

Political Events and Economic Ideas

Edited by

Ingo Barens

Technische Universität Darmstadt, Germany

Volker Caspari

Technische Universität Darmstadt, Germany

Bertram Schefold

*Professor of Economics, Johann Wolfgang Goethe-University,
Frankfurt am Main, Germany*

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PART VI

The Jérôme-Adolphe Blanqui Lecture

19. Harrod's dynamics in the making

Daniele Besomi*

Traditional treatments and traditional solutions are being questioned, improved, and revised. In the end this activity of research should clear up controversy. But for the moment controversy and doubt are increased (Keynes, 1928).

Economics with its 'schools' is still in the phase of quasi-scholastic, in which rivals can persist in their rivalry and mutual invective (Harrod to Knight, 7 July 1937 in Harrod, 2003, vol. 2, p. 708).

When I was invited to give the Blanqui lecture it was understood that the topic would be centred around my book, *The Making of Harrod's Dynamics* (Besomi, 1999b). Although I hope to be able to convey its essential thesis, I would also like to spend some time in describing what lay *behind* the book, and to give you further reflections generated by my recent work *beyond* that research.

I begin from the beginning of the story. My research interest has always been the history of 'formal' economic dynamics.¹ I was first struck by the singularity of Harrod's notion of dynamics when comparing it with other definitions collected in Machlup's essay on the semantics of statics and dynamics (Machlup, 1963). While most authors from the early 1930s onwards had adopted the definition of dynamics proposed by Ragnar Frisch, according to which a theory is dynamics if it explains, by means of functional equations, 'how one situation grows out of the foregoing' (Frisch, 1933, 1936), there seemed to be two major exceptions: Hicks, who suggested that dynamics is characterised by quantities being dated, and Harrod, who claimed that a theory is dynamic if its unknowns are the rates of changes of the main variables (output, in particular). Having already studied two major representatives of the 'mainstream' – my first work was on Kalecki, then I examined Richard Goodwin's strategy for winning consent on the argument that dynamics should be non-linear² – I decided to devote *some* time to these two 'freaks' (this was more than ten years ago . . .). After a cursory reading of their major writings on the subject, Harrod's case seemed to me more intriguing, as along with his notion of dynamics cited by Machlup I found a number of other definitions, often widely different from each other, which he offered in numerous books and articles.

My first problem was whether there was any consistency between Harrod's apparently unrelated statements. One of the definitions conceived of dynamics as 'concerned with an economy in which the rates of output are changing', the unknowns in the equations to be solved not being rates of output per annum, but 'increases or decreases in the rate of output per annum' (1948, p. 4; see also 1939a, p. 17). Harrod, however, also defined dynamics as involving the formulation of 'laws establishing the necessary relationship between various growth factors' (1955, p. 359), as the study of 'the relations between the rates of increase (or decrease) of certain magnitudes in a growing economy' at a given point of time (1960, p. 277) – in particular with regard to the mutual determination and consistency of these rates of change (1962, p. 1009; see also 1938, pp. 402–4, and 1939b, p. 164) – as regarding 'the formulation of a new set of propositions, relating to the increase of wealth and income' (1936a, p. viii), as 'referring to propositions in which a rate of growth appears as an unknown variable' (1939a, p. 17; 1937a, p. 86), as 'concerned with the determinants of the rates of increase of the main categories of demand' (1973, p. 11), and as 'purporting to specify what the steady rate of growth at a given point of time would be' (1959, p. 454); in addition he also stressed that the characteristic feature of dynamics as opposed to statics consists in whether or not the rates of change are continuous (1948, p. 6; 1957, p. 193; 1963, p. 402), and whether or not there are savings and investment (1948, pp. 11–12; 1959, p. 454).

Second, I was particularly puzzled by Harrod's idea that dynamic laws refer to one point in time: instantaneous dynamics is surely a paradoxical notion! Yet Harrod seemed to have built high hopes on it, as he claimed he was providing the foundations of a new and revolutionary science of economic dynamics, and systematically and consistently criticised the 'time-lag theories of the cycle', a portmanteau expression under which he grouped the approaches of the econometricians (Tinbergen, in particular), D.H. Robertson, the Swedes and John Hicks. What he actually meant, however, never became clear to me in spite of several readings and re-readings of his main published contributions on the subject.³

Thirdly, was Harrod's claim justified, that he was saying something intrinsically different from, and more fundamental than, dynamics as defined by Frisch? As Hicks pointed out (1950, p. 7), the econometricians' approach enables us at once to determine the volumes and the rates of growth of the system's variables, and their mutual determinations, not only at one point of time but – given some initial conditions – at all times. As all of Harrod's requirements are encompassed within the rival definition, should Harrod's dynamics be considered a subset of the more powerful approach of the econometricians?

I found some of the answers I was looking for in Harrod's correspondence with Keynes on *The Trade Cycle* and the 'Essay in Dynamic Theory' (Harrod, 1936a, 1939a), printed in Keynes's *Collected Writings* (vol. XIV): when he was taken to task by his friend and was asked the right questions, Harrod was forced to provide clear-cut replies. The discussion revolved around two aspects relating to the instantaneousness of Harrod's dynamics. One was methodological, as Keynes at first missed Harrod's point that his trade cycle theory was based on deviations from an equilibrium *status* of continuous growth for which the appropriate method of analysis, in Harrod's view, was to take a cross-section and study the relations between the various magnitudes. The second was analytical, as Keynes questioned that Harrod's instability result held independently of the interval used for calculating the rate of growth while Harrod, in accordance with his methodological principle, maintained that lags are irrelevant for the study of a moving equilibrium (Harrod, 1939a, p. 20).

I place these first observations of mine in perspective later in the lecture; for the time being, I only wish to emphasise an immediate consequence of the study of this correspondence: it suggested that if more correspondence was extant, with other colleagues equally intelligent and inquisitive, it could reveal much more about the ideas lying behind Harrod's cryptic published papers. The excerpts published in Warren Young's book on *Harrod and his Trade Cycle Group* (1989) looked promising enough to justify more extended studies. This implied a prolonged stay in Japan, at the Chiba University of Commerce, where most of Harrod's papers are collected. Fortunately, Harrod turned out to be a passionate hoarder, and to have had a number of intelligent friends who, like him, enjoyed writing letters.

Working on Harrod's papers in Japan was one of the most intellectually exciting times of my life. I arrived only having a few questions, some vague ideas as to the kind of answers I expected and some prior knowledge of the list of papers deposited there. I soon found out that the correspondence was far richer in content than I had expected, as it dealt with a number of issues which were hardly, if at all, mentioned in Harrod's published essays, and that its arrangement was not conducive to understanding what was going on.⁴ Additional problems were encountered by finding unsigned, undated or misdated correspondence, misattributions by the cataloguer or missing correspondence altogether. Besides, the Harrod papers included, rather naturally, the correspondence Harrod *received*, not what he *sent*. I was therefore faced with a gigantic jigsaw puzzle (the Chiba collection consists of approximately 15,000 leaves), of which I recognised some pieces, many other pieces I did not expect to find, some pieces were missing altogether (for a number of them, however, there was a chance they were preserved in other archives – which turned out to be the case),⁵ and I did not have an

overall picture of the final composition (which, of course, I could not assume to remain constant, as people's ideas change from time to time).

