2 Failing to Win Consent: Harrod’s Dynamics in the Eyes of His Readers*
Daniele Besomi

One who has occasionally had the sense of being a voice in the wilderness (RFH 1950: 553)
In one of the cruel ironies often imposed by history, many fine thinkers win their posthumous recognition only by eponymous linkage with a principle so widely misunderstood that true views turn into their opposite (Gould 1993: 91)

1 INTRODUCTION

Even a cursory reading of Harrod’s pieces on dynamics and his readers’ early comments reveals that they were at cross purposes, the former urging the need for a new approach to dynamics and the latter interpreting Harrod’s theory as a model of steady growth. A study of the literature on Harrod’s dynamics is needed to uncover any recognisable trends in interpretation and explain their origins. I am particularly interested in Harrod’s notion of dynamics and his insistence on the difference between his own approach and the ‘time-lag theories of the cycle’, and I will therefore concentrate on those criticisms (or the lack of them) that elucidate how the peculiarities of Harrod’s method and conception were (or were not) perceived and integrated by the profession. I am less interested in the analytical aspects of the mechanisms Harrod proposed to explain cycles and growth, and thus the details of debates on the conditions for stability, the classification of technical progress, and the several models aiming at representing Harrod’s ideas are excluded from this survey. (This literature has been covered by other authors, and references will be given where appropriate).

Economic dynamics was one of Harrod’s favourite subjects, which he approached systematically from 1936 on. All of his writings raised considerable interest – though not always for the reasons he hoped for – and the secondary literature is vast. I have found about three hundred articles and books concerning various aspects of Harrod’s dynamics, and am sure that several more are in existence. This survey cannot, therefore, claim to be exhaustive, but I am confident that the main lines of argument are represented in what I have read so far.

I have examined most of this material, including Harrod’s own contributions, in chronological order. This procedure revealed that the interpretative traditions illustrated below consolidated very quickly, as can be inferred from the fact that references to Harrod’s original texts were soon substituted by references to commentators and that citations were often quoted (or misquoted) second hand. In the exposition I follow roughly the same order, privileging however some continuity in the narrative by following the debates within the different exegetical traditions.

Here, the opening concerns Harrod’s 1936 book The Trade Cycle. Reviewed in the major journals and discussed with Harrod in private correspondence, the book attracted considerable interest, with readers raising several problems eventually characteristic of debate around the subsequent version of Harrod’s theory.

The contemporary literature on Harrod’s writings between his 1939 Essay in Dynamic Theory and his 1973 book on Economic Dynamics almost unanimously interpreted his contributions as providing a theory of economic growth, and debates were based on this perspective. Initially, the Essay attracted little attention. However, soon after the publication of Domar’s paper in 1946 the similarity between their formulas was noticed (Schelling 1947: 864, 866), and was reinforced by the publication of Harrod’s London School of Economics 1947 lectures on dynamics (Harrod 1948). Within a few years, and for more than two decades, growth theory became one of the major topics of interest for economists, and Harrod was seen as one of its founders. His name was soon associated with Domar’s, and although at first some commentators were careful to point out the differences in their views (Robinson 1952, Pilvin 1952), before

* I am indebted, without further implications, to Mark Blaug, Nicolò De Vecchi, Geoff Harcourt, John Henry and Maurizio Pugno for useful comments and suggestions on a first draft of this essay. Financial support from the Swiss National Foundation for Scientific Research is gratefully acknowledged. I am also thankful to Chiba University of Commerce and to Lady Harrod and Dominick Harrod for permission to quote from Sir Roy’s unpublished writings, and to John Presley for permission to cite Robertson’s unpublished works.
long the distinction faded out or disappeared altogether, and by the early 1960s it was common practice to speak of the Harrod-Domar model.

During this second stage, some specific themes attracted readers' attention, giving rise to different lines of interpretation. Firstly, some interpreters pointed out that Harrod's notion of a 'warranted rate of growth' was somewhat ambiguous, and his assumptions regarding the equilibrium behaviour of entrepreneurs were not the only ones possible, nor necessarily the more reasonable. (These observations, together with Harrod's reaction, will be discussed in Section 3.1.) Similarly, early attempts to formalize Harrod's theory revealed the imprecise nature of his assumptions as to disequilibrium behaviour, and there followed a flurry of model production, based on alternative assumptions, designed to test Harrod's assertions on the instability of equilibrium (Section 3.2). The debate on stability soon took different forms, with neoclassical and neo-Keynesian writers (wrongly) criticising Harrod for having failed to consider the regulating power of changes in the capital/labour ratio and in the propensity to save, respectively (Sections 3.3 and 3.4). Later, the revived interest in expectations brought about the substitution of hypotheses regarding expectations for the behavioural assumption introduced by early commentators. However, this produced an interpretation not dissimilar to the former ones (Section 3.5). From the second half of the 1960s the conclusions of the preceding two decades of debates were consolidated in the textbooks on growth and on macroeconomics (Section 3.6).

From the 1970s some readers returned to Harrod's original writings, challenging the mainstream interpretation by pointing out the specificity of Harrod's dynamic method, the role of instability and non-linearity, and that Harrod's interest in the trade cycle was never dissociated from growth. These contributions are discussed in Section 4, which also examines the only dissident line of interpretation proposed in the 1950s, which originated from interest in the non-linear dynamics of growth and cycles.

Before studying these debates, it is perhaps opportune to point out that the discussion which follows does not claim to be 'neutral', but reflects my own judgement as to the themes and problems of importance and my interpretation of Harrod's dynamics. I thus refer whenever possible to my own writings on the subject, so that the reader is always aware of the presence of the writer and is able to track the premises of this exposition back to their source.

2 MOVING EQUILIBRIUM AND INSTABILITY IN THE TRADE CYCLE

Although The Trade Cycle was Harrod's first systematic theory of business fluctuations, some of its premises were clearly laid a few years earlier. Since 1925 Harrod had maintained that before enquiring into the causes of the fluctuation of output, one should understand the determinants of the level of production (Harrod 1925*), a principle he repeated in the preface of his 1936 book.

Furthermore, his reflections on imperfect competition led Harrod to formulate the principle of instability. In Harrod's view, the traditional approach to the cycle, having assumed the stability of the equilibrium level of production, had to introduce exogenous factors (such as systematic miscalculations on the part of entrepreneurs, or waves of optimism and pessimism) explaining the departure from equilibrium and the permanence of fluctuations. Harrod thought this procedure to be incorrect, and suggested that a factor of instability be introduced at the outset to make changes in the level of output theoretically conceivable. In 1934, he thought that imperfect competition could provide such a destabilising element, via the possibility of equilibrium at increasing returns (Harrod 1934a; for a comment see Besomi 1993, 1997, and 'Harrod and the 'Time-lag Theories of the (Cycle')', § 2, in this volume.)

Finally, the notion that proportionate and self-consistent growth in all sectors of the economy constitutes an equilibrium state was implicit in Harrod's criticism of the Hayekian view that the injection of credit would give rise to sectoral disproportions and automatically produce a trade depression. Harrod's article was one of the last contributions to a debate which engaged the young members of the New Fabian Research Bureau on the monetary conditions and policy for economic advance, Durbin and Gaitskell arguing along Hayekian lines and Harrod and Meade along Keynesian lines. In the background, there was the influence upon Harrod and Meade, and perhaps also upon Durbin (see Durbin 1985, p. 154), of Robertson's study of the conditions for the uniform growth of the economy in Banking Policy and the Price Level (Robertson 1926; see Costabile 1993). Harrod concluded that if in a progressing economy the amount of circulating money is increased in proportion with the increasing turnover, then the conditions for expansion would not be disturbed.

By the end of 1934, Harrod had thus formed the notions of the possibility of equilibrium growth, of the necessity of introducing a
destabilizing element to explain deviations from equilibrium, and of the distinction but interconnectedness of the problems of the determination of the level and the mode of change of output. During 1935 Harrod gathered together the analytical components of his model: the accelerator or Relation (whose connections with trade cycle theory he learned from a 1934 draft of Haberler’s 1934 League of Nations research on the theories of economic fluctuations), and the multiplier (a principle whose full implications he only appreciated after having read The General Theory in proof). By the end of the year, he had devised a mechanism capable of generating an equilibrium mode of advance, yet unstable enough to give rise to fluctuations. The Trade Cycle was published in September.

The book was widely discussed academically and also widely read (Gaitskell 1937: 472-3). Unsurprisingly, reviews dealt with the specific trade cycle mechanism, noting that although Harrod’s tools were not new (apart from the ‘Law of diminishing elasticity of demand’1; Hansen 1937: 520, 530, Haberler, 692), they were assembled in an original way (Samuelson 1939). Most frequently discussed were Harrod’s interpretation of the accelerator2, its interrelation with the multiplier (Samuelson 1939, Hansen 1937), and the lack of precision in the analysis of the ‘bottom’ and of the upturn (Hansen 1937: 525-527; Stafford 1937: 76 n). Several commentators criticised Harrod for lack of realism, for insufficient evidence (for instance Nogaro 1940: 111; Smithies 1937: 111), or for having neglected factors they thought instead to be more important. In particular, many reviewers argued that investment does not only depend on the accelerator but also on interest rates, and therefore criticised Harrod’s lack of confidence in monetary policy: (see Neisser 1937: 443; Smithies 1937: 111; Hawtrey 1937: 327-8; Hansen 1937: 529; Stafford 1937: 82.). Robertson (1937: 124), however, approved of Harrod’s stress on the real features of the cycle.

However, I am less interested in interpretation of Harrod’s analytical mechanism than in the contemporary understanding of his concept of dynamics. Although implicit in The Trade Cycle, the characteristic features of a dynamic theory as distinct from a static theory emerge only sporadically, while further aspects were explained in private correspondence. It is important to stress that also in subsequent versions of his theory Harrod failed to provide a cohesive notion of dynamics, presenting instead a series of different aspects of his notion on different occasions. Not surprisingly, this created confusion among his readers, who received only partial pictures of

Harrod’s theory and no common basis for discussion. It is therefore important to emphasise that these aspects represent different logical levels of Harrod’s dynamic approach. In fact, the problems he was facing ranged from the very possibility of a science of dynamics, to the correct mode of approach to economic movement, to the choice of the appropriate toolbox, and finally to the properties of the object of the study. Correspondingly, he first stressed that the possibility of economic change is rooted in the instability of equilibrium, which is therefore the fundamental ingredient of dynamics as opposed to the theories which see in time-lags the cause of the fluctuations. Secondly, he stressed that the correct approach to movement consists in examining first the conditions for the consistency of the growth rates of the different components, and therefore defined dynamics as referring to rates of growth, while statics refer to absolute levels of the magnitudes. Thirdly, he noted that while the instruments used by statics only enable us to consider discrete changes, dynamics must consider changes occurring with continuity. Finally, statics and dynamics being distinguished by the absence or presence of a movement whose cause is investment (and saving), a fourth characterisation of dynamics regards the presence of saving.

Unsurprisingly, since Harrod failed to systematize the different facets of his notion, his reviewers did not appreciate that his argument was based on a special concept of dynamics, and that the aspects which were discussed in the reviews were not perceived by the commentators as parts of an articulated concept with implications on different logical levels in the construction of Harrod’s theory. Nevertheless, these discussions are of interest due both to their insight, together with their avoidance of the post-war misinterpretations of Harrod’s theory, and because certain aspects of Harrod’s notion systematically failed to attract the reviewers’ attention.

