4 Harrod and the ‘Time-lag Theories of the Cycle’
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1 INTRODUCTION

In all his writings on dynamics, Harrod was eager to stress the points of departure of his own conception from the alternative approaches to the study of economic change. At an early stage he disapproved of the psychological theories of the cycle; in The Trade Cycle he criticized the ‘time-lag theories of the cycle’, particularly Robertson’s lagged relationship between income and saving; he rejected Lundberg’s method of period analysis, and subsequently refuted Hicks’s definition of dynamics as requiring variables to be dated. Finally, from 1939 onwards, he concentrated his efforts in trying to emphasize the difference between his approach and the new orthodoxy in dynamics represented by Ragnar Frisch’s notion, which was quickly gaining widespread acceptance among economists. The range of Harrod’s arguments was as broad as the spectrum of his critical targets: he countered the merits of instantaneous vs. period analysis; he compared lags to ‘frictions’ and claimed that they are of secondary importance for a proper explanation of the cycle and indeed misdirect attention from its ‘true causes’; he stressed the irrelevance of lags to the analysis of equilibrium movement, and proposed a definition of dynamics based on the presence of a rate of growth among the unknowns to be determined in the fundamental equation.

Harrod used one or the other of these arguments on several occasions, but in spite of frequent critical remarks against this or that other rival conception, he never explicitly and exhaustively discussed his own. Nevertheless, he attempted to convince his fellows that his own conception of dynamics, as compared to that of his rivals, and in particular to the ‘time-lag theories of the cycle’, was more fundamental. But without a thorough explanation of his view, his repeated claims of originality passed almost completely unnoticed, and his aversion to time-lags was sometimes ridiculed or found to be in contradiction with his procedure. Yet Harrod’s apparently disparate
arguments were presented as various facets of a unitarian conception, precisely as the rival approaches were interpreted as departing from Harrod's in ways similar to each other.

The aim of this paper is to retrace the logical and chronological origin of Harrod's specific notion of dynamics, to show how the apparently motley arguments he later used are consistent with his original conception and with his early criticisms of the 'time-lag theories of the cycle'. The paper also aims to examine how the econometricians misinterpreted Harrod and how, conversely, Harrod misunderstood the econometricians' position. Finally, I will advance a tentative comparison of the relative merits of Harrod's and the econometricians' approaches to dynamics. I am of course well aware that these are not the only possible perspectives to be compared; Harrod, however, was mainly concerned with the implications of Frisch's definition, and an examination of his contention may help us to understand his approach to the scope, the method, the analytical instruments and the premises of dynamics.

Before embarking on this, it may be useful to provide a brief sketch of the relationships between Harrod and the main characters of this story, in particular Dennis Robertson and the econometricians. Harrod's contacts with Tinbergen, in particular, will help us to appreciate how Harrod came in contact with the approach of the econometricians.

Robertson and Harrod entered into frequent exchanges of correspondence from 1926, and dynamics was one of their favourite subjects. In the second part of 1934, Harrod engaged in debates regarding the notion of saving. Haberler had adopted Robertson's definition of saving as the unconsumed part of the income of the preceding period. By contrast, Harrod put forward a definition of his own, as well as considering Keynes's *Treatise on Money* definition, while later (towards the end of 1934), under the pressure of Kahn, he reverted to the 'common sense' notion of saving as the unspent part of current income. Harrod's position was that definitions are a matter of choice, and that any definition is welcome if it bears fruits. For this reason, he recognized that Robertson had obtained interesting results, and he was eager to learn from his friend the origins of the notion that saving can differ from investment. However, Harrod found more useful for his purpose the idea that *ex post* saving and investment are equal, although in the discussion of some special features of the cycle in *The Trade Cycle* he left some room for the Robertsonian assumption.

At the time of writing his 1936 book, Harrod certainly thought mainly of Robertson when he criticized the 'time-lag theories of the cycle'. He soon became aware, however, that other approaches were taking over: in particular the one systematized by Ragnar Frisch and adopted by the econometricians. In 1935, Harrod read Kalecki's and Tinbergen's contributions in *Econometrica* (Kalecki 1935, Tinbergen 1935). He was aware that Kalecki had something interesting to say, but did not understand much of the specific argument. Later on, after Jan Tinbergen was appointed by the League of Nations to engage in statistical testing of business cycle theories, Harrod exchanged views with him on a number of occasions; he had been a member of the group of trade cycle experts called upon by the League to discuss Haberler's *Prosperity and Depression* since 1934, and he later participated in a meeting concerning Tinbergen's approach. Evidence survives of Harrod's involvement in the discussions of a preliminary Interim Report in July 1937 and of Tinbergen's Report in July 1938. On the basis of his knowledge of Tinbergen and his work at the League of Nations, Harrod felt he had to defend Tinbergen's approach against what he thought to be Keynes's misunderstanding. While Keynes did not think much of the idea of estimating parameters, which he thought would render the model useless, Harrod saw the aim of statistical inquiry to be the acquisition of 'a first approximation to the right figures', 'something a little better than purely hypothetical coefficients'. Harrod expounded to Keynes what he understood to be the nature of Tinbergen's 'lag theory of the cycle' (see below, Section 3), without taking a stance either in favour of it or against it. As we shall see, however, Harrod expressed elsewhere (and consistently through time, notwithstanding a shift of emphasis since 1948) his critical remarks on the econometricians' theory as he understood it, and carefully pointed out the divergence between the two approaches.

2 THE INSTABILITY PRINCIPLE AND THE POSSIBILITY OF TRADE CYCLE THEORIZING

Before arriving at his first mature theory of the trade cycle, in the first part of 1934, Harrod devoted some thought to the preliminary problem of the possibility of theorizing such phenomenon. These reflections seem to have originated from Harrod's criticism of Pigou's psychological approach, which explained economic fluctuations in
terms of waves of optimism and pessimism. In his opinion, Pigou failed to provide an adequate theorization of the cycle because he shifted the burden of the explanation onto the persistence of an error of judgement on the part of entrepreneurs. Harrod ascribed the cause of this unsatisfactory state of affairs to the fact that Pigou’s reasoning presupposed the stability of the equilibrium position. If equilibrium is stable, any deviation from it would set up forces impelling a return to the original state; Harrod argued that if stability prevailed, to account for persisting fluctuations one would have to assume that an exogenous force is superimposed upon the normal operation of the economic system, and conclude that the cycle can endure only so long as this abnormal force is in operation. Harrod’s solution was to insert a destabilizing factor at the outset: if equilibrium were unstable, any accidental departure from it would start up a cumulative mechanism amplifying the original deviation, without making recourse to an exogenous cause (Harrod 1934, pp. 466–470. For a more detailed discussion see Besomi 1997).

At first, Harrod thought that imperfect competition would provide the right kind of mechanism, for in that situation equilibrium could be characterised by increasing returns (Harrod 1931) and therefore result in instability (Harrod 1934). However, he soon realised that the approach of Keynes’s Treatise on Money could be interpreted along the same lines. Harrod thus pointed out, in a letter to Haberler, that Keynes claimed to show that natural and market rates of interest do not tend to converge, and concluded by stressing the importance of instability to the very possibility of formulating a theory of the cycle:

Surely if that is right, it is very interesting and important. The puzzle of the cycle is that when a departure from equilibrium occurs, the system tends to move further from and not back to the equilibrium position. This movement seems contrary to the principles of supply and demand. Now if Keynes shows that these principles don’t operate in the case of interest, which clearly lies at the heart of the system, it would seem that he is supplying just the very kind of explanation that is required. Why don’t the principles of supply and demand operate? Well, that simply takes one to the heart of his theory, where I can’t go in this letter. Suffice it to say here that I do feel a lacuna in your summary, in that you do not note that there is someone professing to give – by reasoning not obviously and palpably absurd – just the very kind of explanation

which a rational account of the trade cycle requires (Harrod to Haberler, 19 Oct. 1934; emphasis mine).

Harrod’s ‘instability principle’ originated from this reasoning, a fact which clearly reveals its epistemic nature: the necessity of introducing a destabilising factor preceded the development of a trade cycle theory both in logical and chronological terms, and was therefore a postulate before being a result of the various versions of Harrod’s theory⁹. In The Trade Cycle, this principle was applied twice. On the one hand, it was applied to the static system of forces determining the level of output of individual producers: Harrod analysed the stabilising power of each of the independent static determinants, he inferred by empathy that three factors (decreasing returns, plasticity of prime costs and diminishing elasticity of demand) operate in a stabilizing sense, and he argued that, given the fact of the cycle, the fourth factor, the general price level (the monetary determinant) must act with a sufficient destabilising power to neutralize the stabilizing effect of the other three static determinants. Harrod therefore postulated that the equilibrium level of output is neutral, so that any position and any displacement between positions are admissible: the instability principle was therefore introduced primarily to make movement possible. The importance of the epistemic principle in Harrod’s reasoning is revealed by the fact that to obtain this result he knowingly sacrificed the methodological homogeneity of his construction (Harrod 1936, pp. 37–41; the stability of the static system is discussed in Ch. 1).

