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Never Stand Still

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## 1. Introduction: Mainstream Views of the Capital Theory Controversy

Roger Backhouse begins his essay on “MIT and the Other Cambridge” (Backhouse, 2014; hereafter RB with page numbers only) citing Joan Robinson’s “challenge to what she chose to call the neoclassical theory of production” (RB, p. 252).<sup>3</sup> His title referred, of course, to Robinson’s protagonists at the Massachusetts Institute of Technology; in particular, Paul Samuelson and Robert Solow.<sup>4</sup> After developing his thesis that disequilibrium macroeconomics emerged as a by-product of the capital theory controversy, Backhouse concludes with the observation: “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” (RB, p. 269).

Robinson’s views changed over the course of the debate, as she gave up the project of analyzing the process of accumulation in a given state of knowledge. It is therefore worth asking if the other part of her critique, which she never abandoned, was ever satisfactorily answered by that majority of mainstream theorists who concluded that the capital theory controversy did not constitute a fundamental critique of general equilibrium theory. The problem of getting into equilibrium—the basis for Robinson’s distinction between history and

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<sup>3</sup> The reference is to Robinson (1953-54). For histories of the ensuing capital theory controversy, see Harcourt (1969, 1972) and, more recently, Cohen (2010) who draws parallels with earlier debates at the turn of the twentieth century, involving Eugene von Böhm-Bawerk, J.B. Clark, Irving Fisher, and Thorstein Veblen; and during the 1930s when Frank Knight, Friedrich von Hayek, and Nicholas Kaldor were the main protagonists.

<sup>4</sup> Robinson may have had other targets in mind when she wrote her “challenge”; namely, the marginal productivity theory of wages found in Hicks (1932). Her contemporaneous 1953 “Lecture Delivered at Oxford by a Cambridge Economist” (in Robinson, 1973) was, of course, also aimed at her Oxford colleagues, and is of particular interest for our purposes. There, she “sets out her views on the nature of equilibrium, of how in her opinion you cannot get into it, or even tend toward it ... [together with a] discussion of the nature of time ... more fresh and exciting (and insightful even)” (Harcourt, 1996, p. 324) than what was to be found in her later work.

equilibrium—is now seen as very likely insurmountable and it was on this issue that she stuck to her guns.<sup>5</sup>

Frank Hahn was among the mainstream majority Backhouse refers to, although he expressed reservations about the theory he was defending. As the controversy subsided, Hahn described the critique with memorable imagery:

The ease with which so much current critique of General Equilibrium analysis can be countered is potentially dangerous...the citadel is not at all secure and the fact that it is safe from a *bombardment of soap bubbles* does not mean that it is safe. Fortunately, those “inside” have begun to build new walls and to lay new foundations (Hahn, 1981, p. 129, emphasis added).

The purpose of this note is to argue that the capital theory controversy was not a waste of time *from the point of view of mainstream theory* precisely because, on close examination, the negative part of Joan Robinson’s critique has, in fact, never been answered. Hahn’s hoped for “new foundations” were never laid down.

## 2. Two Different Aspects of Robinson’s Critique

Less than half the text of Robinson (1953-54) was reprinted in the second volume of her *Collected Economic Papers*, together with a “Postscript” (Robinson, 1960, pp. 114-131). This sustains Backhouse’s observation that her analysis of choice of technique 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” (RB, p. 256). It is indeed remarkable how easily Robinson extracted what she called this “negative part” (Robinson, 1960, p. 130), adding but a single connecting paragraph to link various sections of the original—almost as if there had always been two articles, joined together in publication. The “constructive parts are better done in my book”, she wrote, referring to *The Accumulation of Capital* (Robinson, 1956). Developed with the aid of diagrams, these new results were later brought to prominence in Harcourt (1969, 1972), “the classical account of this debate” (RB, p. 253).<sup>6</sup> As for the “negative” part, Robinson had reworked it completely in “Accumulation and the Production Function” (Robinson, 1959) to show “how the

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<sup>5</sup> The relevant literature concerns the problem of achieving equilibrium in dynamic models with saddle-path stable solutions. Common knowledge of the structure of the economy and of the rationality of all its agents is generally insufficient (Evans and Guesnerie, 2005, p. 226).

