MIT and the Other Cambridge

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1. Preliminaries

In 1953 Joan Robinson, at the University of Cambridge, in England, published a challenge to what she chose to call the neoclassical theory of production. She claimed that it did not make sense to use a production function of the form $Q = f(L, K)$,[1] in which the rate of interest or profit (the two terms are used interchangeably) was assumed to equal the marginal product of capital, $\partial Q/\partial K$, for it confused two distinct concepts of capital. The variable K could not represent simultaneously the physical stock of capital goods and the value of capital from which the rate of profit was calculated. A related critique was then offered by Piero Sraffa (1960). In place of the marginal productivity theory of income distribution, Robinson and her Cambridge colleagues, Nicholas Kaldor and Luigi Pasinetti, argued for what they called a "Keynesian" theory of distribution in which

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1. Solow's C has been changed to the more commonly used K.

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the rate of profit was determined by the rate of capital accumulation and the propensities to save out of wages and profits. This challenge was taken up by the MIT economists Robert Solow and Paul Samuelson, who claimed that her objections were unfounded. The resulting controversy, which continued for more than a decade, came to be known as the "two Cambridges" controversy because the main participants, including a host of younger scholars, including many graduate students, were based at MIT and the other Cambridge.

The classic account of this debate is *Some Cambridge Controversies in the Theory of Capital* (Harcourt 1972), an expanded version of an article in the widely read *Journal of Economic Literature* (Harcourt 1969). This book, presenting the controversy as a boxing match involving two camps, is written from the Cambridge perspective, according to which there was an ideological divide.[2] In criticizing marginal productivity theory, Robinson, Sraffa, and their followers saw themselves as pointing out that income distribution was not simply a by-product of the pricing system but was the result of class conflict rooted in the structures of capitalism, including the institution of private property and the existence of entrepreneurial and wage-earning classes (Harcourt 1972, 2). Because the capital controversy was believed to concern the very foundations of capitalism, it served as a crucial factor in the emergence of a distinct "Post Keynesian" identity (see Hamouda and Harcourt 1988). As Tiago Mata (2004) has pointed out, Harcourt's survey played a role in that process by constructing a history of how the controversy looked from the Cambridge end.

In contrast with Harcourt's survey, the present article seeks to establish how the arguments of Robinson, Kaldor, Sraffa, and their followers looked from the perspective of MIT. It is clear why the debate was important to Robinson—it was the foundation for her left-wing Keynesianism, very different from the centrist, "neoclassical synthesis" Keynesianism of Samuelson and Solow—but why did the latter become so involved in the controversy given that their arguments with Milton Friedman's Chicago were more important for economic policy? The article argues that Samuelson's and Solow's interest in capital theory has to be explained in terms of their interest in the techniques that Robinson was using. She developed her arguments about capital theory using a class of linear models that they thought they understood and for the analysis of which they had developed the relevant techniques. Robinson's arguments did represent a challenge to

2. Henceforth "Cambridge" is used to denote the University of Cambridge, UK.

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the way they thought about the economy, but less because they thought in terms of continuously differentiable aggregate production functions than because it raised questions about the relation between such models and the discrete technologies used in linear programming, analyzed in "Market Mechanisms and Maximization" (Samuelson 1966a), circulated as a series of RAND discussion papers in 1949 and 1953,[3] and *Linear Programming and Economic Analysis* (Dorfman, Samuelson, and Solow 1958).

The article also points to a less-known but significant outcome of the controversy—the pathbreaking article by Solow and Joseph Stiglitz (1968) that marked the beginning of a long period when MIT was associated with various forms of "disequilibrium" macroeconomics and the "new Keynesian" macroeconomics (see Backhouse and Boianovsky 2013). This ushered in a new phase in MIT economics, in which Samuelson and Solow played a smaller role in relation to a younger generation (e.g., Peter Diamond, Stiglitz, George Akerlof, Robert Gordon, John Taylor, Stanley Fischer, Alan Blinder, Olivier Blanchard, Michael Woodford, and Gregory Mankiw), though many of these had been supervised by Solow, who remained active in the field. With this transition, the relationship between the two Cambridges changed significantly.[4]

Interactions between MIT and the other Cambridge would have been very different had there not been close links between the two institutions and had the other Cambridge not been home to many who took a "neoclassical" position on capital theory. These links went back to 1948 when Samuelson, on his first sabbatical from MIT, visited Cambridge, meeting Joan and Austin Robinson, Piero Sraffa, Richard Kahn, Richard Stone, and Dennis Robertson, with whom he formed lasting friendships. On his return home, he sought to develop connections between the two institutions, writing to Sraffa, "I should like to return evil for good. Can we rob you of any of your bright young men for a limited time?"[5] Samuelson returned to Cambridge in 1952, and Solow, after his arrival at MIT in 1950, also built strong links with Cambridge economists, including Harry Johnson (whom he had known when they were graduate students at Harvard, and who taught at Cambridge from 1948 to 1956) and Frank Hahn, whose

3. The editor, Stiglitz, cites these as having been circulated in March and June 1949, but omits mention of an addendum not circulated till 1953.

