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**Joan Robinson and MIT**

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Geoffrey Harcourt

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Joan Robinson and MIT  
Harvey Gram and Geoffrey Harcourt  
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## ABSTRACT

The great question which has always haunted the type of analysis offered by the MIT economists in answer to Robinson's provocative critique (1953) has always been her own question: how to get into equilibrium? If the notion of "vision at a distance," inherent in dynamic equilibrium analysis (Dorfman, Samuelson, and Solow, 1958) means coordination of long-term expectations, recent work shows theory, that "getting into equilibrium" is an impossibility. This vindicates Robinson's position in the capital controversy, at least with respect to the MIT economists.

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Joan Robinson and MIT

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In concluding “MIT and the Other Cambridge”, Roger Backhouse asserts, “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” (Backhouse, 2014, p. 269; hereafter RB with page numbers only).<sup>3</sup> He begins, of course, with Joan Robinson’s famous attack on the use of the aggregate production function in the theory of capital (Robinson, 1953-54). In an open provocation, she asserted that “the production function has been a powerful instrument of mis-education ... [resulting in] sloppy habits of thought [being] handed on from one generation to the next” (*ibid*, p. 81). “This challenge was taken up by the MIT economists Robert Solow and Paul Samuelson, who claimed that her objections were unfounded” (RB, 253). And so began the two Cambridges capital controversy. More than a decade later, during which time Sraffa’s famously short book (Sraffa, 1960) had also attracted great interest, “A Summing Up” (Samuelson, 1966) “marked MIT’s recognition of Robinson’s technical point” (RB, p. 259),<sup>4</sup>

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<sup>3</sup> He also offers a novel thesis, not considered here; namely, “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” (RB, p. 254). See Backhouse and Boianovsky (2013) for a full account. It is noteworthy that Solow and Stiglitz, in order to explain short run unemployment, rely on real wage rigidity in a one-sector model with given speeds of adjustment for the money wage and the price of homogeneous output. Typically, Cambridge short run models rely on structural differences between a consumption goods sector and an investment goods sector, as in Harcourt (1965), originally presented to The Sraffa-Marris research student seminar, at which Solow was present (along with James Meade, Richard Kahn, Joan Robinson, Piero Sraffa, Robin Marris, Luigi Pasinetti, and other well-known research students, except for Nicky Kaldor, who was in Australia).

<sup>4</sup> Robinson is mentioned just once by Samuelson when he refers to “...a general blue-print technology model of Joan Robinson and M.I.T. type...” acknowledging the use by Robinson of models with a discrete number of techniques, as in linear programming. Certainly, many had found it difficult to follow her critique, which unearthed a “curious possibility ... pointed out to me by Ruth Cohen” (Robinson, 1953-54, p. 106; see also Robinson, 1956, pp. 411-18). In particular, Backhouse notes that Solow had found it “impenetrable”, citing a letter to Harry Johnson who, in his reply, remarks that the pre-publication draft of Robinson’s article showed “signs of mental breakdown” which he hoped would be eliminated in the final version (RB, p. 255, n. 7). Without elaborating on Johnson’s “long explanation” of the issues, Backhouse concludes: “This exchange shows the importance of Johnson, not usually associated with this controversy, as a liaison between two different ways of thinking” (*ibid*, p 256). In his published review of a critical assessment by Blaug (1974), Johnson writes of the “usually arid and arcane technical literature on the reswitching controversy” (Johnson, 1975, p. 1083). He praises Blaug for treating “with

which others had analyzed in great detail; namely, the two theoretical anomalies: double-switching or reswitching of techniques, and reverse capital deepening.<sup>5</sup>

It is difficult to square Samuelson's evident desire to nail down exactly what he saw as his own error<sup>6</sup> with the broad claim that, for the MIT economists, the whole controversy had been a waste of time. No doubt, what Backhouse has in mind is the fact that the MIT economists found no reason to doubt the correctness of their analysis of "Efficient Programs of Capital Accumulation", the title of the second of two key chapters of the famous text, *Linear Programming and Economic Analysis* (Dorfman, Samuelson, and Solow, 1958; hereafter, DOSSO), which was at the time a work in progress. As for ending the controversy, Backhouse credits Christopher Bliss (1975) with "the definitive summary of the issues in the controversy, at least from the neoclassical side".<sup>7</sup> Bliss acknowledges (*ibid*, p. xxx) that the neoclassical theory of capital accumulation has no clear, or even intended, connection with economic reality.