Most commentators emphasized that in Harrod’s model the break in the boom is due to a change in the value of the ‘dynamic determinants’ (i.e., the three forces affecting the intensity of the operation of the multiplier and the accelerator: propensity to save and distribution of income, and capital-intensity, respectively) triggered by the boom conditions themselves, the consequences of which are cumulative and amplified by the instability of the process of advance. Two comments are necessary here. Firstly, the readers of The Trade Cycle were generally aware that the relevant coefficients were not assumed to be constant, and that their variation was actually at the heart of the explanation of the turning points. In modern terms, we could say
that Harrod's mechanism was understood to be intrinsically non-linear\(^3\). Although this feature persisted in successive versions of Harrod's theory, the early (correct) line of interpretation was lost, and both the neo-classical and neo-Keynesian lines of criticism of his theory charged him with having postulated fixed coefficients. In post-war years, Harrod rightly rebutted these attacks as unfounded, but apparently did not succeed in referring readers back to his early work, since the mistake kept being reproduced and became enshrined in most textbooks.

Secondly, the instability principle was understood as a result of the interaction of the multiplier and the accelerator, rather than as a premise of Harrod's reasoning. This only captures half of the truth, but the mistake is quite understandable: Harrod's mechanism, in fact, actually produces instability. However, this feature is not an accident due (if I am allowed to use terminology which does not belong to Harrod's 1936 exposition) to some particular configuration of the parameters or to the shape of the functions involved, but is the result of a preliminary decision concerning the kind of explanation one has to offer for the trade cycle. In fact, Harrod applied his principle of instability twice: the first time to the static equilibrium, in order to make (endogenous) change of the level of production possible, and the second time to the dynamic equilibrium, in order to make (endogenous) fluctuations possible as departures from steady growth.

If a study of the genesis of Harrod's dynamics clearly reveals this twofold application of the instability principle, its analytical rendition was quite involved and failed to convince his readers. In fact, Harrod refused on the one hand to accept the stability result of traditional theory while, on the other hand, he did not want to renounce the partial equilibrium approach to the determination of the equilibrium level of production. The latter desire implied that equilibrium could not be absolutely unstable, otherwise entrepreneurs would never be in equilibrium and their behaviour would be described as highly irrational. Harrod's solution thus lay in the middle: he postulated equilibrium as neutrally stable – that is, he assumed that in all circumstances the forces making for an increase in production exactly balanced those deterring further activity. This Blondinian approach to statics satisfied both of Harrod's requirements, leaving entrepreneurs in equilibrium but permitting any motion to occur which other forces would determine, precisely as a ball on a table would stay where it is put but would also freely move if subjected to some impulse. However, both this procedure and the 'change of method' it involved failed to satisfy the only reader who commented on it (Hawtrey 1937: 324–327). Moreover, a second problematic aspect of Harrod's approach regarded the mechanism postulated to keep the static forces in balance and to transmit to the individual entrepreneur the resultant of the (dynamic) forces determining the motion of the whole economic system. Harrod thought the balancing mechanism operated by means of price fluctuations, in turn permitted by changes in the velocity of circulation of money, but both the logic and the specific mechanism he postulated were so convoluted that Keynes needed to have them explained twice in private correspondence (Keynes 1973(II): 152, 173; Harrod's replies are at pp. 169, 176).

Readers of Harrod's book do not seem to have appreciated that the neutrality of static equilibrium was postulated in order to establish a connection between the static forces, inducing the individuals to produce a certain level of output, and the dynamic forces, determining the pace of change of production for the whole economic system. Although several reviewers summarised Harrod's discussion of the static equilibrium, none discussed the link between statics and dynamics. On the contrary, Joan Robinson found the chapter on static analysis 'fanciful' (Robinson 1936: 693), and Henderson (who read the first two chapters in proof), Keynes and Robertson needed to have the point explained to them in private correspondence.

This correspondence reveals that Harrod was deeply disappointed by this lack of understanding. This, perhaps, helps to explain why he decided not to raise the question again in the subsequent versions of his theory. This decision, however, led to unfortunate consequences. The missing link between individual and systemic forces prevented Harrod from solving the ambiguities between the two notions of equilibrium cohabiting in The Trade Cycle, one regarding the satisfaction of each entrepreneur and the second concerning the conditions of reproduction of the system's state of advance. Instead, the ambiguity was magnified in the 1939 Essay, and gave rise to prolonged debates on the real meaning and conditions of the dynamic equilibrium, which were often based on the exclusive adhesion of the commentators to one view or the other view. This aspect will be discussed in detail in Section 3.1. below.

The assumption of neutrality of the static equilibrium reflected one of Harrod's methodological standpoints which, although noticed by some commentators, was not understood in all its implications. Robertson (1937: 124) and Hansen (1937: 511, 523–4) correctly pointed out that by postulating that entrepreneurs are always in
equilibrium, Harrod was assuming a frictionless and rational world in order to avoid attributing the cycle to miscalculations and errors of judgement. However, this is not the whole story, as Harrod in fact also included time-lags among the 'frictions', and took strong issue against the time-lag theories of the cycle. Harrod, of course, neglected neither the existence nor the importance of lags. His point was while it is correct to refer to lags at a second stage accounting for the special features of individual explanations, the fundamental explanation of the cycle must not rely on lags and other kinds of frictions. In The Trade Cycle, this attack was directed mainly against the Robertsonian view that saving depends on the income of the preceding period (and, in fact, it raised the protest of the authors on Robertson's side in the saving-investment debate, in particular Haberler 1937, and Ellis 1938), but it soon encompassed the econometric approach, with special reference to Tinbergen⁴ and implicit reference to Frisch's definition of dynamics (Frisch 1933, 1936).

Harrod's position gave rise to two reactions. Some readers accused Harrod of implicitly introducing lags, or charged him with lack of realism for ignoring some essential lags (Haberler 1937: 691 n; Ellis 1938: 111; Smithies 1937: 110). Other commentators translated Harrod's multiplier-accelerator mechanism in terms of (linear) second order difference, or first order differential, equations, and pointed out that without lags the system could only give rise to exponential growth but no cycles⁵ (Samuelson 1939 and Tinbergen 1937, respectively). Both criticisms failed to understand the methodological nature of Harrod's objection to the introduction of lags, while the 'econometricians' were guilty of the additional mistake of having ignored the non-linear nature of Harrod's relations, which may itself be sufficient to generate limit cycles⁶ (Goodwin 1989: 157–8; Pugno 1992: 128). These criticisms were repeated over and over again in the following years with reference to Harrod's subsequent writings, and I return to them in Section 3.2 below.

Lastly, and symptomatic of Harrod's trouble in making his fellow economists understand the role of the dynamic equilibrium, is a difficulty which was carried over – although in a different form – into the next versions of his theory. With only a few exceptions, the readers of The Trade Cycle either ignored the equilibrium character of steady growth and treated it as a description of a phase of the cycle⁷ (Hansen 1937: 531), or dismissed the notion altogether as having 'no bearing on the trade cycle' (Robinson 1936: 692, and Keynes 1973(II): 171; this view is also implied by Tinbergen 1937 and Samuelson 1939).

Both reactions failed to understand the logic of Harrod's construction, based on the possibility of a dynamic equilibrium and the necessity of its breakdown due to the operation of the dynamic determinants. The view that the system could settle into a behaviour different from the stationary equilibrium must have been extraneous to most of Harrod's contemporaries. Keynes, hardly a careless reader, expressed the most genuine surprise on learning that Harrod's theory related to a mode of advance, and indeed doubted 'whether any reader who has not talked or corresponded with [him] could be aware that the whole of the last half of the book was intended to be in relation to a moving base of steady progress' (Keynes 1973(II): 170).

Yet Harrod's idea was not couched in ambiguous wording (Kregel 1980: 99), nor was it entirely new. Cassel had expressed a similar view in 1918 (available in England since 1923 and in the U.S. since 1924), and some of the debates within the New Fabian Research Bureau were based on the common assumption of a steady base of progress. Indeed, among the authors who recognized Harrod's point, we find Hugh Gaitskell (1937: 474–5), who took part in the NFRB discussions, and Dennis Robertson⁸. To Harrod's disappointment, the latter did not express his appreciation in print, but only later in private correspondence. Robertson's comment witnesses that times were not ripe for such an approach:

But I wish I had put in a sentence 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. as well as JMK) and deserved more explicit welcome in your own (Robertson to Harrod, 10 February, 1937).

Among the possible reasons for the lack of understanding of the role and features of Harrod's moving equilibrium, three seem to be particularly important. Firstly, Harrod's dynamic equilibrium concerned the reproduction of the system's conditions, and was thus more akin to classical and Marxian economics than to the mainstream approach, which instead regarded equilibrium as concerning the
satisfaction of economic agents. As noted, Harrod himself unsuccess-
fully attempted to reconcile these two notions, thereby concealing the
radical difference they imply and contributing to misdirecting the
reader’s attention away from the significance of his own approach.
Secondly, Harrod’s dynamic equilibrium described a kind of beha-
viour radically dissimilar from the stationary economy depicted in the
traditional view. This difference can be seen by analogy with the
difference between rest and inertial motion in mechanics. The asso-
ciation between ‘equilibrium’ and motion is not natural, and indeed in
physics the idea of inertial motion took several decades to be con-
ceived. In economics as well, the idea that other states besides
stationariness could be steady took a while to gain widespread ac-
ceptance, succeeding only after the war. By then, the economic experience
showed a period of steady advance unbroken by cycles, and on the
other hand the mathematical tools for the theoretical understanding
of steady states came into common use.
Thirdly, the attribute of instability Harrod attached to his notion of
moving equilibrium certainly did not convince those who conceived of
equilibrium as a ‘natural’ state of the system; that is, a condition
towards which the system would be eventually attracted. Harrod’s
twofold application of the instability principle rejected the idea that
such states exist, and therefore ran against the reassuring view that an
invisible hand is at work for, and will eventually succeed in, giving
remedy to disequilibrium states. Moreover, pre-war economists did
not command the mathematics which would have permitted them to
deal with unstable equilibria; this only came with non-linear dynamics,
which also brought with it the metaphoric repertoire enabling us to
conceive of unstable points and lines in the broader context of phase
spaces. But this is only a very recent story (see Section 4.1. below).
In any case, it is worth emphasizing that, besides the specificity
of Harrod’s approach, the idea of moving equilibrium as a self-
reproducing state did not meet with much favour among English-
speaking economists, in spite of having been proposed a number of
times. Besides Cassel’s work mentioned above, it is implicit in the
Marxian schemes of enlarged reproduction, and has actually been
worked out in these terms in the course of the debates on the break-
down of capitalism. But the main features of an expanding equili-
brium were also sketched by Marshall and taken up again by Pigou,
although only by way of comparison with stationary states and to
stress the limits to proportionate growth imposed by diminishing
returns of scarce factors. The implications of this notion must have
been perceived as unfruitful, because the exploration of the char-
acteristics of moving equilibria died out with Mill. The reasons for
this would require an unwarranted digression, but it is necessary to
stress that Harrod’s ideas on this subject ranged beyond those of his
contemporaries. Harrod himself realised this when, after the war, he
devoted some pages of his *Towards a Dynamic Economics* to the
history of ‘lapse of dynamics from favour’ (Harrod 1948: 15–18).
However, lack of understanding of his ideas coupled with the failure
to attract hoped-for consent left Harrod disappointed with the reaction
to *The Trade Cycle*. The strong competition of those years of theoretical
turmoil almost certainly contributed to divert the attention of fellow
economists from Harrod’s contribution. On the one hand, in the Key-
nesian field people were busy in applying the new ideas to old problems,
so that Harrod’s book was one of many, or in reducing the new ideas to
the old schemes (in particular Hicks, with his IS-LM interpretation of
the General Theory: see Young 1987, in particular pp. 55–57). On the
other hand, in the field of trade cycle theories the ‘econometricians’
(Kalecki and Tinbergen, in particular) were producing cyclical models
in terms of functional equations, which fostered the hope that mathe-
matical rigour could be coupled with economic theory and statistical
analysis. Although in the subsequent versions of his theory Harrod
stressed with increasing insistence the originality of his notion of
dynamics as against these competing approaches, most of the original
exegetic problems remained, albeit in a different form.