The correspondence often took the form of comments on Harrod's or his correspondent's writings (either published, intended for publication or destined to remain unpublished), and rather often resulted in prolonged debates in which an agreement between the correspondents was not reached. This aspect had already emerged from the exchanges of letters included in Keynes's *Collected Writings*, not only those concerned with dynamics but also in the discussion on the Keynesian and traditional theories of interest which was stimulated by the proofs of *The General Theory*. In spite of Harrod's 'horror of public debates' (which, he believed, 'lower the prestige of economists in the public estimation'),⁶ some of the most interesting and informative exchanges were in the form of controversies, a number of which also had an echo in published writings, such as the correspondence with Haberler, Robertson and Kahn on banking policy, the price level, and saving and investment in 1934 (Harrod, 1934, 1935), with Joan Robinson in 1933 on decreasing costs, excess capacity and 'normal profits'⁷ and again in 1937 on the classification of technological progress,⁸ and the above-mentioned exchange with Keynes on saving, investment and the theory of interest (Harrod, 1937a).

Controversies, whether in private or public, provide an additional challenge for the interpreter: the cause of disagreement is often not clear to the participants in the debate, and the exchanges are characterised by mutual misunderstandings which make it impossible for them to reach an agreement and solicit later readers – who have the benefit of hindsight – to inquire on the implicit assumptions, incompatibilities between concepts or world views, and paradigmatic switches which characterise these debates. This is often a fruitful line of work, and my own research has on several occasions benefited from Harrod's (and some of his correspondents') quarrelsomeness, stubbornness and verbosity.

I return later to some of the features of archival research which made my inquiry such an exciting time. It is now time to outline the main results I reached – and update them from my most recent work.

Depicting the specific features of Harrod's thought on dynamics is rather difficult if one wants to keep an eye on the details, especially regarding the genesis and early development of his ideas: Harrod concurrently pursued several interests, not only in the discipline of economics (he wrote on imperfect competition, monetary and banking policy and theory, international economics, utilitarianism, and population) but also in philosophy and politics, and all these aspects influenced, sometimes crucially, the development of his thought on dynamics. For this reason, in *The Making of Harrod's Dynamics* I followed a chronological approach, which enabled me to

expound the various elements of Harrod's dynamics in the same order as he came upon them. Here I will follow another route, using as a thread a single principle which, logically and chronologically, *organises* Harrod's thought on dynamics, and is therefore the key to understanding it in its making and later developments – namely, the instability principle. As in the book, I will focus principally on interwar years.

A theory of the trade cycle, Harrod argued, must include at the outset some destabilising factor. If stabilising forces prevailed, in fact, a disturbed system would tend to return to equilibrium, and the persistence of the cycle should be explained by means of exogenous factors. A proper theory of economic fluctuations should instead be able to provide an endogenous explanation. This principle was developed by Harrod well before he devised his specific trade cycle mechanism based on the interaction of the multiplier and the acceleration principle: its first appearance is in a published article reviewing the 'Doctrines of Imperfect Competition' (May 1934), where it was applied to the criticism of Pigou's psychological theory of the cycle, while the multiplier-acceleration mechanism dates from the end of 1935. Soon after publication, Harrod stressed – in correspondence with Haberler – the epistemic role of this principle, describing it as 'just the very kind of explanation which a rational account of the trade cycle requires' (Harrod to Haberler, 19 October 1934; the argument is repeated in the same letter and again in another letter to Haberler of 5 November 1934 in Harrod, 2003, vol. 1, pp. 304 and 333 respectively).

Harrod seems to have developed this idea from Hayek's rendition of the argument originally developed by Adolf Löwe in 1926 (Löwe, 1997). In *Monetary Theory and the Trade Cycle* (to which Harrod referred in the above-mentioned correspondence with Haberler) Hayek admitted that there is a difficulty as trade cycle theory relies on the logic of equilibrium theory, which has to assume that the *disturbances* to equilibrium are of an exogenous nature, and interpreted the problem of the trade cycle as consisting in understanding why 'the forces tending to restore equilibrium come temporarily ineffective and why do they only come into action again when it is too late' (Hayek, 1933, pp. 42–4, 65). Harrod radicalised Hayek's approach, as he saw – like Löwe – the problem as consisting not only in the *departure* from equilibrium, but also in the *persistence* of fluctuations.⁹

Harrod at first tried to use imperfect competition as the destabilising factor: if returns are still increasing at equilibrium, a deviation from it would prevent readjustment at the same level as before (Harrod, 1934a, pp. 465–70). However, this road proved impracticable for constructing a theory of the cycle, as a few months later Harrod wrote to James Meade that his ideas on the cycle were still 'amorphous in the extreme' (Harrod to Meade, 4 October 1934 in Harrod, 2003, vol. 1, p. 295). The elements for building

a working model came later: Harrod seems to have learned of the possible use of the acceleration principle for trade cycle theory from Haberler's League of Nations survey, a draft of which Harrod read during the summer of 1934, and with which he experimented in March 1935.¹⁰ As to the multiplier, Harrod first appreciated its implications only after having read *The General Theory* in proof during summer 1935.¹¹ The idea of combining these two principles seems to have occurred to him late in 1935, and the resulting mechanism was shortly described in a paper read on 10 January 1936 in Copenhagen (Harrod, 1936). A few days later, the first draft of *The Trade Cycle* (1936a), the most complete exposition of Harrod's ideas on dynamics, was sent to Meade for comment.

In *The Trade Cycle*, the instability principle was applied twice: to escape from static equilibrium and make movement possible, and to make the cycle possible by permitting deviations from the moving equilibrium. These steps were taken in turn, as Harrod believed that the study of the laws determining how the *level* of output is determined should precede the study of its *variations*.¹²

For the first stage, Harrod recast the partial equilibrium approach, generalised by the introduction of imperfect competition,¹³ in terms of forces acting on individual entrepreneurs and causing them to increase or decrease their levels of output. These independent vectorial forces, which he called *static determinants*, are '(i) The rates of pay at which prime factors of production can be secured. (ii) The efficiency of the prime factors. (iii) The elasticity of demand for commodities. (iv) The general price-level' (Harrod, 1936a, p. 50). The first three of these and their associated laws (the plasticity of prime costs, the law of diminishing returns and the law of diminishing elasticity of demand) act as stabilisers: an increase of production would, for instance, decrease the efficiency of labour, and thereby discourage, *ceteris paribus*, a further raise of output. The fourth determinant is a destabiliser, as an increase in the general price level would – everything else being given – induce entrepreneurs to increase production. The stability of equilibrium depends, of course, on the balance of these forces. Harrod argued, on the basis of the instability principle, that equilibrium cannot be stable, as we actually observe persistent movements in economic variables; nor can it be widely unstable, for otherwise we would observe cumulative growth or depression. Equilibrium must therefore be neutrally stable, i.e. the destabilising effect of the monetary determinant must exactly counterbalance the stabilising effect of the other three determinants.

