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Daniele Besomi

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Daniele Besomi

This business of “influence” is a very elusive one.
—Dennis Robertson to Roy Harrod, 14 April 1950

In a recent issue of *HOPE* Arnold Heertje and Peter Heemeijer (2002) argued that Paul Samuelson’s multiplier-acceleration synthesis (1939a, 1939b) was inspired not so much by Alvin Hansen, as Samuelson claimed and repeated two decades later (Samuelson 1959), but by Roy Harrod’s pioneering book *The Trade Cycle* (1936), which Heertje and Heemeijer see as the genuine historical origin of the fluctuation mechanism based on the interaction of the multiplier and the acceleration principle. The authors argue, however, that in spite of his verbal declaration of intents Harrod failed to grasp the full implication of the mechanism he devised and had to rely on a number of exogenous variables in order to explain the turning points. The authors summarize the chapters of Hansen’s *Full Recovery or Stagnation?* (1938) devoted to Harrod’s book and to the interrelation between the multiplier and the acceleration principle, and affirm that Hansen trivialized this mechanism as an explanation of business cycles. Subsequently they examine Samuelson’s reading of Harrod and Hansen, emphasizing Samuelson’s criticism

Correspondence may be addressed to Daniele Besomi, c.p. 7, 6950 Gola di Lago, Switzerland; e-mail: dbesomi@cscs.ch. I am grateful to Judith Brown for permission to quote from Dennis Robertson’s works, to Chiba University of Commerce for permission to quote from Roy Harrod’s writings in their possession, and to the Provost and Scholars of King’s College Library, Cambridge, for permission to cite a passage by John Maynard Keynes.

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of the “unnecessary complication” Harrod introduced with his dynamic determinants (the exogenous factors referred to above) and his claim that Hansen reviewed Harrod’s book favorably. While they agree on the critical line, Heertje and Heemeijer do not accept Samuelson’s view that Hansen had “guarded admiration” for Harrod’s book, and claim instead not only that Hansen’s distaste for the book is evident throughout, but go so far as to affirm that Hansen’s review “effectively disproves that he understood the fundamental importance such a combination might have had in business-cycle theory” (216). The article concludes by suggesting that Samuelson underestimated Harrod’s role, that Harrod’s model was based on too many exogeneities and that his name cannot therefore be attached to the model, and that Samuelson was not influenced by Hansen’s writings but by the discussions in the Harvard workshops directed by Hansen, so that Hansen’s name should not be attached to the model. The multiplier-acceleration model can therefore rightly claim to be Samuelson’s own.

Samuelson replies by telling the story of how he formalized Hansen’s imperfectly formulated 1938 thesis, commenting that adding Harrod’s name to the list does not take any credit away from Hansen and that Harrod’s merit lies in having closed the Keynesian model, thereby formulating a truly endogenous cycle model (Samuelson 2002).

The story is richer, however, than described here, and actually runs along rather different lines. I agree with Heertje and Heemeijer that Harrod supplied the first clear-cut and explicit theory of the cycle based on the interaction of the multiplier and the acceleration principle.¹ But this statement needs some qualifications. First, Harrod’s original formulation was first published a few months before *The Trade Cycle*, in an obscure Danish journal called *Anglodania* (Harrod 1936a); the corresponding paper was read on 10 January 1936 before the British Union in Copenhagen. There Harrod boldly claimed that it illustrated “how economists explain the connection between growing wealth and growing fluctuation in trade.”

Second, Harrod’s priority in discovery has long been recognized in the literature, first and foremost by Samuelson himself (1939b), and is recorded in monographs (for instance Shackle 1967, 264–71; Pugno

1. The actual wording, which seems to suggest that Heertje and Heemeijer have made a new discovery, is as follows: “And it seems that this historical origin is a book written a few years earlier by Sir Roy Forbes Harrod, an economist working on the other side of the ocean, whose field of research covered business-cycle theory” (210).

1992, 81; Besomi 1999b, 74), in the sectorial literature (Nicholas Kaldor [1940, 78], for instance, described his 1940 trade-cycle model as being essentially similar to the explanations based on the combined operation of the multiplier and an investment function, such as Harrod's and Michal Kalecki's), in various general articles on Harrod's contributions (for instance Weintraub 1978, 124; Brown 1980, 10; and Johnson 1979, 273), and dictionary entries (such as Kurz 1997; Beaud and Dostaler 1995; and Besomi 1997, 1999a). These lists are far from being exhaustive.