<sup>6</sup> A formal treatment of Robinson’s “productivity curves”, with links back to Harcourt’s survey, is found in Salvadori (1996, pp. 243-45). For related work, see Gram (1976). Concerning Harcourt (1972), Harry Johnson was ambivalent, seeming to praise the book as a “Cambridge explanation, justification, and vindication” (Johnson, 1975, p. 1083) while applauding Mark Blaug’s assessment of the controversy as “properly scathing about the ‘essentialism’ of Harcourt and Company” (*ibid*).

neoclassical production function can be rescued if we bring the Keynesian conditions to its aid” (Robinson, 1960, p. 131).<sup>7</sup>

Only Robinson’s constructive contribution to the problem of choice of technique attracted the attention of mainstream theorists. With the further stimulus provided by Sraffa (1960), the two phenomena of ‘reverse capital deepening’ and ‘reswitching of techniques’ came to the fore, resulting in a Symposium (1966) prompted by an ‘impossibility’ theorem (Levhari, 1965) which had drawn the scrutiny of Pasinetti (1966) and was then shown to be false. Contributors to the Symposium credited Robinson for her discovery of capital theoretic anomalies, referring to Robinson (1953-54, p. 106), where she had written of a “curious possibility...pointed out to me by Ruth Cohen”, and also to Robinson (1956, pp. 411-18) and Sraffa (1960). The Symposium concluded with “A Summing Up” (Samuelson, 1966) in which the only reference to Robinson concerns “a general blue-print technology model of Joan Robinson and MIT-type”, acknowledging her use of models with a discrete number of techniques, as in linear programming. When Backhouse notes that Samuelson (1966) marked “MIT’s recognition of Robinson’s technical point” (RB, p. 259), he is clearly referring only to what she regarded as her constructive contribution.

Robinson (1953-54) had adopted the “pedagogically useful device of discrete techniques” (RB, p. 255; quoting a letter from Harry Johnson to Robert Solow by way of explaining what she was up to) in order to show that the value of the capital stock depended on the rate of interest. This was not controversial, but the question lingered as to the role of smooth substitution in sustaining traditional results concerning the inverse relationship between the rate of interest and the capital intensity of production. Samuelson (1962) addressed this question using a pared-down version of the model presented in Samuelson and Solow (1956) which, although it allowed for complete heterogeneity of capital goods, began with a defense of the “heuristic value of the simpler J.B. Clark-Ramsey models of abstract capital substance” (Samuelson and Solow, 1956, p. 538).<sup>8</sup> Samuelson thanked Pierangelo Gargegnani “for saving me from asserting the false conjecture that my extreme assumption of equi-proportional inputs in the consumption and machine trades could be lightened and still leave one with many of the Surrogate [production function] propositions” (Samuelson, 1962, p. 202, n. 1). This footnote may be seen as prelude to the above mentioned Symposium wherein the “extreme assumption” was relaxed in various ways to reveal so-called “Paradoxes in Capital

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<sup>7</sup> Robinson’s dynamics were always informed by her understanding of Keynes. One can also find in both *The Accumulation of Capital* (Robinson, 1956) and its sequel (Robinson, 1962) a shift toward the analysis of *cyclical growth* along the lines of Goodwin (1967) and Kalecki (1968). See the “Introduction” by Harcourt and Kerr (2013) to the Palgrave Classics Edition of *The Accumulation of Capital*, pp. xix-xxv.

<sup>8</sup> Samuelson had long before concluded in work originally done at the Rand Corporation that “discrete numbers of techniques, though it necessitated using different mathematical techniques, did not cause any problems for the underlying economic theory” (RB, p. 257).

Theory”, the title of the Symposium, notwithstanding the direct stimulus provided by the reaction to Levhari (1965).

In the final paper of the Symposium, Samuelson nailed down exactly what he saw as his own error: “The reversal of direction of the ( $i$ , NNP) relation was, I must confess, the single most surprising revelation from the reswitching discussion... I had wrongly confused concavity of [the production-possibility frontier] with concavity of the ( $i$ , NNP) steady-state locus” (Samuelson, 1966, p. 577, n. 6). Crucially, there is no suggestion here that the MIT economists had found any reason to reconsider their analysis of “Efficient Programs of Capital Accumulation”, the title of the second of two chapters of *Linear Programming and Economic Analysis* (Dorfman, Samuelson, and Solow, 1958; hereafter, DOSSO), cited by Backhouse (RB, pp. 254, 258, and 260). As for ending the controversy, Backhouse credits Christopher Bliss (1975) for “the definitive summary of the issues..., at least from the neoclassical side” (RB, pp. 265). See also Burmeister (1980).