The latter proposition looks like sleight of hand, and Harrod was indeed aware that it involved a 'change of method' (1936a, p. 37): while the stabilising effect of the first three determinants is derived from introspection, Harrod deduced that money is a destabiliser not because of any of its

intrinsic qualities, but from the epistemic necessity dictated by the instability principle. The role of this assumption is to make movement possible, and compatible with whatever is decreed by the forces responsible for motion, in strict analogy with a ball lying on a flat surface: the ball would rest wherever it is placed, or would move when subjected to an external force, offering neither resistance nor cooperation.

The independent vectorial forces responsible for motion are the *dynamic determinants*. Instead of affecting individual entrepreneurs, they act on the system as a whole. They are, in fact, the parameters determining the magnitude of the multiplying and the accelerating effects, i.e. the propensity to save, the distribution of income and the quantity of capital necessary for the production of a unit of output. The working of Harrod's proposed mechanism is well known: expected increases in consumption determine the amount of necessary investment (acceleration principle); new investment implies additional income (multiplier), part of which is spent on consumption goods. One of the possible results is that the actual increase in consumption matches what was expected: in such a case the whole system would be in a state of dynamic equilibrium. This, however, is only one among other possible outcomes, as actual consumption could be in excess or short of what is actually produced.

Here is where the instability principle comes in once again: if the moving equilibrium were stable, the natural state of the system would consist in cumulative growth at a constant rate, cycles could only be explained as a consequence of exogenous forces determining regular fluctuations, and the burden of explanation would be shifted on to the causes of such a regular behaviour. But the moving equilibrium is unstable: if, for instance, actual consumption were not up to the mark, the ensuing additional investment would be reduced, income and therefore also consumption would increase less, the situation of disequilibrium would reproduce and aggravate itself. Not only random deviations from equilibrium are amplified by this mechanism, but there are forces at work preventing the system from settling into an equilibrium state: the growth of income, in fact, whether or not at the equilibrium rate,¹⁴ triggers changes in the propensity to consume (for Keynes's psychological principle, a richer community saves proportionately more) and the distribution of income, thereby affecting the multiplier. The accelerator is not constant either, as the decrease in the rate of interest during a slump stimulates the adoption of more roundabout methods.

The moving equilibrium is therefore an unlikely state of the system: even if the economy can, for short periods, grow at a constant rate, sooner or later a cumulative deviation from equilibrium will set in. The boom is broken, and eventually reversed, by the adverse changes in the multiplier and/or the reaching of full capacity, the slump is interrupted and reversed by the necessity of

replacements and/or the more favourable acceleration coefficient. The *vera causa*¹⁵ of the cycle lies thus in the instability of the moving equilibrium.

We have seen that Harrod's reconstruction of the traditional static method of analysis in terms of forces aimed at making static equilibrium compatible with movement. But how precisely are static and dynamic forces linked to each other, the former affecting entrepreneurial decisions and the latter the conditions of reproduction of the *status* of the system as a whole? The first three static determinants depend on individual preferences, and Harrod thought that there are no reasons to think that these fluctuate in tandem with the cycle. The force inducing entrepreneurs to adapt to what the dynamic determinants decree is the general price level. In conditions of advance, the general price level increases, and accordingly entrepreneurs find it convenient, *ceteris paribus*, to increase their own level of output; conversely, during a slump prices decrease, inducing producers to slow down. Price fluctuations, however, should not be regarded as the cause of the cycle: they are only the 'lubricant' of the system (Harrod, 1936a, p. 126), and represent and measure the effect of the resistance of the first three static determinants against increasing production.

The next question is, what induces the price level to fluctuate in such an accommodating way? The velocity of circulation of money is brought into play at this point. Harrod's description of this mechanism is rather lengthy – and in truth not fully convincing (Harrod, 1936a, pp. 125–46). It relies on the time-lag between the receipt of income and its expenditure (Robertson's assumption):¹⁶ when activity begins to slow down, people spend more than they should, firms cash this money but reduce their level of production. There is therefore an unwanted accumulation of cash holdings in the hands of the firms; part of this may be used to pay off the firms' debts, but there is no reason to suppose that all the cash will be used for this purpose: money can be, and indeed is, used as a reserve of value and as a capital asset, and in this case the velocity of circulation drops. If this datum is inserted in the quantity equation of money, considering that the level of activity is jointly determined by the static and dynamic forces, and that the quantity of money is determined by the banking policy, fluctuations in the price level depends on changes in the velocity of circulation.

The instability principle therefore organises a double causal bound between statics and dynamics: fluctuations in the velocity of circulation *permit* changes in the price level, which in turn allow for output fluctuations; the static and dynamic forces, on the other hand, *determine* how production, prices and velocity of circulation should fluctuate. In Harrod's view, statics and dynamics were thus integrated; Harrod actually saw statics as the foundation of dynamics, and dynamics as a complement to, and a generalisation of, statics. Harrod was naturally aware that the 'traditional theory' of the

determination of prices and quantities by means of supply and demand curves had recently been subject to criticism: Sraffa had questioned the independence of demand and supply curves for commodities, Keynes questioned the independence of the supply and demand curves for capital. Harrod interpreted both criticisms not as logically cogent, but as limiting the domain of validity of traditional theory, and proceeded to its generalisation. He thought – and in this, of course, he was not alone – that Sraffa's problem would be solved by allowing elastic demand curves, by means of which the various degree of market imperfection could be seen as a continuum, and accordingly integrated the partial equilibrium approach by introducing the law of diminishing elasticity of demand. As to Keynes's problem, Harrod admitted that traditional theory cannot satisfactorily deal with the increase of output caused by saving and investment, but rather than discarding the whole theory on grounds of inconsistency he limited its domain to *statics*, which he characterised as the absence of saving and investment. But, given these modifications, Harrod thought that the utility functions and the other forces affecting individual behaviour must still provide the background for – and indeed the *foundation* of – dynamic analysis.¹⁷

Harrod firmly believed in the continuity with the economic tradition inherited from Marshall, and indeed urged Keynes (via Kahn) to express his ideas 'in terms which are used in marginal analysis, so that it can be fitted in to the corpus of economic theory such as it is' (letter to Kahn, 17 November 1934 in Harrod, 2003, vol. 1, pp. 345–6). As a matter of fact, Harrod not only retained the method and the analytical tools of the partial equilibrium approach for his own rendition of statics, but these were also at the basis of his dynamics, which was also formulated in terms of independent vectorial forces.¹⁸

Harrod, however, also maintained that the limitations of traditional theory as denounced by Keynes should be overcome, and saw his own dynamics as the truly revolutionary step *beyond the traditional economics*:¹⁹ not because it accounted for saving and investment in a way which, Harrod thought, Keynes failed to do properly,²⁰ but because of the introduction of the instability principle. While there is methodological and analytical continuity, the fundamental break with tradition does not regard the object of analysis, but is of an epistemic nature: Harrod's modification makes movement possible, and indeed necessary, while the static equilibrium reduces to a state of rest.