Third, one may legitimately look for other contestants to the title of priority in this field. In a discussion on this subject in the Post-Keynesian Thought mailing list, J. Barkley Rosser Jr. suggested that Kalecki (1935) and J. M. Clark (1932) actually anticipated Harrod.² I am not fully convinced by either of these candidates: they both included the acceleration principle in their explanations, but neither coupled it with a full-blown multiplier, although both clearly had in mind that such a relationship was necessary for closing the model. This is indeed how Samuelson (1939b, 786–87) interpreted Clark's argument: the passage is cited as evidence that he was aware of the necessity of coupling the accelerator with some relation representing a link between production, purchasing power, and consumption. As to Kalecki, whose original piece was previously read before the Econometric Society in 1933, he formulated a relationship strictly resembling the multiplier and explicitly inverted the traditional saving-investment causal nexus, but his multiplying coefficient related only to consumption and not to the whole of expenditure. Nevertheless, they both anticipated *some* elements of Harrod's multiplier-acceleration mechanism. There have been attempts to go further back: from Hans Brems's observation that Gustav Cassel's macroeconomic model, "fully set out in hard algebra, [is] identical except for notation to that of Harrod (1948) 30 years later,"³ Samuelson (1993, 522) concludes that "Cassel deserves highest marks for having already in 1918 what is essentially the exponential growth model of Harrod-Domar. By hindsight, we recognize it as an accelerator-multiplier model." Samuelson, however, notes that Cassel's model contained no aspect of effective demand, which is quite natural considering that Cassel's problem was that of the *reproduction* of the physical components of real capital (Cassel 1923, vol. 1, chaps.

2. The discussion began on 22 May 2001 under the heading "WWW—Harrod home-page" and continued as "Fw: multiplier-accelerator issue" (<http://csf.colorado.edu/mail/pkt/2001/>).

3. Brems 1989, 171. G. L. S. Shackle had observed this similarity two decades earlier and included Cassel's *Theory of Social Economy* among the antecedents of Harrod's dynamics (Shackle to Harrod, 29 March 1966).

1.5–6), while the multiplier-acceleration idea regards both the formation of capital and the absorption of production. The reason why Harrod and Cassel came up with similar equations was explained by Joan Robinson (1952, 42) at the dawn of the debates on growth theory: Harrod and Cassel (and, before them, Marx in his reproduction schemes) reached similar results because they had applied the same piece of arithmetic. (On the reproduction schemes and Harrod's formula, see Bronfenbrenner and Wolfson 1984.)⁴ Going further back, Howard Sherman (2001, 88) claims that a multiplier-accelerator process can be found in Wesley Mitchell's 1913 *Business Cycles*.⁵

4. This opens the issue of the relationship between Harrod and Cassel. Harrod had certainly read *The Theory of Social Economy*: although he did not keep any reading notes (contrary to what he used to do), his copy of the book (kept in the Harrod Collection at the Nagoya University of Commerce and Business Administration, Japan) is annotated. The annotations, however, are not dated and do not throw light on whether Harrod actually appreciated the implications of Cassel's statement regarding equilibrium advance. Harrod (1938, 393 n) quoted Cassel's book shortly before writing his 1939 "Essay in Dynamic Theory," but only with reference to the fundamental role played by the notion of "need."

Harrod had met Cassel in 1932, on the occasion of a series of three lectures in Oxford on the gold standard and the crisis of the world monetary system (Harrod mentioned these lectures in *International Economics* [1933, 141]), but one can obviously not speculate on the possible subjects of any discussion that may have taken place between them; it must be noticed, however, that during spring 1932 Cassel's book could hardly be considered the subject of the day, while at the time Harrod was not yet engaged in the work that eventually stimulated his interest in a progressive society.

