

CONSTRUCTING IDENTITY: THE POST KEYNESIANS AND THE CAPITAL CONTROVERSIES

BY
TIAGO MATA

I. IDENTITY AND THE WRITING OF HISTORY

Economics in the 1960s was host to a number of dissenting movements challenging the profession's mainstream theories. As this mainstream changed in the 1970s, the dissenters also underwent a transformation of their own. By the late 1970s the dispersed dissenting voices had congregated to form groups of neo-Austrians, post-Keynesians, neo-Marxists and radical economists. Retrospectively, the 1970s appear as a period of intense negotiation among dissenters as they erected theoretical and methodological boundaries and institutions (associations, journals, seminars) that would come to define them. They were constructing not just conditions for carrying on their work but also a narrative perception of who they were, what they stood for and what was the nature of the profession they inhabited, which I hereafter call "identity" or "self-image." The dispersed critiques were being redrawn into new sociological unities inside the profession. This paper aims to track *one* of the routes that brought dispersed critique into an organized and self-conscious grouping, self-identified as Post Keynesian economics.¹ The broad question addressed is how did the Post Keynesians construct their identity?

Department of Economic History, London School of Economics and Political Science, Houghton Street, London, WC2A 2AE, United Kingdom. This research has been supported by a doctoral scholarship awarded by the Fundação para a Ciência e Tecnologia, Lisbon, Portugal. I have benefited from access to the Joan Robinson papers at the King's College Library, Cambridge, to the Mark Perlman and Sidney Weintraub papers at the Rare Book, Manuscript, and Special Collections Library, Duke University, and from interviews with Mark Perlman, Geoffrey Harcourt, Radhika Balakrishna, and Fred Lee. I would like to thank the participants of the conference session, "Dissent and Controversy in Post WWII Economics" at the 2003 History of Economics meetings at Duke University for their helpful comments. I am particularly grateful to Mary Morgan, Roger Backhouse, Cristel de Rouvray, Fred Lee, Avi Cohen, and three anonymous referees for their careful readings and comments. ¹Though I focus on the capital theory debates, there are other routes that may help trace the whys of the emergence of Post Keynesian economics. One such route is the collective protest brought by dissenters to the editorial board of the *American Economic Review* (see Lee 2000b). Another route is the alliance between dissenters from both sides of the Atlantic—American and British. Post Keynesian economics may be portrayed as the product of a conflation of forces.

My claim is that the episode known as the “Cambridge Capital Controversies” of the 1960s was critical. It drew a demarcation between two opposing segments of the profession. In the process, the Cambridge,² England, side created a new intellectual space within which various dissenting perspectives could converge. Furthermore, I note that the dissenting space was not perceived directly through the controversies, it was expressed by the surveys and histories that accompanied the later stages of the debate. The emergence of the Post Keynesians is in fact not so much a product of the capital debate but of the historical accounts that strove to make sense of the debate as it waned into a stalemate. It was the device of historical narrative that provided the group with an identity.

The materials of the debate are revisited not for their theoretical insights but for their sociological ones. So it is not my task to judge the content of the debate that came to be known as Cambridge Capital Controversies. I am not looking to extract theoretical lessons. The actual intellectual content is only retold, briefly, to provide the context for a historical and sociological argument.

The next section of the paper provides a brief guide to the debate highlighting its main moments and participants. The subsequent section discusses the surveys written on the debate, which proved to be the main source through which the debate was read. I wish to show how a newly defined space was constructed and I trace this re-drawing to the surveys and histories of the capital debate. The work of Geoffrey C. Harcourt takes center stage here, and I highlight the rhetorical strength of his writings for the definition of a new economics, a Post Keynesian one.

Section four discusses the importance of the theoretical demarcation put forward in these surveys and histories, for the emergence and further expansion of the Post Keynesian group. In section five, I suggest that the early sketch of an identity proved effective, in the political context of the 1970s, to coalesce the efforts of several authors under the flag of Post Keynesian economics. The “controversies” however were not just a convenient tool for the group’s emergence: through telling and retelling they became also a powerful instrument for its reproduction. I allude to this in section six. As early Post Keynesians narrated the controversies to their students, it framed the latter’s theoretical views and of their perception of the profession.

I conclude by discussing the importance of the history of economics for the Post Keynesian group in light of the roles it plays for the group’s identity.

II. A BRIEF GUIDE TO THE 1960S CAPITAL CONTROVERSIES

In 1961 Joan Robinson visited the U.S. She was at MIT and Harvard, she also lectured at Northwestern, Chicago, Washington State Universities, and the University of Colorado as well as addressing the Midwestern Economic Association (Turner 1989, p. 171). It was her visit to MIT that is worth noting for the focus of this paper, as she engaged the Faculty with the criticisms arising from Piero Sraffa’s work (Sraffa 1960) and the ongoing discussions at Cambridge, UK. That same year Pierangelo Garegnani was at MIT, and he is credited for

²“Cambridge” hereinafter refers to the English Cambridge unless otherwise noted.

having approached Paul Samuelson and Robert Solow with the same critical challenge (Hodgson 1997, p. 97).

The challenge started with the claim that any quantity measure of capital required the assumption that capital was, or could be, approximated as a single homogeneous good. The alternative conceptualization of capital as money, on the other hand, required price measures and therefore, in a world of heterogeneous capital goods, a set of relative prices. In a capitalist economy however, such prices depended on the rate of profit.³ In an aggregate production function, which is a function of capital *a priori* measured in price terms, the determination of profits that followed led to circular reasoning. Following the work of Sraffa (1960), a number of disturbing properties of such aggregate production functions were revealed, namely reswitching or double switching and capital reversing. Reswitching corresponded to the possibility that the same method of production might be the most profitable at more than one rate of profit, even though other methods were more profitable at values in between. Capital reversing corresponded to the value of capital moving in the same direction as the rate of profit.⁴

In June of 1962, the British *Review of Economic Studies* (*RES*) held a “Symposium on production functions and economic growth.” The major piece of the volume was a paper by Samuelson entitled, “Parable and Realism in Capital Theory: The Surrogate Production Function,” which was dedicated to “Joan Robinson on the occasion of her memorable 1961 visit to MIT” (Samuelson 1962, p. 193). In the paper, the author formalized a production function that was safe from the Sraffian results, thus preserving what the author called neoclassical parables, namely: an association between lower rates of profit and higher values of capital per man employed; an association between lower rates of profit and higher capital-output ratios; an association between lower rates of profit and higher sustainable consumption per head; and in competitive markets, a distribution of income between profit-earners and wage-earners explained by knowledge of marginal products and factor supplies.

As Robinson was later to confess: “For several years, everyone (except Piero Garegnani) was somewhat baffled by the surrogate production function. Then, in 1965, a fortunate accident occurred. A disciple of Professor Samuelson claimed to have proved that reswitching can never occur” (Robinson 1975, p. 37). David Levhari’s⁵ 1965 piece in the *Quarterly Journal of Economics* (*QJE*) was an attempt to sweep the Sraffian criticism away. In 1966 the Harvard based *QJE* gave a follow up to the Levhari article under a symposium on “Paradoxes in Capital Theory.”⁶ The symposium included a counter example to Levhari’s

³Throughout the paper I shall refer to the term “rate of profit” following the Cambridge use, since this is the side of the debate that I am following more closely; the neoclassical side preferred “rate of interest” to label the return to capital. See Cohen and Harcourt (2003, n.7) for the distinction.

