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A. Unpublished sources
   - Abbreviations
      - A.1. Correspondence with Harrod
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Preface

The research presented in these pages was initially meant as a part of a more general project aimed at examining the origins, characteristics and development of ‘modern’ economic dynamics. When I came across Harrod’s notion of dynamics, it immediately struck me as very odd with respect to Ragnar Frisch’s definition, which was then gaining acceptance, and quite mysterious in its own right. I felt therefore that it was necessary to understand the roots of Harrod’s ideas, in order to appreciate the extent of the departure but also of the affinities between Harrod and the econometricians.

However, plunging into the original texts soon made obvious that the subject was much more intricate than the pictures of Harrod’s dynamics given in textbook renditions, and also much more intriguing than the technicalities on the cycle and growth models which filled the learned Journals in particular during the 1950s and the 1960s. Harrod seemed to be trying to say something else, and was indeed very polemical against the rival approaches to dynamics.

But the symbolism by which he expressed his ideas was not clear-cut, and it made it difficult to grasp his specific point. Harrod, however, returned several times on the topic of his essays in correspondence with Keynes, who sometimes managed to force him to re-formulate his propositions. Studying this correspondence thus proved very useful, and suggested that a careful examination of Harrod’s unpublished papers and correspondence could give deeper insights regarding the development of his ideas. And indeed it did, and it also revealed a hidden world of activities which played a relevant part in the making of his dynamics, although only the tip of these rarely emerged in rather obscure footnotes or seemingly marginal comments.

Working on these unpublished documents has been like solving a jigsaw puzzle, beginning with only vague clues as to the picture which had to emerge at the end. The portrait which eventually surfaced is surely quite surprising, and much more complicated than I could have expected at the beginning of my research. It is composed of a multitude of details that admirably fit together. It reveals that Harrod’s project of laying the foundations of a dynamic economics had its roots in the whole corpus of his multifarious interests, and that it was articulated around his reiterated attempts to give an answer to specific methodological and epistemic problems in trade cycle theory. All these attempts deeply plunged into the context of the lively debates characterising the early 1930s, but none of them were fully successful. This portrait is now presented here, in the hope of contributing to the understanding of Harrod’s thought as one of the original results of an epoch of intellectual turmoil in economics.
1. Harrod’s Dynamics and its Canonical interpretations

Another book on Harrod’s dynamics! I offer this, well knowing that whoever ventures to add to the multifarious literature on this subject must incur the reader’s extreme displeasure, if he fails to show that he has something of importance to add.

In spite of the abundance of the early and recent literature on the subject, there is an aspect of Harrod’s dynamic theory which never seems to have been given the importance Harrod desired: his notion of dynamics in opposition to other conceptions, in particular that developed by the econometricians. Harrod in fact meant dynamics to be concerned with growth rates at a given point of time, and fiercely opposed the ‘time-lags theories of the cycle’ (among which he included Robertson’s ‘period analysis’, Lundberg’s ‘sequence analysis’, Hicks’s notion of dynamics as concerning dated variables, and especially Tinbergen’s approach) as being ‘clearly less fundamental’ than his own approach. In spite of Harrod’s insistence on this point, his contribution to dynamics was only appreciated for specific details of his construction, i.e. namely, the multiplier-accelerator mechanism and the instability principle. Whereas, Ragnar Frisch’s definition of dynamics as referring to variables evaluated at different points of time took over and was largely accepted by the community of students of the cycle and economic movement.

Harrod’s notion of dynamic theory thus met with the worst destiny of all. Not only it was not understood or appreciated by his fellows economists, who simply ignored its peculiarity, but the mechanism he proposed was soon engulfed by rival conceptions. A first line of interpretation leading to this result was initiated by Tinbergen. In his review of *The Trade Cycle*, he reformulated Harrod’s model in terms of a first order differential equation, and concluded that such a combination of multiplier and accelerator could not give rise to cyclical behaviour, but only to explosive growth or to a tendency towards equilibrium (Tinbergen 1937). In 1938, having been sent the first draft of Harrod’s “Essay in Dynamic Theory”, Marschak was also ready to restate the fundamental assumption and relationships by means of the language of differential equations (Marschak 1938*).

A few months later, Samuelson recognised Harrod as the progenitor of models based on the interaction between the multiplier and the accelerator. Samuelson discussed them in terms of a linear difference equation of order two (thus substituting Tinbergen’s derivative with two lags), and indicated the regions of the parameter space (multiplier and acceleration coefficients) giving rise respectively to cyclical behaviour of the system
(which may be damped, regular or explosive), to an asymptotic approach to equilibrium, or to a cumulative escape from equilibrium. Samuelson also discussed, in qualitative terms, the consequences of the abandonment of the assumption of linearity, thereby giving consideration to the action of Harrod’s dynamic determinants (Samuelson 1939).

After the war, the intrinsic non-linearity of Harrod’s treatment was finally recognised and dealt with explicitly by Goodwin, who blamed Tinbergen for having limited his criticism to a simple linear equation and concluded that a non-linear version of Harrod’s multiplier-accelerator model could give rise to persistent cycles (Goodwin 1951). Goodwin’s article gave rise to the interpretation that Harrod’s dynamic theory was the first shot at non-linear analysis of business cycles. This view eventually stimulated further attempts to account for more and more details of Harrod’s original treatment in terms of functional equations (see e.g. Medio 1979, Glombowski and Krüger 1982).

These studies enabled a re-formulation of the model which gave rise to a more precise and clear expression to some of the details of the mechanism postulated by Harrod. They also permitted an exploration of some unexpected and far-reaching implications of Harrod’s model, and led to the identification of some of the frailties of the original formulation. On the other hand, the interpretation of Harrod’s ideas in the terms dear to the econometricians required to restate them as functional equations. According to Frisch, in fact, a model is dynamic if it can be represented as a system of equations relating to different points of time and permitting to describe the development of the system as a succession of its states (Frisch 1933, pp. 171-172; 1936, pp. 100-102; see Samuelson 1947, p. 314). But the translation into a different language attributed to Harrod’s theory features it did not originally possess, and also missed out other peculiarities which, in Harrod’s view, characterised his own approach. In particular, the econometricians’ versions turned Harrod’s instantaneous determination of the rate of growth into models describing how ‘one situation grows out of the foregoing’.

Of course, from the econometricians’ viewpoint this did not constitute a limit of the translation into the formal language. In fact, as Marschak was quick to point out, Harrod’s approach would be considered dynamic even according to Frisch’s definition, for the rates of growth of Harrod’s concern would involve

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 [of Harrod 1938*; see Harrod 1939a, p. 15] is a little too polemic: “those who define dynamics as having a cross reference to two points of time” do not necessarily require lags and will certainly agree with you that your equation (1) is dynamic involving as it does a rate of growth (a velocity) (Marschak 1938*, heading 1.).

Moreover, the econometricians were certainly aware that the explicit solutions of their dynamic equations -which consist of functions relating a variable to time and to some of the parameters of the equation- certainly enabled the determination of the rate of growth of the variable under consideration. It is therefore not surprising that they could see no
obstacle, but only advantages, in re-stating Harrod’s views in the terms they found more familiar and more general.

A second line of interpretation of the ideas expressed in Harrod’s “Essay” led commentators to elect this article as the founder of the family of growth theories, in the form of the ‘Harrod-Domar model’. Harrod’s ‘fundamental equation’, describing the instantaneous equilibrium rate of growth possibly resulting from the joint effect of the individual acts of saving and investment, was understood as determining (or even prescribing) the conditions for the attainment of an equilibrium growth path.

The debates which arose around this line of interpretation either aimed at attacking Harrod’s conclusions (e.g. as to the instability of the equilibrium rate, or as to the uniqueness of equilibrium) by contesting the realism or the rigidity of his assumptions, or they aimed at identifying different determinants of economic development. On the one hand, Solow accused Harrod of having assumed a constant capital/output ratio, and after having removed this limiting assumption and having restated the function of production in its proper (neo-classical) place, Solow concluded that the system tended to its full employment position (Solow 1956, pp. 65-66). On the opposite (post-Keynesian) front, refined assumptions regarding the propensities to save of different social classes were considered. This led to the conclusion that the possibility of equilibrium also depended on the distribution of income, different states of which could thus give rise to a multiplicity of equilibria (see e.g. Robinson’s 1970 summing up of twenty-one years of debates springing from Harrod’s theory, and Harrod’s 1970 comment on it). On the other hand, we had inquiries aiming at recognising and proposing solutions to the developmental problems of contemporary capitalism, on the grounds of a consideration of a function of technical progress and the specification of the determinants of investment decisions (see e.g. the essays collected in the third part of Kaldor 1960a).

The various interpretations of Harrod’s ideas in terms of a growth model stimulated interesting theoretical developments and thereby accrued the fame of his “Essay”, but ignored Harrod’s careful distinction as to the different stages a science of economic dynamics should go through. The first stage was the determination of the equilibrium rate of growth in a single instant. The second stage, which led to trade cycle theory, was the examination of the possibility (and, indeed, the necessity) of variations in the ‘fundamental conditions’ (the fraction of income saved, and the ratio of investment per unit increase of output). In the light of this distinction, both the interpretations of the fundamental equation as describing a growth path, and the charge of having assumed constant parameters, clearly show their limits. On the one hand, the “Essay” was not meant to provide a ‘model’ of economic growth; on the other hand, the fact it was treated jointly with Domar’s equation shows that interpreters were more keen to accept (or refute) Harrod’s and Domar’s common arrival point rather than to examine closely the different analytical, methodological and epistemic premises from which they took the move.
This bird’s eye overview of non-linear dynamics and growth theory, two of the most notable analytical developments inspired (partly at least) by Harrod’s 1939 ideas, reveals on the one hand that these have been extremely fertile, in the sense that they stimulated reflections on different domains of economic knowledge and gave rise to developments Harrod could not even imagine at the time of writing his “Essay”. This justifies the fame Harrod acquired as an original and powerful mind, and the place he conquered at the right of Keynes in the history of economics (Schumpeter 1946, p. 509). But on the other hand, both the early and recent literature on Harrod’s dynamics reveals that he failed to convince his fellow economists of the distinctive epistemic and methodological character of his own dynamic approach. All the analytical developments springing out of his original contribution were elaborated in a different spirit, and his results were considered from a rather different angle. In spite of its apparent success, Harrod’s dynamic research programme must therefore be studied as the story of a failure, for he did not succeed in convincing students of dynamics that his own approach was the rational procedure to tackle the problem under examination.

Harrod himself insisted since the early formulations that the pertinence of his method was the issue at stakes:

The significance of what follows should not be judged solely by reference to the validity or convenience of the particular equations set forth. It involves something wider, a method of thinking, a way of approach to certain problems. It is necessary to ‘think dynamically’. [...] A new method of approach, indeed a mental revolution, is needed (Harrod 1939a, p. 15 and 1938*, § 3. See also 1948, Chapter 1 and p. 80; 1953, pp. 53 and 55; 1959a, p. 151; 1973, p. 2).

This aspect, however, passed completely unnoticed, and Harrod’s readers concentrated instead on the validity of his results.

2. The premises and early development of Harrod’s dynamics

There were of course several reasons for ignoring this remark. In particular, the alternative approach suggested by Frisch proved to work well, and to give rise to interesting models of the cycle and growth. After the war, it also enabled the incorporation of Harrod’s assumptions and equations. In these conditions, scholars must have considered Harrod’s complaints that the ‘lag theories of the cycles’ are ‘clearly less fundamental’ than his own approach as an oddity which did not deserve much attention.

A second reason why Harrod’s readers ignored his claims lies in the fact that Harrod never made it clear and explicit what he actually meant. In his most complete writing on dynamics, i.e. the 1936 book on *The Trade Cycle*, an explicit definition of dynamics is lacking, in spite of the fact that this is the only one of his writings in which he actually dealt at length with the problem of the connections between statics and dynamics. In the subsequent versions he often provided definitions, but he emphasised at times one or another aspect of his notion, stressing for instance the object of the discipline, or its method, or some analytical property.
The aim of the present study is to contribute to the understanding of Harrod’s notion of dynamics, by inquiring on its origin and early developments. In particular, I aim to disentangle different problems and perspectives which jointly characterised Harrod’s approach but operated at different levels of his reasoning. To denote them, I will use the adjectives ‘epistemic’, ‘methodological’, ‘analytical’ and ‘ontological’ in a somewhat loose sense, to point out that the problems to be faced spanned 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. Harrod’s notion of ‘dynamics’ involved not only the characterisation of the features of a dynamic economy, but also (and primarily) specific reflections on the conceptual instruments, on the relationship between the theory and the object of research, and on the relationship between scientist, theory and object. I am not trying to suggest that Harrod himself operated such distinction between the different levels of his approach to dynamics, but only that it is useful, in trying to understand the making and the significance of his dynamics, to separate different sorts of considerations otherwise juxtaposed in Harrod’s own presentation. It seems to me, in fact, that Harrod’s dynamic thought was organised, both logically and chronologically, around an epistemic premise, stating that a proper theorisation of economic movement requires abandoning the assumption of stability of equilibrium, and introducing some de-stabilising factor in the model at the outset. Only after having devised this principle in 1934, Harrod had a framework enabling him to join the pieces he was assembling into a coherent whole. This framework, in turn, implied quite a radical revision of his former approach to the subject.

I have thus chosen to examine how these different levels of Harrod’s theorisation originated, how they affected each other and mingled together at the epoch of the birth and early evolution of his notion of dynamics. This decision was determined by the fact that the birth of a concept is of course the most interesting event for the historian of thought. Birth is, in fact, the true moment of the break, while later re-interpretations are influenced by the measure of the success of the first formulation. Harrod’s case constitutes no exception. The premises of his dynamics were laid in the early thirties, but the subject and its ramifications almost fully occupied Harrod’s mind only in the second half of the decade. His reflection on dynamics was interrupted by the war, and was only resumed in 1946 (see Harrod 1948, p. v)9. By then, the approach of the econometricians had already firmly conquered the field10, and on this front Harrod was bound to fight a rearguard battle. Moreover, within a few years the economic situation favoured turning the attention from cycle theory, which was one of the favourite themes of the 1930s, to growth theory. Harrod thus found himself in the odd situation of seeing the importance of some of his leading ideas recognised, but framed within schemes alien to his own. His later interpretations and completion of his early contributions were thus influenced by its destiny and cannot be considered as a mere prosecution of the previous work. They are something else, which must be studied separately11.
On the other hand, the moment of the birth of Harrod’s dynamics occurred in the decade of most rapid, profound and intense changes of perspective in the history of the discipline, which constituted the variable environment with which Harrod’s ideas co-evolved. This provides an additional challenge, for the core of Harrod’s approach resulted in a further break with an already discontinuous milieu.

For both these reasons, the present study is only concerned with the early developmental stages of Harrod’s dynamics, the natural border line being the outbreak of war. Only contemporary sources are used, and references to Harrod’s post-war writings are rather exceptional.

The process of the making of Harrod’s dynamics is presented in its chronological development. This permits the identification of the various elements which concurred, as a constituent part or as a critical target, to the progressive building and refinement of Harrod’s conception. A brief summary of the main steps of this process will be provided in the next section, in order to help to illustrate two features of Harrod’s approach. On the one hand, Harrod’s notion of dynamics was the result of a series of unavoidable decisions at some crucial bifurcation points. These ranged from the general and abstract issue of understanding the problem under examination within a world view, to the decision of tackling certain problems rather than others, to the adoption of a general metaphor providing the key for the interpretation of the system under study, to the adoption of certain conceptual and analytical instruments rather than others, to the choice of the pertinent variables and relationships, and to the concrete questioning of distinguishing the significant factors from the noise. These decisions were not the only necessary and logical outcome of the previous steps of Harrod’s reasoning. Indeed, Harrod could well have operated different choices (and other people at about the same time, faced with analogous alternatives, actually opted for other paths), thereby producing a rather different theoretical development and correspondingly a different image of the dynamic economic system. This aspect is extremely important, for Harrod had the power, by deciding in favour of a particular conceptual toolbox, to shape the description of the object he was going to produce. Conceptual instruments are selective, for they induce an emphasis upon certain properties of the phenomena and a neglect of others. But on the other hand, once the decision had been taken all its consequences were to be evaluated against the knowledge he had of the object beforehand.

On the other hand, the chronological exposition also enables the appreciation of the fact that at all stages of the development of his ideas Harrod was extremely meticulous in emphasising similarities and points of departure between his position and traditional theory on the one hand, and on the other hand between his approach and other theoretical innovations. Harrod was aware that his dynamic theory constituted a break with respect to tradition, and that other attempts to depart from orthodoxy were bursting out. He thus found himself faced with the double problem of presenting the break as necessary, understandable and acceptable to his fellow economists, and of convincing
them that his own approach rather than its competitors provided the right line of attack to the dynamic problem.

The dilemma implicit in the first problem is quite subtle, for any attempted solution was bound to be a compromise between emphasising the continuity or the discontinuity with orthodoxy: continuity made access easier to those brought up within the tradition, while the break gave strength to Harrod’s claim as to the necessity of a new approach. The measure of the compromise depended on the reaction of the audience. In the early years, Harrod was extremely concerned with the conditions to be satisfied in order to allow communication between theoretical systems:

I want your [Kahn’s] and Maynard’s economics of full employment expressed in terms which are used in marginal analysis, so that it can be fitted in to the corpus of economic theory such as it is.

I think the equilibrium economists wrongly believe that they have demonstrated that the system tends to move, apart from rigidities, to a full employment position, and that you can only disabuse them of this and impel them to take up your doctrines if they are expressed in the same language as theirs (Harrod to Kahn, 17 Nov., 1934).

Analogously, when discussing Keynes’s attack on the orthodox theory of interest, Harrod insisted on the importance of exposing the subject in a way that could easily be understood and accepted. This scope can only be achieved by finding the right balance between familiar and unfamiliar arguments:

If the economists who read your book don’t take the essential points, the outlook is bad. There is a limit to what the human mind can assimilate. Most of the readers won’t have the time or opportunity or assimilative powers of grasping the points raised in subsequent controversies.

What is important for the initial understanding, which is so much to be desired, is that their minds should be strongly directed on to your essential points. You present them with the most attractive kind of red-herring, namely controversial attacks on certain authors and about subjects they know well. The mind likes to take refuge from the un-familiar (your views which you want to put across) with the familiar (what exactly did Marshall mean in such and such passage). And I don’t think you lead them pleasantly from the familiar to the unfamiliar, because I think your criticisms are much too cursory and, I am bound to say, unjust. ∴ I think the tactics suggested in your letter mistaken13 (Harrod to Keynes, 30 August 1935, in Keynes CW XIII, pp. 555-556).

Accordingly, in his first systematic analysis of dynamics -the 1936 book on The Trade Cycle- and in the related correspondence, Harrod gave full emphasis to the methodological continuity between statics (i.e. traditional analysis in a generalised form) and his dynamics, with the result that the novelty of his approach was not caught by commentators, not even by Keynes, as it appears from the tenor of the reviews14. Therefore, in his subsequent writings Harrod gave more prominence to the differentia specifica of his approach with respect to orthodoxy by stressing with additional strength the necessity of a new approach to face the dynamic problem. Nevertheless, he did not abstain from expressing his views on the appropriateness of the traditional toolbox and on the necessity to extend its use to dynamics.
This shift of emphasis, jointly with the prestige the econometricians’ approach had been able to win in the second half of the 1930s thanks to the works of Kalecki, Frisch and especially Tinbergen, made imperative for Harrod the problem of convincing the scientific community that his solution was the appropriate one. Harrod was thus led to intensify the battle against the notion of dynamics based on the ‘time-lags theories of the cycle’ which he had already engaged in *The Trade Cycle*, and to re-direct his attack from Robertson’s ‘period analysis’, which was the chief target of Harrod’s polemical remarks from 1934 to 1936, to the econometricians’ approach in terms of lagged functional equations. It is important to notice that the aim of Harrod’s controversy regarded the *rationality* of the two approaches, rather than the specific results they could lead to. Harrod recognised that the econometricians’ models could give rise to finely fitting representations of the trade fluctuations, but argued that the study of the forces determining growth must be logically prior to the consideration of lags. Lags only ought to be taken into account at a second stage of the argument, in analogy with the effect of frictions and maladjustments being superimposed to the ‘fundamental forces’ determining ‘velocity and acceleration’.

The present study is therefore concerned with the twofold dialectic which these considerations suggest to be at work. The making of Harrod’s dynamics will therefore be characterised as the search of an analytical framework capable of responding to two needs. On the methodological front, it provides the *element of continuity* with traditional theory, by giving expression to the postulate that the theory of economic movement should be founded on the determinant of the individual’s decisions regarding the level of output. On the epistemic front, it provides the *point of detachment* from traditional theory, by requiring to introduce at the outset a de-stabilising mechanism.

3. **Harrod’s dynamics in its making**

Before plunging into the promised summary of the main stages of development of Harrod’s ideas, I will briefly describe how the present study is organised. I have already discussed the reasons suggesting a chronological exposition of Harrod’s process of thought. However, I must now stress that while in Chapters II to VII I characterise in chronological order specific steps in the making of Harrod’s dynamics (the birth or acquisition of certain concepts, or debates around specific themes or notions) and discuss their analytical or methodological implications, the first and last Chapters represent important exceptions to this general procedure. In fact, they are dedicated to two aspects of one and the same *themata* which obsessed Harrod throughout the whole formative phase of his dynamics: viz., the relationship of his new approach to traditional theory. Besides the necessity, illustrated in the previous section, to expose his theory by means of a language accessible to his fellow economists brought up within that system of thought, there was an actual methodological affinity between the partial equilibrium approach and Harrod’s dynamic procedure.
Chapter I is thus dedicated in the first place to the understanding of Harrod’s methodological premise that the determination of the causes of variation of output (the dynamic problem) must be logically compatible with, and occur after, the determination of the causes of the level of output (the traditional problem, that Harrod identified with statics). In the second place, I will look at his subsequent decision leading to the epistemic premise that the logical compatibility must be achieved not by imposing exogenous changes to the static forces, but by allowing the dynamic forces, the ultimate cause of movement, to operate. None of these premises were logically necessary nor a consequence of a universal guiding methodological principle, but were the result of pre-analytical decisions, which could have been taken otherwise if only Harrod had a different overall view of the nature and purpose of economics, but which, once taken, channelled in a precise direction the further analytical development of his theory. The methodological principle found its expression in Harrod’s interest for imperfect competition, and eventually led him to work out the analytical instruments for generalising the traditional approach under less restrictive conditions regarding competition, which could be put at the foundation of dynamics.

While Chapter I is thus dedicated to the formulation of the premises, Chapter VIII provides an evaluation of their analytical consequences on Harrod’s dynamics. These two Chapters thus supply the logical thread linking the different stages of development of Harrod’s dynamics, a theme which remains in the background in the reconstruction of the chronology, precisely as the confrontation with orthodoxy provided the background of Harrod’s reasoning. Therefore in both cases it has been necessary to transcend the strict chronological order of exposition and to explore the entire field of Harrod’s theoretical contributions over the whole period under consideration. The reader who would like to have a general perspective on Harrod’s dynamics can read Chapter VIII before plunging into the details of the acquisition and refinement of Harrod’s analytical instruments.

These aspects are inquired instead in Chapters II to VII, where the evolution of the analytical and procedural construction of Harrod’s dynamics is examined in its chronological development, from 1931 to the publication of the “Essay” in 1939. Their character of both an exercise in analysis and one in chronology will be apparent as the reader goes through the book, and therefore does not deserve a detailed summary here. It is necessary to stress, however, that the making of Harrod’s dynamics is not a story of a cumulative piling up of concepts and analytical instruments inexorably leading to the formulation of a specific dynamic model. Rather it is the account of successive, sometimes contradictory, sometimes ingenious but some other time ingenuous, attempts at tinkering with the conceptual tools that Harrod could draw from the lumber-room of metaphors, analogies and other instruments of thought he was accumulating by absorbing (not always readily) from the debates currently going on.

It is perhaps necessary to warn the reader that the chronological reconstruction of the birth and early development of Harrod’s dynamics has been carried out with great
care for the details, and with an extensive use of quotations. This approach was suggested in the first place by the fact that the portrait of Harrod which emerges from this reconstruction will be so surprising to many readers, that some could be tempted not to believe it without ample evidence. For example, it is hard to swallow that in spite of the frequent contact Harrod had with Keynes and the Keynesian circle, and in spite of the fragments of Keynes’s theory which leaked in print, Harrod knew very little of what was going on, and actually failed to understand the principle of effective demand and its implications (in spite of a tutorial by Kahn) until he read the *General Theory* in proof. In the second place, besides referring to Harrod’s major publications, in this study I have heavily relied on unpublished materials (letters, essays, notes and memoranda) and on ‘minor’ publications, such as review articles or pieces written for the press; the reader may therefore appreciate that the relevant passages are quoted in full. Finally, the secondary literature on Harrod’s dynamics -including the textbook renditions- suggests that his writings soon shared the destiny of the classics of economics, which are frequently cited but rarely read\(^{17}\). It may therefore be helpful to some readers to see the the passages I am referring to. However, I am aware that the care for details might at times hide the wood behind the trees. To remedy this shortcoming, I have provided a guideline at the beginning of each Chapter. Moreover, in order not to disturb the flowing of the argument I have confined to endnotes some supplements of evidence, the discussion of apparent counterexamples and other remarks I thought opportune but not strictly essential. These can be skipped over without loosing the thread of the story, but I would advise one to read them in spite of the widespread aversion to lengthy footnotes.

The story of Harrod’s dynamics begins with his early attempts to attribute the cause of the cycle to fluctuations in the ‘fundamental conditions’ (Ch. I), while later he tried instead to characterise the cycle as a monetary phenomenon whose solution should be looked for in monetary manoeuvres. The result was an incomplete *theory*, based more on Robertsonian (and, at times, Hayekian) principles than on the ideas advanced by Keynes, while the *policy suggestions* were instead more akin to Keynes’s. Harrod failed to explain the turning points of the fluctuations, but already considered cumulative processes and regarded the cycle as occurring around a base of progress. The trend, however, was not yet treated as an autonomous process, but was seen as exogenously determined (Ch. II). The indecision as to how to deal with the trade cycle persisted in the course of 1934. Although the analytical tools he later used to assemble his dynamic mechanism (the multiplier and the accelerator) were already at his disposal, Harrod was still thinking his cumulative process in terms of a divergence between saving and investment. Only towards the end of the year, a prolonged discussion with Kahn set the premises for converting Harrod to the view that investment determines saving, thereby inducing the shift in Harrod’s reference framework necessary to let him appreciate the full implications of the multiplier doctrine. In the meantime, as a reaction to some of Robertson’s and Hayek’s arguments, Harrod laid a few of the methodological principles
that were later going to guide the formulation of his dynamic mechanism, namely the priority of instantaneous versus period analysis, the necessity to consider continuous rather than discrete changes, and the notion of moving equilibrium as proportionate and internally consistent growth (Ch. III).

In the course of the following year, Harrod began experimenting with the accelerator, and after having read the *General Theory* in proofs he finally appreciated the principle of effective demand. Moreover, Harrod had occasion to return to the theme of the continuity of change, which he refined by developing the critical argument that dynamics cannot be reduced to a succession of static steps. The main analytical and methodological ingredients of Harrod’s trade cycle mechanism were now ready, and indeed at the end of 1935 Harrod started working on *The Trade Cycle*, which was in proofs by June 1936 (Ch. IV). This book constitutes Harrod’s first thorough approach to a dynamic theory, although the notion of statics and dynamics were not explicitly defined. A comparison between the actual use of static and dynamic concepts, however, enables a characterisation of some of their peculiarities regarding the respective objects and methods of analysis, and their theoretical capacity of dealing with economic changes. This is the only one of Harrod’s writings enabling such a comparison, for he carefully discussed the relationship between static and dynamic laws, the first concerning the causes acting on the individual economic agents to determine their level of output and the latter concerning the determinants and the effects on the whole economic system of their decisions of saving and investment. On the one hand, Harrod inquired on the configuration of the static determinants permitting the dynamic forces to determine growth and fluctuations. On the other hand, he specified the mechanism that transmits to the individuals the incentive to adapt their level of output to what is decreed by the dynamic forces. The analytical treatment of these questions was not always satisfactory: in particular, Harrod failed to provide a specific link between successive time periods, he confused between two distinct notions of equilibrium, and had an ambiguous attitude towards the theoretical role of money. However, his book retains its interest for the fact it posed the problem of the mutual relationship between static (individual) and dynamic (aggregate) forces (Ch. V).

While arguing out the ideas he exposed in *The Trade Cycle* and before engaging in the writing of his “Essay in Dynamic Theory”, Harrod discussed the epistemological and methodological aspects of the relationship between scientific laws and empirical evidence, in a perspective (influenced by the emerging philosophical schools of logicism and neo-positivism) aiming at laying the foundations of his dynamic approach. In a 1938 paper on “The scope and method of economics” and in several Chapters of an unfinished book on knowledge, Harrod expressed the view that the starting point of the deductive process leading to the formulation of scientific laws must be the posing of a set of axioms consisting either in relationships derived by generalisation of the facts of experience, or in truisms derived by definitions. Accordingly, he founded the “Essay” on an axiomatic
basis, which was meant to lay down the framework for interpreting the dynamic phenomena, from which it was then possible to proceed by deduction. Harrod maintained that such processes of ‘mapping’ should provide in the first place a simultaneous picture, and should deal with ‘the succession of events’ only at a later stage. Accordingly, in his theory of economic dynamics he carefully introduced the distinction between instantaneous and sequential analysis, which brought him to counterpoise his approach to the line of attack of the econometricians (Ch. VI).

In the introductory section of his “Essay”, Harrod claimed that he was only presenting a development and an extension of the dynamic theory advanced in The Trade Cycle. But although the analytical components of the original mechanism were retained, they were interpreted in a different way, giving rise to both distinct cycle and growth mechanism and to a different notion of dynamics. In particular, Harrod had shifted from an interpretation of the accelerator as a determinant of investment to a perspective in which the acceleration coefficient only represented an a posteriori summing up of decisions whose antecedents were not discussed. In other words, he turned his dynamics as the study of the causes of change into a dynamics uniquely concerned with the registration of effects. The peculiar character of the evolution of Harrod’s conception emerges with particular strength out of the discussions Harrod had with Keynes on both the Trade Cycle and a first draft of the “Essay” (which was also discussed with Marschak). Moreover, the importance of these exchanges for the interpretation of Harrod’s thought is accrued by the fact that the deep incomprehension that characterised them was of the same nature as the reasons decreeing Harrod’s failure to convert his fellow economists to his dynamic viewpoint. In fact, Harrod did not explicitly and thoroughly discuss the methodological foundation of his distinction between successive stages of analysis. This concealed to both Keynes and the econometricians the fact that the confinement of analysis within a single point in time only regarded the simultaneous mapping, while the study of the succession of events (of the trade cycle, in particular) required a different procedure (e.g., parameters were treated as variables). Keynes was thus led to blame Harrod for supposing constant coefficients, while the econometricians were not capable of handling in their own language (functional equations) the complex and ever-changing relationships which Harrod himself only managed to treat verbally. While some specific results of Harrod’s treatment readily appealed to his readers and were thus englobed in their analytical frameworks, the nature itself of Harrod’s approach remained concealed to partial interpretations behind the curtain of Harrod’s method, which prevented the readers from appreciating the multiplicity of the several facets of Harrod’s thought (Ch. VII).

The making of Harrod’s dynamics cannot be seen as a smooth process of accumulation of concepts, instruments and analogies leading to the formulation of the ultimate result; rather it was the outcome of relentless debates, sudden changes of perspective, radical re-interpretation of concepts and pitiless rejections of previous
conclusions. There is however a unifying thread in this story. This must not be sought in the permanence throughout the years of a leading idea\(^{18}\), but in Harrod’s adherence to the methodological procedure of traditional theory, which he extended to dynamics, and in the detachment from orthodoxy implied by his epistemic premise for the possibility of a dynamic theory. Neither the continuity nor the break with the tradition occurred in the analytical domain, although of course they both brought with them important analytical implications, namely the concept of moving equilibrium and the instability principle respectively (Ch. VIII).

4. The Cultural Memory

In the course of the chronological reconstruction of the process of acquisition and assembling of analytical tools and pieces, I will show that Harrod did not draw only from a reservoir of concepts, archetypes, analogies, and metaphors of purely economic origin. He also referred to some physical concepts, and he was clearly influenced by the course of debates concerning political, philosophical and policy issues, in which he participated at various stages of his career. It may therefore be useful to sketch a brief and necessarily incomplete map of the events, readings and relationships contributing to Harrod’s intellectual formation in the period under consideration, with the purpose of pointing out some of the influences affecting the formation of the concepts which eventually constituted his dynamics (but also some of the influences that possibly could—but did not—direct elsewhere his line of thought). The main steps of Harrod’s education and activities have already been skilfully illustrated in three biographical memoirs (Blake 1970, Hinshaw 1978 and Phelps Brown 1980); Warren Young is writing the official biography (Young forthcoming), while Harrod’s own biographies of Keynes (Harrod 1951) and Lindemann (Harrod 1959) provide several auto-biographical considerations complementing an autobiographical sketch he drew in 1965 (which, however, was published only ten years later: Harrod 1975). Therefore, here I only need to remind the reader of the salient episodes of his career, and to insist on some aspects which I think have not yet been sufficiently emphasised, in the light of the fact that they seem to have largely contributed to the shaping of Harrod’s ideas.

Harrod was “brought up in a literary and artistic home in which the tradition was that conversation was by far the greatest pleasure, if not the prime object, of life” (Harrod 1959, pp. 39–40). This bend of his character was further nurtured during his schooldays at Westminster, where he entered at 13 in 1913 to take classics at first and to revert to history in the last two years (ibid., p. 27), and during the years as undergraduate at New College in Oxford, where he read Greats (i.e., philosophy and ancient history) from 1919 to 1921, and modern history in the subsequent year, in both cases taking a First. He later emphasised “that at Oxford and Cambridge the main part of an undergraduate’s education is imbibed from other undergraduates” by means of continuous conversation: “there are the societies for debate and discussion, and there are certain traditions which
the older generation of undergraduates hands on to its successors. Part of the tradition is a certain mode of frankness in discussion” (Harrod 1951, pp. 58-59). By this process, life in college in strict contact with students of other disciplines provides the occasion to widen one’s field of interest, and to acquire notions of domains outside one’s own.

Not many traces survive regarding the topics of such conversations. Nonetheless, it is certain that philosophy was among Harrod’s favourite subjects. He was the secretary of the Jowett society, at which undergraduates discoursed in philosophy and at whose meetings eminent philosophers and scientists sometimes took part (Harrod described some of these meetings in 1951, p. 140, and 1959, pp. 15-27). He regularly attended Alfred North Whitehead’s weekly ‘At Homes’, and also H. W. B. Joseph’s Greats lectures. However, Harrod recalls having been deeply dissatisfied with the state of philosophy at Oxford (Harrod 1951, p. 321, 1956, pp. ix-x, and 1959, p. 63): he had received a training enabling him to discourse fluently on ethics and scientific method (Harrod 1951, p. 327), but in the 1920s the Oxford approach to the latter problem was quite obsolete (Harrod 1959, p. 65). Harrod found some consolation in Cambridge, where he spent a term in 1922 “engaged in a tour of exploration into Cambridge thought” (Whitehead to Harrod, 3 Nov., 1922): Christ Church, in fact, thought Harrod’s qualifications to be suitable to teach economics for the first Honours examinations in Oxford to be held in 1923, and allowed Harrod two terms away (the second one was spent in Berlin). In Cambridge, Harrod met Braithwaite and Ramsey. The latter was only 19 at the time, but had already translated Wittgenstein’s Tractatus, had formulated the only criticism making Keynes uneasy as to his Treatise on Probability, and was correcting the proofs of Russell’s introduction to the second edition of Principia Mathematica. Ramsey shared Harrod’s contempt for the doctrines taught in Oxford (Harrod 1951, p. 321), and the two young men presumably had several chances, in Cambridge and in the course of subsequent meetings, to discuss the logicism of Russell and Whitehead and its criticism in terms of the linguistic analysis of Wittgenstein. As a matter of fact, Harrod saluted with enthusiasm the appearance of Alfred Ayer as a lecturer at Christ Church in 1933, who brought to the Oxford scene the logical positivist ideas developed by the Vienna Circle and eventually gained victory over the older Oxonian mode of thought.

The acquaintance with Ayer was to bring relevant consequences to Harrod’s own thought on the cognitive status and the method of economics, which eventually led him to try to provide an axiomatic basis for his dynamics. The roots and consequences of this attitude will be examined in Chapter VI below. Here it is however worth stressing that in this regard, a further aspect of Harrod’s passion for conversation probably played a part. After he became a Student (i.e., a fellow) of Christ Church in 1923, Harrod found in Prof. F. A. Lindemann (later Viscount Cherwell), physicist and director of the Clarendon Laboratories, a companion for his discussions. Harrod described in his memoir of The Prof. how they used to spend, all through the 1920s and less frequently during the
1930s, a part of the night in conversation, chatting away night after night until three o’clock in the morning. The topics ranged from day-to-day controversies to politics, and from economics to philosophy and to physics. The latter subject is of some interest, for in discussing the state of economics as a science, Harrod used to compare it with the state of physics, presumably deemed to be the more advanced of all scientific disciplines. Harrod’s mathematical background was quite scarce and certainly did not enable him to understand the specific content of physical theories or models. However Lindemann, although not a luminary in his own field, had a thorough comprehension of physics, and was rapidly able to assess the significance of new discoveries for physics as a whole (Harrod 1959, p. 48, citing Einstein’s opinion). Harrod had thus almost certainly imbibed his notions of physics from the discussions with Lindemann, and from the reading of some of his writings on the physical significance of specific theories; from one of these, in his 1938 essay on “The scope and method of economics” Harrod explicitly borrowed the guiding metaphor of the economist as a cartographer. Lindemann also seemed to agree with the main point Harrod took from Russell and Whitehead’s *Principia Mathematica*, that “the certainty that we get in mathematics was due to its being, in some sense or other, tautological” (Harrod 1959, p. 60). Although the Prof. was not interested in “the construction of a symbolic language to escape the contradictions and paradoxes of ordinary speech” (ibid.), their discussions on the topic may have strengthened Harrod’s belief on the necessity to provide an axiomatic foundation for his dynamics (this aspect is discussed in Chapter VI).

As to the necessity to found the axioms on an inductive basis (this is also discussed in Chapter VI), another episode may help understanding Harrod’s bend towards empiricism. In 1931-32, Wesley Mitchell spent a year in Oxford and brought there his “intransigent and strict empiricism”; this, at first, clashed with the prevailing atmosphere of intellectuality favouring deduction and the construction of theories, but soon, “though slowly, opinion evolved in [Mitchell’s] direction: it was felt that serious efforts to encourage investigation of facts had to be made” (Harrod 1937g, pp. 87-88).

A further aspect of Harrod’s multifarious interests which exerted a certain influence on the evolution of his economic thought was his political commitment. In his youth, Harrod was an “ardent liberal”, in particular, during the early 1920s, an “Asquithian Liberal as distinct from a Lloyd George Liberal” (Harrod 1959, p. 120), “rather to the left wing, who favoured in those days, and indeed until the middle ’thirties, a working accommodation, if only that were possible, with the Labour Party” (ibid., p. 51). Harrod’s political activities have already been described by Phelps Brown (1980, pp. 18-19), and additional details will be provided in Chapter II below. It is thus only necessary to emphasise the importance of certain acquaintances of Harrod at this point, and the influence exerted by certain debates in which he took part.

Harrod was on friendly terms with some of the most important personalities of the Liberal Party: he frequently visited the Asquith family, and in 1925 he worked for the
former Prime Minister’s nomination in the election of the Chancellor of Oxford University (the episode is recalled in Harrod 1959, pp. 119-136). In 1919, Harrod had helped John Simon in the Spen-Valley by-election, and had since been a frequent guest at his house (ibid., p. 145). Several years later, Harrod took “advantage of what [was then] an old-standing acquaintanceship” and asked Simon, who at the time was Chancellor of the Exchequer, to support the remedy for the recession he had suggested in an article in *The Times* (Harrod 1938c; Harrod to Simon, 11 Aug., 1938). Harrod also used to be a visitor to Walter Runciman’s house. Sir Walter wrote a letter of introduction for him to Keynes in 1922 (see Harrod 1951, p. 317), which was at the origin of a friendship lasting up to Keynes’s death. Harrod had also approached Runciman in search of support for the policy of reflation he was advocating in 1932 and 1933 (Harrod to Runciman, 10 and 22 June 1932, 3 March 1933).

In this regard, it is interesting to remark that Harrod, like Keynes31, strongly believed in the necessity to convince public opinion of the desirability of the interventionist policy he was proposing, and accordingly often wrote articles and letters to the vernacular press, sometimes also securing signatures of some of his eminent fellow economists. At the same time, he ardently advocated his proposed policies in personal correspondence with some of his political acquaintances, often insisting beyond what was reasonable and never leaving any road unattempted. Some of these contributions are discussed in Chapter II § 1 while their aftereffects are examined in Chapter III. But it is also opportune to cite Harrod’s passionate advocacy for public works at the outset of and during the 1937-38 depression32, and his even more fervid defence of policies favouring fertility in years in which the decline of the rate of growth of population was perceived as menacing the British stock33. It is worth remarking that Harrod’s opinion on the importance of this problem probably led him to attribute pride of place to his notion of the ‘natural rate of growth’ as a maximum sustainable pace of advance depending, among other things, on the growth rate of the working population (for a discussion see Chapter VI § 3).

In the mid-twenties, Harrod actually acted as an ‘ardent liberal’: he was Gilbert Murray’s agent in three general elections (Harrod 1957, p. 25), had been for a while secretary of the Oxford don’s Liberal Association (Harrod 1959, p. 123), and from 1924 to 1926 he took part in the Liberal Summer schools (Harrod 1951, pp. 349-50 and 362-4). From 1929 to 1931, his political activity seemed to fade out, perhaps because of the heavy university duties incumbent upon him (he was the Senior Censor at Christ Church, member of the Hebdomadal Council, and served on the commission appointed by the University to consider the future of the Bodleian Library34: see Phelps Brown 1980, pp. 16-17, and Harrod 1959, pp. 149-56), or due to a nervous breakdown which he suffered in 192835. After the 1931 general elections, with the depression heading towards its peak, Harrod felt with urgency the need for action. During the summer he was asked by Walter Runciman to become economic adviser of the National Government, but Harrod
was strongly opposed to its deflationary policy (Phelps Brown 1980, p. 1836), and in October spoke instead on a number of Labour electoral platforms in constituencies where no Liberal candidates were contesting the seat37. After the elections the Liberal Party, which in the 1920s used to propose the more advanced and better articulated interventionist policies, was lying in ruins, while the Labour Party’s official policy during the economic and political crises had been extremely conservative. As a reaction to the Labour’s orthodoxy, a new generation of Labour intellectuals set to work to re-think the Party’s policy. One of the offsprings of this movement was the New Fabian Research Bureau (NFBR), which got off the ground in March 1931 under the guidance of G. D. H. Cole (for an account of these ferments, see Durbin 1985). James Meade and Evan Durbin were among the most active members of the economic committees, the former bringing forward a Keynesian background, the latter arguing in terms of Hayekian analysis.

From 1931 to 1935 Harrod contributed to some research work related to the NFBR. It is interesting to describe these activities in some details not only because this aspect of Harrod’s political life is quite obscure, but for the important role it played in the formation of the assumption of “a steady base of progress”, which eventually ended up at the core of Harrod’s dynamics. In 1931, Harrod took part in one or two occasions to the gatherings organised by Hugh Gaitskell and Evan Durbin to discuss the economics of a socialist society (Postan 1964, p. 52). In January 1932, he addressed a joint conference organised by the Socialist Society for Information and Propaganda and the NFBR on the socialisation of banking, arguing in favour of national control of central banking “as the only effective means of putting a definite policy into operation”; the objective of such a policy should be the “provision of a stable medium of exchange” on the “basic assumption that real income of the community is increasing and will continue to increase” (Harrod 1932*; for a summary of the debates see NFBR 1932*; see also Durbin 1985, pp. 97, 139, 162-4, 168).

In the next few months Harrod apparently did not take an active part in the work of the NFBR, although he regularly discussed or at least read Meade’s contributions. In November 1932, Dalton went to Oxford, where he had a discussion on how to deal with a financial panic. Harrod and Meade were among the participants. A few days later Dalton proposed to Meade to organise a couple of meetings of economists in Oxford to continue the discussion and to send over the result of their deliberation (Meade to Harrod, 30 Nov., 1932). The result was a confidential “Proposal on the Control of a Financial Panic” to the Labour Party’s Finance and Trade Committee, “Prepared by a group of Oxford Economists who are members of the Party” (Meade, Harrod at al., 1933*). Moreover, Radice, the secretary of the NFBR, had suggested to Meade to “get the Economists in Oxford, who would be willing to work for the N.F.R.B.” to work on the financial and economic relations of the country during the transition to socialism. Meade was enthusiastic about undertaking such a task, and asked for Harrod’s opinion (Meade
to Harrod, 30 Nov., 1932\textsuperscript{38}). Meade probably drafted himself the memorandum on “The Exchange policy of a Socialist Government”, but Harrod (and others) extensively commented upon it; Harrod’s proposed changes were introduced in the final version (see Meade 1934a*; compare the first draft, bearing Harrod’s autograph suggestions, with the final version). In the letter accompanying his comments, Harrod specified as follows his position with respect to the NFRB:

I do not know how much responsibility I have as a member of the “committee of economists”. I am prepared to sponsor it as a workmanlike \& reasonable document. But suppose a member of the Bureau asked -who is your committee and asked me whether I would stand for the whole as it stands at present, I should still have a certain difficulty.

I dont press this point, because I take it that the personal responsibility is not so great as this (Harrod to Meade, 7 June, 1934).

In the meantime, Harrod was one of the “Nine economists from Oxford” writing a Chapter for Cole’s \textit{What Everybody Wants to Know about Money}, which appeared in November 1933 (Harrod 1933e); as Elizabeth Durbin noticed, the collective connection of the authors with the University of Oxford was rather tenuous, but they were all member of the NFRB (Durbin 1985, p. 144). In the same month, Harrod also took part to the NFRB “Conference on some aspects of Socialist Planning”, where he discussed monetary expansion in face of long-period increase in productive efficiency. The same topic was discussed in the opening address by Evan Durbin (for a summary of the debates, see Parker 1934*, and for a comment Durbin 1985, pp. 144 and 175).

In April 1935, Harrod commented upon Meade’s memorandum “Outline of Economic Policy for a Labour Government”, which he thought to be “splendid” and that if it were published “it would make an enormous difference to the attitude of a large part of the electorate to the Labour Party”. However, he feared that the Labour Party was “very far from adopting the principles relating to unemployment”, and criticised some of Meade’s suggestions as to the industries to be socialised (Harrod to Meade, 22 April, 1935. The “Outline” is reprinted in Meade 1988, vol. 1). Later that year, Harrod reviewed Evan Durbin’s \textit{Problem of Credit Policy} for the \textit{Economic Journal}, taking up again the problem of injection of credit in a steadily advancing society (Harrod 1935b).

To complete the account of Harrod’s political activities, it must be noted that towards the end of the 1930s, Harrod regarded himself as “an ex-liberal” (Harrod 1939a*). He channelled his dissent from the Government policy on the Munich agreement by writing a letter to the \textit{Oxford Mail} suggesting that the Labour and Liberal candidates of the pending Oxford by-election stand down in favour of an independent candidate (Harrod 1938j). They actually did so; A. D. Lindsay, the master of Balliol, accepted to stand for Parliament, and Harrod became the chairman of a joint committee supporting him (the episode is reported in Scott 1971, Chapter 14). At the beginning of 1939, Harrod wrote a Circular letter to a limited number of Conservative MPs, trying to coalesce the dissent on the Government’s foreign policy (Harrod 1939a*). His attempt to
generalise to the country the experience of the Oxford liberal-labour pact raised some interest on the part of several politicians (including some conservatives: the extensive correspondence on this topic is preserved in HP VI-E), but eventually failed. From the end of 1938 to the beginning of the war, this activity almost completely absorbed Harrod’s energy, together with intense propaganda in favour of the Population Statistics Bill and a consulence to the Parliamentary Monetary Committee.

To conclude the present biographical sketch, it is of course necessary to trace the main steps of Harrod’s career as an economist. As a student, Harrod did not receive a proper training in economics, but Christ Church allowed him two terms away to take up the subject. One of these was spent in Cambridge, where Harrod attended Keynes’s lectures on the Cambridge monetary theory (Harrod 1951, pp. 325 and 341), writing weekly essays for him that covered the ground of Marshall’s *Principles*\(^39\), and attending his Political Economy Club. The second term away was spent in Berlin, where Harrod attended Moritz Bonn’s and Melchior Palyi’s lectures at the Handels-Hochschule. According to Phelps Brown, Harrod “received no apparent stimulus from his immediate contacts with German economic thought” (1980, p. 8); nonetheless, he kept in contact with Palyi (a few letters from him, dating up to 1932, are preserved among Harrod’s papers) and with some of his colleagues\(^40\), and gained some knowledge of the German language, enough to allow him to approach the economic literature in its original formulation\(^41\).

On his return to Oxford, Harrod attended Edgeworth’s “absolutely fascinating” lectures (Harrod 1937g, pp. 79-80), and “took essays to him on cost curves and international trade”\(^42\) (Phelps Brown 1980, p. 8; Harrod 1963, p. 401). He must have spent the following couple of years in reading, according to his own almost contemporary recollection\(^43\) and to the notebooks densely filled of reading notes which survive and are preserved among Harrod’s Papers at the Chiba University of Commerce. These certify a wide range of interests, as does his lecture list relating to the years 1925-1937, as advertised on the *Oxford University Gazette*. As a matter of fact, within a few years his name was prophetically cited as “one of the rising generation of economists likely to make real contributions to the subject” (Fisher to Harrod, 15 Dec., 1926).

In 1925, Lionel Curtis offered him the secretaryship of a research committee on cartels at the Royal Institute of International Affairs (Chatham House) (the episode is recalled in Harrod 1959, p. 128); Harrod’s acceptance inaugurated a prolonged period of activities at Chatham House, which culminated in the drafting of two memoranda on the economic aspects of international affairs, with particular reference to the role of cartels in increasing cost competitiveness and to international wage-cutting competition\(^44\), and in the writing some fifteen review articles\(^45\) published in the *Journal of the Royal Institute of International Affairs* between 1926 and 1930. Later Harrod took a part (though marginal) in the discussions of the Chatham House study group on the International
Functions of Gold\textsuperscript{46}, and actively collaborated to the study group on International Monetary Problems, leading to the publication of \textit{The Future of Monetary Policy}\textsuperscript{47}.

Still in 1925, Harrod delivered to the British Association a paper on “The trade cycle and the theory of distribution”, in which he set the methodological principle, which guided the development of his more mature dynamics, that trade cycle theory must be consistent with the determinants of the level of output determined by traditional theory (this aspect of Harrod’s 1925\textsuperscript{*} paper is discussed in Chapter I, § 4). During the spring of the following year, the reading of Robertson’s \textit{Banking Policy} stimulated Harrod’s criticism and involved him in the first of the theoretical debates, of which so many traces remain among Harrod’s Papers. Harrod was able to show that the notion of ‘justified’ fluctuation which, according to Robertson, would be the natural behaviour of the economic system, is not in reality independent of the current banking policy, and that the search for the banking policy promoting the ‘right’ amount of fluctuation involves circular reasoning. Robertson admitted that Harrod’s remark had “convicted [him] of serious error” (Robertson to Harrod, 20 May [1926], and a subsequent 1926 undated letter), and invited him to publish his criticism. This exchange gave rise to Harrod’s first review article (Harrod 1927), while Robertson’s book probably made Harrod familiar with the notion of a progressing economy, thereby forming the background of the debates between the New Fabians in the early 1930s (this aspect will be discussed in more detail in Chapter II, § 3 below).

In 1928, stimulated by Pigou’s article on costs and competition (Pigou 1928), Harrod submitted to the \textit{Economic Journal} his first original contribution to economics. In an article on “Monopoly and quasi-competition”, Harrod showed that a general interpretation of the marginal principle, in face of a declining curve of demand, would require a new concept, which he named ‘increment-of-demand curve’ but became famous as the marginal revenue curve (Harrod 1928\textsuperscript{*}). Keynes, as editor of the \textit{Journal}, while appreciating the importance of the new notion perceived some confusion in the second part of the article, and submitted it to Ramsey for a further comment. Ramsey agreed with Keynes’s opinion, and Harrod was invited to think it over, while retaining the part on marginal returns. But in the meantime Harrod suffered a nervous breakdown, which -coupled with his University duties- considerably slowed down his theoretical work. He looked at the matter again a year later, and having recognised the source of Ramsey’s misunderstanding Harrod induced him to withdraw his criticism. The article was eventually published in 1930. This inaugurated a series of important contributions on imperfect competition, the outcome of which, together with the drift of the theoretical debates taking place among economists on the best policy to face the slump, soon induced him to reflect on the place of the assumption of imperfect competition of trade cycle theory. The line of approach of his 1936 book on \textit{The Trade Cycle} was based on the epistemic principles which originated from such reflection (Harrod’s contributions to
imperfect competitions will be discussed in Chapter I, while their relationship with dynamics will be further examined in Chs V and VIII).

In the meantime, his fellow economists were engaged in a debate on saving and investment in trade cycle theory, which was to fill the economic Journals for a few years in the early 1930s. Keynes, in his *Treatise on Money*, had offered a trade cycle mechanism based on the cumulative divergence of saving and investment; Robertson had worked out his 1926 theory and had proposed a method of period analysis based on the notion that saving decisions are taken with respect to the previous year’s incomes; Hayek was arguing that saving naturally brings forth the appropriate amount of investment. The debate originated from a radically different theoretical view of the working of the economic system, and was centred on the kind of action necessary to help recovery. Roughly speaking, from 1932 Keynes and his followers were in favour of strong action in support of investment, while on Hayekian principles it was argued that any banking policy aimed at influencing investment decisions by means of easy credit would induce further disequilibrium and should thus be avoided. While this debate was taking place, Keynes was re-framing his theoretical framework by incorporating Kahn’s multiplier, recognising the principle of effective demand, and developing the liquidity preference theory.

This was the complicated background on which the discussions among the young Fabians took place in the early 1930s. On the one hand, the Keynesians -James Meade and Roy Harrod- supported the view that easy money and public works policies had to be coupled in order to guide the economic system towards a recovery. On the other hand, the Hayekians at LSE -Evan Durbin, in particular-, although less rigidly than their master doubted the suitability of the Keynesian solution. The outcome of these debates were several newspapers articles and letters, while traces of it are to be found in Harrod’s principal theoretical work of that period, a book on *International Economics*. This was commissioned by Keynes in 1927, and was throughout discussed in preparatory stages by Keynes himself, by Robertson and by James Meade (this phase of Harrod’s activity is discussed in Chapter II below).

The debate reflected the theoretical hesitations affecting systems of thought in rapid development. In particular, it seems to have been difficult, even for the Keynesians, to come to grips with the evolution of Keynes’s principle of effective demand. A key role for its understanding on the part of Harrod was played by Richard Kahn towards the end of 1934. Kahn patiently explained to Harrod Keynes’s position, and convinced him to revise his line of attack against the followers of Robertson’s notion of saving, with whom Harrod was engaged in controversy in private correspondence and in learned Journals.

During the Summer of 1935, Keynes sent Harrod the *General Theory* in proofs. Harrod’s comments centred again on the saving and investment question. In particular, Harrod refuted Keynes’s criticism as to the lack of independence between the saving and
investment schedules considered by orthodox analysis. He argued instead that traditional theory only failed in wrongly extending the domain of pertinence of its analytical tools to the case of changing level of income. On the ground of this interpretation, and on the ground of the new analytical tools he acquired from Keynes, Harrod began thinking at the instruments necessary to tackle the dynamic problems relating to a growing economy. The outcome of this effort was a book on *The Trade Cycle*, published in 1936, Harrod’s first complete treatise in economic dynamics (Harrod’s book is analysed in this respect in Chapter V below). It took him about 7 months to work out and refine his theory. During this time, he benefited from the constant advise and criticism of some friends and fellows, in particular Meade, Robertson and Hubert Henderson. After the publication, he kept arguing out his ideas (and of course also Keynes’s, whose book was obviously at the centre of everyone’s interest) with Hawtrey, Kaldor, Robertson, and Keynes.

A suggestion coming from Keynes regarding a formula expressing the growth rate induced Harrod to think his mechanism all over again, and to submit to the *Economic Journal* a new version of his theory. A first draft of it, ready by August 1938, roused a fundamental objection from Keynes which generated a confused and prolonged debate, whose outcome was quite a different article, eventually published in March 1939 (this episode is analysed in Chapter VII).

6. A guided visit through a decade of debates

The gist of the story I have just told is that at every stage of its development, Harrod’s thought was deeply immersed in the current debates, most of which saw his direct participation. Harrod absorbed notions in their making, acquired new analytical and conceptual instruments, learned how to organise his thought, and reshaped his ideas while defending and expounding them and while arguing out his friends’ and fellows’ opinions, not necessarily on strictly economic matters. Harrod’s biography shows that his dynamics is one of the possible products of its time, and as such it will presently be studied, without forgetting however that Harrod was also -though not with the success he hoped for- “a maker of his epoch” (Musil 1918, p. 1030).

Since the controversies characterising the end of the 1920s and all the 1930s played a central role in the story of Harrod’s dynamics, this book may also be seen as a guided visit through a decade of debates, examined from the particular viewpoint of an economist who took part in most of the theoretical developments which marked that age. Such a story is admittedly partial, as it must inevitably be with case studies. Nonetheless, I hope it will not only throw some light on Harrod’s thought, but also that it will contribute another piece to the understanding of those years of high theory.

Notes
* Throughout the text I have resorted to the following conventions for quotations. Unless otherwise specified, emphasis is in the original. the symbol “<+>” indicates an illegible word in the original
Harrod’s notion of dynamics evolved through time, and the discussion of the reasons and the form of this evolution from the early statements up to the war will occupy several of the following Chapters. However, reference to \textit{instantaneousness} is a constant of Harrod’s conception.

The following account is not meant to provide an exhaustive survey on the literature on Harrod’s dynamics or on growth theory, but only to indicate in very broad terms the existence of two main lines of interpretation which both ignored the specificity of Harrod’s approach. For a detailed survey of the literature on Harrod’s dynamics see Besomi 1996a.

The non-linearity, however, was at first seen in the existence of upper and lower limits to the cumulative divergence from equilibrium, rather than in the necessity of changes in the value of the parameters.

Of course the literature on growth theory did not always explicitly refer to Harrod’s “Essay”. It is however interesting to notice that this paper offered a convenient logical and chronological starting point for classifying the literature on the subject. Hahn and Matthews, for instance, in their famous survey on the theory of economic growth, discussed the models of growth without technical progress as departures from one or the other of the simplifying assumptions of the Harrod-Domar model (Hahn and Matthews 1964). Nardozzi as well discussed the development of the debates on growth moving from Harrod’s and Domar’s contributions, but he reserved plenty of space to the shifts of general perspective characterising one or the other approach, rather than simply treating Harrod’s equation as a ‘result’ submitted to different lines of criticism (Nardozzi 1983). For a recent example of a discussion of ‘the Harrod-Domar Impulse’ to the theory of growth see Solow 1994, pp. 45-47.

Solow 1956, p. 65, coined the term ‘knife-edge’ to characterise Harrod’s instability principle. For a survey of the debates on the appropriateness of this notion, see Asimakopulos 1985, pp. 630-632.

For a comparison of Harrod’s and Domar’s views, and a survey of the reciprocal opinion on each other’s work, see Asimakopulos 1986.

There are, of course, notable exceptions to the interpretation of Harrod as a ‘growth theorist’: see in particular Kregel 1980, and Pugno 1992; both these authors criticised as inadequate the ‘standard’ interpretation.

It may be noted that these more careful readings were permitted (and probably also stimulated) by the publication of the correspondence between Harrod and Keynes in 1973 on methodological and analytical problems regarding two different stages of development of Harrod’s theory. It must be recognised, in fact, that although Harrod stewed his early writings with explicit indications as to the methodological target of both his positive and critical remarks on dynamics, the identification of a consistent scheme of thought on these problems (and of a precise line of evolution of Harrod’s conceptions) is enormously facilitated by Harrod’s comments on Keynes’s misinterpretations of his procedure. A study of Harrod’s correspondence with other economists later revealed other of his comments specifying his viewpoints (and their evolution) to an extent his published writings did not let shine through.

During the last years of the war, Harrod was immersed in politics and in policy matters. Although he discussed some aspects of his theory with Robertson (on the latter’s instigation) in March 1945, his interest for dynamics seems to have been revived by the invitation to give a series of lectures at the London School of Economics for February 1947; the lectures were prepared during Autumn 1946, and later published as Harrod 1948. It may be noted, however, that at first Harrod seemed to be inclined to stress the policy aspect of his book, which he thought to title \textit{Is Interest Obsolete}? (as results from a royalty agreement with Macmillan dated February 1947, in HP-IV-E, Box 33); by July 1947, however, the title was settled as \textit{Towards a Dynamic Economics}, shifting the emphasis on the theoretical part of the book, and in particular on the first lecture on “the need for a dynamic economics”.

A measure of the success of Frisch’s definition is given by the fact that since 1939 Schumpeter had already bowed the knee to its usage, in spite of holding a different opinion on the nature of dynamics: Schumpeter 1939, vol. I, p. 48.

Several commentators have accepted Harrod’s later interpretations of his earlier contributions on both economic dynamics and imperfect competition, and have thereby tried to provide an overall picture of Harrod’s work. Such an approach is not necessarily wrong. But in general there are good grounds for arguing that an author is the worse judge of his work twenty years hence, for his view back is shaped by the fulfilment or disappointment of the hopes and the illusions he loaded on it (other examples may be quoted, drawing from economics and other disciplines: see for instance Moggridge 1994 on Kahn). Thus
it is only possible to use a later statement on an earlier model in support to a certain interpretation provided that the continuity of thought is proved beforehand.

12 Throughout this monograph, by the expressions ‘orthodox’ and ‘traditional’ theory I mean, in broad terms, the Marshallian theory of value as it was commonly taught in Cambridge in the mid-twenties. This choice of words was dictated by the fact that Harrod himself currently used them with this generic meaning. Of course Harrod had access to Marshall’s own text, but when referring to ‘traditional’ or ‘orthodox’ analysis he almost exclusively alluded to the textbook version of the partial equilibrium approach, or directly to Pigou’s or -more frequently- Robertson’s ideas. The specific significance of this expression and the relationship between Harrod’s dynamics and traditional theory will be discussed in detail in Chapter VIII.

13 Other passages of the correspondence on the General Theory suggest that Harrod felt concerned with his fellows’ capacity of grasping a new system of thought:

I am thinking of the effectiveness of your work. Its effectiveness is diminished if you try to eradicate very deep-rooted habits of thought unnecessarily. One of these is the supply and demand analysis. I am not merely thinking of the aged and fossilised, but of the younger generation who have been thinking perhaps only a few years but very hard about these topics. It is doing great violence to their fundamental groundwork of thought, if you tell them that two independent demand and supply functions won’t jointly determine price and quantity. Tell them that there may be more than one solution. Tell them that we don’t know the supply function. Tell them that the ceteris paribus clause is inadmissible and that we can discover more important functional relationships governing price and quantity in this case which render the s. and d. analysis nugatory. But don’t impugn that analysis itself (Harrod to Keynes, 1 August 1935, in Keynes CW XIII, p. 533-4).

And it is these very existing doctrines that have been intensively chewed upon by the economists who are not path-breakers, but none the less endowed with some intellectual power. They have pondered on them again and again from many points of view, lectured on them, taught them, considered many possible lines of attack and defence, and finally embraced and endorsed them, and here you come rushing in and in the most airy way accuse them of logical inconsistency and suggest that they haven’t thought much about them. Suppose your reasons in the constructive and critical parts were equally good, you would have a far greater chance of carrying conviction in the former because your adversaries have not had years of thought in which to prepare an answer (Harrod to Keynes, 3 August 1935, in Keynes CW XIII, p. 536).

For what follows, it is however important to stress that the rhetoric argument was not the main reason invoked by Harrod to contrast Keynes’s criticism to the traditional theory of interest: Harrod in fact also refused to accept Keynes’s remarks on the lack of independence between supply and demand for saving. This aspect will be discussed in detail in Chapter VIII, § 7 below.