4. There were also significant changes at Cambridge, but these are beyond the scope of a paper focused on MIT.

5. P. A. Samuelson to Piero Sraffa, December 16, 1948, PASP box 70 (Sraffa). The question mark has been added.

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PhD had been supervised by Kaldor at the London School of Economics, who visited MIT in 1956–57, and who was at Cambridge from 1960 to 1967 and from 1972 onward. Hahn and Solow arranged for graduate students and young faculty members from one Cambridge to spend part of their time at the other. The list included Peter Diamond, Christopher Bliss, Tony Atkinson, Joseph Stiglitz, and James Mirrlees. Samuelson and Solow were repeatedly encouraging their Cambridge counterparts to visit MIT or to stop over en route to other destinations in the United States.

In focusing on relations between MIT and Cambridge, the article is picking out one of many links in a transatlantic group of economists that had developed close friendships and collaborations. Though dealing with only one of MIT's many connections with other groups of economists, it serves as a reminder that MIT's identity was as much the result of interactions with other places as the outcome of developments within MIT itself.

2. The Meaning of Capital, 1948–66

Harcourt (1972, 11) dates the two-Cambridges controversy to Robinson's article "The Production Function and the Theory of Capital" (1953). However, Samuelson has recalled that their first exchange on the theory of capital might have been the previous year, when he attended the "secret seminar" in Cambridge (Turner 1989, 267n23). Unfortunately, the details of their exchange on that occasion are not recorded. Solow's first reaction to Robinson's paper came in a letter, dated August 26, 1953, to Harry Johnson. Solow wrote that he had been struggling with her paper, because he thought it was related to a problem on which he and Samuelson had been working. However, he found it impenetrable, and he could not understand a word of it.[6] This elicited a long explanation from Johnson, who noted that the paper was causing confusion in Cambridge as well.[7] He opined that "the point is quite simple once the usual Joan-isms and the signs of mental breakdown have been eliminated, as they have been, we hope, in the version coming out in the Review [of Economic Studies] in February." Robinson's innovation lay not in identifying a new problem, for the dependence on the measured capital stock on the rate of interest was well-known: it was in supplying a solution through "the pedagogically-useful device of discrete techniques," which Johnson explained using a

6. R. M. Solow to Harry G. Johnson, August 26, 1953, RMSP box 65 (H-J).
7. H. G. Johnson to Robert M. Solow, August 31, 1953, RMSP box 65 (H-J).

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one-commodity "corn" economy. What made the paper complicated was that she refused to regard it as a dynamic problem, insisting on expressing it in terms of comparative statics, and it was mixed in with arguments related to her generalization of John Maynard Keynes's *General Theory* about the impossibility of an economy with a falling rate of interest to adjust smoothly to capital accumulation. Johnson's explanation seemed to help, for Solow responded, a month later, by saying that "I think I see what Mrs. Robinson is driving at."[8] He explained that he did not mind the political propaganda, but was "a little put off by the withering attacks on what may be neo-classical stinking fish in England, but bears no visible relation to any doctrine taught on this side of the Atlantic." This exchange shows the importance of Johnson, not usually associated with this controversy, as a liaison between two different ways of thinking.

The correspondence makes it clear that the problem intrigued Solow because Robinson seemed to be making arguments about issues that Samuelson and Solow believed that they understood because it related directly to their recent work. However, it was difficult to understand because several issues were involved. One was aggregation, where the conditions for rigorous aggregation were known to be very restrictive: Samuelson and Solow were, after all, students of Wassily Leontief (1947), who had shown that the conditions for aggregation of production functions were very strict. Another concerned the relationship between models with finite numbers of techniques (input-output or linear-programming models) and ones with continuous substitution between inputs.