⁴Birner (1996, pp. 227–29) has argued that Sraffa’s book was not the igniter of the controversies. It was Robinson and Garegnani’s challenges made to Samuelson at MIT that led the latter to take notice of Sraffa’s reswitching. The controversy began when Samuelson engaged this criticism.

⁵David Levhari was a graduate student of Samuelson’s at MIT. Levhari completed his Ph.D. thesis entitled “Essays on Optimal Economic Growth” in 1964.

⁶“Paradoxes on Capital Theory: A Symposium” in the November 1966 issue of the *Quarterly Journal of Economics*.

theorem by Pasinetti (1966), two articles discussing the significance of reswitching (Bruno *et al* 1966; Garegnani 1966), a refutation of the non-switching theorem by Morishima (1966) and Samuelson and Levhari (1966) that was an admission that the non-switching theorem was false.

The most important piece was Samuelson's "Summing up," wherein the author both recognized the possibility of reswitching and provided an intuition for it based on an "Austrian circulating capital model" (Samuelson 1966, p. 573). In Samuelson's concluding remarks he praised the Sraffian critics for demonstrating reswitching as a logical possibility of which theorists needed to be aware, he ended: "If all this causes headaches for those nostalgic for the old time parables of neoclassical writing, we must remind ourselves that scholars are not born to live an easy existence. We must respect and appraise, the facts of life" (Samuelson 1966, p. 583).

For a reader more familiar with the controversies, my description may seem rather narrow. I made note of only two sets of exchanges (the *RES* and *QJE* symposiums), in what was later portrayed as a debate "raging" over three decades. The reason for this narrowing of focus is that I wish to present the debate in its immediate temporal context, that is, as it was perceived in the mid-1960s, before the reconstructions of the debate produced by the late 1960s and early 1970s surveys.

Without the surveys and the histories as guides to travel through the literature, the reader of the mid-1960s⁷ would probably have known only the *QJE* symposium and would have recognized a debate but would not have easily considered it as part of a larger controversy. Furthermore, it would not have been easy for such a reader to appreciate the significance of what was being debated. It is against this uncertainty concerning the content and significance of the debate that the surveys emerged.

III. WRITING THE CONTROVERSIES INTO HISTORY: HARCOURT'S SURVEY ARTICLE AND ITS CRITICS

The symposia mentioned above had a significant impact in MIT and Cambridge, and in fact all the main participants in the symposia were connected to these two institutions.⁸ These participants had a reading from their own experience of what was under discussion and what could be concluded, but the same is not necessarily true for the remainder of the profession.

As E. Roy Weintraub (1991) notes: "from the present we look back and see order, but that order was not always evident to those who were in the field at the time" (Weintraub 1991, p. 129). The survey article provides order as it reconstructs past work so that it may be seen as leading somewhere—to knowledge—or in this case, to the debate's resolution.

The first survey appearing in the *Journal of Economic Literature* (*JEL*) in 1969

⁷The reader that I am interested in is American. Post Keynesian economics, despite its strong intellectual roots in Cambridge, is of American design, a sociological fact not often noted in Post Keynesian histories due to their intellectual focus. The American *QJE* exchange would have been better known than the British *RES* one.

⁸See Cohen and Harcourt (2003, p. 200) for a list of the main participants.

was apparently not commissioned as an account of the debate.⁹ Mark Perlman, then editor of the *JEL* was in Australia for the Summer of 1968 and asked Harcourt to write a survey “reviewing recent developments in capital theory” covering the preceding decade. Perlman then added: “I am particularly interested that American readers should have the opportunity to see where Mrs. Robinson’s views come from and how they may be contrasted with the views of other writers” (Perlman to Harcourt, August 28, 1968).¹⁰

Harcourt initially divided the survey into five self-contained working papers and sent them to some of the participants of the debate to gauge early reactions to his interpretations (Harcourt 2000, p. 278)¹¹. That is, in order to write about the controversies, Harcourt engaged in a dialogue with participants from both sides of the debate. To trace Harcourt’s interpretation, and to show how it is related to the construction of an identity, I first try to draw the narrative structure of the survey (Harcourt 1969). After which, I compare his survey with reactions by his contemporaries to highlight the particularity of the framing Harcourt gave the debates.

Harcourt (1969) was divided into four sections: “Malleability, Fossils and Technical Progress”; “Solow on the Rate of Return: Tease and Counter Tease”; “A Child’s Guide to the Double-Switching Debate,” and “The Rate of Profit in Capitalist Society.” The first section discussed a series of exchanges prompted by Robinson’s 1953–54 question: How to measure capital? (Robinson 1953–54). Robinson was placed at the start of the series of contributions, but the main protagonists were neoclassical economists: some responding directly to Robinson’s question (Champernowne 1953–54, Solow 1955–56, Swan 1956), others surveyed to show different responses to the same question even though Robinson’s challenge was never their motivation. Harcourt reconstructed work on growth models as a series of attempts to answer Robinson’s question. This

⁹There was an earlier request made by Perlman to Jack Hirshleifer for a survey on capital theory: “stressing the developments in capital theory in the last five to ten years” (April 24, 1968). Hirshleifer accepted the request but noted: “I follow the Fisher tradition which considers most of the traditional debates about ‘capital’ irrelevant. What all this leads to is that I would be more comfortable if the assigned topic were, say, ‘inter-temporal decision and equilibrium’ rather than ‘capital theory’” (May 14, 1968). Compromise was met with the title proposed by Perlman: “Some Modern Aspects of Capital Theory: Inter-Temporal Decision and Equilibrium” (May 20, 1968). However, by July 17 Hirshleifer wrote to Perlman with the news that he would not be able to write the piece. In Harcourt’s words: “So there Perlman was, with the second issue of this new journal and no one to do the major survey article. Wilfred [Prest] said, “Don’t worry, go to Adelaide and ask Geoff Harcourt. He’s good on capital theory, he’ll do it for you.” So Perlman came over and spent a day talking me into it” (King 1994, p. 176). The evidence suggests that Perlman was not looking for a survey of the Capital debates. Foremost Perlman was looking for a survey on the broad subject of capital theory and was willing to negotiate the survey’s focus with the assigned writer. I should note however that I have asked Perlman whether this was the case and his reply was inconclusive (Private communication, July 5, 2003). All letters in Mark Perlman Papers, Rare Book, Manuscript, and Special Collections Library, Duke University, Durham, North Carolina, Box 24.

¹⁰Letter from Perlman to Harcourt, August 28, 1968, in Mark Perlman Papers, Rare Book, Manuscript, and Special Collections Library, Duke University, Durham, North Carolina, Box 24.

¹¹Harcourt (1969) also provides a list of the people that had access and input to the working papers in the acknowledgments section.

re-interpretation connected a broad and earlier literature with the contemporary debate. Despite all these efforts, Harcourt suggested that the Robinson problem had remained unanswered; no satisfactory measurement of capital was ever presented by the neoclassicals.