Moreover, it is important to stress that Harrod’s view as to the continuity of his dynamics with traditional theory matched his belief that it should be possible to harmonise specific contents of altogether extraneous systems of thought:

I entirely agree with much that you say with regard to the progress of economic theory and the literature thereof. In the natural sciences divergent views are fairly quickly brought to the test of fact; economics with its “schools” is still in the phase of quasi-scholastic, in which rivals can persist in their rivalry and mutual invective. The facts are so much more difficult to muster for the crucial experiment. This being so, I agree, that it is much more important that immature work should be discouraged and writers restrained until they are mature masters of existing doctrine, in order that they make not confusion worse confounded (Harrod to Knight, 7 Jul., 1937).

The argument was also taken up a few months later, when Harrod commented that in his Studies in the Theory of International Trade “Professor Viner brings out the continuity in the growth of thought” (Harrod 1938m, p. 434). Similarly, several years later, Harrod expressed unconditioned admiration for Marshall’s attempt “to find some good in various schools of thought and to preserve historical continuity, such as exists in the more developed sciences” (Harrod 1951, p. 143; see also p. 463, and Harrod 1956a, pp. 470-471). Finally, in his review of Sraffa’s Production of Commodities Harrod wished the co-existence of the elements of ‘truth’ in opposed systems of thought (Harrod 1961).

Incidentally, it may be noted that Harrod’s reference to the historical continuity in science reveals his adherence to the view that science progresses by cumulating pieces of knowledge. This is of course
consistent with his logical positivist outlook (Harrod’s philosophical viewpoint is discussed in Chapter VI below), and contributes to explain both Harrod’s “horror of public debates” (Robertson to Harrod, 4 Oct., 1934) which he thought to ruin the reputation of the discipline (“that there are reasons in the interests of economics being provoked into a dog-fights”: Harrod to Henderson, 9 April, 1936; see also Harrod to Kahn, 2 Nov., 1934, Harrod 1937a, p. 87, and 1951, pp. 142-3), and his interpretation of Keynes’s General Theory as a re-arrangement of the ‘pieces’ of traditional theory (this aspect is discussed in Chapter V, § 4, and VIII, § 7 below). Harrod’s belief in the continuity in science is discussed by Pugno 1992.

14 For a quick overview of the reviews of The Trade Cycle in this connection, see Chapter V, note 38.

15 Explicit analogy with mechanics may have played the role of an aid to thought, but also of a rhetorical device. On the one hand, the analogy with other disciplines is based on the recognition that the problems discussed are isomorphic and can thus be treated by the same conceptual instruments. The analogy process thus implies attributing the object of one’s study the properties belonging to the object described by the other discipline, but also entails ignoring other properties that would instead be given emphasis by the choice of a different analogue. On the other hand, reference to the methods and concepts developed in a discipline which has already proven to have been successful may help in transferring part of its prestige to the younger discipline.

Harrod sometimes characterised his method of approach and his notion of statics and dynamics with reference to physics, but it is not easy to establish which of these two orders of considerations prevailed in his mind. The chronology of the surviving evidence, however, suggests that as regards the method of inquiry Harrod may actually have recourse to the analogy for developing his approach (this aspect is discussed in Chapter VI, § 1), while in the case of the notion of dynamics recourse to the mechanical metaphor rather seems to be an ex-post rationalisation which may have played an essentially rhetorical role (see Chapter VIII, § 5).

16 Harrod himself for a while experimented with the alternative epistemic principle, trying to attribute the cause of the cycle to the factors determining the fluctuation of the ‘fundamental conditions’: changes in the preferences, initial distribution of resources, etc.: see Chapter I § 4.

17 This trend changed from the 1980s: the number of studies on Harrod’s economics drastically dropped with respect to the 1950s and 1960s, but scholars ‘rediscovered’ the original texts. See Besomi 1996a, § 4.

18 It must be admitted, of course, that growth was the permanent theme of Harrod’s dynamics. I do not think, however, this can be recognised as the organiser of his thought, for it played different roles in successive stages of Harrod’s theoretical development. It changed its status from an exogenously determined base around which fluctuation occurred (1934) to the equilibrium outcome of the interplay of multiplier and accelerator (1936); it was discussed in terms of balance of forces (Trade Cycle), and of divergence from an equilibrium rate. The notion of growth was thus interpreted and used (i.e., organised) in the framework of the evolution of Harrod’s dynamic thought, rather than guiding it.

19 “I had read a great deal of philosophy at my school” (Harrod 1959, p. 26).

Contemporary evidence regarding Harrod’s interest for philosophy and his specific train of thought is particularly fragmentary, so that we often have to rely on Harrod’s later recollection -whose documentary value is subject to the limits already stressed in note 11 above. However, what survives seems to confirm Harrod’s story: see in particular his letter to Moore of 29 January, 1936, the letters he received from Ayer in December 1933, and the correspondence with Robertson on “tautology” in November 1935. Harrod’s view, as expressed in print in 1938a and -implicitly- in 1939a, provides a further indirect support to this story (for a discussion see Chapter VI § 1).

20 Harrod recalled that

My friendship with Whitehead originated from the fact that his son […] was my contemporary at Westminster School during the First World War […]. Whitehead went to live in Chelsea, after he became Professor of the College of Science and Technology in South Kensington […]. He had what one might call an ‘At Home’ for his friends once a week after dinner (Harrod to McGuinness, 20 Feb., 1974).

I regularly went to their weekly after dinner At Homes, every week when I was still at Westminster and every week during vacations after I had gone to Oxford. When at Oxford I always spent the vacations -six months per year- living with my mother in London (Harrod to McGuinness, 10 May, 1974).

21 Harrod preserved the notes he took in his undergraduate days (HP VA and B); among these, some regard one of Joseph’s courses on Marx, in 1920. Joseph was also Harrod’s tutor, but his comments on Harrod’s efforts did not leave a good memory in his pupil: Harrod in fact described his arguments with
The Making of Harrod’s Dynamics

Joseph as leading “to nothing but frustration” (Harrod 1959, p. 26; for further comments on Joseph, tending to depict him as a “silly man”, see ibid., pp. 22-27 and 57-59, and 1951, pp. 138-40).

22 The course actually began in the Michaelmas term of 1921. For a history of the Honour School of Philosophy, Politics and Economics (‘Modern Greats’) see Young and Lee 1993, in particular Chapter 1.

23 Harrod’s comments on Ramsey’s criticism suggest that they have discussed the matter at a certain depth (Harrod 1951, p. 635n), while other passages of Harrod’s Life of Keynes seem to imply that Harrod himself was personally quite interested in the topic (see e.g. ibid., pp. 138-40).

24 Some evidence survives concerning a visit by Ramsey in Oxford in February 1925 (Ramsey to Harrod, 22 Feb. 1925), and an invitation to Harrod in April 1929 (Ramsey to Harrod, 18 April 1928), while a further planned visit was cancelled due to Ramsey’s premature death in 1930 (Harrod 1945*). The occurrence of other meetings is of course quite possible, for Harrod spoke of Ramsey in very friendly terms.

25 Harrod recalled he had read Wittgenstein, and some of his comments seem to imply he had met him: 1959, p. 61. It is perhaps worth mentioning that Price thought opportune to write to Harrod that Braithwaite considered the “Tractatus Logico-Philosophicus […] the most important work since the Critique of Pure Reason” (opinion which, however, Price found “difficult to swallow”: Price to Harrod, 19 May 1924).

26 Two years later, Harrod was especially active in looking for a solution enabling Ayer to remain at Christ Church at the end of his Lectureship. He had the idea of reviving a Research Studentship (equivalent to a fellowship in other colleges) which had been created for Einstein in 1931-32 and which was left vacant since the latter’s departure for Princeton; for this purpose Harrod contacted G. E. Moore, Henry Price and Whitehead, asking their opinion on Ayer’s work; their testimonials produced the desired effect. For a description of the episode, see Ayer 1978, pp. 161-163.

27 It may be interesting to remark that Price referred to one of the Cambridge philosophers interested in the ideas developed by the Vienna Circle as “your [Harrod’s] friend Miss Stebbing” (Price to Harrod, 19 May 1924). However, no evidence regarding further contacts between Harrod and Susan Stebbing seems to survive. Stebbing’s criticism to Eddington is cited in The Prof., but only in connection with Lindemann’s appreciation of that work (Harrod 1959, p. 60).

28 In his reflections on method, Harrod often referred to physics as a touchstone for evaluating the scientific status of economics: see Chapter VI § 1.

29 Harrod recalls to have read The Physical Significance of the Quantum Theory in proof (Harrod 1959, p. 67), although Harrod was not listed in the preface of the book among those who read and commented upon it (Lindemann 1932, p. vi). For a discussion of the metaphor of the map, see below, Chapter VI § 1.

30 Harrod 1937g, pp. 87-88; for a later but nonetheless consistent account of Mitchell’s visit and its lasting influence on the Oxford young teacher’s attitude towards empiricism, see Harrod 1949.

31 Shortly before the period I am discussing, Keynes had published his Essays in Persuasion (1931), which he declared in the preface to have been written “in a spirit of persuasion, […] in an attempt to influence opinion” (Keynes CW IX, p. xvii).

32 In 1937 Harrod wrote a two-parts article for the Manchester Guardian and prepared a collective letter for The Times (Harrod 1937h and Harrod, Meade et al. 1937, respectively); during the second part of 1938, Harrod contributed several articles and letters to The Times and The Financial Times: see Harrod 1938c-h. As a consequence of the prolonged debate aroused by Harrod’s articles, in November he was called to present his views in details in front of the House of Commons Monetary Committee; his speech is preserved among Harrod’s Papers (1938a*).

33 A large stock of documents and correspondence regarding the population problem is preserved in HP VI-C.

34 Harrod preserved several documents relating to the Bodleian Library Commission, now collected in HP file VI-A.

35 Between July 1928 and March 1930 he did not manage to publish anything, nor even book reviews.

36 I have not found any document corroborating this episode.

37 In asking Harrod to address a meeting, Maurice Webb (Propaganda officer of the Labour Party) wrote him: “We have received a communication from Professor G. D. H. Cole this morning, informing us that you are willing to speak as a Liberal on behalf of Labour candidates in constituencies where there is a straight fight against the Tories” (Webb to Harrod, 23 Oct., 1931. See also Fleming to Harrod, 22 Oct., 1931).
To the best of my knowledge, this letter by Meade (which in the first part dealt with Harrod’s *International Economics*) is the only surviving document preserved by Harrod where the NFRB is explicitly mentioned. This is somewhat surprising, for Harrod himself collected and organised his papers for the posterity (some blocks of correspondence are prefaced by short notes in Harrod’s handwriting, written some years later, describing their content for the benefit of future historians of thought), preserving almost everything from tailor’s bills to scientific correspondence, and from correspondence discussing his sexual preferences to unpublished economic, philosophic or political papers. Fortunately some evidence as to Harrod’s activities for the NFRB are found in other archives: Harrod’s letters to Meade are preserved among Meade’s papers, while some evidence as the meetings of the NFRB is to be found among the papers of the Fabian Society.

ibid., pp. 323-4. From Keynes’s appointent diaries it results that they met 5 times in term, from 30 October to 4 December.

Several letters by Gerhard Nebel are preserved, but mainly concerned with philosophy: see e.g. Nebel to Harrod, 15 Oct., 1923.

Some of the letters Harrod received from his German correspondents were written in German, although presumably Harrod replied in English. However, he took notes from some German books in their original language. This, incidentally, provides a further disproof of Goodwin’s claim that he translated for Harrod Tinbergen’s 1937 review of *The Trade Cycle* because “Harrod, like Keynes, could not read German” (see e.g. Goodwin 1989, p. 157).

On these topics, one of Harrod’s letters to Edgeworth and three letters from Edgeworth, dated from April to May 1924, survive among Harrod’s papers, file IV-305-308.

“When I was appointed here [at Christ Church] I was, as was recognized, ungrounded in the subject of economics, and I have had to start from the beginning in making myself acquainted with what had been done in it. It is true that I have had three years to do so, but most of the while I have been in full blast teaching, and teaching, too, rather miscellaneous things. I have had to add to this various obnoxious money making activities to keep the pot boiling at home. I have found no one both able and willing to give me any guidance” (Harrod to Lindemann, 24 Feb., 1926).

Harrod observed that international price-cutting competition “gives an immediate advantage to the country that introduces it (principle of comparative costs), but in the long run reduces the efficiency of workers by lowering their standard of life; this sets in a vicious circle” (Harrod 1925a*).

Harrod also wrote several review articles for the *Economic Journal*, from 1925 onwards. The reviews ranged on a rather wide set of subjects, roughly covering the same ground as Harrod’s reading notes mentioned above.

Harrod was one of the discussants of the paper delivered by Robertson: see Royal Institute of International Affairs 1931, pp. 29-30; however, he did not take part to other discussions -due probably to his nervous breakdown.

Royal Institute of International Affairs, 1935. The works of the group began in Autumn 1932, but Harrod was only invited to take part to the preparatory discussions in July 1933. However, he contributed with an essay on “Continuity of Values and the Long-term International Problem) (Harrod 1933b*), and actively discussed the papers read by his colleagues.
Chapter I

Imperfect Competition
and the Possibility of Trade Cycle Theorising

The discussion of imperfect competition occupied Harrod during the formative stage of his theory of the trade cycle. This, however, is not the main reason for beginning a study on the making of Harrod’s dynamics with a Chapter devoted to his analysis of imperfect competition. This lies instead in the fact that he meant imperfect competition analysis to provide the “general theory of value” on which trade cycle theory -and, in general terms, dynamics- should find a methodological foundation. It is therefore expedient, before examining the process by which Harrod appropriated the analytical instruments and the conceptual tools that eventually constituted the ingredients of his model representing the growth and fluctuations of output (Chs. II to IV), to reflect on the preliminary and fundamental stage of the determination of the causes of the level of production1.

The present discussion does not aim at expounding in detail Harrod’s analytical and conceptual contributions to the development of imperfect competition theory (for an illustration of Harrod’s position in historical perspective, see Shackle 1967, Ch. 4), but rather at expounding their implications for Harrod’s dynamics. There are three interesting aspects in this connection. In the first place, Harrod interpreted the toolbox developed for dealing with imperfect competition as capable of universal application to a domain ranging with continuity from monopoly to perfect competition. He thus presented the new analysis as a generalisation of the traditional theory of value, of which it constituted a development rather than an alternative. This aspect will be discussed in Section 1 below; its full importance however will be fully appreciated only at a later stage, when it will be examined in the light of Harrod’s interpretation of his dynamics in relation to orthodox analysis2 (Ch. VIII, §§ 4-6).

Secondly, Harrod thought that by referring to imperfect competition he could help to explain some phenomena of the trade cycle that could not be accounted for in a perfect competition set-up. This aspect will be examined in Section 2.

Section 3 is dedicated to the third aspect of Harrod’s discussion of imperfect competition, which is the most interesting and relevant in relation to his dynamics. Harrod in fact remarked that the assumption of perfect competition implies the stability of equilibrium, in the sense that any deviation from it sets in motion forces tending to re-establish the old position. The level of output compatible with the maximisation of the entrepreneur’s self-interest thus constitutes a state of rest, and is irreconcilable with fluctuations or other sorts of variations of production, unless one supposes that the cost and demand condition alter rhythmically, under the influence of some exogenous cause,
to suit the business fluctuation. Harrod realised that if imperfect competition prevails, the stability of equilibrium is undermined, and saw in this peculiarity the key for the possibility of endogenous trade cycle theorising. Here lies the origin of Harrod’s instability principle, which Harrod thought to provide the right kind of explanation of economic movement; here, therefore, his dynamics finds its epistemic foundation.

Finally, Section 4 frames these results in the perspective of the development of Harrod’s dynamic thought.

1. Towards a General Theory of Value

Since his very first reflections on imperfect competition, Harrod’s contributions were characterised by an attempt to provide a generalisation of the Marshallian approach capable of accounting for the consequences of imperfections of the market. In particular, like most of the participants to the debate, Harrod was concerned with the possibility of the occurrence of decreasing costs in competitive equilibrium. Such a case was ruled out by the assumption of perfect competition, while experience on the other hand seemed to indicate that conditions of increasing returns were prevalent in industry. Harrod’s results concerning this specific problem and their significance for dynamics will be discussed in Section 3 below. Here I shall examine instead how Harrod suggested interpreting the role of the analytical instruments he himself worked out to deal with the features of imperfect competition.

Harrod’s first contribution to the theory of imperfect competition was the invention of the notion of ‘marginal returns’ -which he originally named ‘increment-of-demand curve’ (Harrod 1928*, and 1930). Harrod’s aim was to extend the validity of the Marshallian approach. The condition for the maximisation of profit originally referred to perfect competition only, while Harrod wanted to extend its validity to the case of monopoly. He realised that when the firm is confronted with a demand curve that is not horizontal but downward sloping, what matters is not the comparison of costs at the margin with the average price per unit, but the comparison at the margin of cost and the increment of total revenue. Harrod therefore constructed a new curve by drawing the derivative of the function of which the ordinary demand curve represents the average value, and concluded that the equilibrium output is determined by the intersection of the curves representing marginal costs and marginal revenue, while the price per unit is read on the corresponding point on the demand curve. The new conceptual instrument allowed Harrod to reinterpret, by generalising it, Marshall’s result:

Marshall laid down that the entrepreneur ‘endeavours to employ each agent up to that margin at which its net product would no longer exceed the price he would have to pay for it.’ This is true alike of the monopolist and of the competitive entrepreneur, if the net product is valued in the right way. Where the firms are very many, value of the net product can be given its face meaning, viz. the value of the product which is the difference between the whole product of \( n \) units of agent and the whole product of \( (n-1) \) units of agent. But this, the most natural, interpretation of value of net product makes Marshall’s proposition untrue for the case where the firms are not many. Where the firms
are not many it is necessary, in order to make Marshall’s proposition true, to interpret value of the net product as meaning the difference between the value of the whole product of \( n \) units of the agent and the value of the whole product of \((n-1)\) units. Where the firms are many either interpretation gives the same result. Thus the second interpretation makes the proposition true universally (Harrod 1928*, italics mine. See also Harrod 1930, p. 239: there Harrod concluded less emphatically that “the second interpretation has general application”).

Complementarily, Harrod understood that perfect competition can be interpreted as a special case of this general relationship:

Where the firms are very many the curve showing the demand for the output of any one firm is horizontal and consequently the increment-of-demand curve coincides with it. The output is shown by the point at which the marginal cost curve cuts the demand curve. The average cost curve of the equilibrium firm cuts it at the same point (ibid.).

The next step was to understand that the difference between average and marginal revenue (and thus, in equilibrium, between price and marginal costs) depends on the elasticity of demand. By differentiation of the total revenue with respect to the quantity produced, Harrod obtained a formula stating that “the marginal cost of production is, in equilibrium, equal to the price, less the price divided by the elasticity of demand” (Harrod 1931, p. 570). Let \( x \) be the quantity and \( p \) the price. The total revenue is thus \( px \).

Marginal revenue, obtained by differentiating total revenue with respect to the quantity, is equal to

\[
\frac{d(px)}{dx} = p + x \frac{dp}{dx} = p + p \cdot \frac{x}{p} \frac{dp}{dx} = p - \frac{p}{\eta},
\]

where \( \eta \) is the elasticity of the demand curve.

For the purpose of the present Section, what matters is not the formula itself, but the interpretation Harrod gave of it, which confirms his intention to work out a general theory of value, including monopoly and perfect competition as special and extreme cases. According to Harrod, the above statement has universal applicability. Pure competition with a perfectly organised market is the special case in which elasticity is infinite, and the marginal cost of production is therefore equal to the price (Harrod 1928*, emphasis added).

Harrod expressed a similar position a couple of years later, when reviewing Chamberlin’s Theory of Monopolistic Competition for the Economic Journal. Harrod dedicated a large part of his discussion to the criticism of Chamberlin’s ‘unnatural result’ that “there is no gradual transition from monopoly to competition, but a sudden break at the point at which the individual ceases to think that his own change will have any effect at all on the action of others” (Harrod 1933a, p. 663). Harrod argued that the individual demand curve must account for both the price reduction a firm has to apply in order to increase sales, and for the reduction of sales that other firms will face in consequence of their competitor’s increase of production (given their cost functions). It follows that

the gradient of the individual demand curve is (arithmetically) less than the gradient of the market curve. In a given market it will be less the smaller is the
individual. Moreover, since the marginal revenue curve will diverge from the demand curve more the greater is the output of the individual, his marginal cost will for this reason also be nearer the market price the smaller is the individual. For both these reasons we should expect a gradual transition from the position of the monopolist, whose private demand curve is that of the market, through a series of oligopolists whose private demand curves become flatter the more there are and whose marginal revenue curves diverge less the flatter are the demand curves and the smaller are the oligopolists, to the position of perfect competition when the demand curve is horizontal and its value equal to that of the marginal revenue curve. Professor Chamberlin’s ‘break’ disappears (Harrod 1933a, p. 664).

These considerations show that Harrod was convinced that he was on the way towards a twofold operation of generalisation. On the one hand, the different market conditions were reduced to a continuum of which monopoly and perfect competition are the opposite extremes, characterised respectively by the arithmetically higher and lower inclination of the curve of demand. On the other hand, the invention of the marginal revenue curve permitted to discuss the entire domain with the same analytical toolbox, that is the Marshallian comparison at the margin between costs and revenue, provided that the principle was interpreted in the right way. Harrod did not believe the operation to be concluded, although in specific fields -e.g., the law of decreasing costs- “complete precision […] is almost within our grasp” (Harrod 1933b, p. 337); he thus set the completion of this project as the target for future research:

it still remains for a master mind (perhaps that mind will be Professor Chamberlin’s!) to introduce a greater measure of unity, to construct some apparatus by which it will be possible to demonstrate the various special laws as springing from a single simple principle (Harrod 1933a, p. 662).

2. Imperfect Competition and the Phenomenology of the Trade Cycle

After the publication of Chamberlin’s and Joan Robinson’s books, research on imperfect competition slowed down, and the search for the ‘unifying theory’ did not progress. Harrod, however, must have been fairly satisfied with the results obtained so far, since from 1936 he felt authorised to speak of a “general theory of value” (Harrod 1936a, p. 75; 1936b, pp. 87 and 88), and began considering whether the phenomenology of the trade cycle was compatible with the traditional theory of the determination of equilibrium prices and quantities in its more general interpretation.

The problem was formulated as follows. As his starting point, Harrod took some features of the trade cycle “as established by wide observation”; in particular:

the fact that the commodity price fluctuation has greater amplitude than that of (money) rewards to prime factors; [and] the fact that the profit fluctuation has greater amplitude than any of the afore-mentioned fluctuations (Harrod 1936b, p. 84).

He then proposed to establish whether these features could be accounted for by “the leading principle of the theory of imperfect competition”, viz. “that entrepreneurs tend to equate marginal cost to marginal revenue” (ibid.). As “a first approximation to the truth”,
Harrod thus decided to ignore the fact that in several cases entrepreneurs do not respect this rule (for instance because of the adoption of fallacious rules of thumb or of psychological causes), and proceeded to examine the ideal behaviour of an homo economicus operating in conditions of imperfect competition.

As to the first of the above mentioned features, Harrod examined both the fluctuations of cost and demand conditions during the cycle. In Harrod’s view, work costs tend to increase at the margin in the course of the prosperity, since “when trade is active labour of progressively inferior quality may be taken on”, while the management tends to become careless. On the demand side, Harrod maintained that “as income rises, the elasticity of demand becomes less”. On the one hand, rich people do not bother as poorer people in the discommodity of the quest for the cheapest market while tending to “slip by imperceptible stages into careless habits” when their income is expanding. On the other hand, in good times for enterprises “imaginative planning is [...] more remunerative than attention to rigid economy”. Harrod concluded that

If the principle be correct that in good times demand is apt to get less elastic and competition more imperfect, the phenomenon of the amplitude of price fluctuation exceeding that of cost fluctuation is readily explained. Marginal cost tends to be equated to marginal revenue. If the ratio of prices to marginal revenue rises owing to decreasing elasticity, the excess in the rise of price over that of cost in the boom is accounted for in accordance with the general theory of value (ibid., pp. 86-87, italics mine).

Harrod thought the same principle could contribute to explain the divergence in the relative amplitude of profit and price fluctuations in the course of the cycle. For the purpose of the study of this phenomenon, attention had to be directed on the ratio of price to average prime cost. On the assumption that entrepreneurs equate marginal cost to marginal revenue, the problem was reduced to the evaluation of the movements of the ratio of average prime cost to marginal prime cost on the one hand, and of the ratio of marginal revenue to the price on the other hand (ibid., p. 87). The former of these ratios is affected by the asymmetrical fluctuations in the use of productive capacity. The introduction of new plants in the course of the boom (“sometimes even beyond the limit of requirements at the peak”; ibid., p. 86) is an irreversible process. Since plants cannot be dismantled, excess capacity will manifest in the early stages of the depression. But excess capacity as a rule indicates that marginal prime costs are below average prime, so that in depression a rise in the ratio between average and marginal costs may be expected, and therefore profit may be expected to fall more than prices (ibid., pp. 87-88). Harrod himself, however, doubted that this factor could provide a satisfactory explanation of the phenomenon, and relied more on his previous analysis of the increasing imperfection of competition during the boom. In fact, with a decreasing elasticity of demand the gap between average and marginal revenue curves will widen, thereby increasing the ratio of profit to price (ibid., p. 88).
Harrod thus concluded that the phenomena he took into consideration are indeed compatible with the assumption that entrepreneurs aim at maximising their profits by pushing the level of production up to the point where marginal costs equate marginal returns: his article’s claim for consideration rests on its attempt to rescue the theory of output in the cycle from excessive dependence on the hypothesis of time lags. It gives certain broad reasons for supposing that the entrepreneur’s variations of output within the cycle may be represented not as maladjustments, but as conformable to the postulates of the general theory of value (ibid., p. 88).

3. Increasing returns, instability, and the possibility of a dynamic theory

Most of the articles on imperfect competition that Harrod wrote between 1930 and 1933 are to be seen as contributions to the debate on increasing returns which followed Sraffa’s 1926 attack to the partial equilibrium approach. Sraffa had pointed out that “reductions in cost connected with an increase in a firm’s scale of production, arising from internal economies or from the possibility of distributing the overhead charges over a large number of product units” are “incompatible with competitive conditions”, whilst reductions in costs due to ‘external economies’ are “clearly incompatible with the conditions of the particular equilibrium of a commodity” (Sraffa 1926, p. 540). Sraffa’s conclusion was commonly interpreted as a suggestion to relax the assumption of perfect competition, so that the case could be considered that small individual firms are held in equilibrium by being subject to increasing marketing expenses. Where the product is not completely standardised or the market not organised, the individual producer may, although quite small, have increasing difficulty in marketing increments of produce (Harrod 1931, p. 566).

The goal of Harrod’s articles was thus to show that if imperfect competition prevailed, decreasing cost conditions may be compatible with a competitive equilibrium.

The most systematic discussion of this problem is to be found in a 1931 article for the Economic Journal on “The Law of Decreasing Costs”. At present it is not important to follow Harrod’s argument in detail, but only to summarise his results, for these will constitute the premise of an interesting development of Harrod’s thought concerning dynamics. In the first place, Harrod pointed out that if competition is imperfect and the demand curve downward sloping, it is possible that the marginal revenue curve intersects the marginal cost curve in correspondence of the decreasing tract of the average cost curve. In such a case, increasing returns are compatible with short-run equilibrium, and the firm may suffer from excess capacity. This situation is however compatible with the presence of profits, for the price would exceed the marginal cost of production (Harrod 1931, pp. 569-71; see also § 1 above). In the second place, Harrod stressed that, “if appreciable transport costs are involved, or if a spirit of individualism is prevalent among entrepreneurs”, decreasing costs are compatible with long period equilibrium also (ibid.,
He concluded that “Competitive equilibrium with decreasing costs in the short and long periods may be regarded as \textit{normal} to industries the rate of expansion in the optimum plant of which exceeds the rate of increase in demand” (ibid., p. 576) (because of to technical progress, for instance: ibid., p. 574).

In passing, Harrod polemically observed that

It should be noted […] that orthodox theory does not […] provide for a short-period equilibrium with decreasing costs. That is clearly a matter of importance for the analysis of the trade cycle (ibid., p. 572).

However, for the time being the significance of this proposition remained quite vague. An explicit discussion of the importance of increasing returns for trade cycle theorising only came in 1934, occasioned by an invitation from Taussig to write a review article for the \textit{Quarterly Journal of Economics} on Chamberlin’s and Joan Robinson’s books on imperfect competition (Taussig to Harrod, 17 Oct., 1933). The outcome was what Harrod considered a \textit{summa} of “the principal points of significance for economic theory in the doctrines relating to Imperfect Competition that have been recently evolved” (Harrod 1934e, p. 442). The third part of this essay, relating to “the importance of these doctrines for trade cycle theory”, deserves careful examination, for there Harrod laid the epistemic foundations of his dynamics.

Harrod took up a problem which was left untackled in his previous contributions on imperfect competition theory. There he had discussed the factors affecting the gradient of the cost and demand curves in correspondence of the equilibrium level of production, but had neglected the problem of the \textit{stability} of the equilibrium point resulting from the entrepreneur’s policy of profit maximization.

To study this aspect, Harrod reasoned as follows\textsuperscript{17}. To simplify matters, he considered an economic system where there are only two firms\textsuperscript{18}, whose products in equilibrium are exchanged in a certain proportion. Next he supposed that one of the two firms casually abandoned the position of equilibrium, cost and demand conditions remaining unaltered. He then examined the consequences of such deviation from equilibrium on relative prices and on the firms’ subsequent decisions regarding the scale of production. He found the ultimate effect to depend on the cost structure of the firms. If both firms operated in conditions of decreasing returns,

the old position of equilibrium will be a \textit{stable} one. Any fortuitous contraction of either industry may induce a temporary contraction in the other but will set up forces impelling to both to revert to the old position (ibid., p. 466, emphasis added).

Harrod then examined the case of imperfect competition with increasing returns. In such conditions, if one of the firms reduces its output, the other one finds it convenient to mimic this behaviour and permanently diminish its own level of production.

Thus they may chase each other indefinitely until they chance upon some new equilibrium position at a level of output, which may be much lower than the old
one. The old equilibrium will prove not to have been a stable one\textsuperscript{19} (ibid., p. 467; italics mine).

By disassociating the notions of equilibrium and stability, Harrod’s result emancipated the concept of equilibrium from the idea of rest. This entailed important consequences for the possibility of explaining economic change within the framework of traditional analysis\textsuperscript{20}. So long as equilibrium is stable, to account for an enduring departure from such a state one has to appeal to some mechanism alien to the laws of value. As an example of the search of \textit{ad hoc} causes for business cycles, Harrod cited Pigou’s psychological interpretation advanced in his \textit{Industrial Fluctuations}.

So long as the equilibrium of output as a whole is regarded as stable, departures from it in one direction by conjoint action are essentially connected with the prevalence of error. The error is explained by an abnormal psychological condition. The continuance of recession or over-expansion depends on the persistence of error. Once the error is clearly seen the departure from the long-period position of stability is corrected. Other factors may indeed supervene upon the psychological one and be responsible for a longer duration of the departure than the psychological factor would alone account for. But to that extent the explanation of the cycle ceases to be a psychological one (ibid., p. 469).

If, on the contrary, one recognises that in conditions of imperfect competition and decreasing costs equilibrium is quite unstable, and that the economic system settles on different levels of output according to the state of confidence, the explanation of the cycle does not require reference to exogenous causes. Thus

\textit{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 \textit{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 \textit{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 (ibid., p. 470).}

Harrod thus thought that imperfect competition provided “the key […] for solving the mystery” (ibid., p. 465) of the cycle by introducing the (epistemically) necessary element of instability within the system. However, he soon realised that other approaches could be interpreted as pointing in the same direction.

A few months after “Doctrines of Imperfect Competition” was published, Harrod was engaged in correspondence with Haberler on a preliminary draft of Haberler’s inquiry on trade cycle theories commissioned by the League of Nations (Haberler 1934\textsuperscript{*}). Harrod complained that Haberler had not paid the due attention to Keynes’s \textit{Treatise on Money}, and argued that the importance of this work for trade cycle theory lay on the \textit{epistemic} ground, for it supplied an instability mechanism capable of explaining the cumulative departure from the equilibrium position:

\textit{The orthodox view is that if a market price diverges from a natural, forces are set up to bring the market price towards the natural. The case of interest is a}
special application. Ah, but in the case of credit, it may be objected, this tendency is obstructed if the banks artificially increase (or decrease) supply by their credit policy. But Keynes claims to show that the rates do not tend to converge even if the banks do not artificially alter the supply. 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 principle 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 supplied).

4. Imperfect Competition and Economic Dynamics

To appreciate the extent to which the research on imperfect competition influenced Harrod’s dynamics, we must first consider the premise Harrod posed at the foundation of his reflections on the trade cycle at a very early stage of his career, and to which he remained faithful at the moment he had developed the essential ingredients of his dynamic model.

Since his first surviving contribution on business fluctuations, an essay on “The Trade Cycle and the Theory of Distribution” read on 8 August 1925 before the annual meeting of Section F of the British Association for the Advancement of Science, Harrod stressed that the methodologically correct procedure in trade cycle theorising is “to explain with precision why the volume of production is at any time what it is” before inquiring on the causes of its variations:

Before plunging into an investigation of all the many alleged causes of the alternating expansion and shrinkage in the volume of trade, ought we not to ask: what in any case are the circumstances which cause the volume of trade to be what it is, neither more nor less? It is absurd to express surprise at the oscillation of an object, while ignorant of what the forces are which determine its movement or its stability (Harrod 1925*, p. 1).

Harrod insisted on this point in his more mature writings on dynamics. For instance, in 1934 he premised his discussion on imperfect competition and the trade cycle with the following words:

Ultimately trade cycle theory is concerned with the conditions which determine the equilibrium of the level of output as a whole in contradistinction to the particular equilibria of each industry which are determined by the demand and cost conditions of each. In a recession many particular disequilibria may set up, but the leading characteristic is a general running down of activity. It is the failure to understand precisely what factors determine this general equilibrium of output as a whole that is also responsible for perplexities concerning movements away from it or changes in it (Harrod 1934e, p. 465).

And again in The Trade Cycle:

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 1936a, p. 1).

In his 1925 essay, Harrod attributed the cause of the failure of classical economists to
explain the cycle precisely to their incapacity of satisfactorily answering this preliminary
question. On the contrary, Harrod thought that the marginalist approach of Jevons “and
the modern theory” “gave an outline of the answer to this question”21 (Harrod 1925*, p.
1).

Before proceeding further, it is important to notice that, once the problem is posed
in these terms, the choice of the partial equilibrium approach (for this, in the version
circulating in Cambridge in the mid-twenties, is what Harrod meant for ‘modern theory’)
as the key to the preliminary step of trade cycle theory was the natural one, given
Harrod’s theoretical formation (for an outline of Harrod’s career and his intellectual
lineage, see Introduction § 4). But on the other hand, the separation of two distinct and
subsequent stages -the determination of the level and of the causes of variation of output-
was not the ‘natural’ outcome of Harrod’s intellectual environment, nor was it the only
possible methodological choice. The ‘econometricians’, for instance, moved from a
different premise and therefore developed a dissimilar method of approach to the same
problem. It would be interesting to establish the origins of Harrod’s decision to adopt this
approach, but unfortunately his early essay offers no clues in this regard. Later, after
having gathered all the ingredients of his trade cycle model and having assembled them in
accordance with the principles resulting from this methodological premise, Harrod
insisted on the similarity between his own procedure and that adopted by physics (see
e.g. Harrod 1937a, p. 86; 1938a, pp. 402-403; 1939c, pp. 164-164) -although it must be
noted that he was not able to provide specific elements of analogy. I submit, however,
that this is more likely to be an ex-post rationalisation rather than a correct historical
account, and therefore I will leave the matter unsettled (for a more detailed discussion of
the analogy between economic dynamics and mechanics see below, Ch. VIII § 5).

Whatever its origin, it must be noted that the division into subsequent stages
constitutes a pre-analytical premise of Harrod’s dynamics, for it shaped at the outset the
posing of the problem of the cycle in the terms, peculiar to Harrod, which the following
Chapters of the present study aim at elucidating. For instance, Harrod’s insistence on the
compatibility of the phenomena of the trade cycle with the general theory of value and his
opposition on epistemic grounds to Pigou’s psychological theory of the cycle, were both
the outcome of this methodological premise.

As we have seen in Section 1 above, Harrod’s reflections on imperfect
competition aimed at extending to the exhaustive continuum of situations ranging from
monopoly to perfect competition the domain of application of the analytical tools thought
out to determine, given certain fundamental conditions (cost and utility functions), the
price and the level of output maximising the profit of individual entrepreneurs. The traditional theory of value, generalised by allowing different inclinations of the demand curve and by relating price and marginal costs in equilibrium by means of the elasticity of demand, thus reached the goal of determining the amount of production of the individual firm and of the industry. It thus constituted not only a preliminary stage to, but also the foundation of, any inquiry on the trade cycle, and therefore -in accordance with the methodological premise- it is not possible to do without it.

Having established the fundamental conditions on which the level of output depends, the determination of the causes of its fluctuations immediately ran into a different problem. In fact, two alternatives could be considered: trade oscillation could be attributed either to rhythmic fluctuations in the fundamental conditions, or to some other force superimposed to the operation of the fundamental conditions. In his 1925 essay, Harrod had tried the first line of attack, analysing the behaviour of population, technology and distribution of income in the course of the cycle. But by 1934 he had changed his mind, on the grounds of the epistemic argument, illustrated in Section 3 above, that the alternative approach would simply have shifted the need for an explanation to the causes of the periodic and regular fluctuations of the fundamental conditions. To make the second line of attack consistent with the methodological premise, it was necessary that the forces responsible for the oscillations of output were compatible with the forces determining the level of production. Therefore on the one hand Harrod investigated the possibility of accounting for the phenomena of the trade cycle by means of the ‘general theory of value’ (see Section 2 above), while on the other hand he intuited that in general terms the solution of the problem required, among the fundamental conditions, the presence of an element capable of counteracting any stabilising force that would keep the system permanently tied to a state of rest. Harrod’s instability principle (to which, in the form of the ‘knife edge’, Harrod’s dynamics owes most of its fame), before being an analytical result, therefore constitutes a postulate of his reasoning (this aspect will be further discussed in Ch. VIII, § 6, after having examined the working of Harrod’s models).

Harrod immediately drew some of the consequences of his chosen approach. In particular, he refused to see in maladjustments, time-lags or non-uniformity the ultimate cause of the cycle. He did not deny the possibility that “important and systematic deviations of this kind” (Harrod 1936b, p. 84) may play some part in determining the course of events, but insisted that the correct procedure to adopt consists in ignoring all ‘frictions’ and concentrating on the ‘pure’ postulates of the general theory of value. As we shall see, in this attitude lies the motive for Harrod’s rejection of Robertson’s ‘period analysis’ and of the ‘econometricians’ ‘time-lags theories of the cycle’ (see below, Chapters V § 3, and VI § 3).

Other consequences of Harrod’s approach emerged only later, when Harrod was able to solve the problem of the nature of the forces responsible for the variation of the
level of output and of the precise relationship between these and the determinants of the level of activity. Harrod in fact attributed different ontological, analytical, methodological and epistemic status to these groups of forces, and distinguished their domain of pertinence by different names: dynamics and statics. This aspect is of extreme importance for a discussion of the origins and early development of Harrod’s dynamics, for it lies at the heart of the very concept of ‘dynamics’. It will therefore deserve a careful examination, which will however be postponed to the Chapter V § 3, for before proceeding further it is necessary to understand how, in what order and in what context Harrod gathered the analytical ingredients of his trade cycle mechanism to be grafted onto his methodological and epistemic apparatus.

From this point on, the argument will follow more strictly the chronological steps of the development of Harrod’s dynamics. Thus, the next Chapter will be dedicated to the origin of Harrod’s interest in the notion of a uniformly progressing society.

Notes

1 The reader familiar with The Trade Cycle will have noticed that I am following here Harrod’s own method of exposition. To inquire on the rationale of such method is one of the aims of the present research, and its results will be fully apparent only with Chapter VIII, §§ 4 and 5.

2 By this term, Harrod himself meant the partial equilibrium approach as developed in Cambridge in the late 1920s (see Introduction, note 12). On the development of the Marshallian analysis of the firm into the theory of imperfect competition as elaborated by Robinson and Chamberlin, and on the establishment of Pigou’s equilibrium firm as the exclusive subject of analysis in the conventional theory of the firm, see Moss 1984 (on Joan Robinson’s theoretical filiation from Pigou’s Economics of Welfare, see also Loasby 1989, Chapter 5). This article provides the background for the present Chapter on Harrod’s contribution to the theory of imperfect competition, which indeed moved from Pigou’s 1928 article “An Analysis of Supply”.

3 See e.g. the editorial note to the papers presented at the Economic Journal symposium on “Increasing Returns and the Representative Firm” (Keynes 1930, p.79), the essays themselves, and the debate subsequently published in the same Journal in 1932-33 between Joan Robinson (1932 and 1933a), Shove (1933) and Harrod (1932 and 1933b). Harrod’s position will be discussed in Section 3 below.

4 In the 1972 preface of his collection of Economic Essays, Harrod claimed that he had established this result in the twenties, by conducting his own field work (Harrod 1972, p. viii). However, among Harrod’s surviving papers, I have not been able to find any documentary evidence of research specifically aimed at enquiring into the cost conditions of firms or industries. A memorandum on cartels written in November 1925 for the British (later Royal) Institute of International Affairs seems to provide some indirect evidence of Harrod’s early interest for this topic, for he stressed that “the problem of our export trade is that of contracted markets and excessive capacity”. However, his conclusion that “inefficient firms must be eliminated” suggests that he was thinking of excess capacity in terms of the presence of inefficient firms, rather than of firms working in equilibrium below their optimum regime (Harrod 1925a*). The latter became instead the problem under examination a few years later.

5 Harrod’s original denomination raised some dust on the part of scholars working in the field, who suggested alternative names; in particular, Viner affirmed to prefer his “own terminology, including ‘marginal gross revenue’ for [Harrod’s] ‘increment of aggregate demand’” (Viner to Harrod, 23 Feb., 1932), and Haberler suggested “something like ‘marginal demand curve’”, pointing out that “An ordinary demand curve is then a curve of average demand”. However, his conclusion that “inefficient firms must be eliminated” suggests that he was thinking of excess capacity in terms of the presence of inefficient firms, rather than of firms working in equilibrium below their optimum regime (Harrod 1925a*). The latter became instead the problem under examination a few years later.

6 Harrod’s original argument runs as follows:

The ordinary demand curve shows the price per unit at which a firm can find buyers for x units of product for all values of x. From this curve may be deduced another, which I propose to call the increment-of-demand curve and which shows the aggregate price that a firm can obtain for
Let \( x \) units of product less the aggregate price that it can obtain for \((x-1)\) units for all values of \( x \). Let \( DD' \) be a demand curve and let \( P \) be any point on it. Let \( PM \) be drawn perpendicular to the horizontal axis. Let \( p \) be a point on \( PM \) such that \( DpMO = \) the rectangle \( OP \). Then, \( DD'' \), the locus of the point \( p \) for all positions of \( P \), is the increment-of-demand curve as defined above. The output of a one-firm industry is shown by the point at which the marginal cost curve, \( SS' \), cuts the increment-of-demand curve. It is at once evident that if the output of a single firm were less than this, it would be profitable for it to expand, for the money return due to an extra unit of outlay would be greater than the outlay; if the output were greater than this the firm would be incurring a loss on the excess. The price per unit which a firm obtains is shown by the point on the demand curve directly above the point at which the marginal cost curve and the increment-of-demand curve intersect. The average cost curve is not relevant to this analysis (Harrod 1928*).

The formulation in the published version only differs in the wording: see Harrod 1930, pp. 238-9.

7 For a comparison of Harrod’s result with Yntema’s 1928 formulation, see Shackle 1967, pp. 29-34.

8 Harrod later returned on the subject in correspondence with Henderson: “I still think that the way in which the behavior of capitalists and monopolists is often contrasted is quite fallacious” (Harrod to Henderson, 23 February, 1936).

Maurizio Pugno, in his book on *Roy F. Harrod: From Dynamic Equilibrium to Cyclical Instability*, argued that Harrod “missed in part” (Pugno 1992, p. 47) the target of “proving that by substituting the assumption of perfect competition by the hypothesis of imperfect competition the results of the marginalist theory of partial economic equilibria may be preserved” (ibid., p. 31). Pugno’s opinion seems to contrast with the conclusion reached in the text, but the contradiction is only apparent. Pugno in fact was discussing the possibility of extending to imperfect competition “the fundamental results of the marginalist theory, e.g. the sovereignty of the consumers and the optimum distribution of resources” (ibid., p. 47, emphasis mine; on Harrod’s discussion of the problems relating to the distribution of resources, see Chapter II, §. 2). The purpose of the preceding discussion was rather different, for it aimed at showing that Harrod’s target was the *extension of the domain of application of the procedure and of the analytical instruments* of the partial equilibrium approach.

9 It may be interesting to notice that Joan Robinson as well, though independently of Harrod’s research, had a similar project of providing a general theory of competition: see Marcuzzo 1993.

10 After a first wave of interest for the trade cycle in the mid-twenties, Harrod became increasingly involved in the problem of business fluctuations after 1933. The various stages of his engagement will be examined below, in the Chapters II to IV, where chronologies of Harrod’s contributions are provided.

11 Here I am following the line of exposition of Harrod’s May 1936 article in the *Review of Economics and Statistics*. His view on the subject as expressed in *The Trade Cycle*, Chapter 1, however, remained unaltered.

12 The conditions for profits to fluctuate more than prices in the course of the cycle were the subject of a very long exchange of correspondence between Harrod and Meade, which took place in January and February 1936.

13 Since 1933, Harrod maintained that utilisation of plants, and thereby the possibility of operating at decreasing costs, may fluctuate considerably in the course of the cycle:

after a period of prolonged depression when firms are tending to reach a long-period equilibrium at a low level of output, prices and profits, long-period decreasing costs are more than usually likely to be present in firms that are in imperfect competition (Harrod 1933b, p. 340).
In his 1936 article on “Imperfect Competition and the Trade Cycle”, Harrod proposed a quantitative evaluation of the phenomenon:

In the slump, where there may be some fall in marginal work cost if the plant has been used above its normal capacity (with overtime, etc.) at the peak, no further reduction of marginal work cost is to be expected as the employment of the plant falls from 100 to 50 per cent of capacity or lower (Harrod 1936b, p. 86).

It is possible, however, to find very early reference to changes in the degree of utilisation of plants: in 1924, Harrod concluded an essay on “Interest and Ideal Price” by noticing that

It is useless to build factories, unless they are to be worked. If they lie idle, then indeed is the return to the initial effort of building them unduly postponed. But except during boom periods factories built during boom periods are not likely to be fully worked; the factories had better not have been built on such a scale (Harrod 1924*).

Curiously enough, Harrod ignored these considerations when posing the acceleration principle at the heart of his trade cycle theory. The ‘Relation’, in fact, postulated a rigid relationship between increase of demand for consumption goods and the requirement of new machinery to satisfy it (for a discussion of the place of the accelerator in Harrod’s mechanism, see below Chapter VII § 4). This, of course, tacitly assumed that the productive capacity is used at constant intensity (Tinbergen 1938, p. 166). Needless to say, such assumption was judged to be “excessively gloomy” (Gaitskell 1937, p. 474).

Reference to Sraffa is explicit throughout Harrod 1931. Harrod’s and Sraffa’s attitudes towards traditional theory will be compared below, Chapter VIII § 2.

As soon as he realised that in reaction to his article scholars attempted to extend the use of the Marshallian toolbox to the case of imperfect competition, Sraffa forcefully stressed that the upshot of his criticism did not regard the realism but the consistency of the Marshallian approach, which in his view was to be abandoned altogether (Sraffa 1930, p. 93). Thereafter, he refused to pronounce a further word on imperfect competition theory.

This conclusion was the subject of an articulated debate with Joan Robinson, who argued that in conditions of long-period full equilibrium, average costs for the individual firm not only may be falling, but must be falling (Robinson 1932, p. 549). Harrod’s reply, in private correspondence and in print, stressed that Robinson’s result depended on her peculiar notion of ‘normal profit’ associated to long-period equilibrium; he proposed a new definition of his own, and insisted on the possibility, rather than the necessity, that the firm is subject to decreasing costs (Harrod to Robinson, und. -but mid-march 1933; the intense correspondence on this subject protracted to the end of March. See also Harrod 1933b, p. 340).

Harrod’s argument was long-winded and somewhat involved. I will therefore only expound it in its broad outline, referring the interested reader back to Harrod 1934e, pp. 465-7 for the details.

Harrod affirmed that “the argument from a two industry situation may be extended by like reasoning to the situation in which there are a large number of industries” (Harrod 1934e, p. 467).

The idea that increasing returns eliminates the limits to the process of expansion was brought forward by Allyn Young a few years earlier (Young 1928, especially pp. 533-4).

This problem was also posed in similar terms by Löwe in 1926, in an article in German in which he asked how is trade cycle theory altogether possible (Löwe 1926). Although Löwe’s influence indirectly extended to the English language area via his influence on Hayek’s writings of the late 1920s and early 1930s (see Hagemann 1994), and in spite of an extensive discussion by Kuznets, who also summarised Löwe’s criticism to Pigou (Kuznets 1930a, pp. 387-390; a reference was also provided in Kuznets 1930, p. 128), I doubt Harrod was aware of it. In any case, his solution radically differed (both at this early stage, and later) from Löwe’s, who sought in technical progress the endogenous factor making for fluctuations.

The contradiction between the fact of the business cycle and the stability of static equilibrium was also pointed out by other authors, in particular by Henry Schultz (1931, p. 68*). See also, later on, the reflections of Domar 1952, p. 479.

In his 1936 Book, Harrod argued along similar lines that “Happily we do know broadly why man engages in economic activity” (Harrod 1936a, p. 1). The treatment of the Trade Cycle will be discussed in Chapter V § 1 below.

In The Trade Cycle, Harrod contrasted as follows the two possible approaches with regard to the role of money -which, as we shall see in Chapter V § 1, was the factor balancing the stabilising effect of the other forces in play:
Two views, both consistent with our account, may be taken with regard to the role of money. (i) The first is that money is the original and actuating cause of the cyclical movement; put otherwise this says that the value of the monetary determinant undergoes spontaneous and autonomous variations and that these variations occur alternately in either direction and impart to the general movements of output their cyclical character. (ii) The other view is that money is a passive accomplice in the generation of the cycle, moving up or down, within certain limits, to suit those forces [...] which determine the cyclical movement (Harrod 1936a, pp. 46-47).

Harrod eventually moved towards the second view.

23 A comment on Joan Robinson’s discussion of how different types of changes in demand would influence the price level provides an example of Harrod’s attitude arising in connection with imperfect competition. Joan Robinson considered three examples of changes in demand which would, respectively, leave prices unaltered, cause raising prices to determine a drop. The last is the case of interest, and therefore deserves full quotation:

[a price fall] would occur if the increase in demand were spread evenly over the whole market -for instance, by a uniform increase in the density of population- and if the new firms were set up, so to speak, in between the old firms (either geographically or in respect of special qualities which appeal in various degree to different customers). The difference, from the point of view of buyers, between any one firm and the next would thus be reduced, the customers of each firm would become more indifferent, and the elasticity of demand would be increased (Robinson 1932, p. 553).

Harrod considered this third case as “the most frequent” and “most probable”, and affirmed that “it is this third case clearly that is relevant to Trade Cycle analysis, and I believe that this cost analysis has got to be brought in to make the Trade Cycle analysis complete” (Harrod to Robinson, 23 March, 1933). Why is this sort of analysis so relevant to the trade cycle analysis? Certainly not for its conclusion as to the behaviour of the elasticity of the demand curve, for Harrod maintained the opposite variation to be more probable (see Section 2 above). Harrod must therefore have been referring to the ‘even’ and ‘uniform’ spreading of the increased demand, which was seen as the ‘normal’ kind of raise of demand which occurs in the upward phase of the cycle, by contrast to the other cases considered by Joan Robinson which were characterised by increases in demand only regarding a particular fringe of customers.
The last years of the big slump and the first few years of the recovery saw Harrod engaged on several fronts. On the one hand, he took part in the public debate on the appropriate policy to lead Britain and the world out of the depression; on the other hand, he was writing his *International Economics*, which gave him the chance to discuss the domestic and international consequences of monetary policies; and finally, he took part in some of the activities of the New Fabian Research Bureau.

In these years Harrod did not produce any theoretical work specifically on economic dynamics; fortunately, however, his policy writings throw some light on the state of his thinking and it is therefore useful to examine them with some care. In the first Section, I therefore illustrate some features of Harrod’s early approach to trade cycle which also characterised his diagnosis and proposed remedies to the depression of the early 1930s, while in the second Section I discuss Harrod’s revived interest in the trade cycle from mid-1933.

Harrod’s policy proposals were not particularly original, for he shared the opinion that the appropriate banking policy consisted in injecting money into the system so as to reduce the rate of interest and stimulate investment up to the current rate of saving of the community; otherwise a cumulative process of deflation would ensue, preventing the gap between saving and investment to be filled. However, Harrod thought that if -as it was the case- the recession had already progressed too far, a cheap money policy would not suffice to set the “ball rolling”, and therefore advocated resolute and explicit action on the part of the government, to be financed by a budget deficit.

Harrod’s discussion of these themes, especially from the second part of 1933, shows that he was struggling to provide a conceptual framework for his original intuitions, which culminated in the awareness of the necessity of incorporating the partial analysis of depression within a more general theoretical conception. However, whilst he accepted and propagated Keynes’s policy advice, his conclusions were based on a rather different kind of reasoning. Moreover, Harrod failed to catch up immediately with the evolution of Keynes’s thought towards the doctrine of effective demand. This is the clue for understanding the preliminary phase of Harrod’s dynamics. At first, he tried to find a satisfactory line of approach to the problem of the trade cycle; at a later stage, he gathered the conceptual and analytical instruments which eventually allowed him to work out the theory presented in his 1936 book (the latter developments will be discussed in the next Chapter). On the one hand, Harrod was dissatisfied with the ‘monetary’ approach to the problems of unemployment and the cycle. He perceived that the quantity of money
actually circulating is not equivalent to the amount made available by banking policy and that the essential factors governing it are the willingness and the opportunities to spend it. On the other hand, Harrod was not able to appreciate the full implications of the doctrine of effective demand until at least the beginning of 1935 (but probably not until he read the *General Theory* proofs several months later), so that he struggled to express his convictions in terms of the divergence between saving and investment in a set-up more akin to Robertson¹ (and at places to Hayek) than to Keynes.

The same difficulty also affected Harrod’s approach to long-period growth. This notion was discussed in the circle of the New Fabians both as a realistic assumption characterising capitalistic economies and as a desideratum for socialist planning. After exposing the relationship linking Harrod to the New Fabian Research Bureau, I will thus show in Section 3 that Harrod conceived of the cycle as occurring along a trend line exogenously determined by population and efficiency growth, but also that he was not yet in condition to master the instruments that later enabled him to formulate a self-concluded theory of growth cycles.

In short, this Chapter therefore aims at illustrating (i) how Harrod’s taking part in the debates on how to boost the economy after the depression led him to revive his interest in the trade cycle, (ii) how he interpreted these problems in terms of money and price fluctuations -failing to grasp, at the time, the Keynesian shift towards quantities and output-, (iii) how he failed to provide a consistent theory but only managed to experiment with tools which he himself later found quite unsuitable for the task, and finally (iv) how he perceived the cycle to be intimately connected with economic progress, although again the approach in terms of prices and efficiency prevented him to integrate these two phenomena into an endogenous cyclical growth theory.

1. Cheap Money and Reflation

In Chapter 1 I have cited an essay on “Trade cycle and the Theory of Distribution” Harrod wrote in 1925, in which he attributed the causes of the trade cycle to cyclical fluctuations in the ‘fundamental conditions’ determining the level of output, in particular population, technology and the distribution of income (Harrod 1925*). In 1927 he returned to the subject in a series of three memoranda on the trade cycle, prices and foreign exchange in relation to unemployment (Harrod 1927c*, 1927b*, 1927a* respectively) addressed to the General Federation of the Trade Unions, with which he was associated (through the auspices of MacGregor) in the “effort to scientifically examine the causes of unemployment and possible remedies” (GFTU to Harrod, 23 Oct. 1926). The conclusion Harrod reached in both these early attempts was that the value of money fluctuates in accordance with the changes in the general level of activity, these oscillations determining rising prices in prosperity and falling prices during depressions. However, he failed to draw a causal connection between fluctuating prices and the trade cycle, but noted that the alternating rise and fall of prices is due to, but at the same time
aggravates, the alternating expansion and contraction of trade (1927c*, p. 8). Moreover, Harrod’s early approach to trade fluctuations was characterized by the incapacity of constructing a proper model of the cycle: the mutual implication of falling (rising) prices and contracting (expanding) output was capable of accounting for a cumulative depression (boom), but there was no mechanism describing turning points. As we shall see in Section 2 below, this difficulty persisted in Harrod’s successive attempts to deal with the problem, while the more general question of the relationship between the value of money and the cycle was not satisfactorily solved even in Harrod’s 1936 book (see below, Chapter V § 6.).

However, Harrod concluded -along the lines suggested by Keynes’s Tract (Keynes CW IV, pp. 35-36)- that the remedy must consist in breaking this cumulative mechanism by adopting some measure to stabilise the value of money (Harrod 1927b*, pp. 8-9). Although for several years to come he did not attempt to further discuss the trade cycle, when in the early 1930s Harrod took part in the debates on the causes of unemployment and the monetary policy suitable to drive the country and the world out of the big slump, he resumed and refined the arguments on money and prices he had developed in these earlier essays.

Between June 1932 and the end of 1934, Harrod was deeply engaged in the public debate on monetary theory. He wrote several letters and articles for newspapers (e.g., Harrod 1932a-c, 1933d, 1934b-c), he secured signatures of eminent economists for collective manifestoes on the most appropriate policy for stimulating recovery (Harrod, Meade et. al., 1932 and 1933), he personally tried to suggest to influential politicians a line of action (Harrod to Runcimann, 10 and 22 June, 6 July 1932, and 3 March 1933; Harrod 1933a*), he provided some learned contributions (Harrod 1933e and 1933f), and also in his book on *International Economics* he discussed means and objectives of a policy for stabilizing the value of money (1933c, Chapter VI).

It is not necessary here to discuss these contributions in detail, but it is sufficient to point out that his conclusion that cheap money policies were not enough to boost recovery, and his advocacy for public works financed by an unbalanced budget, did not differ much in their broad lines from Keynes’s advice. Two aspects of Harrod’s argument, however, are important for understanding the approach he took when his interest for the trade cycle revived in 1933. In the first place, in Harrod’s view the objective of government expenditure should not be the direct creation of employment, but pressure on the price level. In the course of the whole debate, Harrod maintained that the immediate aim of government policy should be to reflate until prices returned to the level of 1928 or 1929. For instance, in a memorandum written in March 1933, Harrod claimed that

The policy proposed is not merely to use relief works as a mean of giving employment (which has been probably rightly criticized as an expensive and
wasteful method), but to use them as a device for raising the level of commodity prices or the flow of monetary demand (Harrod 1933a*).

Thus, while Keynes was moving from the Treatise to the General Theory framework, it must be stressed that Harrod was still thinking in terms of prices and costs rather than of quantity adjustments. In other words, in spite of some exchanges of views with Keynes and his closest circle in that period, Harrod does not seem to have been aware, or to have appreciate the importance, of the shift of emphasis characterising the evolution of Keynes’s thought between 1931 -when, under the pressure of the Cambridge Circus, he started thinking over his own approach- and the spring of 1933, when the theory of effective demand was worked out and made public in its broad lines. This point must be kept in mind, for only after having read the General Theory in proofs during the summer of 1935 Harrod managed to gather the analytical elements which enabled him to assemble a proper model of the trade cycle (see Chapter IV § 2 below).

In the second place, Harrod -like several of his colleagues- doubted that cheap money alone would be sufficient to stimulate recovery: in correspondence with Runcimann, he stressed that

It is probable that the situation is much too severe for an ordinary easy money policy to lead to an expansion of credit via encouragement of new capital undertakings. That would have been all right much earlier on, for instance, if we had gone for reflation as soon as we left the gold standard. (Instead we were mislead by the notion that external depreciation was a substitute for internal expansion of credit) (Harrod to Runcimann, 22 June, 1932).

And again, a few days later:

Since the banks can only increase [the money] supply by loans, and since, owing to present conditions, the effective additional demand for short commercial and industrial loans is nil, the government must be willing to borrow sufficiently from the banks to enable them to increase the quantity of circulating medium (viz. bank deposits) till the value of it is reduced to the required level (Harrod to Runcimann, 6 July, 1932).

He thus concluded that further action in support of cheap money had to be taken, and took up the position -which was rather radical, at that time- that “Reflation means unbalancing the budget”: “There is not likely to be any other way of getting reflation now” (Harrod to Runcimann, 10 June, 1932). The first aspect Harrod considered was confidence in the resoluteness of the government to raise prices, in opposition to the prevailing pessimistic sentiment (see also Harrod 1932c). As supplementary reasons for doubting that a reduction of the rate of interest would in itself be sufficient to revert the tide, later Harrod also submitted that in the course of a deep depression there might be “such an excess of productive capacity in the form of plant already existing, that the demand for new capital plant cannot be much expanded by a low interest rate” (Harrod et al. to Roosevelt, 20 Nov., 1933; see also Harrod 1933d and 1933f, p. 581), and that

the value of capital goods depends on the future demand for the consumable goods, the production of which the capital is designed to facilitate. After a prolonged depression producers will be more than usually sceptical about
future prospects. Enterprise directed towards the production of capital goods is thus inhibited (Harrod 1933f, p. 581).

This aspect is also to be kept in mind, because Harrod’s doubts as to the links between low interest rates and investment decisions will prove to be a theme characterising all his subsequent dynamic models (see below, Chs. V § 5 and VII § 4).

2. Unemployment and the Trade Cycle

During Spring or mid-1933, Harrod revived his interest in trade cycle theory. He argued that the discussion of unemployment and depression should be subsumed under the more general topic of the trade cycle.

Harrod maintained that while economics had successfully dealt with the “old economic problem” of determining the best way in which the physical resources are to be distributed among different occupations and the products are to be distributed among the participants to the production, a “new economic problem” had to be considered:

the specifically modern problem is that of unemployment. Unemployment is normally thought of in terms of labour, but in this context it may well be used in the broader sense of the failure of productive resources generally to be fully utilized. Factories lie idle; new capital cannot find channels of investment; the cultivation of soil is restricted (Harrod 1933*, p. 10).

While, according to Harrod, economists have been able to provide a satisfactory solution to the old problem, there is nothing in the classical treatment preventing part of the productive resources not to be utilised at all. Harrod in fact pointed out that the traditional argument that unemployment would be absorbed if redundant factors of production offered themselves at a lower price, was based on a “fallacious transition from the part to the whole”: it is true that a wage reduction for the labourers in some sector of activity would induce “a transfer of demand from other commodities to that which they produce”, but “this principle cannot be generalised”:

for if all classes of labour reduce their offer price there will be no transfer. The gain of employment of the cheaper class was due to the transfer; if there is no transfer, no class of labour gains additional employment (ibid., p. 12).

Moreover, on the ground of the standard approach to the unemployment problem one cannot even argue that a reduction of wages would increase the employment of labour at the expense of some other factor:

Factors of production constitute the consumers. Their incomes shrink as the price they offer for their services is reduced. The market for goods shrinks in proportion to the cost of making them and is therefore incapable of absorbing more goods in the aggregate when they are offered at a lower price. The law of demand only applies to the market for one commodity or group of commodities and cannot be applied, without fallacy, to the market for goods in general (ibid., pp. 12-13).

In Harrod’s view, the new economic problem acquired relevance as the trade cycle became steadily more marked as capitalism grew. Harrod offered a tentative explanation of the increasing amplitude of the fluctuations by suggesting that independent
producers are replaced by capitalist production. While a man producing for his own consumption or to sell on his own account would work harder to compensate the loss ensuing from a decline in his market, “the capitalist may find it best to meet [a recession of demand] by turning off hands”\textsuperscript{10} (ibid., p. 13a). From “this doctrine of the ineluctable advance of the Trade Cycle in a system of un-controlled private enterprise” (ibid., p. 33), Harrod concluded that

The problem of securing full employment […] becomes merged into the more general one of preventing industrial recession and eliminating the Trade Cycle. The broader problem will require measures of the same general nature as the immediate one (ibid., p. 34).

