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CAPITAL CONTROVERSY FROM BÖHM-BAWERK TO BLISS: BADLY POSED OR VERY DEEP QUESTIONS? OR WHAT "WE" CAN LEARN FROM CAPITAL CONTROVERSY EVEN IF YOU DON'T CARE WHO WON

BY

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The author's path from heterodoxy to economic history to the history of economics is used as a case study to explore tensions between "doing economics" and "doing the history of economics," between the ideological vision (Schumpeter) motivating a research agenda and the even-handed execution of research. These same tensions appear in the history of capital controversy, which contains deep questions of history and path dependence versus equilibrium models, limitations of aggregate production functions, and the roles of vision and ideology in the reluctance to abandon insights of one-commodity models when results are not robust.

> *the theory of capital [is] not . . . some quite separate section of economic theory, only tenuously related to the rest, but . . . an extension of equilibrium theory and production theory to take into account the role of time.*
>
> *Bliss (1975, p. 346)*

Those in attendance at the Denver conference will recognize the many differences between the talk I gave and this printed text. The talk, and I use that word deliberately, was not a speech that I read. Instead, it was more like a lecture, organized using PowerPoint slides, introduced by music ("Time After Time," played by the Keith Jarrett Trio), illustrated with photos of economists and incorporating audience participation games of "Who Said It?" What you are reading is a transformation of the talk's message, and some of its spirit, to the printed page. Thanks to Susan Cohen, Evelyn Forget, and Joseph Gladstone for editorial guidance.

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In reviewing Daniele Besomi's editing of Harrod's papers (2003), Roy Weintraub (2005, p. 133) wrote: "They are the saints living among us, these constructors of the historical record: William Barber on Irving Fisher, Donald Moggridge on John Maynard Keynes, Werner Stark on Jeremy Bentham, John Whitaker on Alfred Marshall, Donald Winch on James Mill. To this list we now need to add Daniele Besomi on Sir Roy Harrod."

I want to start by admitting something my wife Susan has long known—I am not a saint. But as a mere mortal in my approach to the history of economics, there are, I believe, useful lessons from my worldly path.

One of the more challenging traditions of the History of Economics Society (HES) is the placement of the Presidential Address immediately before the conference banquet. Here is a chance to weigh in on the state of our discipline, but every pronouncement keeps the audience away from the best meal of the weekend. In accepting the challenge, I want to tell a story of a journey that will hopefully keep your minds off your stomachs, while offering some insights into the ever-present tension between "doing economics" and "doing the history of economics."

Aspiring writers are always advised to "write about what you know." The topic I know is easy for you to guess, as I am a bit of a one-trick pony. When Jerry Evensky and I first met at an HES conference decades ago, he asked the typical question, "What are you talking about?" and I replied, "capital theory." I asked him the same question and he replied, "Adam Smith." For a few years we exchanged these pleasantries, but eventually learned that the question was unnecessary—simply exchanging knowing glances said it all.

I will use my research path—from heterodoxy, to economic history, to the history of economics—as a case study to address the question, "How to do the history of economics?" This question—involving the interplay between the history of economics, economics, heterodoxy, and orthodoxy—arises regularly on the SHOE (Societies for the History of Economics) discussion forum and elsewhere and usually includes Roy Weintraub.[1] What is, and what should be, the role of the history of economics in debates between heterodox and orthodox economists? Coming from a heterodox background and standing here before you as the President of HES, I believe there is something useful in my experience.

History matters, both in my path and in the topic of capital theory controversies. The details of the capital controversies are best left to the papers already in the public record.[2] Instead, my talk tonight emphasizes general themes about what "we" can learn from capital controversy—we as historians and we as economists.

[1]A recent interchanges on SHOE, March 24–26, 2009, involved Steve Kates, Roy Weintraub, Roger Sandilands, and Steve Medema.

[2]Cohen (1984a, 1989, 1993a, 1993b, 1993c, 1998, 2003, 2006, 2008), Cohen and Cohen (1983), Cohen and Drost (1996), Cohen and Harcourt (2003, 2005).

FROM HETERODOXY . . .

This 1972 photograph of an economics student was taken after midnight in the basement of the undergraduate library (UGLI) at the University of Michigan, Ann Arbor.

You can just make out the package of Camel filter cigarettes in the shirt pocket, but cannot see the leather thong with the cowbell around my neck. Upon seeing this picture, my daughter did not comment on the long hair. What she said was "Papa, you once had hair?!"

Enticed by both the orthodox theory and a guest lecture on Paul Baran and Paul Sweezy's (1968) *Monopoly Capital* in an introductory economics course, I became an economics major. The teachers who most influenced me were Dan Fusfeld, Tom Weisskopf, and Robert Stern. I took every course Fusfeld offered in the history of economic thought, Marxian economics, and economic anthropology (Fusfeld was a student of Karl Polanyi's at Columbia University). Ann Arbor was a hotbed for the recently formed (1968) Union for Radical Political Economy (URPE). Tom Weisskopf, one of URPE's founding members, allowed me to take graduate courses in political economy, and supervised my senior honors thesis—"Participation and Power: A Study of the Feasibility of Worker Participation Under Capitalism"—inspired by Steve Marglin's (1974) article "What Do Bosses Do?" Robert Stern ran small yearly seminars for honors majors, where we read and debated more philosophical books like Milton Friedman's (1962) *Capitalism and Freedom*. From these, and other excellent teachers, I developed a respect for theory and evidence—both orthodox and heterodox theories.