At the core of the second section of his paper lay a debate between Solow and Robinson. According to Harcourt, Solow had in his De Vries lectures of May 1963 (Solow 1963) “[prefaced] his analysis with a discussion of why there are recurring controversies in capital theory. He gives two reasons, one of which is ideological—the social function of providing an ideological justification for profit which in the nineteenth century was the non-Marxist backlash; the other reason is analytical—it’s difficult” (Harcourt 1969, p. 380). To escape the ideological weight of the debates, Solow had drawn a “conceptual distinction between the imputed return to capital and the income of capitalists” (Harcourt 1969, p. 380). Robinson attacked Solow, interpreting his work as “an attempt to justify the marginal productivity theory of distribution and, in particular, the microeconomic proposition that in a competitive capitalist economy labor is paid its *full employment* marginal product” (Harcourt 1969, p. 382). Thus, the purpose of this section was not to shed light on another theoretical debate but to “highlight some of the causes of the controversy” (Harcourt 1969, p. 380), namely ideological ones.

The third section of Harcourt’s survey paper presented the reswitching controversy as the high point of the debate and intimately linked these ideas to the work of Sraffa (1960). The author placed Sraffa not as a third independent entity holding a distinct theoretical construct but as a contributor on the Cambridge side of the debate. Sraffa’s work was shown as concerned “with a view of economic life and especially of the production process, price formation and income distribution which was more akin to that taken by the classical economists—Smith, Ricardo, Marx—than by the neo-classicals—Jevons, Wicksteed, Marshall” (Harcourt 1969, p. 378). In doing so, Harcourt suggested a link between Sraffa and Robinson’s own interest in recovering Marx to economics. Harcourt thus added the Sraffian-reswitching and capital-reversing ideas into the arsenal of the Cambridge theorists. The early 1960s debates in the *RES* and *QJE*, and Samuelson’s “Summing up” (Samuelson 1966) were presented by Harcourt as the point at which neoclassicals were forced to abandon their neoclassical parables and therefore aggregate production functions and the marginal productivity theory of distribution.

Finally we are led to a section on the rate of profit. The statement that connected the sections was: “that, in order to determine the rate of profit, further factors have to be introduced from outside the production system.” Such factors were the saving propensities in the work of Nicholas Kaldor, Luigi L. Pasinetti and Robinson. Harcourt, after having presented a Cambridge critique to neo-classical economics, gives the reader an alternative economics—a “Cambridge England” alternative.¹²

¹²It is arguable if what Harcourt presented was an alternative. Such a claim is certainly rejected by neoclassicals but it is also object of debate by those who came to stand side by side with the Cambridge critics. Harcourt himself wrote on the possibility and existence of such a complete and coherent alternative. See King (2002, chap.10) for a review of this literature.

Harcourt molded together several distinct instances of debate into his “Some Cambridge Controversies in the Theory of Capital” (Harcourt 1969), all broadly connected to the concept of capital in growth and distribution, thus justifying the choice of title for his survey, multiple capital controversies, and not a single controversy. While Perlman had commissioned a survey on developments in capital theory over the preceding decade, Harcourt went as far back as 1953 to present the controversies of the 1960s as part of a broader critique set out by the work of Robinson, Kaldor, Pasinetti, and Sraffa, or following the author’s label: Cambridge, England. Harcourt’s survey was not an inquest into a sub-field of theory rather, his presentation was a narrative of emergence. In the narrative, Robinson had challenged the mainstream that promptly reacted, but to no avail. Harcourt told us the mainstream’s efforts were unable to meet her challenge (section 1 of the survey). The debate with Solow was made to show the ideological motivation of the controversy, and its political and social content. Harcourt wrote Solow’s attempt at escaping the ideological content of the debate as a failure (section 2 of the survey). He then brought in Sraffa to administer the final and definitive blow, one that was written as a logical refutation of neoclassical belief and grounded this claim on the fact that Samuelson had conceded defeat (section 3 of the survey). The stage was set for something to happen, and Harcourt presented an alternative that he claimed was immune to the problems raised by the critics (section 4 of the survey).

In contrast to the strong claims made in the body of his article, Harcourt’s conclusion may at first sight seem conservative: “We break off in midstream and few issues are settled. A key one relates to marginal productivity and its role in distribution, about which, as we have seen, there is a complete cleavage of opinion on the significance of double-switching results for this issue” (Harcourt 1969, p. 398). This open-ended conclusion was an invitation for the reader: the critique/alternative that Harcourt had narrated as emerging was an incomplete edifice to which the reader could contribute. And Harcourt predicted that the debate would surely move into more fundamental areas “for it is the *general methodology* of neoclassical analysis, rather than any particular result, which basically is under attack” (Harcourt 1969, p. 398).¹³

¹³With the writing and rewriting of the history of the controversies the theoretical importance of reswitching and its place at the center of the controversies changed. By 1971 Robinson was downplaying the importance of reswitching and by 1975 her plea was to move the debate into methodology and abandon its technical emphasis (namely issues concerning reswitching) (Robinson 1971; 1975). No methodological reenactment of the debate followed, neoclassicals did not engage with Robinson’s redesigned critique (Samuelson and Solow commented on Robinson’s “Unimportance of Reswitching” but merely to discuss the significance and status of reswitching for neoclassical theory (Robinson 1975, Samuelson 1975, Solow 1975)). Her call was met only in later reconstructions of the debate that were now identifying two parallel critiques set out by Cambridge, England: one was the reswitching result which was the core issue debated in the *RSE* and *QJE* symposia; the other was Robinson’s equilibrium versus history critique, presented as a deeper methodological challenge to the mainstream (see Harcourt 1975, 1976). And by 1994 Harcourt commented on the controversies: “I think the methodological nature of Joan Robinson’s critique is the dominant one” (King 1994, p. 178). As the controversies were being reconstructed in later histories and surveys, new readings were being attached to them. It is not my task in this paper to assess these later reconstructions. What matters for this paper is to distinguish these later readings from the Harcourt (1969, 1972) ones where the equilibrium vs. history critique was absent.

My account here is designed to highlight, as the most important aspect of Harcourt's survey, the narrative that lay in the background. The author constructed a parting of the ways and the formation of two distinct camps. In this narrative we see how two sides take shape, a neoclassical one where aggregate production function theorists struggle to solve the puzzles of dealing/measuring capital, and a Cambridge side where there is an abandonment of such attempts and the search for new solutions—initiated through Robinson (1956) and in the later work of Kaldor and Pasinetti.

The success of a survey may be assessed in its ability to clarify and settle the field, in this case, the debate. Indeed, "the 'success' of a survey may be so great that the field comes to be defined as that which was surveyed?" (Weintraub 1991, p. 129). Was Harcourt's survey successful? For the Cambridge side of the divide the survey was "successful": it settled a reading of the debates that was never contested.¹⁴ For the wider professional audience, the survey's construction was challenged. In 1974 three surveys were published that portrayed the controversies in ways that disagreed with Harcourt (1969, 1972). And, by presenting alternative ways to understand the controversies, these three surveys underline the particularity of the vision that Harcourt had put forward.