On the other hand, the instability principle was also applied to the dynamic set of forces determining the variations in the level of output of the system as a whole. The procedure was strictly analogous: Harrod considered three independent dynamic determinants affecting the intensity of the accelerating and multiplying effects (amount of capital used in production, propensity to save and distribution of income), studied their stabilizing effects, and concluded that one of the possible outcomes of their conjoint operation (superimposed on the neutral equilibrium determined by the static determinants) could be a state of moving equilibrium, if the stabilizing and destabilizing effects of the dynamic determinants counterbalanced each other. Such a state is interesting in itself, as it provides an example of self-sustained movement, which does not need to rely on exogenous and ad hoc causes to perpetuate itself. But this does not yet describe a cycle. To obtain a theory of the cycle, the principle of instability was introduced to amplify departures from equilibrium. Harrod, in fact,
pointed out that the dynamic determinants change their value according to the state of the economy, and that therefore we cannot rely on their operation to maintain equilibrium. For instance, as income grows we should expect that the average propensity to save increases, thereby diminishing the multiplier and consequently diminishing the increase of income induced by a unit of additional net investment. This would trigger cumulative consequences: a decrease in the rate of increase of income implies a further reduction of investment (because of the accelerator), which implies in turn a further decrease in the additional income produced (multiplier), and so on, until income actually diminishes and output falls close to the replacement level. This diminishes the average propensity to save and makes substitutive capital-intensive investment profitable, respectively, thereby eventually reversing the tendency (Harrod 1936, Ch. 2).

The instability principle was thus required to make the cycle possible, as a cumulative deviation from a moving equilibrium. In the successive versions of his theory Harrod abandoned his attempt to reconcile statics and dynamics — probably in consequence of the authoritative criticism aroused by his ‘methodological break’ (Hawtrey 1937, pp. 324–327), and of the fact that he was not able to provide an analytical link between the changes of output decreed by the dynamic determinants and the fluctuations in prices which should induce individual producers to adapt to the new state of affairs. He retained, however, the instability principle as the essential component of his trade cycle mechanism. The ‘warranted rate of growth’ describes at any instant the state of moving equilibrium. If the actual rate of growth diverges from this, it sets up forces stimulating a further divergence, until a ‘ceiling’ (the ‘natural rate of growth’, that is the maximum pace of advance permitted by full employment of labour and/or resources) or a floor is reached. During the process of cumulative growth (or downturn), the value of the accelerating and multiplying coefficients changes, so that when the actual rate of growth is limited, for instance by the ceiling, the warranted rate which was dragged along with the actual one eventually surpasses it, and starts off the inverse operation of the instability principle.

3 THE VERA CAUSA OF THE CYCLE

In his writings, Harrod repeatedly stressed the crucial role of the instability principle in explaining the trade cycle, and at the same time often complained that the ‘time-lag theories of the cycle’ missed the vera causa of the phenomenon. The context in which Harrod formulated his criticism reveals that he regarded the difference between the two approaches as lying not in their attribution of different mechanical causes to the same phenomenon but in the fact that they constituted two different kinds of explanation. In fact, he opposed his epistemic principle (which postulates that we need some destabilizing factor making movement and cycles possible as deviations from static and dynamic equilibrium respectively) to the theories based on time-lags, errors of calculation and frictions. Harrod interpreted process analysis as being based on mal-adjustments, and therefore associated them with his criticism of Pigou’s theory. For example, in the preface of The Trade Cycle, Harrod remarked that writers seeking to introduce dynamic considerations have often tended to confine themselves to mere description or to develop a theory regarding time-lags. But is not a theory of time-lags or of friction premature when the fundamental propositions relating to velocity and acceleration remain unformulated? (Harrod 1936, p. viii, italics mine).

Harrod explicitly stated in a letter to Keynes, in which he expounded what he understood to be Tinbergen’s procedure, his interpretation of the time-lag theory of the econometricians in terms of fluctuations introduced into the system by a disturbance: as ‘a time-lag between certain adjustments’ (Harrod 1938, p. 408):

[Tinbergen] is interested in the fundamental point that if you assume certain lags of adjustment, e.g. between increase of profit and plant expansion, or between an increase of income and spending etc. you automatically get a cycle. It is really a lag theory of the cycle. To show how it works you have to make a model. Now in making the model you have to put in certain assumptions, e.g. people spend 1/10 of income etc. The duration and amplitude of your cycle will vary according to the coefficients you put in and the variables you assume important (Harrod to Keynes, 7 September, 1938, in Keynes 1973, p. 303).

A few days later, Harrod emphasized the similarity he saw between the econometricians’ and Robertson’s approaches:
The introduction of a lag into an otherwise smoothly working system may set up an oscillation. Tinbergen reviews a number of theories of this sort in *Econometrica* 1935. Kalecki, Lundberg and others have been working on them. I think it is really only doing systematically and with the help of a sine curve what Dennis does laboriously with his day by day analysis. I take it that the more mathematical part enables one to detect the quantitative implications of one's own theory more easily and with an exactitude that it would be superhumanly laborious to get by the sort of methods Dennis uses (Harrod to Keynes, 18 September 1938, in Keynes 1973, pp. 304–305).

Finally, Harrod interpreted Lundberg's "sequence" method of analysis along the same lines, since he understood the 'reaction time' to reflect an irrational element...connected with ignorance (Harrod 1937, pp. 496–497).

In exact analogy with his criticism of Pigou's psychological theory, Harrod believed that recourse to maladjustments was made necessary for an explanation of the cycle by an underlying faith in the stability of equilibrium:

What I feel about people broadly in your position is that you cling a little too tenaciously to the view that the classical analysis shows that the system must be self-adjusting in the end. You are inclined therefore to emphasize time-lags and miscalculations (Harrod to Robertson, 25 December, 1936).

Harrod was thus contrasting the primacy of place of his instability principle with the subsidiary importance of all kinds of errors in the construction of an explanation of the cycle. In a letter to Tinbergen, commenting on his review of *The Trade Cycle*, Harrod expressed the point as follows:

you will not get at the vera causa of the cycle by looking at lags only. I have no doubt they play some part in the whole thing, but I believe it will be found to be a relatively minor part.

I see you convict me of bringing lags into my argument at various points. Of course I do. But I do not think that the assumption of a lag is present in the fundamental part of my argument formulated in the equations above (Harrod to Tinbergen, 1 July, 1937).

This statement reveals that Harrod was not denying that lags, frictions and errors have a place in the theory of the cycle; these, however, were required to play a different, and logically subordinate, role with respect to the instability principle. While in Harrod's view the alternative approaches to dynamics elevated these secondary factors to the cause of the cycle, Harrod was eager to stress that the cycle would occur even in the most optimistic scenario of an absence of errors of judgement (see e.g. Harrod 1936, pp. 96–97). Errors of course do occur, but their part in Harrod's theory of the cycle was ancillary only. In the first place,

if error is to play any part in the theory, it may be used to account for the initial step. One downward step having been taken by one individual or a group of individuals from error, the rest follows from logical and not psychological reasons. On this explanation error may be called in as a *deus ex machina* to explain the original impetus to a movement; thereafter its services can be dispensed with. While, if perfect competition is postulated, the *deus ex machina* has to be maintained in operation, until the reverse movement begins. The prolonged persistence of these errors is surely an unreasonable hypothesis (Harrod 1934, p. 470).

In the second place, errors are to be considered as *disturbances* in the proper operation of the fundamental dynamic laws, whose effects are to be superimposed, at a logically subsequent stage, upon the 'pure' movement that would be described in the absence of friction. This point is of extreme importance in understanding Harrod's approach, and it is therefore necessary to examine the root of his reasoning.

4 MAPPING THE POSSIBLE STATES OF THE WORLD

From 1934, Harrod claimed that dynamics is primarily concerned with the mutual relationship between magnitudes in a system undergoing steady (i.e., continuous and at a constant rate) advance (Harrod 1934a, p. 287), and that its discussion requires an examination of the state of the system at a single given instant. Although – as we shall better see in Section 6 below – he consistently maintained this standpoint, only in 1938 did he explicitly discuss the foundations of his method in the light of his view of science. He devoted two pieces of
writing to this problem: the Presidential Address to the British Association on ‘Scope and Method of Economics’ (Harrod 1938), and an unfinished book on philosophical issues\textsuperscript{14} (Harrod 1940/41\textsuperscript{*}).

Harrod believed the ‘scientific aspect’ of economics to consist of ‘the formulation of general laws and maxims’ (Harrod 1938, pp. 385–6). As preliminary groundwork, in Harrod’s view it is necessary to draw ‘a simultaneous chart or survey of the economic field’, ‘the main work of the cartographer [being] analysis and classification\textsuperscript{15}’ (ibid., p. 387). As examples, he mentioned the following:

The relevant propositions may be stated in the form of truisms or tautologies\textsuperscript{16}, such as that the price of an article is equal to the sum of rewards to all persons contributing to its production, or again, if services of the same type get equal rewards in different occupations, the prices of commodities will be proportional to the quantity of service required for their production (ibid., p. 392).