Backhouse further explains the mainstream assessment of the controversy by claiming that Robinson “refused to regard [capital accumulation] as a dynamic problem, insisting on expressing it in terms of comparative statics” (RB, p. 256). This makes sense if the presumed “dynamic problem” is the one set forth in models of intertemporal general equilibrium where nothing is learned about the *process* of accumulation from a direct comparison of steady states. If that were all that Robinson was doing, the MIT economists would have been justly incredulous at the thought of being criticized by her for their ‘dynamics’ when she was using a variation on comparative statics.<sup>9</sup> And yet Robinson’s analysis is all about ‘dynamics’ as she saw it—from a Keynesian perspective.

In “Accumulation and the Production Function” (Robinson, 1959), one finds a clear statement of the negative part of her original critique, acknowledged to have been “clumsy and unconvincing” when it appeared as Chapter 14 of Robinson (1956). Her description of an equilibrium path of accumulation is perfectly consistent with formal modelling:

The who's who [of concrete capital goods, including stocks and work in progress] and the values for all past and future dates are implicit in the [present] situation ..., and the *whole history, backwards and forwards, can be seen at any moment in it* (Robinson, 1959, pp. 435-36; emphasis added).

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<sup>9</sup> We thank one of our referees for insisting on this.

This linking of past, present, and future underlies the principle of optimality in dynamic programming (Bellman, 1957, p. 83).<sup>10</sup> What Robinson found unacceptable about the analysis was its reliance on realized expectations:

There is one set of expectations that will be compatible with continuing equilibrium. At any particular moment the past course of the rate of accumulation (the ratio of net investment to the stock of capital) has been such as to make the course of profits such that (being foreseen) they have caused the capital to be embodied in forms that offer employment to just the amount of labour that is available. And they must continue to be such as to fulfil this requirement (Robinson, 1959, p. 434).

And so she concludes:

The requirement that there is in existence, whenever we break into the story, an equilibrium stock of capital (because there have been correct expectations in the past) deprives our exercise of application and reduces it to a mere pastime.

There is essentially no difference between this passage and one in the original critique:

[Equilibrium] entails that there have been no events over the relevant period of past time which have disturbed the relation between the various valuations of a given stock of goods, and that the human beings in the situation are expecting the future to be...entirely devoid of such disturbing events... When an unexpected event occurs, the three ways of evaluating the stock of goods part company and no amount of juggling with units will bring them together again (Robinson, 1953-54, pp. 83-84).

Robinson's view of equilibrium as essentially unobtainable (and therefore unobservable) is also very much in line with the conclusion reached by Franklin Fisher:<sup>11</sup>

There is a tendency to confuse the view that if one is not at an equilibrium, one will not stay where one is, with the view that one must approach equilibrium—and that is quite a different and much harder proposition... There is a big gaping hole in the center of what economists know, namely, the question of what

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<sup>10</sup> Caputo (2005, pp. 511-565) provides a thorough development, including a full discussion of intertemporal duality, which distinguishes his text from others in the field.

<sup>11</sup> Fisher regarded Robinson's criticisms as politically motivated even when a dispassionate reader might not be able to discern any difference between their points of view concerning the problem of approaching or getting into equilibrium. Fisher remarks upon Robinson's ideological position in his book on aggregation (Fisher, 1992, pp. xi and xxi n. 2).

happens out of equilibrium and whether we ever get close to equilibrium...  
(Fisher, 1989, p. 320).

How, then, did the MIT economists finesse this problem, leaving Robinson to wonder why they would not answer her, even after she had distanced herself from her own positive contribution, which she came to regard as unimportant (Robinson, 1975) and which they saw as nothing more than an interesting complication to be dealt with using appropriate mathematical techniques?<sup>12</sup>

### 3. A Closer Look at *Linear Programming and Economic Analysis*

Denying the *comparison* of steady states much, if any, theoretical relevance is the position taken by Bliss (1975) and Burmeister (1980).<sup>13</sup> The same is true of DOSSO, where output is always at a point on an economy's short run production-possibility frontier. It is therefore

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<sup>12</sup> Concerning reswitching, for example, this is clearly shown than in Bruno (1967) who considers various models including the "canonical" two-sector capital good model *without* the "extreme assumption" to which Garegnani had drawn Samuelson's attention (Samuelson, 1962, p. 202, n. 1). Those who regard reswitching and related phenomena as something more akin to a knock-out blow would object to the required equilibrium price "jumps" that Bruno postulates. On their view, such discrete changes undermine the traditional notion of equilibrium as a persistent position relative to which the economy "gravitates".