If the exact extent of Cassel's direct influence on Harrod is probably a matter that will be not possible to assess, it is, however, certain that *The Theory of Social Economy* had an indirect influence on Harrod, via Dennis Robertson. Harrod's approach to dynamics beginning from the analysis of the conditions of equilibrium growth was stimulated by the debates taking place within the New Fabian Research Bureau in 1932–34, whose members were all assuming as a starting point, under the influence of Robertson, a condition of progress rather than a stationary society (Besomi 1999b, 38–42). Robertson's own specific debt to Cassel was made explicit a few years later in a letter to Harrod: expressing his regret for not having pointed out in his review of *The Trade Cycle* that Harrod's main concern was with economic advance, Robertson recognized the influence Cassel exerted upon him: "I wish I had put in a sentence expressing more explicit welcome of your way of looking at the whole problem—'What will preserve a moving equilibrium?'—as being valuable in itself whether or not the pessimism of your own solution is justified. It's probably because I've always—or for long—been so much under the influence of Cassel's 'Th. of Soc. Ec.' in this respect that I rather took this approach for granted: but I quite agree that it isn't prominent enough in even the best English work (A.C.P[igou], as well as JMK) and deserved more explicit welcome in your own" (Robertson to Harrod, 10 February 1937; this reference was confirmed in a further letter dated 15 March 1945). (The implication of Brems 1989 that Harrod was influenced by Cassel is dismissed by Young 1989, 150–52, 175–76).

5. While stressing that Mitchell rejected simple models as possible explanations of the cycle, Philippe Adair (1994, 125) points out that he was aware of both the acceleration principle

Fourth, and most important, it seems to me that Heertje and Heemeijer have missed a fundamental point that prevented them from understanding what Harrod was really about and the difference between Harrod's and Samuelson's approaches. The main piece of their argument runs as follows:

Harrod himself did not see the explanatory possibilities of this interaction. The problem will be evident to any reader of his work: the movement of production between the turning points is made perfectly clear, but when it comes to explaining the turning points themselves, the analysis seems to drown in a massive swamp of exogenous variables. Three "static determinants," four "dynamic determinants": Harrod makes an impressive and, in its own right, quite successful attempt to put them all together in a correct and intelligible way, but in doing so he himself takes away the central importance of the multiplier and accelerator and he himself prevents his theory from being an endogenous and thus for the time a revolutionary one. (Heertje and Heemeijer 2002, 211)⁶

and the multiplier, as he had read and quoted the early formulations by T. N. Carver (1903) and Nicholas Johannsen (1908). Nevertheless, one should be cautious, as Mitchell did indeed describe the recovery in terms of cumulative causation, but this did not include any "leakage" bringing the series to convergence.

6. This last statement refers to the authors' claim that "up to that point, none of the existing business-cycle theories was yet able to explain, in a brief and intelligible way, the movement from boom to depression and back in a fully endogenous manner" (208). If the authors intend this statement to hold in general terms, the point is debatable, as the theories propounded for instance by Mikhail Tugan-Baranovsky, Arthur Spiethoff, and Albert Aftalion were essentially endogenous. The use of the word "essentially" does not aim at congratulating these theorists for having *almost* succeeded: these authors, like most researchers in this field, were fully aware of the importance of *both* endogenous mechanisms and exogenous factors for explaining the regular *and* the irregular features of the cycle. The problem of distinguishing what is systematic from what is accidental in crises and cycles is actually one of the most debated issues in the history of the discipline: the discussion began with the post-Napoleonic crises and happily continues nowadays, with all ranges of opinion being represented. By way of example, the three economists involved in the multiplier-accelerator issue all tackled this problem: see, for instance, Harrod 1934, 468–70; Hansen 1927, 196–206; and Samuelson 1947, chap. 11.

It would seem, however, that the authors mean that no simple self-contained mathematical model describing periodic fluctuations was available, as suggested by their footnote explaining that "there was at the time some mathematical literature on endogenous theories by Tinbergen, Ragnar Frisch, and others, but owing to its complicated and highly abstract nature, most economists were not acquainted with its content." In such a case, it is difficult to see how a simple and brief model could avoid being highly abstract.

This interpretation of Harrod's determinants seems to have been very much influenced by the postwar textbook understanding of Harrod's later "models,"⁷ according to which the parameters in Harrod's equation are given once and for all. But Harrod's dynamic determinants, namely the factors affecting the intensity of the multiplying and accelerating effects (the propensity to save, influenced by the distribution of income and Keynes's psychological law, according to which a richer community saves proportionately more; and the capital-output ratio, influenced by inventions but also by the movements of the interest rate), were never (neither in 1936 nor in the following versions of his theory) assumed to be constant;⁸ indeed, the whole mechanism is based on their endogenous fluctuations. As income grows during prosperity under the joint influence of the accelerator and the multiplier, the proportion of income saved increases; therefore the multiplier decreases and the moving equilibrium cannot be sustained, unless the capital-output ratio happens to increase in the right proportion. This alters countercyclically due to fluctuations in the interest rate, but Harrod deemed it unlikely that this would on all occasions suffice to compensate for changes in the multiplier. Equilibrium advance is thus broken because of the endogenous movement of the multiplier, and the consequences are cumulative. But the downward movement sets in motion changes in the opposite direction: the multiplier increases while the rate of interest decreases and makes the adoption of more capital-intensive equipment profitable (Harrod 1936b, 88–95).