Although the above passage is strictly confined to the question of remedies, the general drift of the argument implies the opportunity of a theoretical subsumption of unemployment under the ‘broader problem’ of the trade cycle. As to the development of Harrod’s dynamics, this commitment -generic as it was- is quite important, for it indicates the new direction his research was going to take. It is important to stress that this choice brought Harrod to emphasise some features of the slump rather than others. On the one hand, the approach of the \textit{cycle} gives pre-eminence to the \textit{regularity} and \textit{repetitiveness} of the events and to the necessary alternation of advance and recession over the violence and suddenness of the crisis\textsuperscript{11} (for a discussion of these alternative attitudes towards cycle and crisis, see De Vecchi 1983, pp. 263-6, 281-290, and 291-2). In this perspective, depressions are conceived as \textit{phases} within fluctuations. On the other hand, this brings to consider them as necessary events, constituting the \textit{norm} rather than the exception, and this feature of trade cycle theories distinguishes Harrod’s approach from the view that depressions are the expression of accidents, frictions or mismanagements.

The return of Harrod’s interest for the trade cycle, after a few years during which his mind was mainly occupied with imperfect competition theory\textsuperscript{12}, seems to have gone through four stages. At first, Harrod saw the cycle in terms of the old \textit{Tract of Monetary Reform}’s argument that differences in the resilience between different sets of prices causes some maladjustments in the distribution of income. In the second place, Harrod reflected on the cumulative nature of the processes of deviation from equilibrium. Thirdly, he found himself dissatisfied with the existing approaches to the cycle, but at the same time he was unable to provide an alternative. This originated Harrod’s attempt at tinkering with some ideas he found here and there, especially in the works of Keynes and Robertson, with the result that his commitment in favour of a theory of the cycle was accompanied by still quite vague ideas as to the actual procedure and the concrete modelling of the phenomenon. In particular, in Harrod’s writings before the spring of 1935 there was no trace of the analytical instruments which were going to characterise the developed stages of his dynamics, while the few elements of explanation of the phenomenon Harrod was able to put forward remained unconnected. As we shall see in more detail in the next Chapter, the final stage -the completion of a theory of the trade
cycle- was only accomplished after Harrod had learned from Haberler of the possibility of applying the acceleration principle to the study of fluctuations, and after he had read the *General Theory* in proofs and fully understood the implications of the multiplier.

One of the starting points of Harrod’s reflection was thus the argument that a fall in prices brings about maladjustments, because some prices are more flexible than others; in particular, the price of labour is especially sticky, implying that a drop in the price of goods is accompanied by a rise of costs and a corresponding fall of profits. By contrast with this diagnosis of the evils of the crisis, Harrod drew the suggestion of the remedy:

The progress of the crisis is continually confirming the view that the most serious evil from which we are now suffering is the great fall in wholesale prices of the last two and a half years. This has brought about serious maladjustments throughout the economic system, owing to the fact that some prices move readily under the influence of supply and demand, while others are relatively inflexible. The most practicable remedy for this situation is to operate upon the prices which are adjustable; these should be raised until they bear the same relation to the fixed prices as they bore at the outset of the crisis (Harrod, Meade et al., 1932; see also Harrod 1932c).

A few months later, Harrod suggested that the process of deflation (and, symmetrically, of rising prices) is of a *cumulative* nature:

at some point the demand for consumable goods fell below the supply of them and the level of prices fell without costs of production falling pari passu.

In consequence, much production that was formerly profitable became unprofitable, and output and consequently income were curtailed. This curtailment of income involved a further and severer decline in the demand for goods, and falling output, falling income, and falling demand chased each other in a vicious circle. Yet the gap between prices and costs remains (Harrod 1933d).

Although this argument was not immediately developed into a thorough model, it deserves detailed consideration, for it provides the only specific (though fragmentary) mechanism Harrod proposed before his *Trade Cycle*. First, it must be noted that Harrod did not expound all the consequences of the causal chain on the price level, for he failed to remark that the drop of output would imply not only a falling income but also a reduction of supply. For the argument to be valid, therefore, Harrod ought to have proved that the fall in supply is not sufficient to offset the drop in demand and thereby stop the deflationary process.

In the second place, it is necessary to observe that the reference to a cumulative process of reproduction of the cause of movement was not yet accompanied by an epistemic argument as to its necessity for an explanation of the cycle. This only came in 1934 (for a discussion see Chapter I, § 3); at this preliminary stage of his research Harrod was probably only tinkering with the few tools at his disposal, without following specific criteria as to the global structure of a theory of the cycle.

In the third place, this mechanism is incapable of determining turning points: one cannot see whether nor where the cumulative process should end, and indeed Harrod
apparently did not trust much in the existence of forces capable of generating revival after a depression:

In the past there has been some confidence that, while unfettered private enterprise is wasteful in allowing lapses away from full employment to occur, it does provide a mechanism of recovery, that expansion follows depression by some sort of inner law. The nature and even the existence of this law is unknown, but it may none the less be at work (Harrod 1933*, pp. 32-33).

If interpreted literally, this rather pessimistic assertion would stand in the way of the construction of a theory of the cycle, for oscillations require an alternation of opposite phases. In reality the purpose of Harrod’s remark was rhetoric, for it was meant to contrast a laissez-faire attitude:

These considerations do not take the force out of my argument. Rather they add to it. […Since] we do not know that the recovery will come of its own and hardly have greater grounds for believing that full recovery will come than that it will not, and since, if it does not, prognostication of disaster are likely to be fulfilled, it is essential to act as if recovery would not come of its own (ibid., p. 33).

In the essay I am considering and elsewhere, Harrod rather stressed the element of regularity of the cycle (ibid., p. 14). However, the lack of turning points reveals Harrod’s hesitation as to the procedure to follow. On the one hand, he was not satisfied with the existing treatments of the trade cycle:

Study of the trade cycle has been intensively pursued by economists since the war. The perplexities of this subject are directly connected with the lacuna in the old economic theory already mentioned. Just because they were ignorant what the forces were which would keep the economic system in a full employment position in a regime of laissez-faire and whether there were any such forces, economists were at a loss where to look for explanations of the movement to and from such a position. Indeed the old economics not only fails to explain if there are forces tending towards a full employment position, but gives no clue as to what kind of forces are required to fulfil this function. If these things had been known, trade cycle analysis would have been greatly facilitated (ibid., pp. 14-15).

In International Economics, Harrod expressed similar doubts:

Of the causes of this movement [the trade cycle] little is known, though of the interaction of forces which occur when the movement manifests itself something is understood. Any attempt to summarize the theories of the subject which have been put forward would be too bulky for insertion in this book (Harrod 1933c, p. 160; see also p. 132).

Harrod was also doubtful […] whether a revolution in the more fundamental parts of [the study of the conditions of economic change] can be effected as easily as he [H. V. Hodson, in Economics of a Changing World] hopes. Old theories may be re-dressed in new words, but the considerations on which they are based and their scope and validity are not altered thereby (Harrod 1933g, p. 694).

On the other hand, Harrod did not seem able to organise an alternative theoretical study of the cycle. Although in several essays he threw out some hints as to his ideas, he did not produce a systematic treatment of the subject. In some places he indicated some
possible causes of the deviation from equilibrium which could be responsible for setting
in motion the cumulative mechanism of the depression, but without inquiring on the
ultimate reason why these causes should occur. In other places, he discussed the same
phenomena in terms of banking policy, but without pointing out the connections he saw
between these different pieces of explanation. The reading of Harrod’s 1933 essays and
newspaper articles suggests that he was exploring the pathways opened by his
contemporaries, Keynes (Treatise on Money) and Robertson in particular, and that he
was using several fragments of their theories -however heterogeneous they were- in
support of his policy advice. The result was somewhat confused and sometimes also
inconsistent, but the reader ought to keep in mind that Harrod’s scope at the time was not
primarily theoretical, but that he was mainly concerned with influencing public opinion in
favour of a resolute policy of Government expenditure.

Harrod seemed to have taken as a starting point for his considerations the
available evidence regarding the behaviour of prices in the course of the cycle:

It is known from historical survey that the fluctuations in general economic
activity have in fact been associated with changes in the value of money,
advances generally with a fall and recession with a rise. Secondly, when changes
in the value of money occur, all prices do not move equally. […] It seems
reasonable therefore to suppose that the changes in the value of money are not
unconnected with the disturbances of the trade cycle

We have seen that the cumulative property of the mechanism proposed by Harrod
resulted from the circularity of the causal chain linking the drop of demand below supply
to prices falling below costs, to falling profits, falling output, falling income and again to
falling demand. Yet, several passages suggest that in Harrod’s mind the causal circle in
reality had a beginning. In fact, he seemed to think that the whole movement springs out
of a change in the value of money, and thereafter feeds on itself. The burden of the
explanation thus fell on this originating cause, and accordingly Harrod’s diagnosis of the
depression stressed the role of monetary disorder: he saw the trade cycle as an essentially
monetary phenomenon, and consistently thought its solution had to be sought in
monetary policy: “Monetary policy is the appropriate instrument for securing full
utilization of productive resources”

For the system to function properly […] it is essential that the total stream of
money coming into the market for the purchase of [the entrepreneurs’] goods
should bear some definite relation to the total expenses they incur in the
production (Harrod 1933f, p. 578).

Harrod maintained that “there is something bringing about a balance” between the
stream of money into circulation and the need for transactions, namely the fact that
the money paid out goes into someone’s pocket and ultimately into someone’s pocket as income. And income tends to be spent. So that all the money paid out as expenses of production should come round again and constitute a monetary demand for the goods produced (Harrod 1933f, p. 578).

However, he pointed out that several causes may produce a disequilibrium between supply and demand for money. In particular, he mentioned hoarding and a gap between saving and investment\textsuperscript{17}. Unfortunately, in neither case did he provide a detailed discussion of the working of the mechanism at the outset of a depression.

Several passages suggest that Harrod thought that people’s hoarding side-tracks money from effective circulation:

There will be a break in the sequence of events if some of the income receivers hoard their money […]. [T]he hoarding of money does tend to produce a deflation of profits, and its brisker turnover tends to produce an inflation\textsuperscript{18} (Harrod 1933f, pp. 578-579).

Harrod seemed to intend ‘hoarding’ as the volume of inactive deposits in the banking system, or as the total quantity of money minus the active circulation. Harrod’s point, in fact, was that what matters is not the supply of money, but the quantity of money actually circulating -the monetary demand for goods\textsuperscript{19}. The same applies in the case of a gap between saving and investment:

So long as the amount of money withheld from expenditure is equal to the amount of productive expenses devoted to the addition to capital goods, the residue of income, namely, that going to make up the monetary demand for commodities should be sufficient to carry the expenses of making those commodities, together with a normal rate of profit. But if savings tend to exceed the productive expenses devoted to capital output, the flow of money coming for the purchase of consumable goods will become inadequate, prices will tend to fall, profits become deflated. The clear exposition of this relation and its multifarious consequences is the work of Mr. Keynes (Harrod 1933f, p. 579).

In Harrod’s view, the root of the trouble thus consists in the failure of money to circulate actively, which sets in motion the cumulative mechanism of falling prices, falling output and worsening perspectives. Accordingly, he suggested as a remedy that in occasion of deep depressions the government should directly provide monetary demand for goods rather than simply rely on private expenditure stimulated by cheap money: “if fear is […] so deeply rooted that private deflation (i.e. unwillingness to circulate the beastly stuff) is very intractable” (Harrod to Runcimann, 6 July, 1932), a mere increase in the supply of medium would be ineffective for it would be “held in idleness by the public, in the absence of profitable forms of investment” (Harrod 1933e, p. 118).

In spite of his critical remarks regarding the effectiveness of monetary policy to recover from severe depressions\textsuperscript{20}, Harrod hinted at the implications of manoeuvres on the rate of interest on both the capital market and the monetary requirements of the community, and recognised that the interaction between these effects lies at “the heart of the problem of the trade cycle” (Harrod 1933c, pp. 131-2). Unfortunately for the present inquiry, in this essay Harrod’s main interest was in the effect of monetary policy on the
Harrod described the banking system as performing two distinct but closely interrelated functions: on the one hand it provides the effective circulating medium for the community, and on the other hand it provides the capital market with funds (Harrod 1933e, p. 101, and 1933c, p. 131). As to the first of these functions, Harrod maintained that money (in the form of notes or deposits) comes into existence when someone acquires a claim on a bank, which happens either when an individual brings gold to the bank, or when the bank gives him a loan (Harrod 1933e). Harrod was particularly eager to stress that bank loans create new circulating medium (ibid., p. 99), and that therefore “The volume of deposits is governed, in the first instance at any rate, not by the thrift and industry of individual members of the community, but by the loan policy of the banks” (ibid., p. 103). The interconnection of the two functions of the banking system is therefore evident, for the emission of a loan -destined to the capital market- is also an addition to the circulating medium (ibid., p. 101).

The Central Bank operates through the control of the rate of interest. In Harrod’s view, its ideal policy for an entirely isolated community should be to maintain such interest rates (and such a consequential expansion of loans) as would encourage additions to real capital to proceed at a rate that kept in line with the collective saving of the community (Harrod 1933e, p. 116; for the conditions regarding an open community, see 1933c, p. 132).

According to Harrod, if the banking system manages to secure the equilibrium rate of interest for the system, “the banking policy appropriate to its monetary requirements […] should not be inconsistent with that required to secure an equilibrium rate of output of new capital goods” (Harrod 1933c, p. 132). This claim was not explicitly justified, but from Harrod’s comments on disequilibrium it seems that the flow of new loans, by enabling investment to keep at the same pace with saving, should compensate the monetary demand for goods which is deflected from consumption. Harrod did not discuss the saving-investment disequilibrium as a process, but only described the conditions characterising it and its immediate consequences; but even if Harrod did not, as it would appear natural, place money at the connection point between his arguments on prices, costs and profits on the one hand, and on saving and investment on the other, his reflections on saving and investment are none the less worth examining for their relevance in evaluating the state of Harrod’s dynamics at the end of 1933.

Harrod described the disequilibrium between saving and investment in terms of ‘inflation’ and ‘deflation’:
Capital production can only be pushed ahead more rapidly than [the collective saving of the community], by the imposition of forced saving on the community through rising prices. If production already bears a reasonable relation to the capacity of the country, so that employment is good, rising prices lead to excessive profit and conditions become “inflationary”. If on the other hand capital production fails to keep pace with the saving of the community, insufficient money will be to hand to buy consumable goods, prices will tend to fall, their output will be restricted and unemployment will grow apace. This is the condition of deflation. It should be the business of the central bank to steer between these two extremes (Harrod 1933e, pp. 116-7).

This passage again attributes the cause of the slump to the monetary factor: if the bank’s rate of interest is higher than the rate which would keep investment in line with the saving of the community, investment falls short of saving; the stream of money deflected from immediate consumption is not fully compensated by the construction of new capital equipment, so that there is not enough money in circulation, and prices will drop. This presumably sets in motion the cumulative mechanism described above; and one may also suppose that the fall of prices persists until the banks’ rate is brought down to the appropriate level by a specific policy decision on the part of the Central Bank (given Harrod’s provisos as to the limits of the possibility of actually circulating money by this means if the depression is too severe). But Harrod did not explicitly draw this inference, and his reader is left wondering whether Harrod referred to the saving-investment-rate of interest mechanism for lack of alternative theoretical frameworks for discussing the business fluctuations, or for positive acceptance of its premises and implications. Both answers may contain a germ of truth. On the one hand, I have already stressed that Harrod expressed his distrust in the existing approaches to the cycle, and moreover one must consider -as we shall see in Chapter III, § 6- that for at least another full year, up to the beginning of 1935, Harrod was desperately and unsuccessfully trying to formulate a valid alternative. On the other hand, as it will become evident in Section 3 of this Chapter, Harrod’s reflections on the long-period equilibrium of a progressive society explicitly referred to the interest and prices mechanisms. It is therefore important to pause on the specificity of such approach.

In the first place, it presupposes two conditions. On the one hand, saving and investment decisions must be independent of each other; in this, Harrod was following the line of thought brought to the forefront in Britain by Robertson’s Banking Policy and the Price Level, and pursued in Keynes’s Treatise on Money and by Meade in The Rate of Interest in a Progressive Society. On the other hand, it requires the absence of any automatic mechanism bringing them to balance. The determinants of saving decisions were not specified, and it is therefore not known whether or not Harrod thought them to be dependent on the rate of interest. However, in the years we are considering here he never seems to have thought in terms of interest as the price balancing saving and investment decisions, while he reasoned instead in terms of the rate determined by the explicit policy of the Central Bank. In Harrod’s view, equilibrium between saving and investment on the one hand, and the rate of interest on the other, do not depend on market
mechanisms, but entirely on the banking policy. Moreover, the “rate at which the community collectively wishes to save” was treated as the autonomous variable, determining the ideal pace of accumulation of capital which the banking policy should try to pursue (Harrod 1933e, p. 120).

The reader has surely recognised in Harrod’s discussion of the depression as a monetary phenomenon, several elements deriving from Keynes’s Treatise on Money or from Robertson’s analysis of saving and hoarding. It is necessary to stress at this point that at this juncture Harrod had not yet appreciated the significance of the shift of emphasis which had taken place in the meantime in Keynes’s thought. By April 1933 at the latest, when The Means to Prosperity and “The Multiplier” were published, Keynes had already outlined in print his principle of effective demand, which implied the reversal of the causal order between saving and investment, the latter no longer being the ‘tail’ but the ‘dog’ wagging it. In spite of having certainly read Keynes’s articles when they appeared in The Times, Harrod kept maintaining that the expansion of capital is limited by the monetary resources which had been deflected from consumption, without being able to grasp the doctrine that saving cannot be treated as a given because expenditure in investment goods creates additional incomes out of which supplementary savings are provided.

The theory of effective demand implied a forceful and definitive discarding of the doctrine of the ‘forced saving’. Keynes had already rejected it in his Treatise, but Harrod did not seem to have appreciated the point, for he referred to the ‘forced saving’ process as the sole means for extorting additional savings from the community. Harrod’s position on this aspect was thus ambiguous. On the one hand, the theoretical components which Harrod referred to were more akin to Robertson’s than to Keynes’s theory. On the other hand, the policy suggestions he drew out of his combination of different theoretical concepts and set-ups were instead of a more Keynesian character. The point that Harrod did not take up from the early Keynesian criticism of the forced saving doctrine was the attack to the inflexibility of the nexuses Robertson (and Hayek) saw between price level and quantity and velocity of circulation of money, or of people’s disposition to hoard unspent money (see e.g. Bridel 1987, pp. 134-6). On the contrary, Harrod seemed to think of money, of the gap between saving and investment, and of prices as rigidly linked. On the one hand, the rise of prices was seen as a necessary (rather than only possible) condition for investment exceeding saving. On the other hand, the injection (or conversely, a lack) of money into active circulation (due to banking policy and/or dis-hoarding by people) was seen as necessarily diminishing its value and thus implying a corresponding rise in the general level of prices. Finally, in Harrod’s view the injection of money into the system strictly corresponded to an increase of loans for investment in a precise proportion.

A final aspect to be remarked about Harrod’s standpoint is the lack of a comparison between the ‘natural’ or ‘normal’ rate of interest and the market rate. On the
one hand, Harrod did not discuss the notion of the ‘natural’ rate in the sense of the rate determined on the real capital market. If this enabled him to avoid the traps inherent in the notion of the ‘natural’ rate and in the harmonisation of volumes I and II of the neoclassical treatises\textsuperscript{27}, the lack of a thorough discussion of capital theory at this early stage left some consequences for the consistency of Harrod’s later inquiry on the effect of capital ‘deepening’. On the other hand, the failure to discuss the cycle in terms of the divergence between the natural (in the Keynesian sense of ‘equilibrium’: Keynes CW V, p. 139) and the actual rates, indicates that Harrod had not yet fully appreciated the potentialities of the mechanism based on the cumulative deviation from equilibrium. This, as is well known, eventually constituted the main pivot of Harrod’s mature model of the cycle (see below, Chapter VIII, § 6).

The above considerations suggest that by the end of 1933 Harrod was trying to develop his own diagnosis of the depression as a phase of the trade cycle by tinkering with some of the analytical tools and conceptual frameworks he could find in the works of Robertson, Keynes (of the Treatise) and possibly also Hayek. In particular, in spite of their deep dissent as to the existence of stabilising factors, automatic equilibrating mechanisms, the tightness of the relationship between money and prices or the policy implications of their analysis, and so on, these men developed their arguments in terms of the monetary causes of the disequilibrium between saving and investment and their consequences upon prices. Harrod shared this common ground, from which he drew the implication that the banking policy was at the heart of the troubles both as a cause and as a remedy. This inference brings a Robertsonian (and partly Hayekian) flavour, but Harrod also shared Keynes’s doubts (and added some of his own) as to the limits of the capacity of engineering a recovery by sole manoeuvres on the rate of interest\textsuperscript{28}. Thus, on the one hand Harrod felt in close agreement with Keynes as to the considerations on policy matters, but on the other hand these were reached on quite different grounds, closer to the Robertsonian viewpoint. Moreover, the distance from Keynes’s line of thought grows considerably if we consider that this was evolving quite rapidly from the Treatise on Money, and that Harrod has not been capable of grasping the new approach, failing in particular to appreciate the theory of effective demand. In fact we shall see in the next Chapter (Ch. III, § 4) that only at the end of 1934, under Kahn’s insistence did Harrod slowly abandon the approach in terms of money and prices to look for an alternative, which he could only develop in full after having read the General Theory in proof, in 1935 (Ch. IV, §§ 2 and 5).

The impression that Harrod was juxtaposing rather than harmonising analytical ingredients deriving from non-homogeneous sources, is strengthened by the lack of a systematic theoretical discussion of the monetary aspects of the cycle: Harrod only discussed some special aspects of these problems in distinct articles, without being compelled to examine the consistency of the pieces that were serving specific purposes.
The resulting strange mixture of Keynesian conclusions expressed in a language marked by Robertsonian (and sometimes Hayekian) features became particularly evident in an essay Harrod published in the August 1934 issue of *Economica* (Harrod 1934a). In the first part, Harrod developed in full details the old Keynesian argument regarding the difficulties involved by a state of falling prices in face of rigidities in wages and interest costs. In the second part, Harrod countered the Hayekian argument that any injection of credit would necessarily involve inflation and maladjustments in the vertical structure of production. He did not challenge the nature or consistency of Hayek’s reasoning, but he pointed out from within the Hayekian assumptions that the Austrian economist failed to consider that, in a progressive society with a given velocity of circulation, the community will have to add to its monetary holdings. In order to guarantee a smooth working of the system, the additional monetary holdings must be compensated by an injection of fresh money into the system29 (this essay of Harrod will be examined in detail in the next Chapter, together with the debates to which it gave rise).

A further aspect of Harrod’s struggle to acquire the appropriate instruments for his dynamic analysis provides an interesting example of how hard it can be for new ideas to get through and to be appreciated in all their implications. It is well known that in *International Economics* Harrod introduced the notion of the international trade multiplier30, and one may wonder how it could happen that Harrod, having in hand a multiplier, was not able to apply the concept straight away to his own dynamics, or at least to understand without troubles the principle of effective demand when it was presented. To complete the paradox, Keynes in correspondence did not seem to appreciate the point of Harrod’s multiplier.

The principle of the international trade multiplier seems to have been almost fully developed by March 1932. From a letter from Kahn to Harrod dated 24 March, in which the author commented upon a mechanism of equalisation between imports and exports, it is possible to reconstruct such a mechanism. In a previous letter (which did not survive), Harrod must have argued that a fall of exports implies a reduction of the internal incomes, which in turn implies a drop of expenditure. Part of this is devoted to imports, while the remainder to home-produced goods. This again implies a reduction of incomes and of expenditure. The process stops when the induced drop of imports equals the original reduction of exports. Kahn pointed out the implicit assumptions underlying Harrod’s argument, and criticised Harrod for not having considered the leakage due to saving (Kahn to Harrod, 24 March, 1932). Towards the end of the year, Harrod had submitted to James Meade a draft of Chapter VI of his book, which Meade tried to express in mathematical terms by considering a country and the rest of the world, and by examining the convergent process of the consequences of their exchanges in face of a change in the propensity to import in one of them31 (Meade to Harrod, und.). Harrod restated Meade’s reasoning in a simplified form (Harrod to Meade, 25 Nov., 1932), which turned out to be a special case of Meade’s more general statement (Meade to Harrod, 30 Nov., 1932).
Keynes also commented on Harrod’s “self-regulating theory of the foreign balance”, which he criticised for being enunciated “in such a way as to make it appear independent of the policy pursued by the Central Bank” (Keynes to Harrod, 26 Dec., 1932). Harrod’s part of the correspondence was unfortunately not preserved; however, he must have restated his position, for in the next letter Keynes affirmed that he thought he took Harrod’s point, but was still unsatisfied: “my difficulty is that the same argument would prove that if I save £1 this will diminish imports by £1 and (presumably) cause an inflow of £1 in gold” (Keynes to Harrod, 30 Dec., 1932). Under Keynes’s pressure, Harrod reformulated parts of the Chapters several times; it is not possible, however, to evaluate the impact of Keynes’s criticism on the final text, for the previous drafts did not survive; it would seem, however, that Harrod did not alter his main contention, but only improved the expositions. In the first edition of *International Economics* the principle of the international trade multiplier was hinted at, but for a formal expression and the appropriate naming it was necessary to wait until the 1939 edition.

The fact that Harrod and Keynes experienced the same difficulty in understanding each other’s propositions on the multiplier, which were analogous in formal and conceptual terms although applied to different domains, shows that there are often intrinsic obstacles to any migration of concepts from one field of thought to the other, especially at a time when both domains are undergoing radical changes which are not readily appreciated by those who are not directly involved in the transition. But the more fundamental explanation of Harrod’s difficulty lies in the fact that the acceptance of the investment multiplier would have required as a preliminary stage the reversal of the causal order between saving and investment, while Harrod still saw saving as determining the ideal pace of accumulation of the community. The investment multiplier could not be integrated within Harrod’s approach, for his framework of thought did not provide fertile ground for it. It is therefore not surprising that Harrod was not ready for this passage for another year, until he was convinced by Kahn to revert to the ‘common sense’ notion of saving. This, however, will be discussed in the next Chapter.

3. The advancing community

The years 1932 and 1933 proved interesting for Harrod’s dynamics because of his ‘conversion’ to the necessity of subsuming the treatment of the problem of unemployment under a theoretical analysis of the trade cycle, but also as a result of his apparent incapacity of pursuing this scope. However, the interest of the debates of those years is not exhausted by these considerations. Harrod, in fact, took up a point which constituted the common ground of discussion within the circle of economists engaged in missionary work for the New Fabian Research Bureau: namely, the assumption that the society is progressing as to its capacity of producing more goods more efficiently. This postulate eventually ended up at the heart of his own dynamics.
From the end of 1931, in fact, Harrod took part in some of the activities of the NFBR, in particular in the discussion on the socialisation of Banking, in commenting upon some of James Meade’s memoranda, and by participating in the “Conference on some Aspects of Socialist Planning” (see Introduction, § 4). Finally, in 1935, Harrod reviewed Evan Durbin’s *Problem of Credit Policy* for the *Economic Journal*. In this review, Harrod returned to an aspect on which he had several occasions to express his dissent from Durbin’s view in the course of the last couple of years, viz. the problem of injection of credit in a steadily advancing society (Harrod 1935b). The terms in which the problem was posed -i.e., the appropriate banking policy in face of sticky wages- confirms that the question was again approached with the analytical instruments which formed a common terrain of the debate in the period immediately following the publication of the *Treatise of Money* and of Hayek’s *Prices and Production*. This is exactly the point to be discussed, for the debates which occurred among the New Fabians can broadly be understood as a clash between Keynesian and Hayekian positions (Durbin 1985, Chs. 5 and 7).

The dissent regarded both theory and policy. As to the latter, James Meade, who was in Cambridge in 1930-31 and had brought back to Oxford the results of the discussions occurring within the Cambridge Circus, had by 1933 managed to convince “the doubting Fabians that expansionary policies were not doomed to failure”, while even later Durbin still “entertained lingering doubts about the suitability of the new Keynesian models to solve long-term problems of growth and stability in the socialist state” (Durbin 1985, p. 136). As regards theory, although Meade fathered “Mr. Meade’s Relation” and in spite of the unambiguous statement of the principle that investment finances itself (expressed in the pamphlet on “Public Works in their international Aspects”: Meade 1933a, pp. 8 and 11), his *Rate of interest in a Progressive State* still dealt with the monetary policy capable of interpreting people’s real decisions by equating saving and investment (Meade 1933, in particular pp. 12-13). Meade’s approach was thus more Keynesian as to its conclusion rather than for its overall conception, especially as to the failure to fully appreciate the *negative* implication of the multiplier principle on the traditional theory of saving, investment and interest. As regards Durbin, he was trying to produce a synthesis between Hayek’s *Prices and Production* and the *Treatise of Money*, with a result “strongly Hayekian in flavour and markedly pessimistic in its conclusions” (Winch 1969, p. 345; see also Durbin 1985, p. 142, where the opinion of Winch is also cited).

Harrod’s position, steering a middle course between an Hayekian analytical framework and Keynesian policy advise, was thus representative of the theoretical uncertainty dominating among the New Fabians, all of whom seem to have been tinkering with the old set of instruments, being incapable of finding any application for the fragments of the doctrine of effective demand which went in print. Before others had access to the whole corpus of the *General Theory*, the doctrine of effective demand thus
seems to have only been in the possession of Keynes and his closer followers (with the possible exception of Meade’s position).

However, one of the hypotheses on which the New Fabians applied their analytical skills is worth examining on its own, for it eventually constituted one of the peculiarities of Harrod’s dynamic approach. The (indirect) debates between Harrod, Meade and Durbin were in fact centred on the interest and price policy adequate for an *advancing* society, where the advance consisted in the increase of real income permitted by the increase of population and/or productivity per head\(^35\). Meade in fact was concerned with a “progressive society”, by which he meant “one in which output per head is increasing” (Meade 1933, p. 1). On the opposite front,

Durbin and Gaitskell were particularly concerned to uncover and demolish what they saw as ‘treasured dogma’ in the Labour movement, the belief that capitalism was ‘incapable of sustaining full employment and maximum output’ (Durbin 1985, p. 137).

Such a starting point was, of course, not new, for Harrod and Meade (and perhaps also Durbin -see Durbin 1985, p. 154) were certainly inspired by Robertson’s study of the conditions for the uniform growth of the economy\(^36\). Meade acknowledged his debt to Robertson in the preface of *The Rate of Interest in a Progressive State*, while Robertson in his short review of the book stressed the methodological significance of Meade’s choice to confine his analysis to the conditions of “equilibrium in a progressive society” (Robertson 1933b). As to Harrod, although his review article on Robertson’s *Banking Policy and the Price Level* critically dealt with the author’s conclusions as to the desirability of allowing some fluctuations in prices, he recognised that the aim of the proposed policy was that of “promoting progress” (Harrod 1927, p. 225). This aspect was stressed by Robertson in correspondence (Robertson to Harrod, undated -but mid-1926), while Harrod in a previous letter graphically represented the fluctuations associated with the various policies as occurring along a line of growth (Harrod to Robertson, und. -but mid-1926). Later, in the years of interest for the present discussion, Robertson was ready to pick out from an earlier draft of *International Economics* an “ambiguity about whether the long period trend of increase of production is due to the increase of population or productivity per head or both” and pointed out the consequences of the two cases in terms of international prices. However, he concluded that “we are only on the threshold of getting any apparatus which can deal with these dynamic changes”\(^37\) (Robertson to Harrod, 14 April, 1932).

Again it is not possible to speculate on this earlier version of Harrod’s 1933 book, for it did not survive and we do not know the extent of the changes introduced thereafter. However, before the final version was ready (and thus in full time to incorporate any suggestion coming from outside) Harrod discussed with the author Meade’s notion of ‘neutral money’. Harrod was unlikely to have read the whole of Meade’s *Rate of Interest* before publication, for his detailed comments on the book only came in November 1933,
when it was already in the book shops. It is therefore not possible to know whether Harrod was able to appreciate the context in which Meade used this notion, for Meade’s letter of Nov. 18, 1932, which stimulated Harrod’s comment, is not preserved among Harrod’s papers. Meade conceived of a neutral money system as “one which simply interprets the decisions of individuals, of companies or of the government without, by its own action or inaction, making the effects of such decisions different from what they would have been in a non-monetary economy” (Meade 1933, p. 11). In particular, Meade maintained that

it is clear that the intention of the economy is that so much less should be spent on consumption goods, in order that exactly so much money may be released to finance an increase in Net Investment. The money system will only remain neutral in so far as this is actually the case, or in so far an alteration in the rate of interest induces people to spend more on consumption again (ibid., pp. 12-13).

Harrod seems to have caught and appreciated this meaning of ‘neutrality’, which he thought “may have some important significance in the sense of being a monetary system which allows real forces to operate in the way that they would if not disturbed by monetary factors” (Harrod to Meade, 4 Jan., 1933). Harrod however criticised Meade’s conclusion that neutrality implies that the final income is constant (Meade 1933, p. 11) by pointing at a counter-example showing inconsistency between such a case and Meade’s notion. He suggested that “the difficulty is in interpreting what people do mean in real terms when they make a monetary decision” and that therefore “there might be a number of different definitions of neutrality, which vary with the interpretations given to individual decisions” (Harrod to Meade, 4 Jan., 1933). Harrod re-stated his criticism a few months later, and pointed out a case which will reveal certain points of interest for the subsequent development of his dynamics. In fact, Harrod contemplated the case in which incomes are increasing, giving rise to the possibility of rising investment, saving and consumption. This conclusion is quite interesting, for it shows that considerations regarding progress could by themselves have given rise to a conception of saving and consumption not as alternative to each other, but as complementary within a model of growth. Harrod, however, was not yet ready to pursue this promising line of reasoning, and soon reverted to the usual framework of thought.

I have stressed in Section 2 above that two notions of equilibrium monetary policy co-habited in Harrod’s approach, which he seemed to consider as (but never proved to be) equivalent: on the one hand, the determination of the rate of interest equating investment to the saving decisions taken by the community and on the other, the control of the volume of monetary demand with the aim of stabilising its value. Harrod maintained distinct the treatment of the two cases also when specifically discussing the state of advance. In fact, in *International Economics* and in more detail in part I of his 1934 *Economica* article, Harrod considered the implications of the injection of money (or, conversely, of the failure to inject purchasing power) in conditions of advance on the
relative prices of goods and factors. In part II of the cited article, he considered instead the compensation of the increased hoarding of the community consequent to an increase of income and saving by means of an injection of money maintaining the market rate of interest at the same level as the natural rate. Again, Harrod failed to indicate the connection he saw between the realisation of the two equilibrium conditions on prices and interest, but only took for granted that some relationship exists.

When discussing prices and growth, Harrod enumerated as the causes of advance the increase in population, in productivity per head (Harrod 1933c, p. 161) and in the quantity of factors employed (Harrod 1934a, p. 291 and passim). Harrod considered a discussion of the first of these causes as an academic question, for social tendencies indicated that fully developed economic systems were “due to enter a long period of declining population” (Harrod 1933c, p. 146), and concentrated instead on the other two. For the purpose of the present Chapter it is not important to follow Harrod’s argument in detail, but only to point out some aspects of his general procedure.

In the first place, Harrod analysed the consequences of both causes of growth in terms of the same principle, viz. their influence on productivity. As we have seen in Section 2 above, the difficulties that may arise depend on the relative rigidity of the price of factors with respect to the price of goods, which in turn determine fluctuations in profits which may adversely affect the decisions concerning investment and output. Since wages and interest rates depend on the marginal productivity of labour and capital, Harrod discussed changes in the productivity per head and in the quantity of factors employed in terms of the relative ratio of capital to labour on the one hand and of the increase of output on the other, and examined the consequences of the alternative policies of injecting enough circulating medium to keep the general level of prices stable, or to maintain the quantity of money constant (Harrod 1934a, part I). Harrod therefore tackled his problem with the help of the marginalist analytical toolbox.

But a second aspect is of great interest as to the making of Harrod’s dynamics. On the one hand, the occurrence of advance was assumed as a matter of realism, rather than for its theoretical role. In the debates on planning and nationalisation of central banking, advance was of course seen as a desideratum to be favoured by providing the conditions for its occurrence. In International Economics, Harrod saw fluctuations as occurring around a base of progress; accordingly, he defined a boom “as an increase in the rate of output which cannot be maintained in the long period”, while

> Prosperity may be defined as a condition in which output is at a level which involves reasonably full use of the productive resources available and is increasing at a rate which can be maintained in the long period (Harrod 1933c, p. 150).

The distinction remained quite vague, for Harrod did not specify the conditions that make the rate of growth as capable or not to be maintained -although one can imagine that the limit to the growth rate is given by the use of resources up to full capacity; such an
interpretation, however, would tend to overrate our knowledge of the future development of Harrod’s dynamics, rather than being fully supported by available contemporary evidence. However, at this early stage of Harrod’s research an explanation is still missing concerning the reasons why fluctuations should occur as a systematic departure from a positive, equilibrium growth rate. Therefore, there is no evidence that at this stage Harrod had been impressed by Meade’s methodological premise, according to which the study of the monetary conditions of dynamic equilibrium should be studied before developing a theory of the trade cycle and disequilibrium (Meade 1933, p. vii), although he ended up sharing this point of view (see Chapter VII § 3).

On the other hand, it is necessary to note that the causes of advance listed by Harrod were all of an exogenous character: there is nothing in Harrod’s theoretical set-up that gives rise to growth. On this point the difference with the later versions of Harrod’s theory is crucial, for growth was conceived as an endogenous, self-sustained process, determined by the accumulation process and at the same time justifying it. Looking back at the development of his own ideas, Harrod claimed that his 1934 *Economica* article provided the first specimen of his dynamics, on the ground that the analysis was led in terms of the consistency of various growth rates (see e.g. the footnote added to the reprint in Harrod 1972, p. 221, of Harrod 1934a). This statement, however, only provides half the truth: these growth rates, in fact, are only consistent with some external event (e.g., technological progress), for which they provide no reason. On the contrary, the specific methodological point of the later developments of Harrod’s notion of dynamics was the necessity of revising the question to be asked. The dynamic problem was no longer that of understanding the consequence of a once-over change occurring in the external conditions, but that of examining the continuous process in which variations in certain variables determine the condition for their further changes (see below, Chapter V, in particular § 3).

4. Harrod’s Dynamics in 1934

The upshot of the preceding Sections is that since mid-1933 Harrod was convinced of the necessity of subsuming the treatment of unemployment under a theoretical analysis of the trade cycle. However, he was not satisfied with the existing cycle theories, while on the other hand he did not yet have at his disposal the analytical instruments to formulate an alternative approach. Harrod in fact was still thinking in terms of the monetary policy appropriate for the stabilisation of the price level, that is, in the terms which were common to the theoretical and policy debate which developed immediately after the publication of Keynes’s *Treatise on Money* and Hayek’s *Prices and Production*. In the meantime Keynes had radically changed the emphasis, and had already developed and published the essentials of his principle of effective demand. James Meade—who took part in the discussions of the Cambridge Circus—had brought back to Oxford the principle that saving finances itself (although he couched it in a somewhat confusing
form). But Harrod was not ready to appropriate the new conceptual tools, for he kept thinking in terms closer to the traditional view, of saving as providing the resources out of which investment is possible.

In the meantime, Harrod had assumed the view that the cyclical fluctuations occur around a base of progress, determined by the increase of population, increases in productivity or by the use of more factors of production. Again, Harrod has not been able to integrate this conception within a proper theory of cyclical growth: the factors of progress were treated as exogenous, and the problem of advance reduced to the monetary conditions enabling these external forces to develop their beneficial effect without affecting the internal consistency of the system. Once more, the adjustment between variables was examined in terms of money and prices rather than quantities, which made it impossible to bring saving and investment, as ultimate causes of widening of the productive base by the employment of more factors of production, within the causal circle and thereby to conceive of growth as a self-sustained process.

At this junction, Harrod found himself in a theoretical cul de sac, for as far as he accepted the traditional starting point that saving decisions determine the possible pace of advance of investment, the cause of the cycle could only be found either in monetary mismanagement causing the market rate of interest to diverge from the equilibrium one allowing the full and harmonic development of growth potentialities, or in inappropriate propensities to hoard on the part of people that would side-track money from its appropriate destination. Harrod did not like the first of these solutions and could not be fully satisfied with the second. A radical change of his mind was therefore necessary before an alternative landscape could develop in front of his eyes. In particular, before the investment multiplier could be conceived as a useful instrument for understanding the effects of the accumulation process and end up to be at the core of his own trade cycle theory, Harrod necessarily had to question the old notions of saving and investment.

He did so, under Kahn’s guidance, in the course of a debate that developed around his 1934 *Economica* article. The purpose of the next Chapter will be to describe how Harrod escaped from the old framework of thought, thereby acquiring the necessary conditions to appreciate the Keynesian innovation and to frame his original trade cycle theory.

Notes
1 Reference is to *Banking Policy and the Price Level*: the developments examined here largely pre-date Robertson’s revival of these themes in September 1933a. These do not seem to have been discussed in correspondence with Harrod prior to publication. However, the debate between them which arose on these themes later in 1934 will be examined in the next Chapter.
2 On the corresponding shift of emphasis in Keynes’s policy advise, see e.g. Clarke 1990, pp. 287-8, Moggridge and Howson 1974, pp. 238-9, and Winch 1969, pp. 206-7.
3 In particular, on a couple of occasions Kahn informed Harrod of the developments which occurred in the Keynesian circle (letters of 25 June, 1931, and 24 March, 1932). Kahn also asked James Meade to
instruct Harrod of the developments taking place in Cambridge: with reference to a joint letter to The Times (Harrod, Meade et. al. 1932), Kahn wrote:

More upsetting I find your insistence that “the Government should obtain funds for these purposes from the banks” and your reference to putting “fresh money into circulation”. It is very curious that your relation, who is such a success in Cambridge, should have made so little progress in Oxford. Can’t you open Harrod’s eyes to her charms? (Kahn to Meade, 14 June, 1932).

Later on, Harrod was sent a copy of Joan Robinson’s “Parable of Saving and Investment”, with the warning that it was already much out of date, since it was written a year earlier (Robinson to Harrod, 18 March, 1933). Of course, it is possible that Harrod and the Keynesians also had occasion to discuss these topics in the course of some meeting.

4 I am referring here to The Means to Prosperity, originally published in The Times between 13 and 16 March, 1933, and the article on “The Multiplier”, originally published in The New Statesman the 1st of April. Harrod was certainly aware that The Times was going to publish Keynes’s articles, for this was the topic of an exchange of correspondence between Harrod and Keynes which occurred in February, 1933 (the letters are to be found in HP II-30-32). For a discussion of the problems in the chronology of the development of Keynes’s ideas, see Moggridge 1992, pp. 562-5.

5 Harrod’s observation is quite paradoxical in the light of the future development of his trade cycle mechanism, for the working of the Relation (the accelerator) as he described it was based on the implicit assumption that productive capacity remained constant.

6 The dating proposed here is based on the dating of International Economics (Harrod 1933c) and of an untitled typescript (Harrod 1933*). The subsumption of unemployment under the trade cycle is explicit only in the latter of these works, but the distinction between the ‘old’ and ‘new’ problems was already hinted at in International Economics. The dating problem arises from the difficulty in dating the drafting of the relevant passages in this book. This was commissioned by Keynes in January 1927 (Keynes to Harrod, 27 Jan., 1927); Harrod gladly accepted, but asked for an ample margin of time (Harrod to Keynes, 3 Feb., 1927). A first draft, including 10 Chapters, one of which was dropped in the final version, was ready by April 1932 (Robertson to Harrod, 6 April, 1932); it was probably written in the preceding months (Robertson to Harrod, 14 April, 1932). Robertson’s and James Meade’s comments did not seem to have stimulated radical re-drafting, while Keynes’s criticisms required some re-thinking of Chapter 6. However, there are no traces of comments upon the passages of interest for the present discussion, and it is therefore impossible to date them with precision. The proofs of the book were ready by April 1933 (Robertson to Harrod, 3 April, 1933). However, the statement of the problem in the new terms was explicit only in the mid-1933 typescript, and I would tend to favour the latter date as marking the passage to the new approach. Here as well, however, there is a problem of dating. The TS itself is not dated; a note by Harrod -written probably within a year or so after having concluded the drafting- attributes it to mid-1933. This is almost certainly correct, for the paper refers to the World Economic Conference -held in London in July 1933- as a recent event.

It is worth stressing that in December 1933 Harrod advertised in the Oxford University Gazette the only course of lectures he gave in Oxford on the trade cycle in the inter-war years.

7 On the distinction between these two sets of problems also see 1933c, p. 137; in a note for discussion of the Chatham House Group on International Monetary Problems, Harrod argued that there is a tendency for the ‘new problem’ to become more and more important with the general progress of society. Harrod did not enter into the discussion of the causes of the cycle, but only suggested that some factors connected to the growth of wealth exert a de-stabilising effect, so that it may be expected that economic fluctuations tend to become greater. These forces are: 1) the proportion of primary producers (agriculturists and small firms) tends to decline in favour of big capitalist producers; in case of recession, capitalists are likely to turn off hands, while people making their own livelihood are likely to work harder, thus exerting a stabilising effect whose importance tends to diminish. 2) “With the growth of average income per head, what may be called necessary expenditure becomes a smaller, and optional expenditure a larger, proportion of income”, which gives “larger scope for hoarding and dis-hoarding”. 3) “With the growth of average income per head, the amount of capital and saving per head tends to rise”: in such a case, if an equal percentage discrepancy between saving and investment occurred, it would involve disturbances in a larger proportion of total income. 4) With the growth of wealth, people tend to spend more on goods whose demand is subject to fashion; but with the improvement in conditions people are less willing to undergo the unpleasant experience of a change of occupation to meet fashion changes; “this is likely to cause a jam leading […] to a recession in the volume of output as a whole” (Harrod 1933b*, pp. 3-8).

8 Later in the same essay, Harrod introduced “a note of scepticism” with regard to his own story.
It has never been shown that there is any mechanism in the system of private enterprise for securing full employment. But it has not been shown that there is no such mechanism, still less that full employment is incompatible with private enterprise (Harrod 1933*, p. 32).

9 Estimates of the amplitude of British business cycles from 1836 to 1937 do not confirm Harrod’s view that the cycle became “steadily more marked”: it would rather seem that the amplitude declined from 1839 to 1907 (though not steadily), while there was a sharp increase between the wars: see e.g. Aldcroft and Fearn 1972, p. 9.

10 Harrod concluded by remarking that non-capitalist producers “gave stability to the old system”, while their “decline may be [responsible] for the growing instability of output as a whole” (ibid., p. 13a). This argument was preserved in *The Trade Cycle*, where however it only constituted part of the explanation.

11 In reality, in his book on *The Trade Cycle* Harrod emphasised the asymmetry between the initiating cause of advance and depression (Harrod to Keynes, 15 April, 1937, in Keynes *CW* XIV, p. 174). This, however, was due to an asymmetric notion of equilibrium growth, which included equilibrium proper and any rate of growth larger than it. As soon as the anomaly was removed (Harrod 1939a), the cycle returned to be perfectly symmetrical.

12 It is however important to notice that, precisely in connection with imperfect competition theory, Harrod expressed some early interest for the trade cycle, although his indications remained quite vague: see his allusions with reference to the possibility of decreasing costs, cited in Chapter I, § 3.

13 In a letter to Runcimann, Harrod had already referred to the cumulative nature of the opposite phases of the cycle. However, instead of emphasising the self-aggravating deviations of profits from their normal rate, he stressed the psychological character of depression and revival:

> What is so harmful at present is the deflationary activity of all governments -of which we are the greatest offenders- which inevitably spreads an atmosphere in which private deflation -hoarding in all its forms- is violently intensified. People feel and indeed are told that they must pull in their belts, cut their coats, repent of their former unjustifiable extravagances, etc., which they thereupon do, with disastrous results on themselves, since the demand for goods and services is mutual; and then they have to do it still more. It is a diffusion of the opposite atmosphere that is needed (Harrod to Runcimann, 6 July, 1932).

In *International Economics* as well, Harrod affirmed that “Analysis appears to show that it is possible for a closed community to become involved in a vicious circle of inflation or deflation” (1933c, p. 133), but did not expose in detail any mechanism capable of giving rise to such a result.

14 After a further couple of months, Harrod suggested that this cumulative mechanism was based on the entrepreneurs’ reaction in front of the loss, thus anticipating the argument lying at the heart of his instability principle:

> The fall (or rise) of prices is not the end of the trouble. As producers find themselves involved in loss, each will tend to strengthen his own position by curtailing output and confining himself to the best part of his market. But this is not a cure for the situation. The curtailment of output will involve a corresponding curtailment of expenses, and consequently of income. There will be a further fall in the demand for consumable goods, and the gap between prices and costs will not be made good (Harrod 1933f, pp. 579-80).

15 It is important to notice that in his book on *The Trade Cycle* Harrod still relied on the empirical positive *correlation* between fluctuations of the price level and of the level of output, for which he did not provide a *causal* explanation. On the epistemic and methodological premises and implications of such procedure, see Chapter V, in particular § 7, and Chapter VI note 14.

16 Consistently with the stress on the importance of monetary policy, Harrod advocated nationalisation of Central banking in order to secure that the appropriate policy could be carried out without resistance:

> National control of central banking may be regarded not primarily as an end in itself, but as the only effective means of putting a definite policy into operation. A private institution would probably be unwilling to be responsible for the policy. The public requires security that everything shall be done which can be done to carry such a policy through; and, in case there were real difficulties, the government and its economic experts must know at the earliest opportunity the nature of these difficulties, in order to take action, perhaps through its other agencies to combat them. The new bank is not simply to do under state auspices what the Bank of England has formerly done itself (Harrod 1932*).
It may be noted that a few months later, when the proposal that the Bank of England should be nationalised was brought forward at the Annual Conference of the Labour Party, Keynes substantially agreed with the proposition provided that the Bank would not express any private interest and would be entirely subject to the control of the Government of the day (Keynes CW XXI, pp. 130-132).

17 Harrod also seemed to consider as a possible source of monetary troubles the “Changes in the rate of turnover”: “There are ups and downs in the short period associated with the trade cycle” (Harrod 1933c, p. 162, italics supplied). It might be noted that, having resorted to an observed correlation rather than to a proper elucidation of the phenomenon, Harrod failed to characterise the cycle as consisting in fluctuations in the volume of output, which were instead at the core of the Trade Cycle and of successive dynamic developments. Harrod’s interest was concentrated instead on the monetary aspects of the phenomenon, and on its remedies: in order to maintain prices constant, fluctuations of the amount of production should be offset either by a spontaneous increase in the velocity of circulation or by the designed injection of a fresh stream of money into the system. Harrod, however, did not claim that “such compensatory action would eliminate the trade cycle”. However, he maintained that it could be used to eliminate changes in the value of money associated with the trade cycle; if it is true, as it seems highly probable, that those monetary changes aggravate the cycle, the amplitude of the cycle would be pro tanto diminished: the scale of compensatory action required would be less, the more promptly and effectively the action was carried out (Harrod 1933c, pp. 162-3).

18 Other passages indicate that Harrod was thinking along this line. For instance, after having discussed some special policy measure Harrod remarked that it would solve the long-period problem, but it would do nothing to offset short-period changes in monetary demand due to changes in the rate of hoarding and dis-hoarding by people generally. It would do nothing to solve the trade cycle problem (Harrod 1933c, p. 171; see also pp. 147 and 153).

See also the passage quoted in note 13 above, where Harrod interpreted private deflation as “hoarding in all its forms”. A further suggestion as to the role of hoarding is implied in Harrod’s remark that hoarding may counteract ‘natural’ or ‘artificial’ injections of money into the system. For instance, after having summarised the conclusions he reached in International Economics regarding the balance of payments, Harrod specified that the potentially beneficial effect of a positive balance or of public works can become actuality only provided that it “is not counteracted by hoarding or some such process” (Harrod to Robertson, 29 April, 1933).

19 It is necessary to note at this point that Harrod never seems to have appreciated Keynes’s emphasis on hoarding as “the expression of the relative preference of capital-owners for savings deposits and other capital assets respectively”, and his discussion of the consequences of changes in ‘bearishness’ (Keynes, Rejoinder to Robertson’s comments on the Treatise, CW XIII, pp. 220-222; see also Robinson 1938a, for a classification of the concepts of hoarding): none of Harrod’s writings of this period gives relevance to the “vital matter” (Keynes CW XIII, p. 220) of the capital-owners’ choice between liquid and non-liquid assets. Harrod was more interested instead in the fact of balances lying idle, or in other terms in the velocity of circulation.

It is worth anticipating that although in 1932-33 Harrod did not devote much energy to the discussion of these problems, this interpretation of hoarding was confirmed with strength in 1934, in occasion of his debate with Haberler on the balance of saving and investment in a progressive society (this will be discussed in Chapter III § 3), and later in the Trade Cycle, where changes in the velocity of circulation constituted the link between the static and dynamic forces (see Chapter V, § 4). Finally, in the light of Harrod’s early perspective on hoarding, it is not much surprising that a few years later, commenting upon the General Theory, Harrod could not see anything revolutionary in the liquidity preference theory, which he interpreted as solving the old problem of the determinants of the velocity of circulation (see Chapter V § 4).

20 The relevant passages are cited in Section 1 above.

21 For a comparison between Keynes’s and Robertson’s positions, see e.g. Clarke 1990, pp. 168-9 and 232 ff.

22 In International Economics, Harrod confirmed that he intended the amount of saving as determining the maximum pace of advance of investment without inflation:

In an equilibrium position the proportion of the community’s factors devoted to adding to the stock of capital goods should be equal to the proportion of consumer’s incomes which they choose not to spend, less the net foreign lending […]. The equilibrium rate of interest […] is that which limits the demand for new capital goods to this proportion (Harrod 1933c, p. 132).
Harrod argued in terms of the forced saving process at least up to November 1934: see below, Chapter III §§ 2 and 3.

Harrod did not think that attempting to influence the velocity of circulation by means of monetary policy would serve any useful scope: “The proportion between people’s deposit accounts at banks and notes in wallet or cash box is a matter of convenience and no purpose could be served by attempts to distort this proportion” (Harrod to Cannan on 12 Dec., 1933).

In an isolated and somewhat obscure passage, Harrod considered reverting the usual causal interpretation of the quantity equation. Harrod maintained in fact that a rise in the level of price is a cause of withdrawal of deposits in form of cash from the banks, and thus implies an increase of cash in circulation:

It is sometimes supposed that when a rise of prices occurs, it is always due to an increase of cash in circulation. The relation of cause to effect suggested in the foregoing paragraphs is the reverse of that. Analysis of the relation of cause to effect remains an obscure and doubtful branch of philosophy. This obscurity affects and destroys the value of much facile economic theorising. Suffice it to say here that only a very Pickwickian and contorted view of the process of causation could justify the view that a rise of prices is usually the effect and not the cause of an increase of cash in circulation in a community with a developed modern banking system. Changes in the general level of prices, incomes and employment are, however, intimately connected with the working of the monetary system (Harrod 1933e, p. 94).

This passage does not find a counterpart in any other of Harrod’s writings of the period, so that it is difficult to evaluate what part it could play in his framework of thought (on the reversal of the traditional causal interpretation of the quantitative equation after the Trade Cycle, see Ch V § 6). However, it must be noted that this passage does not constitute an exception as regards the rigidity of the connection between quantity and value of money (see note 26 below); on the contrary, Harrod discussed the matter as an instance of the causes requiring action on the part of the banks to restore the fixed proportion of reserves to liabilities.

In his essay on “Currency and Banking”, Harrod insisted on the banks’ practice to maintain a definite proportion (about 1÷9) between their cash holding and the volume of their deposits, and argued that the consequence of the Central Bank circulating more cash would be an expansion of loans by the deposit banks until the consequent deposits restore the usual proportion (Harrod 1933e, pp. 104-5). Of course not all these loans are destined to investment, but also include call money, discounts and advances (ibid., p. 102). However, Harrod seemed to maintain that the deposit banks aim at maintaining an approximately constant proportion between all their assets (see his letter to Cannan of 12 Dec., 1933), so that an expansion of money turns into investment in a given proportion.

These were indicated respectively by Sraffa (1932) and by Keynes (CW VII, pp. 182-3).

Keynes, of course, was not the sole advocate of alternative remedies: most economists (including Robertson) in the early 1930s thought public works as a necessary means to boost the economy out of the depression, especially after March 1933 when the cheap money policy which was carried on since February 1932 had eventually shown its limits (see Sayers 1951, p. 3).

A similar criticism of Hayek’s theory was developed by Durbin, who maintained that people’s hoarding in form of cash or deposits withdraw money from active circulation, exerting a general deflationary pressure (Durbin 1933, pp. 270-1).

This concept, however, was so named only in the second edition of Harrod’s book, in 1939.

Meade’s calculations are mistaken at some point, for in a passage a cross-product was left out. This however is not important, for Meade has shown how to calculate the secondary effects of a change in the propensity to import on the balance of payments and the incomes of the countries involved.

Harrod must also have tried to secure Robertson’s agreement to his position. Robertson however replied: “Yes, I rather suspect there was a fundamental difference of opinion between you & JMK on that matter! I think I am on his side, but am not sure” (Robertson to Harrod, 31 Dec., 1932)

The term ‘multiplier’ was invented by Keynes in April 1933, when Harrod’s book was already in the hands of the printer.

Meade himself is reported to have looked through his book many years later and have found it “not as Keynesian as he remembered” (Durbin 1985, p. 142). Kahn, in a letter discussing the ‘common sense’ notion of saving and investment and specifying that investment is always self-financing, reminded Harrod that this notion was to be found in Meade’s book, but nonetheless commented that “Meade’s references to ‘neutral money’ suggest slight infection. But I think he has gone astray. After all, ‘Mr. Meade’s relation’ is the forebear of ‘saving = investment’” (Kahn to Harrod, 1 Nov., 1934).
I am not, of course, arguing that the NFRB was the only milieu in which the conditions for a state of progress were considered, but only that it was certainly one of the main influences on Harrod’s interest in that assumption. A further contribution which later nourished Harrod’s interest in the topic was Henderson’s 1933 “masterly state paper on increasing productivity”; however, Harrod only read it almost one year after it was written (Harrod to Henderson, 20 Nov. 1934).

For an interpretation of Banking Policy and the Price Level along these lines, see e.g. Costabile 1993.

Further evidence of their shared theoretical interest for an advancing society is revealed by the fact that Robertson was the only contemporary commentator on Harrod’s Trade Cycle to understand that the whole book was referring to a base of steady progress. This results for instance from a letter Robertson wrote to Keynes, pointing out that Harrod was trying “to lay down the conditions for a 'steady advance’” (Robertson to Keynes, und. -but Feb. or March 1937). In reality, Robertson failed to emphasise this point in his review of Harrod’s book (Robertson 1937a), but only because he took it for granted:

But I wish I had put in a sentence expressing more explicit welcome of your way of looking at the whole problem -’What will preserve a moving equilibrium?’- as being valuable in itself whether or not the pessimism of your own solution is justified. It’s probably because I’ve always -or for long- been so much under the influence of Cassel’s ‘Th. of Soc. Ec.’ in this respect that I rather took this approach for granted: but I quite agree that it isn’t prominent enough in even the best English work (A.C.P. as well as JMK) and deserved more explicit welcome in your own. Well, well, -please try to forgive me (Robertson to Harrod, 10 Feb., 1937).

Harrod concluded his letter by expressing the “lasting feeling that there may be something in the idea of neutrality”.

The passage runs as follows:

[By the conception of neutral money] you seem to imply that the system of constant final incomes is the only one which would interpret the wishes of individuals simply. This, however, you dont really prove. And I cant believe that this is so. If real incomes are rising, people can simultaneously decide to spend and save more. Why not? And a rising money income would enable them to give effect to their decision simply. The fact is I believe that there are a number of different arrangements which might ideally fulfil your first sentence definition of neutrality. You are entitled to the one you select -ultimately because it makes you to conduct subsequent proofs simply- but you ought not, I think, to claim uniqueness (Harrod to Meade, 13 Nov., 1933).

It was noted in Section 2 above that in previous works Harrod preferred to speak of an ‘equilibrium’ rate. In the paper under consideration he fully reverted to the Hayekian terminology, consistently with his choice to fight on the Hayekian ground. For a more detailed discussion of this aspect, see Chapter III § 1.

Declining population was going to become a thorn in Harrod’s side, especially in the years 1936-39: see below, Chapter VI, § 3.

It may be interesting to note the different reactions of his readers in face of such an approach: on the one hand, Haberler and Robertson found this first part of Harrod’s article lucid enough, while they did not like the second one (Robertson to Harrod, 27 Sept., 1934; Haberler to Harrod, und. -but first half of Oct. 1934); Kahn, on the contrary, preferred the latter while having far less sympathy with part I (Kahn to Harrod, 22 Oct., 1934).

It must be noted, however, that in an extemporaneous passage Harrod anticipated the cause of deviation from equilibrium which later characterised both the Trade Cycle and the “Essay”:

Unfortunately, [keeping saving and investment in their proper relation] may become increasingly difficult. As people grow richer with the natural economic advance, they tend to save a larger proportion of their income; capital production should advance pari passu. As the quantities between which equality should be preserved grow greater, so does the danger of instability. The unprecedent severity of the present depression may well be connected with this fact (Harrod 1933f, p. 580).
Chapter III
Harrod on Saving and Investment, 1934-35

In August 1934, Harrod published in *Economica* an article on “The expansion of Credit in an Advancing Community” (Harrod 1934a), which was intended to expose a fallacy in the argument put forward by Hayek and his followers, that any injection of credit into the economic system would cause distorting effects on the price levels of consumption goods as compared to the prices of capital goods. This article, together with a letter Harrod wrote on the same lines to the Editor of *The Economist* (Harrod 1934b), raised quite a dust: Robertson first, and later Haberler and Bode, replied from the columns of *Economica* (Robertson 1934a, Bode and Haberler, 1935), Harrod rejoined to both papers (Harrod 1934d and 1935a), while Kaldor, Haberler and others replied to Harrod’s letter to the economist (Kaldor 1934, Haberler 1934), stimulating a further rejoinder by Harrod (Harrod 1934c). At the same time, the debate was carried on in an extensive private correspondence between Harrod and Robertson, Kahn, Bode and Haberler; the latter also sent copy of his correspondence with Harrod to, and discussed it with, Kaldor, Robertson and Lindahl.

Harrod’s article, together with the reactions it boosted, is of extreme interest to our story, for in the course of the debates, as a reaction to Robertson’s and Hayek’s approaches, Harrod worked out the methodological principles regarding *continuity* and *instantaneous analysis*, which were to constitute the core of his dynamics (these are expounded in Section 2). Moreover, thanks to the exchange of views with Kahn, Harrod became acquainted with the theoretical developments which had taken place in the Keynesian circle in the last couple of years, although for some time to come he could not appreciate their full implications. These debates thus marked a milestone in the development of Harrod’s dynamics: towards the end of 1934, the methodological and epistemic framework of Harrod’s dynamics was almost completely outlined, and only the analytical instruments to give life to the model were still wanting.

The guiding thread of this Chapter is the evolution of Harrod’s ideas on saving and investment, and on their place in trade cycle analysis. In his August 1934 article, Harrod criticised Hayek’s theory not by refusing the forced saving explanation at its roots, but by introducing some qualifications as to its working (Section 1). Harrod’s argument was misunderstood by Robertson, who interpreted it as relying on a notion of saving as identically equal to investment (Section 2), while Haberler correctly pointed out that some of Harrod’s theoretical concepts were somewhat ambiguous (Section 3). At the time, however, Harrod still thought that the trade cycle consisted in a cumulative divergence between saving and investment, and that therefore for a proper theorising of the phenomenon it was necessary to adopt a definition of these concepts permitting at the
outset the arising of such a divergence. The notion of saving suggested by Keynes in the
*Treatise* satisfied this need, but seemed to Harrod to be too complicated and somewhat
paradoxical. For this reason, in his letter to The Economist- Harrod proposed a new
definition of saving which would meet this requirement. When Harrod expounded these
ideas to Kahn and received his comments, he learned with amazement that Keynes had
thrown overboard his old system of thought and was moving along a completely different
line (Section 4). Harrod proved willing to adapt to the new terminology, and indeed in
his ongoing discussion with Haberler he soon abandoned his attempt to provide an
additional definition of saving and insisted instead that on the ‘common sense’ notion
saving is at any time equal to investment. However, in spite of Kahn’s tutorial, Harrod’s
argument did not run in terms of the principle of effective demand, but assumed instead
as the balancing factor changes in the volume of unsold stocks (which, it is worth
anticipating, was later going to be at the heart of Harrod’s attempt to demonstrate his
instability principle). What Harrod does not seem to have appreciated at this stage was
that the multiplier implies the reversal of the causal relationship between saving and
investment, so that the amount of available saving no longer limits the volume of possible
investment (Section 5). These considerations enable us to ascertain the state of Harrod’s
dynamics before he gathered the analytical tools for his trade cycle model; this is
expounded in Section 6.