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. 114-31). This sustains the 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 remarkable how easily Robinson extracted what she later called this "negative part" (Robinson, 1960, p. 130), adding but a single connecting paragraph to link abbreviated sections of the original. The "constructive parts are better done in my book" (*ibid*), referring to *The Accumulation of Capital* (Robinson, 1956). Her new results, developed with that aid of diagrams, were later brought to prominence in Harcourt (1969, 1972), "the classical account of

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exceptional academic restraint what can only be described as a boisterous display of shoddy pseudo-scholarship in the history of economic thought that substitutes for a blinkered poverty of economic imagination" (*ibid*, pp. 1083-84). A liaison would not usually indulge in such intemperate language. In any case, Johnson appears ambivalent. He regarded Sraffa's *Production of Commodities by Means of Commodities* as the most exasperating book to come out of Cambridge "since its emergence as a centre of economic theory" (Johnson, 1962, p. 464) and yet describes its analysis as "extremely elegant and rigorous" (*ibid*, p. 465).

<sup>5</sup> In later work with Naqvi, Robinson argued that reverse capital deepening was the more fundamental result: "Thus double switching is associated with perversity. The interesting point, however, is the perversity, not the duplicity" (Robinson and Naqvi, 1967, p. 580), a memorable "Joan-ism" of the type Johnson had apparently found objectionable (RB, p. 255).

<sup>6</sup> "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).

<sup>7</sup> Burmeister (1980), more closely connected to MIT, could be similarly described.

this debate" (RB, p. 253).<sup>8</sup> As for the "negative" part, Robinson had reworked it completely in "Accumulation and the Production Function" (Robinson, 1959) to show "how the neoclassical production function can be rescued if we bring the Keynesian conditions to its aid" (Robinson, 1960, p. 131).<sup>9</sup>

Backhouse further justifies his "waste of time" assessment of the capital 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 goes hand in hand with an acceptance of intertemporal equilibrium analysis as the only basis upon which to erect a rigorous analysis of capital accumulation under condition of competition, rejecting Robinson's analysis of various "ages" (golden, leaden, platinum, galloping platinum, etc.), her first steps towards a realistic analysis of accumulation under capitalist conditions. Unlike the straight-jacket of intertemporal equilibrium models, her models were intentionally open-ended or *underdetermined* from a formal point of view (*cf.* Harris, 2005).

Denying the *comparison* of steady states much, if any, theoretical relevance is the position taken by Bliss (1975) and Burmeister (1980).<sup>10</sup> The same is true of DOSSO, frequently cited by Backhouse, where output is always at a point on an economy's short run production-possibility frontier. It is therefore worth looking carefully at chapters 11 and 12 of that justly famous work

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<sup>8</sup> For further analyses of capital theoretic anomalies, see Hicks (1965), Spaventa (1968), Garegnani (1970), Harris (1973), Gram (1976), and especially Salvadori (1996) where the mathematics of Robinson's "productivity curves" is thoroughly developed and where the various results in the literature are linked back Harcourt's survey (Salvadori, 1996, pp. 243-44; and p. 245, n. 1 for the history of Salvadori's work on productivity curves). Concerning Harcourt (1972), Johnson is again ambivalent, seeming to praise the book as a "Cambridge explanation, justification, and vindication" (Johnson, 1975, p. 1083) while applauding Blaug's assessment of the controversy as "properly scathing about the 'essentialism' of Harcourt and Company" (*ibid.*).

<sup>9</sup> It is worth noting that in both *The Accumulation of Capital* (Robinson, 1956) and its sequel (Robinson, 1962) there is a noticeable shift toward the analysis of cyclical growth along the lines of Goodwin (1967) and Kalecki (1968). See the Introduction by GCH and Prue Kerr to the Palgrave Classics Edition of *The Accumulation of Capital*, pp. xix-xxv; and Harcourt, 2015, p. 251.