Hicks (1974) also recognized two competing visions in economics, but the author had his eyes on a longer history. Hicks made a distinction between a Fundist position in capital theory with its roots in Adam Smith and British classical authors (capital is a fund and capital's value is derived from the value of future net products) and a Materialist position going back to Cannan, Marshall, Pigou, and J. B. Clark (where capital consists of physical goods whose value is the market value of the capital goods) (Hicks 1974, p. 309). Hicks however saw no destructive tension between the two since they were both worth pursuing. Both definitions had their place and importance, one was forward looking, the other backward looking. In Hicks's understanding, debates about capital, and the history of economics provided many examples, were often a product of misunderstanding. For Hicks, the 1960s capital controversies were just another instance of this misunderstanding. In this round of the never-ending capital debates, the English side was seen as taking a Fundist position, while the American one a Materialist stance. So in Hicks's account there was none of the novelty in approach that Harcourt associated with the Cambridge side. Rather, what motivated the analytical standoff was not discovery and theory development but two groups working under distinct and well-established conceptualizations of capital.

A more direct challenge to Harcourt came from Stiglitz (1974) and his review of Harcourt's book (1972), which provided a more developed version of his 1969 survey argument. Stiglitz engaged with the Cambridge theories and gave voice to the neoclassical criticisms of this approach. So the critique did not run solely from England to the U.S. Stiglitz identified a number of weaknesses in the

¹⁴ There was no challenge from the Cambridge side to Harcourt's article (Harcourt 1969) or book (Harcourt 1972). These two texts were seen by the Cambridge side as a reliable depiction of the debate, even Harcourt's later reviews, which ever increased the length of the debate, would not hold the same standing as this earlier work (see King 2002, chap. 4).

Cambridge approach, for instance, their models were not amenable to be extended to include more than two factors (beyond labor and capital) (Stiglitz 1974, p. 895). The author also charged the Cambridge theorists with rejecting any attempt at empirical verification (Stiglitz 1974, p. 901). Stiglitz did recognize an emerging theory in the Cambridge group but made it the object of fierce criticism. This stands in contrast with Harcourt's survey wherein neoclassicals were reacting to a challenge but never responded with criticism towards the alternative approach. Stiglitz presents the new space as empty of promise, challenging Harcourt's identification of an alternative.

Of all the pieces discussed here, Blaug (1974) is the most akin to Harcourt's in its structure. It, too, identified the existence of competing theories, the neoclassical and the Cambridge one, the latter defined as composed by the contributions of Kaldor, Pasinetti, and Robinson (Blaug 1974, p. 61). It too assumed the need for comparison and judgment, and saw the controversies in this light. His judgment was clear:

The Cambridge, England, theories are certainly logically consistent, even if they do not always hang together in a logically consistent, total framework of theories. They are possibly more realistic in some of their basic assumptions, although the statement is itself highly ambiguous. But they are not simpler, they are not more elegant, they are totally incapable of producing testable predictions. Whatever is wrong with neo-classical economics (and who doubts there is much to complain of?), it wins hands down on all possible criteria (Blaug 1974, p. 85).

What is noteworthy in Blaug (1974) is that his theory appraisal did not attach laurels of victory to the logically consistent one over the alternative. Unlike in Harcourt's survey, Samuelson's recantation was not sufficient to sustain the claim that the Cambridge theory had "won" and therefore should be adopted.

The comparisons between Harcourt's 1969 article and these three 1974 surveys reveal the controversial nature of the claims embedded in Harcourt's narrative. The comparison with Hicks (1974) shows Harcourt's stress on novelty and importance of the debate in sustaining his claim that a theoretical alternative is emerging. The comparison with Stiglitz (1974) brings out a hidden dimension of the controversies obscured in Harcourt's survey, namely that of criticisms directed from the U.S./neoclassical side to the UK side. Harcourt's article had portrayed only the attack of a challenger on the entrenched status quo. Finally, the comparison with Blaug (1974) leads one to question the resolution of the controversies as Harcourt presented it. While Harcourt saw an admission of defeat by the neoclassicals, Blaug saw an alternative that had not proven itself and a critique that had little impact when considering the neoclassical edifice in all its breadth and strength of analysis.

The comparison also allows me to show how Harcourt's 1969 survey reconstructed the controversies to identify the formation of a new space for theory development, a space filled with promise. Harcourt set the beginning of the controversies at the point when the first line is drawn between the two groups, though according to Hicks (1974) that debate was a long running one. Harcourt portrayed the demarcation line splitting the field of economics as coming from

the Cambridge side, while in Stiglitz (1974) the demarcation was being drawn by critiques originating from both sides of the debate. As the culmination of this process of definition Harcourt identified a victory over the old theory, whereas Blaug (1974) argued that there was no victory and so no new viable space.

IV. THE ROLE OF THE CONTROVERSIES FOR POST KEYNESIAN EMERGENCE

The Post Keynesian economics group took shape in the U.S. during the early to mid-1970s. The publication of Robinson's dissenting views in the course of the controversies motivated a number of contacts with American graduate students and young colleagues in search of theoretical alternatives. The dialogue that Robinson maintained with these American colleagues facilitated the creation of a network of American Post Keynesians.¹⁵ For the purpose of this paper, what is worth noting in this correspondence is the role played by the capital debates, in particular in the early half of the 1970s, once Harcourt had framed them with a narrative interpretation.

The surveys (Harcourt 1969, 1972), which were contemporaneous to this trans-Atlantic correspondence, added status to the Cambridge positions, reinforcing the contact between dissenters in Britain and the U.S. As Weintraub has argued, surveys among other things, validate the importance of the subject for the wider academic audience and may set directions for theory development (Weintraub 1991, p. 132). Harcourt's survey told, as its main moral, that Cambridge was a powerful critique of the mainstream¹⁶ in need of further work, an invitation American Post Keynesians took seriously.

According to the participants in these exchanges the goal was to develop an alternative economics in a moment where neoclassical economics had been shown to be flawed:

¹⁵Lee (2000a) provides an account of one of these dialogues by publishing the correspondence between Alfred S. Eichner and Joan Robinson. Drawing on the Eichner-Robinson exchanges and further correspondence between Robinson and other American economists, Mata (2002) argues that the adoption of the label was a negotiation between American and British dissenters, choosing from "Anglo-Italian theory," "neo-Keynesian economics," "Cambridge school," and "post-keynesian economics." Tracing the multiple definitions of the label, Mata (2002) presents Post Keynesian economics as a construction by Americans dissenters. British dissenters were not involved in the definition of the label; however, their work was classified as Post Keynesian and they provided an ancestry for the group. A similar story is told by Lee (2000b), who has traced the origins of the network of Post Keynesian economics to meetings held between dissenters at the ASSA annual meetings, the first of these meetings in 1971 promoted by Joan Robinson. The group's formation and further development occurs in relative isolation from Britain. Both papers (Mata 2002, Lee 2000b) stress the importance of the dialogue between American dissenters and Robinson for the formation of Post Keynesian economics. They both portray the Post Keynesian group as an American construct, and not as an import of Cambridge England theory as commonly stated in surveys designed to present Post Keynesian economics—for example, Eichner and Kregel (1975) and Harcourt and Hamouda (1988).