3 FROM CYCLES TO GROWTH

Stimulated by his readers’ reaction (Keynes’s in particular: see Kregel
1980 and Besomi 1995), and possibly by further readings (whose
precise influence is, however, very difficult to ascertain), in 1938
Harrod recast his trade cycle theory in a form suitable for publication
in the *Economic Journal*. The animated exchange with Keynes (repro-
duced in Keynes 1973(II): 321–350) caused Harrod to boil down the
detailed discussion of the cycle which characterised the central part of
his first draft into a single short section, and to concentrate instead on
the ‘fundamental equation’ determining the rate of growth of income
and on the policy implications.
Although the subsequent debates could only refer to the published
version, it is worth stressing some features of the draft which explicitly
reveal Harrod’s intention, which was in part obscured by the changes
he later introduced (for a discussion of the changes, see the editorial introduction and the Appendix to Harrod 1996, and Besomi 1996). On the one hand, the shift of emphasis from cycles to the growth rate formula helps us to understand how it could happen that readers turned Harrod’s dynamics into a theory of growth. On the other hand, this exercise will prove useful for assessing the points of consistency between Harrod’s original thoughts and his later reaction to the misinterpretation of his notion of dynamics which prevailed in the next four decades.

Firstly, the draft clearly indicates that Harrod persisted in attributing the fundamental cause of the cycle to the instability of equilibrium. But Keynes’s discussion of the conditions for instability brought Harrod to reveal that he now considered the cumulative character of the divergence between warranted and actual rates as a result (to be proved) rather than a premise to his reasoning. If this change of perspective removed the necessity of smuggling in the factor of instability by a ‘change of method’, it introduced a new problem because commentators soon realised that Harrod’s proof was far from rigorous, and debated at length the correctives to be introduced (see below, Section 3.2.).

Secondly, the specific cycle mechanism was based on the change in the factors determining, in each instant, the equilibrium growth rate. Any deviation from equilibrium is self-amplifying. But growth itself triggers changes in the proportion of income which is saved, so that the rate of growth which equates demand and supply for saving changes in turn. The warranted (equilibrium) growth rate thus propels the actual rate forward, and in turn chases it. But the actual rate cannot exceed the ‘ceiling’ given by the natural rate of growth (or, if the movement is descendent, the bottom given by the level of saving required for long-range capital outlay), while the warranted rate is not subject to this boundary. Sooner or later, the warranted rate will therefore overtake the actual rate, propel it in the opposite direction and thereafter be dragged along with it. This mechanism was still clearly outlined in the published version, but the non-linearity was much less evident, and indeed commentators ignored this feature, and eventually attributed the ‘knife edge’ character of equilibrium growth to the supposed rigidity of the parameters (see below, Sections 3.3. and 3.4.).

Thirdly, it is quite clear that Harrod thought of different, logically subsequent, stages of dynamics. This distinction was outlined in the 1938 Presidential Address to the British Association (Harrod 1938), but in the first draft of the ‘Essay’ (which was written shortly after the

Address) the demarcation line was sharply drawn between firstly ‘the strictest part of the dynamic theory’, whose purpose was to determine the conditions of harmonious growth in one instant and to study the stability of the equilibrium growth rate; then the second stage consisting in the study of the ‘succession of events’, and the final stage drawing the policy consequences. This distinction was blurred in the final version, and it took over forty years for it to be recognized (Kregel 1980; see below, Section 4.2.). Having failed to appreciate this aspect of Harrod’s procedure, his readers interpreted the warranted rate of growth as a description of the equilibrium line of growth rather than an instantaneous description of one of the possible states of the system. Moreover, they misinterpreted the significance of time-lags in Harrod’s conception. Lags, in fact, pertain to the second stage, while the fundamental cause of the cycle lies in the instability of the warranted state of the system. The outcome of this process was the translation of Harrod’s theory in terms of lagged functional equations, describing paths of growth. Harrod eventually rebelled against this interpretation, but his protest was ignored by growth theoreticians.

The account of the debates on different aspects of Harrod’s dynamics which follows, tries to elucidate, in the light of these premises, how the principal misinterpretations of Harrod’s dynamics arose, and the process of their consolidation giving rise to the myth describing Harrod as a growth theorist.

3.1 The ambiguity of the notion of warranted rate

In the Essay in Dynamic Theory, Harrod defined the warranted rate of growth as the growth rate of output equating, at a certain instant, saving and ex-ante investment (Harrod 1939: 19). That is, the increase of capital ‘required by technological and other conditions’ (18), or ‘which producers regard as ideally suited to the output which they are undertaking’ (19), or which does not give rise to undesired accumulation of stocks or to unused capacity (19). If growth kept this pace, it would

leave all parties satisfied that they have produced neither more nor less than the right amount. Or, to state the matter otherwise, it will put them into a frame of mind which will cause them to give such orders as will maintain the same rate of growth (Harrod 1939: 16)

(It should be noted that the last sentence did not appear in the 1938 draft.)
Readers of the Essay readily pointed out that this notion contains several ambiguities. Firstly, as Keynes remarked (Keynes 1973(II) 322), the use of the term 'ex-ante' does not conform to the Swedish usage, because it expresses the equilibrium amount of investment and has nothing to do with investment decisions. Harrod was aware of this, but on the other hand he declared he was treating the demand for saving as depending mainly on the rate of increase of income (Harrod 1939: 14), and at places he introduced behavioural assumptions. Hawtrey also stressed the difference between Harrod's and Ohlin's terminology, and following his own preferences he emphasized the aspect of the undesired accumulation of stocks in disequilibrium, suggesting that it would have been more appropriate to distinguish between active and passive investment, on the ground that the ordering of equipment constitutes demand, regardless of whether or not it will later be found to be redundant (Hawtrey 1939: 469–70).

The notion of equilibrium is even more troublesome. Schelling pointed out that the propositions (i) that the warranted rate leaves all parties satisfied, and (ii) that it induces them to keep on the same line of advance, are not equivalent (1947: 868). McCord Wright noted that in the Essay Harrod provided six qualifications of the notion of warranted rate: it leaves each entrepreneur satisfied as well as entrepreneurs as a whole; keeps them doing the same thing; equates ex ante investment and saving; only concerns the part of investment linked to consumption; and differs from the full employment rate (1949: 326). In addition to Schelling's criticism, Alexander pointed out that maintaining the same rate of advance is only one of the possible patterns of entrepreneurial behaviour, and thus alternative rules could be devised (Alexander 1950: 728).

This ambiguity reflects Harrod's inability to solve the problem he had already posed in The Trade Cycle, that of the consistency between an individual and a systemic notion of equilibrium. Dynamics concerns the rate of growth of income, the latter being an aggregate magnitude which results from the unplanned effects of the interaction of a multitude of entrepreneurs' and consumers' actions. Equilibrium thus concerns at the same time the reproduction of the system's status, and the individual decisions. In his 1936 book, Harrod explicitly dealt with the problem, and devised a mechanism transmitting to individuals the result of the dynamic forces (see Section 2 above). In the Essay, he simply postulated that entrepreneurs (as individuals and as a whole), if they are satisfied with the result of their previous actions, keep on the same pace of advance.13 Taken to task by Alexander's criticism, in 1951 Harrod admitted on the one hand that the notion of ex-ante investment in his context is misleading, and returned to the term – already used in The Trade Cycle – of justified investment (Harrod 1951: 270). He recognized on the other hand that his assumption concerning the behaviour of entrepreneurs in equilibrium is 'rather special and may be unjustified' (271). Finally, he attempted to tackle the problem of the relationship between systemic and individual determinants by resorting to the notion of representative entrepreneur, which was defined as

one whose orders in response to a given current out-turn are such that the sum of the excesses of all entrepreneurs in the economy who would order more in a precisely similar situation over what he would order is equal to the sum of the shortfalls of all those who would order less (272–3),

and by applying to this imaginary being a new behavioural postulate.

This solution raised two new waves of reaction. On the one hand, some readers examined whether or not the new assumption actually entailed instability (these discussions will not be examined here in detail; see Section 3.2, below for an overall survey of the premises of this approach). On the other hand, the notion of the representative entrepreneur itself gave rise to some criticism. Newman observed that it is not clear whether Harrod thought of it as representative 'of the entrepreneurial body, so that his behaviour is typical behaviour, or is it of the economy, so that he is, in effect, the macro-economy in miniature' (Newman 1954–55: 70). In addition to Newman's argument, Pugno observed that the concept of representative entrepreneur ignores the interdependence between sectors (and, I would add, between firms, Pugno 1992: 105). It is thus clear that the very problem this notion was called to solve was simply assumed away. Finally, Asimakopulos pointed out that the evocation of a mythical representative entrepreneur did not even solve Alexander's specific problem, as Harrod implicitly recognized in his last book on dynamic economics (Asimakopulos 1989: 353; reference is to Harrod 1973: 19–20. Joan Robinson was also dissatisfied with Harrod's reply: 1952: 47).

3.2 Formalising Harrod's dynamics: the first facet of the instability problem

After the war, Harrod's ideas were generally interpreted within the framework of 'formal dynamics'.14 This line of approach was started
by Tinbergen and Samuelson in relation to The Trade Cycle, but the
couching in terms of functional equations became the most frequent
rendition of the growth rate formula. Typically, Harrod's axioms, (i)
that the level of a community's income is the most important deter-
minant of its supply of saving, (ii) that the rate of increase of its
income is an important determinant of its demand for saving, and
(iii) that demand is equal to supply' (Harrod 1939: 14) were inter-
preted as providing a system of equations in which (i) the rate of
saving at a certain time is a function of the level of income at some
(other) time, (ii) the rate of investment is a function of the increase
of income during a period, and (iii) the values of saving and invest-
ment are equal at any time. The magnitudes are unknown functions of time,
whose determination is the scope of the exercise. Obviously, it is
possible to devise an infinite number of such systems, differing as to
the assumptions on the form of the functional relationships and on
the time-lags involved, while further complications may also be intro-
duced (the first examples of this kind of discussion were Schelling
1947 and Baumol 1948).

Regardless of the specific assumptions, two aspects concerning the
interpretation of Harrod's dynamics may be noted. Firstly, Harrod's
own use of his axioms was somewhat ambiguous, for while he declared
that the level and the rate of growth of output are the principal
determinants of saving and investment respectively, he also conceived
both s and c in ex-post terms. In fact, s represented the proportion of
income saved (Harrod himself doubted whether it would be possible
to discuss ex-ante saving), while the accelerator coefficient was called
on to play either the role of actual or of equilibrium capital coefficient,
the latter being defined as the amount of investment which (given the
amount of saving) would not give rise to undesired accumulation of
stocks. In either case, no process of decision was actually implied in
this conception. The treatment of Harrod's axioms in terms of
functions determining the amount of saving and investment unam-
biguously opted for the behavioural interpretation. The accelerator
was interpreted as providing the investment motive, while s was under-
stood as the propensity to save.

A second noteworthy aspect is that the rendition of Harrod's ideas
in terms of functional equations refers to the altogether different
dimension of dynamics proposed by Ragnar Frisch, who characterized it
as a theory explaining 'how one situation grows out of the foregoing'
and considering 'magnitudes of certain variables in different points of
time' (Frisch 1933: 171, and 1936: 100). I have compared Harrod's
and Frisch's approaches in 'Harrod and the Time-lag Theories of
the Cycle' (in this volume), pointing out how the former must have
appeared quite trivial in the eyes of the econometricians. However,
apart from a remark by Hicks (1950: 7), I have not been able to find
any other reflection on the difference between these two notions of
dynamics.