Or:

There is the analysis of the contribution of capital to production as consisting essentially of waiting. There is all the work concerning the relation between direct and overhead costs. The so-called law of rent has given rise to a number of dichotomies of great interest. The concept of profit as a reward for skill and judgement has been rendered fairly precise. Professor Knight has shed a penetrating light upon the relation of profit to uncertainty-bearing, but some puzzles here remain. Meanwhile Mr. Keynes has produced another concept – liquidity-sacrifice, which bids fair to find a place as an independent factor (ibid., p. 398).

And again:

In the classificatory work I include truisms like the quantity theory of money and the wages-fund theory, which serve to give precision to the concepts (ibid., pp. 405–6).

Harrod explicitly pursued the geographical analogy by observing that, on the basis of the recognition of truisms and definition of concepts, a ‘map’ is formulated, which consists of a comprehensive identification of all possible states of the world; the map provides a guide for life, for only with reference to a map we can distinguish a piece of scrap paper from a cheque (Harrod 1940/41\textsuperscript{*}, Book I, Ch. 2 TS, pp. 20–21). For an economic example of such a map, Harrod mentioned Adam Smith’s perception ‘that the complex phenomena of markets and prices might be regarded as the result of the efforts of individuals to inform each other of their preferences’ (Harrod 1938, p. 392). Adam Smith perceived that by means of such intuition ‘it would be possible to make sense of the confused and conflicting arguments of economic doctors and reduce chaos to order’ (ibid., p. 390), and on the basis of this ‘comprehensive but simultaneous conspectus of the field as a whole’ (ibid., p. 388)

Economists have constructed a map or model in which individuals are seen informing each other of their preferences. (It may help the reader to regard this map as the ‘theory of perfect competition,’ provided that all reference to the sequence of events is excluded from that ‘theory’) (ibid., p. 392).

The remark regarding simultaneity and the exclusion of the sequence of events is important. In Harrod’s view, in fact, the search for laws of succession must follow the stage of the contouring of the map:

If in addition we can formulate comprehensive laws of succession governing the physical qualities a complete map of the world can be made, specifying every item, so that everything capable of occurring has its counterpart in our map which copies reality as accurately as our methods of measurement allow (Harrod 1940/41\textsuperscript{*}, Book I, Ch. 2, TS p. 43).

And ‘it is with the laws governing the succession of [possible states of the world] that science is mainly concerned’\textsuperscript{17} (ibid., p. 21). Only the elaboration of such a corpus of laws permits us to predict the future course of events (ibid., pp. 20–21 and passim; 1938, pp. 398–9 and passim).

The message of Harrod’s 1938 methodological essay seems to have been that economics may claim scientific status only to the extent that it can repeat the procedure that led physics to success. The first step is therefore the work of the cartographer: that of recognising a handful of ‘economic entities’ by means of ‘analysis and classification’. The second step is to identify the fundamental laws – simultaneous or causal – which constitute the ‘map’. Some of these are tautologies, while the others are based on so broad an experience ‘that the
principle may be taken as an *axiom* of the highest possible degree of empirical probability* (Harrod 1938, p. 387, italics mine; see also pp. 400 and 404), from which it is possible to proceed by way of deduction (ibid., p. 401). Due to their generality, however, these axioms only permit *qualitative* inference, and therefore a limited power of prediction. But

Our laws of succession if they are to be precise, must be capable of expression in quantitative terms. The invented properties of physics, being capable of exact measurement, give great additional scope for quantitative laws (Harrod 1940/41, Ch. 2, TS, p. 42).

This being so, the next step would appear to be to obtain more precise knowledge* (Harrod 1938, p. 400), though at the price of a loss of generality. As it has been for physics, economics too can only hope to enlarge its predictive power

by such empirical observations as make it possible to fill in the blank-forms of equations with quantitative data.

This may be done. It should be noted that the results obtained will at best not have a very high degree of probability. Yet it must be said that if real equations could be substituted for the present empty forms, even if the former were conjectural and hazardous in the extreme, economics would be on its way to looking more like a mature science than it does at present (ibid., p. 401).

Accordingly, Harrod saw economics as proceeding in three stages. The mapping process provides two: an initial analysis of a system of mutual relationships *at a given point in time* and a subsequent one relating to the sequence of events. It is important to notice that this distinction does not correspond with the demarcation of statics and dynamics, but with points at different stages within both statics and dynamics. As regards the former, given the fundamental circumstances (the state of preferences, productive techniques and resources), the equilibrium structure of output and prices is determined; if a change in these circumstances occurs, then the consequences concerning the price-output structure are deduced from the empirically based Law of Demand, in the form of general laws concerning the succession of events (Harrod 1938, pp. 386 and 398). Analogously, the dynamic fundamental conditions (the propensity to save and the acceleration coefficient) determine the equilibrium rate of growth of the system; if a

change in these circumstances occurs, then from the simple laws of growth causal laws are deduced which determine the succession of events constituting the trade cycle (Harrod 1938, pp. 402–404, and 1939). Finally, having completed the two stages of mapping, the empirical determination of the shape of the static and dynamic sets of equations would allow prediction and the shaping of policy advice. The difference between statics and dynamics therefore does not lie in their respective procedures, which are instead strictly equivalent, but primarily in the fact that in statics changes are of a one-off nature, while in dynamics they occur continuously (Harrod 1938, p. 404, 1939, p. 15; see for a discussion Besomi 1997), and secondly in the fact that the changes considered in statics use up their entire force in determining the new equilibrium state, which is therefore stable (or neutral, in Harrod’s recasting of statics in *The Trade Cycle*), while changes in the dynamic equilibrium in turn lead to new changes in the fundamental conditions and thus to instability of the equilibrium growth path.

5 ‘DYNAMICS PROPER’ AND ‘THE SUCCESSION OF EVENTS’

Soon after writing his methodological piece, Harrod jotted down a first draft of his ‘Essay in Dynamic Theory’ (Harrod 1996; for a chronology see Besomi 1996a). This clearly reflected the division into stages which was theorised a few months earlier, although the sharp division in the original formulation was concealed in the published version. The first stage concerned the determination of the dynamic equilibrium in a given instant and the discussion of its stability (Sections 1–11; a simplified case was discussed first, and in Sections 10 and 11 additional considerations on long-range capital outlays and foreign trade were added). In this stage, the fundamental conditions, that is, the propensity to save and the acceleration coefficient, were taken as given. The succession of events occurring in the trade cycle was studied in the second stage (Sections 12–21 of the manuscript): variations in the fundamental conditions not only were allowed but played an important part in the argument. This stage was characterised as involving ‘a certain element of conjecture’. In the final stage, economic policies were discussed (Sections 22–25) in the light of the preceding analysis, according to the relative positions of natural and
normal (later ‘proper’) rates of growth. In the second and third stages, however, Harrod did not push his analysis beyond a qualitative discussion.

This division into stages is quite important for understanding Harrod’s attitude towards the approach of the econometricians – which, at this point, was the main target of Harrod’s criticism. Harrod in fact considered ‘dynamics proper’ to be confined to the ‘simultaneous conspectus’ of the first of these stages: having determined the instantaneous growth rate of the system and discussed its stability, Harrod remarked that ‘The strict part of [“dynamic”] theory ceases here’ (Harrod 1996, pp. 262 and 264). Harrod also stressed that trade cycle analysis is concerned with the succession of phenomena and thus ‘involve[es] a certain element of conjecture’ (Harrod 1996, p. 262). Both remarks have been deleted during revision of the text.

Harrod’s strategy of revision, under the pressure of Keynes’s attack, itself respected the methodological priorities set out in Harrod’s 1938 Presidential Address (for a table of comparison between the two drafts of the Essay, see the apparatus criticus of Harrod 1996). The first eleven paragraphs of the first draft of the ‘Essay’ were the hard core of Harrod’s dynamic theory, and necessarily had to be maintained and to survive Keynes’s criticism practically intact.

The concluding paragraphs of the ‘Essay’ were also not substantially altered by Harrod’s revision. There Harrod examined suitable policies in the event of a divergence of the natural from the warranted rate, paying particular attention to a situation he thought to be representative of modern conditions, that is, a warranted rate higher than the natural rate.

Keynes’s ‘editorial preference for something short’, and Harrod’s own concerns about the length of his paper, thus cast a blow to the central part of the Essay, that relating to the trade cycle. The consequence of the deletions has been most unfortunate, because for many years it was not understood that Harrod’s dynamics was developed as a theoretical treatment of the business cycle – although this necessarily ‘involve[d] a certain element of conjecture’ (Harrod 1996, p. 262) – and when it was finally appreciated, the richness of his original analysis escaped the commentators.

The second important aspect of Harrod’s distinction between the first two stages of analysis for understanding his attitude towards the econometricians lies in the fact that Harrod interpreted Tinbergen’s results as depending upon the presence of lags. In Harrod’s view, the econometricians proceeded by demonstrating that the introduction of lags into the process of adjustment to a new static position in an otherwise smoothly working system may give rise to oscillations. He therefore saw the econometricians’ notion of dynamics as clashing with his own in several respects, although he believed that time lags could be grafted on to his own second stage.