7. Harrod did not like his theory to be considered a "model": he maintained that his approach was more "fundamental" than, and indeed a prolegomenon of, model-building (Harrod 1955, 360–61; 1960, 277). He understood the essence of models as "a formulation that has adjustable parameters" (Harrod 1968, 174), the value of which is subject to empirical research, and claimed his theory to enjoy a higher logical status: "'complete models' . . . require special postulates and assumptions in regard to lags and coefficients, which can only be accepted subject to statistical verification. While it is interesting and satisfactory to have such complete models for comparison with the phenomena of the real world, it is clear that the 'theories' of the cycle based on them have a greatly inferior authority, since their logical status is precarious, while mine rests, like the 'law of demand' itself, on assumptions of the utmost simplicity and generality. But mine makes no pretension to giving a complete explanation of the cycle" (Harrod 1952, 286).

8. The static determinants play no role in the basic cycle mechanism; their purpose is to explain what compels individual entrepreneurs to adapt their decision to what is "decreed" by the dynamic determinants (which regard the workings of the system as a whole). In some way, therefore, they were called to provide macrofoundations to microeconomics (see Besomi 1999b, 101–6).

The same mechanism used in *The Trade Cycle* for explaining the turning points was included in the following versions of Harrod's theory.⁹ In the original draft of the "Essay in Dynamic Theory," Harrod dedicated two entire sections to the discussion of fluctuations in the propensity to save and the acceleration coefficient (and therefore also of the warranted rate of growth) in the course of the cycle.¹⁰ Although this part was drastically compressed under the pressure of Keynes's comments (the exchange is published in Keynes 1973, 321–50), Harrod (1939, 28–29) was adamantly clear also in the published version:

Space forbids an application of this method of analysis to the successive phases of the trade cycle. In the course of it the values expressed by the symbols on the right-hand side of the equation undergo considerable change. As actual growth departs upwards or downwards from the warranted level, the warranted rate itself moves, and may chase the actual rate in either direction. The maximum rates of advance or recession may be expected to occur at the moment when the chase is successful.

One of the most frequently cited (but, I suspect, rarely read) passages in *Towards a Dynamic Economics* is probably the one regarding "the assumption that the capital/income ratio is constant" (Harrod 1948, 82), upon which the neoclassical criticism was based. This, however, ignores the context: Harrod was temporarily treating the coefficient as given in order to determine the dynamic equilibrium, but soon added that

G_w itself fluctuates in the trade cycle. Even if saving as a fraction of income is fairly steady in the long run, it is not likely to be so in the short run. There is some tendency for saving in the short period to be a residual between earnings and normal habits of consumption. Companies are likely to save a large fraction of short period increases of net receipts. . . . in a slump C_r [the acceleration coefficient] will be lower than usual, being confined to the requirements of circulating capital. (Harrod 1948, 89–90)

9. The Harrod-Domar interpretative tradition notwithstanding, Harrod's main concern was always the trade cycle. The dynamic equilibrium was the starting point of his dynamics, its instability being the main propulsion of the cumulative upward and downward movements, and the various nonlinearities (ceiling, floors, and changes in the parameters) explained the turning points. On the origin and development of the mainstream interpretation of Harrod's equation as describing a trend, and Harrod's protests against it, see Besomi 1998a.

10. Harrod 1996, secs. 15, 16; see Besomi 1996, 291–93, for a brief comment on this point.

In his last book Harrod discussed at some length “the postulate that the warranted rate itself (G_w) changes under the influence of boom and slump,” introducing the distinction (which echoes the one advanced in the original draft of the “Essay”)¹¹ between a “normal” warranted rate, “as pertaining in a steady advance,” and “special” warranted rates resulting from fluctuations of both desired saving and capital-output ratio (Harrod 1973, 36–45).