<sup>13</sup> It is important to point out that those who consider Sraffa (1960) as having set the stage for a revival of the standpoint of the old Classical economists reject the claim that a uniform rate of profit on the value of reproducible capital goods can *only* occur in a thorough-going steady state. For them, the method of long-period analysis, which enforces a uniform rate of profits under conditions of competition, was common to both classical and neoclassical economics prior to the ascendancy of intertemporal equilibrium analysis, and is *independent* of any assumption concerning the composition of output, much less that the economy is in a steady state equilibrium. Garegnani (1976) argued that Hicks was responsible for a change in the notion of equilibrium which ushered in this ascendancy. For an admirably clear account of this view, see Milgate (1979). Yet Hicks is mentioned only once in the two chapters on intertemporal equilibrium analysis found in DOSSO, and that in connection with models of the business cycle rather than with capital theoretic problems. Milgate notes that Hayek is cited by Malinvaud (1953); and that Lindahl is credited by Debreu (1959, p. 35) "as having been the first to produce 'a general mathematical study of an economy whose activity extends over a finite number of elementary time intervals'" (Milgate, 1979, p. 6). Again, no reference to either Hayek or Lindahl is to be found in DOSSO. It seems clear then that, for the MIT economists, intertemporal general equilibrium theory had its roots elsewhere. For them, Frank Ramsey (1928) is the mother lode, as stated forcefully by Samuelson and Solow (1956), and cited to this effect by Backhouse (RB, p. 258). Milgate (1979), on the other hand, makes no reference to Ramsey. Finally, Milgate's suggestion that Malinvaud (1953) owed much to Hayek and perhaps to Hicks (Milgate, 1979, pp. 5-6) should be read in light of an interview (Krueger, 2003) in which Malinvaud credits neither. He summarized the importance of his own work on capital theory as contributing "to make mathematical economists understand why they should pay attention to transversality conditions" (Krueger, 2003, p. 189). The latter owe much more to Ramsey than to Hayek, Hicks, or Lindahl.

worth looking at chapters 11 and 12 of that justly famous book in order to come to a fair judgment of the importance of Robinson's most long standing complaint about modern neoclassical economics, namely, the absence "of any plausible account of a mechanism to keep the economy in equilibrium" (Robinson, 1960, p. 131).

On reading these chapters, one is struck first by the fact that the authors' main target is Leontief. They repeatedly take exception to his assumption, in dynamic models, that all *stocks* of capital goods are fully utilized at all points in time. This is shown to be inconsistent with efficiency, apart from special cases. The criticism itself is understandable in view of an essential purpose of the book, which is to show the importance of recognizing that efficiency in production does not necessarily imply that *every* resource is fully utilized. Leontief's assumption that all stocks *are* fully utilized fixes the composition of output at each moment (in a fixed coefficients model), sacrificing the flexibility associated with allowing at least some stock(s) to remain under-utilized in a given period. This can place the economy inside its potential production-possibility frontier in the next period, thereby reducing potential consumption.

Complementing its formalism, there is a fascinating—and somewhat puzzling—half-page of interpretation in DOSSO at the end of a section entitled *Competitive Markets and Dynamic Efficiency*. The authors note first of all that, starting from any point measuring initial stocks net of consumption (consumable stocks allow for a clear two-dimensional diagram), there are many efficient paths forward such that the economy remains always at a point *on* its expanding production-possibilities frontier. Invoking Adam Smith, they write:

The truly remarkable thing about the intertemporal invisible hand is that while it results in efficiency over long periods of time, it requires only the most myopic vision on the part of market participants. Just current prices and [their] current rates of change need to be known, and at each moment long-run efficiency is preserved (DOSSO, p. 321).

The role of the future in determining the present is then made clear:

But for society as a whole there is need for vision at a distance (*ibid*).