As for the specific contributions brought by the functional equa-
tions interpretation, it is necessary to distinguish between two
approaches giving rise to qualitatively different representations of
Harrod's intention, according to whether the functions expressing
Harrod's original axioms were assumed to be linear or non-linear.
In the present section I shall consider the first case only, postponing
the discussion of non-linear accounts until Section 4.1. below.

Two implications in particular determined the course of the debates
on Harrod's theory. As soon as commentators tried to couch Harrod's
analysis of the stability of the warranted rate in formal terms, it
became apparent that he had failed to make explicit and precise his
assumptions on the law connecting different states of the system in
disequilibrium conditions (Baumol 1948: 512; this remark was gener-
ally accepted by commentators). In fact, Harrod postulated that
entrepreneurs adapt to undesired accumulation of stocks or to a
diminished utilization of capacity by reducing their orders, thereby
increasing the divergence between the actual rate of growth and the
rate that would bring the system back to full utilization and to the
right volume of stocks (and, symmetrically, in the case of production
found to be deficient). Such lack of precision is unsurprising, con-
sidering that at that point Harrod was still discussing the conse-
quences of the abandonment of equilibrium within the first stage of
dynamics, not being willing to follow the cumulation of these effects
through time. In fact, Harrod was aware that the very fact of growth
(whether equilibrium or disequilibrium) involves changes in the coeffi-
cients, so that ceteris will not be paribus in different states of
development of the system. Equally, it is not surprising that in the
framework of functional equations such a state of affairs was found to
be far from satisfactory, because this approach presupposes that
the law of succession of events is specified beforehand. From this
perspective, the most obvious procedure was to fill in the missing
assumption in terms of entrepreneurial behaviour, trying to respect
Harrod's vague formulation, and to examine the stability of the system
so completed. A first stream of debate on the stability of Harrod's
system thus originated, giving rise as one would expect to different
conclusions according to the different assumptions on which the construction was based. The discussion was protracted for more than two decades and still leaves echoes in some recent writings, with interpreters accusing each other of not having correctly represented Harrod’s hypothesis (the literature is surveyed by Hahn and Matthews 1964, Miconi 1967,77, Pugno 1992: 109–119 and Ch. VI, and Pugno 1997. Among the participants in those debates not yet cited it is necessary to mention Rose 1959, Jorgenson 1960, Nevile 1960 and 1962, Ackley 1961, Nelson 1961).

The second implication of the couching of Harrod’s theory in terms of linear functional equations occurs here. The common feature shared by the participants in the debates on the stability of Harrod’s warranted rate (including the two other approaches to the problem, to be discussed below in Sections 3.3 and 3.4) is that his original point, that the instability principle was a necessary premise for the trade cycle and had thus to be present in the model beforehand, was altogether disregarded. In fact, the readers engaged in these discussions simply ignored the fact that Harrod aimed not only at providing a new notion of dynamics and the minimum set of conceptual tools for discussing the dynamic problem, but also at outlining the fundamentals of a theory of the cycle. The equations derived from Harrod’s assumptions were, in fact, regarded as giving rise to a line of growth, not to cycles: if a rate of growth is projected into the long period, the result can only be a system growing at a constant rate.18

Surely the fact that Harrod’s discussion of the cycle was compressed into one section only of the ‘Essay’ (1939: Section 15), and not much expanded in Towards a Dynamic Economics, helped the emergence of such interpretation, which was by no means peculiar to the econometricians.19 The characteristics of the linear functional equations approach, however, provided an additional reason for this restricted interpretation.

In terms of linear models, it is very cumbersome to discuss cycles and growth at the same time, while it is not possible to consider the reciprocal influence of trend and fluctuations. A peculiarity of linear functional equations is that their solutions are made up by superposing different components, whose behaviour results from a combination of sinusoidal curves and exponential terms. Pure cycles are a mathematical abstraction, equivalent to the frictionless pendulum. More generally, cycles are exponentially damped or amplified, and can be maintained only by exogenous shocks or by imposing exogenous constraints, respectively (but the latter case would introduce non-linearities, and will therefore be considered in Section 4.1. below). Pure geometric growth is one of the possible results of these systems, depending on the value of the parameters. Growth and cycles can only be obtained by adding an autonomous term to the original system (such a solution was proposed in 1950 by Hicks), but it could not be ignored that Harrod’s equation was meant to provide a base of advance without the need for an exogenous component.20 Therefore this approach was not practicable, and it would have excluded the peculiarity of Harrod’s notion that cycles and growth interact. For this reason, in the framework of linear equations, Harrod’s theory could only be interpreted as a theory of growth.

Harrod reacted by strongly affirming that his theory refers primarily to the trade cycle, that this cycle is caused by the instability of growth while in the econometricians’ approach it is caused by the lags, and that the deviation from equilibrium causes the warranted rate itself to change.21 In other words, Harrod maintained that cycle and growth are theoretically inseparable phenomena.22 However, Harrod’s readers cared little for his rebellion. Within a few years his name was firmly associated with growth theory, which was becoming one of the most carefully explored fields of research in economics, doubtless because of the conditions of apparently steady advance that real economies seemed to be experiencing, and due to the urgency to have a theory for developing economies. Growth was discussed almost exclusively in terms of functional equations, while the peculiarities of Harrod’s notion of dynamics were disregarded altogether. Meanwhile, trade cycle theory was settling out on a different path and, although pioneers on non-linear dynamics still failed to perceive Harrod’s grievance against lags and his distinction between stages of dynamics, they nevertheless recognised their debt to him. This, however, will be discussed below (Section 4.1.).

While these developments were underway, a new line of interpretation (and of attack against) Harrod’s approach was developed by neo-classical theorists. This will be the topic of the next section.

3.3 ‘The neoclassical resurgence’: the second facet of the instability problem

Harrod’s conclusion that equilibrium is unstable failed to satisfy some readers, not so much because this does not follow from the explicit assumptions as because the mechanism proposed by Harrod failed to include factors capable of adjusting to the requirements of
equilibrium. In particular, these commentators identified the cause of instability in the rigidity of the capital-output ratio and of the propensity to save, and suggested that complete flexibility would eliminate disequilibrium.

The first contribution along these lines is due to Fellner (1951: 116–122); his conclusion, however, was somewhat cautious, conceding that the adjustment in the capital/output ratio may bring about other maladjustments and some uncertainty, thus recognizing some force in Harrod’s argument (122). Fellner’s suggestion that the abundance of capital would decrease its price and thus cause substitution of factors along the production function was taken up by Pilvin (1953) two years later. Responding to Hamberg’s observation that recent growth models failed to distinguish between full capacity and full employment growth (Hamberg 1952: 446), Pilvin ‘generalized’ a simple Harrod-Domar model by introducing a production function in order to be able to take account of the growing capacity of capital and labour separately. In this set-up, the same overall rate of growth can be obtained by different combinations of increases of capital and labour along the same production function, while technical change (movement of the production function) may also be considered. In his reply, Harrod did not reject this approach in principle, but observed that several reasons suggest that the rate of interest cannot play the important part Pilvin was postulating for it. In order to avoid unemployment by warranting movement along the production function, one should suppose a continuous fall in the rate of interest (given the state of technology and the increase of population). This would be rather awkward in a mature economy in which the rate of interest is already low, and there would be practical difficulties for the Central Bank because of the trade cycle. Furthermore, ‘the idea of a foreseen steady fall in the rate of interest is a contradiction in terms’, because if the fall is anticipated it is immediately discounted and cannot occur in the future. Finally, Harrod doubted that the choice of the productive method is much affected by the rate of interest (Harrod 1953: 556–7).

However, this matter also was soon to escape from Harrod’s control. Yeager (1954) introduced two elements that were to characterize the neo-classical interpretation of Harrod’s (and Domar’s) model. The warranted rate equation was interpreted as providing a path of full employment both of capital and labour (p. 54), and the instability was characterized by the image of ‘a nervous tightrope walker’ (p. 59), the forerunner of Solow’s ‘knife-edge’. Yeager repeated the criticism of the rigidity of the parameters, and expressed his conviction that if one considers the monetary factor (adjustment in the rate of interest) then the instability is checked. Although without specific reference to Harrod, the same line of argument was resorted by Tobin (1955), and found a final systematization in Solow’s and Swan’s 1956 articles.

This interpretation is misleading in three major respects. Firstly, it fell in with the common error of mistaking conclusions drawn with respect to an instant as describing lines of development. This misinterpretation implies the second one: Harrod never assumed constant parameters, but explicitly considered the dependence of the saving and investment coefficients on the changing level of output. This is what makes his theory a trade cycle theory, not an analysis of a process of actual growth. Finally, the instability was seen as concerning the warranted rate with respect to the natural rate, not the actual rate. In Harrod’s view, the cumulative divergence between actual and warranted rate was meant to provide a cause for the trade cycle, while comparison with the natural rate provided a ceiling for actual growth and enabled us to draw out some long-run characterization of the cycles (e.g. prevalence of phases of inflation or of persistent unemployment). Neoclassical critics ignored the cycle altogether (a symptom of this attitude being the total disappearance of the actual rate from their writings), and only considered the effects of the divergence between warranted and natural rates. Moreover, they inserted in the model the factors enabling them to remedy the knife-edge, turning Harrod’s conception into a theory of permanent full employment of resources. The history of the theory of growth was reinterpreted accordingly as a linear progress from Harrod’s (and Domar’s) oversimplified model towards Solow’s solution, which consisted in eliminating the rigidities and the lack of a precise theory of disequilibrium behaviour which implied misleading conclusions as to the stability of the system (Solow 1970: 8–16, 1988: 307–308). The alchemical transformation was completed in the field of development theory, where the warranted rate formula was interpreted as providing the causes of growth, which were thus found to reside in thrift rather than in the multiplying effects of investment, thereby reversing not only Harrod’s premise but also his general perspective (see e.g. Graziani 1959: 47; McDonald 1960: 243; Shaw 1992: 611).

Harrod at first reacted in a conciliatory way. He specified that Part I of dynamics is only concerned with the rates of growth at a given point of time, so that changes in the coefficients are to be considered only when examining the succession of events through time (Harrod 1960: 279). However, he acknowledged that it is correct to maintain
that the acceleration coefficient depends on the rate of interest. In self-defence, he observed that this possibility was explicitly recognised in the first 'Essay' and in the 1948 book, although — he confessed — at the time he believed that entrepreneurs are little influenced by this factor when taking decisions (1960: 278). However, Harrod later reverted to the Keynesian position that there is no reason to suppose that natural forces automatically tend to establish full employment (1963: 407; 1973: 31, 44–45).

Other authors joined the debate, criticising the neo-classical line of attack. Some emphasised that Harrod had not assumed the marginal capital-output ratio to be fixed, and expounded his line of defence²⁵. It was remarked that the neoclassical criticism was not directed against Harrod's original notion of instability, but against the divergence between warranted and natural rates of growth (Hahn and Matthews 1964: 804–5, 810; Sen 1971: 23; the difference between the two problems, and Harrod's aims in the comparison between the three rates, were thoroughly discussed by Predetti 1961, p. 31 and passim.). Others stressed that the neoclassical approach skipped over the problem altogether (Hahn 1960: 206; Asimakopoulos and Weldon 1965: 64 and 71; Stiglitz and Uzawa 1969: 12–13; Jones 1975: 88–89), diverting the attention from the consequences of investment on income to the relationship between prices and quantities of factors of production (Nardozzi 1983: 151–6). It has also been argued that adjustment requires time, which may be very lengthy, so that for practical purposes Harrod's conclusion holds (Sato 1964: 387). Finally, some pointed out that Harrod was not concerned with the capital-labour ratio, but with the capital-output ratio (Johansen 1959: 157; Hicks 1963: 348–350).