Robinson used examples involving three or four distinct techniques because she did not have the mathematics needed to work with large numbers of techniques, and as a result it was easy to get the impression that some of the effects she was finding were the result of having a finite number of techniques. In contrast, Samuelson and Solow used such models because they were potentially computable. The problem of the relationship of "linear-programming" models to models with smooth substitution was one on which Samuelson had been working since he started working at RAND in December 1948. He had explored this problem in several RAND discussion papers, one of which had been the opening paper in a major conference on linear programming that proved immensely important for the community of American mathematical economists (reported in Koopmans 1951; see Düppe and Weintraub 2014). Samuelson, whose main concern was with the comparative statics properties of maximum

8. R. M. Solow to Harry G. Johnson, September 28, 1953, RMSP box 65 (H-J).

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points, had shown that working with discrete numbers of techniques, though it necessitated using different mathematical techniques, did not cause any problems for the underlying economic theory. He and Solow were, therefore, puzzled by Robinson's claims that she was raising problems that were fatal for neoclassical theory. Furthermore, they were working on models with arbitrary numbers of commodities, in which aggregation over capital goods was not required, having published a joint paper (Solow and Samuelson 1953) in which outputs of n goods were functions of n inputs; in this paper they postulated differentiable production functions, but there was no aggregation over commodities. This meant that they failed to recognize themselves in Robinson's picture of neoclassical economics.

The disagreement became public in the mid-1950s, when Solow (1955) published a response to Robinson's article. He took the point of her article to be proposing a specific, somewhat old-fashioned, way to measure capital in terms of labor units plus interest payments, *not* to be arguing that capital could not be measured. Like Johnson,[9] he understood Robinson to be turning to linear-programming models to solve the well-known problem that the quantity of capital depended on the rate of interest. He challenged this and an alternative approach to capital measurement offered by David Champernowne (1953), by questioning whether it was necessary to have a concept of "capital-in-general" at all (Solow 1955, 101). In explaining the reasons that capital could not rigorously be aggregated, he used production functions of the form $Q = f(L, K)$,[10] an idea that he linked to the Cambridge philosopher and mathematician Frank Ramsey, but he was confident that his results would hold for the case of discrete activities. Noting that "everyone who invents linear programming these days seems to be charmed by it," Solow (1955, 108) averred that using linear-programming technologies solved nothing, for "only in very special cases will it be possible to define a consistent measure of capital-in-general." Contrary to common perceptions, Solow was arguing *against* measuring the aggregate capital stock. His argument became even stronger if one took account of "the intertwining of past, present and future": there was, he contended, something foolish about a theory of capital built on the assumption of perfect foresight" (102).[11] Robinson (1955), however, believed that Solow

9. H. G. Johnson to Robert M. Solow, August 31, 1953, RMSP box 65 (H-J).

10. Solow's C has been changed to the more commonly used K.

11. These were criticisms that Cambridge economists liked to make of neoclassical theory, but Solow was using them to defend his own work against Robinson.

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had missed the point, because he had not considered how capital goods were produced.

For Samuelson and Solow, their alternative to the model of production that Robinson (1956) proposed in *The Accumulation of Capital* was not the "Solow-Swan model" (independently derived by Solow and Trevor Swan [1956]) but more general models with heterogeneous capital goods. Solow (1956, 1957) made it clear that the one-sector growth model was never intended as more than a useful heuristic device for analyzing aggregate data. Because they accepted that capital goods were heterogeneous, they were, in the mid-1950s, working on models involving many capital goods (Samuelson and Solow 1956; Dorfman, Samuelson, and Solow 1958), from which they concluded:

> Even though there is no such thing as a single abstract capital substance that transmutes itself from one machine form to another like a restless reincarnating soul, the rigorous investigation of a heterogeneous capital-goods model shows that over extended periods of time an economic society can in a perfectly straightforward way reconstruct the composition of its diverse capital goods so that there may remain great heuristic value in the simpler J. B. Clark-Ramsey models of abstract capital substance. (Samuelson and Solow 1956, 537–38)

Thus when their debate with Robinson continued in correspondence, Samuelson claimed that his work with Solow showed how any number of heterogeneous capital goods could be handled, sending Kahn (more likely to understand it than Robinson) a copy of *Linear Programming and Economic Analysis*.[12] Robinson's reply was that they were still missing her point: "You and Solow are a case of None so deaf as he who will not hear—so I shan't make myself hoarse shouting at you."[13] Despite this threat to ignore them, their exchanges heated up, in print and in correspondence, with over one hundred letters being exchanged, expressing increasing exasperation with each other, and with the three protagonists being joined by mostly younger colleagues on both sides. In the course of this, Samuelson (1962) sought to show that a Ramsey production function could serve as a surrogate for a more rigorous model; Solow (1963) tried to finesse Robinson's problem by switching to a model of intertemporal choice, in which investment could be measured simply in terms of oppor-

12. P. A. Samuelson to Joan Robinson, February 3, 1958, PASP box 63 (Robinson, Joan [1]).
13. J. Robinson to Paul A. Samuelson, January 22, 1958, PASP box 63 (Robinson, Joan [1]).