¹⁶As noted by King (2002, p. 121): "It seemed for a while to many critics of neoclassical economics as though the enemy had been taken on and beaten, on its own ground . . . There was the real prospect . . . that 'Cambridge,' 'Anglo-Italian,' 'neo-Keynesian,' or 'Post Keynesian' economics might constitute a potential lethal threat to orthodox thinking."

now that a seemingly mortal blow to the neoclassicals has been delivered, creating a tremendous redundancy in the human capital stock on this side of the ocean, the American school will try to restore its capital values by drawing the Anglo-Italian school into an endless controversy as to which of the two simplified unrealistic models—the Cambridge, Massachusetts or the Cambridge, England one—is the least unrealistic. (Davidson to Robinson, December 15, 1969).¹⁷

So Paul Davidson called for the construction of a new and “realistic” economics, portraying a mainstream that had lost credibility from the Cambridge attack. The new theoretical development, however, had to respect the boundaries drawn by the controversies; it had to fit the space defined by Harcourt’s survey. Thus, a sizable portion of the Davidson-Robinson correspondence during the late 1960s and early ‘70s was focused on the introduction of profit into Davidson’s analysis, and his chapters XI-XII in *Money and the Real World* (Davidson 1972) were his self-recognized attempt to integrate his work with the Anglo-Italian theory.¹⁸

The themes of the capital controversies were thus a means to mediate a joining of forces between critics from both sides of the Atlantic. In the late 1960s, early ‘70s, the work of American Post Keynesians was framed to a large extent by the controversies. It was not just the work of Davidson, with his participation in the debates (Davidson 1968) and his book (Davidson 1972), but also the work of Nell (1967a, 1967b), Asimakopulos (1969), Harris (1973, 1978), Kregel (1971, 1973), and Laibman and Nell (1977). This literature explored the significance of reswitching and attempted to develop models under the Cambridge assumptions.

The capital controversies as they unfolded had touched a latent discontent in American economics and this became manifest in the rising correspondence between Robinson and dissenting American economists. So the controversies proved to be a much-warranted instrument to create “the necessary critical mass of academically situated economists to bring the post-keynesian revolution to this country” (Alfred S. Eichner to Robinson, July 3, 1974).¹⁹ For these American dissenters, the controversies, as interpreted by Harcourt, defined both a place of departure to adopt, and a direction to follow—namely the construction of an alternative to replace a “defeated” mainstream.

V. SPREADING THE WORD

The mid-1960s to early ‘70s were periods of political turmoil inside American universities. A generation of student activists felt besieged:

by events too troubling to dismiss. First, the permeating and victimizing fact of human degradation, symbolized by the Southern struggle against racial bigotry, compelled most of us from silence to activism. Second, the enclosing

¹⁷Letter from Paul Davidson to Joan Robinson, December 15, 1969 (King’s College Library, Cambridge, Joan Robinson Papers, JVR/vii/114/88).

¹⁸Letter from Paul Davidson to Joan Robinson, September 25, 1970 (Joan Robinson Papers, King’s College Library, Cambridge, JVR/vii/114/125).

¹⁹Letter from Alfred S. Eichner to Joan Robinson, July 3, 1974 (Joan Robinson Papers, King’s College Library, Cambridge, JVR Misc 79/4).

fact of the Cold War, symbolized by the presence of the Bomb, brought awareness that we ourselves, and our friends, and millions of abstract “others” we knew more directly because of our common peril, might die at any time (Students for a Democratic Society (SDS) 1962).

In 1965 there was the escalation of the war in Vietnam, leading to its recognition as a full-fledged war. The universities initially responded with teach-ins, and in 1968 and 1970 with major strikes and occupations (Sale 1974; Isserman and Kazin 2000). Students rebelling against the government were soon rebelling against the curriculum and the university bureaucracy. “Our professors and administrators sacrifice controversy to public relations; their curriculum change more slowly than the living events of the world; their skills and silence are purchased by investors in the arms race; passion is called unscholastic” (SDS 1962, p. 3). Robinson was the opposite of all this: “In the heady, booming, economics faculties of the 1950s and 1960s [Robinson’s] reputation as a rebel against orthodox economic doctrine of all kinds spread worldwide” (Deane 1989, p. 19). For Joan Robinson, unlike the majority of the economics profession, spoke to the mood of the times. Across the Atlantic, Robinson was calling for Britain to lose its illusions of being a world power and an empire, to wind up North Atlantic Treaty Organization (NATO) and settle the Cold War, to “devote our resources to our own economy we could begin to make the place habitable.”²⁰ And in her numerous talks to student audiences—organized by students—Robinson would exhort them to not be co-opted into business and conformism, to remain radical and alienated until they were forty.²¹

She brought together political and academic radicalism, and in the 1960s she made several visits to the U.S. where she lectured on methodology and the capital controversies.²² The controversies, as retold in Robinson’s lecturing in the late 1960s and early 1970s, were given political significance, particularly in the rejection of the marginal productivity theory of distribution that had direct implications for political debate. Harcourt was by her side in publicizing the controversies and also in bringing out the political dimension. The clarity of Harcourt’s exposition (Harcourt 1969, 1972) gave the group a much called for visibility. As was appreciated by Eichner in 1969, at the time of Harcourt’s *JEL* article: “I quite agree that we have been the victims of a conspiracy of silence on this side of the Atlantic . . . But perhaps the conspiracy is about to end. Harcourt’s review article in the *Journal of Economic Literature* will clearly help to make your work better known in this country, and I think it is only in the beginning.”²³ Harcourt visited the U.S., Canada, and Britain several times in the 1970s, and in the academic year of 1972–73 alone he gave over fifty seminars on the topics of his *Controversies* book (Harcourt 1972).

Cambridge’s economics as I have shown, defined and emerging through the controversies, was also presented as a return to political economy, to Marx, and

²⁰“Britain, 1961” (Joan Robinson Papers, King’s College Library, Cambridge, JVR/ii/36/1).

²¹“On Student Radicalism” (Joan Robinson Papers, King’s College Library, Cambridge, JVR/ii/36/1).

²²I was unable to find a detailed account of Robinson’s visits to the U.S. To my knowledge Turner (1989, chap. 14) provides the most detailed list of visits.

²³Letter from Alfred S. Eichner to Joan Robinson, March 1970 (Joan Robinson Papers, King’s College Library, Cambridge, JVR Misc 79/4).

a more radical Keynes. This made the waning debate speak to a new generation of economists, themselves led towards Marx and political economy by student activism of the late 1960s and early 1970s. In Harcourt's presentation of the controversies, the political divisions mapped themselves into theoretical differences:

It is my strong impression that if one were to be told whether an economist was fundamentally sympathetic or hostile to basic capitalist institutions, especially private property and the related rights to income streams, or whether he were a hawk or a dove in his views on the Vietnam War, one could predict with a considerable degree of accuracy both his general approach in economic theory and which side he would be on the present controversies (Harcourt 1972, p.13).²⁴

The controversies written into history as identifying conflicting approaches in economic theory were mapped into an ideological divide in the profession, one in which the radical students would stand with the Cambridge England side.