This last point was ignored in the course of the debate, and the introduction of the production function into the framework of the Harrod-Domar model gave rise to another misunderstanding, concerning technical progress. In 1948, Harrod had provided a definition of neutrality of technical progress (alternative to that offered by Hicks) as the technical change which leaves the capital/output ratio unaltered at a constant rate of interest. This concept, in reality, was already implicit in The Trade Cycle, as Harrod himself pointed out to Hawtrey in correspondence in 1937²⁶, and was integrated in the concept of the moving equilibrium of the whole system which Harrod maintained through the various stages of development of his theory. The growth of output equilibrating saving and investment depends on the comparison between the amount saved (per unit of output) and amount of capital invested per unit increment of output; the latter quantity depends on the technique and on the rate of interest. In this context, an obvious notion of technical progress as neutral in respect of the mode of economic advance is a change which does not affect the pace of investment per unit increase of output. Hicks's definition of neutrality was developed in the different context of the determination of the distribution of income, and was therefore concerned with the ratio between capital and labour. Already in 1938 Joan Robinson, taken to task by Harrod's review of her Essays in the Theory of Employment (Robinson 1937; Harrod 1937a), attempted a comparison of the two notions. Obviously all three magnitudes involved had to be dragged in, and the natural solution was to use a production function to encompass the three variables. The conclusion was that the two notions are equivalent if the elasticity of substitution between capital and labour is equal to 1 (Robinson 1938: 141).

The problem was revived in the 1960s with several authors engaged in the task of comparing the two definitions along the same lines as Joan Robinson, obviously arriving at the same conclusion, namely that they are equivalent if, and only if, one is using a Cobb-Douglas production function (Uzawa 1961). The authors involved in these discussions generally recognized that Harrod's notion was worked out in connection with growth problems (see for instance Uzawa 1961: 117, and Kennedy 1962: 900), but from this promising premise nothing followed, and Harrod's problem was discussed in neoclassical terms. The only dissonant voice in this choir was John Hicks²⁷, who remarked that while his own notion referred to a physical conception of capital as an aggregate of objects, and was thus more suitable for the analysis in terms of factors of production and for static comparisons, Harrod's notion referred to a conception of capital as a fund, a concept which is more convenient for continuous growth models (Hicks 1963: 343–50. Hicks had also recognized this in correspondence with Harrod, 30 January 1963). But in spite of this observation, the significance and the context of Harrod's contribution were generally misunderstood once again.

3.4 The neo-Keynesian interpretation: Growth, and the distribution of income

During the same years that the neoclassical critics were busy ascribing the peculiarities of Harrod's result to the rigidity of the capital-output coefficient, and examining the consequences of the removal of this
assumption, an analogous operation was being carried out in the neoclassical field with respect to the other coefficient entering the warranted growth rate equation. In 1951, Kaldor attributed to Harrod the hypothesis that saving is a fixed proportion of output (Kaldor 1951: 843n), and diagnosed that Harrod’s problem – whether or not the state of moving equilibrium is achievable (843) – is rooted in this assumption and in the postulate that investment depends on the rate of growth of output instead of on its level (844). Kaldor returned to this interpretation a few years later, asserting that the instability of the dynamic equilibrium of growth presupposes that the saving coefficient is extraneously determined (Kaldor 1957: 594n). By considering changes in the rate of profit such as to bring about full employment, Kaldor ‘solved’ the Harrod problem, but at the price of a radical change of perspective which Harrod did not even consider (see Varri 1990: 34).

An analogous criticism was advanced by Joan Robinson, who argued that the propensity to save is influenced by the rate of profit, and concluded that ‘there must be a range of possible growth rates, corresponding to different rates of profit, not just one’ (Robinson 1970: 732; see also 1975. On Harrod and Robinson on the role of the propensity to save, see Asimakopoulos 1989). Pasinetti also blamed the rigidity of s in the Harrod and Domar models (1974: 96–97). This line of criticism was undoubtedly a corollary of the interest, shared by the various streams of the Cambridge approach to growth, in the distribution of income in relation to saving and accumulation.

In spite of the opposing spirit guiding the neo-classical and the neo-Keynesian understanding of Harrod’s growth equation, both their interpretations that the coefficients were assumed to be constant failed to grasp the essence of Harrod’s method. He therefore ‘mildly protested’, in his comment to Joan Robinson’s survey of growth theory 21 years after Harrod’s Towards a Dynamic Economics, that he had already ‘dealt very extensively with the mutual influence of growth rates and profit rates’ in The Trade Cycle (Harrod 1970: 737–8). The matter was eventually settled in Harrod’s last book on dynamics, where the fluctuations of the proportion of income saved and of the justified rate investment per unit increase of output were explicitly accounted for, and the changes of the warranted rate in the course of the cycle were discussed at length. Harrod’s rejection of the common interpretation, however, came too late. The textbooks on macroeconomics and on growth theory, which flourished in the late 1960s and early 1970s, had already crystallized the view that his ‘model’ assumes constant parameters and describes a line of long-run equilibrium growth.

Before describing the picture which dominated the textbooks and was handed down to generations of students, it is necessary to examine a second line of approach to the unresolved problem of the connection between subsequent states of the system.

3.5 Expectations: another connection between states

Some of Harrod’s readers were not satisfied with the ‘econometric’ solution to the difficulty of the lack of an explicit and precise link between contiguous states, consisting of introducing lags and/or behavioural assumptions relating saving and investment to the past results of previous accumulation and consumption decisions (see Section 3.2 above). They thought instead that considering investment decisions as depending on expectations would better represent the spirit of Harrod’s approach. Indeed, in Harrod’s original formulation we find the assertion that, in equilibrium, entrepreneurs find their expectations as to the increase of output and consumption to be satisfied, and this consideration enters the notion of ‘justified’ investment.

In the negative, this perspective was raised by Hahn, who linked the notion of equilibrium with the realization of expectations and suggested as a corollary that dynamic non-equilibrium models are inadequate to describe moving equilibria, because in disequilibrium expectations are disappointed, and behaviour is thus likely to be modified in order to meet the new state of things. From this, he observed that Harrod’s entrepreneurs are quite perverse, for they never learn from experience (Hahn 1952: 815). In contrast, first Bodenhorn (1956) then Neville (1960), Encarnación (1965), and Miconi (1967), and finally some of the systematizers of growth theory (see especially Sen 1971 and Jones 1975), completed the model by introducing an explicit expectation function.

This solution, which reflects the renewed interest in expectations of the late 1950s and early 1960s, led to conclusions not dissimilar to those reached by the functional equations approach, in the sense that the stability of the system depends on the specific assumptions as to the expectation function, and also suffers from the same limitations as to the understanding of Harrod’s dynamics. In fact, the interpretation of Harrod’s axioms in terms of determinants of saving and investment is strictly analogous in the two approaches, and so is the formal treatment. Ott (1958: 194) even affirmed that considering the
accelerator as regarding the relation of investment to the expected, rather than the realized, increase of income, is equivalent to reversing the sign of one of the time lags.

Moreover, although these models may look more realistic or more ‘Keynesian’, they overemphasize the role of expectations in Harrod’s thought. In *The Trade Cycle*, investment decisions actually referred to the expected increase of consumption. But, as I have stressed in Section 3.2, the decision process – and expectations with it – disappeared from the *analytical working* of Harrod’s mechanism (Besomi 1995: 323). On the other hand, Harrod himself specified that from the viewpoint of the identification of the fundamental causes of movement, expectations belong to the same domain as time lags. In explaining why related investment to the increase of total output in the same period instead of the prospective increases in subsequent periods, Harrod emphasized that he ‘deliberately neglected’ this point in the first part of the argument, ‘along with all questions of lags’ (Harrod 1939: 20). Expectations and lags, therefore, do not pertain to ‘the strictest part of the dynamic theory’ (which, in Harrod’s eyes, includes the analysis of the stability of the instantaneous warranted rate), and can only be brought back in at the stage of examining ‘the succession of events’.

### 3.6 Consolidating the misunderstanding: the textbooks

To conclude this section, dedicated as it is to the birth and development of the mainstream interpretation of Harrod’s dynamics, it is appropriate to examine how it was crystallized in the textbooks on which several generations of students were brought up.

Since the early 1960s, after a dozen years of debate on growth theory, the need was felt to tidy up hundreds of contributions scattered through several books and learned journals (the most active of which were the *Economic Journal, Quarterly Journal of Economics, Review of Economic Studies, American Economic Review*). Some treatises on ‘formal dynamics’ appeared in the early 1950s (Baumol 1951 devoted a chapter to Harrod; Allen 1956: 64–69, expounded the ‘Harrod-Domar model’). Soon after that, some treatises on growth theory were written dedicating some space to different models (Hamburg 1956, Duesenberry 1958). The first textbooks on macroeconomics devoting a chapter on growth appeared in the early 1960s (Ackley 1961, Brooman 1962), while since the end of the 1960s some volumes of ‘readings’ and the first textbooks on growth were made available (Stiglitz and Uzawa 1969, Burmeister and Dobell 1970, Sen 1971, Harberg 1971, Wan 1971, Neher 1971, Kregel 1972, Jones 1975, Dixit 1976, Hacche 1979). However, the systematization which eventually channelled most of the subsequent contributions was Hahn and Matthews’s celebrated 1964 survey on the theory of economic growth. It is thus convenient to start from there.

Hahn and Matthews found the Harrod-Domar model a convenient point of departure for expounding the other models of growth. They carefully stated that this was only meant ‘to help make clear the relation between the various models’, without implying that other authors actually took the Harrod-Domar model as a starting point (784). They represent the model as based on the assumptions that (i) a constant proportion of income is devoted to saving; (ii) the production function is characterized by fixed coefficients of production; (iii) the labour supply grows at an exogenous and constant rate. Hahn and Matthews provided a formalization in terms of differential equations, with the introduction of a term representing past mistakes on the part of entrepreneurs, on the ground of which the new investment decisions are taken. Two versions of the ‘knife-edge problem’ are discussed: the divergence between natural and warranted rate interpreted as a problem of *over-determination* of the system, the solution of which must consist in relaxing one of the three assumptions listed above, and the *instability problem* proper consisting in the divergence between actual and warranted rate. The debate on the latter problem is surveyed, but there is no hint as to the relationship between instability and the cycle. Neo-classical and neo-Keynesian approaches are presented as the relaxation of assumptions (ii) and (i), respectively.