In the first place, the econometricians’ approach does not necessarily concern growth, while the process of advance was at the core of Harrod’s notion of dynamics:

Attempts to construct a dynamic theory have recently been proceeding upon another line, namely by the study of time lags between certain adjustments. By the introduction of an appropriate lag the tendency of a system to oscillate can be established. In these studies there is some doubt as to the nature of the trend on which the oscillation is superimposed. Supposing damping measures could be introduced, to counteract the oscillation caused by the lag, would the system be stationary or advancing? And at what rate? Dynamic theory in my sense may throw some light upon this (Harrod 1996, § 2, and 1939, pp. 14–15).

Harrod concluded in a seemingly conciliatory way by recognising the role of lags in the explanation of the cycle. However, he stressed that growth has logical (not phenomenological) priority with respect to cycles:

Moreover it is possible, and this the following argument seeks to establish, that the trend of growth may itself generate forces making for oscillation. This, if so, would not impair the importance of the study of the effect of lags. But it may be that the attempt to explain the trade cycle by exclusive reference to them is an unnecessary tour de force. The study of the operation of the forces maintaining a trend of increase and the study of lags should go together (Harrod 1996, p. 254, and 1939, p. 15).

In the second place, accordingly, Harrod maintained that logically, the study of the forces determining growth must be undertaken prior to the study of lags:

(b) It may further be objected that even in the sphere in which the acceleration principle holds there must be some lag between the
increased provision of equipment (and stocks?) and the increased flow of output which they are designed to support. There may be some force in this. But the point is deliberately neglected in this argument, along with all questions of lags. The study of these lags is of undoubted importance, but a division of labour in analysis is indispensable, and in this case the neglect is necessary in order to get the clearest possible view of the force determining the trend and its influence as such.

Harrod stressed the point more forcefully in a passage which was omitted from the published version of the Essay. Having formulated his cycle theory, with its emphasis on cumulative deviations of the actual rate of growth from the warranted (equilibrium) path, he commented as follows:

It may be objected that, despite disclaimers, a lag is essential to this account of oscillatory behaviour. For if there were no lag there would always be an immediate adjustment to the new warranted position. Throughout the period in which the actual rate diverges from the warranted rate, producers are releasing output, which, if they had a correct view of requirements in the succeeding period, they would realize to be wrong. The objection must be admitted; a lag is implied. It is only in the formulation of the fundamental equation in its different forms for determining the warranted rate of growth that consideration of lags is rigidly excluded. This is dynamic because it embodies a rate of growth as an unknown variable; and the whole of the argument which follows depends upon it. It provides a framework of thought, within which the significance of lags, the importance of which is not denied, may be fruitfully considered (Harrod 1996, pp. 269–270).

Harrod had already stressed the point vigorously in his review of Lundberg’s Studies in the Theory of Economic Expansion, which turned out to be an attack upon the method of sequence analysis:

If a dynamic system of this kind could be established, the considerations introduced by reference to ‘sequences’ could be superimposed as corrections. My complaint of the sequence analysis is that it seeks to introduce the corrections, before the dynamic principles themselves are established (Harrod 1937, p. 496–7).

Harrod also returned to this argument in correspondence with Robertson, specifying that a proper dynamic theory must provide

the framework of thought (i.e. the map) which is a necessary preliminary to the formulation of specific theories of oscillation around the moving equilibrium. In reply to one of Robertson’s ‘two fundamental objections to the neo-Keynesian analysis’, that it substituted ‘comparative statics for true dynamics in the treatment of short period situations’ (Robertson to Harrod, 23 July, 1938), Harrod agreed that

the theory of the General Theory is fundamentally static for lack of incorporation of ‘the acceleration principle’ or something of the sort. On the other hand I am in sympathy with the main contention that orthodox static theory (say Marshall) does not give us all we want in the way of a framework of explanation of phenomena before we begin to play about with lags. In the end we have got to face the question of lags. But we can only do so profitably if we are sure that we are using lag hypothesis in a framework of concepts which does justice to the forces determining normal equilibrium or normal trend. I don’t think we are yet in that position (Harrod to Robertson, 3 Aug., 1938).

Finally, after the war Harrod took up this argument again in Towards a Dynamic Economics, explicitly stressing the fundamental and complementary roles of the instability principle and of lags and other kinds of frictions respectively:

It is far from my purpose to give a finished theory of the trade cycle. Lags, psychological, monetary and other factors, no doubt play their part. I should suggest that no theory can be complete which neglects the fundamental causes of instability expressed in the equations which have been set out (Harrod 1948, p. 89).

This book, however, marks a turning point in the expression of Harrod’s ideas. Probably because of the debates on growth models it itself helped to generate, and because of the prominence gained by the phenomenon of growth with respect to the cycle, Harrod later redirected his criticism towards a different aspect. Before examining it, however, it is necessary to stress that, in spite of having shifted the emphasis in his later writings towards ‘dynamics proper’ (with the unfortunate consequence that his theory was interpreted as a theory of growth), Harrod never failed to discuss the second and third stages of his dynamics. Accordingly, he consistently stressed the implications
for the trade cycle, and for policy, of the instability principle and the relative positions of the warranted and natural rates of growth respectively. These aspects, however, did not appeal to commentators, who preferred to concentrate on the discussion of the stability of the 'growth path' which they saw in Harrod's 'model' (see, for a discussion, Besomi 1996a).

6 MOVING EQUILIBRIUM AND TIME-LAGS

Having formulated his idea of moving equilibrium as a self-maintaining state of the economy, Harrod stressed that in such circumstances time lags have 'no significance', and indeed he doubted 'whether a lag is an appropriate concept in this connexion': with reference to the Robertsonian assumption of a lag between income and expenditure, Harrod pointed out that

If income rises steadily and a constant proportion is saved, expenditure will rise contemporaneously with income; it may be possible to identify the pennies spent on the nth day with those received on the mth; such identification is of no economic interest. If a steadily rising proportion of income is saved the rise of expenditure will still from day to day bear a constant ratio to the rise of income (Harrod 1936, p. 129; see also Harrod 1939, p. 20, and 1948, p. 132).

The point, of course, was that equilibrium is characterised by the consistency of the growth rates of the different components of the system, so that it does not matter whether variables are lagged or not.

In his later writings, Harrod often reiterated this argument (see for instance Harrod 1959, pp. 452–453; 1960, p. 277; 1962, p. 1009), drawing two conclusions from it. Firstly, he recognized that time-lags become relevant again in the event of a departure from the regular pace of equilibrium advance. But this is the subject of the second stage of dynamic analysis: according to Harrod's method, the analysis of lags must play a logically and chronologically subordinate role in dynamics. Harrod explicitly insisted on both aspects. On the one hand, having pointed out that his fundamental equation of the warranted growth rate enables one to 'formuler certaines lois établissant la relation nécessaire entre les divers facteurs de la croissance', he stressed that the neglect of lags belongs to the logic of his own method:

Il suit de la définition même du concept que les décalages dans le temps sont négligés. Cela semble peu réaliste à première vue, mais un instant de réflexion montre le contraire: c'est simplement une conséquence logique de la méthode d'approche (Harrod 1955, p. 359; emphasis mine)

On the other hand, he also stressed that the analysis of steady advance 'has logical priority and should be undertaken first' (Harrod 1973, p. 16):

as a matter of methodological expediency, the analysis of a regular advance should precede, if possible, that of an irregular advance. Knowledge about the mutual relations between variables, exogenous or dependent, in a regular advance ought to throw light on observed relations in an irregular advance. If we first concentrate attention on necessary relations in a regular advance we need and should consider one point of time only (Harrod 1963, pp. 402–403).

It should be noted that this argument differs from that advanced in the earlier writings, considered in the preceding section. Although in both cases Harrod took as a starting point the division into stages, he was initially reluctant to view lags as anything other than corrections to the 'fundamental' operation of the instability principle and variations of the dynamic determinants, while later he seemed to implicitly acknowledge that lags cannot be ignored in the second stage of analysis.

The second corollary Harrod drew from his argument concerns the intrinsic difference between his approach and the development of complete 'models'. Harrod admittedly never went beyond the first stage of dynamics. Accordingly, he titled his 1948 book 'Towards a Dynamic Economics', and when later he revised it and 'cast aside the modesty implied in the words "Towards a"', he stressed nonetheless that 'we are still on the threshold of the subject', and that his last thorough contribution 'should be regarded as a first beginning' (Harrod 1973, Preface). He firmly believed, however, that the system of mutual relationships he had enshrined in his 'fundamental equation', together with the instability principle, could provide the 'framework of
thought sufficient to inspire a ‘mental revolution’ (1939, p. 15). He thus repeatedly claimed to be providing tools of thought and a basic system of axioms, and to be laying foundations for a new way of thinking (see e.g. Harrod 1948, p. 80; 1953, pp. 553 and 555; 1959, p. 451; 1960, p. 277, and 1973, p. 2).