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tunity cost (a sum of consumption goods); David Levhari (1965), an MIT student, tried to use a nonsubstitution theorem proved by Samuelson to show that reswitching—the phenomenon where, as the rate of interest falls, technique α is replaced by technique β, which is then replaced by α—could not take place. Unfortunately, all these defenses failed in that they turned out to rely on particular assumptions: that the capital-labor ratio used to produce all commodities was the same.[14]

The denouement is widely considered to have come in 1966 when, in summing up a symposium on "paradoxes in capital theory," involving papers by, among others, two economists with recent Cambridge doctorates (Pasinetti and Pierangelo Garegnani) and three with recent MIT doctorates (Levhari, Edwin Burmeister, and Eytan Sheshinski), Samuelson (1966b, 582–83) conceded that the position he and Solow had been defending was wrong.

> Lower interest rates may bring lower steady-state consumption and lower capital/output ratios, and the transition to such lower interest rate can involve denial of diminishing returns and entail reverse capital deepening in which current consumption is augmented rather than sacrificed.
>
> There often turns out to be no unambiguous way of characterizing different processes as more "capital-intensive," more "mechanized," more "roundabout," except in the ex post tautological sense of being adopted at a lower interest rate and involving a higher real wage. Such a tautological labeling is shown, in the case of reswitching, to lead to inconsistent ranking between pairs of unchanged technologies, depending upon which interest rate happens to prevail in the market.
>
> 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.

This statement by Samuelson marked MIT's recognition of Robinson's technical point, but, to the frustration of their critics, Samuelson and Solow did not stop using aggregate production functions. The reason was that they had never considered Ramsey production functions any more than a useful heuristic device that was almost essential in doing empirical

14. This paragraph summarizes a very long and torturous debate; readers should consult other surveys for the details.

work, where it was necessary to work with aggregate data. The "Solow-Swan model" abstracted from heterogeneity of capital in the same way that it abstracted from problems of unemployment, money, and finance. It is significant that, aside from a footnote in which he discussed aggregation over firms, not commodities (Solow 1956, 79n7), the phrase "aggregate production function" is used only in the context of data analysis (Solow 1957, 317). The fact that Solow had himself "thrown up still further obstacles" (312) meant that, as far as he and Samuelson were concerned, the problem Robinson identified was simply one of *many* problems surrounding the Ramsey production function.

One reason that Samuelson and Solow believed that the revaluation of the capital stock caused by changes in the rate of interest was not the most important of these problems is that they were concerned, throughout, with dynamics. Neither Robinson, whose capital theory involved steady-state comparisons, nor Sraffa, whose model abstracted altogether from time, offered any formal dynamic model of capital accumulation, whereas Samuelson and Solow insisted on using properly dynamic models that were not necessarily on steady-state growth paths.[15] As Solow wrote to Hahn,

> The Harrod-Domar legacy of paying attention only to equilibrium paths is by now an obstacle. All these ad hoc stability statements about what happens off such a path are useless without an explicit causal dynamics. But if we had the latter, then the equilibrium paths would appear as certain special motions and one could deal with them directly.[16]

This was exactly the approach taken by Dorfman, Samuelson, and Solow (1958, 329), who concluded that there was nothing special about balanced-growth paths: "General balanced growth is not even intertemporally efficient, let alone somehow special among efficient paths." Efficiency required balanced growth with capital goods in the optimal (von Neumann) proportions.[17] So, from this perspective, the significance of the "Solow model" was that it was a model in which, for all its simplifications, the dynamics were fully specified in that it gave a formal account of what would happen when the economy was not experiencing steady-state growth.

15. Sraffa's book cannot be discussed in detail here. To economists immersed in Leontief models and the von Neumann growth model, Sraffa's model looked familiar.

16. R. M. Solow to Frank H. Hahn, March 23, 1959, RMSP box 55 (E-H).

17. These are the intensities with which different processes are used found in von Neumann 1945.

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Part of the problem with the capital theory debate was that so many issues were intertwined. Robinson turned out to be raising an issue whose significance Samuelson and Solow had not understood, but in explaining it, she introduced many complications that distracted attention from the central problem. Samuelson and Solow believed that they possessed the mathematical techniques necessary to analyze the problems she was identifying, but, even so, they found it necessary to abstract from problems she thought important.