This interpretation represents fairly the contemporaneous understanding of Harrod’s model which, Harrod’s protest notwithstanding, changed little in the next decade. Harrod’s contribution was associated with Domar’s as providing a model of long-term geometric growth. This sentence alone condenses five misinterpretations. The first three, already discussed above, concern the failure to recognize that Harrod’s dynamics referred to an instant, that his discussion aimed at providing an explanation of the cycle and that the coefficients change in the course of the cycle. Fourthly, in spite of the similarity in their formulas, Harrod’s and Domar’s theories had different aims and referred to different variables; of course several readers (including Hahn and Matthews) recognized this, but eventually some careless textbooks failed to distinguish between them and only presented ‘their’ model.
The most important problem is the last. The main failure of the textbook rendition was in mistaking Harrod’s contribution for a ‘model’, as providing a system of equation explaining growth, while Harrod thought his task to be more fundamental, aiming at devising ‘tools for thought’ and at offering a foundation for economic dynamics. Harrod insisted on this, since his first contributions in the subject: in The Trade Cycle he stressed that he was only presenting ‘the outline of a theory’, pointing out however that the new procedure he was suggesting, if successful, would open the way ‘for a great and important extension of economic theory’ (Harrod 1936: vii and ix; see also 1937: 86, and 1938: 405). In the Essay, he invoked ‘a new method of approach – indeed, a mental revolution’ (1939: 15). The first chapter of Towards a Dynamic Economics was dedicated to ‘the need for a dynamic economics’, while the title of the book itself indicated that Harrod aimed at posing the foundations of the discipline. In his collection of Economic Essays, Harrod added a ‘Supplement on Dynamic Theory’ where he specified that his ‘fundamental equation’ differs from the so-called ‘complete models’, because the latter ‘require special postulates and assumptions in regard to lags and coefficients, which can only be accepted subject to statistical verification’, while his own approach is based ‘on assumptions of the utmost simplicity and generality’ and thus has far superior authority, although it ‘makes no pretension to giving a complete explanation of the cycle’ (1952: 286. See also 1955: 360–1; 1957: 6; 1960: 277; 1969: 193. Kaldor seems to be the only commentator to have taken notice of this point: 1954: 65). Harrod soon returned to the subject, qualifying his dynamics as a ‘tool of thought’. He stressed that his analysis ‘only claimed to be a preliminary attempt to lay foundations’ (1953: 553 and 555; 1973: 2), but reaffirmed that it requires ‘far-reaching changes in some of our traditional habits of thinking’ (1959: 151; see also 1948: 80). But in spite of these repeated claims, Harrod’s notion of dynamics was disregarded altogether by textbooks, including those on dynamics, precisely as it was ignored in learned debates.

The story of the recognition of Harrod’s contribution by textbooks concludes even more sadly, for since the 1980s Harrod’s name rarely appears in the author indexes of books on macroeconomics. Surveys of trade cycle theory hardly mention his contributions, while endogenous growth theorists seem to remember Harrod only in passing, for his ‘production function with little substitutability among the inputs’ used ‘to argue that the capitalistic system is inherently unstable’ (Barro and Sala-i-Martin 1995: 10).

Although the mainstream account of Harrod’s dynamics is thus far from being fair, even in the years of the supreme reign of the growth models some interpreters dared to go back to Harrod’s original texts instead of referring to second-hand quotations, and managed to single out some interesting features of his ideas. Moreover, from the early 1950s a new approach to trade cycle theory was emerging, emphasizing the importance of non-linearity. Finally, the reprinting of Harrod’s 1939 article in some collections on growth theory, the publication of Harrod’s last book on dynamics with its increased emphasis on the changing values of the magnitudes and the fluctuations of the warranted rate, and especially the publication of the correspondence between Harrod and Keynes on The Trade Cycle, the Essay and the 1938 essay on ‘Scope and Method of Economics’ stimulated some commentators to reflect on the methodological issues behind Harrod’s approach, eventually leading to a much more careful interpretation of his contributions, which in recent years are being examined in their multiple facets.

4 BACK TO HARROD

In the course of the debates on the stability conditions for the warranted rate of growth, some participants occasionally threw out some interesting remarks on the nature of Harrodian concepts and method. These were hardly systematic, and were rarely taken up by other commentators. However, it is worth listing some of these insights here.

Strange as it may seem, the point that the notion of warranted growth rate was devised to point out that steady growth is possible without exogenous aids took some years to be recognized, notwithstanding Harrod’s explicit qualification that his study aimed at analysing ‘a possible equilibrium of advance’ (Harrod 1951: 271 – Harrod’s emphasis). Smithies (1957: 4) appreciated the implications of this concept for dynamics: in doing so, Harrod ‘has rescued economics from its obsession with static conceptions, particularly the diminishing productivity of capital, which have so frequently led to the conclusion that the ultimate fate of capitalism is breakdown or the stationary state’. In addition, Joan Robinson remarked that the existence of conditions for this possibility to occur ‘contradicts the view that there is, in general, an automatic tendency for capitalism to keep going’ (1952: 42–43).
Since his very first writings on dynamics, Harrod insisted that a new method of analysis was required to account for continuous change, while statics can only account for discrete variations in the ‘fundamental conditions’. The static supply and demand curves determine the equilibrium point, given the relevant circumstances affecting costs, preferences, etc. One-off changes in these can be accounted for: a new equilibrium position is determined, but what happens in between is ignored. In dynamics, the fundamental conditions (proportion of income saved and new capital required for an additional unit increase of output) are subject to continuous change, so that a discrete method is not suitable (Harrod 1934c: 478; 1939: 15; 1948: 7; 1957: 193; 1973: 3; see also the discussion with Keynes, in Keynes 1973 (II): 163, 166). Harrod’s choice of words, however, was somewhat unfortunate, because he used ‘steady’ with the twofold meaning of ‘constant’ and ‘continuous’, the latter sense being generally ignored by earlier commentators. The importance of this qualification was noticed with reference to the classification of technical progress (La Tourette 1964: 213 stressed that Harrod’s definition of neutrality implies a continuous change of technical knowledge, and Hicks pointed out that Harrod chose the notion of capital and the definition of neutrality most appropriate to his continuous growth model, as opposed to Hicks’s own notion useful for static comparison: 1966: 343–5), but the connection with the method of dynamics was hardly seen — it was only mentioned by Hansen (1949: 497) and in passing by Joan Robinson (1949: 68), before being noticed again in the most recent years (Montesano 1972: 210n. Pugno 1992, however, found the distinction between continuous and per saltum change misleading: 166).

Harrod’s notion of equilibrium was reconsidered again in the second half of the 1960s, shifting the emphasis from the behavioural considerations of individual entrepreneurs (where it was left as a result of Alexander’s criticism: see above, Section 3.1; for a further discussion see Pugno 1992, pp. 133–138) to the systemic notions of equilibrium as consistency between the decisions of savers and investors (Cozzi 1966: 17) and between the various growth rates (Burmeister and Dobell 1970: 39), and of equilibrium as reproduction of the state of the system (Shackle 1967: 249, 251; Goodwin 1970: 118, in explicit analogy with the Marxian problem of realisation; Medio 1979: 13). As was the case with the previous discussion, however, the problem of the consistency between the individual and the systemic conditions for equilibrium was not raised\textsuperscript{36}. It should, however, be noted that Harrod himself did not return to the subject.

These observations on Harrod’s method and notions were quite sporadic, and did not affect the mainstream interpretation of his dynamics. Harrod’s writings, however, exerted a powerful impression on the pioneers of non-linear trade cycle theory. It is thus necessary to examine how some of his ideas were rescued and rearranged in a different set-up.

### 4.1 Non-linearity, instability and the cycle

The publication of *Towards a Dynamic Economics* catalysed Hicks’s theory of the cycle, whose fundamentals are notably outlined in his review of Harrod’s book. Although Hicks missed some important features of Harrod’s approach, such as the variation of the parameters over the course of the cycle and the significance of Harrod’s notion of dynamics\textsuperscript{37}, he fully appreciated the importance of the instability principle in connection with the trade cycle (1949: 108, 113), and put it at the heart of his own theory of the cycle in the context of functional equations (1950: 9). Hicks, in fact, reflected on the kind of solutions a linear system may give rise to: (i) damped fluctuations, kept alive by exogenous shocks; he argued that this solution is not satisfactory, because it attributes the burden of explanation to the shocks. (ii) constant fluctuations, which can only result from an extraordinarily lucky combination of values of parameters; this solution is not satisfactory either. (iii) unstable equilibrium, with fluctuations limited by floors and ceilings; Hicks affirms that this solution is the one fitting the facts (89–92), but in reality the implication is that this is the right kind of explanation. The premise of Hicks’s reasoning was thus analogous to Harrod’s, although the treatment proceeded along a different line.

In his comment on Hicks, Goodwin pointed out that ceilings and floors are non-linearities imposed upon the system (Goodwin 1950: 318), and attempted to solve the problem of the endogenous causes of the persistence of the cycle by introducing a different kind of non-linearities in an accelerator-multiplier model, explicitly recognizing Harrod’s influence at this point (Goodwin 1951: 2n). As a matter of fact, it is doubtful that Goodwin’s model was directly inspired by Harrod, since his mechanism is quite different. It is interesting to notice, however, that the instability of equilibrium is still at the heart of the explanation, since the nodes or foci at the centre of the limit cycle in the phase space must be unstable, and thus represent equilibrium states (or paths) surrounded by ‘centrifugal forces’, precisely as
Harrod thought his warranted growth rate to be. Harrod’s original epistemic problem of finding the appropriate kind of explanation of the cycle, when interpreted in the framework of ‘formal dynamics’ produced a similar solution; that is, that the condition for the persistence of the cycle without recourse to exogenous causes must lie in the instability of equilibrium.

Harrod’s theory may actually have inspired an important reflection which was developed in the bosom of non-linear models; that is, the interrelation between cycles and growth (Goodwin 1955: 204). As we have seen (above, Section 3.2), linear models can at most superimpose cycles and growth, but not express the interaction between them as inextricable processes. This was instead one of the problems Goodwin was facing, and it is precisely the approach to which his former teacher was trying to attract the attention.

We may thus agree with Goodwin (and Samuelson before him), that Harrod’s intuition was far superior to his mastery of the analytical tools he devised for solving the important problems he was posing. Of course one must consider that non-linear dynamics still belongs to the domain defined by Frisch, so that Harrod would not have approved of the role of time-lags in the analytical set-up. On the other hand, it must be pointed out that this approach encompassed Harrod’s principal requests, that the instability of the growth process is the premise and the cause of the cycle, which in turn affects the growth process. The trade cycle research brought about by Goodwin’s original investigation has thus provided some of the instruments for understanding Harrod’s aim, by showing that some of the problems he confusedly attempted to tackle can indeed be treated in more rigorous terms, and bring a solution similar to that imagined by Harrod. Thus, time was ripe for someone to go back to the whole body of Harrod’s writings on dynamics, and try to make sense of the mechanism in the background of Harrod’s mind. This inquiry has indeed been attempted, and it will be discussed in the next section among the recent contributions on the subject.

4.2 The recent literature on Harrod’s dynamics

As textbook interest in Harrod and macroeconomics fades, so, concomitantly, some specific and thorough studies have been carried out. These concern methodological issues (Kregel), the evolution of Harrod’s ideas on growth and the long period in the context of post-Keynesian thought (Asimakopulos), the intellectual and sociological context of Harrod’s early contributions (Young) and the analytical aspects of Harrod’s cyclical growth mechanism (Pugno). Here, the first and last of these sets are of particular interest, while Asimakopulos’s and Young’s contributions, in spite of their intrinsic usefulness, will be referred to only in passing.

Harrod’s specification that his analysis only concerns a point in time notwithstanding, Kregel seems to have been the first reader to discuss this characteristic (which is palpably at odds with the Hicksian and the prevailing Frischian notion of dynamics) in depth, and to emphasize it and its relationship with Harrod’s rejection of time-lags (Hansen, however, had already noticed the difference with Frisch’s definition in 1952: 76–77). Previously, commentators had dismissed the point, either implicitly considering the idea of instantaneous dynamics as quite bizarre (see e.g. Hicks 1949: 106), or arguing that time-lags are hidden everywhere in Harrod’s argument (see in particular the otherwise insightful Shackle 1967: 250–261). Since 1971 Kregel, contrarily but rightly, traced the origin of Harrod’s notion back to a 1934 statement on the appropriate method for examining the consistency between the pace of advance of different magnitudes (Kregel 1971: 104–5); observed that the renunciation of time-lags was meant to give priority to the dynamic method (1971: 105; 1972: 37), and noticed that Harrod’s 1973 book aimed at defining a set of basic axioms and that the distinction between statics and dynamics does not lie in the contrast between short- and long-run periods (1973: 905). After the publication of Harrod’s correspondence with Keynes on methodological matters, Kregel returned to these themes to point out that Harrod’s dynamics is characterized by a division in stages, and that the study of the consistency between growth rates within a time-section is logically prior to the study of the succession of events (1980: 114–117. See also Dow 1985: 129–30). These considerations enabled Kregel to recognize the continuity between Harrod’s 1973 discussion of the different values of the warranted rate with the original confinement of this discussion to Part II of dynamics, and to conclude that the problem raised by Joan Robinson: see Section 3.4. above – of the uniqueness of the warranted rate simply did not exist (Kregel 1980: 118).