On the other hand, Harrod maintained that his approach was more ‘fundamental’ than, and indeed a prolegomenon of, model-building (Harrod 1955, p. 360–361, and 1960, p. 277). He understood the essence of models as ‘a formulation that has adjustable parameters’ (Harrod 1968, p. 174), the value of which is subject to empirical research, and claimed his theory to enjoy a higher logical status:

‘complete models’...require special postulates and assumptions in regard to lags and coefficients, which can only be accepted subject to statistical verification. While it is interesting and satisfactory to have such complete models for comparison with the phenomena of the real world, it is clear that the ‘theories’ of the cycle based on them have a greatly inferior authority, since their logical status is precarious, while mine rests, like the ‘law of demand’ itself, on assumptions of the utmost simplicity and generality. But mine makes no pretension to giving a complete explanation of the cycle24 (Harrod 1952, p. 286).

7 NON-LINEARITY AND INSTANTANEOUSNESS

Besides the above logical arguments developed on the grounds of methodological division of dynamics into subsequent stages, Harrod also had an analytical motive for limiting his time horizon to a single instant.

Harrod thought his fundamental relationships – the multiplier and the accelerator – to be variable in time, according to variations of income, the phases of the trade cycle, etc. In The Trade Cycle nonlinearity was vigorously stressed, with explicit discussion of the behaviour of the dynamic determinants in the course of the cycle (Harrod 1936, pp. 92–94). In the preliminary draft of the Essay, the nonlinearities were much more visible than in the final version (Harrod 1996 and 1939; for a discussion see Besomi 1996). In Towards a Dynamic Economics Harrod clearly pointed out that the warranted rate of growth fluctuates over the trade cycle (Harrod 1948, p. 89).

But a temporary assumption laid out a few pages earlier, ‘that the capital/income ratio is constant’ (p. 82), was badly misunderstood by commentators as being a permanent hypothesis. This gave space to the neo-classical criticism that the instability of equilibrium in Harrod’s model depended upon the rigidity of the capital coefficient. Thereafter Harrod’s disclaimers passed practically unnoticed, commentators failed to reflect on the apparent inconsistency between the preliminary assumption and the final statement, and no one understood that Harrod’s method was intimately connected with his otherwise puzzling statement that his analysis relates to one point of time (Harrod 1959, p. 454; 1960, p. 279; 1962, p. 1009; 1963, p. 403; 1973, p. 16).

Harrod had indeed a non-linear interpretation of the trade cycle. His mathematics, however, was totally inadequate for dealing with it. This did not give rise to problems in the ‘literary’ treatment of The Trade Cycle, but Harrod ran into trouble as soon as he attempted a formalization of his reasoning. The determination of the warranted rate of growth depends upon the accelerating and multiplying coefficients. But since growth itself induces a change in the coefficients, Harrod had to assume that the time-range was short enough to enable one to assume that his coefficients did not change. To be rigorous, he had to confine his analysis to a single instant, and be content with recalculating everything instant after instant, updating at every stage the value of his coefficients. But, again, this was only necessary in Stage 2 of dynamics: the determination of the rate of advance allowing the various components of the economy to progress in harmony was happily possible within Harrod’s ‘point in time’, while the succession of events – and the trade cycle – belonged to Stage 2 and their discussion could thus be deferred (for a discussion of instantaneous in the “Essay” see Besomi 1995).

Some considerations are necessary at this point. Firstly it is now easy to imagine that when Harrod formulated (on p. 82 of Towards a Dynamic Economics) his assumption that the capital coefficient is constant, he was discussing the equilibrium conditions: he was still in Stage 1, and his time-horizon was a single instant; and that when (on pages 89–90) he stressed that the warranted rate fluctuates in the cycle due to changes in both accelerator and multiplying coefficients, he was discussing the trade cycle in stage 2.

Secondly, Harrod’s failure to explicitly incorporate the non-linearity in his quasi-mathematical treatment was readily spotted by Marschak, who had the benefit of having read the first draft of the 1939 article: having noticed that the coefficients were supposed to change in the
course of the cycle – he even drew a picture clearly showing a fluctuating warranted rate of growth – he asked Harrod to give his assumption a more precise expression:

I presume that you postulate a change in the form of these functions [the saving and investment functions, expressed in terms of the level and the rate of change of income] themselves; in fact, you did discuss changes in the parameter C [the acceleration coefficient]; could the postulates as to the behaviour of C (viz., its relations to changes in G) be stated explicitly? (Marschak 1938).

Thirdly, one should note that nowadays Harrod's problems could be tackled in different ways, namely those suggested by Marschak, i.e. by means of non-linear differential equations. This solution was well beyond Harrod's mathematical understanding (and, at that time, out of every economist's reach); moreover, even if it would solve this specific problem, it would not be reconciled with Harrod's division of dynamics into stages. Functional equations, in fact, whether they are linear or not, aim to represent a whole history of the system based upon a knowledge of the system of relationships and of any of its states (the initial condition). This raises the questions – to be dealt with in the next two sections – of whether the 'econometric' renditions of Harrod's theory reflected Harrod's own intent, and requires the comparison of Harrod's and Frisch's notions of dynamics.

8 IN THE HANDS OF MODEL-MAKERS

Besides the reviews of Harrod's books, most of the interpretations of and comments on Harrod's theory were formulated in terms of functional equations. The first instance was Tinbergen's review of *The Trade Cycle*, in which Harrod's relationships were translated into a first-order linear differential equation. Tinbergen pointed out that such an equation, unless a lag was inserted somewhere, could only give rise to exponential growth and not to cycles (Tinbergen 1937). Next came Samuelson's well-known multiplier-accelerator article, which recognized Harrod's pioneering role, understood that he had non-linear relationships in mind, but nonetheless expressed the model in terms of a linear second order difference equation, whose qualitative behaviour (explosive, damped or constant fluctuations or growth) was discussed in relation to the value of the parameters (Samuelson 1939).

The Essay was discussed by Marshak in terms of functional equations. He was the first to point out (in a note to Harrod: Marshak 1938) that an explicit link between successive states was missing. A similar criticism was later advanced by Baumol, who actually formulated an assumption about the entrepreneurs' reaction to disequilibrium which enabled him to close the model and to analyse the stability of equilibrium in mathematical terms (Baumol 1948 and 1949). This opened an era of debates on the stability of the 'Harrod-Domar model', which, as should be expected, led nowhere: not surprisingly, the results each commentator obtained depended on the kind of assumption which was made at the outset; most participants, of course, claimed that their own formulation best represented Harrod's original verbal statement.

These debates are characterized by the fact that by this time economists were no longer interested in the cycle but in growth, so that the role of instability in Harrod's theory of the cycle was generally ignored. Given this, the whole sense of Harrod's construction was misunderstood. It was not appreciated that Harrod had introduced a distinction between stages, so his statement that the analysis relates to one instant must, at best, have seemed an oddity, which in any case contradicted the fact that here and there in Harrod's writings some lags slipped in.

Non-linear dynamics, from Goodwin (1951) onwards, produced models which were certainly more akin to Harrod's intention. However, the authors engaged in the production of such models shared with linear modellers an interest in results rather than in premises, so that once again the role of Harrod's instability principle was understated and misunderstood. Similarly, the instability of equilibrium was interpreted as a property of the system of equations under the appropriate assumptions regarding the entrepreneur's reaction to disequilibrium.

As to the interpretation of Harrod's dynamics, the outcome of these decades of model-building is certainly disappointing: not only was the specific trade cycle mechanism he proposed and restated several times not understood, but Harrod's explicit attempts to influence the mode of thinking about dynamic problems were simply ignored. Worse still, his results were interpreted and glorified in terms which Harrod consistently and systematically rejected for decades.
9 DYNAMICS, FORCES AND CAUSES

In his responses to the misinterpretation of his dynamics, Harrod tended to reinforce the aspect of his approach which he thought to be misunderstood by the authors in question. But in spite of his eagerness to stress – especially against the dominant approach to economic dynamics – the originality of his own method of analysis and its logical superiority, he never provided a systematic exposition and defence of his view. It must not surprise us, therefore, that the hints he threw out from time to time were either ignored or ridiculed by his opponents. Taken one by one, his claims (i) that dynamics must be distinguished from statics because one is suitable to deal with continuous and the other with one-off changes, (ii) that statics concerns economies in which there is no saving, and (iii) that dynamic propositions must concern rates of growth at one given time, are not informative and would inevitably appear strange (to say the least) to model-makers trained in the use of functional equations.

Harrod had something very precise in mind when he talked about dynamics, and the three statements mentioned above – and others – were different facets of the same notion. To understand Harrod's idea, and to assess the relevance of his opposition to the 'time-lag theories of the cycle', it is necessary to examine the origins of his theory.

Harrod's epistemic postulate that instability is a necessary condition for the possibility of motion and of the cycle itself was first expressed analytically in The Trade Cycle. Harrod formulated it within a framework characterized on the one hand by a notion of equilibrium as a balance of forces, and on the other hand by the principle that the forces affecting the level of output must be studied prior to, and independently of, the forces determining the variations of output.