Although the authors illustrate the nature of such vision by specifying *final* stock prices, they include a footnote stating that *initial* stock prices will do, setting the system off in just the right direction to ensure convergence to a pre-specified future point.<sup>14</sup>

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<sup>14</sup> The formal theory is the same as that found in Pontryagin *et al* (1962), one use of which is to determine the *angle* of take-off of a space-craft intended to land on or fly by a distant object whose future position can be calculated. Getting the take-off trajectory exactly right is crucial as "tiny errors



In view of the concern of the critics of mainstream theory with the assumption that capital is a *given* quantity of *value*, it is of further interest to quote part of the final paragraph of this interpretive section:

One interesting sidelight before we leave the subject of intertemporal pricing: Consider any efficient capital program and its corresponding profile of prices and own-rates [rates of change of prices]. *At every point of time the value of the capital stock at current efficiency prices, discounted back to the initial time, is a constant*, equal to the initial value. This law of conservation of discounted value of capital (or discounted Net National Product) reflects, as do the grand laws of conservation of energy in physics, the maximizing nature of the path (DOSSO, pp. 321-22, emphasis in the original).

Such constancy means that the various price and quantities defining the *value* of capital are continuously undergoing complex offsetting changes. No connection seems to have been drawn between this idea of the conservation of discounted value and the notion of a given quantity of capital. Perhaps for this reason, mainstream theory descending from Ramsey (1928) did not come into focus for its critics until very late in the game (Garegnani, 2010, pp. 88-93).

Dynamic optimization theory has made familiar the requirement for “vision at a distance” as embodied in one or more *transversality conditions* (first-order conditions for a maximal path using the methods of optimal control theory).<sup>15</sup> It is these conditions that pick out the convergent branch of a saddle-path along which capital stocks and their prices are continuously changing (DOSSO, Fig. 12-9, p. 334). A key property of this path is its *instability*. On either side, prices and/or quantities follow unsustainable trajectories. How to get on to the convergent path is the question Joan Robinson would have insisted on being answered. She did not know the mathematics, but she knew what the problem was and refused to let it pass. As for what happens whenever the underlying parameters of an intertemporal equilibrium model *change*, continued equilibrium requires a discrete jump in the values of economic variables onto a new convergent saddle path. How this happens is the theory’s Achilles’ heel.<sup>16</sup> *Assuming*

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can grow calamitous” (Chang, 2015, p. A1). The same is true of initial asset prices in intertemporal equilibrium models of capital accumulation.

<sup>15</sup> Although there is no reference to transversality conditions in DOSSO, the authors are aware of their importance. In a paper that “generalizes the Ramsey model [Ramsey, 1928] to any number of capital goods”, Samuelson and Solow write: “The resulting mathematical problem turns out to have some intrinsically intricate transversality or end conditions that will probably be of importance in many dynamic programming problems” (Samuelson and Solow, 1956, p. 537).

<sup>16</sup> Some have made a virtue out of necessity. A proponent of the rational expectations hypothesis (which adds well-behaved randomness to the deterministic structure of intertemporal equilibrium models) writes that the ridge-like nature of a saddle-path, “far from being an unlikely freak case, provides the only sensible basis for forward looking expectations when individuals are well informed

that it does is no answer to Robinson's point concerning the reconciliation of past, present, and future capital valuations (Robinson, 1953-54, p. 84); nor is it an answer to Garegnani's questioning that "a Ramsey path to steady states [will] ever be one which the economy can walk" (Garegnani, 2010, p. 91).<sup>17</sup>

#### 4. Conclusion.

Robinson was bewildered by the fact that the MIT economists were content to build an economic theory based on what was, in her mind, an *illogical* foundation rather than a point about epistemology or method. The lack of logic turned on the fact that perfect foresight, or "vision at a distance", is a contradiction in terms, as argued, for example, by Oskar Morgenstern:

Should complete foresight be an indispensable postulate...there results that wider paradox that the science has already posited the object that it is first to investigate; that, without this assumption, the object could not exist at all... (Morgenstern, 1976 [1935], p. 175).

Where, then, does this leave the debate between Robinson and MIT? Obviously, Samuelson understood the saddle-path property of solutions to dynamic optimization problems; and he continued to study the connection between equilibrium models of accumulation and the physical laws of energy conservation (Samuelson, 1990). As for the relevance of saddle-path solutions to the analysis of accumulation in an actual economy—and DOSSO, it should be recalled, is full of asides concerning the possibility of operationalizing the theory—perhaps the clearest expression of faith tempered with doubt is found in Samuelson (1967), a paper appearing in a collection devoted to various applications of Pontryagin's "maximum principle" in economics and not, therefore, one that would have been widely read by many of those following the debate between Robinson and MIT.<sup>18</sup> He heads up a concluding

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about the structure of the economy" (Begg, 1982, p. 40). Recent work signals the demise of such claims. "Coordination of expectations of long-lived agents is necessarily weak. There is no collective view of the future ... that is able to trigger coordination ... a 'crisis,' here an expectational crisis, is unavoidable ... and the real-time falsification of beliefs in the long run, indicates that ... real-time learning must play a significant role" (Evans, Guesnerie, and McGough, 2015, p. 3).