Given my interests, I looked for a graduate school with heterodox faculty. Stanford fit the bill, with a field euphemistically called "Alternative Approaches to Economics." Duncan Foley, who was working on Marx's theory of money, also taught part of the core macroeconomic theory sequence. Jack Gurley, a former editor of the *American Economic Review* who had been radicalized by the Viet Nam war, taught Marxian economics. Donald Harris, a protégé of Joan Robinson recruited in response to graduate student pressure for more heterodoxy, was teaching the Cambridge capital controversies from the manuscript of his book, *Capital Accumulation and Income Distribution* (1978). Harris exposed us to Joan Robinson's (1974) critique of "history versus equilibrium." Nate Rosenberg taught the history of economic thought, which counted towards the Alternative Approaches field. David Levine (1978, 1979, 1981), now at the University of Denver, visited for a year, bringing many of his Yale graduate students in tow. As part of the Alternative Approaches field, there was a weekly, well-attended Political Economy Seminar. Phil Mirowski, at the University of Santa Clara after 1979, often participated.

. . . TO ECONOMIC HISTORY . . .

Stanford also had a remarkable collection of economic historians. As part of a field in economic history, I took courses from Paul David, Alex Field, Gavin Wright, and Nate Rosenberg. Rosenberg's interest in technological change resonated with long-standing interests of mine in technology and science. In the then-current economic theory, technological change was treated as a black box, an exogenous variable, or a residual in the Solow model. Endogenous growth theory had not been born.

Following Rosenberg, I found that equilibrium-based models of optimization subject to constraint were not adequate for explaining technological change. Rosenberg passed on to students the inspiration for his work he found among classical economists—especially Smith and Marx—who dealt with competition as a process, not as an end state.[3] I decided to write a dissertation in economic history, based both on interest and on pragmatic job market considerations—it was easier to sell yourself as an economic historian than as an "alternative approaches" economist.[4]

The title and abstract of one of the papers (Cohen 1984b, 1987) from that dissertation, "Technological Change as Historical Process: The Case of the U.S. Pulp and Paper Industry, 1915–1940," reveals much about my intertwining interests in heterodoxy and the history of economics.

> Technological changes in the U.S. pulp and paper industry between 1915 and 1940 are chronicled, and three patterns—evolutionary bias, output-increasing innovation in response to technological disequilibria, and differences in the timing of innovations between the 1920s and 1930s—are identified and explained by means of a theoretical framework for induced innovation. The framework conceptualizes technological change as a means for growth-seeking firms to overcome barriers to accumulation and provides a general explanation of induced innovation that is situated in historical time.

The framework is rooted in Smith and especially Marx, but with influences from Robinson (1974), Penrose (1959), Chandler (1962, 1977), Schumpeter (1943, 1961), and Salter (1966). Firms are the active agents in the process, dynamically attempting to overcome constraints, rather than maximizing subject to constraints. The actions firms take to survive, compete, and grow create an economic structure of production and demand. That structure subsequently faces the firms as constraints on further accumulation. Those constraints focus innovative efforts toward particular forms and locations of technological change. Successful technological change then transforms that structure, and the process continues.

This interplay between agency and structure would have been difficult to model, even if I had wanted to. But I understood back then, even before Craufurd Goodwin's (2000) incisive paper "It's the Homogeneity Stupid!" that without a mathematical model my chances of acceptance among orthodox economists were negligible. In economic history, which was more methodologically pluralist, I was saleable.

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[3]Rosenberg assigned McNulty (1967, 1968). Blaug (2000) provides a useful summary of this perspective.

[4]Charles (2009) discussed the current receptivity of economic historians to the history of economics in a paper at the Denver HES conference.

. . . TO THE HISTORY OF ECONOMICS

After moving to Toronto, I landed a job in 1982 at York University. The job offer, I suspect, had more to do with my ability to teach introductory economics to 500-student sections rather than my research. Having served as teaching assistant at Stanford for Jack Gurley, a master of large lectures, I had learned how to organize, deliver, and enjoy lecturing. Part of what I learned from Gurley as a teacher—to extract a simple story to communicate—also became a goal in my writing.

Serendipitously, York had a pair of semester courses in the history of economic thought, created by William Jaffé—the first HES Distinguished Fellow—who came to York after leaving Northwestern University.[5] This could be called fate, or path dependence. The courses had not been taught since Jaffé's death in 1980. With my colleague Meyer Burstein, we subsequently developed two courses in U.S. economic history. With the opportunity to teach subjects closest to my heart, the York job seemed too good to be true.

The situation at York was even better by the early 1990s. There was a sufficient collection of faculty with history of economics interests—Margaret Schabas, Omar Hamouda, Ian MacDonald, John Smithin—across the university that we started a York History of Economics workshop.

Sam Hollander, who had been simultaneously running his PhD student workshop at the University of Toronto with students like Sandra Peart, Evelyn Forget, and Rick Kleer, was eventually persuaded to combine forces with us. Together, we created the Joint York/Toronto Workshop in the History of Economics. That workshop, where many of you have presented, continues to this day, organized by Don Moggridge with regulars including Susan Howson, Allan Hynes, David Laidler, and Bob Dimand.