The idea of equilibrium as a state which, if reached, would maintain itself because all the forces acting in different directions cancel out each other's effects, originated from the statics of the sixteenth century, and in turn gave rise to the Galilean and later to the Newtonian approaches to the movement of bodies. In particular, it led to Newton's first law which attributes the acceleration a body undergoes to an actual cause; namely the force which is applied to it. The appropriate language to express this linear causality is the algebra of vectors, which enables one to add the forces acting upon a point and calculate, at any instant, the resultant which causes the magnitude and the direction of acceleration. Rational mechanics, however, soon became interested in a more general problem than that of the forces acting upon a single point, or upon points unconnected with each other: it was interested in systems of simultaneous equations, in which all the variables are mutually dependent and in which it is no longer possible to associate a specific force with the acceleration it causes, because the resulting acceleration depends on the state of the whole system. The appropriate formalism to deal with such systems is the Hamiltonian one: nineteenth-century physics was no longer vectorial, but was a physics of scalars, which substituted the linear causality with a circular notion of causality (Prigogine, Stengers and Pahaut 1979, pp. 39-40).

This brief excursus highlights a fundamental difference between the Harrodian and Frischian notions of dynamics: Harrod was reasoning in terms of forces actually causing changes in the state of motion, while the econometricians were referring to a system of functional equations in which every variable contributes to the determination of some other variable.

The causal structure of Harrod's economics found its clearest expression in The Trade Cycle. There Harrod explicitly discussed the static and dynamic determinants in terms of forces causing entrepreneurs (as individuals in the case of statics, and as a body in the case of dynamics) to change their course of action: the forces did not directly determine the level of output or, respectively, its rate of change, but induced (or deterred) an increase in activity. The analogy with the Newtonian forces causing acceleration, whilst probably not explicitly sought by Harrod, is precise. Accordingly, equilibrium was thus perceived as the status in which none of the forces inducing an alteration in the state of things could become strong enough to induce some change. Both in statics and in dynamics, the stability of equilibrium was discussed in terms of the forces which would prevail in out-of-equilibrium situations (for a detailed discussion, see Besomi 1997). In Harrod's later writings this reasoning was not so explicit, but it is quite clear that he continued to treat the 'dynamic determinants' (although this term was used on a few occasions only) – propensity to save and acceleration coefficient – as independent forces acting on saving and investment respectively.

Dynamics as defined by Ragnar Frisch and used by the 'econometricians' explicitly refers to a system of equations. A 'determinate and quantitative economic theory' consists of a set of n variables and n independent structural relations (Frisch 1936, p. 100). Such theory is 'dynamic' if it
explains how one situation grows out of the foregoing. In this type of analysis we consider not only a set of magnitudes in a given point of time and study the interrelations between them, but we consider the magnitudes of certain variables in different points of time, and we introduce certain equations which embrace at the same time several of these magnitudes belonging to different instants. This is the essential characteristic of a dynamic theory. Only by a theory of this type we can explain how one situation grows out of the foregoing (Frisch 1933, pp. 171–172; see also 1936, pp. 100–102).

In this view, it does not make sense to distinguish a ‘cause’ from its ‘effect’ because the latter, in another equation, ‘causes’ the former: it is the state of the whole system at one instant which determines the successive states of the system. As Samuelson noted, in a closed system of this sort the notion of ‘cause’ is extremely slippery and ambiguous (1947, p. 315).

Seen in this context, Harrod’s refusal to close his system and to turn it into a ‘model’ could be interpreted (although only by way of speculation) as an indication that he was willing to maintain a linear causal structure, in which it is possible to recognise the effect of any given cause. In particular, he insisted on a causal interpretation of the instability principle. On the one hand, its role is necessary in the explanation of the cycle: fluctuations are made possible by the existence of forces offsetting the tendency to bring the system back to a state of equilibrium. On the other hand, instability was interpreted in terms of a field of centrifugal forces [...] causing the system to depart further and further from the required line of advance’ (Harrod 1948, p. 86, emphasis mine). In Harrod’s view, the instability principle is therefore characterized, both in logical and in phenomenological terms, by a causal interpretation of the economic movement as resulting from the inducements or deterrents operated by a set of independent forces.

A second difference worth stressing, between Harrod’s notion of dynamics and that of the econometricians, originates from Harrod’s postulate that the factors determining the level of output must be ascertained before attempting to ascertain those inducing variations in its amount. Harrod laid out this methodological principle in 1925 (Harrod 1925*), and repeated it in The Trade Cycle; the structure of the book itself reflects this position, the first two chapters being devoted to an understanding of the determinants of the level and the rate of change of output: statics and dynamics, respectively.

Later – probably due to some intrinsic analytical weakness in the first part (see Besomi 1997) – Harrod skipped the discussion of the static determinants to concentrate on dynamics. Such division was made possible by his approach in terms of forces, but it must be noted that it is by no means implied by it. It would be possible to tackle both problems at the same time, and this is indeed what the econometricians did. Their systems of functional equations were formulated in terms of absolute magnitude of the variables and of some of their momenta (derivatives and/or lagged values), and their solution represented the time path of one of the variables, from which the explicit magnitude and momenta of the other variables at any point of time may easily be derived. This came at the price, however, of renouncing explicit interpretation of the forces as causes, to be substituted by the notion of functional dependence. Although Harrod’s premise was not logically necessary, it is easy to see that it stems from a common sense distinction between what is primitive (the volume of output) and what is derivative (its mode of change). But this distinction, which led to Harrod’s notions of statics and dynamics as relating respectively to the level and the rate of growth of output, is precisely the same distinction upon which his epistemic principle is founded: that if output changes there must be something in the determinants of its level which allows it to fluctuate.

Two of the partial definitions of dynamics Harrod offered spring directly from the key distinction between statics and dynamics which he based on the forces affecting the level and the variations of output. In the first place, Harrod maintained that statics relates to systems in which there are no savings, while the dynamics of the economy depend on saving and investment. This definition is therefore based on a specific feature of the object of static and dynamic analysis. This property is obviously a corollary of the fact that Harrod’s apparatus is based on the interaction of a mechanism linking the additional income to the new net investment and a device connecting investment to the realisation (or the perspective, in the earlier version) of an increase of income: in the absence of investment (which is, by definition, equal to saving), the mechanism is not set in motion, and the system neither grows nor fluctuates, but simply adapts to the level of output consistent with the static determinants.

Secondly, Harrod proposed a notion of dynamics as a ‘system of fundamental equations’ formulating ‘the laws governing increase [...] as precisely as the static laws’, stressing the necessity that in these equations ‘rates of increase will themselves figur[e] as unknown terms’
Harrod and the ‘Time-lag Theories of the Cycle’

What Mr. Harrod seems to be doing, if one comes to his theory from that side, is simply to be elaborating one of the cases which, on the other line, had been passed by and rejected in favour of a more interesting alternative (Hicks 1950, p. 7).

10 TWO NOTIONS OF DYNAMICS

The upshot of the preceding discussion is that Harrod claimed his approach to be more fundamental than the ‘time-lag theories of the cycle’, on the grounds of an impressive array of arguments. In the first place, he maintained that the ‘right kind of explanation’ of the cycle must be based on endogenous, destabilizing factors permitting movement and fluctuations to occur as cumulative departures from stationary moving equilibrium respectively, while he interpreted lags of adjustment as equivalent to ‘errors and miscalculations’ which can only provide ad hoc causes of the phenomenon. Secondly, Harrod claimed that dynamics must be developed in specific stages: the first is a simultaneous conspectus of the mutual relationships between the variables at equilibrium, the second is an analysis of the succession of events when equilibrium is abandoned, and the third is the application of the analysis to policy. From this he concluded that instantaneous analysis has both logical and chronological priority over the analysis of lags, which can fruitfully be grafted onto the second stage. Thirdly, he remarked that in equilibrium growth lags would be irrelevant, since only the proportions between magnitudes matter. Fourthly, not being able to deal with variations in his coefficients, Harrod thought it appropriate to confine the analysis to a single instant, during which the coefficients can be taken to be constant. Finally, Harrod seemed to be concerned with a causal interpretation of the cycle and growth mechanism, while in the approach of the econometricians causality lost much of its meaning.

Having ascertained that these arguments belonged to a consistent (though evolving) view of the epistemic aspect, the analytical instruments, the scope and the method of economic dynamics, one may ask whether these arguments were correct, both with respect to what Harrod himself was actually doing (as distinct from what he thought he was doing), and with respect to what the econometricians were doing (as distinct from what Harrod thought them to be doing). Historical analysis may help to formulate some tentative replies to these questions.
It is firstly worth mentioning that Harrod’s understanding of the econometricians’ approach in terms of functional equations was far from satisfactory. This was undoubtedly due to his lack of an appropriate mathematical training, as he himself recognised, and led to unfortunate consequences. Harrod, in fact, could not appreciate how the econometricians perceived his criticism of their position, and consequently his attack often missed the point and failed to attract the attention he was hoping for. An early example of this can be drawn from Harrod’s reaction to Marschak’s comment on the preliminary draft of the Essay: Harrod was not able to appreciate that he could not convince the econometricians on the grounds of such an argument, and left the passage untouched in the final version of the article:

I entirely agree with you that dynamics is not identical with Theory of Lags. It is sufficient to have velocities, accelerations etc. and no finite lags, to have dynamics. By definition, velocity involves the comparison between two points of time but they may be as near each other as we please. It seems to me therefore that p. 7 para 2 [Harrod 1996, § 4, and Harrod 1939, p. 17; the passage is cited in section 9 above] is a little too polemic: ‘those who define dynamic as having a cross reference to two points of time’ do not necessarily require lags and will certainly agree that your equation (1) is dynamic involving as it does a rate of growth (a velocity). (Marschak 1938*)

Moreover, Harrod’s comments on models and on the ‘lags of adjustment’ suggest that at times he interpreted functional equation models as relating to the estimation of parameters, and he confused the results with cobweb-like mechanisms which were certainly not what Tinbergen and especially Kalecki had in mind when they formulated their first trade cycle theories.