Although the changes in the multiplier and accelerator were there from the beginning for all to see, with only a few exceptions commentators failed to take notice of them. Harrod complained of this state of things throughout his life. Two examples may suffice: an early one relating to Keynes’s reading of *The Trade Cycle*, and one late in Harrod’s life relating to Joan Robinson’s (and Kaldor’s) criticism of Harrod’s later dynamics. To Keynes, Harrod explained:

I quite agree with what you say about the necessity of supposing that forces operate upon both the Relation and the Multiplier to distort them from normal in boom and slump. So far as the Multiplier is concerned, I have referred, as you recognize, to the shift to profit. But I have not assumed the Relation constant either. cf. p. 59 where I have referred to 2 forces, viz. changes in interest rate, and changes in relative prices of goods (on which you lay stress in your paper) tending to alter the Relation. And I refer anyhow to the first of these in the subsequent argument. (Harrod to Keynes, 7 April 1937; in Keynes 1973, 165)

To Joan Robinson’s neglect of changes in the coefficients, Harrod complained:

In her third paragraph she states that “it was soon [viz., after the publication of my book *Towards a Dynamic Economics*] pointed out that the rate of profit on capital must be supposed to influence both s [the saving ratio] and v [the capital-output ratio].” I must be allowed to make a mild protest about the words ‘it was soon pointed out,’ as if this fact was not already prominently present in my mind and would be something new to me! I dealt very extensively with the mutual

11. One may speculate that that echoing is by no means an accident. Harrod was writing *Economic Dynamics* approximately at the same time as, at the request of Donald Moggridge (who was preparing volume 14 of Keynes’s *Collected Writings* [Keynes 1973]), he dug out his 1938 correspondence with Keynes. The extant documents, unfortunately, do not permit a full reconstruction of the chronology, and this suggestion has to remain a conjecture.

influence of growth rates and profit rates in my *Trade Cycle* 1936 (see especially pp. 65–93). The second of my three dynamic determinants therein set out is the shift to profit connected with a given advance of output. (Harrod 1970, 737)

Harrod's theory is not exogenous: it is fully endogenous, as the fluctuations in the coefficients are triggered by the changes in output they themselves help cause. But it is nonlinear. This point was missed in most postwar comments on Harrod's cycle theory, thereby helping to turn it into a growth theory (see Besomi 1998a). But—Harrod's complaints to the contrary notwithstanding¹²—several readers of *The Trade Cycle* fully appreciated the role of the dynamic determinants in Harrod's model.¹³ Hansen was one of them, as he clearly explains that

12. "I may say that in spite of a number of fingerposts in my book pointing at these dynamic determinants, no review that I have seen has paid any attention to them. I have no doubt it is my fault for being too cryptic" (Harrod to Robertson, 8 October 1937).

13. When it came to expounding Harrod's explanation of the upper turning point, most reviewers emphasized the endogenous and nonlinear nature of the multiplier. Gaitskell (1937, 475), for instance, stressed that "the increase in income will not . . . be proportionate to the increase in investment, because of the shift to profit (explained by the diminishing elasticity of demand) and the propensity to save more from a larger income. These two factors—the shift to profit and the propensity to save—together with 'the amount of capital' are known as 'The Dynamic Determinants.' Together they determine the extent to which new investment will continue. The first two tend to restrict it, the third to encourage it. So long as they counterbalance one another, the increased rate of consumption will be just sufficient to absorb the output of new investment goods." Smithies (1937, 110–11), even if only to criticize Harrod's view, pointed out that "Mr. Harrod's account of the behaviour of profit and the propensity of individuals to save goes to show that as income increases in the upswing, a greater proportion of income is saved"; he added, however, that this is not sufficient to prove that the multiplier will decrease, "which is vital for Mr. Harrod's analysis." Lokanathan (1938, 518) summarizes Harrod's argument as follows: "Consumption diminishes or slows down partly because as income grows the propensity to save increases and partly because the boom brings a shift to profit which leads to larger savings. The effect of these two dynamic determinants—determinants in the sense that they determine the rate of growth of output—is to act as restrictive influences on expansion. On the other hand, the effect of the third dynamic determinant, *i.e.* the amount of capital used in production, is expansive. But as the advance continues, the shift to profits is intensified and the restrictive force of the first two determinants comes to exceed the expansive force of the third, bringing about a break in the boom." Benham (1938, 104) noted that net investment falls in the first place "because as income increases the proportion saved increases (partly owing to the growth of profits)." Similar observations were advanced by Stafford (1937, 75) and by the anonymous reviewer in the *Times Literary Supplement* ("A Theory," 1936). Even Joan Robinson (1936, 692)—who obviously forgot about this when commenting upon Harrod some thirty-four years later—noted that "since the value of the Multiplier falls as output increases, it is impossible for consumption to continue expanding at the required rate." For more general surveys of the reactions to Harrod's *Trade Cycle*, see Young 1989, 139–47; and Besomi 1998a, 41–49.