Teaching the history of economics—one course covering up to 1870, the other beyond—helped move my research focus from economic history to the history of economics. After mining the history of economics for insights into problems I was tackling as an economist—technological change, competition, growth—I began following the literature as a teacher of the history of economics. The journals were rife with methodological debates about Popper, Kuhn, Lakatos, and the question of continuity or discontinuity in the development of economics as a discipline. I structured the two courses as a debate over the dis/continuity issue, highlighting differences in approaches and encouraging students to develop their own informed assessments and conclusions. Those teaching and methodological interests lead to my first attempts at "doing the history of economics." Harvey Walsh and Vivian Gram's wonderful 1980 book, *Classical and Neoclassical Theories of General Equilibrium*, served as a text in my courses. Jon Cohen (an economic historian) and I wrote an extensive review of it (Cohen and Cohen 1983). This was followed by a methodological response to Sheila Dow (1980), combining my graduate school education in the Cambridge capital controversies with a new-found interest in the methodology of economics literature (Cohen 1984a).[6]

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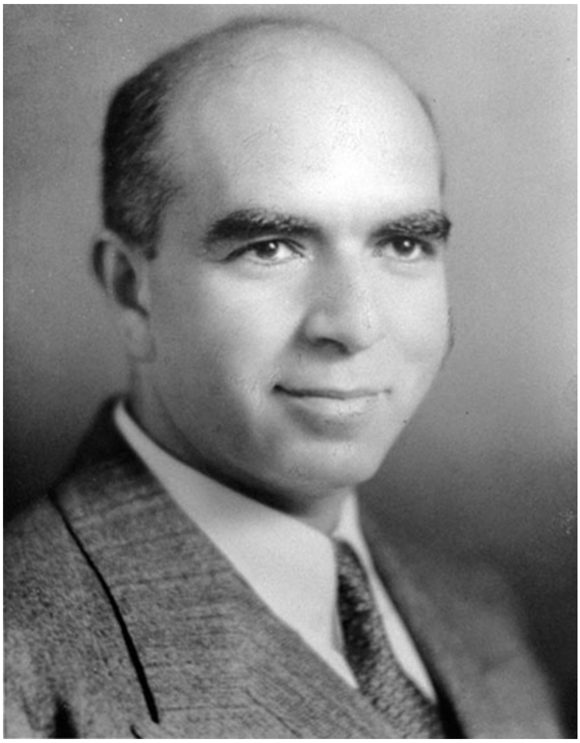
[5] William Jaffe's papers are in the Clara Thomas Archives & Special Collections, York University Libraries. The fonds list is online at http://archivesfa.library.yorku.ca/fonds/ON00370-f0000333.htm.

[6] Phil Mirowski was also an important influence drawing me into the history of economics. As Rizvi (2001) documents, Mirowski's path started similarly, from economic history (and the University of Michigan) to the history of economics.



William Jaffé at the age of 38 (1936) York University Libraries, Clara Thomas Archives & Special Collections, William Jaffé fonds, F0333, image no. ASC05329.

RESEARCH AGENDA

In returning in the 1980s to the waning economics literature on the Cambridge capital controversies, I was struck by scattered references to previous capital controversies. Solow's (1963, p. 9) comment is typical:

> A more learned reader of the literature than I could probably show that the capital theory pot has been simmering steadily every since Ricardo's chapter on Machinery. At intervals it has boiled over on a heroic scale: in the polemics between Böhm-Bawerk and J. B. Clark in the 1890's, between Hayek and Knight—going over much the same ground—in the 1920's and 1930's, and between Mrs. Joan Robinson and almost everyone else outside of Cambridge, England in the present.

The controversies occurred in three historical clusters: at the turn of the twentieth century between Eugene von Böhm-Bawerk, John Bates Clark, Irving Fisher, and Thorstein Veblen; in the 1930s between Frank Knight, Freidrich von Hayek, and Nicholas Kaldor; and in the 1950s–1970s between Piero Sraffa, Joan Robinson, Luigi Pasinetti, and Pierangelo Garegnani on the Cambridge UK side, and Paul Samuelson, Robert Solow, Frank Hahn, and Christopher Bliss on the Cambridge, Massachusetts side.

Looking back over these clusters, Solow (1963, p. 10) made another characteristically pithy comment that became my research agenda: ‘‘when a theoretical question remains debatable after 80 years there is a presumption that the question is badly posed—or very deep indeed.’’

Solow’s orthodox assessment was that the question was badly posed, that capital controversy was a tempest in a teapot over minor anomalies involving the measurement of capital in aggregate production function models. I started with a hunch, or a vision, that the question or questions behind capital controversy were very deep indeed. According to Schumpeter (1954, p. 42):

> Analytical work begins with material provided by our vision of things, and this vision is ideological almost by definition. It embodies the picture of things as we see them, and wherever there is any possible motive for wishing to see them in a given rather than another light, the way in which we see things can hardly be distinguished from the way in which we wish to see them.

This hunch created a research agenda. The first task was to chronicle the individual controversies and fill in the historical record. Not much had been written about most of the pre-Cambridge capital controversies (for reasons I would discover). The research needed to establish similarities and difference among the controversies. If there were enough similarities, the recurrence of controversy would serve as evidence of deep issues that we, as economists, need to take seriously. Those issues, I believed, would include the limitations to analytical, equilibrium-based tools of optimization subject to constraint. If equilibrium-based models continuously generated controversy because of their inability to address the deep issues of capital theory, that would provide an argument for methodological pluralism, for admitting other tools of historians and tools rooted in what I saw as neglected insights of the ‘‘classical’’ (versus ‘‘neoclassical’’) approach to doing economics.