<sup>10</sup> It is important to point out that those who consider Sraffa (1960) as having set the stage of 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 ascendancy of intertemporal equilibrium analysis, and is entirely *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. Notably, there is only one mention of Hicks in the two chapters on intertemporal equilibrium analysis found in DOSSO. It arises in connection with models of the business cycle, and there is no suggestion that the work of Hicks is foundational to intertemporal equilibrium analysis. For MIT, that credit goes, first and foremost, to Frank Ramsey (1928), as is made clear in Samuelson and Solow (1956).

in order to come to a fair judgment 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 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 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).

There then follows an equally remarkable sentence concerning the role of the future:

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<sup>11</sup> to ensure convergence to a pre-specified future point.<sup>12</sup>

Students of dynamic optimization theory are familiar with the requirement for “vision at a distance” as embodied in one of more *transversality conditions* (among the first-order conditions for a maximal path using the methods of optimal control theory).<sup>13</sup> It is these conditions that are shown in modern textbooks to underlie the ubiquitous saddle-path property of capital stocks and their prices (DOSSO, Fig. 12-9, p. 334). A key property of the convergent branch of a saddle-path is its inherent instability—on either side, prices and/or quantities follow an unsustainable trajectory. How to get on to the convergent path is the question that Joan Robinson would have insisted on being answered. She may not have known the mathematics, but she knew what the problem was—a testimony to her deep understanding of equilibrium theory. As for what happens whenever the underlying parameters of an intertemporal equilibrium model change, somehow asset prices must simply jump onto a new convergent saddle path, an adjustment problem that is rarely, if ever discussed.<sup>14</sup>

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<sup>11</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 can be crucial as “tiny errors can grow calamitous” (Chang, 2015. p. A1). The same is true of initial asset prices in intertemporal equilibrium models of capital accumulation.

<sup>12</sup> 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 components of the vector of prices,  $p$ , and the vector of quantities,  $k$ , in the inner product  $p'k$  measuring the *value* of capital are continuously undergoing offsetting changes.

<sup>13</sup> Although there is no reference to transversality conditions in DOSSO, the authors are fully aware of their importance. In a paper that “generalizes the Ramsey model 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). Prescient words, indeed!

<sup>14</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 about the structure of the economy” (Begg, 1982, p. 40). Recent work signals the demise of such claims.

Citing letters, Backhouse observes that “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*” (RB, p. 258).<sup>15</sup> Although no evidence is cited to suggest that Robinson ever discussed the book with Kahn, she did write of intertemporal general equilibrium: “I have never been able to make that theory stand up long enough to knock it down” (Robinson, 1980, p. 128). In view of the knife edge property of convergent saddle-paths, this is an entirely apt description.

Where, then, does this leave the Robinson-Samuelson debate? Obviously, Samuelson fully understood the saddle-path property of solutions to dynamic optimization problems. Moreover, he continued to study the connection between equilibrium models of accumulation and the physical laws of energy conservation (Samuelson, 1990). As for their relevance to the analysis of accumulation in an actual economy—DOSSO is full of asides concerning the possibility of operationalizing the theory—perhaps the clearest expression of simultaneous doubt and faith is found in Samuelson (1967).<sup>16</sup> He heads up a concluding 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 (analyzed in the preceding sections) in just such a way as to satisfy all the conditions of intertemporal equilibrium.<sup>17</sup> Samuelson uses the image of a cyclist, which again is most apt as it requires maintaining a 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

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“The flavour of [our] results is that 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, 2014, p. 3).

<sup>15</sup> Robinson once showed me a letter from Samuelson, written in the early 1970s, in which he referred her to the chapters on efficient capital accumulation in DOSSO. When I suggested that society’s “vision at a distance” was tantamount to perfect foresight, she shot back, “That is exactly my point!” She was bewildered that brilliant minds were content to build an *economic* theory on such an illogical foundation, notwithstanding its formal correctness and usefulness in other contexts.

