept of truth and how it operates in scientific practice, as understood in performative terms, where the traditional correspondence view either fails or requires significant modification in a more holistic sort of setting. A third issue was introduced—namely, Pickering’s treatment of freedom in terms of the forced and free moves scientists make and, relatedly, the concept of emergence as exhibited in scientific practice. This important topic also needs more careful examination.

Finally, to return to a point made at the outset, historians’ absorption in the “greats” may be related to their isolation in the economics profession. One point I made was that historians magnify the stature of past economists to diminish those of today. But this may only signal that just as Smith, Ricardo, and the others constructed models/machines, so our colleagues may compete to be “great” as well, especially when the awarding of a Nobel prize is ordinarily taken as signaling entry into the pantheon. Presented with this possibility, I would think historians would quickly get to work in dispelling the myth of greatness by explaining that ideas sometimes develop haphazardly in real historical contexts. This could indeed be done on the terrain of recent economic thought, because much of what is involved in the production of ideas is evident to us in this more familiar setting. But were this work to proceed without revision of the history of past thought, the ideology of the “greats” would persist. Thus work needs to be done on both fronts at the same time. My suspicion, nonetheless, is that new history of economic thought on recent economics will have to be done for quite some time before it will be clear that a new history of economic thought is needed for past economics.

**References**