In the title of this talk, the purposeful use of the singular ‘‘controversy’’ reflects that belief, now buttressed with evidence, that there is a commonality to the recurring clusters of capital controversy. Böhm-Bawerk’s and Bliss’s names in the title reflect the beginning and end points of the clusters. But those authors also have disproportional importance. Böhm-Bawerk, the key pioneer of modern capital theory, was also the heavyweight of capital controversy, taking on all contenders, dead and alive, including Marx. Bliss’s 1975 book is widely recognized as the end point of the most recent cluster of controversy. And Bliss’s partisan stance on the orthodox side contrasts with my partisan start to this journey on the heterodox Cambridge UK side. This allowed for powerful focal points around issues where we agreed.[7] Described differently, my research agenda was to attempt to become Solow’s ‘‘learned reader.’’

[7]To see the vast distance between our perspective on capital theory, read the contrasting introduction to Bliss, Cohen, and Harcourt (2005) by Bliss (2005) and by Cohen and Harcourt (2005).

DOING THE HISTORY OF ECONOMICS

There is an unavoidable and delicate dance between the ideological vision that motivates a research agenda, and the even-handed execution of the research itself. How do we respond to Schumpeter's challenge and find what is out there rather than what we are looking for?

This is where the partisan economist—orthodox or heterodox—and the historian part company. While economists and historians are all motivated to choose topics because of personal interests and hunches, in donning the historian's hat we must be on guard not always to find, or construct, what we are looking for. Evidence is crucial—textual and contextual—from correspondence, events, and personalities. We are required to be sensitive to historical context, differences between periods, and especially to surprises to our expectations. Too often economists make whiggish use of the history of economics, using the history of economics as pedigree to buttress claims that a particular theory—orthodox or heterodox—is right or wrong.

In investigating these controversies I tried to follow the guidance Duncan Foley provided to his students. He identified three components of a thorough reading of an article. First, read the article sympathetically to understand what the author is arguing. Second, perform an internal critique. Are the outcomes consistent with the assumptions? Is the internal logic correct? Third, perform an external critique. What assumptions are crucial for the argument to go through? What happens if they are modified? Is there an entirely different "alternative approach" to the problem that is more compelling?

Reading the interchanges between authors in the earlier clusters of controversy, I quickly discovered one reason why there had been little work done on these episodes in the history of economics. They were thickets of torturous prose, from which it was a challenge to extract core arguments. Mathematical models are concise, clearly articulating the logic of arguments and interconnections between variables. This is not the case in reading Böhm-Bawerk or Knight. I was boldly (or foolishly) going where almost no one had gone before.

While supplementing a close reading of the original texts with archival work on correspondence, the history I have produced is not the thickest history you will read—one of the reasons I do not qualify as a saint. Given the texts involved, I would describe "the literature as archive." The work associated with the texts themselves was very much like archival work. For each author then, my task was to extract the core narrative from the jumbled profusion of words on the pages and to reconstruct his/her argument sympathetically, connect the debates between authors, and finally identify linkages and differences both within a cluster and across clusters.

Since my peers have refereed and published the work, this approach must be acceptable, both in terms of the historical evidence and the balancing of motivation and execution of research. But the delicate dance around Schumpeter's vision remains an ever-present challenge for the historian of economics.

So what did I find? And what can "we" learn from those findings—we as historians and we as economists—even if you don't care about capital theory?

COMMON CAPITAL CONTROVERSY ISSUES

Economists conceive of capital both as heterogeneous physical equipment used in production, and as a homogeneous fund of financial value flowing among alternative uses to establish a uniform rate of return. All capital controversy originates in the tension between these physical and value conceptions of capital. While economists agree that capital has both physical and value conceptions, controversies arise when the dual conceptions of capital are integrated into economic models and one conception is emphasized to the relative neglect of the other.

Across the clusters of capital controversy, this tension generates three recurring issues: (1) explaining factor price as a scarcity index, (2) methodological issues integrating capital and time into equilibrium models, and (3) the role of vision/ideology in fuelling controversy when results of simple models are not robust.

FACTOR PRICE AS A SCARCITY INDEX

In the pure exchange models of Jevons, Menger, and Walras, price is an index of relative scarcity. Can this conception of price be extended to explain factor prices in models with production and time? The extension is not obvious, since in what sense are commodities scarce if they can be produced (Bliss 1975, pp. 3–4)?

Neoclassical capital theory was the arena for extending the principle of relative scarcity to explain all prices, including factor prices. If factors of production are themselves scarce, the extension works clearly in a one-commodity Samuelson/Solow/Swan aggregate production function model with diminishing marginal productivity and constant returns to scale. The one produced good (Q) can be consumed directly or stockpiled for use as a capital good (K) in conjunction with labor (L).

$$Q = f(K, L)$$

This simple model exhibits what Samuelson (1962) calls three key "parables:"

Parable 1 The real return on capital (the rate of interest) is determined technically by the diminishing marginal productivity of capital.

Parable 2 There is an inverse, monotonic relation between quantity of capital and the rate of interest.[8]

Parable 3 The distribution of income is determined by relative factor scarcities and marginal products.

The price of capital services (the rate of interest) is determined by the relative scarcity and marginal productivity of aggregate capital, and the price of labor services (the wage rate) is determined by the relative scarcity and marginal productivity of labor. These parables originated with J. B. Clark (1891, p. 312), who expressed them in unidirectional, causal language: "as capital increases, while other things remained

[8]Although omitted here for simplicity, Parable 2 also includes inverse, monotonic relations between the rate of interest and capital-output ratio as well as the sustainable levels of consumption per head.

unchanged, interest falls, and, as the labor force increases, if other things remain the same, wages fall." The parables were an immediate source of controversy between Clark and Veblen. Aggregate production functions did not appear until the 1930s debates, introduced by Kaldor (1937).