VI. IDENTITY IN HISTORY

The great transformation of the profession implied in the Post Keynesian reading of the controversies didn't occur. In time, the capital controversies slipped away from the memories of the majority of the profession. As Hodgson (1997) documents with the assistance of citation indexes, the profession as a whole was never much caught by the capital critique. The controversies and the Sraffian assault "[were] not only bypassed, but became irrelevant for the new core theory of neoclassicism. Sraffian results have no apparent critical effect on game theory. Losing their status even as internal criticism of the mainstream core, they became less and less relevant" (Hodgson 1997, p. 104). And, by early 1980s, the focus of Post Keynesian work had also moved away from the battles of capital theory. Debates inside the Post Keynesian field were now on the subjects of methodology and the role of uncertainty in economic processes (prompted, according to King (2002, pp. 181–82), by a rediscovery of J. M. Keynes through the publication of his *Collected Works*). Their debates with the mainstream were most notably on monetary theory, where they offered a critique of monetarist policies (King 2002, chap. 8).

However, unlike the rest of the profession, the capital controversies were not forgotten by Post Keynesians. Time and time again this episode was revisited and reassessed and therefore also relived. What I wish to discuss in this section are the uses given to the capital controversies by a new generation of economists coming into Post Keynesian economics. I show the role the controversies played in the reproduction of the group. The controversies continued to provide direction for new research and, as a narrative, they identified the origins and the role that the group should strive to play in the profession, namely that of a critic and alternative to the mainstream.

²⁴Harcourt was a founding member of South Australia's Campaign for Peace in Vietnam and for two spells, Chairman of its executive committee (Harcourt 1999, p. 45).

The ways through which the new generation came to learn of the controversies are varied. Older Post Keynesians taught the controversies to their students in courses on macro theory and on the history of economics.²⁵ The promotion of summer schools, starting in 1981 in Trieste, also allowed a younger generation to come into contact with the history of the capital debates. The summer schools were an opportunity for Post Keynesians from Europe and America to join in discussion. Post Keynesians brought their graduate students to the summer schools for an intensive course on what was Post Keynesian economics and what were its most recent theoretical developments. Not only was Harcourt present at these meetings but his 1972 book on the capital controversies was included in the reading lists of the school.²⁶

The new generation of Post Keynesians used the capital controversies as a motivation for their work. They brought new denunciation of the limitations of neoclassical models (Baldone 1984), extended the models of the Cambridge, England, side (Fazi and Salvadori 1985) and responded to critiques raised by the neoclassicals (Ahmad 1986). Further contributions re-explored the significance of reswitching (Halevi 1985) and discussed new ways of presenting the Cambridge argument more in tune with the increasing focus on Keynes of the 1980s (Fleck and Domenghino 1987). The critique was even reproduced as an attack on other fields of neoclassical theory, namely neoclassical demand theory: “Reswitching in capital theory led to the destruction of the neoclassical parables regarding the relationship between prices, production and distribution. Likewise, reversal (and reswitching) of commodity bundles ... undermine the orthodox consumer theory” (Watts and Gaston 1982–83, p. 287).

A further use given to the capital controversies was to revisit the debate for methodological insights. As younger Post Keynesians revisited the moment where the demarcation between Post Keynesian economics and neoclassical economics was drawn, they saw themselves as drawing lessons over the main differences between Post Keynesian economics and the mainstream. I provide two examples of this use.

The first example is a debate in the pages of the *Journal of Post Keynesian Economics* from 1980 to 1985, and its main question was: Why did the capital controversies fail to produce a “Kuhnian scientific revolution”? The question was raised by Sheila Dow (1980). She depicted the controversies as showing the weaknesses of the neoclassical paradigm and prompting a crisis, yet they were not followed by a paradigm shift. In her paper, Dow stressed the incommensurability of two paradigms, the neoclassical and the Cambridge one, to explain

²⁵As comes out in King’s interviews of Post Keynesians (King 1994), Rutgers University’s graduate program trained some of the most influential Post Keynesians of the 1980s generation. The controversies were taught at Rutgers in the early 1980s in the history of economics course taught by Nina Shapiro (Radhika Balakrishna, Private communication, June 13, 2003). I should note that some students already knew of the controversies from earlier reading of dissenting literature and were convinced “the capital controversy effectively destroyed neoclassical theory and [were] willing to use the controversy in a number of ways to show it” (Fred Lee, Private communication, October 11, 2003).

²⁶Sidney Weintraub Papers, Rare Book, Manuscript, and Special Collections Library, Duke University, Durham, North Carolina. For more on the Trieste Summer School see Arena (1987).

neoclassical economics' survival of the crisis. Andrea Salanti (1982) took issue with Dow. The author argued that the Cambridge School "has not yet been able to provide a sufficiently general and/or "value-appealing" theory to dislodge the neoclassical standard" (Salanti 1982, p. 129). Salanti saw the controversies in a different light: since the Cambridge School provided an incomplete alternative, the effects of its critique were only partial. Dow's reply (Dow 1982) claimed Salanti held a positivist view, naively expecting theories to be testable and refutable. Dow reasserted her stress on the value-ladenness of paradigm assessments.

This debate continued with Avi Cohen (1984) identifying in the two paradigms competing methodological approaches and therefore distinct evaluative outlooks on theory. The critique of Cambridge stressed the requirement to explain historical processes and underplayed the need for prediction; this was in stark contrast with the methodological and evaluative outlook of neoclassicism. Cohen concluded that Post Keynesians should "be explicit about their methodology, so as to avoid the kind of misunderstandings surrounding the Cambridge controversies . . . [and] use that methodology to provide good historical and theoretical explanations of economic phenomena" (Cohen 1984, p. 627). Cohen's contribution was challenged by Michael Bernstein (1985). Like Salanti (1982), Bernstein saw the Post Keynesian alternative as incomplete: "attention should be focused on the development of a coherent and systematic theory" (Bernstein 1985, p. 610). Bernstein concluded that the methodological re-engagement that Cohen had proposed would not produce the necessary transformation of the profession. Cohen's response (Cohen 1985) was to reject Bernstein's reading of his article. He argued that his methodological re-engagement was not a strategy to battle neoclassical opponents, but a means to bring out the underlying issues dividing the profession.

A second example of exploration into the history of the controversies in search of renewed methodological insight is found in J. B. Davis (1987). Davis derived from the capital controversies one of the three key methodological principles of Post Keynesian economics, which was portrayed as "a response to the failures of the neoclassical theory and its inadequate characterisation of economic activity" (Davis 1987, p. 552). The principle he drew from the Cambridge controversies was that: "Contrary to the neoclassical conception of economic science as the steady accretion of analytical technique that renders past theory obsolete, post Keynesian theory allows for the re-examination of a variety of past theories as the needs of contemporary investigation bring forward new problems of characterization" (Davis 1987, p. 563).

These examples show the capital debates as a starting point for the Post Keynesian (Cambridge) paradigm, and the critique it raised, as a shattering of the neoclassical edifice that should have brought its demise. In these examples, young Post Keynesians discuss the history of Post Keynesian economics through the history of the controversies. Significantly, the capital controversies are said to provide methodological lessons for the future of Post Keynesian economics, and for Post Keynesian economics as an alternative to mainstream economic theory.

The latest reviewing of the controversies (Cohen and Harcourt 2003) maintains this outlook over the Cambridge capital debates:

While many of the key Cambridge, England, combatants stopped asking

questions because they died, the questions have not been resolved, only buried. When economists decide to delve again, we predict controversies over these questions will be revisited, just as they were time and again in the 80 years prior to the Cambridge controversies (Cohen and Harcourt 2003, pp. 211–12).