Secondly, while Harrod included the analysis of the stability of equilibrium within the scope of his first stage, it is quite clear that the study of the stability of the system requires both a comparison between different states of the system and the formulation of some assumption regarding an adaptation mechanism (see e.g. Medio 1979, p. 21). Of course, Harrod knew beforehand that his system was unstable, because the appropriate destabiliser was introduced at the outset in the form of the accelerator, which amplifies deviations by turning rates of increase into absolute increases. But when he was taken to task by Keynes (who insisted that if the period of time is sufficiently long the change in the coefficients might upset Harrod’s result), Harrod had to resort to a comparison between different states in order to prove the instability (see the correspondence published in Keynes 1973, in particular pp. 333–350). Commentators noticed that Harrod was introducing a lag at this point (see e.g. Shackle 1967, p. 261): their criticism of inconsistency here is justified, and cannot be confused with the comments on Harrod’s use of lags in his second stage. More generally, however, it must be pointed out that Harrod’s mechanism refers by its own nature to the ‘succession of events’, because new net investment determines an increase of income in the proportion defined by the multiplier, and this in turn determines a further change in the flow of investment. In The Trade Cycle, in fact, having remarked how dangerous it can be to refer to time-lags in the theory of the cycle (the passage was cited in footnote 23 above), Harrod explicitly emphasized that the fact that net investment is undertaken with a view to facilitating production in the future is clearly a central one; and the interval that elapses between placing an order for, or beginning to undertake the construction of, capital goods and their use in the productive process can hardly be neglected (Harrod 1936, p. 88).

In his book, Harrod was not inconsistent with his methodological tenet and studied equilibrium as a simultaneous conspectus; but ‘simultaneity’ was intended in a logical sense, not with reference to a time-instant. In the Essay, on the contrary, Harrod reverted to a chronological interpretation, which reflected his analytical difficulties with changing coefficients. It is not surprising, therefore, to find that his later texts are strewn with terminological ambiguities as to whether equilibrium refers to a point or to a ‘line’ or as to the length of the period, sometimes taken to be an instant and sometimes referred to as ‘very short’ (about six months: see e.g. Harrod 1970, p. 741).

Thirdly, the revolutionary nature Harrod claimed for his dynamic approach is obscured by the explicit attempt to extend to dynamics the same method of analysis developed for solving the static problem of the determination of the level of income (see, for a discussion, Besomi 1997a). Some modification had, of course, to be introduced: in particular, Harrod emphasized that while statics can only deal with one-off changes in the fundamental conditions (the static determinants: resilience of prime costs, elasticity of demand, diminishing returns), in
dynamics the conditions continuously change (see Harrod 1936, pp. 165–167; 1938, p. 404; 1939, p. 15; 1948, pp. 7–8 and passim; 1957, p. 193; 1973, p. 3). However, in Harrod’s dynamics, as in traditional statics, equilibrium played an essential role. This was indeed different from the traditional one, because of the instability principle which broke the continuity with orthodoxy. However, the econometricians could also claim a radical departure from the tradition, because in their conception equilibrium is nothing other than a degenerate kind of motion, whose evolution does not give rise to an impetus to change the present state of the system (Frisch 1936, pp. 101–102).

Finally, in spite of Harrod’s causal interpretation of the relationships between the level of income and saving described by his axioms, and between the rate of increase of income and investment, the corresponding equations worked in ex post terms only in Harrod’s mechanism. The divergence between actual and warranted rates was in fact defined as equivalent to the undesired accumulation of stocks on which the argument of instability was based, and cannot be interpreted as its cause (for a discussion see Besomi 1995, pp. 322 and 335–336). There is therefore a difference between what Harrod’s analytical mechanism actually described, and what he wanted it to describe.

Harrod’s approach is not characterized only by these inconsistencies, misunderstandings and relative weaknesses, but also by some strong points in his favour. In the first place, Frisch’s definition coupled a statement on the purpose of a dynamic theory (to explain ‘how one situation grows out of the foregoing’) with an assertion that systems of functional equations provide the only language adequate to pursue this scope. Frisch’s definition was thus developed not with reference to the properties of dynamic systems, but in analogy with the language applied by physicists to the movement of bodies. This, of course, entailed a specific characterization of the object of analysis: in the first place, the variables are expressed as functions of time, but a time which is not the time we experience but an abstract dimension. Secondarily, the destiny of the system is entirely entailed by the formulation of the equations and in one of its states (the ‘initial condition’), so that the motion described by this language does not imply any real change: it can be summarized as a ‘changeless change [...] in a timeless time’, to use Alexandre Koyré’s expressive aphorism (Koyré 1965, p. 11). Harrod, on the contrary, defined his notion of dynamics in terms of a specific feature of the real economy, which he wanted his theory to express. For this reason, given his general notion of equilibrium as a balance of forces, he began by laying out the conditions for possibility of movement and of the cycle, and adapted his whole repertoire of conceptual tools to this primary need. If, in terms of production of models, Harrod’s approach was less fruitful than that of the econometricians, the mechanism he proposed was constructed at the outset as a theory, while most of the ‘time-lag theories of the cycle’ were nothing more than clever toys, which could be turned into theories only after a heuristic framework was forced upon them.

The epistemic idea that there must be some kind of instability at the root of the cycle strikes me as extremely interesting in itself, and as such has indeed been later rediscovered by those followers of the econometricians who explored the implications of non-linearities. I find extremely fascinating the fact that Harrod’s pet idea of the instability principle tends to reappear in the simplest non-linear cyclical models, whose phase diagram represents convergence to a limit cycle (or a strange attractor) surrounding an unstable steady point. This provokes the conjecture (which I leave to the experts of non-linear dynamics to verify or to disprove) that Harrod’s intuitive postulate may indeed be correct: that unless we are satisfied with exogenous random shocks to keep damped fluctuations alive, a persistent cyclical behaviour of the system requires two conditions. On the one hand, it is necessary that a destabilizing factor is at work, preventing the system from sinking into a state of stable equilibrium (whether stationary or moving). On the other hand, there must exist some other factor (whether endogenous, like the changes in the coefficients as growth proceeds, or exogenous, like the ceiling of full employment of labour (for a discussion of these factors in Harrod’s analysis see Pugno 1992, p. 122–125) preventing the system from exploding.

Notes

1. Formulated in 1933 and refined in 1936, Frisch’s definition was first adopted by the ‘econometricians’ and provided the background for the models published in *Econometrica* and elsewhere by authors such as Tinbergen and Kalecki. By 1939 Schumpeter bowed the knee to Frisch’s definition, which was finally glorified by Samuelson’s *Foundations*. Frisch’s definition is discussed in Section 9 below.
we are conducted through a severely rational and frictionless world in which nobody ever loses his head or makes mistakes or finds himself in a bottle-neck, in which the capitalist producer is at every moment successfully striving to equate an ascertainable marginal revenue with an estimable marginal cost.... At times we are impelled to cry 'But this is not so! Such assumptions are in place in other connections, but not in the hugger-mugger of the trade cycle.'... Nevertheless the hugger-mugger of fluctuations is no excuse for not attempting to analyse it; and so strong is my own sympathy with Mr. Harrod's desire to show that it is not wholly a matter of error or minor frictions that I am willing to let him lead me by the hand (Robertson 1937, p. 124).

11. It must be noted, however, that in *The Trade Cycle* and in his subsequent writings Harrod carefully stressed that the non-linearities of the dynamic mechanism itself are sufficient to account for the original deviation from equilibrium which the instability principle successively amplifies: growth of income, independently of whether or not it occurs at an equilibrium rate, induces an increase in the propensity to save, thereby altering the equilibrium rate itself; a deviation is bound to occur, sooner or later.

12. In his response to Tinbergen's review of *The Trade Cycle*, Harrod explained: 'My idea is that my curve gives the basic fact of the cycle on which various lags must be superimposed' (Harrod to Tinbergen, 1 July, 1937).

13. Pointing out that he was not considering the transition from a state of stationariness to one of advance but that he was examining the conditions pertaining to a period of regular advance, Harrod observed that:

The difference between the two sets of problems is analogous to the difference between the dynamics of getting a train to move and the dynamics of a train in motion at a constant velocity. I was concerned to investigate the latter problem, and for that purpose it is proper to take a cross-sectional view, assuming that the immediately preceding and succeeding periods yield similar developments, and to find out what assumptions with regard to the increase and mutual relations of the factors concerned are self-consistent and consistent with normal economic motives (Harrod 1934b, p. 478).