3. Two Views of Distribution, 1958–68

If Robinson's critique were accepted, it was necessary to find an alternative theory of distribution. Inspired by the "widow's cruse" theory found in Keynes's *Treatise on Money* (1930), and the work of the Polish economist Michał Kalecki, several Cambridge economists tried to develop a "Keynesian" theory in which the rate of profit was determined by aggregate demand. Such a theory had been developed by Hahn (1972) in the PhD dissertation he wrote under Kaldor's supervision and submitted to the London School of Economics in 1948. However, the version that attracted interest was Kaldor's "Alternative Theories of Distribution" (1955). Here, Kaldor viewed the theories of David Ricardo and Karl Marx and "neoclassical" theories involving marginal productivity and monopoly power, before offering the Keynesian theory that he supported. The basic idea is that if there was a reverse-L-shaped aggregate supply curve, rises in aggregate demand would produce rises in output (the Keynesian multiplier) up to the kink (full capacity); after that point rises in demand would cause prices to rise relative to wages. If, as Kaldor believed was the case, the propensity to save out of profits (s_p) was higher than the propensity to save out of wages, this process would raise savings, choking off the rise in demand. In the simplest case, where workers consumed all their income, equilibrium implied that $I = s_p P$ (where I is investment and P is profits). Dividing both sides by the capital stock, K, yields a relationship between the rate of profit and the growth rate of the capital stock, $r = g/s_p$. The ideological significance of this equation was that its distribution was, as for Ricardo and Marx, the outcome of an asymmetric relationship between capitalists and workers.

On September 4–11, 1958, the International Economic Association held a conference on the island of Corfu, on the theory of capital, at which Cambridge was represented by Sraffa, Kaldor, and Austin Robinson, and

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MIT by Samuelson, Solow, and Evsey Domar (Champernowne, also present, had moved from Cambridge to Oxford). Immediately afterward, Solow wrote to Hahn, "Nicky [Kaldor] and I had an eight-day battle royal, in the course of which I came to have considerable respect and even a certain affection for him."[18] The cause of this battle was the growth model that Kaldor had constructed around his Keynesian theory of income distribution.

One reason for their disagreement was that Kaldor, who had been familiar with capital measurement problems since the 1930s, chose not to work with an aggregate production function but with what he termed a "technical progress function" in which the rate of growth of output was a function of the growth rate of the capital stock, as shown in figure 1. If, as Kaldor believed was the case, this was nonlinear, it could not be integrated to obtain an aggregate production function, for output would depend not just on the capital stock but on the time path over which it had been accumulated. However, to solve his growth model, Kaldor worked with a linear technical progress function. As Solow explained to him, not only could a linear technical progress function be integrated, it was equivalent to a Cobb-Douglas production function.

That was not the only reason for disquiet over Kaldor's model. His Keynesian distribution theory required full employment, but he also wanted Keynesian unemployment to be possible, so he sought to reconcile the two with a theory of demand that involved elements of monopoly (Kaldor 1961, 195–203). In Solow's view, "Nicky's model simply will not stand up under scrutiny. When it is not self-contradictory it is more or less completely arbitrary."[19] In the next few years, the theory provoked considerable debate.

Shortly afterward, Kaldor, assisted by Mirrlees, who was to become a frequent visitor to MIT (Kaldor and Mirrlees 1962), provided a more rigorous model based on improved assumptions about technology but adopting the same assumptions about saving and investment. Kaldor's theory of distribution was challenged by Pasinetti (1962) on the grounds that it was wrong to specify propensities to save out of profits and wages. If workers saved, they would receive a share of profits, undermining the sociological basis for assuming different propensities to save out of these two types of income. Remarkably, Pasinetti showed that, if capitalists and

18. R. M. Solow to Frank H. Hahn, September 30, 1958, RMSP box 55 (E-H).
19. R. M. Solow to Frank H. Hahn, March 23, 1959, RMSP box 55 (E-H).