the advance in activity and income comes to a halt by reason of a slowing down in the rate of increase of consumption, and that the author accounts for this slowing down by the action mainly of two “dynamic determinants”—the increased propensity to save and the growth of profits as income rises. These two determinants decree that in the upswing phase of the cycle a larger and larger proportion of the income will be saved. If the advance is to be sustained, therefore, it follows of necessity that the ratio of capital goods to income must somehow rise. . . . If the ratio of capital to output [the third dynamic determinant] increases by reason of capital using inventions the advance may be maintained in spite of the action of the first two determinants. . . . [Or production] may become more capitalistic by reason of a fall in the rate of interest. This exerts an expansive force and operates through the third determinant to overcome the restrictive forces. . . .

But this is held to be highly improbable. . . . we may, accordingly, find increasing difficulty in maintaining the advance, and, if the restrictive influence of the first two determinants exceed the expansive influence of the third, the boom comes to a close. (Hansen 1937, 517–18; the order of the sentences in this quotation was slightly altered)¹⁴

14. If Hansen was not the sole commentator to notice the nonlinearity of Harrod's theory, he did at least provide a rather accurate rendition of it, although at times he certainly was critical. He correctly characterized the role of the static determinants (see note 8 above), stressed Harrod's quest for an explanation of the cycle not relying on frictions, lags, and “errors of judgement,” and understood that the cycle was intended as a deviation from a moving equilibrium. Samuelson's recollection that Hansen felt “resentment that a scholar would in that year proclaim as his own novelty (with a new name, the ‘relation’) what had been a staple in the literature for more than twenty years” (Samuelson 2002, 220; see also 1959, 183) is probably accurate, as Hansen did not miss any occasion to point out that Harrod's novelties were not so novel and that his sweeping generalizations as to the other theories of the cycle were hardly adequate. Indeed, Harrod was far from being scholarly to that effect: as for the acceleration principle, he candidly confessed to Robertson that he had “only learnt since publication [of *The Trade Cycle*] that this is a well-established term of art,” and explained that he first became acquainted with it while reading Pigou's first edition of *The Economics of Welfare*: “He discusses it and while explaining that it accounts for the greater fluctuation of capital goods output denies that it can be regarded as a true cause of the cycle. In the days when I first read it I was very much over-awed by Pigou's authority and tended to accept what he said uncritically” (Harrod to Robertson, 25 December 1936). (Harrod's early notebook [page 52] confirms his recollection, as chapter 2 of part 6 of the first edition of Pigou's book is summarized as follows: “The fact that fluctuations are larger in instrumental [than in consumer industries is] accounted for and . . . shown not to indicate that the causes of booms lie within these industries.”) Harrod eventually understood the possibility of applying the acceleration principle to trade-cycle analysis when reading a draft of Gottfried Haberler's *Prosperity and Depression* in autumn 1934; see, for a chronology, Besomi 1999b, chap. 3, in particular pp. 58–60. (*continued*)

The words *linear* and *nonlinear*, of course, were not in common use in those days.¹⁵ There was, however, one commentator who fully understood where the problem lay: Paul Samuelson. In his second article on the multiplier-accelerator, not only did Samuelson fully recognize Harrod's claim to priority,¹⁶ but he qualified the second-order difference function formalization as being capable of providing an *alternative* to various mechanisms based on nonlinearities, of which Harrod's was one. While J. M. Clark, Ragnar Frisch, Gottfried Haberler, and Hansen explained the upper turning point by having recourse to "the ceiling of full employment, or at least perverse price-cost movements due to the appearance of bottlenecks,"¹⁷ "Harrod appeals to the action of two dynamic determinants, each of which lowers the average propensity to consume for the community" (Samuelson 1939b, 788). Samuelson's equation proves that an alternative explanation of the turning point can be supplied—at least for moderate values of the parameters—"even without the action of Harrod's two dynamic determinants and without any price or interest-rate changes" (792, 794). For large values of the parameters, however, one must rely on "the fact that the relationships are not linear (of which Harrod's dynamic determinants are one expression)" (795). A footnote appended to this passage makes it definitely clear that Samuelson perfectly understood the issue:

There remains one interesting problem still to be explored. Mathematical analysis of the nonlinear case may reveal that for certain equilibrium values of [the coefficients] a periodic motion of definite amplitude will always be approached regardless of initial conditions. Such

Keynes's judgment that Hansen's review was "miserable (and frequently dishonest)" does not therefore seem justified (postcard to Harrod, 6 June 1937; Harrod's reply, if there was one, does not seem to be extant), and Heertje's and Heemeijer's emphasis on the critical aspects of Hansen's review of Harrod seems exaggerated.

15. Paradoxically enough, one of the few exceptions to Harrod's contemporaries' understanding of the nonlinear character of his relationships was one of the very few reviewers who had the technical mastery of functional equations: in his review, Jan Tinbergen (1937) assumed the coefficients to be constant, and having formalized Harrod's model by means of a first-order linear differential equation he came to the conclusion that it could only represent cumulative growth but not cycles.

16. Incidentally, one should notice that Harrodian terminology is used in Samuelson's first paper as well, as the acceleration principle is systematically referred to as the "relation."

17. When the nonlinear character of Harrod's theory was rediscovered after the war by J. R. Hicks (1949) and especially Richard Goodwin (1950, 318), stress was laid on the floors and ceilings. For a treatment of fluctuating parameters, see Pugno 1992, 85–90, 121–28; and 1998, 159–62.

a relation can never result from systems of difference equations with constant coefficients, involving assumptions of linearity. This illustrates the inadequacy of such assumptions except for the analysis of small oscillations. (795)

Nevertheless, Samuelson stressed that nonlinearity alone would be insufficient to explain a cycle, and maintained that a time-lag is necessary: "the time lag between income paid out and consumption is necessary if there is to be a cyclical pattern. Otherwise, there can be only a one-way movement away from equilibrium" (796–97). This statement was probably delivered as a rebuttal to Harrod's methodological rejection of the "time-lag theories of the trade cycle," which depended on his belief (rightly emphasized by Hansen [1937, 511] in his review of Harrod's book) that the fluctuation mechanism should not be based on any kind of friction or maladjustment (see, for a full discussion, Besomi 1998b). At any rate, the validity of Samuelson's claim depends crucially on his analytical framework, based on difference equations; for differential equations, nonlinearities of the kind described by Harrod may be sufficient to generate self-sustained oscillations.

Here we come to the crux of the matter. Heertje and Heemeijer interpret Samuelson's discussion of Harrod's approach as a criticism of the "unnecessary complication caused by the static and dynamic determinants" (214). The endogenous changes in the dynamic determinants, however, are by no means an "unnecessary complication," but the quintessential feature of Harrod's approach, and Samuelson was well aware of this. He aimed at showing how even a very simple model with only two equations, two unknowns, and two parameters can give rise to different behaviors (asymptotic approach to a steady state; cumulative growth; damped, constant, or explosive oscillations) depending on the value of the parameters, without necessarily having to resort to nonlinear equations. Accordingly he was able to conclude that "contrary to the impression commonly held, mathematical methods properly employed, far from making economic theory more abstract, actually serve as a powerful liberating device enabling the entertainment and analysis of ever more realistic and complicated hypotheses" (Samuelson 1939a, 78).

Harrod's theory, on the other hand, is intrinsically nonlinear: its philosophy is based on the epistemic, preanalytical premise that equilibrium must be unstable in order to allow for endogenous fluctuations;¹⁸ the

18. The word "preanalytical" should be intended both in its logical and chronological sense. The logical aspect is briefly illustrated in the text: the chronology indicates that Harrod devel-

multiplier-accelerator provides a mechanism capable of cumulating the effects of disequilibrium; and the nonlinearities (not only the dynamic determinants, but also ceilings and bottlenecks [Harrod 1936b, 102, 114]) maintain the deviation from equilibrium within boundaries and eventually reverse the direction of movement. Harrod, however, lacked the mathematical capacity for turning his verbal theory into a full-blown model,¹⁹ and probably would not have liked it: as G. L. S. Shackle (1967, 270) observed, *The Trade Cycle* is concerned with “what happens in a world of humans, whose prime requirement before they act is some data and recorded facts about what others *have been* doing, and who must form on the basis of these data some expectations and intentions about what they themselves *will* do. . . . the *Trade Cycle* [is] full of the natural give and take of a world where people must find out, compare, decide, before they act; then register results and make fresh plans and decisions.”