<sup>17</sup> Robinson and Garegnani did not see eye to eye on the implications of the capital theory controversy for economic theory as a whole. It is all the more interesting, therefore, that the negative part of her critique and his remark about the inability of an economy to "walk" a Ramsey path are of a piece.

<sup>18</sup> The collection containing Samuelson (1967) also contains a paper by Michael Bruno, very likely the one referred to by Samuelson in "A Summing Up" (Samuelson, 1966, p. 582, n. 6). It is of special interest because Bruno takes up the reswitching phenomena within the framework of intertemporal general equilibrium theory, only to point out that, along an equilibrium path, the economy will jump past those

section describing his faith in the ability of a market economy to right itself, re-aiming prices and quantities along a saddle-path of accumulation in just such a way as to satisfy all the conditions of intertemporal equilibrium.<sup>19</sup> Samuelson uses the image of a cyclist maintaining balance while moving forward:

The image in my mind is that of a bicycle. The rider of the bicycle is the bulk of the market, a somewhat mystical concept to be sure—like its analogue, the well-informed speculator who gets his way in the end because his way is the correctly discerned way of the future; and those who think differently are bankrupted by their bets against (him and) the future... Even if there is something valid in this heuristic reasoning, one must admit that the system need not—and, generally will not—move from its present position to the golden age in the most efficient way: it will hare after false goals, get detoured, and begin to be corrected only after it has erred. (Samuelson, 1967, pp. 229-30).

In a reply to Robinson, Samuelson again refers to the role of “perspicacious planners, or avaricious speculators in forward markets” (Samuelson, 1975, p. 45, n. 7) while granting that:

...a skeptic may legitimately doubt that...a competitive market system will have the ‘foresight’ or the perfect-futures markets to approximate in real life such warranted paths that have the property that, if everyone knew in advance they would occur, each will be motivated to do just that which gives to them. (Samuelson, 1975, p. 45).<sup>20</sup>

Robinson’s view of the present as a break between an irrevocable past and an unknowable future rejects such faith in the ability of markets to anticipate the future.

This was the heart of the capital theory controversy for Robinson. It is surprising then that Backhouse, quoting Solow’s reply to Robinson (1953-54), omits the following:

...dispensing with the notion of the “quantity of capital” will make the theory...harder. But the real difficulty...comes not from the physical diversity of capital goods. It comes

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sections of the wage-profit frontier that might otherwise exhibit reswitching (Bruno, 1967, p. 215). Such price jumps are foreseen, ruling out arbitrage profits.

<sup>19</sup> This view informs an earlier and similar claim about asset prices, also in a paper not accessible to anyone but the mathematically sophisticated: “This re-aiming is, so to speak, what an optimizing society is constantly doing” (Samuelson and Solow, 1956, p. 548).

<sup>20</sup> The skeptic in this passage is Harcourt: “To conclude the reply to Professor Harcourt’s query, the vast literature on the ‘Hahn problem’ should be consulted to form a reasonable opinion on how tolerably inefficient or efficient are market and planned systems in the real world...” Samuelson (1975, p. 45). The “Hahn problem”, a reference to Hahn (1966), concerns the unstable nature of a convergent saddle-path picked out by “optimizing society”.

from the intertwining of past, present and future, from the fact that while there is something foolish about a theory of capital built on the assumption of perfect foresight, we have no equally precise and definite assumption to take its place (Solow, 1955-56, p. 102).