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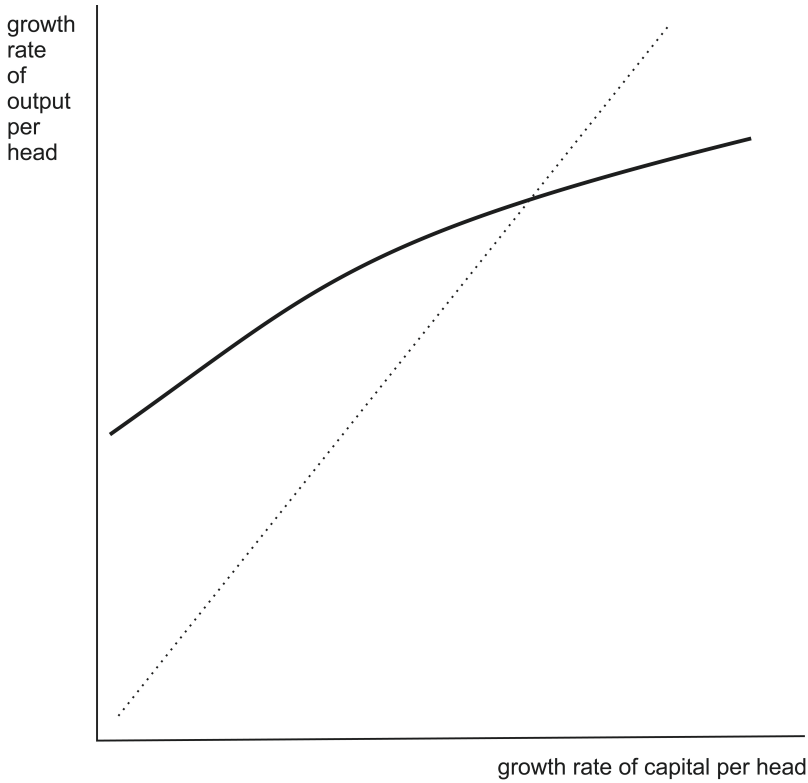

Figure 1 Kaldor's technical progress function. Drawn by author, based on a figure Kaldor uses in virtually all of his papers on growth

workers had different savings propensities, allowing for the fact that workers earned interest (profits) on their savings simplified the model. Even if workers saved, the rate of profit would depend only on the capitalists' propensity to save and the rate of capital accumulation. This was taken up by Samuelson and Franco Modigliani, who had by then moved to MIT (Samuelson and Modigliani 1966), who pointed out not only that Pasinetti's result required that the workers' propensity to save was sufficiently small but also that there was a dual case where profits depended solely on the workers' propensity to save. In contrast to Kaldor and his colleagues, Samuelson and Modigliani saw Kaldorian and marginal productivity theories not as alternatives but as parts of a more complicated theory.

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The relation between marginal productivity theory and what he called the "Cambridge" wage (output minus profits determined according to the Kaldor formula) was taken up by Joseph Stiglitz in the year that he spent at Cambridge, receiving supervision from Hahn as well as advice from Solow by mail.[20] He produced a draft, involving imperfect competition and Kaldorian savings propensities, putting them together with a standard model of production involving a production function. Solow pointed out that adding differential savings propensities was entirely consistent with traditional Keynesian theory: "If savings are sensitive to the distribution of income, fine. If there are other variables, like wealth, in the saving function, fine. The theory doesn't mind."[21] In short, there was no reason why the Kaldorian theory should be seen as an alternative to traditional theory.

What makes this development significant is that through looking for ways in which the "Cambridge wage" might differ from the full-employment marginal product of labor, Stiglitz began to develop a theory of macroeconomic disequilibrium. Solow encouraged him, argued that there was great potential in using imperfect competition in macroeconomics, but saying that Stiglitz needed to work it out with more care, paying greater attention to the dynamics describing out-of-equilibrium behavior. In his reply, Stiglitz, referring to the effects of lags in consumption and production, asked Solow, "Does all of this boil down to what assumptions we make about adjustment mechanisms and speeds?"[22] Solow agreed with this, though he emphasized that a wide variety of outcomes was possible.[23] He continued:

> The hard problem is to formulate a good theory of what micro-behavior corresponds to a macroeconomic deficiency of effective demand; the problem is to reconcile the assumption that aggregate output is limited by effective demand with the worm's-eye view that any single approximately-competitive firm whose price exceeds marginal cost can apparently increase its profits by hiring more labor (even with a slight increase in the real wage) and selling more. It doesn't surprise an old Keynesian like me that except in a singular case there should be either unemployment or inflation.

20. He was also appointed a supervisor from the other side of the Cambridge faculty, and had discussions with Robinson.

21. R. M. Solow to Joseph E. Stiglitz, September 28, 1965, RMSP box 60 (R-S).

22. J. E. Stiglitz to Robert M. Solow, October 3, 1965, RMSP box 60 (R-S).

23. R. M. Solow to Joseph E. Stiglitz, October 14, 1965, RMSP box 60 (R-S).

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In an undated "note on the marginal productivity wage and the Keynesian wage at full employment," Stiglitz took up these points, exploring assumptions about wage and price dynamics and using a labor demand curve that was kinked at full employment, to reach the conclusion:

> The point again is clear: if there is unemployment in an economy with profit maximization and a competitive labor market, but in which adjustments do not occur instantaneously, whether the real wage will eventually be zero, equal to the marginal product, or somewhere in between depends on the adjustment mechanism assumed. We have explored two quite striking cases; one in which depending on the relative speeds of adjustment, the real wage is either zero or equal to the marginal product of labor, (in the competitive case), and the other in which the real wage [is] determined completely by initial conditions.[24]