In this Chapter I also aim at providing the evidence of a fact which may surprise
some readers. From the well-known fact that Harrod was sent the proofs of the *General
Theory* for comments, from the contacts he had with James Meade in Oxford when he
brought back the result of the discussions occurring in the Cambridge *Circus*, and from
the role he later assumed in the defense and propagation of Keynesianism, one would
imagine that Harrod played some part, at least as a discussant, in the development of the
ideas of Keynes from the *Treatise* to the *General Theory*. Harrod, instead, was almost
totally unaware of what was going on in Cambridge, and was not able to interpret the
little information which leaked through him in discussion or in print, until in October
1934 Kahn began to instruct him. At least as regards terminology, Harrod proved willing
to adapt, as results from the later correspondence with Haberler. In order to show
precisely in what terms this conversion occurred, and how Harrod’s solution of the
saving and investment terminological wrangle based on unplanned changed in stocks
arose, in this Chapter I will examine the exchanges between the discussants almost letter
after letter. The result may appear at places overdetailed and perhaps tedious, but this risk
is worth taking in the light of the importance of this step in Harrod’s formation. I thus
beg the reader to be patient and follow me in this reconstruction.

1. Harrod on Hayek’s forced saving process

The polemical target of both the *Economica* article and the *Economist* letter was
the Hayekian argument against the expansionary policies based on the injection of money
in the form of producer’s credits (Harrod 1934a, p. 287 and 296; 1934b). As usual, in his *Economica* article Harrod was reluctant to provide detailed references regarding both his critical target and the literature influencing his own view; but in the prefatory note to the reprint of this article in his 1952 collection of *Economic Essays*, Harrod specified that “although the name of Professor F. A. von Hayek is not mentioned in this essay, it was intended to be a refutation of certain ideas set forth by him in his *Prices and Production*, which had considerable influence in the following years” (Harrod 1952, p. 221). It is however clear that Harrod had in mind not only Hayek’s specific argument, but more generally the position of the followers of Hayek in the debates of the early 1930s on the appropriate monetary policy. As we have seen in Chapter II, § 3, in those years Harrod was involved in the activities of the New Fabian Research Bureau, where the Hayekian position was held in particular by Evan Durbin (subject, of course, to some important qualifications: see Chapter II, § 3), who was therefore one of the targets of Harrod’s polemics. Harrod himself pointed out to Kahn that he felt bound to write the article by the fact that, although “Hayek himself does not seem keen to make himself clearer”, “there are always people like Durbin ready for the fray. And I do find in circles far wider than those of the LSE that the impression has been created that these people have made a good case against the extension of loans at any time” (Harrod to Kahn, 25 Oct., 1934). Asked what he meant by ‘circles wider than those of LSE’, Harrod replied: “well, I dont know, but I meet odd people now and again, e.g. some of the younger members of the Economist staff -do you know Jay?- who have no connexion with the L.S.E.” (Harrod to Kahn, 29 October, 1934). It is important to stress that Douglas Jay was also active in the NFRB (see Durbin 1985, pp. 112-3), for this seems to indicate that Harrod’s scope was not primarily to argue directly against Hayek’s argument, but instead against the ‘considerable influence’ it had on the rival faction of the NFRB.

To understand Harrod’s position, it is however necessary to outline Hayek’s own argument. In his view, the accumulation of capital could occur in two ways: “either as a result of changes in the volume of voluntary saving […], or as a result of a change in the quantity of money which alters the funds at the disposal of entrepreneurs for the purchase of producers’ goods” (Hayek 1931, p. 45). In both cases, the process takes place through changes in the prices of consumption and producer’s goods, the latter at first increasing relatively to the former, thereby attracting resources. If the stimulus to accumulation was provided by a change in the decisions to save, the new productive structure would correspond to the desires of consumers and therefore turn out to be persistent. The new savings, being conceived as “simple shifts of demand between consumers’ goods and producers’ goods” (ibid., p. 46), would immediately find their way to investment. At the end of the process, natural and market rates of interest would coincide at a lower level, the adjustments of the latter guiding individual entrepreneurs in shifting resources through different stages of production. The new state of affairs was therefore described as ‘natural’, and corresponded to a policy of ‘neutral’ money
consisting in keeping the quantity of money multiplied by its velocity of circulation constant.

If the reorganisation of production was stimulated instead by an expansionary monetary policy injecting new money into circulation in the form of producers’ credits, the ‘natural’ movement of prices described above would be disturbed. Since in such a case investment would not be accompanied by an adequate reduction of demand for consumption goods, the price of the latter would not fall. The real resources necessary to support the investment process should thus be provided through an involuntary reduction of consumption. According to Hayek, however, this tendency would probably be resisted by consumers, who would keep their demand higher than the quantity of goods that the productive system could supply. This would induce a return to shorter methods of production, unless additional credit was provided. But the banks cannot indefinitely reduce the market rate, and investment would sooner or later be bound to decline, giving rise to a cumulative fall in prices, which spells depression.

In his confutation of this argument, Harrod counterattacked on Hayek’s own terrain, discussing the choice of the appropriate banking policy in terms of the relationship between prices and the gap between natural and market rates of interest. Harrod’s criticism did not aim at the nature of Hayek’s approach, but only questioned a special assumption regarding the destination of saving:

Harrod was thus proposing to distinguish two possible employments for saving, defined as “the difference between the value of [the community’s] total income and the value of the consumable goods purchased therewith” (1934a, p. 297): one part destined to the loan market (the ‘loanable funds’), and another to add to the monetary stocks.

Harrod maintained that, given the velocity of circulation of money and considering a community progressing with continuity, if the banking system provided the entrepreneurs with credits such as to exactly offset the component of saving retained as monetary stock, in order to cause “real capital to be increased at a rate equal to that at which the aggregate savings of the community increase” there would be no need to depress the market rate of interest below the natural rate (1934a, p. 297), nor would there arise sectional disturbances in the price level. The argument ran as follows.

Harrod postulated that the economy was growing with continuity at an equilibrium rate maintaining saving equal to investment (that is, the market rate of interest equalling the natural rate), under a regime of stable prices and constant velocity of circulation. He aimed to show that under certain conditions such a system could
reproduce this pace of advance and maintain its internal consistency, so that the process could be self-perpetuating⁴. He thus assumed that prices are constant and that “a regular advance is under way”, and divided the community into two sections: a business section, and income receivers, the former selling consumption goods and securities to, and buying services from, the latter; obviously some people may be member of both sections. Both sections desire to hold a part of their increasing income in the form of cash or banking deposits; let us say that the business community desires to add \( x_1 \) and the income receivers \( x_2 \) -a part of their new income- to their monetary holding. Within the unit period, income receivers buy goods and securities for an amount equal to their income (i.e., the reward of factors) minus \( x_2 \); hence the business section cashes \( x_2 \) units of money less than it has been paying out; moreover, it desires to retain \( x_1 \) in cash, so that their total monetary needs are \( x_1 + x_2 = x \). Thus if the banking system provides credit to producers for \( x \) units of money (= total amount of additional cash needed), each section of the community would be satisfied, without unbalancing the previously established price ratio between consumers’ and producers’ goods.

Consider now the same transactions from the viewpoint of the loan market. Suppose the total amount of saving in the period amounts to \( s \). Income receivers buy securities for \( s - x_2 \). To maintain equilibrium between saving and investment, the business community needs to raise \( s \); but it only receives \( s - x_2 \) from the sale of securities, while it desires an additional \( x_1 \) as supplementary reserve in cash; thus it needs to raise \( x_1 + x_2 = x \) from other sources, i.e. producers’ credit. Therefore, if the banking system provides \( x \) units of credit, saving is kept equal to investment without upsetting the balance between natural and market rates.

Naturally, this conclusion is valid only under the assumption of velocity of circulation remaining constant; if instead

the velocity of circulation was rising at the same rate as national income, [the additional cross-entry in the system of national accounting, viz. the addition to their monetary stocks that both sections or at least one section of the community will require to make] would not be required, but then the further consequence would also follow that no additional loans would be necessary for the maintenance of a stable price level (Harrod 1934a, p. 298).

In order to fully appreciate the analytical implications of the debate on Harrod’s article, and the sometimes subtle theoretical developments it stimulated, it is necessary to look at this contribution in the context of the debate on saving and investment⁵. It was common ground of the theoretical controversies in the late twenties to discuss the trade cycle in terms of the divergence between market and natural interest rates. To this scheme referred both Keynes in the *Treatise* and Hayek in *Prices and Production*, that is those that were seen as the rival theoretical systems in the early thirties. But besides the shared terminology of Wicksellian origin, Keynes and Hayek did not have much else in common. In particular, for what follows it is important to stress that while Keynes treated the natural rate as the one balancing saving and investment decisions in terms of *value*
(Keynes CW V, p. 139), the Hayekian notion refers to the equilibrium in real terms\(^6\) (Hayek 1931, pp. 20-21; see Sraffa 1932, p. 49, for a discussion of the implications of such definition); as we shall soon see, in fact, Harrod discussed Hayek’s policy proposal on the ground of a notion of the natural rate based on the real productivity of capital.

Whatever the differences between the contrasting positions involved in the debate, after Robertson in 1926 brought back to the attention of his British colleagues the notion that saving can differ from investment\(^7\), and up to 1932, it was common practice to analyse the trade cycle in terms of the gap between these two magnitudes and of the changes in the price level associated to it. After 1932, on the Keynesian front different developments took place. In the *Treatise*, Keynes had already clearly outlined the limits of the usefulness of the quantity theory of money in disequilibrium situations (*CW V*, p. 120; Bridel 1987, p. 126), and had departed from the explanation in terms of the forced saving process\(^8\). Shortly after the publication of his book, Keynes started to recast his analysis by shifting the emphasis from changes in prices to changes in the level of output (for a description of Keynes’s 1932 Easter term lectures, which already saw the shift of emphasis towards movements of output, see Rymes 1989, Chapter 2)\(^9\). In the meantime, Kahn on the one hand insisted in pointing out to Keynes the special character of the *Treatise*’s definition of saving, and on the other hand developed (with Meade) ‘Mr. Meade’s Relation’. By March 1932 Keynes renounced his asymmetrical notion of saving and ‘bowed the knee’ to the “‘common sense view’ that savings and investment are, necessarily and at all times, equal, -being, indeed, the same concept looked at from opposite points of view”\(^{10}\). Keynes was ready to infer the implication of this use of language that it is investment which drags along saving at an equal pace and not the opposite, as common-sense supposes: “S is not the voluntary result of virtuous decisions. In fact S is no longer the dog, which common sense believed it to be, but the tail” (*CW XIII*, p. 276); but another year had to elapse to see this notion formally connected with changes in output in the form of the doctrine of the multiplier.

The upshot of this brief outline is that at the time Harrod wrote his article (probably during Spring 1934\(^{11}\)) and began arguing it out (October 1934), his reasoning in terms of natural and market rates and his reference to equilibrium as equality of saving and investment and stability of prices, referred to a conceptual framework that had been abandoned at least one year earlier by the circle of Keynesians. It is worth anticipating here that Harrod was not fully aware of the developments occurring on the Keynesian front, and in any case he did not appreciate their implications. This is implicit in Harrod’s discussion of Hayek’s, Robertson’s and Haberler’s position, and is explicitly revealed by the correspondence with Kahn in October and November 1934. This will be discussed in detail in Section 4 below.

As to the criticism of the Hayekian position, Harrod refused the forced saving process not as a principle of explanation, but only in so far as it ignored the component of saving destined to the construction of monetary stocks. If the banking system’s injections
of credit did not exactly compensate for such a component, Hayek’s inference as to the consequences on saving, investment and prices would not have to be rejected. Price changes in correspondence of given increases of income were determined by the quantity and the velocity of circulation of money, in strict accordance with the quantity theory. The injection of money and increase of monetary holdings, unless the former was devised to balance the increase of the latter, determined a gap between natural and market rates, and therefore between saving and investment. In Harrod’s view, price changes and gaps between saving and investment were one and the same phenomenon. Accordingly, he suggested that, in order to maintain equilibrium by compensating with additional credits the leakage from savings due to the maintenance of monetary stocks in proportion to the increase of income, the banking policy ought to keep prices constant:

If prices are stable, the value of additional bank loans is equal to the value of the increment in the value of the community’s monetary holding. If prices are rising, the former value is greater than the latter, and if prices are falling, the former value is less (Harrod 1934b).

As regards the causal link between saving and investment, Harrod was still on the traditional (i.e., as far as we are concerned, Hayek’s and Robertson’s) line. Harrod’s argument treated the rate of saving as the autonomous variable, and implicitly accepted the view that the resources to be turned into ‘aids to production’ can only come from the unconsumed part of income: “the natural rate of interest is that which causes real capital to be increased at a rate equal to that at which the aggregate savings of the community increase” (Harrod 1934a, p. 297, emphasis mine). Throughout his essay, Harrod stressed several times that the natural rate, determining the equilibrium path, is determined by the marginal productivity of capital (e.g. at pp. 292 and 293), and is thus a notion referring to real magnitudes. Such an approach brings us back to the age-old problem, which Harrod did not discuss, of the relationship between the ‘natural’ and ‘market’ rates, i.e. the link between real and monetary aspects of the economy. This was presumably left to the quantity theory of money, for Harrod implicitly referred to the quantity equation when considering the causal relationship between quantity of money (controlled by the banking policy) and the price level. As to the two rates of interest, Harrod only discussed one of the alleged causes of a discrepancy between them, without examining whether or not, nor under which conditions, the economic system would show a tendency to their convergence.

Harrod’s approach to the saving-investment issue thus seems to be closer to Hayek’s than to the Treatise’s line of argument. On the one hand, in fact, Keynes opposed the independence of the decisions concerning those two magnitudes to Hayek’s strict connection between monetary expansion and excess of saving over investment: saving and investment (as I define them) can get out of gear without any change on the part of the banking system from ‘neutrality’ as defined by Dr Hayek, merely as a result of the public changing their rate of saving or the entrepreneurs changing their rate of investment, there being no automatic
mechanism in the economic system (as Dr Hayek’s view would imply there must be) to keep the two rates equal, provided that the effective quantity of money is unchanged (“A Reply to Dr. Hayek”, *Economica* 1931, in *CW* XIII, p. 251).

On the other hand, Harrod ignored Keynes’s subtler distinction between hoards as inactive deposits, as the relative preference for liquid and non-liquid assets, or as the degree of bearishness on the part of the public. In other words, while Harrod rightly accused Hayek of having failed to account for the decision that savers have to make regarding the proportion of monetary holdings and securities, he himself ignored Keynes’s discussion of the motives governing such decisions, in particular the role of the interest rate on the speculative component of money demand. This aspect is quite important, for it led Keynes to deny the existence of a direct connection between “a growth of inactive deposits and an excess of saving” (*CW* XIII, p. 226. For a discussion, see Bridel 1987, Chapter 7). Harrod’s contribution thus fits in Keynes’s characterisation of “all the theories, other than [his] own” (*Treatise of Money*, *CW* V, p. xxiii):

> The conception is, if I understand it aright, that the money value (or, alternatively the money cost of production -it is not always clear which) of current investment, i.e. the money-value (or cost) of the current increment of the community’s capital, is made up of two parts added together, the first of which is equal to the ‘voluntary’ savings of the public and the second (which may be either positive or negative) to the change in the volume of currency and bank money created by the banking system after allowing for certain offsets (ibid., p. xxiv).

Keynes was at great pains to distinguish his theory from the traditional approach:

> my theory is different from the theory outlined above. For whilst I hold that the policy of the banking system influences the difference between saving and the value of investment, I do not hold that there is any direct, necessary or invariable relationship between this difference and the amount of credit, whether after allowing for various specific offsets or not, which could be deduced from a knowledge, however complete, of banking and currency statistics (ibid.)

At this juncture, Harrod’s acceptance of the forced saving doctrine and of the existence of a rigid causal connection between money (quantity and velocity of circulation) and values (prices and quantities) strengthens the conclusion, expressed in the previous Chapter (Chapter II § 2), that while Harrod’s policy advice was of an undoubtedly Keynesian character, his theoretical approach still consisted in tinkering with Keynesian and Robertsonian analytical instruments (with occasionally some flavour of Hayekianism). Harrod’s position, however, underwent some important changes in the course of the debate that followed the publication of his *Economica* paper. I will turn to this in the next Section.

Before discussing the debate and its implications, it is necessary to provide a preliminary note about timing, because Harrod changed his mind in the course of intricate exchanges of correspondence with Robertson, Haberler and Kahn. The position of Kahn, who entered the debate at a late stage, was that of instructing Harrod about the
developments which had taken place in the Keynesian circle in the last couple of years. Harrod eventually moved in the direction indicated by Kahn (although, as we shall see in the next Chapter, Harrod did not get to grips with the multiplier until he had read and fully digested the *General Theory*), the evolution of his position being reflected in the correspondence with Haberler. For this reason, it is essential to bear in mind the chronological order in which letters were exchanged and articles written. Harrod’s article, probably written towards the end of the Spring, appeared in *Economica* in the August 1934 issue, and his letter to *The Economist*-dated Sept. 23- was published on October 6. In the meantime Harrod had sent offprints of his article to Robertson, Haberler and Kahn. The first substantial comment came from Robertson, in the form of his reply for *Economica*, of which he sent a copy to Harrod probably at the end of September or beginning of October. By October 6 Harrod had already sent Robertson his rejoinder. The real debate with Haberler began only after the exchange with Robertson was already concluded\(^1\), when Haberler wrote a letter to the *Economist* in reply to Harrod’s. Kahn’s correspondence with Harrod only took off in the second half of October, and the changes Harrod introduced in favour of the ‘simple minded’ notion of saving can be detected from the shift of emphasis in the correspondence with Haberler from November 1934 to February 1935.

2. Robertson and the ‘Grand Monetary tautology’

The first reply to Harrod’s August article came from Robertson\(^2\), who reacted negatively to the argument against forced saving, and announced a formal criticism in *Economica*:

> Part II seemed to me so misguided that I have written a brief assault, which I propose to offer to the editors in the hope of drawing from you a riposte. I had to do this, for if your line of reasoning is right it makes nonsense of everything which I (as well as Hayek & Co) have been trying to say for the last 8 years!

(Robertson to Harrod, 27 Sept., 1934).

In a further letter, he added:

> I feel sure that you are missing something of fundamental importance which has been revealed to me, and I think to Meade, Hicks and Durbin as well as to the stricter Hayekians. And it does surprise me that you should be surprised at my interposing my body: for I have never felt that on this part of the story there has been much difference except of language between my analysis and H[ayek]’s (Robertson to Harrod, 4 Oct., 1934).

Hayek’s forced saving mechanism was in fact similar to that developed by Robertson in *Banking Policy and the Price Level*. In the view of both authors, saving logically and chronologically precedes investment, because the resources directed to the production of new capital goods must come from the unconsumed part of income. So far as saving and investment decisions spontaneously balance each other, the economy can advance along an equilibrium path. In Robertson’s view, however, there is no mechanism ensuring that this should occur. For instance, the monetary authority could decide to increase the money supply by means of credit injections, or changes in the
hoarding habits of the public may spontaneously occur to the same effect. These cases are particularly interesting for our story. The additional money supply would stimulate the demand for investment goods, but without at the same time increasing the supply of saving. The supply of production goods being relatively rigid, production (and hence income) can increase only some time after orders have been forwarded. In the meantime the price level would increase due to the additional demand, faster in the sector of capital goods than in that of consumption goods. Therefore there would result a tendency to move resources away from the latter sector towards the former, against the will of the consumers who are thereby forced to save.

It is necessary to notice that with respect to Hayek’s thesis, Robertson’s approach did not neglect the hoarding factor, whose interactions with ‘voluntary’ and ‘forced’ saving were studied in detail. Moreover, Robertson’s analysis included the time element in an essential way. Robertson in fact interpreted the time-honoured ‘saving prior investment’ concept in dynamic terms, for he did not see saving as constituting a stock of unused consumption goods to be set apart before the construction of production goods can take place, but treated it as a flow of resources becoming available while investment takes place. The Robertsonian method (‘period analysis’) was based on the recognisance of the time lags necessary for the adjustment of certain variables in response to changes in other magnitudes. In particular, in an essay published shortly before Harrod wrote his *Economica* article, Robertson carefully discussed the period of time necessary for investment to be realised and for income to become ‘disposable’. Robertson thus assumed “the existence of a period of time, to be called a ‘day,’ which is finite but nevertheless so short that the income which a man receives on a given day cannot be allocated during its course to any particular use” (Robertson 1933a, p. 399), and analysed the events occurring in these slices of time in their succession. Discontinuity was thus of the essence of his story (ibid., p. 413). Accordingly, saving was defined as the unconsumed part of the disposable income, i.e. of the income of the ‘day’ before. It is this concept of saving that Robertson, and later Haberler, opposed to the notions proposed by Harrod on the one hand, and Keynes on the other.

Robertson’s reply opened with a charge which, in the light of Harrod’s reflections on Hayek’s forced saving process, appears rather surprising:

Mr. Harrod seems to me […] to have succumbed to the charms of the Grand Monetary Tautology […]. The bank’s balance-sheet always balances: alias Savings always equal Investment: alias all money which is anywhere must be somewhere (Robertson 1934a, p. 473).

According to Robertson, this argument “prove[d] too much”:

The principle can be used equally well to justify the expansion of bank money at any rate whatever, since the newly-created money will always be ‘saved’ by someone, i.e. will find its way into somebody’s deposit account. The bank “has only lent what has been deposited with it”: not only is there an asset to be set against every liability, but, what is even more gratifying and disarming, there is a liability to be set against every asset! (ibid., p. 473).
Evidently Robertson was here interpreting Harrod’s position as if the latter shared Keynes and Kahn’s view of saving and investment. This is certified by the following passage from a letter to Haberler:

The Keynes-Kahn-Harrod ‘Grand Tautology’, as I call it, rests on the following piece of reasoning:

Income (in any period) = sale value of consumable goods plus sale value of new capital goods.

Income (in any period) = expenditure on consumable goods plus savings.

Sale value of consumable goods in any period = expenditure on consumable goods.

\[ \therefore \text{(by subtraction), saving} = \text{expenditure on new capital goods (‘investment’).} \]

[...] If the banks finance new investment, the money created passes into somebody’s bank balance, i.e., is saved by somebody, so both saving and investment are increased (Robertson to Haberler, 1 Nov., 1934).

At the time of writing, Robertson’s reading clearly did not provide an accurate rendition of his friend’s position, although Harrod later shifted towards this view. As we shall see in Section 4 below, Harrod was certainly not aware of the implications Keynes and Kahn drew from the ‘simple minded’ notion of saving, while he still thought that the notion that saving may differ from investment is a useful tool for trade cycle analysis -and, as we shall see in Chapter VIII, § 6, in the “Essay in Dynamic Theory” he finally found the way to reconcile these views. Harrod himself therefore refused Robertson’s interpretation of his argument, and couched his conclusion in the following terms, which made his “opinion much clearer than the original part II of the *Economica* article” (Harrod to Haberler, 19 Oct., 1934):

A certain part of public savings are devoted to the holding of extra value in the form of money. Define the part so absorbed in any period as the increment in the value of the community’s monetary holding in that period. Total new savings are then the public funds put out to loan plus this value. Total new loanable funds are the public funds put out to loan plus the new bank credits. Total new savings are then equal to total new loanable funds, if the increment in the value of the community’s monetary holdings is equal to new bank credit (Harrod 1934d, p. 477).

Harrod also rejected the inference Robertson drew from his interpretation, from which it would not follow that any banking policy is justified, [but rather] that the dictum that new loanable funds must be equated to new savings is no guide to banking policy, since it does not discriminate between different banking policies. I should not have proved that any banking policy is justified, but only that a particular dictum which looked prima facie like a promising guide for banking policy is not in fact a good guide and leads nowhere (ibid., p. 476).

He also specified that the original aim of the *Economica* article and *Economist* letter was to “rescue the dictum [that new loanable funds must be equated to new savings] from futility” (ibid., pp. 476-7):

this equation will not always hold whatever banking policy is adopted; it will only if the value of money is stable. For only then is the increment in the value of total money equal to the value of the increment (new bank credit). Thus total
new savings will only be equal to total new loanable funds, if the banks provide sufficient new credit to keep the value of money stable. With this definition the dictum becomes fruitful once more (ibid., p. 477).

Harrod then reverted to a methodological point, which will be found to be central in his statement of the dynamic problem. Trying to figure out where Robertson’s difficulty in understanding his argument lay, Harrod suggested that Robertson “illicitly assumed that the period prior to that considered was one of complete stationariness”, while on the contrary his own argument supposed that the community was under a state of steady (i.e., continuous and regular) advance:

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 (ibid., p. 478).

Here we find sketched for the first time three elements on which Harrod insisted in his later writings on dynamics: the notion that dynamics refers to a single point in time rather than to periods, the stress on the regularity and continuity of change, and the requirement of self-consistency, i.e. equilibrium\(^{19}\). The implications of these methodological premises of Harrod’s dynamics will be discussed further on (in particular in Chapter V § 3), but it is important to notice here that the first two arose from the rejection of the ‘period analysis’ method (which Harrod considered equivalent to the econometrists’ approach, Tinbergen’s in particular) and its discontinuous interpretation by Robertson, while the requirement of consistency was a corollary of the rejection of Hayek’s claim as the sectorial disturbances supposedly caused by the injection of credit.

3. Haberler on saving, hoarding and velocity of circulation

Haberler’s first reaction to Harrod’s argument was of approval with respect to part I of the *Economica* article, and of incomprehension with respect to part II. The first part referred to the difficulties connected with the rigidities in money wages and to the fact that certain contracts are fixed in terms of money, and the second part referred to the criticism of the Hayekians for having overlooked the component of saving destined to the monetary holding of the community:

The second part I simply cannot understand. This is to be taken literally and is not a polite way to say that I believe that your deduction is wrong. Frankly, I think there is something wrong, but I do not understand it well enough to be sure (Haberler to Harrod, undated but probably first half of October, 1934).

In particular, one of Haberler’s questions needs to be mentioned, for it helps us to understand the origin of his misinterpretation of Harrod’s thesis. Haberler noticed that Harrod’s notion of ‘monetary holdings’ was far from clear:
Let me point out specifically where I feel uncertain about the meaning of your theory: Page 297. “q may be zero, in the case in which all members of the business section are overdrawn.” What does that mean? Same page last paragraph: “In the unit period [income receivers pay to the business community (1-q)x less units of money by the purchase of goods and securities] than they receive from it as factors.” So they hoard, but why? Or does that mean that they spend (1-q)x less than they ought in order to keep prices stable? Unit period means obviously the period before money has been injected, but after (or in <+>) volume of production has risen. Or do you assume that they hoard, because their real income has risen? (ibid.)

Harrod’s reply did not survive; however, his explanation must not have been convincing, for Haberler soon reached the conclusion that Harrod went wrong in confusing two completely different things, the ‘real value of monetary holdings’ […] on the one hand, and the proportion of real income or resources which the community chooses to keep in form of money (i.e., k in the Cambridge equation) on the other (Haberler 1934).

Haberler explained to Robertson the ground for his interpretation in the following terms:

The symptom for that is that he says in his letter to The Economist “the community requires from time to time to add to the value of its holding of cash” when real income rises. Here he thought of k, otherwise “from time to time” makes no sense. Later on he talks of “value in the form of money” which is Marshall’s phrase in connection with k. In the same sentence he says that society gets this extra value by the fall of prices! And in the preceding paragraph he says implicitly that he assumes V to be unchanged. A frightful muddle! (Haberler to Robertson, 30 Oct., 1934) 20.

Haberler’s point in his letter to The Economist was that an increase in the community’s ‘monetary holdings’ does not necessarily constitute an act of hoarding, and therefore “does not absorb any savings”. The premise of this reasoning was based on the correct observation that “an addition to the value of monetary holding […] does not involve an increase in k” (Haberler 1934). Harrod, however, was certainly aware of this, for the distinction lies at the heart of his methodological interest for a progressing economy: under steady advance, a constant value of k implies a proportionate increase in the absolute value of the monetary holdings21. Haberler’s conclusion relied on a misunderstanding of Harrod’s peculiar notion of saving, according to which the unspent part of income is destined partly to the loan market and partly to monetary holdings, while in Haberler’s view only increases in k, being equivalent to decreases in the ‘income’ velocity of circulation of money, involve “an act of hoarding, a decrease in consumer’s outlay, in the flow of money per unit of time”, and can thus be said to absorb savings (Haberler 1934). In Harrod’s view, on the contrary, decisions were taken first with regard to consumption, and only in the second place the residual (saving) was destined either to the loan market or to the accumulation of additional monetary holdings. These monetary holdings, in Harrod’s view, did not therefore provide a further depletion of consumption beyond the intentions of consumers, but rather depleted the amount of money into active circulation. Harrod’s point was that in order to avoid deflation, this amount should be compensated for by the injection of new credit. It must be admitted,
however, that on previous occasions Harrod had formulated this principle in much clearer terms (see Chapter II, § 2), and it is thus understandable that Haberler was misled by Harrod’s terminology.

Harrod at first was not interested in tackling the velocity of circulation issue (on which instead the later correspondence between the two men centred), but of course was ready to point out that Haberler’s criticism required “a definition of savings somewhat different from, and perhaps more natural than, that implied by my first letter [to The Economist]” (Harrod 1934c). He therefore restated his previous criticism of Robertson (which had not yet been published), according to which the definition of saving as equal to investment would render completely sterile any policy seeking to equalise saving with new loanable funds.

So far, Harrod’s position had not substantially altered from what he thought in 1932 (described in Chapter II). But when Kahn entered the discussion, a new element came into the picture.

4. Kahn and the ‘simple minded’ definition of saving

Harrod had sent Kahn an offprint of his August Economica article on 15 October, explaining that it was meant “to get the Hayekian fallacy out of the way”. In Harrod’s view, the mistake consisted in the fact that

\[ \text{[Hayek] uses the loans should be equal to savings dictum in a form (unlike JMK’s: I am very impatient to know his revised version) in which loans = savings is a tautology. And he then condemns certain banking policies on the ground that loans would not be equal to savings (Harrod to Kahn, 15 Oct., 1934).} \]

Harrod then specified that he did not think that referring only to the notion of saving put forward in the Treatise on Money would do\textsuperscript{22}, and explained that therefore his next letter to The Economist attempted to solve the issue by proposing a new definition of saving “which makes loans = saving not a tautology” (ibid.). At this juncture, Harrod did not explain to Kahn his desire to avoid a notion of saving that made it identical to investment; it is thus not surprising that Kahn was somewhat puzzled by Harrod’s approach.

In addition to that, this exchange between Harrod and Kahn must be considered in the light of the fact that in a few occasions in the three preceding years Kahn had already expounded to Harrod his reflections on the terminology regarding saving and investment. At the end of June 1931, Kahn had emphasised the special character of the definition of ‘income’ given in the Treatise of Money:

\[ \text{It does not include “profits”, so that the profits that are spent […] appear as negative saving. In other words, } E - S \text{ is inevitably, and by definition, the amount being spent on consumption, so that } Q_1 = (E - S) - (E - \dot{I}). \text{ But, of course, it all depends on the definitions (Kahn to Harrod, 25 June, 1931).} \]

In the same letter, Kahn opposed Keynes’s definition of saving to the “ordinary simple-minded definitions as the difference between net receipts and expenditure”, and mentioned the discussion that occurred within the Circus. In March 1932, while
commenting on Harrod’s *International Economics*, Kahn discussed the relationship between international lending and saving, arguing in terms of ‘Mr. Meade’s relation’ (Kahn to Harrod, 24 March, 1932).

To complete the picture, in January 1933 Keynes himself informed Harrod that he had reverted to the ‘common sense’ definition of saving. Shortly afterwards, Joan Robinson sent Harrod an offprint of her “Parable on Saving and Investment” (Robinson 1933), where she emphasised the role of speculation on the security market and of the adjustments in output and employment for savings to find their way to the security market (Robinson to Harrod, March 18, 1933). Finally, Harrod was certainly familiar with the *Means to Prosperity*, since Keynes kept him up to date about the scheduled date for the appearance of the original articles in *The Times* (see Chapter II, § 2).

Kahn’s puzzlement therefore certainly originated from the assumption that Harrod was familiar with the recent developments occurring in the Keynesian circle, while in reality Harrod had not grasped the implications of what he had read. This became evident to both parties as the exchange of views progressed, so that Kahn soon assumed the role of ‘tutor’ while Harrod applied the new knowledge he acquired in the ‘exercise’ of arguing against Haberler’s position. But let us proceed in order.

Kahn’s comments on Harrod’s article and letter were sent on October 22, 1934, that is, after the correspondence with Robertson was concluded and the first draft of Harrod’s reply for *The Economist* was sent to Haberler. In this and in the following letters, Kahn firmly expressed his (and the Keynesians’s) preference for the ‘simple minded’ definition of saving, he drew out its consequences, and carefully pointed out the distance between his position and Harrod’s:

I take my standpoint on the fundamental truism that savings are always and in every situation equal to investment. (Why by the way did you refer in your letter to the “somewhat complicated rival system of J.M.K”? could anything be simpler and more beautiful than this truism and all that goes with it?) Your proposition that savings are always equal to loanable funds is merely another aspect of this same truism. It is surely clear that your truism like mine must be universally true. The proof which you give in your letter to the “Economist” is of general validity and cannot be restricted to the case of stable prices (Kahn to Harrod, 22 Oct., 1934).

According to Kahn,

it is the most complete nonsense to suppose that the ideal behaviour of banks can be framed in terms of any proposition involving the level of prices. How prices behave depends on how wages behave, and that in turn depends on how Trade Unions behave (ibid.).

He thus concluded:

In short, I do not think in terms of money and prices. In the view of Keynes and his followers the Theory of Money has ceased to exist. Of course, that is an exaggeration (it is the quantity of money which determines the rate of interest), but the exaggeration is a pardonable one (ibid.).
Harrod, however, did not fully appreciate the implications of Kahn’s point. In fact, for some time he kept arguing with Haberler on the basis of variations of prices and of the forced saving process\textsuperscript{24}, but on the other hand he argued (with both Kahn and Haberler) that the whole matter was a question of the definition of saving. Harrod maintained that some notion of saving allowing a difference with investment is necessary to discuss the trade cycle:

I don’t agree that your position is simple. The negative part is of course simple, viz[.] that new loanable funds = new savings is a tautology. But then that doesn’t when the phenomenon which it is required to characterize, namely that in some sense in the boom Investments > Savings. That is where the complication comes in. To explain in what sense, you have to expound Maynard’s definitions. And they are not simple, nor, it is agreed I take it, altogether satisfactory in their old form. That is why I am impatient for the new book (Harrod to Kahn, 25 Oct., 1934).

Kahn objected to both of Harrod’s arguments, pointing out that it is not possible “to fight [the Hayekians] on their own ground because this ground does not exist”:

It seems to me that you are giving far too much away to them by implying that the price level is of any particular significance […].

The fact, I fear, is that we are not yet in complete accord. There is no sense, as I see it, in which “investment > savings”, even in a boom. In fact, in my philosophy there is no such thing as a boom -not what you mean by a boom. The great point to get clear is that investment is always equal to savings; and that is the whole of the matter (Kahn to Harrod, 28 Oct., 1934).

After having received this letter, Harrod finally made explicit to both Kahn and Haberler the whole ground of his search for a new definition of saving. On the one hand, he thought that the trade cycle is characterised by a cumulative disequilibrium between saving and investment, and thus maintained that an appropriate “kind of explanation” (Harrod to Haberler, 19 Oct. and 5 Nov., 1934) of the phenomenon must admit the possibility of a difference between saving and investment. On the other hand, he was not satisfied (for reasons he did not explain in detail) with the notion Keynes put forward in the Treatise, so that he felt the necessity to work out a new one. He thus explained to Haberler that “unless you give an unusual definition to savings, the proposition that saving = investment is true in all circumstances”, and that therefore

In order to construct a theory of the Trade Cycle based on the disparity between saving and investment, either saving or investment must necessarily be given an unusual sense. Otherwise the theory is necessarily fallacious (Harrod to Haberler, 29 Oct., 1934).

On the same day, Harrod expressed the same opinion to Kahn:

I disliked the Treatise on terminological grounds very strongly, but adopted the terminology, because I had to tell people to read it in order to discover the truth. There has been a certain amount of acclimatization by this time, but now everything is to be altered again. If the alteration is towards old fashioned terminology, I am all for it. But when you say that “there is no such thing as a boom” my heart shrinks.

When you say that Investment is always equal to saving and that is the whole of the matter, I think it may be the whole of the matter so far as the
Hayekian arguments are concerned; but this tautology does not contain the secret of the trade cycle! (Harrod to Kahn, 29 Oct., 1934).

This aspect of the debate is extremely important for the understanding of the state of Harrod’s ideas on the trade cycle. It also provides further evidence that Harrod was completely unaware of the developments taking place among the stricter circle of Cambridge Keynesians. Kahn replied with an altogether too brief tutorial on the Keynesian position, in which he overstressed the advantages of the new definition of saving with respect to the old. According to Kahn, the choice of definitions is not merely a question of convenience, but also one of truth. ‘The difference between savings and investment’ is universally interpreted as having a real meaning, in the sense of affording a causal interpretation of observed phenomena, and not merely as the difference between actual income and some arbitrarily chosen ‘normal’ income, which is all that it really is. The fault may be partly that of Keynes (of the Treatise), but if so, Keynes was definitely guilty of an error of logic and not only of a badly chosen definition (Kahn to Harrod, 1 Nov., 1934).

Kahn went on by providing “the two main arguments for the new definition of saving”:

(a) that it is what is ordinarily meant by saving,
(b) that the truism ‘saving = investment’ points to some important truths, e.g. 
(i) investment is always self-financing: there need never be any question of ‘where the money comes from’ (Cf. my ‘Mr. Meade’s relation’ of my article on “Home investment and Unemployment” and Meade’s own book). ‘Savings equal investment’ sums up all this stuff in three words,
(ii) All reference to ‘forced savings’ etc. are meaningless.
(iii) It makes no difference what part the banking system is playing in supplying credit or taking charge of hoarded funds.
(iv) The rate of interest cannot be determined by the ‘supply and demand of savings’ (ibid.).

Kahn also argued in favour of the use of the ‘common sense’ definition of saving in trade cycle theory, introducing in the debate the notion of the multiplier:

As regards the trade cycle, the new definition is also very helpful. Employment is always such as to provide that income out of which people, with their given propensity to save, will in part save an amount equal to investment. A change in investment simply alters output by an amount determined by the ‘multiplier’. The whole thing thus reduces to a study of the causes of changes in the rate of investment (ibid.).

Harrod found Kahn’s reply “paradoxical”, in particular the statement that the saving-investment equality implies that the rate of interest must be determined elsewhere: “That is simply frightful, though I have no doubt you could justify it” (Harrod to Kahn, 17 Nov., 1934). Harrod’s first approach with the critical implications for the traditional theory of interest was thus of complete bewilderment; and, as we shall see (Ch. VIII, § 3), even after prolonged discussions with Keynes Harrod never accepted the logic of this line of attack. As to the Treatise, Harrod returned to the point that an explanation of the trade cycle required definitions permitting saving to differ from investment:

What I take to be the most important point in the Treatise is this. You define equilibrium rate of interest in some way that has significance for the trade cycle (e.g. that which make \( I = \hat{S} \) of the Treatise). You then
demonstrate that the mechanism of the market does not secure an automatic reversion to equilibrium, when there is a movement away from it. I still teach that and suppose it to be right. I want that stated in a new way that avoids both the paradoxicality and also certain formal defects (e.g. that shown by Meade) of the *Treatise*. But I do not want new paradoxes introduced, because I scent in them further dangers. You have got to consider not only your pupils, who are like wax in your hands, but also the main body of economists, who are working perhaps at different problems and can only abort new theorems if they are couched in terms consonant with the accepted definitions and categories of thought of, shall I say, equilibrium economics\(^{26}\). You may blow upon equilibrium economics, on the ground that it is partly a vain show, but economists are not going to give up their at worst harmless interest in it at anyone’s behest (Harrod to Kahn, 2 Nov., 1934).

At this point, it is perfectly evident not only that Harrod had not been able to grasp the Keynesian doctrine of effective demand from the fragments which went in print since the Spring of 1933, but also that he could not see any practicable approach to the problem of the trade cycle besides those based on the adjustment between saving and investment\(^{27}\). Harrod was thus fully inserted in the tradition springing out from “the logic of the traditional Marshallian approach, [according to which] a gap between saving and investment reflects a disequilibrium situation and is the potential instrument *par excellence* of economic prognosis” (Bridel 1987, p. 126).

Harrod had not yet developed an alternative mechanism for describing economic fluctuations. However, by this time he had firmly set in his own mind the epistemic principle that the *kind of explanation* required for trade cycle theorising must be based on the possibility and cumulativeness of a departure from equilibrium, that is, on its *instability* (see Chapter I, § 4).

For the time being, the remainder of Harrod’s discussion with Haberler confirms that in spite of Kahn’s tutorial Harrod did not understand the sense and the implications of the new notion of saving as regard the causal order in the saving-investment relationship, nor the importance of the theory of the multiplier to which Kahn had made a passing reference. The whole debate, in fact, regarded the opportunity of one or the other terminological choice, while Harrod never attempted to turn upside down the causal nexus pointing from saving towards investment. For this, we will have to wait until the Summer of 1935, after Harrod had read the *General Theory* in proofs.

5. The simultaneous identity between saving and investment

Kahn’s insistence on the equality between saving and investment produced as a first result a change in emphasis between the draft and the published version of the conclusion of Harrod’s reply to Haberler for *The Economist*. Before receiving Kahn’s comment, Harrod was planning to conclude in the following terms:

> this whole way of approaching banking policy proves, when subjected to proper tests, to be completely unfruitful, and its further use can only serve to confuse the public mind. By the omission of a cross-entry, it has already beguiled a number of people into believing that there is an objection to the policy of stable prices, which does not in fact exist. This is not to say that there may not be other objections. The moral is that we should revert to more effective methods of
tackling this problem (draft of the part of the second letter to The Economist, sent to Haberler for comments, 21 Oct., 1934).

The final draft of the letter to The Economist was sent in on 31 Oct., and concluded:

My reason for implying a somewhat curious definition of savings in my first letter was that I supposed that those who sought to use this dictum as a criterion of banking policy must wish to imply a definition of terms such as the dictum is not satisfied by any and every banking policy. But I find that Dr. Haberler, at any rate, does not so wish. I joyfully accept his position and only point the moral. We must abandon all attempts to use this dictum to judge between banking policies, since it necessarily acquits them all, and we must revert to more effective methods of tackling the problem of banking policy. (Harrod 1934c).

In the subsequent correspondence with Haberler, Harrod abandoned his attempts to find a new definition of saving capable of accounting for a divergence with investment, and accommodated to the use proposed by Kahn. However, while he was at great pains to explain to his opponent why the common sense definition of saving implies its equality with investment, Harrod kept flirting with the idea of forced saving. This emerges quite clearly from a letter in which Harrod tried to convince Haberler that the forced saving explanation would not work in the case where new investment was financed by an increase in the velocity of circulation rather than in the quantity of money (Harrod to Haberler, 19 Oct., 1934). Haberler replied that it would not make any difference whether the increase in $MV$ came from $M$ or from $V$ (Haberler to Harrod, 25 Oct., 1934). At this Harrod opposed the argument that

It does not help to talk about saving ‘enforced’ through a rise of prices (due to rise in $V$). How does this get into the hands of entrepreneurs? The position is quite clear when the enforced saving is due to the issue of extra loans. These extra loans increase the number of units of purchasing power, thus causing enforced saving and at the same time put an equivalent of purchasing power (= that lost by the community) into the hands of entrepreneurs to whom the loans are issued. The case is exactly parallel to a government paying its way by inflation. It illicitly taxes the people by issuing new purchasing power, which it provides itself with from the printing press. But if prices rise by an increase in $V$, does that give a government an increased purchasing power? Not in the least. Then why should you suppose it to give entrepreneurs increased purchasing power? No new loans are issued to them. But in that case investment is equal to saving (Harrod to Haberler, 1 Nov., 1934).

Here again, Harrod’s rejection of the forced saving thesis was only partial, limited to the special case in which investment or government expenditure were not financed by direct increases in the quantity of money. By contrast, the reader will remember that on the Keynesian front, the movement away from the forced saving explanation already began with the last revision of the Treatise, and was certainly concluded by April 1933. Moreover, in Harrod’s argument the direct relationship between prices and quantity of money was still in its place. The terrain on which Harrod was moving of course proved very slippery. Haberler in fact could easily object that financing investment by means of an increase in $V$ obviously entails that
somebody must dishoard; money which was lying idle, comes into the market. To say that money circulates more rapidly on the average is only another way of saying that it is on the average not kept so long in the pocket of individuals, that the average time between two successive payments has become shorter.

Now, if a person dishoards, it increases the demand for money (in the respective period). The case is therefore exactly analogous to the increased purchasing power of a Government, which pays its way by inflation. The people are illicitly taxed, as you put it, this time not in favour of the Government, but in favour of those persons who dishoarded. There is nothing mysterious in this (Haberler to Harrod, 6 Nov., 1934).

While arguing in these terms, Harrod also attempted to convince Haberler “that if the normal definition of savings is taken, saving = investment in all circumstances”. Although Harrod’s first shot was somewhat clumsy, he introduced into the discussion the notion that if saving is defined as identical to investment, their equality must hold at any time:

Suppose the banks create new credit = £100. Some people must simultaneously begin to hold £100 of additional money, i.e., they must devote part of their income to this purpose, they must make a gap between their income and lending + spending of £100. If they cut down lending by the whole £100, this simply means that the banks lend the £100 which private people would otherwise have done and there is no increase in investment. If they cut down spending by £100 this means an increase of saving of £100 to balance the new credit of £100. They may do a little of each. Whatever they do, their increase of saving + their decrease of investment must be equal (without time lag) to the increase of loans by the banks; thus the creation of new credit produces no disparity between saving and investment (Harrod to Haberler, 29 Oct., 1934).

It is important to stress that at this point, i.e. before receiving Kahn’s letter of 1 Nov. (quoted above) mentioning that “investment is always self-financing”, Harrod was trying to equate saving and investment without considering variations in income: the new investment was matched by cuts in private lending and/or in expenditure. Harrod stressed again that saving and investment adjust instantaneously when he next attempted to convince Haberler to abandon the view that “under the ordinary definition of saving and income, differences between saving and investment are possible and that such differences are equivalent to acts of hoarding or dishoarding, of inflation and deflation, to increases and decreases in demand for goods in terms of money per unit of time” (Haberler to Harrod, 2 Nov., 1934). However, also in this occasion he did not refer to the principle of effective demand. Changes of income were indeed considered, but only in the sense that investment, being an act of expenditure, immediately constitutes income for the seller of production goods. Harrod ignored the induced effects of the primary expenditure, which suggests that he had not yet appreciated the doctrine of the multiplier. The main mechanism equating saving and investment consisted in the variations in stocks as the factor instantaneously compensating sudden changes in investment. Because of the importance that variations in stocks and instantaneous analysis were going to play in the subsequent development of Harrod’s dynamics, and since this seems to be the first time such an explanation was mentioned, it seems worth quoting it at length:
If a bank makes a loan of £100 to an entrepreneur and he does not use it, viz. he leaves it lying in his account at that bank, that is not an investment of £100. Do you wish to challenge that? Next, if this entrepreneur, whom we call A, buys some raw materials from entrepreneur B out of his B’s stock of same, and B depletes his stock of goods by this amount and adds to his money balance by this amount, that is not investment. For if you imagine A and B an integrated concern, this latter case is the same as the former. A+B have not used the loan to increase the output of producer’s goods but have merely let it lie as an extra balance. Consequently investment only begins when A or B or both begin to pay out some or all of the £100 as income to factors of production. Consequently I challenge your sentence in brackets. I do not think the period in which the newly created money is paid out as someone’s income is a following period but a simultaneous period, and I do not think what happens then is another question but the question.

Simultaneously, then, with the investment of £100, there is an increase of consumers’ income of £100. What do the consumers do with it? You might suppose that simultaneously they do nothing with it (spend it somewhat later). But in that case there is an increase of saving of £100 and the new investment of £100 is exactly balanced by a new saving of £100.

But suppose, seeing their income rise, they simultaneously spend £50. This involves a rise in entrepreneurs’ incomes of £50. Thus simultaneously with the original investment of £100, there is a total rise of income of £150 of which only £50 is spent and ∴ £100 is saved. Further if the entrepreneurs simultaneously spend £50 more, there is an additional rise of other entrepreneurs’ incomes of £50, viz. a total rise of £200 and total expenditure of £100 and ∴ saving of £100. Or if the entrepreneurs invest the £50, viz. pay out to the factors, there is a total investment of £150, rise of income of £200, rise of expenditure of £50 and new saving of £150.

You may trace out the path of the original new loan of £100 as you will, making what hypothesis you please, but the same result ensues -that you have additional saving of £100 29 (Harrod to Haberler, 5 Nov., 1934).

This passage offers some interesting insights on the state of Harrod’s thought on dynamics at the end of 1934. First, the multiplier was still extraneous to Harrod’s reasoning, and so remained until after he read the General Theory in proofs (see Chapter IV, § 2). The expenditure in investment goods or in wages was thought to turn immediately into income for someone, but saving was not seen to increase as a consequence of acts of deliberate abstention from consumption on the new, increased income in the proportion determined by the marginal propensity to save. Rather, saving equated investment only in virtue of the definition considering saving as the difference between income -as arising from expenditure- and consumption, and of the definition of investment as including the volume of stocks. Anything that was not immediately spent was pigeon-holed as saving, and whatever was not sold was classified as an investment in stocks. Harrod’s solution was therefore not in terms of a process, but of independent and instantaneous pictures30.

In the second place, Harrod’s attention was centred on individual instants. This feature matched with Harrod’s methodological principle that the proper procedure in dynamic analysis is to “take a cross-sectional view”. It must be noted, however, that Harrod’s stress on the simultaneity of the equality between saving and investment did not descend from the methodological principle. However, they had a common origin, since in both cases they arose -though indirectly- as a criticism of Robertson’s period analysis: Haberler, in fact, was adopting the Robertsonian notion of saving, which was referred to
as the unconsumed part of the income of the preceding period. The debate between Harrod and Haberler soon focused on this topic, evidencing an interesting aspect of Harrod’s approach to the matter, with regard to the nature of Harrod’s criticism of Robertson’s definition.

In his rejoinder to Bode and Haberler 1935, Harrod discussed four possible definitions of saving\(^3\): the ‘usual definition’, leading to saving = investment; Keynes’s definition in the Treatise, an unusual one “put forward … as a convenient vehicle for certain propositions”; the notion Harrod himself proposed in his article, allowing for the inequality between saving and investment in order to suit the requirements of the “writers of the ‘neutral money’ school [who] are interested in the inequality of saving and investment” and who did not like Keynes’s definition; and finally, the definition resorted to by Haberler, i.e. Robertson’s. As to the last of these definitions, Harrod commented on two distinct grounds. On the one hand, he pointed out that its adoption would lead to an absurd conclusion:

> Saving, according to this, is the difference between expenditure on consumption goods in a particular period and the income earned in the previous period. Thus, if a man’s income happens to be rising he can get little credit for frugality; he may refrain from spending the whole increment, but this is not to count as saving, since to find his saving you deduct his expenditure from the old income of his previous impoverished period. Indeed, if his expenditure exceed his old income, he is said to dis-save, even although he sets aside a vast fortune and purchases the most admirable securities therewith. Or, to take the opposite case, a man whose fortunes have declined may persist recklessly in riotous living, selling his house, his land, his securities, in order to maintain his extravagant habits -has he dis-saved? Not at all, according to this definition, provided that his expenditure does not exceed his income in the previous period. The definition is commended by Drs. Haberler and Bode as being ‘merely a correct understanding of the usual concept’! (Harrod 1935a, p. 83).

On the other hand, he pointed out to a limit concerning the operational capacity of the Robertsonian concept in face of changes in the level of income:

> if it is required to assess the amount of saving in a time of rising or falling income […] the answer on this definition will be different according to the number of ‘periods’ into which the year is split. With rising income the saving is greater the greater the number of periods, but with falling income it is less (ibid., p. 84).

It is interesting to notice that Harrod did not question the fruitfulness of an explanation of the trade cycle in terms of the gap between saving and investment: such a use would be consistent with his own view as to the epistemic character of a correct approach to the trade cycle. Harrod’s point was that writers who want to follow a similar approach should adopt a definition of saving consistent with it.

It must be remarked that in the end Harrod abandoned his original position and accepted Kahn’s view that the notion of identity between saving and investment makes nonsense of the proposals of banking policy formulated in terms of the equality of saving and investment. It must be stressed, however, that Harrod failed to discuss Kahn’s point
that the common sense notion of saving deprives of sense the concept that saving is brought into equality with investment by changes in the rate of interest. In fact, Harrod accepted Haberler’s “claim that the table of well defined concepts will ‘not contribute to essential parts to the problem’”, and concluded that

what banking policy will serve to prevent crisis and depression is a question of fact and cannot be decided by terminological wangling. There is no objection to allowing the neutral money school a definition that will make the equation of saving to investment entail constant income, provided they solemnly undertake never to use as an argument in favour of a policy to secure constant income that it equates saving to investment (Harrod 1935a, p. 84).

A further aspect of the debate between Harrod and Haberler on the definition of saving is worth examining. Harrod’s accusation of inconsistency with the common use of the language was certainly not on solid ground, and in fact Haberler easily reversed the charge:

money remains always for some time in individual pockets. (i.e. velocity of circulation is not infinite). If, now, a person gets more money on the first day of the week and he spends the additional money with his ordinary income, we can surely not say that he saves, just because he keeps the money on the average (say) half a week in his pocket. You say, however, that the consumer saves in these circumstances (Haberler to Harrod, 7 Nov., 1934).

The observation in brackets regarding velocity of circulation gave rise to repeated accusations of Harrod having tacitly assumed the velocity of circulation being infinite. The origin of Haberler’s argument lies in his adherence to Robertson’s notion of saving as depending on the income of the preceding period. In such case, saving would necessarily be equal to investment at any time only if the time-lag between earning and expenditure were zero, i.e. if money circulated immediately. On this point, Harrod and Haberler never came to terms. Harrod’s first reply unfortunately was not preserved, but Haberler’s successive remark helps in imagining what his line of defence was:

I cannot accept your proposition that, if a man who receives his income in the first spends it not at once but gradually during his income period, saves or hoards. You simply substitute for the words ‘spending’ the expression ‘dissaving or dishoarding’. This is surely a misuse of the language. You had to invent another expression to denote what we really understand by dishoard and hoard. The absence of hoarding (in your new terminology) would be equivalent to a velocity of ∞! You see, that your <position> involves you in absurdities. You can prove that, under the ordinary definitions of S and I, these must always be equal, only by substituting for the ordinary definitions, quite absurd definitions, which Robertson or I would never accept (Haberler to Harrod, 12 Nov., 1934).

Harrod’s position does not seem to have changed in the next few months, for in face of a subsequent attack on the same line at the end of February, 1935, he replied by repeating in clearer terms his explanation of the instantaneous equalisation of saving and investment in terms of variations in stocks:

I don’t understand why you suppose that your question puts me into a difficulty or why I should be likely to give an answer involving \( v = 0 \) or ∞. Everything is going well when the banks make an inflation of £1000. This, we
agree, only raises income when it is paid to factors of production. Suppose that 
they don’t spend the money at first. The income is up £1000, I is up £1000 and 
S is up £1000. Then suppose at the end of a certain period they spend the whole 
£1000 on consumption goods. We may either suppose a) that this depletes 
stocks by £1000 or b) that stocks are simultaneously replenished by fresh 
production. The argument would apply \textit{mutatis mutandis} to intermediate cases. 
This event on supposition a) constitutes dis-saving of £1000 and dis-investment 
of £1000. Still I=S. On supposition b) there is dis-saving of £1000, there is a 
rise of income of £1000 (paid to factors making consumption goods required 
to replenish stocks) and there is a saving of £1000 by the factors receiving this 
extra income. Still I=S. Where is the difficulty? And where the supposition that 
\( v = 0 \) or \( \infty \)? So much for the definition of \( S \), that \( S=I \) (Harrod to Haberler, 28 
Feb., 1935).

The aspect to be remarked is that Harrod failed to understand the premises of 
Haberler’s argument. In the view of Haberler, the quantity theory of money has an 
important part to play, because the length of the income spending period influenced the 
average velocity of circulation, and thereby affects the rate of interest, the price level and 
the level of activity itself. On the contrary, the ‘usual’ definition of saving as intended by 
Harrod dispensed with such a notion. It is significant, however, that Harrod did \textit{not} 
object to Haberler’s remarks by explaining that, and why, the velocity of circulation and 
the quantity of money have nothing to do with the saving-investment relationship. His 
reaction shows that he simply could not understand the link between velocity and the lag 
between earning and expenditure of income. This indicates that Harrod, unlike the 
Keynesians, was not yet standing on solid ground as regards the quantity theory, and 
was probably not prepared to renounce it; and indeed we will see that the quantity theory, 
although its causal direction was reverted, still had a part to play in the mechanism of \textit{The 
Trade Cycle} (see Chapter V \S 5).

\textbf{6. Harrod’s Trade Cycle analysis before the \textit{General Theory}}

Shortly before Harrod found himself engaged in the debate with Haberler, he 
described in the following words the state of his theoretical interests and of his incapacity 
to organise the conceptual tools at his disposal to conduct research on those issues:

The two subjects in theory which are interesting me particularly at the 
moment and on which statistic theory could throw light are:

(i) the question of increasing returns in imperfect competition, which is 
first cousin to your [Meade’s] short period elasticity of the demand for labour, and

(ii) to put it dramatically, whether the equilibrium rate of interest is at 
present zero, or, more generally, the actual relation of the rate of saving to the 
rate of investment in a society which has reached our stage of development.

But I confess that my ideas of how to enquire into this are at present 
amorphous in the extreme\textsuperscript{33} (Harrod to Meade, 4 Oct., 1934).

Harrod was thus thinking of the saving-investment relation in terms of the gap 
between equilibrium and market rates of interest, and had no clear ideas as to how to deal 
with the matter otherwise. By the time he was writing these lines, Harrod had set off the 
definition of the epistemic and methodological features of his dynamics. On the one hand, 
he had firmly stated the principle that any explanation of the cycle must include an
instability factor (see Chapter I, § 4), and on the other hand it became clear to him that the cycle must be conceived as a departure from a line of equilibrium growth, which has to be studied in terms of continuous changes and with reference to a single instant.

The debate with Haberler, Kahn and Robertson, brought Harrod one step further in his quest for a theory of the cycle, and prepared the ground for the final stage. On the one hand, in fact, he laid down the principle that the analysis of steady advance should be concerned with the study of the consistency of the rates of growth of the various magnitudes, and therefore only with an instantaneous picture rather than with process analysis. As we shall see (in particular in Chapters VI and VII), this principle is at the heart of Harrod’s notion of dynamics. On the other hand, Kahn’s ‘tutorial’ certainly contributed to open some doubts as to the link between the saving and investment gap and the divergence of equilibrium and market rates of interest, and prepared the ground for the reversal of the assumption that investment is constrained by the amount of saving that can be loaned, while Harrod seems to have learned from Haberler’s “Systematic Analysis of the Theories of the Business Cycle” (which was discussed in parallel with Harrod’s *Economica* article at the end of 1934) of the possible application of the accelerator to the trade cycle theory.

At this point Harrod knew how he had to proceed, but had no analytical instruments capable of giving shape to the methodological and epistemic framework he had devised. Moreover, he was not ready yet to appreciate the implications of the doctrine of effective demand. The next stage of the making of Harrod’s dynamics consisted in his acquiring the analytical instruments which eventually made up his toolbox—the accelerator and the multiplier—and in experimenting with them. Some evidence survives indicating that he actually started toying with the accelerator in the spring of 1935. As regards the multiplier, there is no mention of it until shortly before the elaboration of *The Trade Cycle*, so that we must think that Harrod did not fully realise the importance of that concept until seeing it applied in Keynes’s book, which he read in proof during Summer 1935. The examination of Harrod’s discovery of the components of his analytical toolbox will accordingly constitute the next step in this reconstruction also.

Notes

1 Hayek’s ‘forces saving’ thesis will be compared with Robertson’s in Section 2 below, where some additional details are provided, which are not necessary for the understanding of Harrod’s position at this point.

2 This limit of Hayek’s assumption was already spotted by Sraffa, in his review of *Prices and Production* (Sraffa 1932, p. 46n.). Moreover, Durbin pointed out to Harrod that no one would deny “the view […] that account must be taken of any tendency to accumulate balances by the public before monetary policy can secure equilibrium. […] You may remember that James Meade shows -Appendix to Chapter II of his book [Meade 1933, pp. 21-23]- that it is precisely this consideration which invalidates Hayek’s argument in favour of a constant $MV$” (Durbin to Harrod, 16 Nov., 1934).

3 We shall soon see (below, Section 3) that in Harrod’s article the relationship between monetary holdings and velocity of circulation was not well specified, thus giving rise to an intricate discussion with Haberler on its meaning.
The origin of Harrod’s notion of the ‘advancing community’, which eventually constituted the core of his dynamics, is discussed in Chapter II § 3 above.

For a detailed account of the Cambridge tradition with respect to the saving-investment relationship, see Bridel 1987.

The same applies to Robertson: “my own thoughts <run> -and if I could dictate to my colleagues our language would <run>- in terms of acts of real abstinence and increments of real capital” (Robertson to Harrod, 5 Oct., 1935).

On Harrod’s request, Robertson offered the following “dogmengeschichte”:

I do not know who first used the formula of difference between saving & investment in the sense of capital outlay. I seem to remember seeing it in ‘Nation’ articles in the years just before 1931, -whether by JMK or HDH I do not know. I am inclined to think JMK invented it, & this use of the word investment (a most important invention, I think); but I find it also on p. 24 of the 1st edt of Hayek’s Prices & Production, -for the temporal relation of this work to the Treatise on Money see p. xiv of the preface to the former. The importance of divergence between saving and investment in the ordinary sense is clearly set out by Lavington, The Capital Market, pp. 70-2, the connection of the latter with capital outlay being separately discussed. Wicksell’s discussion is in terms of the divergence between “the demand for loan capital and the supply of savings” (Lectures, II, 193). In the introductory chapter of the same work (pp 6-14) he explains very clearly how saving may fail to eventuate in the accumulation of real capital, without actually using the phrase of a divergence between them (Robertson to Harrod, 28 April, 1937).

See e.g. Bridel 1987, Chapter 7.

A precise assessment of the relative importance of the steps conducing from the Treatise to the General Theory has given rise to some disagreement among scholars (see Moggridge 1992, pp. 558-566). For the purpose at hand, however, the dating of the fundamental acquisitions listed in the text is sufficient -‘fundamental’ being referred not to Keynes’s own theory, but to the elements which will later enter Harrod’s analysis.

Keynes, “Notes on the definition of saving”, in CW XIII, p. 276. Significantly, these notes were originally sent to Robertson.

The dating is tentative only, since I have not found any documents indicating when Harrod wrote this article. The waiting time for publication, of course, was much shorter that nowadays.

This was discussed by Keynes in the following words:

When a man is deciding what proportion of his money income to save, he is choosing between present consumption and the ownership of wealth. In so far as he decides in favour of consumption, he must necessarily purchase goods […] . But in so far as he decides in favour of saving, there still remains a further decision for him to make. For he can own wealth by holding it either in the form of money (or the liquid equivalent of money) or in other forms of loan or real capital (Keynes CW V, p. 127).

Harrod and Robertson returned on the subject one year later, in October 1935, after they both had read the General Theory in proofs.

Unfortunately Harrod’s letters to Robertson relevant to this exchange have not survived. Harrod’s aims, however, are specified in his Economica rejoinder to Robertson (Harrod 1934d).

Robertson 1926, Chapter 5. See also Bridel 1987, Chapter 6. For the purpose of the present discussion, whose only scope is to provide the minimum co-ordinates necessary to understand the root of the disagreement between the two men, it is not necessary to refer to Robertson’s detailed distinction between the different ‘kinds of saving’ or to the somewhat complicated terminology associated to it. On this, and for an account of the evolution of Robertson’s notion of forced saving, see Presley 1978, part I, Chapter 4.

Harrod’s attitude towards Robertson’s dynamic method of ‘period analysis’ will be examined while discussing Harrod’s criticism to the ‘time-lags theories of the cycle’, in particular in Chapter V, § 3 and Chapter VI, § 3.

For Robertson’s original reaction to the ‘simple-minded’ definition of saving, see 1933a, pp. 411 and 413, and the over two-years long correspondence with Keynes, collected in CW XIII, pp. 271-330.