<sup>16</sup> This paper is in a collection containing one by Michael Bruno, possibly referred to by Samuelson in “A Summing Up” (Samuelson, 1966, p. 582, n. 6). 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 sections of the wage-profit frontier that might otherwise exhibit reswitching (Bruno, 1967, p. 215). Along such paths, prices and growth rates may have to jump discretely, but this is foreseen and so not an opportunity for arbitrage profits to be earned.

<sup>17</sup> It seems that faith also informs an earlier and similar claim about asset prices: “This re-aiming is, so to speak, what an optimizing society is constantly doing” (Samuelson and Solow, 1956, p. 548).

the future; and those who think differently are bankrupted by their bets against (him and) the future. (It is easier to identify the well-informed speculator *ex post* than *ex ante*, and the image can easily dissolve into an empty tautology.) ... 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)

Elsewhere in his debates with Robinson, he refers to the role of “perspicacious planners, or avaricious speculators in forward markets” (Samuelson, 1975, p. 45, n. 7) while granting that:<sup>18</sup>

...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)

Returning to the metaphor of the bicycle, one of us once asked, echoing Robinson’s view of the present as a sharp break between an irrevocable past and an unknowable future:

Is the cyclist’s sense of balance sufficient justification for using a magic wand, called the transversality condition, to pick out a convergent saddle-path? This sleight of hand erases the troublesome effects of past decisions made under different circumstances and sets the economy on a new convergent path where events unfold as expected. For Robinson, this methodology is unacceptable. It ignores the dead hand of history and regards the future as knowable. (Gram, 2005, p. 121)

## Conclusion

Without looking more carefully at the arguments contained in the main work of the MIT economists that he cites, Backhouse fails to give full recognition to Joan Robinson’s intuitive understanding of the Achilles’ heel of intertemporal equilibrium models of capital accumulation. The real mystery is why the MIT economists failed to acknowledge more often and more clearly that their theory imposed the condition of perfect foresight which was such anathema to Robinson.<sup>19</sup> She thought that their theory could be undermined by its apparent need for “leets”, a magical type of capital (“steel” spelled backwards) that can somehow change

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<sup>18</sup> The hypothetical skeptic in this passage is, no doubt, 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 precisely the unstable nature of a convergent saddle-path somehow thought to be picked out by “optimizing society”.

<sup>19</sup> See also Gram (2010, p. 362).

form whenever needed to accommodate a change of technique.<sup>20</sup> As for perfect foresight—the absurdity Robinson knew would have pernicious consequences for economic theory and policy—it is notable that Backhouse, in quoting Solow’s reply to Robinson (1953-54), omits a final crucial part of what he wrote:

...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 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, emphasis added).

The great question which has always haunted the type of analysis offered by the MIT economists in answer to Robinson’s provocative critique 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 shows, within the context of dynamic general equilibrium theory, that “getting into equilibrium” is an impossibility.<sup>21</sup> This vindicates Robinson’s position in the capital controversy, at least with respect to the MIT economists.<sup>22</sup> Their effort to formalize the process of accumulation as an equilibrium path using the methods of dynamic optimization may finally be seen as the real “waste of time” to which Backhouse might have drawn his reader’s attention. A student seeking inoculation might therefore do well to go back to “A Lecture Delivered at Oxford by a Cambridge Economist” (Robinson, 1953)—a favorite of ours—in which 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 is to be found in her later work.

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<sup>20</sup> The consummate neoclassical price theorist, George Stigler, did argue that any reasonable notion of the marginal product of labor requires the assumption that capital is a given *value*, transmutable in form (Stigler, 1987, p. 136). In earlier editions of his once widely used text, Stigler (1952, p. 117) cited Dennis Robertson (1931) who wondered how ten shovels might be transmuted into eleven, less durable ones of equal value when defining the marginal product of an eleventh man joining ten digging a ditch—or should he just be sent to fetch a bucket of beer? See Gram (2013, p. 39-40) and Harcourt (2015, p. 244).

<sup>21</sup> See the impossibility theorem in Evans, Guesnerie and McGough (2014).

<sup>22</sup> Of course, it says nothing about her differences with those who find in Sraffa (1960) the basis for an entirely different set of arguments and possibilities for fruitful analysis.

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