Starting with the problems posed by the capital controversy, Stiglitz, guided by Solow (and presumably in consultation with Hahn), developed an approach to macroeconomics that they modestly described as "a slightly novel theory of the determination of aggregate output and employment in the short run" (Solow and Stiglitz 1968, 537).[25]

4. Countering the New Classical Macroeconomics after 1970

Research into the constellation of growth, capital, and income-distribution theory framed by the two-Cambridges controversy did not lose momentum till well into the 1970s. Solow's *Growth Theory* (1970) was widely read, and textbooks appeared throughout the decade, by the MIT students Henry Wan (1971), Burmeister and A. Rodney Dobell (1970), while those committed to the "classical" approach inspired by Sraffa, whose *Production of Commodities by Means of Commodities* (1960) offered a theory of value that could be combined with the Keynesian theory of distribution to provide a theory that dispensed altogether with marginal analysis, continued their criticisms of neoclassical theory. The definitive summary of the issues in the controversy, at least from the neoclassical side, was arguably

24. J. E. Stiglitz, 1965, A note on the marginal productivity wage and the Keynesian wage at full employment, RMSP box 60 (R-S), last page.

25. Here the focus is on Stiglitz. The episode is discussed in relation to the development of Solow's thinking by Michaël Assous (2013). This literature is discussed in detail in Backhouse and Boianovsky 2013.

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Capital Theory and the Distribution of Income (1975), in which Christopher Bliss, a Cambridge student who had spent 1963–64 at MIT, framed the issues in terms of a rigorously formulated intertemporal general equilibrium model, resolving the apparent paradox of reswitching by expanding the set of steady states to include ones in which rates of profit were not constant, and distinguishing clearly the problems of dynamic investment processes from comparisons of steady states. However, the momentum was lost: not only were problems now understood, but it was clear, at least to most economists, that the theory had little light to shed on the problems of stagflation that erupted after 1973–74. Indeed, the most dramatic productivity shock for a generation provided renewed scope for using Solow's aggregative model and the associated notion of total factor productivity to work out why productivity had fallen so far: the need for a practical way to measure contributions to productivity growth trumped concerns with the conceptual precision.

However, although the problem that had motivated Stiglitz to start working on the disequilibrium macroeconomics no longer resonated, the type of modeling that he and Solow had developed was perceived to have traction, even though their paper received less attention than works by other economists. The importance of adjustment speeds for macroeconomic equilibrium came to be associated not with Solow and Stiglitz but with Axel Leijonhufvud (1968), who had got to the idea by reexamining the foundations of Keynesian economics. The idea of a macroeconomic equilibrium with rationing, a dimension of Solow and Stiglitz's more complex model, came to be associated with Robert Barro and Herschel Grossman (1971), who discussed explicitly the microfoundations that Solow and Stiglitz had thought too obvious to need explicit statement. Though this literature came, in retrospect, to be equated with "fixed-price" models, it was seen by some as essential to explain stagflation, for it strained credibility to suggest that markets were in equilibrium during the 1970s (see Backhouse and Boianovsky 2013).

One reason that interest in capital theory declined was the emergence in the 1970s and 1980s of new theoretical challenges to the Keynesianism associated above all with Samuelson, Solow, and Modigliani, and their Yale colleague James Tobin. In challenging Keynesian policy prescriptions, the new classical macroeconomists (led by Robert Lucas, Thomas Sargent, Robert Barro, Fynn Kydland, and Edward Prescott) raised new theoretical issues. Rational expectations not only challenged the foundations of Keynesian policy analysis but also posed serious conceptual problems for econometricians, prompting a reconsideration of the relationship

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between macroeconomic theory and econometric modeling. The argument that models must assume consistent rational behavior on the part of economic agents (an assumption that not even Milton Friedman had been willing to make) and that markets must be modeled as being in equilibrium meant that Keynesian macroeconomics had to be defended in new ways.

Standing against this trend, Solow continued to teach disequilibrium macroeconomics, the central ideas of which went back to Stiglitz's time at Cambridge, and he trained many of those who were instrumental in forging a macroeconomics that reconciled the analytical innovations of Lucas with the Keynesian observation that markets might not work as efficiently as in the Lucasian world. Economists linked to MIT, including Stiglitz, Peter Diamond, and Michael Woodford, showed that asymmetric information, imperfect competition, and search theory could generate results sharply at variance with models of perfect competition.