Cohen and Harcourt invite the reader to use the controversies to understand the nature of the profession and the nature of orthodox economic theory, and they suppose that the controversies may assist economics in overcoming the mainstream theory's shortcomings. The main thrust of the Cohen and Harcourt review article is that differing outlooks in both "ideology and methodology, two subjects most economists would rather avoid" (Cohen and Harcourt 2003, p. 210) fueled the controversies and prevented their resolution. This explanation brings together old and new readings of the debate: the older emphasis on ideology present in Harcourt's 1969 review article and Cohen's stress of differing methodological perspectives inside the discipline (Cohen 1984).

Cohen and Harcourt (2003) bring out the main point of this section, that the capital controversies were instrumental in bringing together older and younger generations of Post Keynesians. Addressing the themes of the controversies, and providing it with new readings meant that a younger generation appropriated the history of the controversies as their own. Revisiting the controversies allowed them to position themselves side by side with older Post Keynesians and share the identity that the controversies' history provided, namely Post Keynesian economics as inheritor of a devastating critique of the mainstream and as the key to an alternative economics.

VII. CONCLUSION—THE POST KEYNESIAN ECONOMICS RELATIONSHIP WITH HISTORY

In a contribution to the *History of Political Economy* conference on the "Future of the History of Economics," Dow offers an "account of the organic way which history of thought is embedded in Post Keynesian economics" (Dow 2002, p. 319). The author argues that there is a Post Keynesian brand of history of thought: "history of thought is pursued primarily to inform modern economics, this goal is seen as being best served by building up a historian's understanding of older texts" (Dow 2002, p. 321). How does this relationship between theory and history of economics work? Dow suggests two connection points. The first is historical, grounded in ancestry: "In developing their theories, all three [founders of Post Keynesian economics, Keynes, Michal Kalecki and Sraffa] presented their work in awareness of the history of thought and their place in it; in that sense the separation between history of thought and theory development was by no means complete" (Dow 2002, p. 324). The approach was passed on to later Post Keynesians: Robinson, Eichner, Davidson, Phillip Arestis, Marc Lavoie, and Victoria Chick (Dow 2002, pp. 325–26). There is a second reason for this organic relationship between economics and its history, namely as a methodological option. "Studying different historical contexts and the theories developed to address them helps economists to build up the judgment necessary for developing theories appropriate to new contexts" (Dow 2002, p. 330). For

Dow, the Post Keynesian use of the history of economics arises as a methodological tool and as a tradition inherited from ancestors.

My argument makes a stronger claim. I have presented Post Keynesian use of the history of economics as a critical weapon in the emergence of the Post Keynesian group and to its reproduction as a separate entity inside economics. I identified the 1960s capital controversies as the point where Post Keynesians part ways with the mainstream and emerge as a separate body. But it was the writing of the history of the controversies that produced the group. To constitute itself, the group required an identity, a definition not only to be recognized by the body of the profession but also to provide a guide for the founding members to follow in framing their work. This identity was constructed by the narrative of the controversies.

The history of the controversies has continued to serve a purpose. New adherents to Post Keynesian economics have engaged in conversations over the meaning of the controversies. Both founding members and new adherents looked to the controversies as the starting point, looked to the history of the controversies to find their birth and a direction to follow. Their sense of self—their sense of identity as Post Keynesians—is found by them in the history of economics.

REFERENCES

- Ahmad, S. 1986. "A Pasinetti Theory of Relative Profit Share for the Anti-Pasinetti Case." *Journal of Post Keynesian Economics* 9 (Fall): 149–58.
- Arena, R. 1987. "L'école internationale d'été de Trieste (1981–1985): vers une synthèse classico-keynésienne?" *Économies et sociétés* n 7 (Mars): 205–38.
- Asimakopulos, A. 1969. "A Robinsonian Growth Model in One-sector Notation." *Australian Economic Papers* 8 (June): 41–58.
- Baldone, S. 1984. "From Surrogate to Pseudo Production Functions." *Cambridge Journal of Economics* 8 (September): 271–89.
- Bernstein, M. A. 1985. "The Methodological Resolution of the Cambridge Capital Controversies: A Comment." *Journal of Post Keynesian Economics* 7 (Summer): 607–11.
- Blaug, M. 1974. *The Cambridge Revolution, Success or Failure? A Critical Analysis of Cambridge Theories of Value and Distribution*. London: Institute of Economic Affairs.
- Birner, J. 1996. "Cambridge Histories True and False." In M. C. Marcuzzo, L. L. Pasinetti, and A. Roncaglia, eds., *The Economics of Joan Robinson*. London: Routledge.
- Bruno, M., Burmeister, E. & Sheshinski, E. 1966. "Nature and Implications of the Reswitching of Techniques." *Quarterly Journal of Economics* 80 (November): 526–53.
- Champernowne, D. G. 1953–54. "The Production Function and the Theory of Capital: Comment." *Review of Economic Studies* 21 (2): 112–35.
- Cohen, A. J. 1984. "The Methodological Resolution of the Cambridge Controversies." *Journal of Post Keynesian Economics* 6 (Summer): 614–29.
- Cohen, A. J. 1985. "Issues in the Cambridge Controversies." *Journal of Post Keynesian Economics* 7 (Summer): 612–15.
- Cohen, A. J. & Harcourt, G. C. 2003. "Whatever Happened to the Cambridge Capital Controversies?" *Journal of Economic Perspectives* 17 (Winter): 199–214.
- Davidson, P. 1968. "The Demand and Supply of Securities and Economic Growth and its Implications for the Kaldor-Pasinetti Versus Samuelson-Modigliani Controversy." *American Economic Review* 58 (May): 252–69.
- Davidson, P. 1972. *Money and the Real World*. London: Macmillan.
- Davis, J. B. 1987. "Three Principles of Post Keynesian Methodology." *Journal of Post Keynesian Economics* 9 (Summer): 552–64.