In more general models, the parables run into problems because the quantity of heterogeneous capital goods cannot be aggregated or measured in physical units. Wicksell pointed this out in 1911.

> Whereas labour and land are measured each in terms of its own *technical* unit (e.g. working days or months, acre per annum) capital ... is reckoned ... as a sum of *exchange value*—whether in money or as an average of products. In other words, each particular capital-good is measured by a unit extraneous to itself. [This] is a theoretical anomaly which disturbs the correspondence which would otherwise exist between all the factors of production. The productive contribution of a piece of technical capital, such as a steam engine, is determined not by its cost but by the horse-power which it develops, and by the excess or scarcity of similar machines. If capital were to be measured in technical units, the defect would be remedied and the correspondence would be complete. But, in that case, productive capital would have to be distributed into as many categories as there are kinds of tools, machinery, and materials, etc., and a unified treatment of the role of capital in production would be impossible. Even then we should only know the *yield* of the various objects at a particular moment, but nothing at all about the value of the goods themselves, which it is necessary to know in order to calculate the rate of interest, which in equilibrium is the same on all capital (Wicksell 1911 [1934], p. 149; emphasis in original).

This is the tension between the physical and financial conceptions of capital. Heterogeneous physical capital goods must be aggregated in financial terms to determine the uniform rate of interest. But the problem is as much about *time* as it is about aggregation. The value of capital goods can be measured either as the cost of production, which takes time, or the present value of the future output stream they produce. Since either measure involves time, it presumes a rate of interest—which, in the simple one-commodity model, is determined unidirectionally by the quantity of capital. The new interdependence causes Wicksell effects—changes in the value of the capital stock associated with different interest rates. Wicksell effects arise from either inventory revaluations of the same physical stock due to new capital goods prices (price Wicksell effects) or differences in the physical stock of capital goods (real Wicksell effects).

In the Cambridge controversies, the problems created for the neoclassical parables by Wicksell effects were termed reswitching and capital-reversing. Reswitching occurs when the same technique—a particular physical capital/labor ratio—is preferred at two or more rates of interest while other techniques are preferred at intermediate rates. The same physical technique is associated with two different interest rates, violating parables 1 and 2. Despite determined attempts in the 1930s, neither Hayek, Knight, nor Kaldor could sustain the inverse, monotonic relation between capital intensity and the interest rate in heterogeneous commodity models.[9]

[9]Kaldor (1937, p. 230), while accepting that Wicksell effects preclude a unique measure of capital, maintains that "real productivity, and thus the real rate of return on any resource will depend on the relative scarcity of the services of that resource."

With capital-reversing, a lower capital/labor ratio is associated with a lower interest rate. In comparing two steady-state equilibrium positions, it is as though capital services have a lower price when capital is "more scarce." Capital-reversing implies that the demand curve for capital is not always downward sloping, violating parables 2 and 3. Explanations of price as an index of scarcity could not be consistently extended to factor prices in models with production and time.

Samuelson (1966b, p. 568) admitted that outside of one-commodity models, reswitching and capital-reversing may be usual, rather than anomalous, theoretical results and that the three neo-classical parables "cannot be universally valid." Consequently, aggregate production function ended up in the doghouse, falling out of favor in the 1970s and early 1980s. Hahn (1972, p. 8) dismissed aggregate production functions, which "cannot be shown to follow from proper [general equilibrium] theory and in general [are] therefore open to severe logical objections."

Besides the fallout from the external Cambridge UK critique, neoclassical aggregate production functions were also being subjected to an internal critique by Phelps Brown (1957), Franklin Fisher (1971, 1992), and Herbert Simon. In his Nobel lecture, Simon (1979, p. 497) argued that the good statistical fits to the Cobb-Douglas production function "cannot be taken as strong evidence for the [neo]-classical theory, for the identical results can be readily produced by mistakenly fitting a Cobb-Douglas function to data that were in fact generated by a linear accounting identity (value of output equals labour costs plus capital costs)." Why aggregate production functions have been resurrected with endogenous growth and real business cycle models is a topic waiting for historians of econometrics to explain.[10]

CAPITAL, TIME, AND EQUILIBRIUM

Economists typically use comparative statics or comparative dynamics to explain events occurring over time. Do comparisons of equilibrium positions capture, or obscure, key features of economic activity over time? Authors in all clusters of capital controversy struggled with this question and the roles of path dependence and history.

To generalize about those struggles, it is necessary to set aside the shifting meanings of the terms "equilibrium," "statics," "dynamics," and "stability" over a century of capital controversy.[11] Bliss's (1975, p. 27) definition of equilibrium, which is consistent with most of the authors' usage of the term, is a helpful focal point: an actual outcome "which would be expected to be realized, because the dynamic forces which operate ... bring the economy to an equilibrium."[12]

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[10]Other external and internal contributors to the critique include Shaikh (1974, 1980), Felipe and Fisher (2003), and Felipe and McCombie (2001).

[11]See Weintraub (1991, 2002).

[12]This history could be "thickened" by taking different methodological perspectives on the changes in the concept of equilibrium occurring over the period. Sraffians emphasize the transition from long-period to short-period equilibrium in the post-WWII era. Because I do not find that transition central to explaining the cluster of three controversies, it does not play a central role in my story. See Petri (2007) for the Sraffian critique and the reply by Cohen and Harcourt (2008). In contrast to Petri, I believe that adding the long/short period distinction will make for a richer, but not fundamentally different, story.