Later (after having been instructed by Kahn of Keynes’s adhesion to the simple minded notion of saving) Harrod repeatedly claimed that the argument of his “first Economica article did not entail the
and investment decisions in Keynes’s work. Harrod’s interpretation must be cited, that is, Hawtrey’s discussion of the treatment of saving.

There is no evidence enabling one to trace back the origins of this idea. However, at least one possible source of Harrod’s interpretation was Robertson’s article in the light of the forthcoming Keynesian International Economics: (for a discussion of Harrod’s international trade multiplier in the light of the forthcoming Keynesian multiplier, see Chapter II § 2).

22 “I feel it is not enough to reply by pointing to the somewhat complicated rival system of JMK” (ibid.)

Keynes’s comment was formulated at the time Harrod was revising his International Economics:

As regards nomenclature, I am now in lectures using Income in the old-fashioned sense, as you use it, and I shall do so in my next book. So please do not try and accommodate yourself to me on that point. I am, however, for what seemed to me overwhelming reasons, continuing to use Savings in my own revised sense. For Savings in the old sense, meaning the excess of income over expenditure, I now use the phrase Surplus income, or Surplus for short. Unconsumed Income would do equally well (Keynes to Harrod, 31 Jan., 1933).

23 Harrod himself later specified: “if I wanted to refer to a rise in $K$, I should have said not ‘increase in the value of monetary holding’ but ‘rise the proportion of income (or expenditure) held in monetary form’. $K$ is a fraction. The increase I refer to is not an increase in the value of the fraction, but an equal [Harrod should have said: ‘proportionate’] increase in the numerator and denominator, supposing the denominator to represent the number of units of income or whatever it may be” (Harrod to Bode, 31 Dec., 1934).

24 Harrod admitted that his “proposition that new loanable funds = new savings entails a stable price level involves a funny definition of savings”, and that he had “put it in that form to épater the Hayekians” (Harrod to Kahn, 25 Oct., 1934). Kahn was quite bewildered by such an argument:

Your letter is very cheering. But it leaves me puzzled. Why say something which, to all intent and purposes is untrue, simply as a prime of strategy? Isn’t it a bit foolish? I quite agree that it is extraordinarily improbable that any of the blighters [i.e., the Hayekians] will have the wits to point out the fallacy in your argument (that is what makes their position so very comic). But it is hopeless, as I look at it, to try to make things look plausible to them (Kahn to Harrod, 28 Oct., 1934).

25 Harrod’s own declarations to Kahn are completely unambiguous: on 29 October, he expressed his surprise at learning that Keynes was adopting the ‘common sense’ notion of saving: “JMK in his day gave a (different) special definition of savings, which, I gather from your letter, is now thrown overboard” (italics mine); this indicates that Harrod had overlooked Keynes’s statement expressed in his letter of January 31, 1933, quoted in footnote 23 above. On 2 November, he stated that he regarded himself “in a way one of Maynard’s staunchest supporters”, and that he had “continued to teach what [seemed to him] right in the Treatise”, but he admitted that he was “out of touch with recent developments” (emphasis mine).

By ‘equilibrium economics’ Harrod meant “the body of theory connected with marginal analysis”. Harrod thought “the equilibrium economists wrongly believe that they have demonstrated that the system tends to move, apart from rigidities, to a full employment position” (Harrod to Kahn, 17 Nov. 1934).

This is quite understandable, because once refused Pigou’s explanation in terms of optimism and pessimism (Harrod 1934e; for an evaluation of its epistemic implication, see Chapter I, § 4) the remaining most trendy theoretical approaches -Robertson’s, Hayek’s and Keynes’s- were in these terms.

28 There is no evidence enabling one to trace back the origins of this idea. However, at least one possible source of Harrod’s interpretation must be cited, that is, Hawtrey’s discussion of the treatment of saving and investment decisions in Keynes’s Treatise on Money.
In order to escape from the tautological character of Keynes’s identification of the gap between saving and investment and super-normal profits or losses, Hawtrey suggested to consider as part of investment both the ‘normal’ and the undesired stocks of goods, the latter therefore constituting “an addition to investment which is not due to any decision on the part of entrepreneurs, but to a mere failure to sell output”. He then proposed that Keynes’s analysis in terms of the difference between saving and investment could be reformulated by attributing a very similar part to the accumulation of unsold stocks (Hawtrey 1932, pp. 346-347). It is worth noticing that in 1937 Robertson had attributed to Harrod-Hawtrey the “expedient of adjusting the meaning of Amount Invested so as to take account of unintended variations in commodity stocks” (Robertson 1937b, p. 429).

Haberler strongly disapproved the notion of instantaneous adjustment between saving and investment, which he attributed to an illicit confusion between successive periods; Haberler reasoned of course having in mind Robertson’s scheme of ‘period analysis’ (Haberler to Harrod, 7 Nov., 1934). It may be interesting to notice that already before entering in correspondence with Harrod on saving and investment, Haberler had advanced a similar objection to Harrod’s notion, advanced in International Economics, that the equilibrium in the balance of payments is instantaneously restored if a change in exports or imports occurred: Haberler 1934a, pp. 100-101). There Haberler relied on Neisser’s distinction between ‘Kassenreserven’ (money people hold in order to have a reserve for unforeseen emergencies) and ‘Betriebsfond’ (money people hold in order to spend it in between the points of time at which they receive their income) (ibid., p. 101).

Haberler returned on his attack to the simultaneous interpretation of the saving-investment relationship in his review of Harrod’s Trade Cycle (Haberler 1937), taking up again the criticism to the Keynesian notion of the multiplier (Haberler 1936).

It will be noticed, moreover, that Harrod did not set a limit to the successive act of expenditure and the corresponding increases in income, while under the usual assumptions regarding the propensity to consume being positive and less that unity the multiplier indicates a convergent process.

Kahn commented as follows:

I enormously enjoyed reading your Rejoinder. I think it is frightfully important that you should publish it whether there is anything to rejoin to or not. The ideas that it contains should most certainly be brought before the public eye and I think your note does exactly what is wanted in a very effectual manner (Kahn to Harrod, 8 Jan., 1935).

In view of the ambiguity of the notion of ‘hoarding’ (see Robinson 1938a), it would be interesting to know for sure whether it was Harrod bringing it back to the scene, or whether this terminology was used by Haberler to summarise some other concept used by Harrod. Unfortunately Harrod’s letter is missing.

On the revival of Harrod’s interest for the trade cycle in 1933, see Chapter II § 3.
Chapter IV
Towards the Trade Cycle

In the preface of *The Trade Cycle*, Harrod listed the analytical tools which constituted his trade cycle model:

there are three main sources from which the ideas that are developed in this volume are derived. (i) There is a well-established relation, vouched for by experience and the laws of arithmetic, between the demand for consumable goods and the demand for durable goods, the essence of which is that the absolute amount of the latter depends primarily on the rate of increase of the former. [...] (ii) Mr. Keynes, in his recent volume, *The General Theory of Unemployment, Interest, and Prices*¹, has developed certain important ideas concerning the relations between the demand for capital goods, the propensity of the community to save, and its general level of income. [...] (iii) I have had occasion in the past to work upon the theory of imperfect competition (Harrod 1936a, p. vii).

As we have seen in Chapter I, Harrod started working on the theory of imperfect competition towards the end of the 1920s and only slowed down the research on the topic after the publication of Joan Robinson’s and Edward Chamberlin’s books in 1933. But he became familiar only much later with the other two instruments of analysis (the multiplier and the accelerator), not long before actually getting down to work on his book.

Harrod seems to have appreciated the possibility of using the accelerator in trade cycle analysis in the second half of 1934, after having read Haberler’s League of Nations memorandum on business cycle theories; the first evidence of his attempts to experiment with the new tool dates from March 1935. These early endeavours are examined in Section 1. As to the multiplier, Harrod did not seem to have been conscious of its implications before reading the *General Theory* in proofs during the Summer of 1935, and discussing it with Keynes (Section 2). In the Autumn, Harrod had a long exchange of views with Robertson on both the multiplier and the accelerator, from which it is apparent that in so far as the causal interpretation of the relationship between saving and investment is concerned, he could rightly call himself a ‘convert’ to Keynesianism. However, it is also evident that he had not yet thought of how to gear the multiplier and accelerator together to obtain his cycle model (Section 4).

In the meantime, stimulated by Durbin’s book on *Credit Policy*, Harrod extended his argument that the appropriate procedure to discuss the conditions of a regular advance is to assume changes occurring with continuity, and concluded that the method of reducing the dynamic problem to a succession of static steps is not appropriate (Section 3). At this stage, all the epistemic, methodological and analytical ingredients of Harrod’s book were ready to use, and he did indeed shortly combine them into his trade cycle theory. Unfortunately I have not been able to find any documents illustrating the mental
process by which he came to assemble these pieces. However, the surviving documents relating to the discussions taking place while Harrod was writing and revising his Chapters enable one to establish a precise chronology of the steps through which the book was written; this is given in Section 5.

This Chapter only aims at illustrating how and when Harrod came across the last two components of the analytical set-up of his book, the accelerator and the multiplier. Their actual role in the construction and working of Harrod’s models will be discussed in Chapters V and VII, dedicated to the analysis of Harrod’s dynamic thought.

1. The Accelerator

The earlier evidence certifying Harrod’s interest in the principle of the accelerator in connection with the trade cycle is given by the correspondence with Haberler on the League of Nations memorandum on *Systematic Analysis of the Theories of the Business Cycle*, which Haberler circulated for comments at the end of August, 1934. This was obviously not the first time Harrod had heard of the accelerator. However, as he later told Robertson, he was deceived by Pigou’s treatment of the subject. Pigou had warned that the fact that the amplitude of fluctuations in the capital good industries is larger than that of consumption goods, is not sufficient to warrant the inference that the former is the cause of fluctuations. Harrod understood this admonition as a refutation of the possibility of using the accelerator in trade cycle theory:

In the last Part of the early Economics of Welfare [… Pigou] discusses [the acceleration principle] and while explaining that it accounts for the greater fluctuation of capital goods output denies that it can be regarded as a true cause of the cycle. In the days when I first read it I was very much over-awed by Pigou’s authority and tended to accept what he said uncritically. I remember accepting and even expounding that view of his, and for a number of years, it put me off the scent (Harrod to Robertson, 25 December, 1936).

Haberler’s survey of business cycle theories captured Harrod’s attention for this principle, and eventually stimulated further reflections. The principle was named ‘Acceleration of derived demand’, and was enunciated as follows:

changes in the demand for consumers’ goods which produce only slight changes in the activity of the industry producing these goods will produce much more violent changes in the production of producers’ goods. This holds not only for consumers’ goods in respect to the preceding stage, but also for intermediate goods in respect to their preceding stage – in short, for all lower stages in respect to higher stages. Even a slackening of the rate of growth in one stage can thus be converted into an absolute decline in the preceding stages. As this intensification works through all stages, it is quite natural that fluctuations should be most violent in those stages of production which are farthest removed from the sphere of consumption (Haberler 1934*, p. 24).

Haberler mentioned two of the reasons usually adduced for explaining this phenomenon. In the first place, it is assumed that stocks are held in proportion to the rate of output. This implies that as output increases in lower stages of production, more than proportionate additions in the volume of stocks are required in the higher stages. The second reason lies in the use of fixed capital goods, whose stock must be increased in the
same proportion as the demand for consumption goods. Of course Haberler provided the necessary qualifications, in particular with regard to the absence of excess capacity (ibid., pp. 24-26). Combining both causes, he concluded that

in order to increase the rate of output, it is very frequently necessary to make heavy immediate investments [...], the fruit of which will mature only in the more or less distant future. This process of investment is essentially a discontinuous one. In the terminology of the monetary over-investment theory, this fact can be expressed by saying that roundabout methods of production must be undertaken, or that the average period of production must be lengthened (ibid., p. 27).

It is apparent that in his exposition, Haberler was interpreting the increase of investment due to the acceleration principle as a lengthening of the period of production. Harrod disagreed with this characterisation, and submitted that the increase in productive equipment does not necessarily involve “any change in productive technique or relative productivity of the factors”, and that “the larger proportion of employment at the higher stages will occur without any change in the real marked rate of interest -which could not happen if this higher proportion was a sign of a genuine increase of roundaboutness” (Harrod to Haberler, 19 Oct., 1934). In his response, Haberler admitted that “the exact relationship between the principle of accelerated demand on the one hand and changes in the degree of roundaboutness on the other must be made clearer” than he was able to do in his memorandum. He also conceded that in the case in which unemployed resources existed, “it is conceivable that the roundaboutness of production for the economy as a whole will not be changed, because the new production can be equipped, in all its stages, with unemployed resources”. However, he argued that when there are no unused resources

factors must be hired away from other branches, consumption industries as well as capital goods industries, and the roundaboutness of production of the economy as a whole increases. This reasoning suggests that, except in some limiting and exceptional cases, the working of the acceleration principle leads to increased roundaboutness of production (Haberler to Harrod, 25 Oct., 1934).

Harrod replied that in the latter case “there could be no increase in output and the hypothesis is illegitimate in the analysis of a boom”, and concluded:

I think the general case is this. Assume an increase in the demand for consumers’ goods, when there is some unused labour. In accordance with the principles of pp. 24-27 [of Haberler 1934*] the demand for factors at the higher stages will be initially disproportionately great, the initial demand for the waiting factor will be disproportionately great and the price of waiting is more likely to rise or likely to rise more than the price of the other factors. This would tend to make the production process actually become less roundabout at the time that more resources were employed at the higher stages (Harrod to Haberler, 1 Nov., 1934).

Harrod’s argument is interesting for two reasons. In the first place, Harrod seemed to be aware that a mechanical interpretation of the accelerator is not free of difficulties; this is suggested by his remark that the working of the acceleration principle depends on the presence and the extent of unemployed resources. It is thus rather
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surprising that he did not give account of these difficulties either in the Trade Cycle or in the “Essay”, in spite of being repeatedly asked to do so, for instance by Keynes. Secondly, Harrod’s argument confirms the absence of the multiplier mechanism from his reasoning (see Chapter III, § 4). In fact, instead of implying an increase of income and consequently of saving, the increase in investment involved instead a higher market rate of interest and hence a lower roundaboutness; the offer of ‘waiting’ was thus not supposed to follow up the rise of demand stimulated by the increase of consumption.

The correspondence with Haberler on this subject was not carried any further, since Haberler expressed the need to consider Harrod’s point more carefully (Haberler to Harrod, 6 Nov., 1934), and never came back to the subject. Harrod, however, went on experimenting with the new tool, and a few months later, in March 1935, he had an exchange of views with Kahn and Keynes on inventions, rate of interest and roundaboutness. Unfortunately the remaining evidence is fragmentary, since none of Harrod’s letters to Keynes nor to Kahn survived. Nonetheless, from Kahn and Keynes’s replies to Harrod’s unknown letters it is possible to guess to a certain extent the sort of mental path he was following. I will thus cite the relevant passages from the fragments of surviving correspondence, and later expose some implications they have suggested.

As a starting point, it has to be noted that Harrod’s approach must have been quite out of the ordinary, since Kahn felt bound to say: “To be completely frank with you, I find your letter rather bewildering”. Kahn continued by suggesting to “begin by supposing that no invention takes place, i.e. that there is a fixed state of technical knowledge” (Kahn to Harrod, 6 March, 1935). In his next letter, Kahn summarised as follows a part of Harrod’s contentions:

Let us suppose that people save a fixed proportion of their income. Then:
(i) you argue that if inventions are zero the maintenance of full employment requires a falling rate of interest;
(ii) you define “neutral” inventions in such a way that, according to you, the maintenance of full employment requires a constant rate of interest (Kahn to Harrod, 23 March, 1935).

Moreover, he observed:

It seems to me that you have never properly faced up to the fact that investment can take place even in the absence of inventions. How does your equation \( \frac{dC}{dt} = \frac{dI}{dt} \) apply in such a case? Investment is designed not merely to add new equipment when there was no equipment before, but also to replace old equipment by better equipment.

On his part, Keynes replied to some unknown letter by Harrod:

I am under the handicap that I have never been able to know exactly how round-aboutness was measured! I confess that I was assuming that when the volume of investment increased, round-aboutness increased, and I have never yet seen a definition of the latter which made it possible to distinguish between the two (Keynes to Harrod, 21 March, 1935).

And further specified, in his next letter:
I see the sort of things you are driving at by round-aboutness, though I think you might find it difficult to make your definition precise. In fact, you really have two distinct things in mind, (1) a change in technical methods and (2) a change of demand in the direction of goods and services in the production of which capital plays a larger part.

But even if you can find a precise definition, what warrant have you for attributing this to the Austrians? If you will look at the articles in the Economic Journal for December 1933, you will see some warrant for the view that the Austrians, in so far as they have any precise ideas, have been disposed to measure round-aboutness by amount of capital per head; in which case, increased round-aboutness is necessarily the same as an increase in investment (Keynes to Harrod, 28 March, 1935).

These replies attest that Harrod had been experimenting for a while with the accelerator, and also provide some indications as to the direction of his thought on the matter. In the first place, it is quite clear that Harrod was trying to give a precise expression to the relationship between the rates of increase of investment and of the demand for consumption goods. In other words, he attempted a formalisation of the acceleration principle, actually formulating an equation expressing the equality between the rates of increase of consumption and of investment. It must be stressed, however, that this formula does not represent a general expression for the accelerator, but an implication for growth occurring at a constant rate, namely, the state of steady progress envisaged in his August *Economica* article, or the equilibrium growth of *The Trade Cycle*.

Furthermore, it is evident that Harrod was conscious that the relationship described by the accelerator depended on the state of the technique, but he was not able to disentangle the influence of each variable on the final result. The symptom of this is that he tentatively defined ‘neutral’ inventions not as those which given the rate of interest would not affect the operation of the accelerator, but as those which would counteract the effect of constantly increasing saving on the rate of investment and on the rate of interest. Harrod’s insistence on the consequences of the technical progress on the relationship between investment and interest rate could, by itself, be compatible with both the Austrian theory of capital and an approach in terms of production functions. However, his use of the terms ‘lengthening of the period of production’ and ‘roundaboutness’, together with Keynes’s reply on Harrod’s attempt to attribute to the Austrians the distinction between changes in the volume of investment and in the technique of production, clearly reveal that Harrod was referring to the Austrian conceptual system.

Harrod seems to have been concerned mainly with the relationship between accelerated demand for investment goods and an increased roundaboutness, which he continued to doubt was of a necessary character. From Keynes’s comment, it would seem that Harrod was trying to distinguish two components of investment, one relating to the *volume* and the other to the *composition* of investment. However, it is not clear whether this exploration followed the same line that Harrod had suggested in the course of the previous year’s discussion with Haberler. In fact, while Harrod originally argued
that the increase of investment adversely influences the rate of interest, Keynes’s response to his queries seems to suggest that the causal chain could have been inverted, since technical change seems to have been taken as exogenous and to determine, via changes in the rate of interest, variations in the length of the production period.

Some of the above deductions are certainly of a tentative character, but the formula quoted by Kahn suggests that at the beginning of 1935 Harrod already went quite far in the exploration of the implications of the accelerator in a state of regular advance. In fact, other conditions being given (such as the technique and the degree of utilisation of resource), the acceleration process by itself only suggests the existence of a proportionality between the increase in the demand for consumption goods and the increase in the stock of capital goods necessary to produce them, or in other terms between the increase in consumption and the volume of investment. Harrod’s formula (as reported by Kahn) equates instead the rates of increase in both investment and consumption. The only condition assuring this result, under the assumption that the rate of investment is caused by the increase in consumption via increases in the volume of capital, is that the stock of capital grows at a constant rate; in such a case, all rates of increase of capital, investment and consumption are equal7.

It must be noticed that in so far as the increase in consumption is taken as exogenously given, this result does not require the operation of the multiplier. Harrod’s earlier remarks on the constant character of the progress of society (see Chapter III, § 2) suggest that his intuition already told him that if an equilibrium process of growth was possible, it had to be associated to a uniform increase at a constant rate8. This conclusion, however, was not yet supported by an analytical set-up. Similarly, the steadiness of growth does not necessarily imply that the process is in equilibrium. This would require that the process was self-sustaining, but at this point Harrod had not yet devised a mechanism capable of explaining why consumption should grow at the same rate as investment. We now know that Harrod eventually solved this problem with the help of the multiplier. However, in spite of the fact that Keynes’s version of the theory of the multiplier had been available in print since two years, Harrod seems to have made use of it only after he read the General Theory in proofs, in July 1935.

2. Saving, Investment and the General Theory

Keynes sent Harrod the galley proofs of “almost the whole” of the General Theory (“with the exception of the three concluding chapters”) at the end of June, 1935 (Keynes to Harrod, 26 June, 1935). Harrod’s first reaction to Books I and II was dated 31 July. In the following week or so he read the rest of it, and in the meantime he sent detailed and general comments to Keynes. The debate between the two men soon centred on saving, investment, and the theory of interest. Whereas Harrod accepted Keynes’s positive conclusions regarding the implications of the equality between saving and investment arising from the revised Keynesian definition of income, he rejected the
critical inference Keynes drew with respect to the orthodox theory of the interest rate as determined by the supply and demand for saving.

Their discussion on this point is of extreme interest to the student of Harrod for two reasons. First, it throws some light on his attitude not only towards Keynes’s work, but also towards the object of Keynes’s attacks, namely the ‘traditional theory’ of interest. In the second place, it provides the first evidence that Harrod appreciated the importance and understood the implications of the concept of the multiplier. The first aspect will be dealt with in Chapter VIII § 3, where Harrod and Keynes’s arguments will be specifically analysed. Here I will only mention a few aspects of Harrod’s interpretation of the *General Theory* which later entered in the foundations and the mechanism of the Trade Cycle.

The traditional theory of interest considered the supply and demand for saving as related to the rate of interest, which was thought of as the price that equilibrates saving and investment. In the *General Theory*, Keynes developed the line of attack to the logical ground of this theory, which had already puzzled Harrod when Kahn summarised it on occasion of their exchange of views in Autumn 1934 (Chapter III, § 4). Keynes argued in the first place that since saving equals investment by definition, the rate of interest cannot be thought as equating them. In the second place, he pointed out that the schedules of saving and investment are not independent of each other, and concluded that the tool of partial equilibrium analysis “breaks in [the classical theory’s] hands” (Keynes to Harrod, 10 Sept., 1935, in Keynes *CW* XIII, p. 559 and *passim*). Harrod did not accept Keynes’s lines of attack, and suggested instead that the traditional doctrine fails not in its logic, but by violating its own ceteris paribus assumption as to the level of income. In Harrod’s view, the “positive doctrine” of the *General Theory*, and in particular the multiplier, is sufficient to undermine the traditional analysis, for it proves that both saving and investment are related to the level of income so that “to cover the level of income by the cet. par. clause is to refuse to examine the problem”:

What you seem to me to have shown is that there are changes in other things which are so relevant and of such overpowering importance, that the old [supply] and [demand] analysis had better be put away. You have incidentally shown also that we know very little about the supply schedule of saving, i.e. amount of saving considered as a function of the rate of interest cet. par. which includes the level of income being given. You have further shown that the level of income has an over-riding importance in determining the amount of saving (= amount of investment). You have further shown that the level of income is linked in a roundabout way (and ∴ the level of saving in a still more roundabout way) to the rate of interest. And in such a way that the level of income and the rate of interest are indeterminate unless you bring in another equation, which you do in fact, viz. the liquidity preference schedule (Harrod to Keynes, 1 Aug., 1935, in Keynes *CWXIII*, p. 531-532).

Two important building blocks for the analytical mechanism of the Trade Cycle originate out of this understanding of Keynes’s theory. On the one hand, Harrod finally recognised the implications of the doctrine of effective demand, namely the inversion of the traditional causal nexus between saving and investment, and the role of income
changes in bringing these two magnitudes to equality. On the other hand, Harrod interpreted the *General Theory* as providing an approach to the problem of the determination of the level of activity in terms of schedules of propensities to save and to invest; Harrod thought that in this respect Keynes’s technique of analysis was not much different from the Classical one, although of course the respective schedules are “determined by something different” (Harrod to Keynes, 21 Aug., 1935, in Keynes *CW* XIII, p. 545):

The demand schedule for saving is a different thing from the propensity to save. (Now don’t accuse me of saying here that interest is determined by them. I merely re-iterate that thinking saving the same as investment doesn’t itself make nonsense of the view that the amount of it is determined by different things: a demand schedule and a supply schedule.) (Harrod to Keynes, 30 Aug., 1935, ibid. p. 554).

Harrod himself was soon going to re-interpret the traditional approach in terms of the determinants of saving and investment decisions, and to put it at the heart of his dynamic mechanism (see below, Chapter V, §§ 2 and 9, and Chapter VIII § 6). Both the equilibrium and disequilibrium movements of the economic system were determined, in Harrod’s view, by the interaction of the consequences of the decisions to save and to invest, the former depending on the level of income and the latter on its rate of change. Harrod was thus ready to pursue the inquiry on the trade cycle along the line of reasoning based on the saving-investment adjustment. But in contrast to his previous approach, he specified that those two magnitudes are always and necessarily equal, in spite of resulting from the independent decisions of different people based on distinct motives. Accordingly, he referred to changes in income instead of variations in prices and in the rate of interest. However, this aspect of Harrod’s project was not explicitly enunciated in the correspondence with Keynes, although there were further signs that Harrod was inclined to consider Keynes’s contribution as a discovery regarding the determinants of saving.

Although after reading the *General Theory* Harrod had at hand both tools that were going to constitute the analytical framework of his dynamics, it took him some more time to be able to formulate the dynamic problem in the way that eventually led him to propound his trade cycle mechanism. Harrod’s next published contribution and subsequent exchange of views with Robertson, provide some evidence that he was not yet ready for this step.

3. The review of Durbin’s *Problem of Credit Policy*

Before October 1935 Harrod reviewed *The Problem of Credit Policy* for the *Economic Journal*. Harrod dispatched a copy to Robertson, which became the subject of the subsequent exchange of views (Section 4 below).

It is not necessary here to deal extensively with Harrod’s review -which was overall negative-, but only to highlight some features revealing Harrod’s trend of thought. In the first place it is worth noting that Harrod emphasised Durbin’s concern
with the equilibrium conditions for an economy characterised by expanding output. This followed naturally from their common interest in the problem of credit policy as it was formulated in the circle of the New Fabians (see Chapter II, § 3).

Secondly, Harrod criticised on methodological grounds Durbin’s conclusion that credit injections disturb the smooth working of the system, thus resulting in the trade cycle. Harrod pointed out in fact that Durbin was assuming that the additional credit was injected at once, and observed that such an hypothesis is not appropriate for the discussion of the long-term policy suitable for a regularly advancing community. Harrod’s argument thus resumed the continuity argument already advanced in his rejoinder to Robertson’s criticism of the 1934 *Economica* article (see Chapter III, § 2), thereby providing another anticipation of the distinction between his own dynamics based on continuity on the one hand, and on the other the method of ‘period analysis’, which he thought to characterise most of the rival approaches to the trade cycle (see Chapter V, § 3, and Chapter VI, § 3). In discussing Robertson’s view, Harrod had simply opposed steady progress to the passage from stationariness to continuous advance at a constant rate. His criticism of Durbin extended the implications of this methodological argument to the reduction of the dynamic problem as a succession of static steps. Harrod maintained in fact that in the case of a community with expanding real income,

it is clear that the supposition of a single injection of credit, or even of a series of such injections occurring after and before a static period is inappropriate. It is to be expected that a single injection will cause a disturbance in the whole system and give rise to a series of fluctuations (as described by Mr. R. G. D. Allen in a mathematical appendix). If Mr. Durbin had supposed a continuous flow of new producers’ credits […] he would have reached very different conclusions. He would not have found a consequential oscillation between the demand for producers’ and consumers’ good in subsequent periods. For since the antecedent causes springing from the injections are the same for period 2 as for periods 3, 4, or any other, the effects of the injections could not be different in these periods. The disturbances which Mr. Durbin traces are really due to the jolt given by an isolated injection and not to the fact that it is a producer’s credit rather than a consumer’s credit or any other sort of interference that is operating. This method of attack debars him at the outset from reaching any valid conclusion relevant to the aim of his investigation12 (Harrod 1935b, p. 727).

Finally, another of Harrod’s critical remarks on Durbin’s book is of interest here, for it provides the first expression in print of Harrod’s interest in the acceleration principle. Although the accelerator was not explicitly named, Harrod had certainly in mind the causal nexus between increase in consumption and investment postulated by the acceleration principle when he opposed Durbin’s view that the expansion of capital at a level “that cannot thereafter be maintained in the normal working of the system […] follows in time the attainment of full employment in the consumption goods industries”:

There is reason to believe that the recovery in consumption itself provides an abnormal and temporary stimulus to investment, and that there will already have been an expansion of investment unmaintainable by ordinary means before full prosperity in the consumption goods industries is reached. On this view the
expansion of producers’ credit is an incidental by-product and not the cause of high activity in the capital goods industries (Harrod 1935b, p. 728).

4. Prices and Finance

Harrod had sent his review of The Problem of Credit Policy to Robertson at the beginning of October 1935. Robertson thought it was “very interesting”, but expressed some perplexities as to Harrod’s criticism to Durbin (mentioned in § 3 above) based on the acceleration principle. Robertson maintained in fact that on the one hand the accelerator may contribute to explain the inducement to invest but cannot provide an explanation as to the financing of the additional investment, and that on the other hand different components of aggregate investment may react in different degrees to changes in the demand for consumption goods:

The motive to ‘unmaintainable’ investment may come partly from an expansion in the consumption trades, but the means comes from abundant producers’ credit, -there is no opposition between the two propositions, and from the point of therapeutics the latter may be more important, since one cannot control people’s desires but only their actions!- The interactions of ‘consumptive’ and ‘constructional’ activity & employment are intensely muddling: I fancy one needs, under the latter lead, to distinguish between forms of capital-goods (labelled ‘machinery’) the demand for which is liable to be closely associated with the movement of the market for consumable goods, & forms of capital-good (labelled ‘building’) in which the association is less close (Robertson to Harrod, 5 Oct., 1935).

Harrod did not immediately react to the second of Robertson’s remarks, and in The Trade Cycle the accelerator worked in a somewhat mechanic way. It is perhaps worth anticipating that this eventually raised further protest on the part of Robertson and Keynes, which eventually induced Harrod to recognise that the relationship between increase in consumption and volume of investment is not rigid and that the decisions to buy new capital goods partly depend on long-run considerations. In his 1939 “Essay”, Harrod introduced in his equation a term accounting for this fact and mitigating the strength of the accelerator.

Robertson’s remark on the problem of finance raised the question of where the resources for investment come from, implying that whatever its motive, investment must be accompanied by an injection of credit; Robertson attributed “‘unmaintainable’ [i.e., inflationary] investment” to excess of credit. In his reply, for the first time (to the best of my knowledge) Harrod made use of the multiplier. His explanation was far from being original, but it is worth reproducing here because it shows that at the time of writing he had not yet conceived how to gear the multiplier and the accelerator together. Harrod argued that it does not make sense to refer to excess credit, because banks cannot create credit:

May I put this simple point. 2fold function of banks: -money creating and lending, or, rather, I should say, purveyance of lending. After much argument -Cannan defeated, etc. it has been established that the banks can by their credit operations create additional means of payment. From that it has been confusedly inferred that they can in some sense create lending, and in particular that this acts which create additional money also create additional
lending. Whereas it is perfectly clear that the banks can only lend what is lent to them (Harrod to Robertson, 8 Oct., 1935).

In the following letter, Harrod specified:

Banks can't supply the means for additional investment, since they can lend out no more than is lent to them and their action produces no effect on the total of loanable funds available for enterprise. How then can they affect the rate of interest? Why, they can cause the community other than themselves to have less interest bearing and more non-interest bearing assets (here you see the sinister influence of JMK!) thus sending up the value of the interest-bearing. A lower rate of interest stimulates investment and the funds for the extra investment are —not provided by the banks— but found out of the higher savings from the higher income which the extra investment necessarily entails [emphasis added]. And if you say -ah, but there may be a time lag before the higher income materializes, the answer is that there will be within the lag you are thinking of either some dis-investment (drawing on stocks) so that net investment isn't as high as it looks or some abnormally large savings in the interval in which income receivers receive but do not spend their income (Harrod to Robertson, 9 Oct., 1935).

The italicised passage shows that Harrod had learned the lesson involved in the Keynesian multiplier, and that he was prepared to recast his argument inverting the causal relationship between saving and investment. However, at this stage he had not yet recognised the possibility of solving the two problems, set by Robertson, of the motive and the finance of investment simultaneously. In other words, Harrod failed to recognise the connection between the increase of saving and income on the one hand, and the incremental demand for consumption goods that constituted the original cause of the increase of investment on the other. Or, said differently, Harrod had not yet been able to link together the two questions regarding the origin of the increase of consumption causing investment and of saving providing the means of it, and thus failed to envisage the solution in terms of the interaction between multiplier and accelerator giving rise to self-sustained growth by mutually sustaining each other’s effects on income.

As a final note on this exchange of views between Harrod and Robertson—which also concluded the preparatory work leading to the *Trade Cycle*—I would like to mention a few of Harrod’s scattered remarks specifying the state of his ideas on the themes I have discussed in these first Chapters. In the first place, it is worth recording Harrod’s confession that after having read the *General Theory* “carefully and most of it twice”, “on the whole [he] should be called a ‘convert’” to Keynesianism16 (Harrod to Robertson, 7 Oct., 1935).

As to the notion of saving and investment, Harrod declared that “in general [he liked] to think of saving as equal to investment” (Harrod to Robertson, 3 Oct., 1935), but admitted that definitions are valuable for the results they permitted us to reach; Robertson’s terminology, for instance, he thought to be valuable on this criterion17, although he felt that “the definition of saving, which [goes] currently under [Robertson’s] name, which makes it the difference between spending in the present period and income in the last” was “a very awkward definition”, and wondered “whether [Robertson had] built anything on it” (Harrod to Robertson, 11 Nov., 1935).
Finally, Harrod implicitly expressed his doubts as to the meaning and the implications of the network of concepts in terms of which he had been reasoning up to a year earlier. In fact, while asking Robertson to explain how the notion of ‘inflation’ could be accounted for in terms of Robertson’s definition of saving, Harrod questioned the possibility of determining a precise relationship between two notions of inflation, one consisting in the divergence between saving and investment and the other in the divergence between prices and costs:

> Take it that we are agreed about what is inflation by reference to the result, i.e. the behaviour of prices relatively to money costs. If prices rise relatively to money costs there is inflation and not otherwise. Now when you define saving in your way, you want, I presume, to be able to say that when \( I > S \) there is inflation. In order therefore to justify your definition of saving, you have got to show that whenever \( I > S \) in that sense, prices will be above costs. But have you ever shown that? Can it, in fact, be shown? But if it is not the case, we have 2 different kinds of inflation, 1. about which we agree of which the criterion is the cost to price relation and 2. depending on your definition of saving, when \( I > S \). What is the relation of these 2? Is it undesirable to have inflation (2) if inflation (1) is not present? If so, why?18 (Harrod to Robertson, 3 Oct., 1935).

By this time, Harrod was thus definitely convinced that an explanation in terms of prices and costs could lead to confusion, and firmly moved towards an explanation of the cumulative process characterising the boom privileging movements in quantities to price changes. The last step in this process was the gearing together of the multiplier and the accelerator. In the course of his October and November exchange with Robertson, there is no sign that he was ready to take this step. However, the right idea must have occurred to him quite soon, for at the beginning of January, 1936, Harrod was able to send to James Meade a draft of the first two Chapters of his book, where this mechanism was advocated in connection to the notion of moving equilibrium and was called to provide reasons explaining the deviations from this path.

### 5. Assembling the mechanism

At this point, Harrod had gathered all the elements for the elaboration of his trade cycle theory. He had the epistemic principle that some instability is necessary at the outset to permit abandoning the equilibrium position; he had the methodological principle that before studying movement and fluctuations one ought to understand the determinants of the level of output; to this, he had added the principle of continuity; and finally, he had the analytical components of his mechanism. Assembling multiplier and accelerator according to the principles he had thought out earlier on was only a matter of time, and at the beginning of 1936 Harrod started jotting down the first draft of his book.

Before examining, in the second part of this study, how Harrod geared together his tools, and how his theory evolved under the pressure of the comments of his fellows, it may be useful to provide a chronology of the events, in order to clear the way for the understanding of the frenetic and sometimes overlapping activities regarding the book.
Up to October or even November 1935 Harrod did not seem to have thought of the cumulative process in terms of the interaction between the multiplier and the accelerator. However, he must have conceived of this solution soon after 19: at the end of December, in fact, he had completed a draft of an address to be read in Copenhagen on January 10, 1936 (Harrod 1935*), where the multiplier-accelerator process was clearly outlined (although the two principles were not explicitly named). Harrod presented it as “how economists explain the connection between growing wealth and growing fluctuations in trade”. He argued that the possibility of profitably utilising the productive equipment depends on the growth of the system, which in turn provides the demand for consumption goods while the continuous flow of investment maintains growth itself 20. This formulation implied the possibility of self-sustained growth, but Harrod also stressed the instability of this process: in fact, he expressed the view that, as a consequence of the accelerator, a small decline in the equilibrium rate of growth would cause a complete downturn:

A high income [...] implies a high degree of saving, of provision by individuals for their future. Saving fructifies in the creation of material capital equipment to aid production. [...] So long as production is on the upward incline that equipment may be fully utilized. But its profitability does depend on that upward incline. If the world suddenly became stationary, a great deal of the current increase of capital equipment would have to stop. There would be no prospective use for it.

Now the fact of the matter is that the system as a whole depends on investment, the creation of new real capital goods, being maintained. [...] If the demand for additional capital goods suddenly ceased, there would be no way of utilizing the savings; then and then only would saving create a ‘deficiency of purchasing power.’ The existence of a field for the investment is essential to the maintenance of a demand for goods equal to the supply. If savings are just not used for any purpose, the demand becomes short, and depression ensues.

So, if our system is to work, we must have the production of additional capital goods going on. But that will not go on, unless production in general is increasing. Thus our system will only work if it is growing. A stationary state is unthinkable without a complete re-modelling of our system. We can only exist in a world of glorious crescendo. Now from time to time the pace of advance may alter - we cannot expect uniformity in this world of chop and change. The trouble is that a slackening of advance is not possible without a definite set-back. For if there is a slackening of advance, the amount of new capital goods required goes down, men are thrown out of work in the industries making them, and their consumption must decline for lack of purchasing power. This affects other industries and there is a decline of consumption all round. But in a world of declining consumption, what need of new capital goods? The capital goods industries will find the depression one of extreme severity. What this amounts to is that there cannot be a small depression without there being a great one. The matter may be reduced to a mathematical law. The amount of activity in the capital goods industries depending not on the amount of activity elsewhere, but on the increase of activity elsewhere, it follows that a small set-back elsewhere involves a great recession in the capital goods industry. After that the scene is set for recovery; for a time the forces of recovery are strong enough to overcome small obstacles; but when they are nearly spent a little set-back will again produce a large depression (Harrod 1935*, TS pp. 8-10)

Before leaving for Copenhagen, Harrod had sent the first two Chapters of his book to James Meade for comments 21; Chapter 1 contained Harrod’s interpretation of statics, while Chapter 2 dealt with the multiplier, the accelerator (or Relation, in Harrod’s
terminology) and their interaction, and formulated the theory of the cycle. On January 12, 1936, Meade sent two long notes of comment relating to Chapter 2, the first concerning the assumptions regarding the rate of interest and the capital/labour ratio in relation to the accelerator, and the second regarding the conditions on the ratio between marginal and average cost for a redistribution of income in favour of profits. There followed an intense interchange of views (both viva voce and by letter) on both topics which lasted until 22 January and was shortly resumed on 25 and 26 February. In the meantime, on 13 January, Harrod communicated to Meade that he had already written the “best part of Ch. 3”; Meade received it on January 16, and read it hurriedly without apparently commenting upon it.

On 7 February Harrod expressed to Durbin the view -already formulated in Chapter 3 of the book, as it results from Meade’s mention of the relevant part of the Chapter- that the velocity of circulation increases in the boom, thereby providing the increased circulating medium necessary to permit the augmented volume of transactions accompanying growth. This was an important step, because in the structure of Harrod’s theory the velocity of circulation played the most important part of connecting statics and dynamics (this aspect will be discussed in detail in Chapter V § 4 below).

On 21 February, Hubert Henderson sent Harrod his comments, relating to Chapters 1 and 2. Henderson devoted his attention in particular to Harrod’s assumption that entrepreneurs maximise profits, and this subject almost monopolised their subsequent discussions, which protracted until the beginning of March. Then Harrod fell ill, and only at the end of the month he resumed the correspondence with Henderson, on the subject of a paper Henderson wrote on Keynes’s General Theory (Henderson 1936*). In the meantime, Harrod was toying with the idea of writing himself something on Keynes’s book, and he asked Robertson’s opinion about submitting to the Economic Journal an “account of how the theories of the G.T.E. […] stand in relation to ‘orthodox theory’”. Harrod had already delivered a speech to the Oxford Political Economy Club on these lines (Harrod to Robertson, 18 May, 1936), and his project eventuated in a paper Harrod presented in September at the Oxford meeting of the Econometric Society (the article was eventually published as Harrod 1937a). The paper was discussed beforehand with Keynes (the correspondence is partly transcribed in Keynes CW XIV, pp.83-86) and later with Robertson, who also handed it over to Pigou. As it will be seen in Ch V § 4 and Chapter VIII, § 7, this article is particularly important for its hints on Harrod’s interpretation of traditional theory and its relations to statics as intended in The Trade Cycle.

On May 1st, Meade had read and sent back to Harrod the last two Chapters of the book, enclosing a note on “The effect of subsidies on Employment” (i.e. relating to the last Section of Chapter 4 of The Trade Cycle) and some other comments regarding the foreign balance (last Section of Chapter 3). This must have occurred some time after Harrod originally sent the Chapters for comments, for in the meantime he had time to
change his mind on the topic of subsidies (Harrod to Meade, 4 May, 1936). In May, an article by Harrod on “Imperfect Competition and the Trade Cycle” (Harrod 1936b) appeared in the *Review of Economics and Statistics*, aiming at providing an explanation compatible with the general theory of value of the behaviour of prices and profits in the course of the cycle; this article thus summarised the third Section of Chapter 2 of Harrod’s book.

On 18 May, Harrod asked Robertson’s approval for the tone and temper of some passages of Chapter 3, where Harrod had referred to Robertson’s notion of saving. Since these passages were already mentioned by Meade on 16 January, Harrod must have been completing the last revision. In several places in the correspondence with Robertson, Harrod hinted that he was in a hurry to complete the revision of the book. Harrod mentioned that three copies of the book existed, “one copy I am working on, one is held by M[aureice] Allen, who, I am told, is notoriously dilatory and one by Hubert Henderson, to whom it is a red rag” (Harrod to Robertson, 21 May, 1936). Later, however, Allen actually provided some comments relating to Chapter 1, on “the stability influence of plasticity of prime costs” and “profits and plasticity of prime costs” (Allen to Harrod, und. note).

At the beginning of June the book was in proofs (Harrod to Robertson, 4 June, 1936), and appeared in print in September. Within the next year or so, the book “has been extensively reviewed […] has attracted the attention of subsequent writers and has been the subject of much discussion in academic circles” (Gaitskell, 1937). It is not possible to establish exactly which of these reviews have been read by Harrod. He certainly saw Joan Robinson’s (Robinson 1936), which he found “very fair and better than I had any right to expect”, although he pointed out that she failed to remark on the “absolute necessity for a dynamic analysis of interest and the consequent invalidity of all static (inc. classical) theories” (Harrod to Robertson, 25 Dec., 1936). Harrod discussed Robertson’s review (Robertson 1937a) and Hawtrey’s Chapter in *Capital and Employment* (Hawtrey 1937) dedicated to *The Trade Cycle* directly with the authors. Moreover, he discussed Keynes’s views, as expressed in some miscellaneous notes and some lecture notes on Harrod’s book that he wrote in March 1937 (Keynes CW XIV, pp. 151-179). Finally, among Harrod’s Papers lies a draft of Bretherton’s “Note on the Law of Diminishing Elasticity of Demand” (Bretherthon 1937*), who accepted the concept but provided some qualifications; I have not been able to locate Harrod’s reply.

Here I will not examine the reviews one by one, although some of the points raised by the reviewers will be discussed in Chapter V, note 38 and in Chapter VII below in the light of the line of development of Harrod’s dynamics (for a detailed discussion, see Besomi 1996a). It might however be noticed here that one year after the book was published Harrod remarked (not without a certain bitterness) that the core of his message had not been appreciated by the readers of the *Trade Cycle*.
I may say that in spite of a number of fingerposts in my book pointing at these dynamic determinants, no review that I have seen has paid any attention to them. I have no doubt that it is my fault for being too cryptic (Harrod to Robertson, 8 Oct., 1937).

Notes

1 The mistake in reporting the title of Keynes's book is quite curious, for the title which went in print was on the proofs Harrod had received in June 1935, while Keynes himself confirmed, apparently on Harrod's request, that the title was definitive on 27 November 1935. The book was however in the book shops from February 1936, and Harrod's preface, although undated, was certainly written after that. I have not been able to find any hint as to the origin of this mistake, and Clarendon Press could not help either; it is quite likely, however, that it was nothing more than a misprint.

2 Haberler to Harrod, 20 August, 1934. The correspondence on this memorandum interwove with the debate on Harrod's 1934 Economica article discussed in the previous Chapter.

3 Reference is to the first edition, part VI, Chapter II, of Pigou 1920.

4 In The Trade Cycle, Harrod adopted the acceleration principle under the name of 'Relation'. Later he claimed that he had "only learnt since publication that [acceleration principle] is a well-established term of art" (Harrod to Robertson, 25 Dec., 1936).

5 This is the more natural notion, given Harrod's analytical set-up, at which he arrived in the Trade Cycle and in his later reflections on technical change. For an appreciation of the difference between Harrod's and Hicks's definitions of 'neutrality' see Hicks 1963, pp. 343-346.

6 In the following years, Harrod consistently used a terminology based on notions such as 'roundaboutness', 'increased period of production' and 'capital intensity', and understood (and used) Hawtrey's notion of 'deepening' in the same sense. He seemed to think that the notion of the production function was not fully suitable to his scope, and accordingly devised a notion of 'neutrality' of technical progress different from that used in the other contest. This issue is interesting in the light of the debates on technical progress which raged in the 1960s, but since it would not carry the story told in these pages much further it will have to be discussed elsewhere.

7 Given the conditions that ensure the proportionality between rate of consumption and stock of capital goods, the equality of the respective rates of growth follows from the accelerator itself. It remains to be proved that if the stock of capital grows at a constant rate, investment also grows at the same constant rate.

Let \( K_t \) and \( I_t = \frac{dK_t}{dt} \) represent respectively the stock of capital goods and the rate of investment at time \( t \). Under the assumption of a constant rate of growth of \( K \), we have \( \frac{1}{K_t} \frac{dK_t}{dt} = \alpha \), that is \( \frac{dK_t}{dt} = \alpha \cdot K_t \), whose solution is \( K_t = c e^{\alpha t} \). Therefore \( I_t = \frac{dK_t}{dt} = \alpha e^{\alpha t} \), and the rate of growth of investment is \( \frac{1}{I_t} \frac{dI_t}{dt} = \frac{\alpha^2 e^{\alpha t}}{\alpha e^{\alpha t}} = \alpha \).

Of course, if \( \alpha \) is not a constant but a function of \( t \), the derivatives of \( K \) and \( I \) include the first and second order derivatives of \( \alpha \) as well, and the respective rates of growth will differ; more precisely, if the rate of growth of capital is increasing, the rate of growth of investment will be larger than the rate of growth of capital, and conversely.

8 This result was already enunciated, also in an intuitive way and only by way of comparison with stationary states and to stress the limits to proportionate growth imposed by diminishing returns of scarce factors, by Marshall, and later revived by Pigou: Marshall 1961: V, V, 3. (these passages date from the 4th edition of 1898), and Pigou 1935: 11-12.

9 It is perhaps worth emphasising that, to the best of my knowledge, the multiplier was mentioned only once in the correspondence that Harrod entertained with economists and politicians before reading the General Theory; it was named by Kahn in his letter to Harrod of 1 Nov., 1934, (the passage is quoted in Chapter III § 4). An earlier reference to the process of "secondary employment" can be found in a letter from Ernst Dick of 5 July, 1932, criticising Harrod's 'monetary' approach to policy (unfortunately I could not ascertain whether Harrod replied this letter).

10 It has been argued that the genesis of The Trade Cycle was largely independent of The General Theory (see e.g. Asimakopulos 1989, p. 345, and 1991, p. 138); Harrod himself claimed that "Much of my book
was thought out before I saw the proofs of [Keynes’s]’ (Harrod to Robertson, 25 Dec., 1936). The chronology indicates however that both the use of the multiplier and its interpretation as a pivot for inverting the traditional relationship between saving and investment could only come after Harrod had read Keynes’s book.

11 There survive no documents certifying when exactly Harrod sat down to write his review. However, it was ready by the beginning of October, 1935, since Harrod discussed it with Robertson, who found it very interesting (Harrod to Robertson, 3 Oct., 1935, and Robertson to Harrod, 5 Oct., 1935).

12 Durbin wholeheartedly accepted Harrod’s criticism (Durbin to Harrod, 4 Feb., 1936). In his reply, Harrod suggested the following analogy: “You can advance in a tiny little boat to within a few yards of the foot of the Niagara Falls. Very different would be the case if they suddenly began to pour into a hitherto undisturbed lake” (Harrod to Durbin, 7 Feb., 1936).

13 For Robertson and Durbin on *The Problem of Credit Policy*, see E. Durbin 1985, pp. 154-155.

14 After having read *The Trade Cycle*, Robertson commented as follows:

> A propos of this whole topic, and of your book, I feel that there is probably an important distinction to be drawn between forms of investment which are closely “geared” by the “principle of acceleration” to the demand prices of particular kinds of consumption goods, & those -especially power and transport in all their numerous forms- which are highly susceptible to invention on the one hand, and dependent on rather vague estimates of ultimate consumers’ prosperity on the other (Robertson to Harrod, 23 Dec., 1936).

Robertson later enclosed a remark in this sense in his review of Harrod’s book (Robertson 1937a, p. 126).

15 See the correspondence with Keynes on the *Trade Cycle*, in Keynes CW XIV, pp. 167 and 169, and Harrod 1939a, pp. 26-27.

16 On 10 October, Harrod wrote to Kaldor: “I have been studying Keynes’ new system of thought, which is very interesting …. I feel I now understand what he has to say - which I fear many of his readers wont”.

17 “Do not suppose that if I do not use your terminology in writing to you or elsewhere that I do not regard it as valuable. I am sure it is, because I believe you have got good results with it. It is difficult to hold various terminologies in one’s mind at the same moment, and if one is thinking along certain lines one is almost driven to cast out for the moment, but not necessarily to limbo or perdition, rival terminologies” (Harrod to Robertson, 11 Nov., 1935).

18 Harrod further specified his query as follows:

> Suppose that by a miracle wages rose in precise proportion to the marginal efficiency of labour over a term of years. And suppose the monetary authority thought it right to keep the price level stable by a suitable adjustment of the quantity of money. (Suppose also constant \( V \)). Do you think that in fact \( S \) would be less than equal to or greater than \( I \) on your def[initions] of \( S \) and \( I \)?

(Harrod to Robertson, 9 Oct., 1935).

19 In the meantime, however, Harrod wrote a note for the *Economic Journal* proposing “Another Fundamental Objection to *Laissez-Faire*” (Harrod 1936c), which he submitted in the first half of October and revised soon after. This is certified by two letters he received from Keynes, one dated October 16 acknowledging receipt of the note, and the second dated October 23. There Keynes commented on the additions Harrod had proposed to meet Keynes’s previous request of giving sufficient emphasis to the assumption on which the argument was based.

20 It must be noticed, however, that instead of recognising that new investment is the cause of additional saving, as the multiplier would imply, Harrod still spoke of investment as one of the alternative uses of saving (see the text for the actual quotation), and therefore implied the possibility of a divergence between saving and investment, which he presented as a cause for the inequality of demand and supply of goods and thus as the cause of depression. This view clashes against Harrod’s opinion on saving and investment as expressed in the last year or so, and also against the exposition of the multiplier in the *Trade Cycle* (which must have been written a few days later only); this leads to suppose that he used it as an expository device which he might have thought more appropriate to the audience, probably mainly composed of British traders operating in Denmark.

21 It is difficult to decide whether Harrod had sent the original manuscript or a typed transcription. On the one hand, Meade wrote he had read the “MS with great pleasure” (Meade to Harrod, 12 Jan., 1936). On the other hand, the pagination of Meade’s copy seems to coincide with the page numbers mentioned by
Henderson, which suggests instead that it was a typescript copy. Allen, who in May had one of the three copies of the draft of the book, explicitly mentioned a typescript (Allen to Harrod, und.).

22 To the best of my knowledge, neither the original manuscript nor the TS circulated thereafter survive. However, judging from the page number of the readers’ comments, the original structure of the book was maintained in the published version.

23 The occasion for expressing this view came in the course of an exchange with Durbin regarding Harrod’s review of Credit Policy. Durbin was still reasoning in terms of the principle that saving has to provide the resources necessary to investment beforehand, and he could not therefore understand how could consumer’s income rise “without a prior increase in producers’ credits and a price boom in the capital industries” (Durbin to Harrod, 4 Feb., 1936; emphasis mine). Harrod’s reply ran as follows:

    you wish to connect the boom with an increase of producers’ credits. I dont deny that there often is an increase of producers’ credits. But I don’t regard it as the leading feature of the boom. Money may (and does) circulate more rapidly (Harrod to Durbin, 7 Feb., 1936).

24 On Henderson’s reaction, Harrod remarked:

    His reaction to it, when first manifested last term, did, I confess, rather put me out. His great gravamen was that I made the wholly unrealistic assumption that the entrepreneur tries to maximize profit. I dare say it is unrealistic, but what can the poor theorist do? (Harrod to Robertson, 21 May, 1936).

25 For a description of how Harrod was upset by Allen’s delay, and in general for an account of the personal feelings involved in the discussions with other fellow economists relating to the preparatory phase of the book, see Young 1989, pp. 130-131 and in general the whole of Chapter 4.

    Young suggests that, following Allen’s advice, in his 1939 “Essay” Harrod gave prominence of role to expectations and anticipations. But, as I will argue below (Chapter VII), in the “Essay” expectations and anticipations played a marginal role only; this view thus seems unwarranted.
Chapter V
Statics and Dynamics in The Trade Cycle

The preparation of the ground for the writing of Harrod’s book on The Trade Cycle began more than ten years earlier. The first brick in Harrod’s building was the methodological principle that the study of the forces determining the level of output must be preliminary to the study of the determinants of its variations (Chapter I, § 4). The second and most fundamental step towards his dynamics came much later, in 1934, and consisted in the epistemic principle that the possibility of trade cycle theorising imposes to eliminate at the outset the ties binding the economic system to the state of rest postulated by the traditional approaches to the theory of value and production. This principle implied, as a constructive corollary, that the equilibrium of the system -either static or dynamic- must be capable of some degree of instability, while as a critical corollary it entailed the rejection of the explanations seeking the cause of the cycle in the fluctuations of the state of costs or of preferences, or in alternations of pessimism and optimism (Chapter I, § 3). This premise permitted him to conceive the theoretical possibility of a persistent state of the system, characterised by steady advance (Chapter III § 4). Finally, another year later Harrod had gathered the analytical tools which enabled him to give expression to his intuition (Chapter IV, §§ 1 and 2), and by mid-1936 he had formulated his first comprehensive model of the cycle and completed an important step towards the definition of his peculiar notion of dynamics.

An explicit definition of ‘dynamics’, however, was not formulated in the book, and the purpose of this Chapter is thus to contribute to the characterisation of such a notion. This requires in the first place to understand what part the different components of Harrod’s reasoning (epistemic, methodological, analytical and onthological) were playing, and how they were related. Harrod in fact had provided several qualifications for a system to be dynamic, each pertaining to a different level in the construction of the theory according to whether he was considering the problem of the possibility of a theory of the cycle, or of the mode of approach to economic movement, or of the instruments of analysis, or of the object of the theory.

In this Chapter I will argue that the concept which organises Harrod’s thoughts is the postulate that endogenous movement can only be conceived if some destabilising force permits at the outset a departure from equilibrium. This is suggested both by the genesis of Harrod’s ideas and by his own logical arrangement of the argument in The Trade Cycle. I will therefore begin the study of Harrod’s conception regarding the particular characteristics of a dynamic system by examining the role of the premises of his reasoning in outlining the framework onto which he developed the analytical set-up. Then I will discuss their implications on the working of Harrod’s specific mechanism and the
connections between the specific components of the analytical structure of the book, and finally I will examine the various facets of ‘dynamics’ as characterised by Harrod’s reasoning in its different phases.

The instability principle, which is a corollary of Harrod’s epistemic postulate, permeates his reasoning throughout. It is applied twice in the working of the trade cycle mechanism, for it first permitted an escape from the static equilibrium and therefore to imagine different sorts of states of the system, and secondly it permitted an escape from the moving equilibrium of advance, thereby making it possible to conceive of the cycle as a succession of systematic departures from such a state. But the introduction of instability in the static system also made it necessary to re-shape the traditional instruments used to determine the equilibrium level of output. Moreover, the connections between the various building blocks of Harrod’s model (the dynamic system, the static compartment and the monetary domain) were also devised on epistemic grounds, while lacking analytical consistency. Finally, Harrod’s choice of the method of approach explicitly changed where the principle of instability required it.

In this Chapter I will therefore try to show how Harrod characterised the different facets of his notions of statics and dynamics according to the specific roles the instability proposition was to play in different levels of his theory, and accordingly how he conceived of the links between static and dynamic forces.

Since the Chapter is very long, the reader may appreciate having a more detailed outline of its content, to use as a reading guide. Section 2 will be dedicated to Harrod’s re-formulation of the traditional approach, so as to make it suitable to configure the static equilibrium with the necessary amount of instability to permit the occurrence of fluctuations while also accounting for the entrepreneurs’ rationality in maximising their profits. In this Section it is also shown how Harrod’s dynamic analysis was conceived and formulated in analogy with the procedure followed in the case of statics, and how the instability of the dynamic equilibrium constituted a premise for the possibility of the cycle.

In the next Section, some features of statics and dynamics will be compared. From the fundamental distinction as to their capacity of accounting for economic movement, Harrod identified the object of the discipline as being the determination of the level of output for the case of statics, and of its variations for dynamics. Correspondingly, he characterised the respective notions of equilibrium as describing a state of rest and of steady growth. Static and dynamic forces, however, in Harrod’s view operated at different levels, the former (costs and preferences) affecting the individual motives and the latter (multiplier and accelerator) determining the movement of the system as a whole. Such a distinction entailed different notions as the corresponding concepts of equilibrium, which in statics was conceived of as referring to individual maximisation of utility or profit while in dynamics implied the reproduction of the state of the system.
Harrod was not fully aware of the implications of the coexistence of the two sets of forces operating on the individual and on the systemic levels, as it clearly results from the confusion surrounding his crucial discussion of the role of expectations in the determination of the moving equilibrium. Harrod, however, realised that it was necessary to make explicit the relation between static and dynamic forces, with particular regard to the question of how the dynamic laws determining the increase of output compel the individual producers to increment their own level of production, compatibly with their ‘fundamental conditions’ of costs and preferences. This problem lies at the heart of the relationship between statics and dynamics, and Section 4 will therefore be devoted to the discussion of Harrod’s conceptual re-organisation of economics around this theoretical core. Harrod’s solution, in fact, saw in the changes in the general level of prices, induced by the variations in output via changes in the velocity of circulation of money, the mechanism transmitting the effect of the dynamic forces to the individual motives. This approach led Harrod to interpret the theory of money as providing the necessary link between static and dynamic laws (and, incidentally, to interpret Keynes’s *General Theory* as being structured in a similar way).

Sections 5 and 6 will examine in detail the specific mechanism transmitting to the individual producers the cogent stimulus to adapt to the general laws of movement determined by the multiplier and the accelerator. This mechanism is based on fluctuations in the general price level, which in turn implies appropriate oscillations in the velocity of circulation of money. Here it will thus be necessary to discuss in the first place the relationship Harrod thought to link money and prices (Section 5), and in the second place how the fluctuations in the volume of economic activity induce the suitable changes in the proportion of money excluded from the process of effective circulation (Section 6).

Section 7 will discuss some weaknesses of Harrod’s scheme. Harrod gave the highest priority to his epistemic postulate and to the instability principle, to the extent that he deliberately sacrificed the methodological consistency of his approach. However, he failed to provide a satisfactory analytical construction capable of matching the subtle system of connections he devised on the epistemic level.

Section 8 will draw out the conclusions achieved so far, while Section 9 will aim at introducing the transition towards the subsequent stage of Harrod’s dynamic theorising, the 1939 “Essay in Dynamic Theory”. In his book, Harrod had provided an interpretation of his model, alternative to the one presented in the first Sections of this Chapter, in which he shifted the emphasis from the forces affecting the operation of the multiplier and accelerator towards the interaction between saving and investment decisions. I will thus compare the analytical implications of Harrod’s interpretations, concentrating in particular on the causal significance of the model and on the role of the formation of expectations in the process of decision taking. In a way, the “Essay” will turn out to be a hybrid between Harrod’s two original interpretations. Here I will only anticipate which of these peculiarities of Harrod’s model were privileged and which
instead were discarded, leaving for Chapter VII a detailed comparison between the
notions of dynamics underlying Harrod’s book and its subsequent development.

Before plunging into the detailed examination of Harrod’s notions of statics and
dynamics, however, in Section 1 I will briefly outline the essentials of Harrod’s
argument, giving particular emphasis to the aspects which will be discussed in the
following Sections.

1. The Trade Cycle

The structure of the Trade Cycle reflects the methodological principle which
Harrod set out in 1925 (see Chapter I, § 4), according to which the analysis of the trade
cycle has to provide answers to two questions, the first preliminary to the second: What
causes the level of output to be what it is?, and What determines its rate of increase? As to
the first problem -discussed in Chapter 1 of the book-, Harrod found the reply in the
orthodox (static) analysis of utility and productivity, which he reformulated in terms of
forces inducing (or inhibiting) the individual producers to expand their output. In Chapter
2, he treated the second (dynamic) problem by applying the same approach, that is by
identifying a set of forces responsible for the maintenance, expansion or reduction of the
rate of increase of output.

In the opening Chapter of his book, dedicated to the quest for the factors
determining the level of output, Harrod began by examining the considerations guiding
an isolated producer (Robinson Crusoe on his desert island) in his decision as to whether
or not to increase production by an additional unit. He progressively generalised his
conclusions for the case of members of a community where products are exchanged (on
markets where competition is not necessarily perfect), where capitalists hire workers, and
finally where payments are made in money rather than in kind1. At the end of this
process, Harrod found four static determinants2: “(i) The rates of pay at which prime
factors of production can be secured. (ii) The efficiency of the prime factors. (iii) The
elasticity of demand for commodities. (iv) The general price-level” (Harrod 1936a, p.
50). These are the factors producers consider when deciding whether to use their
machinery more intensively and/or to hire more workers in order to increase output. The
law of Plasticity of Prime Costs is associated with the first determinant. This deters
producers from expanding output indefinitely: in fact, to apply a further unit of work, the
producer has to consider that his decision, so far as it resorts to overtime, implies
payment of higher rates to the individual workers, and so far as it involves a decrease in
unemployment it induces an increase in the general rates of pay, while on the other hand
available labour becomes less and less efficient (ibid., pp. 27-29). The efficiency of
prime factors obeys the Law of Diminishing Returns (though probably only when plants
are operating close to full capacity), which also deters producers from expanding output,
because the amount of output that can be obtained by applying a further unit of work
diminishes as the quantity of effort per unit of time increases (ibid., pp. 30-31). An
additional impediment is derived from the difficulties of marketing additional quantities of the product in conditions of imperfect competition: as people become more affluent, their sensitiveness to price differences declines, so that the producer will have to accept a lower rate of exchange for his commodity. Harrod called this, the Law of Diminishing Elasticity of Demand\(^3\) (ibid., pp. 15-22). Finally, the fourth consideration affecting the producer's decisions is the movement of the general price level\(^4\): if it is increasing, higher returns are expected and production is stimulated, and vice versa. This factor may thus counteract the adverse operation of the first three static determinants\(^5\).