14. The MS (Harrod 1940/41*) bears no title and is undated, but on 11 November, 1942, Harrod offered it to Macmillan explaining it was written in 1940/41, after the long working hours in a Government office. Harrod described the state of the work as follows: 'It is less than half done and what is done will need drastic revision. It would probably take me a year of vacations after the war.' The proposed title was *The Known and the Unknown*. Until 1944, no further work was done on the book (Harrod to Macmillan, 16 October, 1944), while in 1951 Harrod could write: '...The Known & Unknown...is taking shape in my mind. I think it might have a reasonable sale among thinking people; but it can hardly appear for 3 or 4 years' (Harrod to Macmillan, 26 March, 1951). In Besomi (1996), footnote 7, p. 288, 1
suggested that the manuscript was written in 1938, because of the similarities with the terminology of Harrod (1938). I have only found the correspondence with Macmillan at a later stage. Only 14 chapters survive, three of which are in both manuscript and typescript form, while of others more than one draft was preserved; some bear no title, some are not numbered. From a fragmentary index left by Harrod and from the chapters found, the index of the book may be partially reconstructed as follows:

**Book I:**
- Ch. 2: The external world (TS)
- Ch. 4: Materialism (TS)
- Ch. 5: Knowledge (1st, 2nd and 3rd versions); including a loose leaf on mathematical knowledge
- [Ch. 6?]: Mathematical knowledge
- Ch. 8: Other selves (TS Ch. 5)
- Ch. 9: Political theory
(There is a reference to a chapter on Memory).

**Book II:**
- Ch. 4: [Perfection]
- Ch. 6: Pleasure
- Ch. 7: Goodness
- Ch. < + >: Beauty

**Book III**
- Ch. 1: [The existence of the conscious being]
- Ch. 2: God (2 versions)
- Ch. 3: [Dialogue between the author and a Theologian]
- Ch. 4: [Infinity].

15. The cartographic analogy is likely to have been inspired by F. A. Lindemann (physicist and member of Christ Church), who used it himself to expose his view as to how quantum physics suggests to reformulate the description of the world in terms of a system of co-ordinates different from that provided by conventional co-ordinates (mass, length and time). Lindemann expressed his metaphor of the physicist as a cartographer in the following terms:

The physicist [...] assumes, and nobody can do more than assume it, that there is something external to himself, an external world between which and his sense experience there is some relation. His aim is to make some picture, model or map of the external universe. [...] Now it is evident that whatever map the physicist makes will never be complete. No map can ever be. If it were it would cease to be a map and simply be a duplicate of the whole external world. The idea of a map, picture or model implies a certain selection. It only pretends to represent certain aspects. But between these aspects and our sense experiences we are entitled to demand a one-to-one relation (Lindemann 1936, p. 817).

An offprint of the article from which this passage is cited is collected among Harrod's papers (HP V-121). It is likely that Harrod and Lindemann discussed this, as other topics concerning

the physics of quanta, in their frequent nightly meeting that Harrod described in his biography of 'the Prof.' (Harrod 1959a).

16. Harrod shared the viewpoint of logical positivists, that 'A valid generalization is either a tautology or based on empirical evidence' (Harrod to Robertson, 15 November, 1935). Accordingly, he saw theories as deductive systems, and tried to base his theory on a set of axioms to be derived by empirical generalisation or from a definitional process. The 1938 Presidential Address was meant to explore the peculiar interplay between induction and deduction: 'anything I might have had to say about recent monetary literature has been squeezed out by deduction, induction etc.' (Harrod to Robertson, 5 July, 1938). This eventually led to Harrod's 1956 contribution on induction, of which he later wrote: 'Of my writings, the one that I believe to be the best is *The Foundations of Inductive Logic*, but its tenets are grossly out of fashion. It could one day gain recognition, but it may not do so' (Harrod 1966*).

17. See also Harrod (1938), p. 390: 'in fully developed sciences laws of causation [i.e., concerning the succession of events] have primacy of position'.

18. The idea of the empirical determination of the parameters was discussed at length with Keynes, also in connection with Tinbergen's procedure (see Section 1 above).

19. Harrod forcefully stressed the analogy between the procedures of statics and dynamics on several occasions: see e.g. Harrod (1938a), pp. 403–4, and (1939a), p. 14. For a discussion of the implications of this for the relationship between Harrod's dynamics and traditional theory see Besomi (1979a).

20. Two of these stages, however, were clearly recognised by Kregel (1980), pp. 114 and passim. For a comparison of the two versions, see Besomi (1996) and (1996a).


22. Harrod specified that lags are 'inseparable from a system in which decisions have to be based on imperfect data and to some extent therefore on trial and error' (Harrod 1948, p. 117).

23. Shortly after the 'Essay' was published, Harrod still thought it possible to proceed to the second stage of dynamics dealing with the sequence of events without making recourse to time lags. In fact, in May 1939, he announced to the members of the Oxford Economists' Research Group that

he was hoping to make a statement of his theory of the Trade Cycle in a form which could be statistically tested. This would take the form of a model sequence of events. Such a construction had been the basis of Tinbergen's research, but whereas this is based on the assumption of lags, Mr. Harrod thinks that it is not necessary to introduce the concept of lags. He will therefore submit a scheme of investigation based upon such a model sequence (Oxford Economists' Research Group 1939*).

24. This point of view echoes a much older statement of Harrod's in *The Trade Cycle*, where he expressed the opinion that
All references to time-intervals in this topic are highly dangerous; it is so easy to give plausible explanations on the basis of a time-lags hypothesis; the hypotheses that may be introduced are so many and various that with their aid the facts can be made to fit almost any theory; it is extremely difficult to demonstrate that one hypothesis is more probable than another (Harrod 1936, p. 88).

He showed it himself by means of a specific example, which led him to stress how troublesome this country of time-lags is. Alternative hypotheses might easily have been framed. Their very multiplicity suggests that we cannot hope much of the a priori method in this field. Luckily the main part of our theory of the cycle is not beset with such difficulties (Harrod 1936, p. 138).

He concluded that

Hypothesis is no substitute for investigation. The suggestions [advanced in the example mentioned above] are of an interim and tentative nature. In this respect their status is entirely different from the proposition about the cycle which derive their cogency from the nature of the [accelerator], the multiplier, and the determinants (Harrod 1936, p. 128).

25. For a survey of the interpretations of Harrod’s dynamics and Harrod’s own reactions, see Besomi (1998). Harrod’s objection to attempts to interpret his theory and to discuss the stability of equilibrium in terms of functional equation models is well represented by the following statement, in which he stressed that his theory must not be confused with the time-lags approach:

my trade-cycle theory – if it can be dignified with that name – was not of the same genus as those theories which econometricians were developing with great vigour at that time, and which, I doubt not, must be a part of the full and final theory of growth; these essentially depended on systematic lags between variables in a model which generated an oscillation, explosive or damped. There was danger that my instability principle might become confused with such theories, to which it really has no direct relation (Harrod 1959, p. 459).

26. This distinction must not, of course, be confused with the linearity or non-linearity of the equations: in the XIX century the equations of the Hamiltonian were all linear, but the causality of the system was circular because everything contributed to the determination of everything else.

27. Throughout his book, Harrod treated as equivalent the terms ‘determinant’, ‘cause’ and ‘force’. Indeed, in the vast majority of occurrences the term force was associated with the term cause (or effect) or with verbs such as to determine, to ordain, to prescribe, to govern or similar. A couple of examples will illustrate the case: ‘...the kind of forces that...cause the level of activity to be what it is’ (1936, p. 1); ‘the rate of interest must so move as to provide a force which, when operating conjointly with the forces exerted by the propensity to save, the elasticity of the demand for goods, and inventions, causes the three dynamic determinants to justify a continuance of the advance’ (ibid., p. 116).

28. ‘The most fundamental feature of the trade cycle is fluctuation of activity and output. As a preliminary to seeking an explanation of this fluctuation it seems natural to ask the question – what are the circumstances which we should expect to govern the level of activity? Various hypotheses have been put forward with regard to the causes of fluctuation; examination of these cannot be effectively undertaken unless in the first instance our ideas are clear as to the kind of forces that, whether there be fluctuations or not, cause the level of activity to be what it is’ (Harrod 1936, p. 1).

29. For instance he wrote to Robertson: ‘Not that I like mathematical formulations particularly, and I am unable to understand a lot of the detail in Bowley, so ill-educated am I’ (25 December, 1936).

30. There exist, of course, relevant exceptions, among which I would like to mention Kalecki and Hicks.

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Besides the published sources listed above, the following unpublished material has been referred to (HP indicates items belonging to the Harrod Papers, Chiba University of Commerce, Ichikawa, Japan; DHR indicates the papers of Dennis H. Robertson, Wren Library, Trinity College, Cambridge; JTP indicates the Tinbergen Papers, Erasmus University, Rotterdam; LoN indicates the League of Nations Archives, Geneva; HPB indicates the Harrod Papers at the British Library, London; AP the Andrews-Brunner Papers, British Library of Economics and Political Science, London).

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