This enlightening reading is in some measure obscured by Kregel’s excessive emphasis on growth with respect to cycles. This found expression in the interpretation of the concept of equilibrium in The Trade Cycle as a purely ‘notional’ or reference concept, and conversely in Kregel’s opinion that in the 1939 article equilibrium was meant to provide the description of an actual state of affairs while deviations
from it had a ‘notional’ character (1980: 104–5; this view, however, is challenged – rightly in my opinion – by Pugno 1992: 94n, 128, 151–2 and passim). A few years later, in his otherwise acute characterisation of the evolution of Harrod’s view of growth in comparison with Domar’s and Robinson’s, Asimakopulos further elaborated on Harrod’s distinction between ‘normal’ and ‘special’ warranted rates, and accepted Kregel’s remarks on equilibrium in the light of the traditional bias in favour of a reading of Harrod’s dynamics as primarily concerned with the notion of an equilibrium line of growth, thus failing to perceive the implications of the distinction between Part I and Part II of dynamics (Asimakopulos 1985: 623, 633; 1986: 279–81; 1991: 142–3, and 156n).

Although both Kregel and Asimakopulos recognized that Harrod had some interest in the trade cycle, and that the instability principle was meant to play a part in the explanation, they failed to discuss it in detail. The subject is carefully studied instead by Pugno, in a book which in a way complements Kregel’s writings, being concerned more with the working of the mechanisms devised by Harrod than with the method and the notion of dynamics itself. Pugno’s emphasis points exactly in the opposite direction to Kregel’s, Asimakopulos’s and the mainstream interpretation, recognizing that Harrod had explicit interest in the factors affecting long-run growth but pointing out that Harrod had elaborated a theory of the boom and the slump, working through the instability principle, and of the turning points, although he did not provide a complete model of business fluctuations (1992: 283–4).

The background of Pugno’s interpretation is non-linear dynamics. It is therefore not surprising to find in his book some contempt for Harrod’s methodological discussion of the different stages of analysis and of the ‘time-lag theories of the cycle’, which Pugno considered as a veil hiding the lack of specifications (284–5), and the assertion of the importance of lags in determining the behaviour of the system (193; on this point see also Medio 1979: 29). However, Pugno’s exposition of the place of the instability principle, and of the factors limiting instability, in the trade cycle mechanism answers a felt need and therefore deserves further consideration. Harrod’s conception of the movement of the economic system is essentially based on the notion of ‘cyclical instability’, with instability playing the role of magnifying the (systematic or accidental) deviations from equilibrium, while some factors are at work to limit this process of amplification. The instability principle is therefore the logically primary component of Harrod’s dynamics, which is intrinsically non-linear because of the mechanism contrasting the instability after it started off the cumulative process (Pugno 1992: 121). In The Trade Cycle, the instability is offset by the changes in the dynamic determinants triggered by the fluctuations themselves (85–92), while in the later versions some limits are endogenous (in particular the changes in the propensity to save, which affects the ‘special’ warranted rates: 123–4), and some are exogenous (ceilings and floors: 122–3).

Pugno also came close to exposing the epistemic role of instability in Harrod’s trade cycle theory. In fact, he correctly pointed out that in the 1930s the problem of the theory of business fluctuations was that of escaping the dominating role of equilibrium. His main interest, however, is in the analytical working of Harrod’s mechanisms, rather than in the genesis of Harrod’s approach; Pugno therefore did not pay attention to the fact that Harrod introduced instability as a premise of his trade cycle theory in an explicit attempt to escape this problem, before becoming familiar with the General Theory’s emphasis on the possibility of a multiplicity of equilibrium levels of income (not necessarily full employment). Pugno stresses instead that Harrod’s solution – of which the instability principle is an integral part – was an elaboration of Keynes’s approach (270–1). It is important, however, to remark that Harrod came to the conclusion that some destabilizing factor is essential to introduce the possibility of endogenous movement before becoming familiar with the ideas of the General Theory. In fact, the epistemic premise of the instability principle originates from the criticism of Pigou’s theory of the cycle, which was based on the alternation of optimism and pessimism on the part of entrepreneurs. Harrod was not satisfied with this approach, which he considered as equivalent to the introduction of a deus ex machina. One of the solutions Harrod proposed in the first place was based on the destabilizing influence of increasing returns associated with imperfect competition conditions (Harrod 1934a: 465–470), but he thought the approach of the Treatise on Money, based on the self-amplifying character of the deviations between market and natural prices of credit, to be equally valuable (he expressed this view in correspondence with Haberler, also in 1934. The passage is cited and further commented on in ‘Harrod and the “Time-lag Theories of the Cycle”’, § 2, in this volume).

The importance of Harrod’s reference to the Treatise has been overlooked by interpreters. His theory has often been presented as the dynamic version of Keynes’s General Theory. Indeed, several of
Harrod’s concepts can be seen as the dynamic counterpart of Keynes’s ideas, as Harrod himself pointed out. In the literature, this ‘dynamization’ was interpreted either as the ‘closure’ of Keynes’s system by means of an investment function, or as an extension of Keynes’s analysis to the long period, either as long-run equilibrium conditions or as the counterpart of the stagnation thesis (this classification is due to Rostow 1953 [1960]: 87–88). The first and last of these interpretations contain a grain of truth, although the interpretation of the accelerator as an investment function is open to the reservations expressed in Section 3.2. above, while Harrod expressed the ‘stagnation thesis’ in terms of tendencies expressing themselves in the relative length of the upward and downward phases of the cycle, rather than as actual processes (Harrod 1973: 103). These interpretations are consistent with the mainstream reading of Harrod’s (and Keynes’s) ideas. It is however necessary to point out that Harrod himself stressed that he found the Treatise on Money, with its idea that the divergence between saving and investment gives rise to the forces making for expansion and contraction, a good basis for trade cycle theory (Harrod 1963: 412–3). The mechanism he devised in the Essay was explicitly interpreted in Treatise terms (Harrod 1939: 19), while one of Harrod’s alternative formulations of the mechanism devised in The Trade Cycle clearly reflects Keynes’s 1930 approach (1936: 160–167). Harrod’s explicit reference to the conceptual scheme of the Treatise passed practically unnoticed by commentators, with the exception of Young, who pointed out how, on several occasions, Harrod found the Treatise definition of saving more useful to trade cycle theorizing.40 This matter, however, would deserve more careful study, in the context of the debates on saving and investment which raged in the 1930s, with the terminological and conceptual wrangles which characterized them.41

5 CONCLUSION

In the foregoing sections I have attempted to elucidate the nature and the origin of the most common misunderstandings of Harrod’s dynamics. At first, commentators failed to appreciate that the theory advanced in The Trade Cycle presupposed a base of advance. Later the tide changed, and Harrod’s approach was mistaken for a theory of growth paths. Only in recent years have some of his dynamic themes been taken up again, and there have been attempts to enquire as to the context in which they have been originally developed. This, however, was too late to affect the representation of Harrod in the view of the economists brought up associating his name with the pioneering stages of the theory of growth. The upshot of this survey is that not only did the ‘mental revolution’ Harrod advocated never take off, but his call for a ‘new way of thinking’ was not even understood by his contemporaries.

Harrod’s dynamics thus met with an unfair fate: his approach to the discipline was ignored, while he gained recognition for a theory whose premises he hardly accepted. What I have recounted here is thus the story of Harrod’s failure to win consent. In spite of this lack of success, I think there are still some good reasons for reading (or re-reading in a new perspective) the writings of this innovative economist. On the one hand, the interpretations produced during the recent revival of interest for Harrod’s thought show that his ideas can still be fertile as to their economic content. The implications are plain, and there is no need to discuss them further.

On the other hand, the story of a failure may provide an interesting challenge for historians of thought, because it raises the problem of the conditions for the understanding and acceptance of a theoretical system with much more urgency than the story of a complete success. In this respect, Harrod’s dynamics provides material for a case study, for it is possible to identify at least three groups of conditions it failed to meet. In the first place, Harrod’s analytical apparatus was somewhat confused, and some of the problems he raised were tackled quite unsatisfactorily. For instance, the notion of equilibrium was open to several different interpretations, the assumptions as to the links between states were not stated with precision and the analysis of the succession of events was only roughly outlined. Given these premises, it is not surprising that the debates on the details overshadowed the picture of the whole, and that readers eventually felt justified in abstracting some features they found interesting and using them in a different setting. In the second place, historical contingencies helped shift the emphasis away from Harrod’s original intent: at the time of writing The Trade Cycle, the notion of a self-sustained advance was somewhat premature in a world experiencing wild swings, while on the contrary after the war the interest in trade cycle theory was fading away in a world apparently enjoying cumulative progress. Finally, the conceptual furniture of Harrod’s contemporaries was not ready to receive some of Harrod’s contributions, particularly the intrinsic non-linearity of his apparatus and (in the early years) the idea itself of endogenous and persistent
distance from stationariness or (later, but for some readers only, given the Keynesian breakthrough) from dynamic equilibrium.

Reversing these considerations into the positive, it would seem that the success of a theory does not depend only on its clarity, logical consistency, and heuristic value (let alone empirical accuracy), but also on its capacity to be integrated with the accepted currents of thought. To raise interest, a theory must contain some element of novelty; but to be understood and integrated, the break with the tradition must not be too radical. Harrod was aware of these requirements and tried to adjust the emphasis given to one or the other element in accordance with the reaction of his readers. In the early years he was quite prudent, and preferred to stress the methodological continuity between statics (‘traditional economics’) and dynamics. His readers perceived this point (Stafford 1937: 69–70; Gaitskell 1937: 474–5; Nogaro 1940), but failed to appreciate the novelty of his approach. In the light of the reactions to his book, Harrod stressed with insistence the necessity of a mental revolution and polemicized with the alternative approaches to dynamics, without however succeeding in imposing his own view on the attention of his fellows. It is interesting to notice that as a consequence of this shift of emphasis, the fact that the method of Harrod’s dynamics was devised as the counterpart of the static method was generally not noticed by commentators (a notable exception is Kregel 1980: 116. See Besomi 1997a).

The story of Harrod’s dynamics is thus much more complicated (and interesting) than the straightforward sequential reading of his and his critics’ writings would suggest: Harrod deliberately adapted the emphasis and the expository strategy in the light of the reactions it stimulated. However, some ingredient for success was still missing, surely for the reason that Harrod raised many more problems than he could satisfactorily solve. But – if I am allowed to conclude with a second-hand quotation – ‘of what investigator worth his salt is this not true?’

Notes

1. Harrod’s belief that as people become more affluent, they pay less attention to price changes, gave rise to a discussion on the conditions of validity and the very existence of this law: Bretherton 1937, Gaitskell 1937: 473, Singer 1938, Makower 1938, Kalecki 1938; the debate is surveyed by Summer 1940.