None of these forces, whose strength depends on the actual output, directly determine the level of output, but they only relate to the motives for increasing (or decreasing) production. The first three determinants, and the three associated laws, tend to exert a stabilising effect, in the sense that once equilibrium is reached they discourage abandoning it, while the fourth determinant acts in the opposite way. Equilibrium is the state in which incentives and deterrents balance each other, so that no tendency to change in either way can predominate; in such a case, the producers are satisfied with the prevailing state of affairs. Equilibrium is stable if any deviation from it sets in motion the tendency to return to it; it is unstable if, on the contrary, the forces corresponding to the new state give motive to move further from it; it is neutral if the new position is indifferent (i.e., if in the new position incentives and deterrents still balance each other).

Fluctuations and growth of output require as a premise that the level of output can change, without being permanently tied to an equilibrium level (on Harrod's early discussion of the epistemic implications of this postulate, see Chapter I § 3). The actual occurrence of the trade cycle thus implies that the equilibrium is not stable. Equilibrium must also not be treated as unstable, for this would imply restlessly cumulative growth or depression. In Harrod's view, the solution lay in the middle: equilibrium must be conceived as neutral, so that the static determinants can accommodate to whatever the forces responsible for movement decree. In other words, the static forces must balance over the whole field, and in particular the destabilising influence of the monetary determinant (the general price level) must exactly compensate the stabilising effect of the other three determinants. For instance, while in a boom - whose causes have still to be explained - the first three determinants tend to counteract the advance, and this tendency is offset by the increase of prices: “the de-stabilizing influence of money embodied in the ups and downs of prices may be taken to be a measure of the power of the other three stabilizing forces” (ibid., p. 43).

Turning to the causes of movement, in his Chapter 2 Harrod considered two mechanisms whose interaction provides the origin of advance: the accelerator (Relation, in Harrod’s terminology) and the multiplier. The former is an arithmetical relation, linking the need for new capital goods to the expectations of demand for consumption goods, and the latter is the doctrine according to which the amount of saving
The symbol $\Delta C_{t}^{e_{t-1}}$ represents the increment of consumption for time $t$ as expected at time $t-1$, $\Delta$ as usual indicates an increment, $I$ and $Y$ stand respectively for net investment and income.

Harrod had a propensity to interpret the progress of society in terms of rates of growth (for the antecedents of this attitude, see Chapter III § 2, and Chapter IV § 1). Such a reading was permitted by the comparison between increments and absolute values of some variables, that the interlocking of the mechanisms of the accelerator and the multiplier naturally implies. In particular, Harrod was interested in asking his dynamic mechanism the same questions that traditional theory asked the static system: does some equilibrium rate of growth exist, that possesses the same attributes as static equilibrium? If so, what can we say about its stability?

The procedure followed by Harrod was strictly analogous to that pursued in his reinterpretation of static analysis as a system of forces. His first problem was therefore that of recognising the set of forces determining the magnitude of the effects on net investment of expected increases in consumption, and of net investment on income and thus on consumption. The three dynamic determinants were: (i) the propensity to save, (ii) the distribution of income, and (iii) the quantity of capital necessary for the production of a unit of output involved by the method of production in use. As in the static case, the value of the determinants is not given once and for all, but varies with the state of affairs. On the one hand, “there is reason to believe that people tend to save a larger proportion of a higher income”, while “experience is that there usually is a shift to profit in a pronounced upward movement” (ibid., p. 92). One can therefore expect the value of the multiplier to fall in the course of a boom and to rise during a depression. On the other hand, new investments provide the occasion to introduce new productive techniques (ibid., pp. 93-94). It is therefore possible that in the course of advance the amount of capital per unit of output increases, thereby augmenting the quantity of investment required and possibly counteracting the restrictive force of the other two determinants.

Again analogous with the case of statics, equilibrium corresponds to the contingency in which the dynamic forces balance each other out, and is characterised by a steady rate of growth. It is easy to see that in such a circumstance the increase of consumption due to the new net investment is such to give employment to the capital goods exactly in the measure that was desired by the capitalists on the ground of their expectations regarding future demand. In other words, the entrepreneurs feel satisfied with the results ensuing from their investment decisions, and -unless a change in the
value of the determinants occurs - feel disposed to continue along the same path of advance\(^\text{10}\). The analogy with statics also holds regarding the fact that the dynamic determinants do not directly control the rate of growth of income, but only establish “whether the existing […] rate of advance is to be increased, maintained or diminished” (ibid., p. 92).

If the dynamic equilibrium was stable, the model would not describe a cycle, but only self-sustained growth at a constant rate. Consistent with his reasoning regarding the stability of static equilibrium and the possibility of growth, Harrod thus maintained that the moving equilibrium cannot be stable. As a matter of fact, in his theory there is no mechanism assuring that the expansive influence of the third dynamic determinant will balance the restrictive influence of the other two\(^\text{11}\). On the contrary, when disappointment of expectations first occurs entrepreneurs react by slowing down the rate of investment, with the downward cumulative consequences that can easily be followed iterating the causal chain summarised in the above schematic representation. The depression, however, will reach a bottom:

So long as there is some output, some replacement is necessary; this may give an opportunity for introducing more capitalistic methods. Moreover, inventions and improvements proceed -and, indeed, are diligently sought for by desperate entrepreneurs- and these may make it worth while to scrap existing plant even before its pre-ordained term. Happily the rate of interest is inclined to fall in the slump and this may stimulate the process of substituting more capitalistic methods […]. For this reason there is some chance that a point may be reached, well above zero output, at which the necessary replacements and improvements of plants still profitable will exceed the greatest amount that can be got for the amortization funds, and that this excess will be sufficient to absorb what will be voluntarily saved at that level of output and income. This is the bottom.

[…] Once the bottom is reached, revival is likely to come, for the mere passage of time increases the amount of replacements required to maintain a given level of output. This increase involves a rise of net investment. This gives scope for the three dynamic determinants to ordain a period of steady or cumulative advance (ibid., pp. 100-101).

A peculiarity of the approach of the Trade Cycle must be highlighted, for later it generated some confusion (Chapter VII, note 8). Harrod treated equilibrium growth not only as a reference path for the study of the cycle, but as the actual process characterising the boom. After having explained to Keynes why equilibrium growth consists in a constant rate of increase, Harrod replied as follows to a possible criticism:

You may suppose that population or efficiency in fact shows an arithmetic increase. That may be so. If it is we must have a cycle to allow things to get into arrears and then go forward for a time in a geometric spurt (Harrod to Keynes, 6 Apr., 1937, in Keynes \textit{CW} XIV, p. 164)

Harrod of course considered ‘cumulative advance’ as a possible mode of growth, but thought that there is an asymmetry between advance (whether steady or cumulative) and recession:

so long as there is something in the nature of acceleration, all sorts of combinations of relation and multiplier values are possible. But as soon as there
is any sign of deceleration investment must sink down towards zero. That is the peculiar and essential nature of the slump. [...] What is striking and seems at first out of all proportion is the calamitous nature of the slump. The amount of recession seems out of all accord with such changes as may be occurring in ‘fundamental conditions’ [...] Now I believe that the secret of this unexpected, disproportionate movement depends on this unsymmetrical relation between the relation and the multiplier that I have explained. It is always possible to accelerate, but it is not possible to decelerate without starting again from the zero line (Harrod to Keynes, 15 Apr., 1937, ibid., pp. 174-175).

2. Static and Dynamic Forces, and Equilibrium

With *The Trade Cycle*, Harrod was able for the first time to translate his epistemic considerations regarding the possibility of a theory of the cycle into an analytical mechanism and a corresponding methodological framework. The result was not only an original model of business cycles, but also the implicit definition of a new concept of *economic dynamics*, associated with a new notion of *equilibrium*.

In the previous Section, I hinted that the method and the analytical procedure of Harrod’s dynamics were developed in continuity and in strict analogy with the method and procedure of statics. The notion of statics itself, however, was not simply transposed from some other domain or adapted from previously existing definitions given by other authors. On the contrary, Harrod founded it anew on the basis of a re-interpretation of the scope and limits of the traditional theory of value. Dynamics, in turn, was developed as a complement to statics, in order to provide the instruments necessary to tackle the problems left unsolved by static theory. These aspects are extremely important for the understanding of the genesis and the nature of Harrod’s notions of both statics and dynamics, and will be discussed in detail in Chapter VIII. This premise suggests that the study of the properties of Harrod’s dynamics cannot take as a starting point the countering of statics and dynamics, because these notions co-evolved in the development of Harrod’s thought. The appropriate starting point must rather be the considerations which induced Harrod to regard the traditional instruments of thought as inadequate for providing an *explanation* of the cycle.

Given the relevant circumstances (the state of preferences, technique etc.), the orthodox supply and demand analysis postulated the existence of a mechanism determining the stable equilibrium levels of prices and quantities, so that any attempt to produce more or less would set in motion forces tending to bring the old situation back. In such a mode of thought, the only way to conceive fluctuations or growth of output would be to imagine that the ‘fundamental conditions’ alter accordingly, e.g. by waves of optimism and pessimism, or by systematic mismanagement of the banking policy (see Chapter I § 3, for Harrod’s criticism to Pigou along these lines12). But Harrod explicitly refused to accept such attempted solutions as true explanations of the cycle:

*We do not* suppose the cycle to be governed by cyclical variation in the fundamental conditions (utility functions, cost functions etc.). There may be some variations there, but we reject them (Harrod to Keynes, 7 April, 1937, in Keynes CW XIV, p. 169).
As a matter of fact, the orthodox solution negates a dynamic approach by excluding from its own premises the possibility of change as a specific feature of the economic system. The causes of movement were attributed to mistakes, when the authority incorrectly evaluates the interest of the community, or when entrepreneurs misjudge their own convenience. Equilibrium was thus conceived as a natural state of the system, towards which any accidental deviation would be attracted; in order to be enduring, any other configuration should be sustained by a specific cause operating permanently.

Harrod’s approach was meant to provide a reply to two needs left unanswered by the approaches following the traditional line. On the one hand, ‘motion’ must be conceivable as a state of the system capable of existing without the necessity of being supported by the action of some ad hoc cause, and on the other hand changes in output must be consistent with the rational pursuit of the entrepreneurs’ self-interest, in the form of maximisation of profits. Correspondingly, on the one hand, Harrod revised the notions of motion and equilibrium and accordingly devised a discipline of economic dynamics with its own method and domain of application capable of dealing with it. On the other hand, he also revised the traditional, static approach in order to make dynamics consistent with it.

As to the first of these problems, I think the best way to understand Harrod’s line of attack is to regard it as analogous to the Galilean rejection of the Aristotelian physics. According to the latter, different bodies reside in their ‘natural’ place and correspondingly move according to different laws of motion: heavenly bodies move in circles and terrestrial bodies in straight lines, heavy bodies fall while light bodies ascend. Any movement along a different, unnatural path could occur only if an impelling force was imposed on the body, and could persist only as long as the impulse persisted. In this conception, movement thus needed a medium in which to take place: hence the indispensable role of air in sustaining motion by perpetuating the cause of its occurrence. Motion was thus conceived as a process of change which could not spontaneously occur or automatically protract unless, and only as long as, the action of its cause or motor persisted. Galileo substituted the Aristotelian notion of motion with an entirely different conception. By opposing the uniformly accelerated motion characterising a perfect sphere rolling down a smooth, inclined surface to the uniformly retarded motion of a sphere rolling up the same surface, he imagined that the sphere rolling on a horizontal, frictionless surface would maintain its constant velocity. In such a conception air was seen as an obstacle, and no longer as a support of motion. Galileo’s achievement thus presupposed the elimination of the very factors that enabled motion to occur in the Aristotelian conception. For Galileo’s contemporaries, his notion of motion was quite paradoxical. It is therefore not surprising that Galileo did not himself formulate the principle of inertia, though his mechanics was implicitly based on it: its full sense and implications were first fully understood only by Descartes, and later the principle was given pride of place in Newton’s first Law of Motion. This affirms that “every body
continues in its state of rest or of uniform motion in a right line, unless it is compelled to change that state by forces impressed upon it.” The notion of a self-sustaining *status* of motion, as opposed to the conception of motion as a *process* maintained by some external action, is the true core of the distinction between the physics of Newton and Galileo on the one hand, and the Aristotelian physics on the other:

> *Status* of motion: by using this expression Newton implies or asserts that motion is not, as had been believed for about two thousand years -since Aristotle- a process of change, in contradistinction to *rest*, which is truly a *status*, but it is also a *state*, that is, something that no more implies change than does rest. Motion and rest are [...] placed by this word on the same level of being, and no longer on different ones. [...] Now it is precisely and only because it is a *state* -just like rest- that motion is able to conserve itself and that bodies can persevere in motion without needing any force or cause that would move them, exactly as they persist at rest. It is obvious that bodies could not do so as long as motion was considered a process of change. Nothing changes without a cause -at least before quantum physics- as Newton expressly states. Thus, so long as motion was a process, it could not continue without a mover. It is only motion as *state* that does not need a cause or mover (Koyré 1965, 66-67).

Obviously «not all motion is such a *state*, but only that which proceeds uniformly and in a right line, *in directum*, that is, in the same direction and with the same speed» (Koyré 1965, p. 67).

The analogy with Harrod’s notion of *moving equilibrium* (Harrod 1936a, p. ix) is precise. Like Galileo substituted the Aristotelian argument in terms of impressed force (*impetus*) and displacement from the body’s natural place with a reasoning based on velocity and direction, Harrod abandoned the traditional explanation in terms of *exogenous* causes of divergence from equilibrium to replace it with a reasoning in terms of self-perpetuating rates of advance. Harrod’s equilibrium growth is in fact a state of ‘inertial’ motion, in the sense that once the system is in such a state, its motion persists without the need for additional causes sustaining it. At this juncture, the problem was not that of figuring out the process leading to such a state of affairs, but to assume as a starting point its abstract possibility, in order to determine the conditions compatible with it, and to study its stability.

To understand this crucial step of Harrod’s reasoning it is necessary to reflect on his reformulation of the partial equilibrium analysis in terms of static determinants. The traditional schedules of demand and supply were derived respectively from the individual’s considerations regarding the relative utility of money and of goods, and from the firm’s cost function. Harrod resolved these factors in their elementary components, and treated them as independent forces acting on the entrepreneurs’ decisions regarding increments or decrements of production. His argument was thus expressed in the Newtonian language of vectorial *forces* conceived as *causes* of change. The static determinants, in fact, comprise all the forces determining whether the existing *state* of the system is to be maintained, or whether its size is to be increased or diminished (Harrod 1936a, pp. 7 and 9-11. The same applies to the dynamic determinants: in such a case, the state to be considered is the equilibrium growth: ibid., p. 92). The next step was to
examine how the inducements (or deterrents) exerted by these forces change in correspondence to different levels of output. The gradient of the schedule expressing this relation was interpreted as indicating whether the effect of the force is stabilising, neutral or destabilising (the corresponding step in traditional analysis was to inquire how the utility of goods etc. influenced the degree of stability of the equilibrium price and output levels by determining the gradients of the demand and supply curve). Equilibrium in turn was conceived as the configuration of the system characterised by a balance of the inducements and deterrents to alter the established position; in other words, in equilibrium the resultant of all the forces is zero so that there is no cause inducing abandonment.

The possible configurations may be represented graphically. By way of illustration, the inducement schedules are coupled with a diagram representing the forces acting on a marble in the corresponding situations of stable, unstable and neutral equilibrium:

\[
\begin{array}{ccc}
\text{Quantity produced} & \text{Quantity produced} & \text{Quantity produced} \\
\Sigma \text{inducements} & \Sigma \text{inducements} & \Sigma \text{inducements} \\
q^* & q^* & q^*
\end{array}
\]

Stable equilibrium Unstable equilibrium Neutral equilibrium

In the upper part of the figures, inducements and deterrents have been added together. Equilibrium is the state of rest corresponding to any situation in which there are no inducements to produce more or less. In the lower part of the figures, such a case occurs when the supporting force of the ground (directed perpendicularly to it) exactly balances the gravitational force (directed vertically downwards).

Up to this point, Harrod’s results regarding the determination of the level of output do not differ from the traditional conclusion: if the schedule of the inducements emerging from the resultant of the forces is inclined negatively, the static forces uniquely determine the equilibrium quantity and price of production (the price level is not indicated on the graph, because it is one of the static determinants and is therefore included among the components of the resultant of the only force indicated. The price level corresponding to the state of the other static determinants is found by countering the first three determinants against the fourth). What was the purpose of Harrod’s exercise then?
Although in this case Harrod’s rephrasing of the traditional analysis of the determination of the level of output enables restatement of the same result in different terms, in general the two representations are by no means equivalent, since Harrod’s version allows a discussion of a case -of extreme importance for his treatment of the cycle- that would be very awkward to express in terms of demand and supply curves.

Having renounced resort to ad hoc causes for explaining fluctuations of output, Harrod had no other choice but to refute the assumption that equilibrium is stable. On the other hand, in Harrod’s eyes the assumption of instability of the static equilibrium would have implied a different but related sort of drawback; this clearly emerges from Harrod’s criticism of the cumulative mechanism postulated by Keynes in the Treatise:

Entrepreneurs were conceived never to be in temporary equilibrium during the process of boom and slump. (Were they even in temporary equilibrium at the turning points? Surely not!) They were always behind the times, producing more or less than the current conditions of loss or profit dictated. It is by no means clear that this treatment corresponds with the facts of the case. It is by no means clear that at any and every given point of the boom the representative entrepreneur, if asked whether he now judged that in the immediately preceding period he had produced too little, would answer in the affirmative. Yet that is what the notion that the present level of profit requires him to revise his ideas upwards implies. [...] The same arguments applies to this analysis of the recession (Harrod 1936a, p. 66).

Both the assumptions of stability and instability imply that the entrepreneurs act irrationally throughout the course of the cycle. If equilibrium is stable, the fact of the cycle can only be explained by supposing that entrepreneurs are guided by waves of unjustified optimism and pessimism. If instead equilibrium is unstable, entrepreneurs can never be satisfied with the amount they have produced. Harrod’s solution was to lay in the middle.

Consistent with the formulation of traditional theory in terms of inducements and deterrents, the wording of the criticism of the Treatise on Money again suggests that Harrod was thinking of the satisfaction with the level of profits as the actual engine driving the entrepreneurs’ decisions. At the same time, Harrod’s accusation of Keynes for not having discussed the marginal position of entrepreneurs (ibid., pp. 66-67) reflects his opinion that the theory must postulate the consistency of the fluctuations of output with the entrepreneurs’ rational search for maximum profit:

In the following argument it will be assumed that entrepreneurs arrange matters in the short period, so far as they can, to make marginal revenue equal to marginal cost. This is the condition for the maximization of profit in any given set of circumstances. Mistakes, of course, are often made. [...] But to suppose that in the short-period decisions how much current output to produce there is systematic error in one direction enduring throughout the whole phase of the boom or the whole phase of the slump by all or most entrepreneurs seems to me altogether far-fetched. If my view be accepted, it can legitimately be assumed that broadly -with minor discrepancies- entrepreneurs do equate marginal revenue to marginal cost in determining the level of current output (Harrod 1936a, pp. 75-76).
This reasoning led Harrod to take a twofold decision regarding the construction of his trade cycle theory. On the one hand, he adopted the view that equilibrium is neutral. Accordingly, he postulated that the static de-stabiliser, viz. the level of prices, moves on any occasion in such a way as to balance the stabilising effect of the other determinants. On the other hand, he treated the forces determining the growth of output as operating on different grounds than the static forces.

With regard to the first aspect, the neutrality of equilibrium assures that instant after instant entrepreneurs are in equilibrium. That is, they feel they are doing the right thing given the structure of their costs and demand. In the second place, it allows ‘inertial’ motion to occur: no change in output encounters any resistance on the part of the static forces, so that any sort of motion compatible with what the dynamic determinants decree is conceivable. The analogy with the case of a marble lying on a flat surface best illustrates Harrod’s point. At any given point on the table the force of gravitation is exactly offset by the resistance of the surface; therefore, if the marble is placed anywhere on it, there it stays. At the same time, if the marble is set in motion, its state of uniform (i.e., rectilinear and at constant velocity) motion will persist (until and unless some other force will disturb it -e.g., friction). The analogy with Harrod’s notion of steady (equilibrium) growth is once again precise. Once the economic magnitudes start growing at a constant rate consistent with the configuration of the dynamic determinants, its mode of advance persists until and unless a restrictive or expansive dynamic force prevails, upsetting the harmony of progress.

As regards the different levels on which the static and dynamic forces operate, it must be noticed that the static determinants affect the individual entrepreneur’s self-interest on the basis of some given fundamental circumstances. On the contrary, the dynamic determinants do not affect, and are not affected by, the achievement of self-interest, but rather regard the functioning of the economy as a whole. In fact the accelerator, and the dynamic force determining the intensity of its mode of operation, relates the decision of investment to the prospected increase in the demand for goods. The actual increase depends in turn upon the multitude of individual acts of expenditure for investment and consumption goods. This process, which is regulated by the determinants influencing the magnitude of the multiplier, will eventually justify the expectations that set the mechanism in motion:

You have now to consider the economy as a whole, with the tendency to increase or decrease in all its parts. In fact you are in the dynamic system (Harrod to Robertson, 8 Oct., 1937).

Thanks to the distinct nature of static and dynamic determinants, the multiplier-accelerator mechanism can be superimposed on to the static equilibrium, and determine a mode of motion which is compatible with it. This is precisely the sort of process that cannot be represented in terms of the old supply and demand curves. In fact one would have to imagine some mechanism continuously shifting both the demand and supply
curves, and in such a case the level of prices and of output, although still located at the intersection of the demand and supply schedules, would actually be determined by the mechanism causing the transposition of the two curves. The old instrument would therefore prove to be completely inadequate to represent the dynamic interaction of the individuals’ motives and their effects.

3. Statics versus Dynamics

In *The Trade cycle* Harrod did not formulate an explicit definition of either statics or dynamics. In order to understand what he meant when using these terms it is necessary to examine in the first place how he opposed the characteristics, the tasks and the methods of the two disciplines, and in the second place how he related statics and dynamics. This Section is therefore dedicated to the study of the direct comparison between various aspects of the static and dynamic approaches. This will be complemented in the next Section by a discussion of how Harrod’s statics and dynamics were related to each other.

The argument of the preceding Section pointed to an initial opposition, lying in the epistemic domain, between statics and dynamics. Dynamics is capable of giving an account of change, and it was actually devised with that specific purpose. Statics must account instead for the possibility of change, compatibly with the fundamental conditions. This distinction lies at the core of Harrod’s notion of dynamics, to which it provides the foundation. The instability of equilibrium is its corollary: in order to allow different levels of output, static equilibrium must not be stable, while dynamic equilibrium must be unstable to allow fluctuations rather than growth at a constant rate only. With regard to the logical structure of the *Trade Cycle*, the instability principle was therefore an epistemic premise to trade cycle theory, rather than an analytical result of Harrod’s dynamics (this conclusion will be discussed in more detail and extended to the later developments of Harrod’s dynamics in Chapter VIII, § 6). On the contrary, the analytical apparatus had to be conceived as capable of giving rise to instability. With regard to statics, an antidote to stability had to be superimposed onto the traditional interpretation of the static relations, as is certified by the otherwise curious idea that since fluctuations in prices and output de facto occur and are correlated, the price level must act as a de-stabiliser and compensate the stabilising effect of the other determinants. In relation to dynamics, the mechanism carrying the seeds of instability is the Relation, for it translates changes in the first order differentials into variations in the same direction of absolute magnitudes, which in turn cause a further change in the differentials; the multiplier, on the contrary, is only responsible for a finite amplification of the acceleration effect.

Harrod’s aversion to time-lags, which explicitly emerged from *The Trade Cycle* at every turn and also characterised his later contributions on dynamics, also finds its root in his epistemic reflection. Harrod, of course, did not negate the existence of time-lags, nor
their theoretical relevance on certain occasions. The target of his contention was rather the attempt to “give plausible explanations [of the trade cycle] on the basis of a time-lag hypothesis” (Harrod 1936a, p. 88, my italics). Harrod was certainly referring to Robertson’s analysis in terms of the inequality of saving and investment based on the definition of saving as the unconsumed part of the income of the preceding period, but possibly also to the econometricians’ notion of dynamics as a theory explaining “how one situation grows out of the foregoing” and in which “magnitudes of certain variables in different points of time” are considered. Harrod recognised in this approach the same sort of fallacy that Galileo found in the Aristotelian claim that air is the medium necessary to sustain motion, which he substituted with the opposite conception that it is an obstacle to movement. Like errors of judgement and miscalculations, time-lags are a disturbance to the working of the static and dynamic forces operating in the system. Harrod therefore treated their effect as secondary, in contrast to the approaches that placed them at the heart of the explanation of the cycle. Of course, by assuming the appropriate behaviour of the lagged variables it would be possible to devise some plausible representation of the observed facts; but

the hypothesis 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 (ibid., p. 88).

Harrod therefore associated time-lags, errors and misjudgements on the same epistemic domain, as analogous attempts to overcome the stability of the system; he therefore addressed Robertson with the same accusation he had raised, years before, to Pigou’s psychological theory (see Chapter I, § 3):

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 Dec., 1936).

In his book, Harrod coupled an analytical argument to these epistemic considerations. In his procedure, a discussion of the possibility and the conditions of a moving equilibrium had to be preliminary to the analysis of the cycle. But in conditions of steady advance, time-lags have “no significance”. With reference to Robertson’s lag between income and expenditure, Harrod maintained that

indeed, it is doubtful whether a lag is an appropriate concept in this connexion. 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 n\textsuperscript{th} day with those received on the m\textsuperscript{th}; 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 1936a, p. 129).

Both orders of argument led Harrod to consider his own approach as providing a more fundamental formulation of a dynamic theory:
The 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 1936a, p. viii, emphasis added).

Other distinctions followed on from the fundamental differences of statics and dynamics in their capacity to explain change. The more direct filiation obviously regarded the object of the two disciplines, statics being concerned with the *level* of output and dynamics with its *growth*. Since it was generally recognised that the ultimate cause of growth was to be found in saving (although the mechanisms postulated as to the precise relationship between saving and growth were quite different), it is not surprising that Harrod excluded the analysis of saving and investment from the domain of statics, and confined it to dynamics:

the supposition of saving is inconsistent with the pre-requisites of a static analysis, for, if any net saving is occurring, the quantity of capital and the income-earning capacity of the community must be growing, and the factor of growth does not appear among the static assumptions (ibid., p. viii).26

This ontological distinction called for its analytical counterpart, regarding the choice between known and unknown variables, and the determination of the equilibrium behaviour of the system. I have already mentioned that Harrod’s project was that of devising an analysis of the factor of growth based on the same procedure followed by statics, which assured pride of place to the equilibrium position. In both cases, certain fundamental conditions -the static and dynamic determinants, respectively- were taken as given, and the corresponding state of equilibrium was characterised. In the case of statics, equilibrium corresponded to a state of rest, i.e. of a constant flow of goods per unit of time such that “no party to the exchange feels disposed to alter his conduct” (ibid., p. 159); in other words, in static conditions the system remains equal to itself, simply reproducing the conditions of its own existence.

In the case of dynamics, the requisite for the system to be able to reproduce its own conditions on an enlarged scale was found to consist in a constant rate of growth. On the one hand, this result was a *premise* of Harrod’s reasoning even before being an analytical *result*. The reader will remember (see Chapter III § 1) that already in 1934 Harrod knew by intuition that a regular advance would imply the preservation of the existing proportions between the different magnitudes of the system: his essay on “The Expansion of Credit in an Advancing Community” was in fact meant to be an enquiry into the relation between the rates of increase in a regularly advancing society, with a view to determining what kind of system would allow the full potentialities of progress to be realised while being internally self-consistent (Harrod 1934a, p. 287; see also p. 296, and Harrod 1934d, p. 478).

The growth mechanism resulting from the interaction of the multiplier and the accelerator later provided analytical support to Harrod’s earlier belief. In fact, the accelerator relates an absolute magnitude to the increase in the value of another variable, while the
multiplier transmits local variations to the global scale of the system, so that the
determination of the mode of advance requires the comparison of the current increase
with the past configuration of the system:

This is a matter to which I gave very long thought and reached my conclusions
after much trial and error […].

Why did I assume a geometric increase? […] I was on the lookout for a
steady rate of advance, in which the rates of increase would be mutually
consistent. An arithmetic increase of income won’t do, because then, with
inventions neutral, no increase of saving at all is required, only the same amount
each year. But that is inconsistent, on your psychological principle, with any
increase of income at all (Harrod to Keynes, 6 April 1937, in Keynes
CW XIV, pp. 163-164).

Harrod’s solution, however, is vitiated by the peculiar notion of expectations it
implies. Let us consider again the scheme given in § 2 above illustrating the interaction of
the multiplier and the accelerator. According to Harrod, the moving equilibrium “is the
rate of growth which, if maintained, will leave the parties content to continue behaving in
a way consistent with it” (Harrod 1936a, p. 150). This implies, in the first place, that the
multiplier-accelerator process will generate an increase in consumption exactly equal to
what the entrepreneurs expected when deciding the amount of investment goods to order.
In the second place it implies that the ‘justification’ of past anticipations will induce
entrepreneurs to expect consumption to increase in the next period at the same rate as the
present one. The first assumption regards the realisation of equilibrium period after
period, while the second assumption enables one to link the subsequent equilibria in a
chain. But while the satisfaction of the first condition entirely depends on the
configuration of the dynamic determinants, Harrod did not provide reasons why
entrepreneurs should behave consistently with the prosecution of advance29. Therefore,
Harrod’s analytical set-up assures the theoretical existence of an equilibrium rate of
advance compatible with the expectations, but the configuration of this equilibrium
depends on the nature of expectations themselves.

In an isolated passage, Harrod himself posed the question of expectations in the
correct way: some

writers have sought to make economics more dynamic by taking more fully
into account the effects of anticipation on the static equilibrium. But even this
approach does not give us what we want. The question is not simply -what
effects will certain anticipations have on the present situation? Rather it is -what
anticipations will have such results in the future as to justify those anticipations?
And in answering this we have to take account of the fact that the world, in
which the justification of those anticipations will be tested, will by reason of
their having been entertained be different from the present world (Harrod
1936a, p. 167)

But Harrod’s conclusion was based on the above-mentioned tacit assumption regarding
the formation of expectations, so that he failed to consider that other equilibrium paths
were conceivable, so far as systematic or even erratic changes in the value of the
determinants were offset by appropriate systematic changes in the expectations. In such
cases, depressions, cumulative growths and also irregular jumps could be ‘equilibrium’
processes if expectations systematically anticipated the actual results, in the sense that investment would always appear justified in Harrod’s sense. It is scarcely surprising then that Keynes readily spotted and qualified such a peculiar use of the notion of ‘expectation’:

it has dropped out in your treatment, I feel, that, given the rate of interest, $y$ [the rate of growth of capital] is a function of the widely fluctuating state of expectation (for which you substitute ‘the expectation of a steady growth of consumption’, which does not hold in the short period) (Keynes to Harrod, 20 April, 1937, in Keynes CW XIV, p. 178).

Curiously enough, when Harrod discussed what would happen if entrepreneurs correctly foresaw the decline decreed by a change in the dynamic determinants, he vaguely realised that the recession could appear justified to businessmen:

Might not the turn of events on the crucial given day have been anticipated, it will be asked, and the decline of net investment have begun not on that day but on the day on which the orders maturing on that day were placed? It might. [... But] if the decline of net investment had begun in anticipation of the turn of events on the given day, say six months earlier, disappointment, too, would have come six months earlier. [...] This being so, any interruption in the steady advance of investment orders will be immediately justified (emphasis added) by the results, and it will always be justified too soon, viz. when previous orders are still maturing (Harrod 1936a, pp. 95-96).

Of course such an ‘equilibrium’ would appear quite unnatural in spite of the ‘justification’ of the entrepreneurs’ decisions, and this is probably the reason why Harrod did not recognise it as such. He drew another conclusion instead: since correct expectations would only anticipate a turn of events prescribed by other factors without altering the qualitative character of their occurrence, expectations need not play a role in the explanation of the cycle:

Whether entrepreneurs wait for the actual day on which the evil effect of their [the dynamic determinants’] working is felt before reducing orders, or whether they anticipate that day by the full gestation period of orders, and so precipitate the end of the boom, is a minor matter (ibid., p. 96).

Harrod came back on the epistemic role of expectations in replying in similar terms to a letter from Hans Richter-Altschäffer, who proposed to distinguish between statics and dynamics as being concerned with ‘objective economic magnitudes’ and ‘entrepreneurial expectations’ respectively (Richter-Altschäffer to Harrod, 4 Feb., 1937):

I notice that you wish -and in this you are not alone- to draw the line at a somewhat different place between statics and dynamics, placing all factors depending on expectation on the side of dynamics. Now I fully recognize the difficulties for static theory of the existence of uncertainty. I fear that that same uncertainty will present great difficulties to the dynamic theory also.

May I put my point in this way. Suppose there were no uncertainty at all about what is going to happen: there would still on my view be room for a separate department of dynamic theory, this being concerned with the rate of growth of the various magnitudes in the system and the mutual consistency of these rates. Growth of the productive power of other factors entails demand for new capital equipment; growth of income entails growth of saving. What rates of growth are consistent with the equality of saving and capital accretion. [...] Now all this is quite independent of the existence of uncertainty. It appears to me that
it may be possible to divide uncertainty factors into two classes, viz. uncertainty with regard to matters which affect the static equilibrium, e.g. changes of consumers’ tastes affecting a redistribution of the expenditure of an income regarded as fixed, and uncertainty with regard to dynamic matters, e.g. what the rates of growth are destined to be and how increments of income will be treated (Harrod to Richter-Altschäffer, 1 March, 1937).

Harrod’s confusion regarding expectations and equilibrium seems to be due to the fact that two different notions of equilibrium cohabited in Harrod’s thought: on the one hand the individualistic equilibrium characterised by the maximisation of profits and the satisfaction of consumers’ desires, and on the other hand the capacity of the economic system to reproduce its own status and its own conditions from period to period. Given that statics refers to individual motives, static equilibrium belongs to the first class. Dynamic equilibrium belongs to the second class, because Harrod’s dynamics refers to the system as a whole. Harrod, however, seemed to think that his static equilibrium also assured the reproduction of the stationary state, while dynamic equilibrium warranted the satisfaction of the individuals regarding the actual rate of growth. Statics was in fact seen as determining which “flow of goods per unit of time through the exchange process is such that, given tastes, &c., no party to the exchange feels disposed to alter his conduct” (Harrod 1936a, pp. 149-150): in equilibrium, in fact, the price and amount of each commodity are consistent with the given circumstances (ibid., p. 166). Dynamic equilibrium, conversely, was meant to justify to the eyes of entrepreneurs the current rate of advance by the experience of the given day (cf. e.g. ibid., p. 90) and thus to induce them to be insistent in their decisions regarding that rate of advance.

This pair of complementary suppositions certifies Harrod’s pre-analytical belief in the existence of some sort of mechanism harmonising the independent individual decisions with the economic results of their interactions. His analytical apparatus was not, however, appropriate to incorporate this conviction. In the case of statics, there was no variable that referred to the whole of the economic system, while dynamics incorporated no reference to individual motives. These had to be added afterwards (in a logical sense), in the form of the expectations linking one period to the other. Such implementation was bound to be inconsistent with the bones of Harrod’s dynamic analysis, unless some mechanism was postulated explaining the formation of subjective anticipations on the basis of the objective result of the global accumulation process. This would have required a different dynamic model and another analytical set-up.

A further analytical distinction between statics and dynamics, which apparently renders the Trade Cycle an oddball with respect to the development of Harrod’s dynamic thought, was occasioned by Harrod’s recognition -notwithstanding his horror of time-lags- 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.
Who places such an order gives a hostage to fortune; his judgement can only be vindicated after the interval has elapsed (Harrod 1936a, p. 88).

Harrod therefore opposed dynamics - concerned with “a movement over a period of time” - to statics - dealing “with different possible levels of output at a particular point of time” - in the dynamic domain, “we are living in a more spacious world” (ibid., p. 83). This characterisation seems to conflict with Harrod’s 1934 claim that the appropriate method for studying equilibrium growth is to examine an instantaneous section of the system and with his later opinion that dynamics must be concerned with rates of advance at a point in time; the matter therefore needs examining more closely.

The analysis of the Trade Cycle was concerned with the succession of ‘daily’ events, but before qualifying it as ‘period analysis’ it is necessary on the one hand to specify its features and on the other to understand when reference to the succession of periods becomes analytically relevant. As to the first problem, it is important to notice that the ‘day’ was not meant to reflect the ‘gestation period’ of capital goods, but was rather designed to allow the expenditure of money on investment goods to turn into actual net investment. In fact, on the one hand Harrod introduced the “highly unrealistic [assumption], the utility and shortcomings of which are so patent as to render [it] innocuous” that during the whole time necessary for the construction and delivery of machines investment flows at a uniform rate (Harrod 1936a, p. 88). On the other hand, he specified that

in reality the level of activity related to a given amount of investment is not accurately determined in any space of time so short as a calendar day owing to the unforeseen variation in the level of stocks. But it will be appreciated that the period of time for which this analysis is really designed is longer than a day (ibid., p. 89).

Harrod’s mention of the variations in stocks explicitly referred to his previous discussion of the multiplier. If it is true that at any time saving equals investment, it must be kept in mind that in the very short period the equality is maintained via changes in the level of stocks, which involve dis-investment. Only in a second phase, requiring a “slightly longer period”, will this “lead to action intended to rectify matters. If stocks have to be replenished there is a consequent rise in the level of activity and income, and conversely” (Harrod 1936a, p. 73-74). In other words, the expenditure for investment is met at first with existing stocks, which are only replenished at a later stage. Therefore the relevant period in Harrod’s analysis depends on the time necessary for entrepreneurs to react to the depletion of their stocks; when the new production actually begins, net investment becomes positive and the multiplier instantaneously determines the level of income that maintains the equality between saving and investment.

As to the analytical relevance of the division of time into ‘days’, it is necessary to note that the consequences of any change in the dynamic determinants unfold within the same day. For instance,
If people saved a larger proportion of their increment of income or there were a shift to profit on the given day, the value of the multiplier would fall below its previous level, and, so far as these two determinants were concerned, consumption would advance less than the capital goods increased on the given day (Harrod 1936a, p. 91, italics mine).

Now, if steady growth is under advance it is obvious that the division into periods is analytically redundant, for nothing occurs from the point of view of the balance of the dynamic forces and the configuration of the system does not change from day to day but by its size. If instead the restrictive force of some determinants should prevail on balance, in the course of the same day something happens that makes the system different from the day before and induces the entrepreneurs to adapt their decisions. In the case of recession, on the contrary, it clearly makes sense to study the daily events in their succession. In \textit{The Trade Cycle} Harrod did not explicitly associate equilibrium with instantaneous analysis and disequilibrium with the succession of events. However, he later discussed this distinction in a methodological essay (Harrod 1938a) and explicitly applied it in the “Essay” (see Chapter VI § 3 below). The impression that \textit{The Trade Cycle} is at odds with Harrod’s previous and successive concern with instantaneous analysis must therefore be rejected, for in all instances equilibrium is entirely characterised by its own internal consistency at any instant in time. To be precise, then, the \textit{Trade Cycle}’s distinction between instantaneous and period analysis does not regard statics and the whole of dynamics, but only the analysis of the cumulative deviations from equilibrium growth -which, however, Harrod at this point seemed to treat as a proper component of dynamics, while later he emphasised on the contrary that trade cycle analysis lies outside the study of ‘pure’ dynamic theory.

The absence of an explicit demarcation line between equilibrium analysis (to be led in \textit{instantaneous} terms) and disequilibrium \textit{processes}, together with Harrod’s implicit assumptions regarding the formation of expectation in equilibrium, indicate that in \textit{The Trade Cycle} Harrod thought of equilibrium as a growth \textit{path} rather than as a reference \textit{point} as he did in the “Essay”. The proper discussion of this aspect, which will prove illuminating for the interpretation of the “Essay”, would require a comparison with the approach of the latter work, and it is therefore expedient to postpone it to § 9 of this Chapter and to Chapter VII § 6).

In the methodological domain, in \textit{The Trade Cycle} and later in correspondence with Keynes, Harrod returned to a distinction between statics and dynamics he had already sketched in 1934 (see Chapter III § 2) and repeated in 1935 (see Chapter IV § 3). Static analysis adopted the following procedure:

It has been usual to suppose a certain given state of technology and a certain given set of desires on the part of consumers, and to ascertain what is the equilibrium price and the equilibrium amount of output of each commodity consistent with these circumstances. If a change in these circumstances occurs, then a new equilibrium will \textit{in due course} be reached consistent with the new situation (Harrod 1936a, p. 166, italics mine; see also p. viii).
The method of statics thus allows change to occur, but only as a consequence of a change in the fundamental circumstances. If such a change occurs, the only thing statics can say relates to the new equilibrium position. What occurs in between (e.g., oscillations around the new equilibrium position) might be interesting, but it is a matter that lies outside the method of demand and supply curves. In contrast, the fact that saving and investment imply growth suggests that a qualitatively different question should be asked:

It is no longer appropriate to ask -as in the case of a particular commodity- what amount of saving will be justified on the assumption that the surrounding circumstances remain the same within the period in which the equilibrium is established. For the saving itself entails a change of no little importance in the surrounding circumstances, viz. a growth of productive power. The question has to be asked -what amount of saving will prove justified, taking into account the factor of growth which the saving necessarily entails? (ibid., pp. 166-167)

In other words, in dynamic considerations one cannot forget that saving (or investment) and growth -each other’s cause and consequence- occur at the same time, and the distinction between equilibrium states and ‘what happens in between’ is therefore not legitimate.

On this ground, Harrod criticised Keynes for having discussed, in his critical notes on *The Trade Cycle*, the relationship between the rates of increase of investment, capital and consumption in terms of discrete changes, while Harrod’s technique of analysis presupposed continuous variations of the relevant magnitudes:

The fact is that you in your criticism are still thinking of once over changes and that is what I regard as a static problem. My technique relates to steady growth (Harrod to Keynes, 6 Apr., 1937, in Keynes CW XIV, p. 163; see also Harrod 1937b, p. 330).

Despite of the lack in *The Trade Cycle* of a precise definition of the notions of ‘statics’ and ‘dynamics’, it has been possible, by considering how Harrod opposed the epistemic, methodological and analytical features of his newly coined concepts, to gather a few elements which provide an outline of Harrod’s notion of dynamics in 1936. He had identified growth as the object of the theory, specified the fundamental mechanisms engendering economic progress, characterised the peculiarity of an equilibrium mode of advance, understood the analytical ground for the methodological requirements regarding continuity and the definition of the domains where to apply instantaneous and process analysis, and posed the principle of instability as the foundation of the possibility of explaining growth and the cycle. It is still necessary, however, to understand how the new discipline of economic dynamics was related to statics, and how the latter was related to orthodox theory. The first question will be tackled in the next Section, while the discussion of this last problem will be deferred to Chapter VIII.

4. The Link between Static and Dynamic Laws

Harrod’s opinion as to the links between statics and dynamics emerges with clarity from a paper on “Mr. Keynes and Traditional Theory” presented in September
1936 at the Oxford Meeting of the Econometric Society. There he maintained that the classical theory’s incapacity of dealing with investment and growth called for a “re-ordering of concepts”\textsuperscript{40}, and he submitted that Keynes’s \textit{General Theory} had contributed to a great extent to the necessary re-organisation. In Harrod’s view, however, Keynes did not go far enough, and failed to affect “a revolution in fundamental economic theory” but only brought about “a re-adjustment and a shift of emphasis” (Harrod 1937a, p. 85). In fact Harrod held that Keynes had rearranged some of the components of traditional theory, giving them a new logical ordering without introducing new pieces: in the \textit{General Theory} “the old pieces in the traditional theory reappear, but sometimes in new places” (ibid., p. 85). Keynes’s contribution “has been to consider certain features of traditional theory which were unsatisfactory, because the problems involved tended to be slurred over, and to reconstruct that theory in a way which resolves the problems” (ibid., p. 84).

Harrod summarised as follows Keynes’s restructuring of the orthodox components. The first piece was the marginal efficiency of capital which, according to Harrod, was nothing else than a new name for the old marginal productivity of capital: “it does not appear that there is a difference of principle here”. Although “the stress which [Keynes] lays on expectations is sound, and constitutes a great improvement in the definition of marginal productivity”, the state of confidence also “might be incorporated in traditional theory without entailing important modifications in its other parts” (Harrod 1937a, pp. 76-77)\textsuperscript{41}. The second piece was the multiplier: “those to whom the doctrine of the multiplier seems an alien morsel in the corpus of economic doctrine should remember that it is merely a disguised form of the ordinary supply schedule of free capital, but with the level of income treated as a variable” (ibid., pp. 77-78.). If there was a difference here, according to Harrod it laid in the shift of emphasis from the rate of interest in favour of income as a determinant of the amount of savings. Third, the liquidity preference theory: this “is not really a new piece. The old theory pre-supposed that income velocity of circulation was somehow determined. But precisely how was something of a mystery. Thus the old theory assumed that there was a piece there but did not state exactly what it was. Mr. Keynes’ innovation may thus be regarded as a precise definition of the old piece” (ibid., p. 85).

The \textit{Trade Cycle} was based on a similar rearrangement of elements, but with the additional distinction between “the theory of value and distribution in a not-growing community and the laws of growth” (Harrod to Robertson, 25 Dec., 1936). To the former domain belong the traditional laws of cost and utility, to the second belong the factors determining saving and investment, that is the multiplier and the accelerator (which had to substitute the Keynesian marginal efficiency of capital, due to the fact that the latter, according to Harrod, is calculated on the basis of a \textit{given level of output} while the dynamic problem presupposes a \textit{continuously changing output}). According to
Harrod, this distinction constituted a major break with traditional analysis, while Keynes’s emphasis stressed the wrong point:

I dont quite like Maynard’s distinction between his general theory and the classical theory as covering the special case of full employment: the classical theory provides the mechanism for full employment or is prepared to specify why the mechanism doesn’t work (wages too high) Whereas I think the classical theory breaks down in not analysing growth; it is when you come to that that you see that there cant be full employment, however high or low wages, in certain circumstances (Harrod to Robertson, 25 Dec., 1936)

Hence Harrod’s judgement that, even if Keynes had “thrown out a big hint” for the construction of a dynamic theory (Harrod to Robertson, 25 Dec., 1936), his “system is still static”, and is therefore far from the wanted “mental revolution” (to borrow Harrod’s own characterisation of dynamics formulated a few years later):

Note has been taken of the fact that at certain important points, e.g. in his definition of the marginal efficiency of capital, Mr. Keynes lays great stress on the importance of anticipations in determining the present equilibrium.

But reference to anticipation is not enough to make a theory dynamic. For it is still a static equilibrium which the anticipations along with other circumstances serve to determine; we are still seeking to ascertain what amounts of the various commodities and factors of production will be exchanged or used and what prices will obtain, so long as the conditions, including anticipations, remain the same. But in the dynamic theory, as I envisage it, one of the determinands will be the rate of growth of these amounts (Harrod 1937a, pp. 85-86).

Such a sharp division between ‘the laws of value and distribution of income’ and ‘the laws of growth’ suggests the problem of studying the relations between these two sets of laws. I have already remarked that static laws concern individuals, while dynamics concerns the economic system as a whole (§ 3 above). This question turns therefore into the problem of understanding how changes in magnitudes such as aggregate saving and investment, income etc., determined on the global scale but resulting from the interaction of a multitude of individual and independent resolutions, may be consistent with the conditions leading to these decisions. In other words, at this point Harrod was facing the analytical counterpart of the epistemic problem of the possibility of change of the level of output. It is therefore not surprising that the solution was found in the same locus, that is in the balancing of forces giving rise to the neutrality of static equilibrium (see § 2 above). For the solution to the question of the possibility in principle of variations in output, Harrod had postulated that the static equilibrium is neutral, that is that the diverging tendencies stimulating or inhibiting an expansion of output were mutually compensated over the possible range of output fluctuation. The dynamic determinants are thus, in Harrod’s model, entirely responsible for movement: their action finds no obstacle nor additional support in the resultant of the forces determining the level of output, but only passive compliance. The solution of the problem of how the dynamic forces impose their consequences on the static forces required an additional step. It was still necessary, in fact, to explain why individuals are compelled to behave as the dynamic determinants decree for the whole economic system.
Harrod proceeded by dividing the static determinants into two groups, one being affected by the change of output and the other independent of it, so that, given the state of the latter group, in order to maintain the static equilibrium in a state of neutral stability the dynamic forces must induce the necessary changes in the other. As already mentioned, Harrod considered as fundamental the conditions relating to the state of tastes and costs, and refused to attribute the cause of output fluctuation to rhythmic changes in individual preferences (§ 2 above). Consequently, the task of assuring the consistency between the laws of growth and the laws of value and distribution was attributed to changes in the remaining determinant, money - which, the reader will remember (§ 1 above), operates with a de-stabilising effect:

The ultimate cause of the cycle is the peculiar relation of the creation of new capital to saving. The price fluctuation is the mechanism by which this ultimate cause operates upon individual units to induce them to carry out the variations in output required. [...] The price fluctuation represents what remains in our complex system of man's natural determination to continue earning his livelihood, even when conditions become less favourable, and of his unwillingness on the other hand to be rushed into overwork. An analysis of how these natural impulses still operate in our modern society was therefore highly relevant (Harrod 1936a, p. 171; see also p. 179).

For this reason, the alternative explanation, which recognised price fluctuations as the cause of the cycle was, in Harrod's view, fundamentally wrong:

The rise and decline of activity and its present level are determined by dynamic forces. But all the while the static forces remain. The phenomena of price- and profit-fluctuation represent the resistance of the static to the dynamic forces. The prevailing error in cycle analysis heretofore has been the mistaking these phenomena of resistance for the true cause of the cycle (ibid., p. 172).

The reader may wonder why prices should fluctuate in such an accommodating way. The velocity of circulation of money comes into play here, as it was indicated by Harrod's interpretation of Keynes's theory of liquidity preference as a precise definition of the unsolved problem of the determination of the velocity of circulation, in opposition to the old quantity theory which presupposed it as "somehow determined". Although Harrod did not adhere to Keynes's liquidity preference theory, he recognised in the first place the necessity of getting rid of the old analysis of interest, and in the second place the need for a theory of the velocity of circulation. He therefore accepted the hint he read in Keynes's work to re-organise the analysis of this particular piece of classical theory. Harrod turned the conclusion of the Quantity Theorists that price fluctuations are a consequence of changes in the quantity of money upside down. He formulated instead the view that it is rather the play of the dynamic and static forces which jointly determine the fluctuations in the quantity of money (which is determined by banking policy) multiplied by its velocity of circulation.

Before turning to the specific mechanism permitting the fluctuations of the velocity of circulation, it is necessary to notice that this inversion of the causal relationship between the left and right hand sides of the quantity equation constituted the
last step in Harrod’s first attempt to systematically reorganise economic theory. His static and dynamic laws, their difference in method, scope and content, and also the mechanisms connecting them and posing statics at the foundation of dynamics, must be interpreted in this sense\textsuperscript{45}. In this attempt we can see the implicit formulation of a project tending towards a reformulation of the science of economics along the same line.

5. Money and prices

In the preceding Section I have mentioned that price changes are the force that impose on individual entrepreneurs, given the other static determinants, the variations in production decreed by the interplay of multiplier and accelerator. I have also hinted that, according to Harrod, such accommodating behaviour of the price level is permitted by the appropriate fluctuations in the velocity of circulation of money. The specific mechanism accounting for these fluctuations, however, was left unexplained, and it is the purpose of this Section to fill the gap. In order to understand Harrod’s solution in the context of his view on the link between statics and dynamics, it may be useful to summarise the logical structure of his book by means of a diagram representing the determination nexuses constituting the causal, explanatory scheme of *The Trade Cycle*:

\[
P \cdot Y = M \cdot V
\]

It is appropriate to begin our reflection by observing the peculiar role of the price level in the above scheme. The general price level was meant to be a static determinant, while in fact it is determined by the set of dynamic forces, given the value of the other static determinants. In a sense the price level belongs to both the static and dynamic domains: as a static force, it contributes to determine the individual level of output; but given the output of the preceding period, the general level of output of the present period is entirely determined by the interaction of the multiplier and the accelerator, according to the resultant of the dynamic forces. Thus, due to its role of connecting statics and dynamics, the price level fulfils the task of causing the individual entrepreneurs to adapt to the global rate of growth compatible with the state of the dynamic determinants, and its magnitude is determined by the configuration of the latter, given the new state of preferences and costs. In Harrod’s analytical set-up, therefore, the price level is entirely
determined by real forces, and is independent of the decisions of the banking system regarding the volume of money. Harrod therefore pointed out that “the set of ideas to which the doctrines of this essay are most repugnant are those connected with the Quantity Theory of Money” (Harrod 1936a, p. 125). While on the basis of quantity theory it was maintained that real forces determine the relative prices and, at the given volume of transactions and velocity of circulation of money $V$, the general price level is dependent on the quantity of money $M$, Harrod argued that the set of static and dynamic forces determine the effective circulating medium $MV^{46}$.

In spite of the firm rejection of the quantity theory, Harrod’s attitude towards money was somewhat ambiguous. On the one hand, Harrod -following Keynes, to a certain extent- implicitly referred to the functions of money other than that of facilitating exchanges, which are responsible for the failure of the orthodox mechanism of equilibration that would make crisis and unemployment impossible. On the other hand, the working of Harrod’s dynamic mechanism does not imply any reference to money. Real and monetary analysis were kept separate$^{47}$: dynamics referred to quantities rather than to values, while changes in monetary magnitudes were only called on to provide a link between statics and dynamics. In dynamics the fundamental role Harrod attributed to money was that of behaving “as a lubricant to the system” (ibid., p. 126), and in this he was consistent with the neoclassical tradition that considered money primarily as a means of exchange. Therefore, against the views (e.g., Hayek’s$^{48}$) that “money is the original and actuating cause of the cycle” (ibid., p. 46), Harrod countered the idea that money is a passive accomplice in the generation of the cycle, moving up or down, within certain limits, to suit those forces […] which determine the cyclical movement. […] Its part is utterly subordinate -it does what it is told, puts up no resistance to the worst happening, has no will of its own, is a useful and abject tool (ibid., pp. 46-47).

A second feature characterising Harrod’s views of money and of static and dynamic forces, is that he maintained that the monetary sector scarcely influences the real working of the multiplier-accelerator mechanism. As the diagrammatic representation of The Trade Cycle illustrates, the only link between the two sectors is provided by the influence of the rate of interest on the third dynamic determinant. This is not direct but quite roundabout, for investment primarily depends on the prospective increase in the demand for consumption goods (the Relation), while the rate of interest only affects -jointly with the state of technology- the acceleration coefficient$^{49}$. Robertson therefore rightly pointed out that the main thesis of this closely-reasoned and stimulating book is that the causes of the trade cycle are deeply seated in certain ‘real’ features of the modern economy, the part played by money being that of a passive accomplice rather than an active ringleader, and the prospect of control by monetary policy less favourable, therefore, than is sometimes supposed$^{50}$ (Robertson 1937a, p. 124).
Robertson’s insight was correct in the strongest sense, for Harrod’s multiplier-accelerator mechanism was originally postulated to work without reference to values, but only to physical quantities. Harrod accepted and incorporated the Keynesian multiplier as connecting output to investment, via changes in the level of income: in Harrod’s interpretation, in fact, Keynes had propounded

the view that the general level of economic activity is determined by the amount of investment taking place, in such wise that, given the community’s propensity to save, the activity must be just so great as to give people an income from which they will choose to save the amount that is required for that investment. The ratio of the increment of income (\(=\) the increment of output) required to make people save an amount equal to the increment of investment is called the Multiplier (Harrod 1936a, p. 70; italics mine).

As to the Relation, Harrod’s own wording was unequivocal: net investment was conceived as the amount of additional capital goods necessary in order to increase the output of consumable goods (Harrod 1936a, p. 55 and passim).

The possibility of concentrating on real magnitudes seems to have depended upon Harrod’s belief that he could bypass the study of changes in the system of relative prices to concentrate on the general movement of prices and output. From the very beginning of his approach in terms of successive approximations (see § 1 above), he constrained his argument to the case of uniform advances and recessions, defined as the changes in the level of output that leave unaltered the rates of exchange between different goods (ibid., pp. 14-15). The otherwise controversial notion of a general level of prices, on which the de-stabilising power of the monetary determinant was based, found its justification in the assumption of uniformity\(^51\) (ibid., p. 36). Of course Harrod admitted that “the trade cycle does not constitute an alternation of uniform advances and recessions” and that “the deviations from uniformity are large and notable” (ibid., p. 42), but in view of the generality of advance in almost all sectors he regarded the real movement “as compounded of that amount of uniform movement and the deviation from it”:

If the fluctuations of any real system were examined from this point of view there is little doubt that they would be found, so far as the uniform factor is concerned, to consist of alternating increases and decreases in the alternating phases of the trade cycle (ibid., p. 43. See also Harrod 1936b, p. 85).

On this ground, Harrod ignored the deviations and kept reasoning, in the dynamic as well as in the static compartments of his book, in terms of uniform advances\(^52\). Only as an afterthought stimulated by a comment by James Meade (Meade to Harrod, “Note 1” attached to a letter of 12 Jan., 1936), Harrod inserted “one or two sentences” in his book mentioning changes in the relative prices of capital goods compared with those of consumable goods (Harrod to Meade, 7 Feb., 1937). Keynes as well, in his proposed March 1937 “Lecture Notes” on *The Trade Cycle*, stressed that the Relation could not be treated as immutable in the course of the cycle, because of the changes in the relative prices of different consumption goods and of capital goods (Keynes CW XIV, pp. 157-158). Harrod protested that he had not assumed the Relation to be constant\(^53\), and quoted
as evidence for his claim the above-mentioned passage inserted on Meade’s suggestion (Harrod to Keynes, 7 April, 1937, in Keynes CW XIV, p. 165).

This exchange reveals a further aspect of the peculiarity of Harrod’s interpretation of Keynes’s notion of investment decisions. As I have noted in § 4 above, Harrod treated the concept of marginal 

**efficiency** of capital as equivalent to the traditional notion of marginal 

**productivity** of capital. Such an interpretation reduced Keynes’s emphasis on expectations to a component to be added to make the analysis more accurate, and failed therefore to recognise the fundamental role of uncertainty for Keynes’s approach to knowledge. This difference in the epistemology of Harrod and Keynes crystallised in the analytical contrast between Harrod’s concern for the quantities and Keynes’s attention to the monetary values involved in the investment process. In fact Keynes saw entrepreneurs as interested in investment for the production of monetary profits rather than for material goods, and the uncertainty couched in the investment decisions thus regarded possible changes of the set of prices during the interval between the decision and the full realisation of investment (see Carabelli 1988, p. 210). Harrod, on the contrary, saw investment decisions as aiming at providing the means for the expected increase in the demand for consumption goods, and thus missed the monetary dimension of the ‘realisation problem’. In view of this contrast, it is not surprising that in Harrod’s reconstruction of the logical structure of the General Theory, Keynes’s emphasis on the monetary character of capitalist economies and on the multiple functions of money were left in the background. The role of money was confined to the determination of the interest rate, and the analysis of liquidity preference was understood as solving the problem left over by traditional theory “to explain how the total stock of money is divided between liquid reserves and active circulation”, that is the determination of the velocity of circulation of money (Harrod 1937a, p. 85).

**6. Velocity of circulation as a link between statics and dynamics**

Harrod’s solution to this question, which he identified with the problem of the link between statics and dynamics (above, §. 4.), relied on the property of money as a reserve of value.

As is illustrated by the diagram at the beginning of § 5, the ultimate cause of this phenomena related to the velocity of circulation lies on the one hand, on the determinants of the price level and the volume of transactions, and on the other, on the choices of the banking system regarding the quantity of the circulating medium. In Harrod’s view, the Trade Cycle’s doctrine of trade fluctuation is itself the theory of velocity. Those forces which have been enumerated govern the volume of output and the level of prices; these in turn cause the velocity of circulation to be what it is. Or rather, they cause the quantity of money multiplied by its velocity of circulation (MV) to be what it is. And velocity is the resultant of banking policy, which determines the quantity of money, and of the forces enumerated, including any effect that
banking policy may have, for instance via the rate of interest, on the forces enumerated (Harrod 1936a, p. 126, emphasis added).

To explain how the circulation of money slows down at the beginning of the depression, Harrod resorted to Robertson’s assumption that expenditure is related to the income of the preceding period. If such a time-lag is irrelevant in the case of a steady advance, for income and consumption increase at the same rate irrespective of the lag (see § 3 above), it becomes important in the event of a transition from a phase of steady advance to a recession (or conversely). When the rate of increase of income begins to slow down, the multiplier has an abnormally high value because the proportion of saving is related to the level of income prior to the change. This at first helps to postpone the advent of the recession. During the first circulation period, people spend more than they would on the ground of their current income, and firms keep receiving their monetary receipts from sales as usual. Given that investment has declined, firms have to reduce their activity; they are therefore not able to make productive use of part of the money that they receive. The firms thus have in their hands a surplus of cash holding, which they cannot distribute as dividend because it is not profit but part of their capital:

the receipts in the consuming trades are maintained, but their outlay is reduced. This excess of receipts over outlay does not represent profit and cannot be distributed as dividends. The reduced outlay implies a reduction in the volume of physical circulating capital. The surplus money in hand must be reckoned as part of the firm’s capital. Thus the withdrawal of money from circulation, so surprising to the quantity theorists, has a natural explanation (Harrod 1936a, pp. 135-136).

The firms use part of this cash to reduce their indebtedness to the banks, thus reducing $M$. This, however, does not fully compensate the fall in $P \cdot Y$, for there is no reason to suppose that the firms come into possession of the surplus cash in the exact proportion of their indebtedness. The velocity of circulation therefore drops, unless the idle balance could exert an indirect influence on the level of activity via a reduction of the rate of interest. This counteracting factor is theoretically possible, because one could imagine that when the cash holders try to exchange their liquidity for more remunerative assets the price of the latter rises, and consequently the interest rate drops, increasing the expansive force of the third dynamic determinant and thereby stimulating activity. In Harrod’s opinion, this process is not enough to rehabilitate the quantity theory in its plenary sense, nor to stimulate the system back to its previous level of activity. In fact the rise of the price of fixed interest securities cannot offset the fall of prices elsewhere “and leave the general price level proportionate to the quantity of money”:

the notion that the general price level may be expected to vary in the same proportion as the quantity of money is only appropriate when the whole stock of money is passing along the income stream, when holders of money do not care how many units of it they hold provided that they hold in money the power to purchase so many goods, e.g. to finance their purchases for a week or a month. In these circumstances the price level will vary in the same proportion as the quantity of money; the elasticity of the demand for money will be equal to one.
When part of the stock of money is used as a capital asset, these rules no longer hold. The rate of interest which induces people to hold fixed interest securities instead of money and vice versa depends on a variety of factors. The curve showing the preference for one compared with the other as a function of the rate of interest may have an elasticity far greater than one (Harrod 1936a, p. 140).

With regard to the effect of the drop of the interest rate on investment, in Harrod’s view

Still less reason is there for supposing that the fall in the rate of interest, which is likely to occur in a slump for the foregoing reason, may be expected to restimulate the system to its previous level of activity. That would only be so on the palpably false assumption that people were absolutely unwilling to hold money as a capital asset (that is, if the elasticity of demand for it as such were equal to zero) and that they would bid the price of securities up and up until the consequential fall in the rate of interest and the consequent stimulus to trade took the idle money off again into active circulation (ibid., p. 141).

Thus, so far as money is used as a reserve of value and held as a capital asset, the reduction in the level of activity causes the required drop in the velocity of circulation, and does not allow the equilibrating mechanism postulated by traditional theory to operate: the whole process resolves into a “thorough reshuffling of idle balances” (ibid., p. 142). This is the mechanism through which velocity fluctuates, thereby enabling the price-level to fall and to overcome the resistance of the static stabilisers as a consequence of the action of the dynamic determinants (Harrod 1936a, pp. 125-142).

7. A Methodological break

The study of the particular functions that money was called to play in different parts of the Trade Cycle’s analytical set-up suggests that, with respect to money, Harrod’s theory consisted of three distinct blocks, hierarchically structured. The hard core of the whole construction was the dynamic model, with the dynamic forces directly determining the intensity of the multiplier and accelerator effects and consequently the rate of growth of output (and, given the level of output of the former period, the present level of production). A second compartment concerned the determination of the price level appropriate to the current output, given the configuration of the ‘fundamental conditions’ (costs structure, preferences etc.). The third department was the monetary sector proper, where the appropriate velocity of circulation was determined to suit the requirements of the combined action of the static and dynamic forces on output and prices (given the volume of money resulting from the decisions of the banking system).