So far I have described Harrod's approach to dynamics (as expressed in *The Trade Cycle*) in terms of the implications of the instability principle. Although I believe this to be Harrod's seminal theoretical innovation, his dynamics were naturally also characterised by other components and were subject to other influences. The idea that the trade cycle takes place against the background of steady progress, for instance, was part of the shared

premises of the Oxford and London economists engaged in 'missionary work' for the New Fabian Research Bureau (the Keynesian Harrod and Meade, and the Hayekian Durbin and Gaitskell respectively). Its origin is almost surely to be found in Robertson's *Banking Policy and the Price Level*, but the political element in this assumption should not be underestimated, as Harrod's first formulation of this idea took place in a contribution to the ongoing debate with the London Fabians on the implications of monetary policy (the title itself of Harrod's paper is illuminating: 'The Expansion of Credit in an Advancing Community', 1934b).

The major external influence on Harrod's economic thought, however, came from his philosophical reflections on the 'mapping' of the possible states of the world into scientific laws and concepts, and on induction and deduction. Although these problems had occupied Harrod's mind since at least 1935, we only see their full implication for dynamics in the 'Essay in Dynamic Theory' (Harrod, 1939a), where Harrod recast his 1936 model in axiomatic terms. This reflects the logical positivist tenet that meaningful propositions are either empirical or tautological: Harrod turned his dynamic 'forces' into determinants of the supply of and demand for savings, based on an inductive generalisation from empirical observations, while the equality between saving and investment is a tautology.²¹

The most important development deriving from Harrod's philosophical reflections concerns the distinction of the different stages in dynamics. I have already mentioned that in 1934 Harrod developed the notion that the appropriate method for the study of equilibrium states is to take a time section and examine the relationships between relevant magnitudes. From 1938 on he developed the view that the construction of economic laws should proceed in three stages, the first being a description of the instantaneous state of the system, the second dealing with the succession of events and the third concerning policy applications. Accordingly the 'Essay' (or, better, its original draft: Harrod, 1996) was neatly divided in three parts: the instantaneous analysis of the moving equilibrium and the discussion of its stability, in which coefficients are taken as given; the succession of the events occurring during the trade cycle, when the multiplier and acceleration coefficients change following income variations; and policy issues arising from different relative positions of the warranted (equilibrium) and natural (maximum feasible) rates of growth.

It is now clear that the different definitions of dynamics given by Harrod throughout his career all belong to the same composite notion, of which they represent different facets. The stress on instantaneous analysis and on the mutual consistency of rates of growth reflect a methodological concern; the distinction between continuous and *per saltum* changes reflects the analytical properties of dynamic and static equation, as the equations deter-

mining the level of output must take the 'fundamental conditions' (preferences, cost functions etc.) as given while equilibrium growth implies a continuous change of output; finally, the reference to the presence of saving and investment reflects an ontological property of the dynamic system, as opposed to its absence in a static system.

So far as the 'time-lag theories of the cycle' are concerned, Harrod had the feeling that they shifted the burden of the explanation of the cycle onto the lags themselves, and therefore were subject to the same criticism as Pigou's psychological theory, namely that they resorted to frictions, maladjustments and errors of calculation rather than identifying the 'vera causa' of the cycle (letter to Tinbergen, 1 July 1937, in Jolink, 1995, p. 442 and in Harrod, 2003, vol. 2, p. 706) – the instability of equilibrium. The charge is explicitly addressed to Robertson (but analogous statements can be found with respect to Hicks, Lundberg, and the econometricians): 'What I feel about people broadly in your position is that you cling a little too tenaciously to the view that the classical analysis shows that the system must be self-adjusting in the end. You are inclined therefore to emphasize time-lags and miscalculations' (Harrod to Robertson, 25 December 1936 in Harrod, 2003, vol. 2, p. 599). Harrod did not deny that lags play a part in the trade cycle; they are, however, only relevant in disequilibrium situations, as during growth at a constant rate they are systematically discounted by entrepreneurs and consumers alike. Lags therefore only come in in stage two of dynamics, and for this reason Harrod always considered these kinds of explanations as 'less fundamental' than his own theory.²²

I find Harrod's dynamics, especially as presented in *The Trade Cycle*, a somewhat grandiose system of thought, as they explicitly encompass static and dynamic forces, characterise individual and systemic equilibria, and are based on a reflection on all different aspects of the discipline (epistemic, methodological, analytical and ontological). Their relationships to traditional theory and to the rival explanations were clearly the subject of much thought. This, of course, is not to say that Harrod's construction is watertight. Far from it: there are indeed some fundamental flaws, most of which commentators have readily picked up. The 'methodological break' from which the destabilising effect of the general price level is deduced, for instance, was harshly criticised by Hawtrey, who pointed out:

The acceptance of the association of rising prices with increasing activity and of falling prices with declining activity as a merely empirical conclusion has the serious disadvantage that it does not show which is cause or which is effect. It leaves Mr. Harrod, therefore, with a very unsatisfactory kind of determinant (Hawtrey, 1937, p. 327).

Others pointed out that turning points are not well accounted for (Hansen, 1937, pp. 525–7; Stafford, 1937, p. 76n.). Or again, although

Harrod took great pains to ensure that individual preferences were compatible with the outcome of dynamic laws, the whole notion is based on a confusion between individual and systemic notions of equilibrium, as he attributed to static equilibrium not only the maximisation of profits and utility but also the reproduction of its own state. Conversely, he saw the dynamic equilibrium not only as equating the aggregate demand and supply of goods and capital, but also as ensuring the satisfaction of entrepreneurs. Generally speaking, reviewers of *The Trade Cycle* failed to notice Harrod's attempt to reconcile the micro- and macro-levels of analysis and to export to dynamics the method of statics, and only discussed the technicalities of the cycle model. As to the epistemic postulate, a few commentators noticed that Harrod assumed a 'frictionless' world in order to avoid attributing the cycle to miscalculations (Robertson, 1937, p. 124; Hansen, 1937, p. 511, 523–4), but no one went further than that²³ – not even Neisser, who had been a member of the Kiel School where the epistemic principle was first developed by Löwe, realised that Harrod's position was exactly the opposite to that of the equilibrium approach (1937, p. 441).