The relationship between Harrod's version and Samuelson's is one of indirect filiation, via Hansen; but along with some elements of continuity, there are also some important discontinuities. Samuelson interpreted Harrod's contribution in a spirit completely different from Harrod's: Samuelson couched the multiplier and accelerator in terms of two independent lagged equations with constant coefficients, and related them to each other by feeding one equation with the result of the other. Such an arrangement still represents a process similar to the one described by Harrod, but one which acts mechanically, without room for expectations, trade unions, changes in people's behavior, decision making and revising, interactions between economic agents, government action, etc.; in other words, Samuelson produced something quite different from what Harrod had in mind. While borrowing the idea from Harrod and Hansen, Samuelson invented a *new* approach to the whole problem. More precisely, he was taking part in the redefinition of the problem of economic dynamics, in its conceptual reorganization, and in the choice of a new point of view on it, which began with the work of the econometricians (in particular Frisch, Jan Tinbergen, and Kalecki) and eventually became the mainstream approach after the war (thanks, of course, to

oped the logical point *before* being able to envisage a mechanism capable of satisfying it—indeed, before becoming acquainted with either the multiplier or the acceleration principle; see, for a discussion, Besomi 1999b, chaps. 1.2 and 3.

19. In this Harrod was not alone, as nonlinear functional equations were a largely unexplored field of mathematics, at least in Western countries; even Samuelson, having noticed the intrinsic theoretical irrelevance of his own assumptions, stopped short of elaborating further.

Samuelson's own *Foundations*). The choice of switching from one analytical language to another is not neutral, as it takes part in the definition of the object of research. For instance, expressing the problem in terms of functional equations as opposed to verbal treatments requires that the economist define variables as functions of mechanical time and take into account only what is susceptible of being expressed as an equation; it also implies that notions such as "law," "cause," and "time" be redefined. In other words, the object of research has to be reinvented in a form suitable to that specific mathematical treatment, and its interpretation takes place in a new conceptual framework. This, of course, is not to say that verbal analysis, with its own premises and possible outlets, is better or worse than a mathematical treatment, but only that verbal analysis and mathematical treatment are based on distinctive assumptions and different interpretations of the problem itself: neither of them is the "natural" development or a "mature" version of the other. They are different perspectives on a different object, which, however, have an idea in common.

Whether we want to give Harrod's or Samuelson's name to the result of this process of redefinition depends on what we are looking for. If we are after an explicit formulation of the interaction between the multiplier and the acceleration principle, then Harrod is its father. If we are content with some looser definition of the fundamental relationship and just want a mechanism working more or less along the same lines, linking consumption and accumulation as "mutually interacting forces, returning on each other in a vicious circle of cumulative disturbance," then there may be other claimants to priority—in particular J. M. Clark²⁰ and Wesley Mitchell before him. If closure of the Keynesian system is our aim, then Kalecki probably came first, even before the Keynesian system was fully thought out. If we are happy with an equation describing the rate of growth of income, we can go back to Cassel. If we want both an explicit formulation and an elegant mathematical discussion, then Samuelson is our man; and he is also the ancestor of the modern versions of the multiplier-accelerator models.

Heertje and Heemeijer began their article by pointing out that "it is not always easy to pinpoint the intellectual provenance of a scientific theory or model." Not only is it indeed difficult, but the search for forerunners may surreptitiously induce readers to overlook the fact that the various trains of thought inspiring the formulation of a new theory,

20. The quote is from Clark 1932, 693; see note 2 above.

model, or approach are often interpreted in an altogether different way than they were originally intended; that they are abstracted from their specific context and immersed in a different one; and that they are often turned into something which bears only a vague resemblance to the original. The risk is that of reducing the complexities of the history of thought, with its respect for the plurality of approaches examined as interacting entities, to a history of analytical instruments, which could easily turn into the apologetic narration of a linear development of economic theory (in the singular). Perhaps it is better to remain agnostic and keep wondering: "God knows who influences whom and when and how!" (Robertson to Harrod, 28 April 1950).

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