How were these compartment connected within the structure of Harrod’s theory? On the one hand they were coupled in a determination chain: dynamics determined prices which determined velocity. Conversely, the lower links in the determination chain provided the condition of possibility for the higher links: the conditions enabling velocity of circulation to fluctuate allowed price changes, which in turn satisfied the prerequisite for output to grow or fluctuate according to the prescriptions of the dynamic forces. In the face of this double epistemic connection, there remained an analytical dichotomy between the departments. This is because Harrod did not elucidate the mechanism by
which prices change in the face of fluctuations or growth of output, nor is it clear how velocity and prices were connected so that the variations in the former enabled the fourth static force “to act as the arch-destabilizer”. The reason for this gap lies in a methodological break Harrod introduced when discussing the de-stabilising power of money:

so far in reviewing a determinant after the other we have been able to rely on our knowledge of human nature, on the known purposes of economic activity reduced to their simplest terms, of certain broad principles amounting almost to truisms, and have deduced what may be expected to happen as the level of activity varies. But in this case it is proposed to ask not what may be expected to happen to the general level of prices as output varies, but what does in fact usually happen. This is a complete change in the method of procedure, and it is important to emphasize very strongly that it is a change (Harrod 1936a, p. 37).

In Harrod’s view, the fact “that prices rise when goods are turned out in greater abundance and fall in the opposite situation” is “one of the very few generalizations vouchsafed by empirical observation in economics; and it is probably the best established of any” (ibid., p. 41; see also 1936b, p. 84). He thus concluded:

If it is true, the monetary determinant clearly embodies a de-stabilizing force. The restorative effect of the other stabilisers in the case of a downward departure from a given equilibrium might be entirely offset by a sufficient fall in prices, and conversely in the case of an upward departure. Consequently, if prices do tend to fall in the case of a downward departure, the fact constitutes a de-stabilizing force tending to counteract wholly or in part the forces of the stabilisers; conversely, again, if rising prices accompany increasing activity (Harrod 1936a, pp. 41-42).

On the ground of the argument that static stabilising and de-stabilising forces must balance each other (see § 2 above), Harrod solved the equation $\sum \text{Forces} = 0$ in terms of one of the forces involved, $\text{Force}_i = -\sum_{j \neq i} \text{Forces}_j$, and concluded that the movement of prices is a measure of the strength of the other three stabilising forces (ibid., p. 43). As a consequence, he attributed the cause of the fluctuation of prices to the changes of the strength of the individual static stabilisers entailed by output fluctuations, consistently with his view that the monetary system is “a neutralizing medium accommodating itself to the other factors, which determine the level of output” (ibid., p. 82). To sum up: the fluctuation of prices was assumed as a premise; this observed behaviour was thought to be compatible only with a de-stabilising effect; the list of static forces being exhaustive, the epistemic necessity of neutrality of static equilibrium over the whole range of output fluctuations implied that the only de-stabilising force continuously balanced the resultant of the other determinants; finally, the variables whose behaviour had an explanation in terms of ‘the known purposes of economic activity’ were taken as a cause of the behaviour of prices. But no mechanism was suggested to explain how, for instance, a joint increase of activity and of prime costs would lead to an increase of the price level. Harrod’s argument reduced to the consideration that a rise of price has to occur, for
otherwise the increased prime costs per unit would dissuade the entrepreneurs from increasing output.

Analogously, Harrod did not provide reasons as to why the de-stabilising effect of the price-level requires a precise behaviour of the velocity of circulation, although he clearly stated that it is the variations in velocity that enable money [i.e., the general level of prices] to behave as a lubricant to the system and to allow it to follow its chosen course. In particular they enable money to act as the arch-destabilizer, overcoming the stabilizing forces of the static determinants and allowing the level of output to move up and down as the dynamic determinants prescribe (Harrod 1936a, p. 126).

The mechanism illustrated above in § 6 to explain the drop in velocity only took as its cause the decreased level of activity, while the price level was not even mentioned. In reality Harrod was interested in determining the direction of the causal relationship between the complex of variables on the left and on the right hand sides of the quantity equation, whose validity he did not challenge (see 1936a, p. 125). But he turned the traditional quantitativist interpretation upside down, for instead of seeing the quantity of money as determining the general price level he saw the volume of transactions as determining the effective quantity of circulating medium. Harrod stressed this point in an epistolary debate he had with Hawtrey concerning *Capital and Employment*. Hawtrey had interpreted Harrod’s theory as tracing “trade depression to a deficiency of demand arising from absorption of cost” (Hawtrey 1937, p. 320). Harrod objected as follows:

*I* do not trace trade depressions to an absorption of cost! That is your view. I dont deny that an absorption of cost occurs. But I claim that to be the consequence of the depression, which is itself traced to the interaction of the relation and the multiplier. […]

The causal forces seem to me to arise out of the nature of investment and saving; the consequence is a decrease in the velocity of circulation which has to be explained. My explanation is that in consequence of forces connected with real investment making for depression money is shifted over from active circulation to capital account. […] It can only get back to active circulation if something occurs to stimulate trade. What is that something? It can only be those forces already discussed in the preceding pages. It may be that the long rate will fall and provide some stimulus; but I repeat that there is no reason to suppose that it must fall sufficiently to send all the money back. This is the crucial point on which Keynes’ analysis of liquidity preference, though I agree in thinking it in many respects unsatisfactory and incomplete, seems to me a real contribution, needing further development (Harrod to Hawtrey, 4 Mar., 1937).

Hawtrey refused Harrod’s interpretation, and traced the absorption of cash back to the excess of saving over capital outlay, that is, to the undesigned accumulation of stocks which occurs during the depression (see Chapter III § 5, and Harrod 1936a, pp. 71-74).

Thus the absorption or cash by the investment market is not merely a consequence of depression but is an essential link in the causal chain whereby “the amount of saving undertaken is accommodated to the amount of net investment through changes in the level of income” (your page 74) (Hawtrey to Harrod, 24 April, 1937).
Harrod did not accept Hawtrey’s point, and in his reply explained how he saw the relationship between hoarding and absorption of cash in his own sense:

I appreciate that my account of the slump involves an absorption of cash. The question is what emphasis should be attached to that. I should only speak of an absorption of cash as a cause of trouble, if it followed from a change on balance in the desire of people to hold cash. But without any change in his desire to hold cash, a man may hold more cash because he has more cash to hold. If a shift of cash from those who desire to use it to those who desire to hold it occurs, there may be a more intense pressure to hold cash without anyone being more desirous to hold cash. I believe that in the discussion of hoarding there has been great confusion owing to a slurring of the following distinctions. 1. There is the amount actually hoarded, which is governed by the banking system. 2. There is each man’s propensity to hoard, considered as a function of his economic opportunity. 3. there is the corresponding collective propensity to hoard, which is kept in check by the rate of interest. Now without any change in 1 or 2, the rate of interest required under 3 may rise or fall. If opportunities change, without anyone’s desire for cash being in the least different, the pressure which, given a constant quantity of cash determined by the banks, has to be kept in check by the rate of interest changes.

This may happen if a firm loses business. Its attitude to cash in general may be precisely the same as before, viz. its estimate of the comparative desirability of cash and other things. But because business has gone down in period 2, it has more cash to dispose of and holds it; it would equally well have held it in period 1 if it had had the cash. It seems to me entirely misleading to call this an increased propensity to hoard. I should only speak of a change in propensity when the relative subjective valuation which people give to such things as liquidity, risk etc. changes.

Again it seems misleading to speak of the absorption of cash in such a case as a cause. The true cause must be found in the change of circumstances which causes the firm to loose business and so bring the unwanted cash into its coffers (Harrod to Hawtrey, 1 Oct., 1937).63.

Harrod thus seemed to conceive a sort of ‘forced hoarding’ doctrine, according to which the velocity of circulation must adapt to the dynamic behaviour of the economic system, whether or not people are willing to accommodate their individual propensity to hoard: “in the absence of sufficient hoarding in [Robertson’s] sense, the money automatically gets trapped into the capital account of firms” (Harrod to Robertson, 21 May, 1936).

Harrod’s remarks to Hawtrey regarding the causal order of events followed an analogous complaint against Robertson’s review of The Trade Cycle:

You suggest to the reader that I make an increase of hoarding play an important part, whereas I would contend that in any ordinary sense of hoarding there is no increase. I was very amused to find that Hawtrey in his account makes me attribute an important role to the “absorption of cash”. Of course I do mean an absorption of cash, but Hawtrey regards it as cause and I effect (Harrod to Robertson, 3 Apr., 1937).

It therefore seems that if on the one hand Harrod was anxious to stress the causal nexus linking the three sections of his theory (dynamics, prices and money), on the other one he has not been able to connect them within an integrated analytical framework. Formally, Harrod’s dynamics work as a barter system, upon which money is superimposed64. The only, tenuous link with money is given by the influence of the rate of interest on the third dynamic determinant. But Harrod treated on different grounds the effect on investment of perspective increases in demand and of changes in the third dynamic force. He conceived
of the relationship between accumulation and increase in demand as much more important than the effects of interest changes. These were only seen as correctives to the fact that the ‘pure’ operation of the acceleration principle ‘over-explained’ the cycle.

In 1933, Keynes lamented that a monetary theory of production was still needed, “in which money plays a part of its own and affects motives and decisions and is, in short, one of the operative factors in the situation” (in Keynes CW XIII, p. 408). This still applies to the first post-General Theory attempt at a dynamic theory. Harrod himself, however, recognised the existence of a dichotomy between the compartments of his theory where money counts and where it only facilitates exchanges. In a letter to Robertson, after having summarised his theory of the “trapping of money” into the firm’s capital accounts, Harrod explained that the monetary holdings could be used to buy securities, but specified that

I, like Maynard, treat the relation between the quantity of money held on capital account to the price of securities in a sort of part II, part I being concerned with the relation between the money flowing into and out of the income stream and the flow of goods (Harrod to Robertson, 21 May, 1936).

8. Upshot and conclusion

The Trade Cycle marks Harrod’s first attempt to shape a mechanism capable of explaining the cycle in the framework of the methodological and epistemic principles he had devised a few years earlier. The result of this effort, although presenting several interesting aspects, from the analytical viewpoint was far from satisfactory. Harrod failed to link the three compartments of his model in two crucial points: he did not provide a mechanism explaining how prices change in the face of fluctuations in output, and he failed to explain precisely how the prices and the velocity of circulation of money are related. Harrod presupposed, on the ground of his epistemic premise, that there must be a double bind between his three sectors, fluctuations of velocity permitting price changes permitting in turn output variations, and output changes determining the amount of price fluctuations determining in turn the amount of variation of velocity. He knew that the mechanism he proposed was not sufficient to represent this double bind, and invoked a methodological break in the procedure which reveals that he gave higher priority to the epistemic foundation than to the analytic procedure.

Harrod was probably aware that on this point he was on shaky ground, and this feature of his model was criticised by Hawtrey. Perhaps for this reason, in his later writings on cycles and growth he never attempted again to provide an explicit link between static and dynamic forces, but limited himself to the part of the model which instead seemed to hold water from the analytical viewpoint and to satisfy at the same time his epistemic requirement.

The successful part of Harrod’s story was the instability principle, for the multiplier-accelerator mechanism as described in The Trade Cycle actually gives rise to the required instability, and therefore provides an endogenous mechanism explaining the
cumulative character of a deviation from the state of uniform progress, the original
departure being caused by the non-linear character of the dynamic forces (in particular of
the propensity to save). In his later writings, Harrod particularly insisted on this point,
although a shift of emphasis from an explanation based on the causes to an explanation
based on the effects of saving and investment was going to introduce some new troubles
from the analytical viewpoint. But this is another story, and must be told later on
(Chapter VII).

With regard to the notion of dynamics, The Trade Cycle is extremely useful for
understanding what Harrod meant, since it brings together different strands of his
reasoning which -because of his renouncing further attempts to describe the links
between static and dynamic forces- were never discussed again in depth in the same
writings. In the first place, dynamics accounts for change (fluctuations and growth),
while statics must provide the possibility of change, compatibly with the fundamental
conditions. The first corollary of this epistemic distinction is the instability principle,
which is a premise before being a result of Harrod’s reasoning, and which is applied
twice in his book. The first destabilising force is introduced in static analysis, to allow the
system to move away from static equilibrium. The second destabiliser allows the system
to move away from the moving equilibrium of advance, and to force the system into
fluctuations. The second corollary of this distinction is the fundamental character Harrod
attributed to his instability mechanism, which he saw as the primary cause of change, in
opposition to the explanations based on time lags, which he saw as frictions and
maladjustments (this theme, which is of primary importance for understanding Harrod’s
claim of originality with respect to the alternative notions of dynamics mushrooming in
those years, will be discussed in detail in Chapter VI § 3).

As to the object of the disciplines, statics is concerned with the level of output and
dynamics with its change. Since output changes are triggered by investment, statics is
characterised by the absence of saving, and dynamics by its presence.

In the analytical domain, the difference between statics and dynamics reflects on
the notion of equilibrium: equilibrium as a state of rest, or equilibrium as a state of steady
growth in which the various rates of growth would be consistent. In the first case,
equilibrium concerns the individual behaviour and motivations, while in the second case
it concerns the reproduction of the system’s status and condition. Harrod seemed to think
that these notions are equivalent, or at least that in a state of dynamic equilibrium
entrepreneurs feel their actions to be justified by their results. In reality, Harrod did not
discuss the matter in depth, relying instead on his general conception of the link between
static and dynamic forces. But, as we have seen, his analysis on this point was far from
convincing, and he later dropped it altogether. It is therefore not surprising that one of the
branches of the debate on Harrod’s subsequent works regarded his notion of
‘satisfaction’, and that a lack of consistency (see Schelling 1947, p. 868, and McCord
Wright 1949, p. 326) or a lack of specification in the assumptions (Alexander 1950, p.
728) was pointed out, as Harrod himself had eventually to recognise (Harrod 1951a, p.
270. For a discussion of the ambiguity of Harrod’s notion of the warranted rate see
Besomi 1996a, § 3).

Finally, on methodological grounds statics and dynamics are distinguished for the
kind of changes they enable to be discussed. Statics determines the equilibrium level of
output, given certain conditions; if the conditions change, a new equilibrium is
determined. But ‘what happens in between’ is not the concern of statics, which can
therefore only deal with once-over changes. Saving and investment, on the contrary,
entail growth and therefore a continuous change in the circumstances themselves. Growth
and investment are each other’s cause and consequence, and one cannot ignore ‘what
happens in between’. Dynamics is thus concerned with continuous changes.

Although it is not the aim of this monograph to discuss the post-war development
of Harrod’s ideas and the debates which surrounded them, it is worth pointing out that
when he argued out his notion of dynamics in public debates or in private
correspondence, Harrod often referred to one or the other aspect of the distinction
between statics and dynamics, but never showed again how these aspects were related.
For this reason, commentators characterised -and often caricatured- Harrod’s view by
emphasising one aspect or the other in isolation, and proceeded to discuss what they
found useful (the growth equation) in the context of another notion of dynamics
-generally the one proposed by Ragnar Frisch. The failure to re-read The Trade Cycle is
therefore surely one of the causes of the later misunderstanding of the specificity of
Harrod’s notion of dynamics.

9. Towards the “Essay”: the return of saving and investment

It is now time to move on towards the next stage of the making of Harrod’s
dynamics: the famous 1939 “Essay in Dynamic Theory”. The Trade Cycle itself helps in
understanding the transition: Harrod, in fact, provided in his book an alternative
formulation of his theory of the cycle in terms of the saving-investment relationship,
which it is useful to compare to the features of the approach (illustrated in § 1 above) that
he followed in the first part of his book. The “Essay” eventually incorporated some
components of both approaches while ignoring others, and an examination of Harrod’s
choice will provide interesting elements for interpreting his later writings. A detailed
discussion of this aspect will however be postponed to Chapter VII, while here I will
only carry out the comparison between Harrod’s two lines of approach in the Trade
Cycle.

The difference between the two descriptions of the cycle mechanism lies more in
the emphasis on one aspect or the other rather than in the ideas underlying the model
itself. In Chapter II of The Trade Cycle Harrod stressed the importance of the realisation
or the disappointment of expectations, while in Chapter IV he concentrated instead on the
formation of expectations and on the causes and consequences of the entrepreneurs’ and consumers’ decisions.

In Chapter II, Harrod described fluctuations as cumulative deviations from equilibrium: he first showed that steady growth is a state of moving equilibrium, for it satisfies the expectations of the entrepreneurs. Then he argued that such equilibrium is unstable, for growth itself induces some changes in the value of the dynamic determinants in consequence of which the actual growth of consumption falls short of the expected increase on which investment was based. Harrod characterised the consequent abandonment of steady growth using a terminology which reflects, in the negative, the attributes of equilibrium: orders “do not prove fully justified” (Harrod 1936a, p. 92, italics mine), and available capital equipment is not “utilized as fully as was intended, when it was ordered, for the production of current output”, so that “there is disappointment” (ibid., p. 95). “This suggests a slowing down in the rate of increase of orders for new equipment” (ibid.), which in turn sets in motion a cumulative mechanism by which the multiplier and the accelerator amplify the deviation from equilibrium and bring further disappointment.

Harrod’s assumption that ‘disappointment’ induces entrepreneurs to slow down the rate of increase of investment only appears twice, while in the remainder of the Chapter Harrod skipped over this step and only stressed the mechanical aspect of the multiplier/Relation interplay:

To recapitulate this central part of the theory. As soon as disappointment in the results of past investment occurs or is anticipated in consequence of the working of the three dynamic determinants, the rate of increase of investment slows down. This, in accordance with the Multiplier, entails a further slowing down in the rate of increase of consumption. This, in accordance with the Relation, entails an absolute fall in net investment. This, in accordance with the Multiplier, entails an absolute fall in income and consumption. This, in accordance with the Relation, entails that net investment is rapidly reduced to a very low level, if not zero (ibid., p. 98; see also p. 97).

But the formation of expectations is a logically fundamental step in Harrod’s mechanism, for it provides the only reason why a decline in the rate of growth of consumption should influence the rate of investment, which is supposedly dependant on the expectations of advance of consumption. In other words, the divergence from equilibrium can be proved to be cumulative only by adding to the rigid multiplier/accelerator mechanism some assumption chaining the disappointment resulting from a given state of the expectations and the determinants to the decisions to be taken afterwards.

Of course this difficulty is irrelevant in the case of equilibrium growth, for such a state is characterised by the balancing of dynamic forces and by the realisation of expectations. The study of equilibrium configurations of the economy can therefore ignore expectations, for the assumption that steady growth is persisting enables one to substitute in the causal scheme the expected with the realised increase of consumption.
The proper domain of analysis is a single period, but if equilibrium is supposed to persist the same configuration repeats itself over a succession of periods, which are not linked in a chain but are analytically independent ex-post pictures of the same state taken in different moments.

If equilibrium conditions are not satisfied, all the circumstances must be examined, for (as Keynes’s remarked in a letter to Harrod of 12 Apr. 1937, in Keynes CW XIV, p. 172) the system has several degrees of freedom, and changes in any of the forces affecting the multiplier, the accelerator or the formation of expectations may be compensated by equal and opposite changes in other determinants (for a more detailed discussion, see above, § 3). In order to be able to deal with the out-of-equilibrium response of the relevant magnitudes, Harrod’s scheme must therefore be complemented by two sets of assumptions, one regarding the formation of expectations and the second concerning the behaviour of the determinants. Although in the second Chapter of The Trade Cycle there are traces of these hypotheses, they are not properly integrated within Harrod’s framework. In the first place, although Harrod formulated some assumptions regarding the value of the dynamic forces as the volume of production fluctuates, he seemed to confine the determinants’ role to providing the initial disturbance of equilibrium growth and to “ordain a period of steady or cumulative advance” (Harrod 1936a, p. 101) at the end of the depression, for there is no mention of how the intensity of the multiplier and Relation alter in the course of the slump affecting the cycle mechanism.

Second, Harrod dealt with expectations according to the simplistic rule that ‘disappointment of past expectations induces entrepreneurs to reduce the rate of increase of investment, which in turn leads to a further disappointment’. It must be noticed that Harrod did not indicate which criteria entrepreneurs adopt for deciding whether or not they are disappointed; in particular, his notion of capital equipment not being “utilized as fully as was intended” (ibid., pp. 95 and 89) was left unspecified, suggesting that plants and machinery may go under-employed during the slump. Such an interpretation would however contrast with Harrod’s implicit assumption, necessary for the accelerator to express proportionality between capital and consumption, that the degree of utilisation of capital is constant. Moreover, in Harrod’s scheme of thought there was no room for autonomous fluctuations of expectations (while Keynes, on the contrary suggested that ‘unsteady expectations’ are one of the factors to deserve pride of place): otherwise the system could not be closed, and the responsibility for the changes of output would have fallen on the unexplained factors affecting expectations.
In the third place, Harrod failed to discuss the interaction between changes of the expectations and of the determinants as a fundamental component of his cycle mechanism, although he seemed to maintain that the downhill run of expectation determined by (and determining) the multiplier and Relation interplay was sufficient to offset the slightly expansive force exerted by the determinants in the course of the slump. Here Harrod’s approach in terms of forces shows its intrinsic limit: while equilibrium is defined as a state in which forces balance each other, the study of disequilibrium situations requires the possibility of comparing the magnitude of these forces in order to determine the direction, speed and higher moments of movement (even if only in qualitative terms). The reader will remember the change of method Harrod introduced while discussing the de-stabilising effect of the price level (the fourth static determinant), when he renounced the method used in studying the behaviour of the first three determinants and argued that since, on epistemic grounds, static equilibrium must be neutral, and since observation tells us that the general level of prices fluctuates in the same direction as output does, a rise of prices must exactly offset the stabilising effect of the other determinants (see above, § 7.). It is therefore tempting to argue that since depression must be characterised (after a while, at least) by a drop in the level of output, and since we know that slumps actually occur from time to time, the adverse influence of pessimistic expectations must in such occasions prevail over the overall expansive effect of the dynamic determinants. But such an argument would leave fluctuations unexplained, for it amounts to attributing the cause of the cycle to oscillation in the resultant of some forces, and to conclude that since there are fluctuations there must have occurred some appropriate change in the state of the determinants.

Harrod, however, did not fall in this trap. On the other hand, he was also not able to discuss the interaction between determinants and the state of expectations in the course of the cycle. The recourse to a terminology worked out in opposition to the notions expressing the state of equilibrium therefore reflects the only way out which was left to Harrod: each state of the system was implicitly compared to equilibrium, in the form of realisation or disappointment of expectations. Realisation would induce entrepreneurs to continue along the same line of progress, while disappointment would induce them to reduce the rate of growth of investment. At this point the determinants were removed from the scene, for the attention was shifted from the causes to the results of the decline of investment. Once they have determined the original displacement from the equilibrium growth path,

However the dynamic determinants are behaving, the decline in the rate of increase of net investment will bring disappointment on the following day, in consequence of the decline in consumption in accordance with the operation of the multiplier. This being so, any interruption in the steady advance of investment orders will be immediately justified by the results (Harrod 1936a, p. 96).
The determinants were only called back on the scene at the lower turning point of the cycle, where a cause was necessary to reverse the direction of movement (ibid., p. 101).

In Chapter IV of his book, Harrod proposed a different “diagnosis” of the trade cycle, emphasising the role of a component which the first interpretation somewhat neglected:

On the one hand there are the decisions of those who order the construction of capital goods, having in mind that the uses to which they will be put in future are likely to yield a profit. On the other hand there are the decisions of those who decide to refrain from consuming a part of their incomes, in order to make provision for the future. These two sets of decisions are made by different people, but are necessarily mutually interdependent (ibid., p 160).

The cycle precisely results from the interplay of these decisions, and their effects on future decisions. On the one hand, the amount of saving and investment are necessarily equal, for

the capital goods cannot be constructed, unless the wherewithal is surrendered by income receivers for this purpose […] nor can income receivers set aside funds for future use, unless new capital goods are concurrently created to the ownership of which they can acquire the titles (ibid., p. 161).

But, on the other hand, investment and saving decisions are taken by different people on different grounds: “of paramount importance for the former is the rate at which the whole level of activity and consumption of the community is advancing” (ibid.), while as regards saving “the determining factor is quite different; it is not the prospect of an increase of income but the absolute level of income at the moment” (ibid., p. 162).

Since a decrease in the rate of growth of income does not necessarily imply a decrease (even less in the same proportion) of the level of income, saving and investment decisions may diverge: for instance, if the increase of income slows down but its absolute magnitude does not fall the demand for additional capital goods will go down while savers will not choose to save any less (ibid., pp. 162-3).

When this happens those responsible for purveying consumable goods become faced with a falling market. For the purchasing power which savers keep out of the market for consumable goods is no longer fully made up by the expenditure on consumption of those engaged in making capital goods. On the first impact of this situation, stocks of consumable goods are likely to accumulate. In spite of intentions, the two magnitudes are kept equal (ibid., p. 163).

But the accumulation of stocks will induce a reduction in orders for consumption goods, and therefore a reduction in production and income; and the fall in demand for consumption goods dissuades entrepreneurs from ordering new capital goods.

This sets up a further discrepancy between the intentions of those who give orders for capital goods and the intentions of savers, far greater than the original discrepancy. This by the same reasoning brings another much larger wave of trouble for the consumable goods industries (ibid., p. 164).

The kind of analysis illustrated above clearly belongs to the ex-ante/ex-post family, although Harrod did not explicitly use the terminology of the Swedes73. In the
first place, it is a true *disequilibrium analysis*, which does not require nor imply a comparison with equilibrium: the divergence between saving and investment decisions has an autonomous existence, for these sets of decisions are conceived as completely independent of each other. Second, by this interpretation of Harrod’s model qualifies it as a *period analysis*, for the stages of accumulation of stocks and the consequent revision of decisions belong to one and the same cumulative process. This clearly contrasts with the instant after instant treatment proposed in the alternative interpretation of his theory Harrod advanced in Chapter II of the *Trade Cycle*. In the third place, Harrod had shifted his attention from the consequences of *expectations* to the causes and effects of *decisions*, including a precise statement as to the criterion by which entrepreneurs decide whether and how much they are disappointed: instead of the vague reference to ‘the amount of use of capital goods’, here Harrod proposed that entrepreneurs judge on the ground of the piling up of undesired stocks of unsold goods.74

The divergence between the two interpretations can be traced back to the fact that in Chapter IV of his book Harrod directly tackled the problem of the cycle without first developing the analysis of equilibrium which was required by his epistemic premise; his scope, in fact, was only that of ‘summarising’ his ‘diagnosis’ of the problem in order to discuss the possible remedies. In his abridged version, Harrod started from what he later considered the ‘second stage’ of dynamic analysis, namely, the study of the “succession of events”, as contrasted with the instantaneous analysis of equilibrium (for a detailed discussion of Harrod’s distinction of different stages see below, Chapter VI § 3). He could thus develop straight away a disequilibrium period analysis, free of the difficulty implicit in his previous approach in term of forces and states of expectations. The two approaches thus pertain to two successive stages of dynamics, and are therefore to be seen as complementary rather than as antagonistic.

Some specifications are necessary at this point, for in his 1939 “Essay” Harrod employed the Swedes’ terminology in a context apparently similar to the approach of his Chapter IV, and it is important to avoid confusion. In his later work, Harrod had retained some features of both approaches and renounced others. In a sense, it is therefore true that the “Essay” constitutes a development of his previous book, as Harrod himself claimed in the first page of his famous 1939 *Economic Journal* contribution; this statement, however, needs to be qualified. A full discussion of the differences between these two stages of Harrod’s dynamic thought will be postponed to Chapter VII, but the reader may appreciate to know in advance the line of development. From the interpretation he advanced in Chapter II, Harrod preserved the approach in terms of the instant after instant comparison with the would-be equilibrium state, while from the alternative exposition he maintained the emphasis on the importance of undesired accumulation or decumulation of stocks. He renounced to the analysis of both expectations and decisions, which constituted instead the respective and complementary cores of his previous approaches. The final result was characterised in the first place by a
re-definition of the notion of *ex-ante*, which no longer referred to the decisions of entrepreneurs and consumers but to the equilibrium solution of his simple system of equations. In the second place, Harrod deprived changes in stock of their causal significance, for instead of considering them as the *result* of the divergence between saving and investment intentions and as the *cause* of the revision of the future decisions, he implicitly defined them as *equivalent* to deviations from equilibrium.

It therefore seems to me that, in spite of the analytical difficulties that Harrod was not able to solve (and sometimes even not to recognise), his book on *The Trade Cycle* has the advantage with respect to the more celebrated “Essay”, to devote some attention to the role of expectations and the considerations guiding decision taking. Before turning to a detailed analysis of this evolution in Harrod’s dynamic thought, it is however necessary to reflect on his conception of science and method. There, in fact, lies the foundation for Harrod’s peculiar notion of the role of time in dynamic analysis, which is at the origin of the transition.

**Notes**

1 This scheme reflects Pigou’s steps in the analysis of stationary states, as well as the terminology relating to the ‘fundamental conditions’ (Pigou 1935, p. 4).

2 It is important to notice that 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” (1936a, p. 1); “the rate of interest must so move as to provide a force which, when operating jointly with the forces exerted by the propensity to save, the elasticity of demand for goods, and inventions, causes the three dynamic determinants to justify a continuance of the advance” (ibid., p.116). The significance of this equivalence will be discussed in Section 2 below.

3 For a discussion of Harrod’s law of diminishing elasticity of demand and for an early review of the discussions it arose, see Sumner 1940. On Harrod’s law and the phenomenology of the trade cycle, see Chapter I § 2.

4 Harrod was conscious of the difficulties that the notion of general level of prices involved, and proposed to treat changes in the price level as compounded of an element of uniform advance or recession -that is, leaving relative prices unchanged- and of deviations from it (Harrod 1936a, pp. 36 and 42-43). In general this procedure is far from being satisfactory, for no mechanism was postulated to explain the nature and the magnitude of the deviations from uniformity, but Harrod was only concerned with the empirically observed and generally recognised feature of the trade cycle that prices in general tend to increase during booms and fall during depressions.

5 Harrod’s notion that the general price level acts as a destabiliser is implicitly based on his former considerations regarding the stickiness of monetary wages (see Chapter II, § 3): in fact, only if the rewards of prime factors, being contracted in terms of money, do not change in the exact proportion of the prices of commodities (cf. 1936a, pp. 37 and 46n), an increase in the general level of prices entails higher profits margins, and therefore stimulates production; on the contrary, if all prices of goods and factors changed in proportion, the exchanges would occur precisely as in a barter system, and money would be perfectly neutral.

It must be noted that Harrod accepted the stickiness of money wages as a fact of experience, without explicitly discussing the properties of money as a *numéraire*:

Changes in reward costs [i.e., the rates of pay accruing to factors per unit as measured by existing practice] are observable phenomena; we assume that the generalization may be accepted that the fluctuation of reward costs (measured in money) is smaller than the fluctuation in prices (Harrod 1936b, p. 85).
The time structure of the process involved in this scheme will be discussed below in this Chapter, § 3. Time and dynamics, however, is one of the leading themes of the present study, and is thus discussed in several places, in particular in Chapter VI § 3 and Chapter VII §§ 4 and 6.

Harrod’s model (including its later versions), however, has rarely been interpreted in such terms. It was rather considered as the ancestor of the multiplier-accelerator models (Samuelson 1939 recognised the pioneering role of The Trade Cycle in the study of the mutual interactions of the multiplier and the accelerator), and such a scheme was represented in terms of functional equations. Harrod’s contribution was thus re-stated in the language characterising the ‘time-lag theories of the cycle’ of the econometricians (see Introduction, §1), and its key concepts were thus radically re-interpreted (on the reinterpretation of concepts implied by shifts of language with particular reference to the econometricians’ notion of dynamics, see Besomi 1991, pp. 256-260).

Harrod’s rates of growth are obviously one of the key elements in the solution of the functional equations constituting the multiplier-accelerator models: the solution of the characteristic equations associated to linear functional equations provides the rate of growth of the variable under consideration, while a phase diagram also indicates, among other things, the instantaneous rates of growth. If in a sense Harrod’s interpretation of his own mechanism could thus be understood as focusing on a sub-set of the information provided by a mathematical analysis of the corresponding functional equation—and this is surely one of the reasons why Harrod’s dynamics could so easily be engulfed by the rival conception—, it must be anticipated here that the divergence between the two approaches is of a methodological nature; for a full discussion, see Chapter VI § 3.

This aspect will be further discussed below, § 3.

In reality, Harrod treated on the same grounds both exact equilibrium and excess of equilibrium growth (he thought the boom to be characterised by steady or cumulative advance), and contrasted them to the less than justified growth leading to depressions. His main concern, in fact, was with the outset of the depression. This asymmetry between boom and depression is further discussed at the end of this Section (see also Chapter VII, note 15).

It must be noticed that Harrod’s conclusion depends on his implicit assumptions regarding the formation of expectations; this aspect will be discussed in Section 3 below.

“We cannot rely on the three dynamic determinants to maintain [a steady advance]” (ibid., p. 104)

In addition to this epistemic line of criticism, Harrod also advanced some doubts as to the actual direction of influence of optimism and pessimism:

It is worth, however, mentioning a leading example of a factor supposed to operate in this way, viz. psychological optimism and pessimism. In the case of our second problem (installation of equipment) I suppose it is clear in which way the factors would work, namely, optimism would lead to excessive and pessimism to deficient installation. But with regard to the first problem [What determines business men in the volume of current output which they produce?] I have always been unable to see why optimism should lead to increase output, since it might equally well lead an entrepreneur to set the price too high, viz. hope he could sell more than he actually could at a given price and so sell and produce less than he would have done if his judgement had been more balanced. Which way optimism would work might then depend on institutional arrangements and it might be advantageous to classify these with this end in view (Harrod 1937*, pp. 6-7).

This analogy was not explicitly pursued by Harrod; I only refer to it as an expository device for illustrating the extent of Harrod’s detachment of the traditional (Pigouvian, in particular) approach. However, I do not want to overemphasise the novelty of Harrod’s proposal. In fact, although Harrod himself thought his dynamics to constitute the genuine revolution, his approach was explicitly meant to rescue all the valid elements of orthodoxy; this aspect will be discussed in the Chapter VIII.

On Galileo and the principle of inertia, see Koyré 1966, part III.

In a footnote, Koyré specified that «status comes from sto, stare, “to stay”, and means station, position, condition. Status movendi is just as paradoxical as statical dynamics» (ibid., p. 66).

The tacit target of Harrod’s criticism was Pigou’s psychological explanation of the cycle: see above, Chapter I § 3.

That is, its existence in potentia. This is the ethymology of ‘dynamics’.

The independence of any forces upon the others is a necessary condition for assuming that they can be added together. Harrod seems to have been conscious of this requirement, and discussed it with reference to a special case on p. 3 of his book.
A further reason, why Harrod framed the supply and demand analysis in terms of forces, lies in his desire to have the general level of prices to be determined not *conjointly with* but *by* the level of output (Harrod 1937a, p. 80). This change of perspective was made possible by the property that if independent forces are added together, in equilibrium the value of each of them can be determined as the resultant of the others. As to Harrod’s motives for introducing such a shift of perspective, see below, § 4. It is however worth anticipating that the reason to be exposed below results from another of the methodological implications Harrod drew from his epistemic criticism to the orthodox approach to the cycle.

Explaining his position to Keynes, Harrod specified that for the system to be “in neutral equilibrium over the range within which it swings”,

any one force must be equal and opposite to the resultant of all the others. The price movement is a de-stabiliser. ∴ the resultant of the others is a stabiliser. If the price drop required to induce a unit change in output is large, the stabilisers must be acting strongly. If I have to exert great strength to make the billiard ball move an inch over the cloth, the forces opposed to that action must be great.

Conversely, if “the stabilising forces are weak to the point of non-existence”, “an infinitesimal price drop will take the billiard ball right across the table” (Harrod to Keynes, 7 April, 1937, in Keynes CW XIV, p. 169).

Harrod’s peculiar approach kept puzzling Keynes even after Harrod explained to him the necessity of the neutrality of static equilibrium:

I still do not understand why you call those forces stabilisers which, we agree, are those which cause prices to fluctuate. However, I am not really quarrelling here with anything except a lack of explanation as what you have in mind (Keynes to Harrod, 12 Apr., 1937, in Keynes CW XIV, p. 173; see also p. 152).

As we shall see in Section 7 below, Harrod was well aware that a methodological break was involved in this rather tortuous argument; however, the reason for the possibility of a theory of the cycle had to prevail over the reason of its methodological consistency.

It is thus far from surprising that a few years later Harrod affirmed that he did not “regard the multiplier principle as belonging to dynamics” (Harrod to Robertson, 20 March, 1945).

An example will be discussed in Section 7 below.

Frisch 1933, p. 171; see also Frisch 1936, p. 100. There is no evidence that at the time of writing the *Trade Cycle* Harrod was already concerned with Ragnar Frisch’s rival notion of dynamics, while certainly Harrod’s later grievance against time lags was mainly directed against the econometricians’ approach (Tinbergen’s in particular), which Harrod discussed with Keynes in 1938 (the relevant correspondence is transcribed in Keynes CW XIV, pp. 295-305). However, Harrod might have tried to familiarise with the writings of some of the econometricians before beginning his work on the *Trade Cycle*. On October 3, 1935 he wrote to Robertson: “I have before me *Econometrica*, with its Tinbergen and Kalecki. I suspect that Kalecki is saying something that I have been feeling towards, and I have got to try and find out. But what a paper it is!” A letter from Kalecki survives, dated 9 Oct., 1935, certifying that Harrod actually read Kalecki’s *Econometrica* article and suggesting that he found it difficult to understand (Kalecki 1935). Kalecki sent Harrod an offprint of the less mathematical version of his article, published in French (Kalecki 1935a). Harrod’s letters to Kalecki unfortunately did not survive.

The relationship between Harrod and the econometricians will be discussed in detail in Chapter VI § 3 below.

See also Harrod 1936a, p. 150, and his letter to Keynes of 6 April, 1937: “the static system provides an analysis of what happens where there is no increase which entails […] that saving = 0” (in Keynes CW XIV, p. 164). Harrod repeatedly raised the issue with Robertson also:

I fear I only repeat myself, but I havent heard the answer from you or anyone else -to treat saving as on a par with the other factors of production is logically fallacious because saving involves growth which contradicts the static assumptions. I have slightly the impression when I say this that you mentally brush it aside -oh, that old dynamic business, we can make corrections for that in Pt II. But I think it makes what you say in Pt I definitely wrong, wrong as it would be if one said that the square on the hypotenuse is equal to the difference between the squares on the other two sides (Harrod to Robertson, 4 Oct., 1937).
The reviews the book received. Some reviewers of The Trade Cycle have noticed that the process generated surprising that this aspect of Harrod’s analysis was not perceived by his contemporaries, as is certified by the Aristotelian physics, according to which motion had to be sustained by air. It is therefore not thought of the 1930s as Galileo’s idea of the permanence of uniform motion in a straight line was alien to the economic investments, might possibly increase steadily (i.e., with continuity and regularity), without the growth status of the system, i.e. a condition capable of self-reproduction without the help of forces sustaining it. After all, the idea that the level of activity of the economic system, its consumption, income and investments, might possibly increase steadily (i.e., with continuity and regularity), without the growth being caused by some exogenous forces responsible for its maintenance, was as alien to the economic thought of the 1930s as Galileo’s idea of the permanence of uniform motion in a straight line was alien to the Aristotelian physics, according to which motion had to be sustained by air. It is therefore not surprising that this aspect of Harrod’s analysis was not perceived by his contemporaries, as is certified by the reviews the book received. Some reviewers of The Trade Cycle have noticed that the process generated...
by the interaction of the Relation and the multiplier is cumulative in nature (Haberler 1937, 691; Hansen, 1937). Others mentioned the instability of justified growth (Robertson 1937a, 125), sometimes even refusing to admit that the notion of steady growth might be relevant to the analysis of the business cycle (Robinson 1936, 692-3). Finally, it was denied that Harrod’s mechanism had anything to do with the cycle, for the functional equation resulting from the interaction of the multiplier and the accelerator was of the first order and could not therefore give rise to oscillations but only to exponential growth (Tinbergen 1937). None of them has been able to understand the analytical role of the status of uniform growth and of its instability. Only Jack Stafford seems to have perceived the similarity in the status of static and dynamic equilibria (Stafford 1937, 69 and 74-75). This lack of understanding of the role of steady advance in Harrod’s method is also certified by the fact that economic dynamics ignored for the following fifteen years his analysis in terms of the rate of growth.

Not having grasped this feature at his first reading, Keynes could obviously not appraise its methodological counterpart. Harrod’s analysis presupposed that the economic system was experiencing growth at a constant rate, and aimed to determine the conditions at which such a status might be maintained. Keynes’s calculations concerned instead the problem of getting the system into motion, since he considered sudden variations in investment. Keynes thus interpreted Harrod’s procedure as “a logical slip in the argument” and “a straightforward slip in arithmetic” (Keynes to Harrod, 31 March 1937, in Keynes CW XIV, pp. 151 and 155 ff.).

39 Analogously a few months later, discussing the orthodox theory of interest with Robertson, Harrod maintained -on the ground of the same principle- that it “is wrong because it is one dimension out” (Harrod to Robertson, 8 Oct., 1937).

40 This was meant to constitute a main step towards economic dynamics:

So far as the dynamics is concerned, all or almost all the constructive work still lies before us. Nothing very systematic has been achieved. Writers like yourself have thrown out valuable hints. Maynard has thrown out a big hint. I have tried to make a tiny little nibble in my last book. These hints and strivings cannot be divided into the orthodox and unorthodox, the traditional and the rebellious, since there is no tradition against which to rebel. The only rebellion consists of saying or rather recognizing that this re-orientation is now necessary. The difference is perhaps between those who imply it and those who explicitly recognize it (Harrod to Robertson, 25 Dec., 1936).

41 It is well known that the relationship between marginal efficiency of capital and the rate of interest in Chapter 11 of the General Theory presents some formal analogies with the neoclassical explanation of investment as depending on the rate of interest and the marginal productivity of capital. Therefore it is not surprising that Harrod could not see much difference between the two notions. The point that Harrod (and many of his contemporaries) missed out was that marginal productivity depends on the revenue of the capital as calculated on its original cost after its life is over, while marginal efficiency represents the expected, future (and uncertain) rate of return of that decision to order some new capital goods.

The main difference therefore lies in Harrod and Keynes’s views about time and expectations, and is emblematically represented by the fact that in his paper Harrod completely ignored Chapter 12 of the General Theory. For a discussion of Harrod’s notion of expectations in the Trade Cycle see above in § 3 above, while for a comparison of the evolution of Harrod’s attitude towards the role of time in economics from The Trade Cycle to the “Essay” to Keynes’s ideas on this topic see Chapter VII, in particular §§ 4 and 7.

42 This argument was already expounded before Harrod illustrated the operation of the dynamic determinants:

when the still unrevealed forces ordain that output shall recede, money is capable of exerting a sufficiently de-stabilizing force to overcome the forces of the stabilizers. In fact, it provides a medium enabling output as a whole to stand, so far as all the determinants are concerned, in a condition of neutral equilibrium. If a recession is to take place, the monetary mechanism does not oppose an obstinate resistance but allows prices to drop by whatever amount is required; if an advance is to take place, it allows the requisite rise of prices (Harrod 1936a, p. 47).

43 This is a further instance of the analogy of Harrod’s criticism of traditional theory with Galileo’s attack on Aristotelian physics: the latter based its theory of motion on the presence of air, which was meant to provide a medium and to sustain movement, while Galileo saw in the friction of air an obstacle to movement. Thus Galileo accused the Aristotelians of confusing a resistance with the true cause of the persistence of motion. See above, § 2.

44 In The Trade Cycle there was not much room for the rate of interest, for Harrod maintained that changes in this variable could only be effective if their magnitude was quite substantial and if they
occurred within a short lapse of time (the ‘breathing space’) after the dynamic determinants decreed the downturn but while outstanding orders still held the recession within fairly narrow limits (Harrod 1936a, pp. 104 and 168-169). Harrod thus felt no need to discuss the determination of the rate of interest, but just noticed that the liquidity preference approach was not necessary to, nor incompatible with, what he had to say (ibid., p. 120). On the other hand, the traditional theory of interest had to be discarded for the reasons I have mentioned above, so that to complete the theoretical set-up some alternative formulation would be anyhow necessary. Harrod thought therefore that Keynes’s theory had to be accepted, provisionally at least.

45 For this reason I think that Harrod’s article on “Mr. Keynes and Traditional Theory”, where the relationship between prices and money is discussed at length, although not very illuminating about Keynes nor about orthodox theory, is enlightening as regards the relationship between Harrod and Traditional Theory.

46 Already towards the end of 1933, Harrod anticipated the reversing of the customary causal interpretation of the quantity equation (see Chapter II, note 25).

47 This dichotomy was a prelude to the vanishing of money altogether from the subsequent version of Harrod’s dynamics: see below Chapter VII § 7.

48 On Harrod’s earlier polemics against the Hayekian viewpoint, see Chapter III, § 1.

49 This position was further radicalised in the “Essay”, where money almost completely disappeared.

50 Before the book went on print, Robertson commented: “certainly I shall not be prejudiced against it because it looks for real causes behind the monetary ones, -of that I heartily approve” (Robertson to Harrod, 22 July, 1936).

51 As a consequence of the assumption of the uniformity of advances and recession, Harrod could circumvent the problem of defining the units of measure of his magnitudes, for he could neglect the possibility (and the consequences) of changes in the relative prices.

On this point, the contrast with Keynes is striking: the problem of the choice of units of quantity and of measure was in fact one of the recurring questions of his intellectual career (see Carabelli 1992). On the contrary, Harrod probably did not appreciate the importance of Keynes’s discussion of the choice of the unit of measure. Keynes’s impression in fact was that Harrod missed the whole point as expressed in the proofs of the General Theory: “I do not agree that my method does not avoid units of aggregate output. The whole of your comment here indicates that you have not got me” (Keynes to Harrod, 9 Aug., 1935, in Keynes CW XIII, p. 538).

Throughout Harrod’s correspondence of the 1930s there does not seem to be any reference to the problem of measurement, but for a few specific remarks on the measurement of capital (correspondence with Kaldor, Joan Robinson and Keynes), which however it will not be possible to discuss here.

52 Uniform advance must not be confused with a steady advance, the former being defined in terms of constant relative prices and the latter in terms of constant rate of growth.

53 The non-linearity of Harrod’s model implied in the variability of the accelerator and the multiplier was a fundamental ingredient in the 1939 version of his theory, and will therefore be discussed below, in Chapter VII § 8.

54 That a reasoning in terms of values rather than quantities was completely extraneous to Harrod’s mode of thought is also revealed by his dismay -as expressed in a letter to Meade- in front of “a point which if right is surely important”: at one of the meetings of the Oxford Economists’ Research Group, Hitch argued that it is the value not the volume of capital goods output which is important as touching the absorption of saving. Consequently, he held, that, since the demand for constructional goods as a whole is probably inelastic in a slump, arrangements tending to hold up their prices were beneficial in mitigating the severity of the slump. We argued your point against him that a drop in capital goods prices is helpful in tending to make the productive process more capitalistic at a moment at which it is all important that investment should be stimulated, but he argued that the drop in prices with an inelastic demand reduces net investment even if it does lead to some substitution. […] Do you agree? (Harrod to Meade, 7 Feb., 1937).

55 The recourse to Marxian terminology in comparing Harrod’s and Keynes’s views on the monetary conditions of equilibrium, beside providing a neat expression representing the problem under examination, also seems to be appropriate because of the formal similarity between Harrod’s notion of moving equilibrium and the more mechanical of the interpretations of Marx’s schemes of enlarged reproduction (see Orzech and Groll 1983 and Bronfenbrenner and Wolfson 1984); on the other hand, reference to this terminology is justified by the fact that Keynes himself referred to the realisation problem in his 1933 lectures (see Carabelli 1988, p. 210).
“In a depression and in the absence of hoarding in [Robertson’s] sense, the money automatically gets trapped into the capital account of the firms. [...] I believe that my notion of the trapping of money in this way is new” (Harrod to Robertson, 21 May, 1936).

Harrod also expressed this view in a letter to Henderson: “Maynard’s point that the rate of interest cannot be expected to be naturally adjusted so as to provide full employment seems to me to stand” (Harrod to Henderson, 9 Apr., 1936).

On the analogous cognitive status of the assumption of stickiness of monetary wages in this precise connection, see note 5 above. On the role of empirical generalisations in Harrod’s philosophy of science, see Chapter VI § 1, and in particular note 14 for a reference to the ‘change of method’.

This emerges with particular clarity in the Section on “The movements of Prices and Profits” (Harrod 1936a, pp. 75-88), where the three stabilisers are called to “account for the whole of the movements [of prices and profits] observed” (p. 85). Harrod’s reasoning developed along the following line (this particular example regards Harrod’s argument relating to the Plasticity of Prime Costs, but was extended to the other determinants):

A rise in [the] rates of pay involving a proportionate rise of marginal costs is associated, ceteris paribus, with a rise of prices. Consequently, if these rates of pay do tend to rise with increasing employment or to fall with increasing unemployment, corresponding rises and falls of the price-level in boom and slump may be expected (ibid., p. 81).

“Our list of governing considerations is exhaustive” (ibid., p. 85).

The fluctuation of velocity was again assumed as a fact: “we know that velocity is subject to variation”, and the opposite assumption of steadiness would require the construction of “a complete model of an economic system different from anything we know”. Again: “the conviction that, if people have money they will use it, is responsible for the most dogmatic pronouncements of quantity theorists. Of course, we know that in fact they do not use it” (Harrod 1936a, pp. 126-127).

In this his view differed from Keynes’s, according to whom all the magnitudes involved in the quantity equation interact with each other, so that it is not possible to distinguish among them exogenous and dependent variables, and to single out any causal nexus whatsoever (see e.g. Davidson 1978, p. 47).

Hawtrey’s final comment is quite interesting and worth quoting, even if Harrod does not seem to have replied:

The absorption and release of cash are of fundamental importance because it is through them that arises an inequality of output and sales. If, in the notation used in Chapter VI of my Capital and Employment, there is to be any difference between \( A^1 \), sales to final purchasers, and \( A \), output, there must on balance be an absorption or release of cash on its way from the consumers who receive incomes to the amount of \( B = A - K^1 \) to the final stage of sale, \( A^1 - K^1 \). The absorption or release of cash may occur among consumers, or in the investment market, or in the banks, or in industry. Any theory which traces fluctuations to an excess or deficiency of demand in comparison with output requires the occurrence of an absorption or release of cash somewhere. Your theory depends on it occurring in the investment market, Keynes’ on it occurring among consumers. I attach special importance to the absorption of cash by industry induced by a rise in the short-term rate of interest. But these are all particular applications of the same general principle, differing as to the motives at work, and the groups of people affected by them. If, in working out any theory of this type, the absorption or release of cash is supposed not to occur, the theory simply collapses (Hawtrey to Harrod, 24 Dec., 1937).

The method of successive approximation Harrod used in the first Chapter of the Trade Cycle also contributes to convey such an impression: the monetary ‘complications’ were introduced only at the end, after exchanges and production were analysed in real terms. It is at this point that the change in the method of procedure was introduced, so that there was no analytical continuity between the treatment of quantities and prices.

This peculiarity did not escape the attention of Keynes, who was ready to point out that “this [referring to a cycle mechanism based on the acceleration principle] seems to assume, as a good deal of the argument does, that interest rates are constant” (Keynes to Harrod, “Miscellaneous Notes” on the Trade Cycle, 31 Mar., 1937, in Keynes CW XIV, p. 152).

Hawtrey found the argument on which this conclusion was based “very unsatisfactory”: 1937, p. 327.

This later generated some confusion, and may indeed be one of the main reasons why the peculiarity of Harrod’s notion of dynamics was ignored by commentators. Harrod, in fact, from time to time kept referring to one or the other aspect characterising his idea of dynamics which found its roots in the global
treatment of the subject in *The Trade Cycle*, but without providing context. Such attitude gave rise to endless debates, which is not possible to summarise here (for a detailed discussion see Besomi 1996a).

68 As Alexander 1950 noted, there is no intrinsic reason to assume that disappointment induces entrepreneurs to modify their current rate of growth of investment, rather than e.g. its absolute value.

69 For the necessary qualifications regarding equilibrium, expectations and the resultant of the dynamic forces, see above, § 3.

70 Keynes, in considering how changes in some forces could offset movements in others, mentioned as examples

(a) steady growth of consumption and steady growth of investment with unsteady expectation offset by unsteady interest, or (b) steady growth of consumption and unsteady growth of investment offset by an unsteady relation and an unsteady multiplier (Keynes to Harrod, 12 Apr., 1937, in Keynes *CW* XIV, p. 172).

Furthermore, Keynes remarked that “the rate of interest is a function of the widely fluctuating state of expectation” (Keynes to Harrod, 20 April, 1937, in Keynes *CW* XIV, p. 178).

71 As income decreases, there is a shift from profits and a general decrease in the propensity to save, while the rate of interest falls stimulating the adoption of more capital-intensive techniques.

72 If one should try to express in formal terms the relationships indicated by Harrod, a highly non-linear functional retarded equation would be obtained, which would of course be impossible to solve analytically.

73 We know however that he came in contact with the terminology of the Swedes since 1934, since Haberler discussed with Harrod Lindahl’s “Dynamic Pricing Problem” (Lindahl 1934*; Haberler had sent Harrod a copy of his letter to Lindahl of 12 Nov., 1934). Later on, Harrod referred to the ex-ante/ex-post distinction in his discussion of the multiplier, where he compared the intentions of savers and investors and the result of their decisions, attributing the difference to the undesigned accumulation of stocks: Harrod 1936a, pp. 72-3.

74 If on the one hand such precision enables the reformulation of the model in concise and clear-cut terms, it must be kept in mind that it involves renouncing to develop otherwise interesting analytical aspects, for the accumulation of unsold goods is certainly not the only feature of depression nor the unique indicator for entrepreneurs.
Chapter VI
Interlude: Harrod on Method

In August 1938, Harrod organised and chaired the meeting of the Economic section of the British Association, and in the position of President he delivered a long speech on the “Scope and Method of Economics” (later published as Harrod 1938a). Harrod first thought of methodology as a subject for his presidential address in January 1938, when he wrote to Keynes saying that he was “thinking of doing a heavy methodological piece about statics & dynamics, the scope of pure theory, the place of induction etc.” (Harrod to Keynes, 24 Jan., 1938). Keynes tried to dissuade him, on the grounds that the annual meeting of the British Association is “a somewhat popular occasion, and, as the President’s address gets a fairly good summary in the press, it is not a bad platform for matters of general interest. But what you have in mind looks very serious and academic for such an occasion” (Keynes to Harrod, 26 Jan., 1938). Harrod, however, felt “a strong inner urge to say something” on method (Harrod 1938a, p. 384), and as a result of this choice “anything [he] had to say about recent monetary literature has been squeezed out by deduction, induction etc. and whether the utilities of two people can be compared!” (Harrod to Robertson, 5 July, 1938).

Harrod’s methodological reflections are precious for understanding his dynamics. In the first place, the Presidential Address represented Harrod’s first attempt in print to “emphasise the limitations of deduction” (ibid., p. 386) and reconcile it with induction, in economics in general and also with special reference to dynamics. Harrod’s view must be seen in the context of his adherence to the neo-positivist method of linguistic analysis, which led him to found his dynamic laws on an axiomatic basis derived from empirical generalisations. This enables one to assess the scientific status Harrod attributed to his dynamics and reveals that its scope is confined to an instrument of thought, while its usefulness is limited as a diagnostic tool for the state of the economy (Section 1).

In the second place, in his methodological essay Harrod explicitly introduced the distinction between simultaneous analysis and the study of the succession of events, which lies at the heart of his method of dynamic analysis, providing the thread that unifies the development of his thought from the 1934 essay on “The expansion of Credit in an Advancing Community” to the “Essay in Dynamic Theory” (written shortly after Harrod had revised the methodological essay2). Here also lies the fundamental difference between Harrod’s approach to dynamics and the econometricians’ ‘time lags theories of the cycle’ (Section 3). Harrod insisted on this distinction both in “Scope and Method” and in the subsequent correspondence with Keynes on the heuristic and descriptive roles of models in economic analysis (Section 2).
As to the chronology of the making of Harrod’s dynamics, his reflections on the place and significance of induction and deduction for scientific thought may appear an interlude between the formulation of his two early versions of the theory of cycles and growth. However, from the logical point of view (and, I dare to say, from the point of view of Harrod’s intellectual history) they must be considered an essential step in the systematization of Harrod’s approach to dynamics. By discussing the scientific status of his dynamics, Harrod aimed to substantiate his claim that his line of approach was the rational procedure to tackle the problem of economic change. In his view, in fact, these reflections provided the ground for the claim that his notion of dynamics is more fundamental than that provided by Frisch (which eventually became the accepted one), and for the idea (discussed in detail in Chapter VIII below) that his dynamics constitutes the true revolution in economic science. It is therefore worth abandoning for a moment the economic domain, and following Harrod in his philosophical reflection before returning in Section 3 to the opposition between Harrod and the econometricians.

1. **Axioms and basic empirical laws**

   In the preface to his *Foundations of Inductive Logic*, Harrod recollected that he had “devoted concentrated and arduous research for numerous periods in the last twenty years” to the subject of induction, which had “been of interest to [him] over a long stretch of time” (Harrod 1956, p. v). Since at least 1934, Harrod’s bent in favour of a reappraisal of induction manifested itself as a favourable attitude towards empirical research. In fact his response to James Meade’s memo on “Short Period Demand for Labour. Plan of Statistical Work” (Meade 1934*) was so enthusiastic that he expressed the feeling

   > that we *must* form a committee to develop this. Its object would be to enumerate and classify the quantitative enquiries the results of which would be of interest to theorists. I suspect that many of us would want to know much the same thing. It should be emphasized that our object is not to discover some subject for investigation that could be appropriately undertaken by a university, but to discover what subjects of investigation would yield results actually required by working theorists (Harrod to Meade, 4 Oct., 1934).

   A few months later, in February 1935, Harrod became an ardent supporter of Henderson’s “projected new technique of economic research” which consisted in “wedding theory to fact-finding” (Harrod to Henderson, 25 Feb., 1935). This eventuated in the constitution of the Oxford Economists’ Research Group, in which Harrod took active part until the war.

   However, until 1938 there is no evidence of any attempt to discuss the subject in a systematic way. The Presidential address to the British Association provides the first evidence in print of Harrod’s interest in induction. Shortly afterwards, he started working on a book on the topic which, however, was never published. The critical target of “Scope and Method” was the excessive weight usually given to deduction in economics, while the book aimed (among other things) at emphasising the place of inductive
reasoning for knowledge in general, with special attention to scientific knowledge⁴. The two works are therefore worth reading jointly, for the manuscript provides some specifications to notions that were not fully worked out in the methodological essay⁵.

As preliminary groundwork to the “scientific aspect” of economics — “namely, the formulation of general laws and maxims” - (Harrod 1938a, pp. 385-6), Harrod thought “a simultaneous chart or survey of the economic field” to be necessary, and specified that “the main work of the cartographer is analysis and classification”⁶ (ibid., p. 387). As examples, he mentioned the following:

The relevant propositions may be stated in the form of truisms or tautologies, 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 ground of the recognition of truisms and definition of concepts, a “map” is formulated, which consists in the specification of the set of possible states of the world. The map provides a guide for life, for only with reference to a map can we distinguish a piece of scrap paper from a cheque (Harrod 1940/41*, Book I, Chapter 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 1938a, p. 392). Adam Smith perceived that by means of such an 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). On the ground 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 specification regarding the exclusion of the sequence of events is important, for the search for laws of succession constitutes the stage following 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*, Book I, Chapter 2, TS p. 43).

And “it is with the laws governing the succession of [possible states of the world] that science is mainly concerned” (ibid., p. 21). Only the elaboration of such a corpus of laws permits us to formulate predictions as to the future course of events (ibid., pp. 20-21 and passim; 1938a, pp. 398-9 and passim).

Harrod stressed in several places that the elaboration of both the instantaneous and the sequential maps relies on induction:

The information represented in the map of possible tracks implies a threefold use of the inductive principle. i. Reliance on memory is assumed. ii. The law by which a reverse flow of phenomena follows on appropriate acts of volition designed to move my body is taken to be established by experience. iii. Laws governing the succession of phenomena, the burning of fire, the ebb and flow of tides etc. have to be assumed in order to keep the map to date. Thus almost all we know about what are called particular things or, to put it otherwise, our geographical knowledge, depends on generalizations based upon the experience of uniformities of succession (Harrod 1940/41*, Book I, Chapter 2, TS p. 20-21).

He thus concluded as follows:

Knowledge of the law[s of nature] must be divided into stages.
1. There must be a comparison of remembered concomitances […].
2. There is an extrapolation or assumption of the occurrence of resemblances. How this assumption is used to build up our idea of the world has already been described in the [above-mentioned passages of the] second chapter.
3. There is or may be an appreciation of the inductive principle which justifies the extrapolation.

[…] There are two possible ways of defining [reasonable] belief in laws.
1. Where the extrapolation is made in accordance with the inductive principle.
2. Whether the extrapolation is made in accordance with and with the appreciation of the inductive principle. (Harrod 1940/41*, Book I, Chapter 5, p. 30).

To know, is to use the inductive principle (ibid., p. 32).

Here lies the problem that Harrod struggled for twenty years to solve: on the one hand, the laws and theorems of economics have a deductive character, but on the other hand they “must have an empirical basis” (Harrod 1938a, p. 386). How could the contrast between the deductive character of the conclusions inferred from basic laws and the inductive nature of the process aiming at establishing these laws be solved? This problem of course was not new at the end of the 1930s, since it had been debated with respect to the specificity of economics for almost a century (for an account of the debate, see e.g. Blaug 1980, Chapter 3). But Harrod’s reflection came on the background of “prospects of an invigorating revolution” (Harrod 1959, p. 65) in the Oxford
philosophical scene, for in the early 1930s Alfred Ayer introduced the linguistic analysis of the “Viennese Circle”. Earlier on, in Cambridge Frank Ramsey had translated into English Wittgenstein’s *Tractatus* and recasted Russell’s and Whitehead’s *Principia* on Wittgensteinian lines. Both these men frequently met and discussed with Harrod, and undoubtedly exerted a deep influence on his thought on these matters.

According to Harrod’s own recollection, since his days as a student he “was excited by the idea that Russell and Whitehead had put the logic of mathematics on a better foundation than did Mill”, and seems to have been impressed by the conclusion that “the certainty that we get in mathematics was due to its being, in some sense or another, tautological” (ibid., p. 60). The *Principia* stimulated not only different lines of development in logic, but together with Wittgenstein’s *Tractatus* also provided the method of logical analysis of language adopted by the Viennese circle. Among the upshots of this movement, Ayer’s 1936 *Language, Truth and Logic* is of the utmost interest for understanding Harrod’s viewpoint. Ayer, in fact, in discussing the nature of ‘genuine’ (i.e., non senseless or non metaphysical) propositions, divided them into two classes, the *empirical* and the *a priori*, the former concerning ‘matters of fact’ and the latter concerning ‘relations of ideas’. As regards the validity of propositions, again he distinguished two classes. The validity of *analytic* propositions depends solely on the definitions of the symbols it contains; the validity of *synthetic* propositions is determined by the facts of experience. In Chapter 4 of his book, Ayer confuted Kant’s view that some propositions (in particular some mathematical propositions) are of a synthetic *a priori* nature, and supported instead the view that “the truths of logic and mathematics are analytic propositions or tautologies”. This result relied on the one hand on the formalisation of logic, which enabled the logical positivists to get rid of “irrelevant psychological questions” and eventually revealed the analytical character of mathematical propositions. On the other hand, the invention of non-Euclidean geometries proved wrong Kant’s view that the propositions of geometry have factual content, which was based on his understanding of *space* as the result of intuition based on sensation. Logicists therefore understood that the axioms of geometry are simply definitions and that therefore geometry is not about physical space. On the ground of this analysis, Ayer came to the conclusion that all genuine propositions are either empirical or analytical *a priori*. Empirical propositions, if valid, provide us with some information about matters of fact, while tautologies are entirely devoid of factual content. This, however, does not imply that analytical propositions do not give new knowledge: “They call attention to linguistic usages, of which we might otherwise not be conscious, and they reveal unsuspected implications in our assertions and beliefs”. It is however obvious that their deductive nature cannot but reveal what was already implied by the premises of the reasoning (Ayer 1936, Chapter IV).
Harrod fully accepted the neo-positivist tenet, which he had already expounded to Robertson in 1935\textsuperscript{11} (Harrod must therefore have discussed it with Ayer before the publication of *Language, Truth and Logic*\textsuperscript{12}):

You do not disturb me by the charge of tautology. A valid generalization, in my view, is either a tautology or based on empirical evidence. Unhappily the number of generalizations in economics based on empirical evidence is extremely small. And if you decry tautology, you are decrying almost the whole of economic theory\textsuperscript{13} (Harrod to Robertson, 15 Nov., 1935).