- Deane, P. 1989. "A Bibliographical Memoir." In Ingrid H. Rima, ed., *The Joan Robinson Legacy*. New York: ME Sharpe.
- Dow, S. 1980. "Methodological Morality in the Cambridge Controversies." *Journal of Post Keynesian Economics* 2 (Spring): 368–80.
- Dow, S. 1982. "Neoclassical Tautologies and the Cambridge Capital Controversies: Reply." *Journal of Post Keynesian Economics* 5 (Fall): 132–34.
- Dow, S. 2002. "History of Economic Thought in the Post Keynesian Tradition." In E. Roy Weintraub, ed., *The Future of the History of Economics*. Durham, NC: Duke University Press.
- Eichner, A. S. & Kregel, J. A. 1975. "An Essay on Post-Keynesian Theory: A New Paradigm in Economics." *Journal of Economic Literature* 13 (December): 1293–314.
- Fazi, E. & Salvadori, N. 1985. "The Existence of a Two-class Economy in a General Cambridge Model of Growth and Distribution." *Cambridge Journal of Economics* 9 (June): 155–64.
- Fleck, F. H. & Domenghino, C.-M. 1987. "Cambridge (U.K.) versus Cambridge (Mass.): A Keynesian Solution of 'Pasinetti's Paradox'." *Journal of Post Keynesian Economics* 10 (Fall): 22–36.
- Garegnani, P. 1966. "Switching of Techniques." *Quarterly Journal of Economics* 80 (November): 555–67.
- Halevi, J. 1985. "Switching and Employment." *Eastern Economic Journal* 11 (July–September): 229–34.
- Harcourt, G. C. 1969. "Some Cambridge Controversies in the Theory of Capital." *Journal of Economic Literature* 7 (June): 369–405.
- Harcourt, G. C. 1972. *Some Cambridge Controversies in the Theory of Capital*. London: Cambridge University Press.
- Harcourt, G. C. 1975a. "Capital Theory: Much Ado About Something." *Thames Papers in Political Economy* (Autumn): 1–16.
- Harcourt, G. C. 1975b. "The Cambridge Controversies: The Afterglow." In M. Parkin and A. R. Nobay, eds., *Contemporary Issues in Economics*. Manchester, UK: Manchester University Press.
- Harcourt, G. C. 1976. "The Cambridge Controversies: Old Ways and New Horizons—Or Dead End?" *Oxford Economic Papers* 28 (March): 25–65.
- Harcourt, G. C. 1999. "'Horses for Courses': The Making of a Post-Keynesian Economist." In Arnold Heertje, ed., *The Makers of Modern Economics* Vol. 4. Cheltenham: Edward Elgar.
- Harcourt, G. C. 2000. "Harcourt, Geoff Collin." In P. Arestis and M. Sawyer, eds., *A Biographical Dictionary of Dissenting Economists*. Cheltenham: Edward Elgar.
- Harcourt, G. C. & Hamouda, O. F. 1988. "Post-Keynesianism: From Criticism to Coherence?" *Bulletin of Economic Research* 40 (January): 1–33.
- Harris, D. J. 1973. "Capital, Distribution, and the Aggregate Production Function." *American Economic Review* 63 (March): 100–13.
- Harris, D. J. 1978. *Capital Accumulation and Income Distribution*. Palo Alto, CA: Stanford University Press.
- Hicks, J. 1974. "Perspectives in Twentieth Century Economic Theory: Capital Controversies, Ancient and Modern." *American Economic Review* 64 (May): 307–16.
- Hodgson, G. 1997. "The Fate of the Cambridge Capital Controversy." In P. Arestis and M. C. Sawyer, eds., *Capital Controversy, Post Keynesian Economics and the History of Economics: Essays in Honour of Geoff Harcourt*. London: Routledge.
- Isserman, M. & Kazin, M. 2000. *America Divided: The Civil War of the 1960s*. New York: Oxford University Press.
- King, J. E. 1994. *Conversations with Post Keynesians*. Basingstoke: Macmillan.
- King, J. E. 2002. *A History of Post Keynesian Economics Since 1936*. Northampton, MA: Edward Elgar.
- Kregel, J. A. 1971. *Rate of Profit, Distribution and Growth: Two Views*. London: Macmillan.
- Kregel, J. A. 1973. *The Reconstruction of Political Economy: An Introduction to Post-Keynesian Economics*. London: Macmillan.
- Laibman, D. & Nell, E. J. 1977. "Reswitching, Wicksell Effects and the Neoclassical Production Function." *American Economic Review* 67 (December): 878–88.
- Lee, F. S. 2000a. "Alfred S. Eichner, Joan Robinson and the Founding of Post Keynesian Economics." In W. J. Samuels, ed., *Research in the History of Economic Thought and Methodology* 18c, pp. 9–40.

- Lee, F. S. 2000b. "The Organizational History of Post Keynesian Economics in America, 1971–1995." *Journal of Post Keynesian Economics* 23 (Fall): 141–62.
- Levhari, D. 1965. "A Non-substitution Theorem and Switching of Techniques." *Quarterly Journal of Economics* 79 (February): 98–105.
- Mata, T. 2002. "How Important are Labels? The Label of Post Keynesian Economics." History of Economics Society Annual Meeting 2002 (July). Davis, CA. Unpublished.
- Morishima, M. 1966. "Refutation of the Non-Switching Theorem." *Quarterly Journal of Economics* 80 (November): 520–25.
- Nell, E. J. 1967a. "Theories of Growth and Theories of Value." *Economic Development, Cultural Change* 16 (October): 15–26.
- Nell, E. J. 1967b. "Wicksell's Theory of Circulation." *Journal of Political Economy* 75 (August): 386–96.
- Pasinetti, L. L. 1966. "Changes in the Rate of Profit and Switches of Techniques." *Quarterly Journal of Economics* 80 (November): 503–17.
- Robinson, J. 1953–54. "The Production Function and the Theory of Capital." *Review of Economic Studies* 21 (2): 81–106.
- Robinson, J. 1956. *The Accumulation of Capital*. London: Macmillan.
- Robinson, J. 1971. "The Measure of Capital: The End of the Controversy." *Economic Journal* 81 (September): 597–602.
- Robinson, J. 1975. "The Unimportance of Reswitching." *Quarterly Journal of Economics* 89 (February): 32–39.
- Salanti, A. 1982. "Neoclassical Tautologies and the Cambridge Capital Controversies." *Journal of Post Keynesian Economics* 5 (Fall): 128–31.
- Sale, K. 1974. *Students for a Democratic Society*. New York: Random House.
- Samuelson, P. A. 1962. "Parable and Realism in Capital Theory: The Surrogate Production Function." *Review of Economic Studies* 39 (June): 193–206.
- Samuelson, P. A. 1966. "Summing Up." *Quarterly Journal of Economics* 80 (November): 568–83.
- Samuelson, P. A. 1975. "Steady-State and Transient Relations: A Reply on Reswitching." *Quarterly Journal of Economics* 89 (February): 40–47.
- Samuelson, P. A. & Levhari, D. 1966. "The Non Switching Theorem is False." *Quarterly Journal of Economics* 80 (November): 518–19.
- Students for a Democratic Society. 1962. *The Port Huron Statement*. Port Huron, MI: SDS.
- Sraffa, P. 1960. *The Production of Commodities by Means of Commodities*. Cambridge: Cambridge University Press.
- Solow, R. M. 1955–56. "The Production Function and the Theory of Capital." *Review of Economic Studies* 23 (2): 101–18.
- Solow, R. M. 1963. *Capital Theory and the Rate of Return*. Amsterdam: North-Holland Publishing.
- Solow, R. M. 1975. "Brief Comments." *Quarterly Journal of Economics* 89 (February): 48–52.
- Stiglitz, J. E. 1974. "The Cambridge-Cambridge Controversy in the Theory of Capital: A View from New Haven: A Review Article." *Journal of Political Economy* 82 (July–August): 893–903.
- Swan, T. W. 1956. "Economic Growth and Capital Accumulation." *Economic Record* 32 (63): 334–61.
- Turner, M. 1989. *Joan Robinson and the Americans*. London: M. E. Sharpe.
- Watts, M. J. & Gaston, N. J. 1982–83. "The "Reswitching" of Consumption Bundles: A Parallel to the Capital Controversies?" *Journal of Post Keynesian Economics* 5 (Winter): 281–88.
- Weintraub, E. R. 1991. *Stabilizing Dynamics: Constructing Economic Knowledge*. Cambridge: Cambridge University Press.