Consider the typical choice a firm makes between two production techniques, *alpha* and *beta*, where *beta* has a higher capital/labor ratio. Before factory *alpha* is built, the quantity of capital—whether measured as a dollar amount of investment, the cost of production, or the expected net present value of the factory—has the same value. The three measures of capital for factory *beta* are also equal, but at a higher capital/labor ratio. Based on prevailing factor prices, suppose the firm builds factory *alpha*. But—and this is where the problems begin—what if the relative price of capital services (the interest rate) unexpectedly falls?

In the conventional comparative statics story, the firm substitutes factors to minimize costs. The fall in the interest rate implies that the firm with factory *alpha* will move to the more capital-intensive factory *beta*. But in comparing techniques *alpha* and *beta*, is the increase in the quantity of capital simply an increase in the net present value of the existing capital goods (price Wicksell effects due to the lower interest rate), or is it an increase in physical capital goods (real Wicksell effects)? Furthermore, will the firm actually switch to factory *beta*? After all, that is the factory the firm would have built from scratch if factor prices had been, and were expected to be, at the new ratio. Will a firm with the installed equipment of factory *alpha* necessarily move to the technique of factory *beta*? Does the existence of installed, specific physical capital goods create path dependence affecting the firm's decisions? And what about the stability of the existing or new equilibrium positions? Once the firm is out of equilibrium, to what technique does it return, and what are the disequilibrium dynamics?[13,14]

Many debates in the clusters of capital controversy were over the role of history and path dependence in influencing outcomes, and the adequacy of equilibrium as a method for analyzing such sequences. Böhm-Bawerk, in arguing against J. B. Clark's concepts of financial capital and static equilibrium states, claimed that out of equilibrium, "in a dynamic economy ... where concrete capital goods are ... changing" (1895, p. 127), then "the whole subject of the transfer of capital must be studied with reference to capital-goods" (1906, p. 18). Fisher's (1930, p. 484, fn. 39) emphasis on financial aspects of capital led him to reject Böhm-Bawerk's quest for one-directional "causal" explanations, stating that "the advance of all science has required the abandonment of such simplified conceptions of causal relationship for the more realistic conception of equilibrium."

In the 1930s cluster, both Knight and Hayek express leanings towards history over equilibrium. Although Knight never relinquishes his equilibrium-based Crusonia Plant model, he also claims, "long-run, historical changes must be faced as problems of historical causality and treated in terms of concepts very different from those of given supply and demand functions and a tendency toward equilibrium under given

[13]Rosenberg (1976, p. 63) argues that firms know little about factor substitution possibilities far removed from the current technique: "Since ... the production of knowledge is itself usually a costly activity, why should technological alternatives representing factor combinations far from those justified by present prices be known?" Rising factor prices can induce a search for innovations rather than factor substitution, and the technological parameters of existing techniques guide innovative efforts. History matters.

[14]In an excellent paper with intriguing parallels to the capital controversy literature, Hands (forthcoming) explores the role of path dependence in the history of consumer theory in the form of endowment effects and the reference dependencies of behavioral economics.

conditions" (Knight 1931, p. 210). Hayek (1934, p. 227) states that "once unforeseen changes occur after capital has been invested in a definite form, all further investment will be influenced by the historical accident of the existence of certain capital goods, and the movement towards a state of equilibrium will at best be an asymptotic movement." Kaldor (1938, p. 164) argued against comparative statics and for "a process of change."

Do comparisons of equilibrium positions *capture* the essential forces at work underlying economic activity while stripping away less important complications, or do such comparisons *obscure* historically specific adjustments? The answer depends on what aspects of economic activity the author is attempting to explain. Author's attitudes towards equilibrium models are often correlated with their conception of capital. Authors emphasizing the financial conception of capital as a homogenous fund (Clark, Fisher, Knight, Samuelson, Solow) find equilibrium models useful. Authors emphasizing the physical conception of heterogeneous capital goods (Böhm-Bawerk, Veblen, Hayek, Kaldor, Robinson, Garegnani, Pasinetti, Sraffa) find equilibrium models to be inadequate. (This latter group would also include economic historians interested in technological change!)

VISION/IDEOLOGY FUELING CONTROVERSY WHEN RESULTS OF SIMPLE MODELS NOT ROBUST

The essential tension generating most capital controversy, between the physical and financial conceptions of capital, can be dissolved by limiting a model to one commodity. Almost every author in the three clusters of capital controversy gets clear, internally consistent demonstrations of their positions this way. Böhm-Bawerk, Hayek, and Kaldor get Austrian capital theory results from their one-commodity models; Clark, Solow, Swan, and Samuelson get the standard neoclassical parable results; Knight's Crusonia plant model justifies his unique perspective; and Sraffa's reconstruction of Ricardo's corn model validates the classical alternative to the conception of price as a scarcity index—price as an index of the difficulty of production. All of these models either assume one commodity, or make other assumptions that eliminate the complications of physically heterogeneous commodities—"putty" capital, which can instantaneously and costlessly change physical form, or equal factor proportions in all industries which eliminate Wicksell effects.

The power of the one-commodity/putty capital/equal factor proportions assumptions lies in merging the physical and financial aspects of capital, thereby *eliminating the effects of interdependence, history, and time*. Without physical heterogeneity, capital goods can flow among alternative factor substitution possibilities like financial capital, equalizing rates of return and eliminating any path dependence measurement anomalies.[15]

[15]Salter (1965, p. 268) describes the power of one–commodity/putty capital/equal factor proportions assumptions: "one consequence of the assumptions of fluid capital and instantaneous adjustment is that they prevent analysis of the actual time path of an economy . . . the assumption of fluid capital effectively cuts off an economy from its own past history. At each point of time, the economy is assumed to start off, as it were, with a clean slate independent of its past history and techniques."