The immediate consequence of these reactions (about which Harrod complained in correspondence with Robertson, Keynes and Henderson) was the abandonment of the analysis of the static forces, which was never resumed in his writings after *The Trade Cycle*.²⁴ But the subsequent versions of Harrod's theory were also not free of shortcomings. For instance, in spite of the disappearance of references to static equilibrium, in Harrod's 'Essay' up to six distinct and sometimes conflicting notions of dynamic equilibrium coexisted,²⁵ giving rise to the criticism that maintaining the same rate of advance is only one of the possible patterns of equilibrium entrepreneurial behaviour, and thus alternative rules could be devised (Alexander, 1950, p. 728). Harrod conceded that his assumption concerning the behaviour of entrepreneurs in equilibrium is 'rather special and may be unjustified' (1951a, p. 71), and tried to solve the problem of the consistency between individual and systemic notions of equilibrium by resorting to the notion of *representative entrepreneur*, which just raised another stream of objections.²⁶

But more important for the subsequent interpretations of Harrod's dynamics, a crucial assumption was not specified in the 'Essay', contributing to the distraction of the commentators' attention from the epistemic character of the instability principle. Bowing the knee to Keynes's insistence, Harrod submitted what he thought was a rigorous proof of the instability of equilibrium (letter to Keynes, 22 September 1938, in Keynes *CW* XIV, pp. 432–43 and in Harrod, 2003, vol. 2, pp. 870–73) in which, however, a vital step was missing. The analysis of stability cannot be instantaneous, as it involves comparison between *different* states of the system, for which some causal nexus should be

given. Such a complaint was first advanced by Marschak while reading a draft of Harrod's article,²⁷ and noticed again in 1948 by Baumol who was the first to attempt a more rigorous formalisation of Harrod's argument. A prolonged debate on the stability of the warranted growth rate ensued. Commentators filled the gap by inserting some behavioural equation (in the earlier years of the debate) or assumptions on the formation of expectations, and drew – not surprisingly – different conclusions according to the kind of additional equations they added to Harrod's formulation.

The debate took a different turn when neoclassical writers²⁸ believed they could attribute the instability of the moving equilibrium to the rigidity of the capital/output coefficient, which they thought instead should be flexible in response to changes in the rate of interest. These writers were soon echoed by Keynesian authors,²⁹ who attributed the instability instead to rigidity in the propensity to save. These interpretations altogether missed the role of different stages of analysis: the acceleration and multiplying coefficients were indeed taken as given, but only in the first stage, devoted to the characterisation of equilibrium at one point in time, while in the subsequent stage, devoted to the trade cycle, the variation of coefficients played a fundamental role. The second stage was ignored altogether, and Harrod's theory was seen as describing growth as an equilibrium process.³⁰

A further difficulty was technical: even if they had noted the intrinsic non-linearity of Harrod's theory, mathematical economists did not know how to deal with variable coefficients: non-linear dynamics was still in its infancy, and the pioneer's claim that he was inspired by Harrod's *Trade Cycle* remained unheard (Goodwin, 1951, p. 2n.). Nor, of course, did Harrod's poor mathematics enable him to work out neat formulas for his non-linear problem: his division in stages actually was instrumental to his incapacity of dealing with this problem, as he found a catchy equation describing the equilibrium growth rate, but for the cycle he could do nothing better than re-calculate everything with the new set of parameters consistent with the new level of income, instant after instant.

Yet some of Harrod's ideas turned out to be quite powerful, and were eventually implicitly vindicated by the later developments taking place within the 'time-lags theories of the cycle' which Harrod so fiercely opposed. When they learned how to deal with non-linearities, trade cycle theorists working in the Frisch tradition found out that cyclical solutions of functional equations are characterised by an unstable steady point in the phase diagram surrounded by limit cycles or strange attractors. Harrod's instability principle and his idea that changes in the major forces affecting the rates of growth, both developed quite intuitively, therefore jointly found a strong confirmation in rather complex mathematical models. It is even somewhat ironic that the solution to these models can

only be found by means of Harrod's method, namely by recalculating everything instant after instant (using computers, of course), as these equations do not normally admit an analytical solution to be worked out using paper and pencil.

These results make the relationship between Harrod and his contemporaries even more paradoxical than I had thought at the beginning of my research. I noticed, as a starting point, that Harrod was trying to say something which his 'antagonists' – the econometricians – did not understand and even snubbed; Harrod, in turn, failed to understand what the econometricians were saying. As, however, there clearly were some relevant points in common, both in terms of premises and of results, there must have been some problem in communication.

In the epilogue to *The Making of Harrod's Dynamics* I reviewed the secondary literature on Harrod's dynamics, which I read in chronological order interspersed with Harrod's own replies and restatements of the problem. Commentators were not able to see the gist of Harrod's dynamics and concentrated on his formula for equilibrium growth – which was, however, reinterpreted as describing a growth *path* rather than a rate of growth *at a point of time*. Harrod tried to meet specific criticisms by pointing to the corresponding feature of his dynamics – methodological, analytical, ontological – but never attempted to characterise the whole conception in its relation to the alternative notions of dynamics and, worst of all, did not stress enough the epistemic character of the instability principle, which at that point perhaps he was happy to consider a 'result' rather than a premise of his reasoning (after all, most of his fame among his contemporaries was linked to that proposition and to his formula). The history of Harrod's dynamics can therefore be summarised as the story of a *failure to win consent*: not, of course, a failure on all fronts, as his contributions certainly left a permanent mark in the history of economics (witness the success of notions such as the 'marginal revenue'³¹ and the 'international trade multiplier', or the 'multiplier-accelerator model' and the 'growth equation', all well codified in textbooks). Harrod failed, however, to make his contemporaries aware that he was proposing an alternative and rather complex definition of dynamics, and even less could he convince them that this was 'more fundamental' than the competing notions.

When I realised I was writing the story of a failure I had to ask myself whether I was wasting my time: surely exploring someone's mental laboratory, where so many widely different things were taking place at once, was a challenge and such an extraordinarily interesting exercise; but was it also a useful exercise? I decided it was – actually, I realised that it was even more useful precisely because it was the story of a failure – so a short discussion about this may perhaps be useful.

Harrod's original work in dynamics took place with the *years of high theory* in the background, when several theoretical systems competed for attention and supremacy (with their associated policy implications). He was obviously in touch with the most recent developments, either via personal acquaintance with some of the leading figures or via the literature. These developments nourished Harrod's thought, forced him to reconsider his own positions, posed new questions and proposed new answers, gave him new analytical tools and theoretical concepts. Harrod himself contributed to these developments, and he could see some of his ideas being integrated within one or the other of the new frameworks which were taking shape while, on the other hand, he also had to acknowledge that some of his other ideas never really caught on. Such a state of affairs was, of course, shared, to some extent, by the other participants in this theoretical turmoil. Debates were abundant, and only rarely (if at all) could they be resolved with an agreement. Some of these are retrospectively quite difficult to understand, especially when they turned into logomachies, the dissent regarding the meaning of words and the choice of the 'best' definition – the notions of saving and investment, in particular, were the subject of several multilateral debates which ended nowhere near an agreement.