However, even if MIT economists were generally lined up against the new classical/real business cycle view of the world, these developments were never associated with MIT in the same way that the neoclassical stance on capital theory had been. Though the number of high-profile "new Keynesians" coming out of MIT was high, too many economists from Columbia,[26] Princeton, Harvard, and Berkeley were involved, not to mention a very significant group of French economists, for this to be possible. If Robinson's debates with Samuelson and Solow had been central to the capital-theory phase of the MIT-Cambridge relationship, the close relationship between Hahn and Solow was central to the new phase. Not only did Cambridge graduate students come over to MIT, and vice versa, but Hahn and Solow embarked on a long project to counter the new classical macroeconomics, culminating in their jointly authored book, *A Critical Essay on Modern Macroeconomic Theory* (1995).

5. Conclusions

The capital controversy was important for the group of Cambridge economists challenging Samuelson and Solow, for it helped establish the identity of those who came to identify themselves as "post-Keynesian" (Mata 2004).[27] They increasingly developed a Keynesianism that was politically and conceptually very different from the Keynesian orthodoxy in which

26. The influence of Edmund Phelps pervaded this literature.

27. The term *Post Keynesian* had also been used more literally in the early 1950s to describe work that came after Keynes, contributing to the Keynesianism that was later challenged by those appropriating the term in the 1970s.

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Samuelson and Solow were significant players. For around twenty years, "Cambridge" came to be identified with this group, centered on Robinson, Sraffa, and Pasinetti, despite the presence at Cambridge of many influential Keynesians whose intellectual commitments were closer to those of their counterparts at MIT.

However, whereas their Cambridge counterparts might focus on the political element, the MIT economists were motivated much more by technical issues. Samuelson (1962, 193), in a paper dedicated to Robinson on the occasion of her "memorable" visit to MIT, explained that the "MIT school" was often seen to involve the use of the tools of "modern linear and more general programming" to analyze growth in models with heterogeneous capital goods. Because of the methods she chose to use, Robinson engaged directly with the linear models with which Samuelson and Solow hoped to operationalize general equilibrium theory. Neither the passion with which Samuelson and Solow engaged with the arguments of Robinson and her colleagues, nor the way in which they argued, can be understood apart from their prior work on linear programming and multisector modeling. *Linear Programming and Economic Analysis* and the papers that led up to it were important because they analyzed precisely the techniques that Robinson used to make her case about the measurement of capital. However, though this book contained the first published formulation of the theorems about efficient capital accumulation that Samuelson and Solow considered their main contribution to the theory of growth, it was not mentioned in the best-known account of the controversy (Harcourt 1972).[28]

A further reason for their engagement concerned the relationship between different types of model. Taking Solow as exemplifying the MIT school, Samuelson (1962, 193) opined that

> one might almost say that there are two Solows—the orthodox priest of the MIT school and the busman on a holiday who operates brilliantly and without inhibitions in the rough-and-ready realm of empirical heuristics. Just as red wine and white wine are both good, so are both Solows of vintage quality. But if I were forced to choose between red and white wine, I for one would reveal a preference for the red.

The important characteristic Samuelson identified here was the simultaneous use of two different types of model, the choice being made accord-

28. Harcourt did rectify this neglect in other work.

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ing to the problem in hand. Robinson appeared to be challenging the crucial result, demonstrated by Samuelson in the 1940s, that lay beneath this modeling strategy: that though the mathematics of linear inequalities might look very different from the differential calculus traditionally used by economists, they were consistent with each other.

The controversy between the two Cambridges eventually came to be seen by MIT economists (and most of the economics profession) as a waste of time. Samuelson and Solow become increasingly frustrated by Robinson's repetition of what they saw as the same points, and they published replies to her work out of a sense of duty. They found Kaldor's models completely unconvincing. However, the controversy had an important spin-off: the development of disequilibrium theory, which Solow continued to teach to generations of MIT graduate students whose work was a significant input into what became the "new Neoclassical synthesis" codified by Woodford (2003).

The story of MIT's relationship with Cambridge is one of the passing of the generation associated with the Keynesian revolution. On the Cambridge side, the end of the controversy was connected to the departure of Robinson, Kaldor, Sraffa, and Kahn, all of whom had done their main work in the 1920s and 1930s, and for whom Samuelson, in particular, had immense respect.[29] On the MIT side, though Samuelson and Solow remained active, a younger generation became more important. After Hahn's appointment as a professor at Cambridge and with the rise of monetarism and the new classical macroeconomics, strongly opposed by both Hahn and Solow, the dominant strand of MIT-Cambridge interaction became one of cooperation against Chicago and Minnesota rather than competition with each other.

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29. Solow never understood Samuelson's enthusiasm for Sraffa's work.

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