Ironically, the great question which has always haunted the type of analysis offered by the MIT economists in answer to Robinson's provocations has always been her own question: how to get into equilibrium? If "vision at a distance" (DOSSO, p. 321) means co-ordination of long-term expectations, recent work done strictly within the context of dynamic general equilibrium theory, shows that "getting into equilibrium" may well be impossible, thereby vindicating Robinson's position in her debate with the MIT economists.<sup>21</sup>

## References

- Backhouse, R. 2014. "MIT and the Other Cambridge." *History of Political Economy*, 46 (annual supplement): 252-271.
- Begg, D.K.H. 1982. *The Rational Expectations Revolution in Macroeconomics*. Baltimore: Johns Hopkins.
- Bellman, R.E. 1957. *Dynamic Programming*. Princeton: Princeton University Press.
- Blaug, M. 1974. *The Cambridge Revolution: Success or Failure?* London: Institute of Economic Affairs.
- Bliss, C.J. 1975. *Capital Theory and the Distribution of Income*. Amsterdam: North-Holland.
- Bruno, M. 1967. "Optimal Accumulation in Discrete Capital Models." In Shell, pp. 181-218.
- Burmeister, E. 1980. *Capital Theory and Dynamics*. Cambridge: Cambridge University Press.
- Caputo, M.R. 2005. *Foundations of Dynamic Economic Analysis, Optimal control theory and applications*. Cambridge: Cambridge University Press.
- Chang, K. 2015. "The Long, Strange Trip to Pluto, and How NASA Nearly Missed It." *New York Times*, July 19.
- Cohen, A. 2010. "Capital Controversy from Böhm-Bawerk to Bliss: Badly Posed or Very Deep Questions? or What 'We' Can Learn from Capital Controversy Even If You Don't Care Who Won." *Journal of the History of Economic Thought* 32(1): 1-21.

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<sup>21</sup> Again, see Evans, Guesnerie, and McGough (2015, p. 3).

Debreu, G. 1959. *Theory of Value: An Axiomatic Analysis of Economic Equilibrium*. New Haven: Yale University Press.

Dorfman R., P.A. Samuelson, and R.M. Solow. 1958. *Linear Programming and Economic Analysis*. New York: McGraw Hill.

Evans, G.W. and R. Guesnerie. 2005. "Coordination on saddle-path solutions: the educative viewpoint—linear multivariate models." *Journal of Economic Theory* 124: 202-229.

Evans, G.W., R. Guesnerie, and B. McGough. 2015. "Eductive Stability in Real Business Cycle Models." Online: <http://pages.uoregon.edu/gevans/>

Fisher, F.M. 1989. "Stability Analysis in Micro and Macro Theory: An Interview." In *Joan Robinson and Modern Economic Theory*, edited by G. Feiwel. London: Macmillan.

Fisher, F.M. 1992. *Aggregation, Aggregate Production Functions and Related Topics: Collected Papers of Franklin M. Fisher*. Edited by John Monz. Cambridge MA: MIT Press.

Garegnani, P. 1976. "On a Change in the Notion of Equilibrium in Recent Work on Value and Distribution." In *Essays in Modern Capital Theory*, edited by M. Brown, K. Sato, and P. Zarembka. Amsterdam: North-Holland.

Garegnani, P. 2010. "Capital in the Neoclassical Theory." In *Production, Distribution and Trade: Alternative Perspectives, Essays in honour of Sergio Parrinello*, edited by Adriano Birolo, Duncan K. Foley, Heinz D. Kurz, Bertram Schefold and Ian Steedman. London: Routledge.

Goodwin, R.M. 1967. "A Growth Cycle." In *Socialism, Capitalism and Economic Growth, Essays presented to Maurice Dobb*, edited by C.H. Feinstein. Cambridge: Cambridge University Press.

Gram, H. 1976. "Two-Sector Models in the Theory of Capital and Growth." *American Economic Review* 66 (5): 891-903.

Hahn, F.H. 1966. "Equilibrium Dynamics with Heterogeneous Capital Goods." *Quarterly Journal of Economics* 80 (4): 633-646.

Hahn, F.H. 1981. "General Equilibrium Theory." In *The Crisis in Economic Theory*, edited by D. Bell and I. Kristol. New York: Basic Books.

Harcourt, G.C. 1969. "Some Cambridge Controversies in the Theory of Capital." *Journal of Economic Literature* 7 (2): 369-405.

Harcourt, G.C. 1972. *Some Cambridge Controversies in the Theory of Capital*. Cambridge: Cambridge University Press.