There are (at least) two ways of interpreting such a situation. One could say that, as a matter of fact, some theories, conceptual systems or assumptions were eventually proven – in the light of subsequent development of the discipline – to be useless or plainly wrong, and were accordingly discarded. In this view, which sees the history of economic thought as a more or less linear cumulation of more and more useful and powerful analytical tools, the failure to win consent is symptomatic of confused, inadequate or fallacious thinking. On the other hand, one could see more in a theory than a set of analytical instruments: a theory also consists of points of view, which may or may not be compatible with the points of view in the background of other theoretical systems. With such a view, failure to reach an agreement is not a symptom of stubbornness and sterile fossilisation into one's own stale ideas in spite of which the world evolves, but rather reflects clashes of intrinsically conflicting doctrines. If one accepts this standpoint, debates – whether implicit or explicit, direct or via the pages of professional journals – and the success or failure of concepts and theoretical systems become the focus of the study of ideas from their genesis to their structuring and eventual acceptance, disappearance or rediscovery in a different form and context. Debates consist in the counterpointing of different positions; as such, they provide in fact a favourable setting for bringing to light the elements which make conceptual systems incompatible with each other.

Although Harrod may partly be blamed for not being sufficiently consistent and clear in the exposition of his ideas, the 'fault' cannot be entirely

his: in the various debates in which he was involved with Keynes, Joan Robinson, Haberler, Robertson, Kaldor and others, misunderstandings were mutual, and these people cannot all be thought to be particularly stupid. I have studied these debates with some care, some in my book and others in later writings. I have already referred to the gist of Harrod's exchanges with Keynes on dynamics and on the traditional theory of saving and investment. I would like to mention two other debates where effectual communication would have required in one of the participants a radical switch in his or her perspective on the problem under discussion.³²

Harrod criticised as ambiguous Joan Robinson's notion of 'neutral inventions' (which she borrowed from Hicks) as those leaving the ratio of marginal productivity of capital to that of labour unchanged when the relative amounts of the factors are unchanged (Robinson, 1937, p. 132), and suggested instead 'to divide inventions into those which *at a given rate of interest*, and an infinitely elastic supply of capital at that rate, increase, leave unchanged or diminish the length of the productive process' (Harrod, 1937b, p. 329). Harrod's review generated an extensive correspondence (of which, unfortunately, only Robinson's side is extant), which eventually led Joan Robinson to formulate the conditions under which Harrod's and Hicks's classifications of technological progress coincide (Robinson, 1938). Robinson's successive attempts at this problem are extremely interesting, as they show that she never understood that Harrod had in mind a completely different problem and that his proposal was based on a different conception of capital. Robinson's problem addressed how inventions affect employment in the long run, and she accordingly examined how new techniques alter the marginal efficiency of capital by affecting the production function. Harrod was interested instead in how inventions – everything else being given, including the rate of interest – alter the effect of the third dynamic determinant, namely the amount of capital used in production. His reference variable was therefore the amount of capital per unit of production, a notion which he associated with the Austrian concept of 'roundaboutness'. Although it is rather difficult to recognise this point in *The Trade Cycle* and in later published writings,³³ Harrod discussed it in detail with Kahn and Keynes in 1935 and again with Keynes and Hawtrey in 1937. Such a conceptual shift was too much for Joan Robinson: nowhere in the correspondence are there any hints that she understood that Harrod was approaching the problem from a completely different perspective.

But Harrod, as well, failed to see Robinson's point. This is indicated by an exchange with Kaldor about a year later: again, Kaldor argued about the rate of interest and the method of production having in mind a production function, while Harrod replied with reference to roundaboutness. Needless to say, they ended up by accusing each other of being 'tricksters

and knaves',³⁴ and failed to realise that they were talking about the same object in different and incompatible languages. The matter could be cleared only a quarter of a century later by Hicks who, with the benefit of hindsight, understood that his own 1932 problem was rather different from Harrod's, that their corresponding viewpoints altered accordingly, and that the tools and concepts developed for the one's problem simply could not be used for the other's.³⁵

A second example of failure to communicate is even more paradoxical. In 1932, Harrod applied Kahn's employment multiplier to foreign trade. He clearly understood the technicalities involved in the multiplier as an infinite series of global repercussions of local events, which he summarised in his foreign trade multiplier formula and saw expressed in a more rigorous mathematical form by Meade, in correspondence (1932) on a draft of Harrod's *International Economics* (Harrod, 1933b). At the end of 1932, Keynes, as well, commented on Harrod's draft, and clearly failed to see Harrod's point. This was not a temporary blank on Keynes's part, as the situation was repeated in February 1937 when Keynes read the restatement in *The Trade Cycle*. The problem had to be explained to him by Robertson (who is well known never to have liked multipliers!).

Meanwhile, Keynes himself transposed Kahn's multiplier to investment. Harrod was exposed to this formulation very early, as he certainly read Keynes's *Means to Prosperity* as it appeared in *The Times* in March 1933 (Keynes, 1972, pp. 339–45). Yet, he failed to understand Keynes's point. This is certified by an exchange Harrod had with Kahn in 1934, in which – after a 'tutorial' by Kahn on the new developments taking place in Cambridge, of which Harrod admittedly knew nothing – he considers the implications of the new point of view as 'frightening' and 'paradoxical'. He understood the message (but nonetheless never accepted the implications of the multiplier on the traditional theory of interest) only after having read the *General Theory* in proof during summer 1935.

The problem in the circulation and integration of the multiplier theory does obviously not reside in its technicalities, as both Keynes and Harrod had been independently capable of solving them. What both Keynes and Harrod missed in each other's approach was that they involved a radical conceptual shift: Keynes inverted the causal relationship between saving and investment, while Harrod inverted the nexus between variations in imports and exports; moreover, both had to change the variable responsible for bringing into equality the magnitudes they were dealing with, and both – and this adds further irony – referred to variations in the level of income. Such a conceptual shift must not have been easy: it took Keynes a long time to develop on his own the implication of Kahn's employment multiplier, and Harrod was consistently deaf to warnings that he was disregarding in

his multiplier the leakage due to saving,³⁶ and is not the causal nexus between saving and investment the main point on which Keynesian and neoclassical economics still diverge today?³⁷

These, of course, are just case studies, and one would not be justified in drawing sweeping generalisations from their implications. Nevertheless I believe these results to be of importance not only for the specific cases, but also as suggesting a possible key to understanding how new theories and concepts are born, established or discarded, with the shifts of perspectives and new viewpoints they carry with them.