2. Neisser 1937: 456; Hansen 1937: 522; Lokanathan 1938: 517; Tinbergen 1938: 166, 176. Later on, Eckhaus noted that Harrod, in spite of ‘being one of the first to incorporate the acceleration principle into a fullfledged business-cycle theory and even gave it “pride of place” in [his] theory, did not go deeply into its logic’ (1953: 212).

3. Tinbergen’s interpretation is a relevant exception; this will be discussed below.

4. In printed writings Harrod rarely referred to Tinbergen. However, he was well acquainted with Tinbergen’s work, which he considered a more systematic version of what Robertson was laboriously doing with his day-by-day analysis (for a discussion see ‘Harrod and the “Time-lag Theories of the Cycle”’, § 1, in this volume).

5. The ‘anecdotal myth’ that Harrod’s interest shifted from cycles to growth under the influence of Tinbergen’s review has been disposed of by Jolink (1995).

6. Samuelson recognised that Harrod’s equations are intrinsically nonlinear, but nonetheless represented them in linear terms.

7. This interpretation is in part justified, for Harrod seemed to think that the recovery and the boom are characterised by advance at a steady or growing rate, and conceived both these conditions as equilibrium states, because both reproduce the conditions for their maintenance (the asymmetry between growth and depression was rooted out in the ‘Essay’). The point, however, is that these commentators ignored that steady growth was treated as a steady state.

8. The third and last exception to the mainstream interpretation was Stafford (1937: 69, 76).

9. Galileo himself, after thirty years of study, was not able to formulate the notion of inertial motion, which is due to Descartes.


11. In a letter to Robertson of 1 October, 1937, Harrod complained 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 that it is my fault for being too cryptic.

12. The term, whose use was later introduced by Hicks (1950: 83), actually appears in the 1938 draft and in a letter to Keynes of 21 August, 1938: (Harrod 1996: 265) and Keynes (1973:II: 329).

13. Here an additional problem is involved, which will be discussed in Section 3.2 below, concerning the connection between subsequent states of the system.

14. This expression is due to Shackle (1967: 249).

15. Some readers actually pointed out that Harrod failed to express explicitly the causal relationships involved, which only lay behind the model (Higgins 1950: 267; Solow 1955: 34; 75n; Neisser 1954: 255, criticized Harrod for not having clearly distinguished between the two possible meanings of the accelerator, as a cause of, or as a requirement for, growth). Harrod protested that only the equation expressing actual values, being an identity, does not express causal nexuses, while ‘the rest of the analysis is intended to be a study of causes’ (Harrod 1951: 192).


18. Hawtree was one of the few commentators recognising that the warranted rate fluctuates in the course of the cycle: 1952: 293.

19. To mention a couple of examples only, Joan Robinson, in her 1949 review of Harrod’s book, devoted much of her attention to the log-run aspect of his theory, while in his review Kenneth Boulding did not say a single word on the trade cycle: Boulding 1949. A notable exception is Hansen 1949, but this is not surprising in the light of his 1937 careful interpretation of The Trade Cycle.

20. Harrod had provided a second equation, considering a term representing long-run capital outlay in order to correct the too rigid influence of the accelerator (Harrod 1939: 26–28); against the temptation to interpret this as an autonomous term, Harrod argued that long-term plans are reconsidered from time to time on the ground of the considerations coming from the accelerator, so that the distinction between autonomous and induced investment – introduced by Hicks (1950: 59) – is somewhat blurred (Harrod 1951: 267).

21. In his first reaction, however, Harrod failed to distance himself from the linearity of the econometricians’ approach: he did not stress the variability of his coefficients, and referred to the cycle as ‘oscillations around a line of steady growth’ (1951: 261). This problem, however, was soon going to be felt in its urgency by the neoclassical criticism (see below, Section 3.3.).

22. Harrod (1951), significantly titled ‘Notes on Trade Cycle Theory’, is almost entirely dedicated to drawing a line between his approach and that based on time-lags, with specific reference to Samuelson and Baumol; Harrod returned to these themes on several occasions: besides the locations concerning time-lags quoted in footnote 14 above, see also 1936: viii, 138, 167; 1948: 82–83, 89, 91, 93, 115; 1952: 286–8; 1959: 461; 1973: 30–45.

23. Although Pilvin (1952) pointed out some differences between Harrod’s and Domar’s ideas and provided at first different representations, by subsequently treating them in a single model he contributed to the interpretation of the Harrod-Domar model.

24. Solow also used the expression ‘tightrope view of economic growth’ (1956: 91). The knife-edge metaphor eventually caused some confusion, because it was interpreted in at least three distinct senses with reference to as many distinct problems. Neoclassical writers originally adopted it for indicating the divergence between warranted and natural rates, which they thought to characterise Harrod’s thought. Harrod interpreted the term as referring to the divergence between warranted and actual rates, i.e. as representing the instability principle as he originally formulated it. Eventually, Harrod thought this metaphor to be too sharp, and (unsuccessfully) suggested the substitution for it of the analogy of a shallow dome (Harrod 1970: 740) or of a ball rolling on a grassy slope (1973: 32–33). Joan Robinson interpreted instead the term as referring to the uniqueness of equilibrium (Robinson 1975; she was backed up by Kregel 1972: 43, and 1980: 110 and 118. See below, Section 3.4, for a short summary of the context of Robinson’s interpretation).

25. Eissner (1958: 709–10); Green (1960: 63–64); Hahn and Matthews (1964: 790); Asimakopulos and Weldon (1965: 56–57); Jones (1975: 59–62). Predetti (1961) and Hacche (1979: 13) pointed out the role of the variability of the coefficients for Harrod’s theory of the cycle. Shinkai 1963: 460n, recognized that Harrod was flexible in his attitude towards the capital-output ratio, and attributed instead instability (in the neo-classical sense) to the rigidity of relative prices (460).

26. ‘I did a great deal of work on this, behind the scenes so to speak, for my book in order to ensure that I was formally correct (cf. the last sentence of my footnote on p. 91)’ (Harrod to Hawtree, 31 January 1937).

27. Later on, however, in their survey of technical progress Kennedy and Thirlwall remarked that Harrod’s and Hicks’s definitions were put forward with very different purposes in view: 1972: 20.

28. In his criticism to this position, Chaudhuri raised the relevant point that Harrod assumed entrepreneurs to react rationally to the signals set up by disequilibrium (1989: 58–61). This aspect of Harrod’s notion of entrepreneurial behaviour had been neglected since Robertson’s and Hansen’s remarks relative to The Trade Cycle (see section 2 above).

29. This expression was used in the 1938 draft of the ‘Essay’ to characterize the first part of dynamics: Harrod (1996: 262).

30. While the neo-Keynesian models were almost surely not devised with the main scope to refute Harrod’s assumptions or conclusions, I think, however, that it would not be unfair to suggest that Solow actually aimed at disproving Harrod’s instability principle (or better, what he understood to be Harrod’s instability principle).

31. Hahn and Matthews specified, however, that Harrod was aware of the problem that was going to be raised by neo-classical critics (790): see Section 3.2. above.

32. Harrod’s own distinction between his theory and Domar’s is in Harrod (1959). It was soon pointed out, however, that he underestimated the differences: Ahmad 1961. For another account, see Asimakopulos (1986).


34. See, for a comment, Pugno 1992: 267–269.

35. The first reprint seems to have occurred in Hansen and Clemence (1953); there followed several others, beginning from Stiglitz and Uzawa (1969) and Sen (1971).

36. Eltis, for instance, having pointed out that between 1939 and 1952 Harrod had referred to three distinct meanings for the warranted rate, noted however that all of them are compatible with equilibrium, and instead of inquiring into the implications of the differences looked
for a unifying definition in terms of the capital coefficient entrepreneurs wish to maintain (1966: 75–77). Similarly, Gozzi stressed that the two notions of equilibrium based on the satisfaction of expectations and the constancy of the equilibrium rate are not equivalent, but also not inconsistent with each other (1989: 287).

37. Having treated Harrod's fundamental equation in terms of differential and difference equations with constant parameters (Hicks 1949: 107), Hicks failed to recognize that because of its fluctuations the warranted rate cannot be considered as describing a line (117). Finally, Hicks criticized Harrod's departure from the econometricians' and from his own notions of dynamics, without really attempting to understand what inspired Harrod's definition and aversion to lags (106).

38. Rose (1963: 72), Glommowski and Krüger (1982: 131) and Di Matteo (1987: 9n) were aware that instability is an epistemic premise to trade cycle theorizing before being an analytical premise for the operation of Harrod's trade cycle mechanism. The latter role was stressed by Haberler (1956: 199), Kalecki (1962: 134), Brooman (1962 [1970]: 346), Krauss (1970), Costa (1972: 403), Hache (1979: 13) and Nardozzi (1983: 141–144), and was implied by a parenthetical remark by Hahn (1971: xiii); Predetti provided a thorough discussion of Harrod's mechanism of 'cyclical instability' (1961: 32–35, 87–94). For a discussion of Pugno's position, see Section 4.2.

39. The non-linearity of Harrod's model and the interaction between growth and cycles did not fail to be noticed by students of non-linear models: Ichimura 1955: 25; Glommowski and Krüger 1982; Di Matteo 1987: 2; Punzo 1988: 31. It must be noted, that the first author to have remarked that Harrod was linking up fluctuations with growth seems to have been Robertson (1950: 12); his starting point was quite different, but his understanding benefited from long discussions with Harrod on this subject.

40. Young (1989: 7, 195–8) and Kregel (1972: 36–37) came instead to the misleading conclusion that Harrod found the Treatise a useful approach to growth.

41. It is worth stressing, in this connection, that some readers found some similarity between Harrod's and Wickless's cumulative process arguments: Haberler 1937: 691, Guitton 1953: 244, and Hicks 1965: 121.

42. Harrod insisted in correspondence with both Kahn and Keynes that 'there is a limit to what the human mind can assimilate' and that therefore the new Keynesian doctrine had to be expressed in a language compatible with the traditional approach (Harrod to Keynes, 30 August 1935, in Keynes 1973(1): 556; Harrod to Kahn, 17 November 1934, unpublished). Other suggestions along this line can be found in Keynes 1973(1): 533–4 and 536). Analogously, when – towards the end of his career – he faced the fact of the persistent, gross misrepresentations of his views on dynamics, he attributed them 'to a very powerful resistance of the human brain to any new idea. It has a strong tendency to reinterpret and redress the new idea in the light of established doctrines to which it is accustomed; by doing so it will almost certainly falsify the new idea' (Harrod 1971: 78).

REFERENCES


Besomi, D. (1992) 'Roy Harrod, la concorrenza imperfetta e la possibilità di una teoria dinamica', Studi Economici, 50, 2, pp. 41–70.


—— (1934b), 'The Expansion of Credit in an Advancing Community', *Economica*, NS 1, August, 287–299.


Montesano, A. (1972), 'La nozione di economia dinamica', *Giornale degli Economisti e annali di economia* XXXI (NS), 3–4, pp. 185–228.


— (1997), 'In Search of Harrod's Contribution to Economic Dynamics: A Note', in this volume.


Economic Dynamics, Trade and Growth


Sumner, J. D. (1940), 'A Note on Cyclical Changes in Demand Elasticity', American Economic Review, XX:2, June 1940, pp. 300–308.


Unpublished documents


Harrod to Kahn, 17 Nov. 1934, in Kahn Papers (King's College, Cambridge) 13/57.


Harrod to Robertson, 8 October 1937, in Harrod Papers (Chiba University of Commerce, Ichikawa) IV-990–1069d-52.

Hicks to Harrod, 30 Jan. 1963, in Harrod Papers (Chiba University of Commerce, Ichikawa) IV-496.

Robertson to Harrod, 10 February 1937, in Harrod and Keynes, Notes and Memoranda (University of Tokyo) 208–215.