- Harcourt, G.C. 1996. "Some Reflections on Joan Robinson's Changes of Mind and Their Relationship to Post-Keynesianism and The Economics Profession." In *The Economics of Joan Robinson*, edited by M.C. Marcuzzo, L.L. Pasinetti, and A. Roncaglia. London: Routledge.
- Harcourt, G.C. and P. Kerr. 2013. "Introduction" to reprint of Robinson (1956). London: Palgrave Macmillan.
- Hicks, J.R. 1932. *The Theory of Wages*. London: Macmillan.
- Johnson, H.G. 1975. Review of Blaug (1974). *Journal of Political Economy* 83 (5): 1083-1084.
- Kalecki, M. 1968. "Trend and Business Cycles Reconsidered." *Economic Journal* 78 (310): 263-276. Reprinted in *Selected Essays on the Dynamics of the Capitalist Economy, 1933-1970*. Cambridge: Cambridge University Press.
- Krueger, A. 2003. "An Interview with Edwin Malinvaud." *Journal of Economic Perspectives* 17 (1): 181-198.
- Levhari, D. 1965. "A Nonsubstitution Theorem and Switching of Techniques." *Quarterly Journal of Economics* 79 (1): 98-105.
- Malinvaud, E. 1953. "Capital Accumulation and the Efficient Allocation of Resources." *Econometrica* 21 (2): 233-268
- Milgate, M. 1979. "On the Origin of the Notion of 'Intertemporal Equilibrium'." *Economica* 46 (181): 1-10.
- Morgenstern, O. 1976 [1935]. "Perfect Foresight and Economic Equilibrium," translated by F.H. Knight and reprinted in A. Schotter and M.I. Nadiri, editors, *Selected Economic Writings of Oskar Morgenstern*. New York: New York University Press.
- Pasinetti, L.L. 1966. "Changes in the Rate of Profit and Switches of Techniques." *Quarterly Journal of Economics* 80 (4): 503-517.
- Pontryagin, L.S., V.G. Boltyanski, R.V. Gamkrelidze, and E.F. Mishchenko. 1962. *The Mathematical Theory of Optimal Processes*. New York: Interscience (John Wiley).
- Ramsey, F. 1928. "A Mathematical Theory of Saving." *Economic Journal* 38 (152): 543-559.
- 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. 1959. "Accumulation and the Production Function." *Economic Journal* 69 (275): 433-442.
- Robinson, J. 1960. *Collected Economic Papers*, Vol. II. Oxford: Basil Blackwell.
- Robinson, J. 1962. *Essays in the Theory of Economic Growth*. London: Macmillan.
- Robinson, J. 1973. *Collected Economic Papers*, Vol. IV. Oxford: Basil Blackwell.
- Robinson, J.V. 1975. "The Unimportance of Reswitching" *Quarterly Journal of Economics* 89 (1): 32-39.
- Robinson, J. 1979. *Collected Economic Papers*, Vol. V. Oxford: Basil Blackwell.
- Robinson, J. 1980. *What are the Questions? And Other Essays*. Armonk, NY: Sharpe.
- Salvadori, N. 1996. "Productivity Curves in *The Accumulation of Capital*." In *The Economics of Joan Robinson*, edited by M.C. Marcuzzo, L. Pasinetti, and A. Roncaglia, 232-248. London: Routledge.
- Samuelson, P.A. 1962. "Parable and Realism in Capital Theory: The Surrogate Production Function." *Review of Economic Studies* 29 (3): 193-206.
- Samuelson, P.A. 1966. "A Summing Up." *Quarterly Journal of Economics* 80 (4): 568-583.
- Samuelson, P.A. 1967. "Indeterminacy of Development in a Heterogeneous-capital model with Constant Saving Propensity." In Shell, pp. 219-231.
- Samuelson, P.A. 1975. "Steady-state and Transient Relations: a Reply on Reswitching." *Quarterly Journal of Economics* 89 (1): 40-47.
- Samuelson, P.A. 1990. "Two Conservation Laws in Theoretical Economics." In *Conservation Laws and Symmetry: Applications to Economics and Finance*, edited by R. Sato and R.V. Ramachandran, 57-70. Dordrecht: Kluwer.
- Samuelson, P.A. and R.M. Solow. 1956. "A Complete Capital Model involving Heterogeneous Capital Goods." *Quarterly Journal of Economics* 70 (4): 537-562.
- Shell, M., editor. 1967. *Essays on the Theory of Optimal Economic Growth*. Cambridge MA: MIT Press.
- Solow, R.M. 1955-56. "The Production Function and the Theory of Capital." *Review of Economic Studies* 23 (2): 101-108.

Sraffa, P. 1960. *Production of Commodities by Means of Commodities: Prelude to a Critique of Economic Theory*. Cambridge: Cambridge University Press.

Symposium. 1966. "Paradoxes in Capital Theory: A Symposium." *Quarterly Journal of Economics* 80 (4): 503-583.