For this reason I would like to conclude with a plea in favour of archival research. As I mentioned earlier, my book and a good deal of my subsequent research on Harrod are based on unpublished papers and correspondence. It is in the nature of these materials that they contain more debates and controversy than published writings: quibbles sometimes regard apparently minor points, misunderstandings are settled, or the resulting articles and essays may not find their way to journals or books for a number of reasons.³⁸ On this ground alone, exploring archives could prove extremely fruitful: apparently trivial details can, retrospectively, reveal their importance, and contribute to explaining how some ideas came about or were abandoned.³⁹

The importance of unpublished sources is sometimes belittled or altogether dismissed on the grounds of not representing the fully developed version of one's thought, which is properly expressed only in published writings. Patinkin even claimed that he would not consider press articles as evidence of the dating of theoretical statements on the grounds that such would not constitute part of one's central message to the scientific community (1982, p. 85). This view seems to me unduly restrictive, as it ignores the processes involved in the discovery of a concept of theoretical statement, including the numerous trials and errors, the exploration of different paths (and the reasons for discarding some of them), the influences of other authors⁴⁰ and of contemporary events, and so on. In other words, if such a view is accepted historians would have no means for exploring a theoretical construction in its making; but there lies all the intellectual excitement of the author, and also that of the historian.

As to my own research, of which I hope I succeeded in conveying the gist, I shall never be able to stress strongly enough how much I owe to what was buried among the documents collected and preserved by Harrod and his correspondents. And I am so convinced that much more is waiting to be discovered that I have undertaken the editing of Harrod's interwar papers and correspondence.⁴¹ The Harrod-related materials would naturally only cover a small fraction of the economists' papers available in archives, but I hope it will stimulate scholars to engage in similar research: it is hard and

labour-intensive work, but it is often rewarding – and it is certainly a lot of fun!

NOTES

* I am grateful to Nicolò De Vecchi, Geoff Harcourt and Bruna Ingraio for helpful comments and criticism. Geoff Harcourt also commented upon *The Making of Harrod's Dynamics*, of which he was external examiner when it was originally a Ph.D. thesis; due to an unpardonable oversight his name does not appear among those whose help was acknowledged. I take the occasion to remedy this here.

I am indebted to the libraries where Harrod's and his correspondents' writings are held for permission to examine the papers in their possession; without the kind collaboration of their librarians and archivists this research would not have been possible.

1. The terminology is taken from G.L.S. Shackle (1965).
2. Besomi, 1988, 1992. Of course I do not mean that Kalecki and Goodwin were orthodox economists, but only that they accepted the 'winning' notion of dynamics.
3. Commentators have indeed pointed out that 'Harrod's eccentricities . . . extended to his exposition' and 'something was lacking in the communication process' (Baumol, 2001, p. 1038), and that Harrod's 'own exposition of his model is almost as confusing as the interpretation that neo-classicals have put upon it' (Robinson, 1971, p. 110).
4. See note 41 below.
5. Besides the collections referred to in this paper (listed in the Reference section), important correspondence by, and documents relating to, Harrod are preserved among the papers of Keynes, Joan Robinson, Robertson, Durbin, Douglas Woodruff, Knight, the Fabian Society, Cannan, Lord Cherwell (F.A. Lindemann), Macmillan, H.B.W. Joseph, Haberler, Henderson, Hawtrey, Marschak, Meade, the Oxford University Archives, the Rockefeller Foundation. Other batches of Harrod's own papers are held at Nagoya University of Commerce and Business Administration, Tokyo University, Georgetown University, and the British Library.
6. Harrod to Haberler, 21 October 1934 (Harrod, 2003, vol. 1, p. 308). Similar remarks can be found, for instance, in a letter to Henderson of 9 April 1936 (Harrod, 2003, vol. 2, p. 545): 'I think that there are reasons in the interests of economics against being provoked into a dog-fight. I cant (*sic*) see that it will serve any useful purpose and merely make more stink.' The expression 'horror of public debates' is due to Robertson in a letter to Harrod of 4 October 1934 (Harrod, 2003, vol. 1, p. 297), replying to a letter not found where the words were probably used by Harrod himself.
7. Harrod, 1932, 1933a, Robinson, 1932. For a discussion see Sardonì, 1999.
8. This exchange is further discussed below.
9. At the time of writing *The Making of Harrod's Dynamics* it did not seem likely to me that Harrod was acquainted with Löwe's article, even indirectly (see note 11 to Ch. 1). The correspondence with Haberler, part of which I read after the book was published, suggested, however, that Harrod's version of the principle originated from the reading and criticism of Hayek – who was indeed familiar with Löwe's article. I elaborate on this in Besomi, 2002.
10. Letters from Kahn of 6 and 23 March, 6 April 1935; letters from Keynes, 21 and 28 March 1935 (in Harrod, 2003, vol. 1).
11. More on this below.
12. The principle was outlined a decade earlier, in an essay on 'The Trade Cycle and the Theory of Distribution' read before the British Association in 1925, now in Harrod, 2003, vol. 3, pp. 1021–32.
13. Harrod's generalisation of 'traditional theory' is further discussed below.
14. There is an asymmetry in *The Trade Cycle*, which Harrod eliminated in the later versions of his theory, as he seems to consider growth at an increasing rate and at a decreasing

- rate on different grounds: he did not worry about the cumulative effects of accelerating growth, nor did he set an exogenous 'ceiling' as he did with the notion of 'natural rate of growth' in the 'Essay'. The causes of downturn are entirely endogenous.
15. The expression was used by Harrod in a letter to Tinbergen, 1 July 1937 (reproduced in Jolink, 1995 and in Harrod, 2003, vol. 2, p. 706).
 16. Such a lag is irrelevant during steady growth, but becomes important at turning points. This is consistent with the methodological division of dynamics in stages that Harrod developed later, and on which more is said below.
 17. This view is not explicitly expounded in *The Trade Cycle*, but is clearly stated in the correspondence with Henderson (21 February 1936 in Harrod, 2003, vol. 2, p. 512), Keynes (15 April 1937, in *CW XIV*, p. 176 and in Harrod, 2003, vol. 2, p. 676) and Robertson (25 December 1936 in Harrod, 2003, vol. 2, pp. 600–602).
 18. Given the use of the independence assumption in his own analysis, one should not be surprised that Harrod could not see much point in Keynes's criticism of the neoclassical theory of interest and in Sraffa's remark on the lack of independence between supply and demand curves.
 19. This expression was used by Harrod himself, in a letter to his publisher describing a proposed collection of articles: 'The book would be divided into 3 main blocks of articles. One concerns method, the other the economics of growth and the third the economics of imperfect competition. They have this in common that they are all feeling out towards new developments of economic theory. I thought at first for a title of "Towards a new economics", but I don't (*sic*) quite like this because it suggests something too subversive, namely a scrapping of the old, which is not at all my intention. What do you think of "Beyond the traditional economics"?' (Harrod to Sisam, 26 September 1938 in Harrod, 2003, vol. 2, p. 875).
 20. 'In my judgement Mr. Keynes has not affected a revolution in fundamental economic theory but a re-adjustment and a shift of emphasis' (Harrod, 1937a, p. 85).
 21. Harrod's actual use of it, besides his intentions, is rather ambiguous, as he employed this equality to reduce the degrees of freedom of his system, and, in the *ex-ante* interpretation of the statement, as an equilibrium condition.
 22. This claim is repeatedly expounded in Harrod's published and unpublished writings: see for instance Harrod, 1936a, p. viii, and letter to Tinbergen, 1 July 1937, in Jolink, 1995 and in Harrod, 2003, vol. 2, p. 706. For a more detailed account and several further references see Besomi, 1998a.
 23. The anonymous reviewer in *The New Statesman and Nation*, however, rightly pointed out that the importance of Harrod's work 'lies in his attempt to give precision to the idea of trade fluctuation as arising, under the existing economic system, inevitably out of the instability of the dynamic relationship between investment and consumption' (1937, p. 220).
 24. The empirical inquiry of the Oxford Economists' Research Group laying stress on the full cost principle may also have contributed to Harrod's decision to abandon his attempt to use the price mechanism to explain how the macro-dynamic factors determining the oscillations of the magnitudes relating to the economic system as a whole are transmitted to the individual's decisions to expand or contract output. For an account of Harrod's participation in this research see Besomi, 1998b.
 25. The 'warranted growth rate' was conceived as 'the rate which (1) leaves each entrepreneur satisfied with what he has done; (2) allows for some individual disappointments but keeps entrepreneurs as a group, on balance, satisfied; (3) keeps them doing the same thing; (4) equates *ex ante* S and I ; (5) concerns only the part of investment directly linked to consumption; (6) somehow differs from the "proper" warranted rate which "would obtain in conditions of full employment"' (McCord Wright, 1949, p. 326).
 26. See for instance Newman 1954–55, p. 70; Pugno, 1992, p. 105; Asimakopulos, 1989, p. 353; Robinson, 1952, p. 47.
 27. "The reaction to the divergence $G - G_w$ as postulated in the Essay (the "cumulative process") may be written, I suggest, as $dG/dt = \lambda(G - G_w)$, where λ is some increasing function. Am I right in understanding this as an empirical assumption of the boom (and