But all authors encounter problems attempting to sustain their results in more general equilibrium models with heterogeneous commodities. The tensions between the physical and financial conceptions of capital emerge as Wicksell effects, complicating the neoclassical parable results. This happens to authors in all clusters, denying anyone a knockout debating blow and contributing to the lack of resolution at the end of each cluster.

In the most recent cluster of the Cambridge capital controversy, Bliss (1975, p. 85) concludes that "there is no support from the theory of general equilibrium for the proposition that an input to production will be cheaper in an economy where more of it is available." Sraffians got the same result (Schefold 2000). There are problems extending the conception of price as an index of relative scarcity to explain factor prices in models with production and time.

There are also problems sustaining the conception of equilibrium as the outcome of a dynamic economic process. In the first cluster, Veblen criticizes Clark's static equilibrium as inadequate for modeling an evolutionary conception of a cumulative causal sequence. Clark recognizes, but cannot deal with, dynamic complications to his static state explanations. Böhm-Bawerk and Fisher argue over whether the interest rate can be viewed as the equilibrium outcome of a simultaneous equations model, or if an additional, "causal" explanation is necessary. In the 1930s cluster, Hayek, Knight, and Kaldor all wrestled, ultimately unsuccessfully, with problems of history and time. And in the most recent cluster, the Sonnenschein-Mantel-Debreu stability results undermine belief in the stability of general equilibrium outcomes. In discussing these results, Hahn (1984, p. 53) writes: "[T]he Arrow-Debreu construction . . . must relinquish the claim of providing necessary descriptions of terminal states of economic processes." The lack of adequate stability results raises questions about the conception of equilibrium as the end of an economic process and the adequacy of comparative statics as explanations of economic activity over time (Fisher 1989; Ingrao and Israel 1990).

Despite the complications that arise in more general models, all authors refuse to relinquish the intuition of the clear results of their simpler models. The endemic problem of the "significance" of the complications underlies debates in all clusters. What is the meaning of a simple model whose clear-cut results are not sustained when restrictive assumptions are loosened? Is it nonetheless a valuable parable, useful heuristically and empirically to isolate crucial tendencies that get obscured in more general models? Or is it a mistake whose insights must be discarded while searching for completely different explanation?

With conclusive results in simple models and complications in more general models, the authors stick to their respective simple-models theories. The role of vision and ideology enters in maintaining faith in the underlying theory. Recall Schumpeter's (1954, p. 42) words: "vision . . . embodies the picture of things as we see them, and wherever there is any possible motive for wishing to see them in a given rather than another light, the way in which we see things can hardly be distinguished from the way in which we wish to see them."

Three of the more notable expressions of faith in an underlying vision, the belief that the world works "as if" it were described by the simple models, appear in Clark, Samuelson, and Bliss. Clark's (1899, p. 442) magnum opus, *The Distribution of Wealth*, ends with this claim: "Yet, whatever movements the dynamic division of economic science may discover and explain, static laws will never cease to be dominant. All real knowledge of the laws of movement depends upon an adequate knowledge of the laws of rest." Clark makes this claim without ever making a serious foray into economic dynamics.

Regarding the complications of Wicksell effects for the extension of the scarcity theory of price to explain factor prices in models with production and time, Samuelson concludes:

> Until the laws of thermodynamics are repealed, I shall continue to relate output to inputs—i.e. to believe in production functions. Until factors cease to have their rewards determined by bidding in quasi-competitive markets, I shall adhere to (generalized) neoclassical approximations in which relative factor supplies are important in explaining their market remunerations. ... A many-sectored neoclassical model with heterogeneous capital goods and somewhat limited factor substitutions can fail to have some of the simple properties of the idealized J. B. Clark neoclassical model. Recognizing these complications does not justify nihilism or refuge in theories that neglect short-term microeconomic pricing (Samuelson 1966a, pp. 444-5).

And confronting problems with disequilibrium dynamics in more complex models, Bliss nicely characterizes the resort to faith.

> [I]t may seem more sensible to simply assume equilibrium will prevail. ... We could attempt to justify this procedure as a useful starting point to what one might eventually hope to see realized in a complete account of the behaviour of the economy, including ... its disequilibrium dynamics. This approach ... may ... be more attractive, if only because more tractable, than the Herculean programme of constructing a complete theory of the behaviour of the economy out of equilibrium (Bliss 1975, p. 28).

There are two main "motives" for maintaining faith in a vision. One is a methodological "determination to ignore logical anomalies in a theory until they are shown to be empirically important" when no better rival theory is available (Blaug 1975, pp. 42-3). This is a corollary to the Duhen-Quine thesis. Why would an author, in the face of disconfirming evidence, first question the primary links in a chain of reasoning that must be tested jointly? Suspicion first focuses on empirical data, testing techniques, and other secondary links before the author contemplates jettisoning a primary theory or underlying vision. Don't throw out the baby with the bathwater.