- depression) rather than a conclusion following from the other equations. In fact, one could imagine the opposite case: a shortage of capital equipment being met by reduction of output rather than leading to an increase. It seems that the definition of the “unstable equilibrium” implies certain empirical postulates regarding the psychology of entrepreneurs’ reactions to current profits made in capital good production. Could this be stated explicitly?” (Marschak, ‘Remarks to R.F.H.’s “Essay in Dynamic Theory”” in Harrod, 2003, vol. 2, pp. 844–50).
28. Beginning from Fellner, 1951, pp. 116–22, followed by Pilvin, 1953, Yeager, 1954, and finally Solow, 1956 and Swan, 1956.
 29. In particular Kaldor, 1951 and 1957, Joan Robinson, 1970, Pasinetti, 1974.
 30. Keynes’s criticism to the first draft of Harrod’s ‘Essay’ is to be blamed to some extent, as it induced Harrod to compress the discussion of the trade cycle into a couple of sections of his paper, which failed to attract the readers’ notice. Harrod, in turn, failed to re-expand that section in his subsequent writings. After the war, moreover, the trade cycle looked like an out-of-date problem, and people were then more interested in growth.
 31. Harrod insistently claimed priority in discovery, in spite of Yntema’s precedence in publication, as the idea was submitted to the *Economic Journal* in July 1928. The story – supported by the documents Harrod preserved (see Harrod, 2003, vol. 3, pp. 1063–8, in particular note 1) – was reported in an ‘egoistic footnote’ to Harrod’s *Life of John Maynard Keynes* (1951b, pp. 159–60), and the ‘rather boring question of priorities’ was taken up again in Harrod 1967 (p. 65n.). The episode is also referred to in Harrod, 1972 (p. 394).
 32. A full discussion is obviously not possible here: see, however, Besomi, 1999a and 2000, respectively. Another example, involving Hayek and Joan Robinson, is discussed in Ingraio, 2004 and Ranchetti, 2004.
 33. Harrod’s classification of inventions was actually noticed by commentators, and gave rise to comments in print, as expressed in *Towards a Dynamic Economics*, where it was discussed at length. In *The Trade Cycle* there is only one sentence in a footnote (Harrod, 1936a, p. 91) indicating that he had thought about this problem; this was likely to pass unnoticed, were it not for a reference in a letter to Hawtrey of 31 January 1937.
 34. Harrod to Kaldor, 14 June 1938 in Harrod, 2003, vol. 2, p. 788.
 35. The point is acknowledged in a letter to Harrod of 30 January 1963, written while revising *The Theory of Wages* where the classification of technological progress was originally proposed (Hicks, 1932, 1963). The only early commentator who grasped Harrod’s point (before the debate on the relative merits of Harrod’s and Hicks’s criteria of classification of inventions) seems to have been Hansen (1937, p. 517).
 36. Kahn to Harrod, 24 March and 18 April 1932; Keynes to Harrod, 16 February 1933 in Harrod, 2003, vol. 1. Harrod only admitted this undervaluation in *The Trade Cycle* (1936a, p. 149).
 37. See on this aspect the literature cited in Dalziel and Harcourt (1997, p. 621). Sometimes Harrod was mistakenly supposed to be more acquainted with Keynes and Keynesian thought than he actually was (see e.g. Black, 1997, p. 89, and Brown, 1980, p. 26). I have the impression that – on theoretical rather than policy matters – Harrod was less close to Keynes than he thought, and closer to Hayek and the Austrians than he would have liked to think (as Hicks noted, ‘there are Hayekian influences . . . even on Harrod . . . , if one looks for them’: 1967, p. 205).
 38. Including the ‘horror of public debates’ mentioned above. This did not affect only Harrod; Henderson, for instance, wrote as follows: ‘I have allowed myself to be inhibited for many years from publishing many things by a desire not to quarrel in public with Maynard. Also I have a very lively sense which is confirmed by some incident or other nearly every day of the immense damage that is being done to economics as a subject, at a time when it is being presented with an unparalleled opportunity, by the logomachal controversies of recent years’ (Henderson to Harrod, 2 April 1936 in Harrod, 2003, vol. 2, p. 540).
 39. For further considerations on this point see for instance Marcuzzo, 1997 and Marcuzzo and Rosselli, 2004.

40. This could lead to troubles when an author does not cite his sources – as was the case with Harrod.
41. Since published as Harrod, 2003. The editing involved, among other things, a rearrangement of the transcriptions of the documents in chronological order, as they were written and read by Harrod. This process enabled me to understand something which was hidden by the original arrangement of the correspondence among Harrod's papers, in thematic or author order: Harrod sometimes changed his mind in the course of a prolonged exchange of letters under the influence of his contemporary correspondence with other authors. This is clearly relevant for the genesis of one's ideas, and cannot be fully appreciated until all the links between pieces of written papers are re-established. A particularly relevant example is the correspondence between Harrod and Haberler in 1934–35, taking place at the time Harrod was subjected to Kahn's tutorial on saving and investment described above in the text.

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