The other motive for faith is the ideological commitment to the vision. Capital theory has always been a normatively charged subject, involving a justification of the returns to capital and capitalists. Marx's (and Henry George's) critiques of capitalism inform the first cluster of controversy between Böhm-Bawerk, Clark, Veblen, and Fisher. Normative overtones fuel the emotional energy surrounding seemingly arcane technical debates about measuring Böhm-Bawerk's periods of production, Hayek's investment function, the role of simultaneous equations, or the fine points of stability analysis. While these ideological issues were rarely debated explicitly in the later clusters (Robinson (1970) is the exception), the intensity and passion surrounding later capital theory debates, and "strongly ideological overtones which attach to seemingly technical debates ... must be unintelligible" (Bliss 1975, p. 347) without understanding the impetus (both con and pro) from the original Marxian vision of how capitalism works. As a historian, these expressions of faith and ideological overtones are phenomena to be explained, not judged.

WHAT WE CAN LEARN AS HISTORIANS (EVEN IF YOU DON'T CARE ABOUT CAPITAL THEORY)

Setting aside capital theory (I can hear your collective sigh of relief), what lessons emerge from using my research path as a case study into "how to do the history of economics?" The lessons are not about identifying "right" or "wrong" ideas in the history of economics, but about issues of motivation, execution, and "hats" confronting historians. My research path began in heterodoxy, and my interest in capital controversy was initially partisan, taking the Cambridge UK "side." Having a partisan interest in a topic is not unique, and does not disqualify a scholar from being a historian of economics. The *sine qua non* of the historian is to gather, organize, and respect evidence in executing a research agenda.

In a previous HES presidential address, Donald Walker (1988, pp. 107-108) described the delicate dance around the influence of Schumpeterian vision on the historian: "We should cherish our normative views and motivations. Our biases, as Jaffé said, are vitalizing. Let them energize our scholarly activities. . . . Let us also . . . seek out our prejudices, our social and moral norms, and strive to diminish their influence upon our interpretations of the history of economic thought."

There was a symbolic end point to my own research path at the 2009 AEA meetings in San Francisco. In an URPE panel on "Capital Controversy Revisited," Bliss presented for the neoclassicals, Garegnani for the Cambridge UK side, with Heinz Kurz and me commenting. In the Garegnani paper, I was drawn to discuss the historical accuracy of his claims about Hicks's 1930s reformulation of the concept of capital as vector and the adoption of temporary equilibria, rather than the criticisms flying between "sides." A journey that began in the basement of an Ann Arbor library came full circle, with the long hair of heterodoxy replaced by a historian's hat.[16]

WHAT WE CAN LEARN AS ECONOMISTS (EVEN IF YOU DON'T CARE ABOUT CAPITAL THEORY)

This case study from the history of economics also has lessons, I would suggest, for economists. In an inspiring HES presidential address, Karen Vaughn (1993, p. 178) justified the study of the history of economics by saying:

> We need to say straight out that the history of economics is "useful" not because it helps students to sharpen theoretical skills or because it gives them a little interdisciplinary breadth, but because it can affect the understanding of economics itself, its potential accomplishment and its important limitations.

The history of capital controversy enhances our understanding, as economists, of the limitations of analytical tools. There are three prime examples. The first is the

[16]Backhouse (2004, p. 328) provides this description of the role of the historian: "The history that is written ceases to be either conservative (celebrating the achievements of modern economics) or revolutionary (revealing its fatal errors in order to overthrow contemporary orthodoxy). It serves to provide economists with a vision of where their own work fits into a wider story."

limitations of aggregate production functions, with their lack of sound micro-foundations, for explaining factor prices and for applied econometric work. Aggregate production functions have strengths, but also weaknesses. Second is the limitations of equilibrium tools for explaining economic activity where history and path dependence matter. And third is the limitations of simple models for settling complex controversies. Such controversies exist around continuity or discontinuity in the history of economics, and around differences among underlying visions of schools of economic thought—whether they be orthodox and heterodox, or real business cycle and new Keynesian varieties of macroeconomic theory.

Those who understand these limitations *may* make more nuanced and effective use of the relevant tools, which hold value when appropriately applied. The word *may* is chosen purposefully—I am not Pollyannaish in my expectation. The way in which tools are chosen and applied in economics is itself a complex historical process, the subject of analysis of the present history and sociology of science literature, tracing some of its roots back to the methodology literature emanating from Kuhn, Popper, and Lakatos.

Lessons about what "we" can learn as economists from the history of economics are hidden in one final presidential address. In reading Sir Roy Harrod's (1938, pp. 384-5) address to Section F of the British Association, delivered in Cambridge, on "Scope and Method of Economics," substitute "historian of economics" for "methodologist."

> the methodologist is bound to occupy the rear, and not the vanguard. . . . On first glance this relegation of the methodologist to the rear might seem to give public endorsement to what has all the time been the inward suspicion of the pioneer that he is an utterly useless being. But in fact by reducing his claims he at once becomes much more useful. The forward worker is inevitably influenced by methods used in the past; methods that have already achieved good results may be expected to achieve more; tools ready to hand are taken up. By going over the old ground and making a stricter survey, the methodologist may considerably modify this influence of the past upon the present . . . he may show that there are certain limitations . . . to the productiveness of a given method . . . [that] may alter the forward worker's sense of proportion and the reliance he implicitly places on certain tools. They may give him a greater understanding of the nature of past achievements, and so insensibly influence him in his gropings towards fresh discovery. To do this is very different from trying to lay down the lines on which he *ought* to work.

A SUMMING UP

With a return to Harrod, the journey you have taken with me comes full circle. I hoped to convince you that the history of capital controversy contains very deep questions of history, equilibrium models, theoretical tools, vision, and ideology, both for economists and for the motivation and execution of the research agenda of any historian of economics. These deep questions emerge only with the study of history—history matters. And with this telling of the historical tales behind us, my most saint-like act is to release you all to the banquet. Thank you.

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