Harrod’s approach to the problem of induction and deduction was developed within the framework of thought which saw theories as deductive (analytical) systems founded on an initial set of axioms. These had to be derived either from empirical generalisations, or from a defining process; in the latter case, they would be nothing else than tautologies. The solution Harrod proposed pointed at a two-fold complementarity of induction and deduction. On the one hand, he attributed to the former the scope of providing the basic laws from which the deductive apparatus is developed\textsuperscript{14} and the appropriate equations are derived in qualitative form\textsuperscript{15}. On the other hand, he claimed that the precise functional relationships underlying these equations are to be found by way of empirical observation\textsuperscript{16}. Both aspects are worth examining, for the former provided the methodological foundation of his dynamic equations (this aspect will be discussed in the present Section) while the latter stimulated Keynes’s reply, which highlighted the mutual inconsistency of Harrod’s twofold interpretation of the role of models and their relationship with the object under consideration (§ 2 below).

In Harrod’s view, the process behind the physicists’ mapping of the world consisted in detecting some “hypothetical or, more strictly, imaginary” “physical properties” like mass or temperature, for instance. These are “strictly pure inventions”, for they do not immediately correspond to direct sensation but are measured by means of some device (e.g., a spring balance, or a thermometer) which is devised “by assuming on the basis of experience certain regularities of behaviour” (Harrod 1940/41*, Chapter 2, TS pp. 41-42). The accomplishment of physics lies in having succeeded in relating the facts to a comparatively small number of distinct kinds of physical entities and in showing them to be subject to a few simple laws. This triumph has depended upon great courage of mind in making simplified hypotheses; there has been steadfast confidence in the uniformity of nature, which has been repeatedly justified by experiment and observation (ibid., Chapter 4, TS)

Of course, such success implies the persistence and continuity in the physical entities:

The progress of our knowledge of the physical order has been closely connected with our success in showing the persistent existence of certain mensurabilia behind the changing kaleidoscope of appearances. The first was represented by the laws of conservation of energy etc., later we have had persistent protons and electrons. Where there is no underlying persistent entity our knowledge about the laws of succession is always very flimsy (ibid.).
The point of Harrod’s 1938 methodological essay seems to have been that economics may claim scientific status only as far as 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”. In the second place, the fundamental laws (simultaneous or causal) which constitute the ‘map’ are identified. 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 1938a, 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*, Chapter 2, TS, p. 42).

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

It is now possible to examine the scientific status of Harrod’s dynamics according to his own criterion. Harrod maintained “that a few basic empirical laws, of a generality not much inferior to that of the Law of Demand in statics, may yield, in connection with the study of mutual implications, an elaborate structure of deductive theory” (ibid., p. 404), and therefore presented his “Essay in Dynamic Theory” as built on an axiomatic basis:

The axiomatic basis of the theory which I propose to develop consists of three propositions -namely (1) that the level of a community's income is the most important determinant of its supply of saving; (2) that the rate of increase of its income is an important determinant of its demand for saving, and (3) that demand is equal to supply. It thus consists in a marriage of the “acceleration principle” and the “multiplier” (Harrod 1939a, p. 14).

Let me consider these in turn. The first axiom, being the multiplier reinterpreted as the saving function, was meant to be one of the “few basic empirical laws”:

An example of a basic empirical generalisation may be found in the proposition put forward by Mr. Keynes in his recent work, that at a given rate of interest people will save a larger absolute amount from a larger income. We could get still further if we could establish -but this is perhaps too audacious for the early stages- that people save a larger amount of a larger income. Both these propositions are clearly open to empirical verification. They will be subject to
*ceteris paribus* clauses regarding the distribution of income and institutional arrangements, but these would probably not impair their high scientific utility (Harrod 1938a, pp. 404-5).

As to the acceleration principle, the matter is more complicated. The strict proportionality between investment and increase of consumption is subject to a multitude of *ceteris paribus* clauses regarding the states of the technology and of confidence, the rate of interest and long-range plans; all these factors may affect the intensity of the accelerator in the course of the cycle. However, in *The Trade Cycle* Harrod insisted on the empirical character of the Relation in terms that were very similar to those mentioned above with reference to the multiplier:

> It has long been a matter of observation that in the upward phase of the cycle, activity in the trades producing durable or capital goods increases more rapidly than that in the trades producing currently consumable goods, and conversely in the downward phase. This fact has struck the notice of many writers and has been the basis of various theories. [...] The investigations confirm the generalization mentioned above, which may be regarded as well established. It is one which of its very nature commands thought and attention. Before letting loose the hounds of theory upon this appetizing morsel, it is well to mention a simple arithmetical relation existing between the demand for currently consumable goods and the demand for capital goods, which should be considered in close connexion with the generalization. [...] Its simplicity, ineluctability, and independence of all special theories as to the workings of the cyclical process demand for its pride of place (Harrod 1936a, pp. 53-54).

> The [Relation] in and by itself gives a reason why the activity of the capital goods industries might be expected to fluctuate more than that of the consumable goods industries, and may be considered in connexion with the generalization that in fact they do so, which is derived from experience (ibid., p. 56).

The third axiom is the most troublesome of the lot. On the one hand, it was meant to be one of the tautologies arising from the preliminary, cartographic stage of the mapping (Harrod 1939a, p. 18, and 1938a, p. 405). Saving = Investment in a sense is certainly a truism, an *analytical* identity, the equality being logically implied by the definitions of the terms involved (on Harrod’s struggle over the definition of saving, see Chapter III). On the other hand, Harrod also used this equality in different senses. In the first place, he thought of saving and investment as the result of decisions guided by different motives, so that Saving = Investment was used to reduce the degrees of freedom of Harrod’s system of three equations in three unknowns (saving, investment and income). Secondly, Saving = Investment (or, in the terminology of the passage cited above, “demand is equal to supply [for saving]”), in their *ex-ante* interpretation of the terms, also indicates an equilibrium condition. Thus, although *ex-post* investment is necessarily equal to saving, one of the components of *ex-post* investment -namely, the undesired change in the volume of stocks- is the engine of the cumulative process at the root of instability. The logical status of Harrod’s third axiom is thus quite ambiguous but his intention was clear. This emerges from the following assessment of the state of his theory with respect to deduction and induction:
You ask me the leading question whether my theory that there are centrifugal forces on each side of the equilibrium (warranted rate) is based on some empirical view as to the entrepreneurs’ probable reactions. I do not think so. On the other hand there must be some empirical basis for my theory. This may be summarised in 2 propositions. 1. The volume of saving supplied depends mainly on the size of income. 2. A considerable part of the demand for saving depends on the rate of growth. There, I think, empiricism ends and deduction begins (Harrod to Marschak, 7 Sept., 1938).

Harrod did not work out a quantitative model representing his deductive system of relationships, and was therefore conscious that his results could only indicate directions of change: the theory would not profess to determine the course of events in detail, but should provide a framework of concepts relevant to the study of change analogous to that provided by statics for the study of rest (Harrod 1939a, p. 14; emphasis added). Harrod was thus eager to emphasise that the “Essay” did not proceed beyond the stage of the formulation of the map, and we must therefore appreciate it as solely presenting “a tool of analysis, not a diagnose of present conditions” (ibid., p. 33). Incidentally it must be noted that Harrod consistently insisted on this point in his post-war writings on dynamics, especially when refusing to consider his theory as a fully developed analytical mechanism representing the actually dynamic processes of economic systems. His readers, however, equally insistently ignored Harrod’s warning, and treated his ‘fundamental equation’ as describing the growth process of capitalist economies (for a discussion of this point with reference to the post-war debates on Harrod’s dynamics, see Besomi 1996a).

2. Harrod and Keynes on Models and ‘the External World’

Harrod seemed to attribute a double task to scientific models. On the one hand, the map was called to ‘bring order to the chaos’ by organising the relevant concepts in a consistent framework. In other words, the model was conceived as an instrument of thought necessary for the interpretation of the apparent heterogeneity of the immediately perceived experiences. Accordingly, Harrod saw the preliminary formulation of his dynamics as providing “a method of thinking, a way of approach to certain problems” (Harrod 1939a, p. 15). On the other hand, Harrod thought that between the map and the world there may be (as an ideal of knowledge, at least) a complete isomorphism; the model was therefore called to provide a representation of “the external world”, and -as a corollary- to enable prediction.

Harrod thought these two roles as logically subsequent, and did not seem to pause on the implications of the respective conceptions. On the one hand, in fact, he pointed out that in the process of bringing order to the world, the data of experience are not taken rough and ready, but are organised by means of concepts which are nothing less than ‘pure inventions’, ‘fictions’ or ‘imaginary properties’. Modelling therefore provides a re-construction of reality, or more properly the invention of a reality by means of a new language or framework of concepts (‘invention’ here is used in the etymological sense...
of *invenire*: to find, to discover: see Prigogine and Stengers 1980 and Stengers 1983, pp. 72-73). For instance, when Harrod opposed his notion of the cycle as an intrinsically out-of-equilibrium process to Pigou’s interpretation of the cycle as deviations from the natural state of equilibrium brought forth by exogenous causes, he conceived the cycle as a different phenomenon, just as Galileo and cardinal Bellarmino perceived different phenomena when they were watching the same sunset. Therefore, while Harrod perceived the scientific process of modelling as taking part in the definition of the object to be understood, at the same time he treated the world as an ‘external’ entity, given once and for all independently of the observer, and did not therefore realise that he was referring to both the two traditionally antagonistic epistemological conceptions of the relationship between the knowing subject and the studied object.

When Harrod’s essay on “Scope and Method” came into Keynes’s hands, this contradiction did not pass unnoticed. In fact, Keynes remarked that “economics is a branch of logic, a way of thinking”, and criticised Harrod for not having repelled “sufficiently firmly attempts à la Schultz to turn it into a pseudo-natural science”:

it is of the essence of a model that one does not fill in real values for the variable functions. To do so would make it useless as a model. For as soon as this is done, the model loses its generality and its value as a mode of thought. […] The object of statistical study is not so much to fill in missing variables with a view to prediction, as to test the relevance and validity of the model (Keynes to Harrod, 4 July 1938, in Keynes *CW* XIV, p. 296).

Keynes, by his own process of thought, had at the time of reading Harrod’s presidential address already fully developed the implications of the notion of models as “an instrument of thought” (Keynes to Harrod, 16 July, 1938, in Keynes *CW* XIV, p. 299), and had reached the conclusion that the heterogeneity through time of the object of economics and its lack of uniformity and regularity oppose the nature of economic thinking to that of “the typical natural science” (Keynes *CW* XIV, p. 296 and 299). His criticism to Harrod did not therefore regard the heuristic quality of the model, but its rigidity derived from Harrod’s belief that scientific handling of facts is possible only as far as these show some persistence, i.e. as far as nature unveils some uniformity, which implied that axioms and maxims could be fixed once and for all:

One can make some quite worth while progress merely by using your axioms and maxims. But one cannot get very far except by devising new and improved models. This requires, as you say, ‘a vigilant observation of the actual working of our system’. Progress in economics consists almost entirely in a progressive improvement in the choice of models. […] Economics is a science of thinking in terms of models joined to the art of choosing models which are relevant to the contemporary world (Keynes to Harrod, 4 July 1938, in *CW* XIV, p. 296).

Keynes therefore opposed Harrod’s interpretation of the empirical generalisations, in terms of axioms giving rise to qualitative equations with parameters to be filled in, to the *art* of choosing new and improved models. Both men’s starting point presupposed ‘vigilant observation’. But while Harrod, like Tinbergen, aimed at getting the general laws and afterwards at specifying their functional form, Keynes did not rely on induction
as a mere procedure of empirical research. On the contrary, he interpreted it as providing the logical foundation for the scientific role of analogy (Carabelli 1988, Chapter 4). But analogy is a selective process, for it highlights certain features and neglects others:

Reasoning from analogy required a judgement through which some characteristics of instances were considered essential for the generalisation and others were not. It was only through such a judgement that it was possible to distinguish between positive and negative qualitative likeness of instances, that is, between what was essential in their likeness and what was inessential in their difference (ibid., p. 64).

The difficulty lying in ‘the art of choosing models which are relevant to the contemporary world’ thus translates Keynes’s awareness that the language (e.g., differential equations) used for asking questions to a specific object of analysis constrains the replies of the object within the limitations of that formalism. Keynes knew that a risk is incurred where judgements as to what is essential in the analogy point at the wrong properties, so that transferring a language from one domain to another may mutilate the object under consideration. Hence Keynes’s insistence that

The specialist in the manufacture of models will not be successful unless he is constantly correcting his judgement by intimate and messy acquaintance with the facts to which his model has to be applied (Keynes to Harrod, 16 July, 1938, in Keynes CW XIV, p. 300).

The selective character of analogy also implies that models cannot be in a one-to-one correspondence with the modelled object. The success of the analogy therefore does not lie in the similitude between objects, but in its capacity of revealing new features of the phenomenon under examination. Modelling by analogy thus constitutes the heuristic stage of economic thinking, for it is not meant to provide a representation of a ‘mysterious’ object by means of a known one, but rather to re-define, enlarge and enrich the domain of possible discourses on the object. In other words, models set out a verbal schematism (Schlanger 1983, pp. 186-190 and 1971, pp. 256-8), while an analogy suggests images and ideas, and proposes conceptual instruments for research (Ménard, 1980, p. 208, and 1981; see also Borutti 1991, Chapter 2).

Despite Keynes’s insistence, Harrod only conceded that the quantitative value of parameters needed re-estimating from time to time (Harrod to Keynes, 6 July, 1938, in Keynes CW XIV, p. 297) and that the most important task is that of getting a suitable model (ibid., p. 298). However, he skipped over Keynes’s point that inserting figures destroys the heuristic value of the model. This suggests that Harrod did not appreciate Keynes’s argument of the incompatibility of the two views of the cognitive role of models.

It is not clear whether Harrod later changed his mind, for the subject of the debate shifted from Harrod’s methodological essay to Keynes’s disagreement with Tinbergen. Harrod maintained that Keynes somewhat misdirected his attack, and suggested a different interpretation of Tinbergen’s aim and procedure. However, he did not explicitly
take a stand in favour or against the econometricians’ use of statistics. Nonetheless, in “Scope and Method of Economics” Harrod mentioned the econometricians’ lag theory of the cycle as an example of empirical research under way, and expressed the opinion that a theory of fluctuations aiming at showing that a time-lag may give rise to oscillation of behaviour offers the possibility of statistical verification at both ends:

On the one hand it may be possible to verify the particular lag assumed by reference to two statistical series. On the other the cycle mathematically deducible from the assumption of such a lag may be compared as to its general features with the real cycle (Harrod 1938a, p. 409).

Harrod concluded by hoping that by coupling the lag hypothesis with the framework of dynamic concepts he himself envisaged a few pages earlier in the same essay, it would be possible to obtain “a theory of the trade cycle, which would be self-consistent and consistent with the generalisations of theory, and also subject to fairly approximate empirical verification at both ends” (ibid.). In the light of this comment, Harrod’s discussion of Keynes’s views on Tinbergen does not seem to suggest any change of mind on his part regarding a possible contradiction between heuristic and representative conceptions of models. As a matter of fact, Harrod saw as a possible line of development of the framework of ideas outlined in his “Essay in Economic Dynamics” the filling in of the values of the parameters. In May 1939, in fact, 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*).

Such a development, however, never took place. The outbreak of the war, which distracted Harrod’s immediate interests from dynamics, was certainly one of the factors inducing him to renounce to pursue this way. However, in spite of having publicly hinted at the possibility of integration between his own dynamic framework and the econometricians’ procedure, Harrod was eager to stress the methodological gap between the respective conceptions of dynamics, with special reference to the necessity of the presence of some time-lag. Harrod’s urge to differentiate his position from the econometrician’s emerges from the report of the OERG meeting cited above, but also from several specific comments in the “Essay in Dynamic Theory” and in private correspondence. It is therefore necessary to turn to the relationship between Harrod’s dynamics and the contemporary ‘time-lag theories of the cycle’.

3. Instantaneous and sequential analysis: Harrod and the econometricians.

In §1 of this Chapter it was mentioned that Harrod distinguished two stages in the process of mapping, a simultaneous one and a subsequent one relating to the sequence of
events. It is important to notice that this distinction does not correspond to the demarcation of statics and dynamics, but points at different stages within both statics and dynamics. As regards the former stage, 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, from the empirically based Law of Demand the consequences concerning the price-output structure are deduced, in the form of general laws concerning the succession of events (Harrod 1938a, 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, from the simple laws of growth causal laws are deduced, which determine the succession of events constituting the trade cycle (Harrod 1938a, pp. 402-404, and 1939a). Finally, after 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 offer of policy advice. It is important to remark that the difference between statics and dynamics does not lie in their respective procedures, which are instead strictly equivalent. It consists instead, on the one hand, in the fact that in the former case changes are of a one-off nature, while in dynamics they occur continuously (Harrod 1938a, p. 404, and 1939a, p. 15; see also Chapter V, § 3). On the other hand, 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.

Correspondingly, Harrod’s “Essay in Dynamic Theory” in its original formulation (Harrod 1938*) presented a sharp division between three stages. 1) The determination of the dynamic equilibrium at a single 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, namely, the propensity to save and the acceleration coefficient, were taken as given. 2) The study of the succession of events occurring in the trade cycle. In the second stage, variations in the fundamental conditions not only were allowed, but played an important part in the argument. 3) Economic policies were discussed in the light of the preceding analysis, according to the relative positions of natural and normal rates of growth, although Harrod did not push his analysis beyond a qualitative discussion.

This division in stages is quite important for understanding Harrod’s attitude towards the approach of the econometricians. Harrod in fact thought “dynamics proper” to be confined to the ‘simultaneous conspect’ of the first of these stages, whose end he marked by the remark “The strictest part of [“dynamic”] theory ceases here” (Harrod 1938*, § 9). Harrod also stressed that trade cycle analysis is concerned with the
succession of phenomena and thus “involve[s] a certain element of conjecture” (Harrod 1938*, § 9). Both remarks have been deleted in the course of the revision of the text\(^{40}\).

Incidentally, it is interesting to notice that Harrod’s strategy of revision, under the pressure of Keynes’s attack, 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 pass practically intact through Keynes’s criticism.

The concluding paragraphs of the “Essay” were also not substantially altered by Harrod’s revision. There Harrod examined what policies would be suitable in case of a divergence between the natural and the normal (later ‘proper’: see Chapter VII, note 8) warranted rate, showing particular attention to the case he thought to be representative of modern conditions, that is, a normal rate higher than the natural rate. The choice of preserving these paragraphs practically intact probably reflected two of Harrod’s major concerns of practical policy in the pre-war years. On the one hand, in fact, Harrod saw the problem of the decline of the growth rate of the population (that is, of one of the components of the natural growth rate of the economy) as “one of great urgency (some would say the most serious which confronts the present generation)”, and devoted many energies in advocating for policies tending to stimulate fertility. Harrod published in fact a number of articles on the subject in learned journals (Harrod 1938b, 1939d and 1939e) and in newspapers (1934f, 1936d, 1937c, 1937d, 1937e), and also dealt with it in correspondence with fellow economists (in particular Joan Robinson, James Meade and Dennis Robertson) and others\(^{41}\). On the other hand, shortly after finishing the first draft of the “Essay”, Harrod engaged in a controversy on the pages of The Economist, The Times and The Financial Times on the appropriate policy to face the 1937-38 recession (Harrod 1938c-g, k, l). The publication of the “Essay” was therefore an occasion to provide a theoretical ground for his diagnosis presented on the basis of an intuitive reasoning.

Keynes’s “editorial preference for something short” and Harrod’s own concerns about the length of his paper thus wreaked their damage upon the central part of the “Essay”, that relating to the trade cycle\(^{42}\). The consequence of the deletions have been most unfortunate, because for many years it was not understood that Harrod’s dynamics was elaborated in view of a theoretical treatment of the business cycle, and when this was finally appreciated the richness of his original analysis escaped the commentators.

The second aspect of importance of Harrod’s distinction between the two stages of analysis for understanding his attitude towards the econometricians lies in the fact that Harrod interpreted Tinbergen’s results as depending on the presence of lags. In Harrod’s view, the econometricians proceeded by showing that the introduction of lags in the process of adjustment to a new static position in an otherwise smoothly working system may give rise to oscillations\(^{43}\). He therefore saw the respective conceptions of dynamics
as clashing in several respects, although he thought that time lags could be grafted on to his own second stage.

In the first place, the econometricians’ approach does not necessarily regard growth, which was instead 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 1938*, § 2, and 1939a, pp. 14-15).

Harrod therefore concluded in a seemingly conciliatory way, which however implied the logical priority of growth with respect to cycles and of the study of the forces determining growth with respect to lags:

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 1938*, § 2, and 1939a, p. 15).

In the second place, accordingly, Harrod maintained that the study of lags must be logically and chronologically subsequent to the determination of the growth equation:

(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. Moreover the lag referred to in this sub-heading (b) may properly be regarded as unimportant, since, in the event of a steady advance, $G$, being maintained, the difference between $x_1 - x_0$ and $x_2 - x_1$ will be of the second order of magnitude. In other words, it matters not whether we regard the increment of capital as required to support the increment of total output in the same period or in the one immediately succeeding it (Harrod 1938*, § 7, and 1939a, p. 20).

Harrod stressed the point more forcefully in a passage which was omitted from the published version of the “Essay”. After 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 1938*, § 19).

Harrod had already stressed the point forcefully in his review to Lundberg’s *Studies in the Theory of Economic Expansion*, which turned out to be an attack to the method of sequential 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 1937f, p. 496-7).

Analogously, commenting on Tinbergen’s review of his book Harrod stressed that his “idea is that my curve gives the basic fact of the cycle on which various lags must be superimposed”, while he opposed Tinbergen’s approach as failing to depict the fundamental explanation of the cycle:

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).

Finally, 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 dont think we are yet in that position. I think J.M.K. is right to suppose that we need a further clearing up of the general theory, but he has not gone far enough (Harrod to Robertson, 3 Aug., 1938).

In the light of these comments, it seems that Harrod had not changed his view as expressed a few years before in connection with *The Trade Cycle*, when he treated time-lags on the same epistemic ground as frictions or disturbances, which had to be ignored in the formulation of ‘dynamics proper’ and could only be accounted for in a subsequent
stage of analysis (see Chapter V §. 3). At the time of writing his book, Harrod was more concerned with Robertson’s sequence analysis than with the econometricians’. However, when he discussed the matter with Keynes Harrod suggested the interpretation that Tinbergen’s “time-lag theory of the cycle”

is really only doing systematically and with the help of a sine curve what Dennis [Robertson] 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 method Dennis uses (Harrod to Keynes, 18 Sept., 1938, in Keynes CW XIV, pp. 304-5).

To sum up, Harrod’s remarks on the econometricians’ viewpoint on cycle theory as compared to his own reveal that he was more concerned with the very concept of dynamics, rather than with details of the procedure. In fact he did not question that lags play some role in determining the features of oscillations, but while -in his understanding- the econometricians saw them as the ultimate causes of fluctuations, he interpreted them as secondary causes or disturbances to the operation of the dynamic forces which determine the rate of growth of the system. In Harrod’s view, the fundamental explanation of the cycle lies in the instability of the moving equilibrium, which determines the possibility -and ultimately the necessity- of the oscillations. Therefore, while Ragnar Frisch defined dynamics with respect to the capacity of the analytical instruments to incorporate reference to different instants of time and thereby to describe the succession of states of the system through time, Harrod proposed his own 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 1938a, p. 403). In fact, after having formulated the “Essay”’s ‘Fundamental Equation’, Harrod observed:

It should be noticed that the warranted rate of growth of the system appears here as an unknown term the value of which is determined by certain “fundamental conditions”, namely the propensity to save, the state of technology, etc. Those who define dynamic as having a cross reference to two points of time will not regard this equation as dynamic; that particular definition of dynamic has its own interest and field of reference. I prefer to define dynamic as referring to propositions in which a rate of growth appears as an unknown variable. This equation is clearly more fundamental than those expressing lags of adjustment (Harrod 1938*, § 4; see also 1939a, p. 17).

Harrod’s definition therefore opposed the econometricians’ on two grounds. In the first place, it was formulated with regard to the object of analysis (growth) rather than to the instrument of analysis (functional equations), for the scope of the dynamic equations was to determine the rates of growth of the system. In the second place, the ‘fundamental equation’ on which the notion of dynamics was based required that the ‘fundamental conditions’ -viz., the propensity to save and the acceleration coefficient- were given. But since growth itself induces a change in the fundamental conditions, the
time interval in which the equation is valid must be short enough not to allow such changes to occur. Rigorously, this can be obtained only by reducing the analysis to a single instant. Here lies the origin of Harrod’s urge to contrast his instantaneous analysis to Frisch’s “cross-reference to two points of time”, and to qualify in this latter respect his approach as “clearly more fundamental” than that of the econometricians.

Harrod’s insistence on the instantaneous character of his analysis is a key point for understanding not only his aversion to the time-lags theories of the cycle, but also his ‘positive’ dynamics as developed in the “Essay”. This aspect will therefore be discussed in detail in the next Chapter, dedicated to the evolution of Harrod’s ideas from *The Trade Cycle* to the “Essay in Dynamic Theory”.

Notes

1 As Robbins remarked in the 1935 preface to the second edition of his *Essay*, the debate on his book was centred around his “denial […] of the scientific legitimacy of interpersonal comparisons of utility” (p. vii). Robbins’s attack was directed against the opinion that a redistribution of income in favour of the lowest income receivers is ‘economically justified’ (ibid., p. 137), and it is therefore not surprising that the matter raised inflamed discussions (see e.g. Durbin 1985, pp. 199-200). In this connection, Harrod argued that the common sense finding that the marginal utility of a penny is higher for a beggar than for a millionaire, though not scientifically warranted for lack of a possible test is however not less legitimate than other widely accepted propositions of classical economics. For instance, the maxim of free trade is subject to the same limitations, “for the individuals who gain from the opening of a market are often different from those who suffer some loss”. Harrod thus concluded that

\[
\text{This objection would be very weighty if economics itself were a mature and exact science. Yet in fact its achievements outside a limited field are so beset on every side by matters which only admit of conjecture that it is possibly rather ridiculous for an economist to take such an high line} \\
\text{(Harrod 1938a, p. 396).}
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As we shall soon see, the purpose of “Scope and Method of Economics” was precisely that of specifying the extent to which economics can be considered as a science, with special regard for the theory of distribution of income and for dynamics.

2 A first draft of “On Scope and Method” was ready by the end of May, 1938, for Harrod received on May 25 and on May 31 two letters (from Phelps Brown and from Hall, respectively) discussing some specific points. On June 13 Harrod announced to Keynes that a 12,000 words version for the *Economic Journal* was going to be ready in one week, but Keynes asked him to shorten it if possible. On June 16, Harrod complained that “keeping the presidential address down in length has been an awful job”, but promised that he would have done his best (Harrod to Keynes, 13 and 16 June; Keynes to Harrod, 15 June, 1938). The final version was published in the September issue of the *Economic Journal*. In his 13 June letter, Harrod also told Keynes he had another piece -the “Essay”- on the stocks, which “is concerned with the fundamental dynamic equation (law of growth) which I think is now formulated as neatly as may be together with some explanation about definitions of saving & investment etc.”. The first draft of the article was ready by the 3rd of August, 1938. For a full chronology of the events relating to the “Essay”, see Chapter VII § 1.

3 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).

Only 14 Chapters survive, three of which 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:
In the introductory paragraphs of his Address, Harrod paid tribute to John Neville Keynes’s *Scope and Method of Political Economy*, where both inductive and deductive methods in economics were carefully discussed.

In spite of having been written a couple of years after the methodological reflections which eventually contributed to shape the structure of the “Essay”, the surviving Chapters of the manuscript show a remarkable continuity with Harrod’s thought in 1938. This is also certified to by the fact that Harrod used certain expressions that we can find in “Scope and Method” (in particular, he referred to the metaphor of the ‘map’), and indicates that *The Known and the Unknown* constituted a widening and a deepening of Harrod’s original study. For this reason, and in spite of the chronological gap, in this Section I will illustrate the implications of Harrod’s 1938 reasoning with the help of some statement drawn from the 1940/41 manuscript. It seems to me that in this particular case the use of asynchronous sources is not misleading. Harrod’s theoretical work, in fact, was practically completely suspended after the writing and revising of the “Essay” (September 1938). The later reflections therefore resumed the thread which was interrupted two years earlier, taking the “Essay” and “Scope and Method” as the starting point. The reader, however, is required to keep in mind the existence of a chronological gap.

The cartographic analogy is likely to have been suggested to Harrod by F. A. Lindemann (Physicist and member of Christ Church), who used it himself to expound 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 was found among Harrod’s papers (HP V-121). It is likely that Harrod and Lindemann discussed this and other topics concerning the physics of quanta in their frequent night meetings that Harrod described in his biography of *The Prof.* (Harrod 1959).

Harrod did not specify the polemical target of his methodological essay, but Robbins’s attack to the ultra-inductivism of the American Institutionalists (Robbins 1935, V.3) may not be extraneous to it (Robbins’s 1932 *Essay* -the second edition was published in 1935- is indeed the most frequently quoted reference in Harrod’s presidential address, although Harrod qualified his differences from “Professor Robbins’ brilliant essay” as regarding “certain matters of emphasis”: Harrod 1938a, p. 384).
9 Harrod repeatedly complained of the state of philosophy in Oxford in the pre-war years: see e.g. Harrod 1951, p. 321, 1956, pp. ix-x, and 1959, p. 63.

10 Like Harrod, Ayer was a Student of Christ Church; they had therefore many occasions of personal interchange of ideas. Among the Harrod papers some letters from Ayer are preserved, dating from 1933; two of them in particular, dated 2 and 26 December, 1933, explicitly discuss the truth value of propositions; unfortunately Harrod’s letters to Ayer did not survive. Harrod and Ramsey frequently met in the late 1920s until Ramsey’s premature death, and also had some exchange of views in writing (Harrod preserved Ramsey’s letters, but there is no trace of letters by Harrod among Ramsey’s papers). These concerned economics, but as regards to philosophical matters Harrod recollected having met Ramsey in occasion of his 1922 stay in Cambridge and sharing his contempt on philosophy at Oxford and trusting his opinion on the development of the logicist position as an alternative (Harrod 1951, p. 321). For a more detailed description of these interchanges, see Introduction § 4.

11 Although in a less explicit form, Harrod was almost certainly referring to this view in his 1934 review of Pigou’s Theory of Unemployment, where he countered the “erroneous and harmful” opinion that the old Wage Fund Theory was “disproved and superseded by the marginal productivity theory” on the ground that “These old theories are firmly founded on tautology, and may well prove […] to be most useful analytical tools” (Harrod 1934, p. 21). Again, in his review of the “Doctrines of Imperfect Competition”, Harrod maintained that downward sloping demand curves are “a far more widespread [phenomenon] than has been commonly supposed or implied, and the chief basis of this claim is observation”. Furthermore he commented that “This is a notable instance of the assistance which the inductive method may give to the course of a priori reasoning” (Harrod 1934e, p. 443). Later on in the same year, Harrod returned to the subject in the course of his debate with Robertson on credit policy (see Chapter III § 2 above). Robertson had characterised the saving = investment proposition as “the Grand Monetary Tautology” (Robertson 1934a), drawing from Harrod the reply that “Tautology has played a notable and useful part in economic theory” (Harrod 1934d, p. 477). In neither of these occasions, however, Harrod discussed the premises and the implications of the view to which he was referring to. This task was taken up by Hutchison 1934-35.

12 Ayer was at Christ Church from 1933 on (see Introduction, § 4). He recollected that he had given Harrod two of the central Chapters of his book in typescript, to be handed onto Whitehead (Ayer 1978, p. 162; Ayer to Harrod, 20 December, 1934). Harrod expressed a good opinion of these two Chapters (Ayer to Harrod, 14 Jan., 1935), but seems to have read the whole book only at the end of January, 1936 (Harrod to Moore, 29 January, 1936).

13 Harrod later qualified his early position with the following words:

[The word ‘tautology’] came into favour in my youth in consequence of that movement in logic which discarded the possibility of a priori synthetic judgements and divided all statements into tautologies (or deductions therefrom in accordance with arbitrary rules) and those susceptible to empirical test. This movement was due as much to the downfall of Euclidean geometry as to the cogitations of symbolic logicians (Harrod 1968, pp. 183–4).

14 A similar view was already proposed in The Trade Cycle, but in a less articulated form. Harrod, in fact, had explained the “change of method” he introduced when discussing the de-stabilising power of the fourth static determinant (see above, Chapter V, §. 7) as a shift from a priori statements to an empirical generalisation: after having observed that ‘the fundamental facts of human nature’ cannot provide a reason for prices increasing along with activity, he concluded that

When the a priori yields nothing, it may be well to revert to observed facts. […]

The behaviour of the trade cycle, perverse and obstinate as it is, has provided a great stimulus to those who hope to arrive at general economic laws by observation of the facts. And the trade cycle phenomena have also suggested conclusions to some who, on principle, would scorn such empiricism. The repeated recurrence of certain phases gives a scope for the use of inductive methods otherwise rare in the chop and change of human affairs. It must not be supposed that anything very precise and accurate has been achieved on these lines; only a few rough and tentative generalizations have so far been possible. But nothing in economics is likely to be more fruitful than the work of such as Professor Wesley Mitchell who are making strenuous efforts to extract further empirical generalizations from the observation of trade cycle phenomena (Harrod 1936a, p. 38; for a further reference to Mitchell in this connection, see 1937g, pp. 87-89, and 1949).

However, in spite of these precise hints Harrod did not enter into a systematic discussion of these topics.

15 Harrod polemized against the claim of “rigid demonstrability and certainty, of an almost geometric kind” for the laws concerning the succession of phenomena (Harrod 1938a, p. 386), probably on the
ground that analytical statements cannot provide any additional information besides what is already contained in the axioms (although they can enable one to overcome the limits of non-formal languages and reveal unsuspected implications of the system of axioms), and therefore are empty of empirical content (see e.g. Ayer 1936, Chapter IV). This must be sought in the axioms themselves, in spite of the possible margin of error, otherwise economics would be reduced to “an exercise in pure geometry, [...] harmless so long as it was kept quite pure and did not claim to represent what would happen in the real world” (Harrod 1936a, p. 127); “if empirical evidence is lacking, the proposition can be no more than a definition of the terms which it employs” (Harrod 1938a, p. 406). Therefore “Only by abandoning the theological claim to certainty, and explicitly allowing a wide margin of error, can economics rebut the charge of scholasticism and claim scientific status” (Harrod 1938a, p. 401). Additional polemical remarks on economics as more akin to scholastics than to science can be found in Harrod’s letter to Durbin of 7 Feb., 1936, in 1937g, p. 88, and in The Trade Cycle:

[the theorist] may pride himself on the demonstrative character of his reasonings and the assured certainty of his conclusions. But their assured certainty will be spurious if they have material content. It is that hallmark of science -of science, that is, actual and realized, not the science of Aristotelian dreams- that its conclusions do not have demonstrable certainty. Those theorists who seek to make economics more scientific by eschewing the uncertainties, which are necessarily attached to empirical methods, are in fact taking the path which leads away from science to pure scholastic (1936a, p. 39).

16 It may be noted that Harrod’s 1938 solution substantially differs from the approach outlined in Foundations of Inductive Logic, where Harrod argued that the principles of induction come into play at the moment of establishing the truth of the hypotheses on which deduction is based (Harrod 1956, p. 6):

It must be emphasised that inductive logic does not purport to consist of a set of rules for suggesting hypotheses; its rules, if and when we can formulate them, should determine whether all the evidence, including the so-called verifications, tends, and in what degree, to establish the truth of the hypothesis. In the forming of hypotheses we are free; the acid test of logic comes when we collect all available evidence and assess its bearing on the probability of the hypothesis (ibid., p. 8).

17 In his discussion of whether our knowledge with regard to causal sequences has inductive or deductive origins, Harrod strongly supported the former, though not excluding that clues can be found deductively:

On the one hand there are those -I believe that it is fair so to represent the view of Professor Wesley Mitchell- who believed that clues are most likely to be obtained by the diligent scrutiny, arrangement and rearrangement of empirical data. The facts will one day speak for themselves. By patient and continuous observation, the investigators will find the appropriate generalisation borne in upon him. On the other hand, some believed that clues are more likely to be found by an inspection of the existing body of theory. Close examination of it will reveal gaps, and in those very gaps may be found clues suggesting new generalisations which will render the theory more coherent, or even wide generalisations leading to a revolution of the kind which occur from time to time in physics. Or, more moderately, they may lay stress on observation, but urge that this should be done very much in the light of existing theory, to test hypotheses directly suggested by that theory.

Both schools must be given our cordial blessing (Harrod 1938a, pp. 406-407).

However,

Two circumstances militate against the more deductive method. One is the impossibility of the crucial experiment. In the mature science which rely mainly on this method, such as physics, or, to name a more recent comer, genetics, the crucial experiment is of central importance. Secondly, it is extremely difficult to test hypotheses by the collected data of observation. The operation of the plurality of causes is too widely pervasive. Thus numerous hypotheses are framed, and never submitted to decisive tests, so that each man retains his own opinion still (ibid.).

The example of an inductive generalisation that Harrod mentioned is important for our understanding of the ‘change of method’ he introduced in The Trade Cycle in order to qualify the behaviour of the monetary determinant (the general level of prices) as destabilising (see Chapter V § 6, and note 14 above):
It is an accepted generalisation, not indeed possessing the universal validity of the Law of Demand, but none the less of substantial authority and interest, that in the upswing of production prices have a rising tendency and in the downswing a falling tendency. It may safely be said that this could not be deduced from the propositions of static theory nor from that part of monetary theory, which is deducible from them. Falling prices would be regarded as an equally (if not more!) likely accompaniment of rising output, and vice versa. The generalisation is a direct result of observation, an excellent example of the facts speaking for themselves. And if theoretical explanations have subsequently been woven round it, this must not blind us to the true source of our knowledge (ibid., p. 408).

18 On Harrod’s attitude towards the problems of measurement, see Chapter V note 51.

19 Harrod returned on both these themes while commenting on the Oxford Economists’ Research Group’s empirical inquiry on “Price and Cost in Entrepreneurs’ Policy”. In his discussion of the scope of the inquiry, Harrod compared again the possibility of resorting to analytical and to empirical methods:

It is important that economists should have clear ideas as to the scope within which empirical inquiries may be useful, for otherwise immense time may be wasted. What is validly vouchsafed by the deductive method is in no need of verification. For instance, a correctly drawn formula for the ‘quantity theory of money’ is incapable of being further fortified by statistical inquiries. The empirical method is appropriate in the case of questions to which pure analysis cannot provide an answer (Harrod 1939b, p. 1).

Commenting on the impossibility of assuming perfect knowledge on the part of entrepreneurs, and on the consequent necessity for the theorist to “take refuge in a doctrine of probability”, Harrod maintained that the method “would be quite acceptable”, provided that it could be assumed that no systematic distortions operate (ibid., pp. 1-2). Such considerations were already hinted at in 1937, when Harrod commented on the first results of the interviews with entrepreneurs and on the premises of such inquiry:

The principal objection to this a priori analysis is that it assumes knowledge where there is in fact ignorance. The entrepreneur is often largely ignorant about the value and elasticity of the demand curves for his goods here and now and a fortiori about their prospective values and elasticity (Harrod 1937*).

20 The idea of filling in blank forms was discussed at length with Keynes, also in connection with Tinbergen’s procedure, and will be examined in Section 2 below.

21 In the “Essay” Harrod observed

that the instability theory makes the empirical verification of the acceleration principle more arduous. For it leads to the expectation that in the upward phase of the cycle the actual rate will tend to run above the warranted rate, and the accretion of capital to be less than that required by the acceleration principle; and conversely in the downward phase. Thus a finding that the volume of investment fluctuates less than is required by direct computation from the acceleration principle is consistent with the theory here set forth, in which, none the less, the acceleration principle is presented as a leading dynamic determinant (Harrod 1939a, p. 26).

22 I shall return to this point in Chapter VIII § 6.

23 Such a logical tangle, however, was shared by most participants to the saving-investment debate in the 1930s; for a discussion see Dickson 1960 and Andvig 1991.

24 Reflecting on the state of his dynamics after the break due to the war, Harrod expressed himself in the following terms in a letter to Robertson:

I am afraid my dynamics wouldn't be very scientific in the proper sense. I am still thinking rather in terms of a geometry, of a system of relations which can be defined by pure thought. I am not altogether pessimistic about the Tinbergen type of empiricism, but that is beyond me. What I have in mind is something much simpler (Harrod to Robertson, 20 March, 1945).

25 See the passage cited in Section 1 above on the possibility of making a complete map of the world, and Lindemann’s notion of the mapping process as quoted in note 8 above.

26 This is the title of Chapter 2 of Harrod 1940/41*.
One of the recurrent themes of Book I of Harrod 1940/41* is in fact the “heterogeneity of fact and experience”, i.e. of events subject to a certain uniformity in nature and therefore capable of being related to a small number of physical entities and amenable to scientific treatment, as opposed to the kaleidoscopic world of our experiences -by this term Harrod meant our subjective way of perceiving the facts-, continuously broken up by acts of volition.

This should not suggest that the scientific process of discovery is seen as a creation that is free of constraints. On the contrary, Prigogine and Stengers stress that invenire is associated to the uncovering of hidden archaeological treasures, the search of which is far from being guideless but is directed by some text or tradition or miracle: “invention is a response. But when something is actually discovered, even if only some bones, the response introduces a novelty in the cultural environment which originated it: something has been ‘found’, a chapel is built, a new domain of facts, of problems and of relationships opens for archaeology. A new story begins, which superimposes to, cohabits with, or opposes to, the preceding ones” (Prigogine and Stengers 1980, p. 6). Invention is therefore a process deeply rooted, in both its antecedents and consequences, in the cultural memory of the scientific community. For a more detailed discussion of this problem see Besomi 1991.

On Harrod’s criticism to Pigou see Chapter I § 3; for the implications on Harrod’s dynamics see Chapter V § 3 and Chapter VIII § 6.

As another well-known instance, one may mention that astronomical entities like the Earth, the Moon and Venus changed their status in the transition from the Ptolemaic to the heliocentric theories: see e.g. Kuhn 1979, p. 416.

One of these conceives knowledge as independent from the researcher and the process of observation (for a discussion of this notion, see e.g. Heidegger 1950 and Stengers 1983), while the other emphasises the view that facts are imbued with theory and that science imposes its categories on reality (for a discussion see e.g. Stengers 1985). For a comparison of the historiographical views rooted in these conceptions see Besomi 1991 and 1992.

Keynes was referring to the classical conception of science, which was still prevalent at the time of his writing.

Recently, however, physics and other natural sciences had to recognise the singularity and specificity of matter and had to abandon the project of reducing nature to the hidden simplicity of a handful of universal laws. Moreover, relativity and quantum theories have forced us to reconsider the role of the observer, who can no longer be assumed to be extraneous to his observation. Accordingly, new languages and new formalisms have been found to be necessary to account for the complexity of the physical, chemical and biological worlds, and correspondingly alternative epistemologies have been developed (obvious references are to the reflections associated to the notions of dissipative structures, catastrophe theory -René Thom’s Structural Stability and Morphogenesis was significantly sub-titled An Outline of a General Theory of Models-, autopoiesis, chaos etc. For a first and very partial account of the epistemological developments which followed, see Livingston 1984, and more specifically Prigogine and Stengers 1984). Keynes’s aversion to formal treatment as well reflected the relatively poor state of mathematical modelling of his times, while today’s formalism is capable of a certain flexibility. It is therefore probable to guess that Keynes would have looked with interest at, and possibly also approved of, these developments, provided that the use of mathematics were not disconnected from the appropriate reflections as to the nature of the knowledge so developed (for a similar opinion see Carabelli 1988, p. 281, footnote 15). Keynes’s appraisal of Newton as the last of the magicians against the positivist myth of Newton as the ‘pure’ scientist, points in the same direction (see the essays on Newton in Keynes CW IV, and for a further comment Carabelli 1988, pp. 108-110).

Keynes was eager to stress that he did not expect Harrod to differ much from his own reflections: see CW XIV, pp. 297 and 299.

It must be noted however that Harrod -as against Mill- did not believe in the full uniformity of nature, but only admitted that nature has in the past shown a remarkable degree of uniformity. In other words, he did not claim too much for induction:

it must be remembered that there is no reason in logic for supposing that the world of science, the beautiful symmetrical universe with its simple laws, will remain with us for more than a little longer […] To suppose that they will hold eternally would be fallacious as well as presumptuous; it would involve dethroning the inductive principle on which the propositions of science depend (Harrod 1940/41*, Chapter 4).

Nowadays it is becoming increasingly clear that modelling complex systems implies the articulation of local viewpoints, and therefore imposes the question as to the validity of the researcher’s judgement as to which configurations are of interest and which ones can be discarded in view of the specificity of the system and of the problem under consideration. By contrast, to-day it is no longer possible to rely on a
general method of approach. It is quite remarkable that the wording of eminent contemporary scientists and mathematicians, when specifically reflecting on the status of models in scientific thought, strictly coincides with Keynes’s characterisation of modelling as an art. According to Prigogine, modelling a system implies a decision as to the kind of its singularities which are of interest for the researcher, and therefore implies risky decisions as to the features which are ignored: “the soundness of this choice depends on the circumstances. Modelling implies the adoption of a simplifying procedure, but this is an art, not a method: simplification is necessary, but the actual procedure must be invented according to the circumstances. Modelling cannot be an abstract and general method of procedure, but requires a concrete familiarity with the problem” (Prigogine and Stengers 1981, p. 727; see also 1984, pp. 203-207). Thom expressed himself along similar lines: “these methods, too indeterminate in themselves, lead not to a once-and-for-all explicit standard technique, but rather to an art of models” (Thom 1975, p. 323; see also Thom 1983, p. 22). On the shift from the representative to the hermeneutic interpretation of models see Borutti 1991, in particular Chapter 2.

36 Here originates Keynes’s “aversion to superfluous formalism in economics unless it is quite clear that it is required for the accurate statement of the ideas involved” (Keynes to MacDougall [copy for the attention of Harrod], 2 July, 1936). In the letter to Harrod accompanying the copy of the above, Keynes specified that the nature of economic materials is not suitable for mathematical treatment:

I feel increasingly that one cannot think as an economist unless one’s method of thought is capable of handling material which is not completely clear-cut and which is, so to speak, symptomatic thinking, (I do not know if that quite expresses what I mean) rather than completely formal, water-tight thinking. What one hopes from people like Champernowne and MacDougall is that they may learn to be mathematicians and economists simultaneously, capable of keeping in their minds at the same time formal thinking and shifting uncertain material (Keynes to Harrod, 2 July, 1936).

Keynes later repeatedly emphasised that “one has to be constantly on guard against treating the material as constant and homogeneous”: see in particular his 1938 correspondence with Harrod on the latter’s methodological essay (Keynes CW XIV pp. 300, 296).

37 Harrod’s later writings on the matter tended to emphasise the verificationist role of induction (Harrod 1956), while in a 1968 essay on “What is a Model” Harrod mentioned the notion of model as “a substitute for the words ‘general proposition’” (Harrod 1968, p. 173), but readily specified that he meant “a formulation that has adjustable parameters” (ibid., p. 174; see also p. 190).

38 Harrod forcefully stressed the analogy between the procedures of statics and dynamics in several occasions: see e.g. Harrod 1938a, pp. 403-4, and 1939a, p. 14. For a discussion see Chapter VIII, § 7.

39 This division was somewhat concealed in the published version, which Harrod considerably altered under the pressure of Keynes’s criticism (two of these stages, however, were clearly recognised by Kregel 1980, pp. 114 and passim). The Harrod-Keynes debate will be discussed in detail in this light in Chapter VII § 6. For a collation of the draft and the published version of the “Essay”, and for a more extensive comment on Harrod’s distinction of the three stages, see respectively the editorial footnotes to Harrod 1996, and Besomi 1996; I will however return to this subject in Chapter VII § 8.

40 It is worth remarking that the threefold division of analysis is also reflected in the different degrees of fluency in which Harrod actually wrote. The first and the last paragraphs of the first draft of the “Essay” only show minor signs of changes of mind, all of them regarding rephrasing and verbal adjustments. On the contrary, the central part is quite messy, with frequent additions, deletions and insertion (also insertions within pervious insertions) of new and long portions of text (sometimes entire paragraphs). The applications of the instruments of dynamic analysis to the trade cycle must therefore have been quite painful for Harrod; on the other hand, it seems clear that he already had in mind the general structure of the argument.

41 The extent of Harrod’s preoccupatio for the population question is certified by the impressive mass of press cut-outs and correspondence preserved among Harrod’s Papers at Chiba University of Commerce and at the British Library.

42 The choice of sacrificing these Sections undoubtedly also reflected the confusion surrounding Harrod’s notion of the normal value of the warranted growth rate, which was pointed out by Marschak and Keynes: see Besomi 1996.

43 Harrod to Keynes, 7 and 13 Sept., 1938, in Keynes CW XIV, pp. 303-5, and 1938a, p. 402. It would seem that in spite of a reference to Kalecki, Harrod was thinking of a cobweb-like mechanism.

44 Ragnar Frisch defined “an essentially dynamic theory” as
a theory that 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 (Frisch 1933, p. 171; see also Frisch 1936, p. 100).

This notion of dynamics underlay the oscillation models of the econometricians (e.g., Kalecki adopted Frisch definition explicitly in 1935, p. 327), and in the end it was (and still is) widely adopted (for a survey of the notions of statics and dynamics see Machlup 1959). The definition implies that dynamics is characterised by the presence of a functional equations defining the law of succession from one state of the system to the successive. Different sorts of equations satisfy such requirement: difference, mixed difference-differential, integral, integro-differential equations and others (see e.g. Samuelson 1947, Chapter IX). In correspondence with Harrod, in 1938 Marschak commented upon the passage of the “Essay” where Harrod distinguished his notion from the approach of the econometricians by noticing that differential equations as well are among the functional equations that satisfy Frisch’s requirement:

I entirely agree with you that dynamics is not identical with Theory of Lag’s. 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 is a little too polemic: “those who define dynamics 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*)

It is interesting to notice that Marschak’s comment opened the line of interpretation of Harrod’s fundamental equation as a special case of Frisch’s dynamics, which ignored the epistemic foundation of Harrod’s need to distinguish between the two approaches. But more on this in Chapter VII § 8.
Chapter VII
From a Dynamics of Causes to a Dynamics of Effects: The “Essay in Dynamic Theory”

The two years that intervened between the publication of The Trade Cycle and the writing of the first draft of the “Essay in Dynamic Theory” saw quite a radical evolution of Harrod’s notion of dynamics and of his growth and cycle model. In truth, Harrod claimed continuity between his two works: both in the foreword of his 1936 book and in the first lines of his 1939 article on economic dynamics, Harrod maintained that his theory was based on two principles: “it [...] consists in a marriage of the ‘acceleration principle’ and the ‘multiplier’ theory”. The “Essay”, according to Harrod, was meant to provide “a development and extension of certain arguments advanced in [his] Essay on the Trade Cycle”1 (Harrod 1936a, p. vii, and 1939a, p. 14).

There was indeed an important idea which persisted in the transition from the Trade Cycle to the “Essay”: in both cases, the trade cycle mechanism and the very conception of dynamics were organised around the principle of the instability of the equilibrium movement consisting of steady growth (this epistemic thread unifying Harrod’s notion of dynamics will be discussed in detail in Chapter VIII). But in spite of the two versions of Harrod’s theory being based on the common foundation of the instability principle, and of their using the same analytical pieces, Harrod introduced such a shift of emphasis that the analytical structure, the working of the model, the place the accelerator played in it, and more importantly the definition of dynamics itself, underwent drastic re-formulation, which is the purpose of this Chapter to understand in its historical context.

Fortunately most stages of elaboration of the “Essay” survived, and can provide interesting insights into the development of Harrod’s ideas. In February 1937 Harrod discussed with Keynes some notes on The Trade Cycle, and in particular a formula, suggested by Keynes, representing the rate of growth of capital in terms of the values of the average multiplier and the accelerator. This “fundamental equation” greatly impressed Harrod, and was certainly one of the major sources influencing the elaboration of his 1938-39 “fundamental equation”, to which it also bears a strict formal analogy. The starting point of this Chapter will therefore be a comparison of Harrod’s notion of the accelerator in The Trade Cycle with Keynes’s interpretation of his formula on the one hand, and with the “Essay”’s equation on the other. This will reveal how Harrod’s analytical set-up evolved from a qualitative to a quantitative discussion of growth, in which the independent coefficients condensing the values of the multiplier and the accelerator substituted for the interaction of the multiplier and the Relation. This aspect will be examined in Section 3. In Section 4 the comparison will be continued,
concentrating on the role of the accelerator coefficient in Keynes’s equation and in
Harrod’s two formulations. It will emerge that Harrod substituted the Trade Cycle’s
notion of dynamics as the study of the forces determining movement with a notion from
which all determination processes disappeared, to be replaced by the study of the effects
of saving and investment decisions whose causes were not discussed.

In the following Sections, these aspects are taken up again, this time in the light of
Harrod’s August and September 1938 exchange of correspondence with Keynes on the
first drafts of the “Essay”. The debate was quite confused, the two men never coming to
agree as to which point was under discussion and evidently not understanding each
other’s point of view. In Section 5, I show that the source of the confusion lay in the
different hypotheses regarding the time-structure of saving and investment that Harrod
and Keynes tacitly assumed in their construction and understanding of the “Essay’s”
‘fundamental equation’. In spite of Keynes’s warning that one should not disregard the
units of measure of non-homogeneous quantities, both discussant omitted the time-
indices from their calculations. This practice eventually concealed the source of the
divergence in their opinion and the irreconcilability of their respective results eventually
diverted their attention from the real point of disagreement. Whereas, consistently with
his methodological tenet (see Chapter VI § 3), Harrod confined his analysis of growth to
a single instant, in Keynes’s view time was a dimension that really mattered and could
not be neglected. In Section 6 I discuss how Harrod’s point that equilibrium growth
should be examined in terms of the simultaneous relationships between the rates of
growth of various magnitudes was interpreted in two different ways in The Trade Cycle
and in the “Essay”, evolving from a logical to a chronological notion of ‘simultaneity’.

In Section 7 I return to Harrod’s notion of investment and to the causal structure
of the “Essay”. In particular, I stress that equilibrium growth is no longer the result of
specifically defined behaviour as it was in The Trade Cycle, but is only a particular state
of the system which is recognised, ex-post, as not having given rise to undesired
accumulation of stocks. The transition between the two works can thus be summarised as
consisting in the shift of emphasis from the study of the causes to the analysis of the effects
of saving and investment.

In his 1938 presidential address and in the “Essay”, Harrod indicated the road he
thought a science of economic dynamics ought to take, and set out the first model
materialising his methodological precepts. Harrod, however, failed to convert his fellow
economists to his dynamic viewpoint. In the concluding Section of this Chapter, the early
reasons for this failure are examined. Again, the exchange of views Harrod had with
Keynes and Marschak concerning the first draft of the “Essay”, provides useful hints for
understanding the difficulties that his approach met in being understood and appreciated
by his fellow economists. But before plunging into the analysis of this correspondence, I
will reconstruct the chronology of the events leading to the publication of the “Essay”
(Section 1), and outline the basic features of Harrod’s model (Section 2).
1. Chronology

The Trade Cycle was published in September 1936, only a few months after the General Theory appeared in the book shops. Keynes only read it after publication, and on 31 March 1937 he sent Harrod some lecture notes on The Trade Cycle’s explanation of fluctuations and other miscellaneous notes on the book. From the debate that followed (published, together with the notes, in Keynes CW XIV, pp. 150-79) it emerges that Keynes misunderstood Harrod’s main point. After receiving Harrod’s explanations, Keynes attributed the confusion to lack of clarity in the text:

I have undoubtedly misunderstood you and there is no question of an arithmetical slip. But the odd thing is that, having invented so interesting a theory, you should not have mentioned it in the book! For I do not see how I could possibly have understood what you were driving at in the passage where I accuse you of an arithmetical slip. Indeed, I should doubt whether any reader who has not talked or corresponded with you 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 to Harrod, 12 April 1937, in Keynes CW XIV, p. 170).

Harrod was particularly disappointed by Keynes not being able to find “any application of the theory worth mentioning to the trade cycle” (Keynes to Harrod, same letter, p. 173), while he maintained that his book “contains the essence or the germ of the theory of the trade cycle”, and promised he would have tried “to develop [his] ideas for a form suitable for publication” in the light of Keynes’s reaction (Harrod to Keynes, 15 April 1937, in Keynes CW XIV, p. 174). After some more comments on Harrod’s theory, Keynes concluded the exchange hoping Harrod would soon carry his ideas a stage further (Keynes to Harrod, 20 April 1937, in Keynes CW XIV, p. 178).

More than one year elapsed before Harrod could communicate to Robertson that he had “in his head, though not yet written”, his fundamental equation (Harrod to Robertson, 5 July 1938). The day after, he also told Keynes that he was working on the announced development:

I have now got my ‘dynamic’ theory, I think, into a much better form than I had it in my book. I hope to send you a short article quite soon (Harrod to Keynes, 6 July 1938, in Keynes CW XIV, p. 298).

In the meantime, Harrod had just finished writing his Presidential Address to the Economic section of the British Association (the methodological issues arising from Harrod’s paper were also discussed at some length with Keynes3). There Harrod already envisaged dynamic theory as relating to the “laws governing increase”, which needed to be formulated in terms of “a system of fundamental equations using simplifying assumptions -cf. the frictionless surface, etc.- in which rates of increase will themselves figure as unknown terms”4. When, on August 3, the first draft of the “Essay” (Harrod 1938*) was finished, Harrod wanted it to be published as soon as possible:

I have just finished writing my re-statement of the ‘dynamic’ theory, which is, I think, a great improvement on my book. […] It has gone off to the typist and ought to be ready to send off in 2 or 3 days. I should like it to appear as soon as you feel you can find some room for me (in December?) […] as I have been
throwing out hints in a number of places of the possibility of formulating a simple law of growth and I want to substantiate the claim (Harrod to Keynes, in Keynes CW XIV, p. 301).5

The typescript was sent on August 66, and Keynes acknowledged receipt of it on August 11, but expressed serious doubts as to the possibility of including the paper in the December issue (Keynes CW XIV, p. 302). However, by 17 August he had “read it carefully”, and had sent Harrod detailed point by point comments. There followed an intense exchange of letters, in which the authors debated some of the points raised by Keynes. On September 7, Harrod announced a first revision of his paper, which was sent on September 14 -the accompanying letter listing the changes introduced-, for which Keynes thanked him five days later (in Keynes CW XIV, pp. 336-9).

But Keynes was still not satisfied with the result:

You have got some extremely interesting ideas and materials in it, as I said before. But, reading it again, I feel even more strongly that, from the reader’s point of view, you have not done justice to them, and I doubt whether the ideas will have the full impact which they deserve. I still find the exposition half-baked and prolix […]. I hope I am not too much influenced in saying this by the editorial preference for something short (ibid.).

He then suggested to Harrod that he put the paper aside for a month or two and promised that if Harrod were still satisfied with it after that time it would go into print as it stood. But Keynes himself carried on with his criticism -which by the time was concentrated on the conditions for the validity of Harrod’s instability principle-, so that the correspondence continued with the same intensity until Keynes’s final blow, dated September 26. Harrod did not reply, and on December 21, 1938, he announced a second, drastic revision of the article, including the introduction of two new paragraphs and the substantial rewriting of the second half of the text.

In the meantime, Harrod’s paper was also sent to Marschak and Robertson. Whereas there is no trace of detailed comments by Robertson on Harrod’s manuscript, nor of a reply by Harrod, Marschak wrote some detailed “Remarks”, to which Harrod replied on the 7th of September with 2 letters. The influence of Marschak’s comments on the final text mainly regarded a number of aspects of detail7, but the epistolary discussion between the two men did not push as deep as the debate with Keynes did. However, Marschak’s complaint as to the lack of clarity of the notion of ‘proper warranted rate of growth’ paralleled Keynes’s criticism, and contributed to inducing Harrod to revise the central part of the “Essay” quite radically.

The result of this process of revision eventually appeared in the March 1939 issue of the Economic Journal, except for a couple of misprints that were corrected in the June issue (p. 377).

2. Basics of the model

The ‘Essay in Dynamic Theory’ is certainly one of Harrod’s most frequently cited works, and is thus probably well-known to the reader. It may however be useful, before
plunging into the details of the development of Harrod’s dynamics from the *Trade Cycle* to the “Essay”, to remind the reader of the basic outline of Harrod’s 1939 model.

It was noted in Chapter VI § 3 above that Harrod’s dynamics is structured in three distinct stages, that is, the identification of an instantaneous picture of the system of relationships, the construction of general laws relating to the succession of phenomena, and finally the discussion of economic policies.

Accordingly, the first stage of the “Essay” regarded the determination of the instantaneous growth rates of the economic system. Let

\[ s = \frac{S}{Y} \]  

(1)

and

\[ C_p = \frac{I}{\Delta Y} \]  

(2)

These equalities define the fraction \( s \) of income “which individuals and corporate bodies choose to save” and the value \( C_p \) “of the increment of capital per unit increment of output actually produced”, by income \( Y \), income changes \( \Delta Y \), savings \( S \) and investment \( I \). Solving (1) and (2) with respect to \( I \) and \( S \), and equating the right hand sides, Harrod obtained

\[ G = \frac{\Delta Y}{Y} = \frac{s}{C_p} \]  

(3)

This expression defines the growth rate of income at a certain instant. All these variables are *ex-post* records of what happens in the system, and Harrod referred to them as ‘actual’ values of the variables. In particular it has to be noted that \( C_p \), that is, the actual value of the ratio of investment to the increase in income, comprehends unplanned variations in the stocks (that are by definition considered as part of *ex-post* investment).

It might happen that \( C_p \) assumes a particular value \( C \) to which there corresponds no undesired accumulation of stocks. In that case the growth rate appears entirely “justified by the circumstances”, and is considered as an equilibrium quantity. The expression of Harrod’s *warranted growth* is therefore as follows:

\[ G_w = \frac{s}{C} \]  

(4)

The equilibrium rate so defined is highly unstable. Any deviation from it would tend to be amplified, rather than to be automatically corrected. For example, Harrod maintained that insufficient expenditure on investment would cause, through the multiplier process, unforeseen accumulation of stocks that would be interpreted by entrepreneurs as a signal of excess investment. They would therefore correct their future action by further reducing investment, thus reproducing and enlarging the former difficulties.

The second stage of Harrod’s argument regarded the long run. Harrod considered that growth itself triggers changes in \( s \) and \( C \) and consequently in the equilibrium rate of growth. When income increases (because of \( G > G_w \), one has to expect the propensity to
save to increase as well. This in turn determines that $G_w$ chases $G$. Since resources are only available in limited amounts, depending on the increase of population and technological change, there exists a limit $G_n$ ($n$ standing for ‘natural’) to actual growth $G$. When this limit is reached, $G_w$, not being subject to the limit defined by $G_n$, grows larger than $G$; this starts a cumulative process in the reverse direction, which symmetrically repeats itself.

The discussion of the final stage, concerning some “practical” problems, required the introduction of two additional concepts. On the one hand, it was necessary to define the \textit{natural rate of growth}, that is “the maximum rate of growth allowed by the increase of population, accumulation of capital, technological improvement and the work/leisure preference schedule”; on the other hand, among the conceivable warranted rates of growth resulting from the possible combinations of $s$ and $C_p$, Harrod regarded as “proper” the one which would result in conditions of full employment. Harrod then proceeded by comparing the proper warranted and the natural rates of growth. Since the latter determines the maximum possible value for the actual rate, if the proper rate is higher than the natural one the actual rate is bound to remain below the warranted rate; such a situation spells depression. This, in turn, drags down the warranted rate below its proper level, so that chronic unemployment ensues. If, however, the proper rate lies below the natural rate, it is possible that the warranted rate is sustained above its proper value by a succession of conditions of price and profit inflation. Harrod therefore concluded that both states of affairs have their proper evil, and suggested that the ideal situation would therefore consist in proper and natural rates being equal. Policy-makers should thus aim at manipulating the proper warranted rate (via the control of the interest rate and programs of public works) so as to equal the natural rate.

3. The fundamental equation

As anticipated in § 1, the story of the “Essay” begins with the exchange of correspondence on \textit{The Trade Cycle} between Harrod and Keynes in March 1937. At first Keynes misinterpreted Harrod’s method of analysis, which he did not understand to be related to the continuous growth characterising the equilibrium state of the system, and was thus led to attribute to Harrod an arithmetical slip. After having admitted his mistake and correctly interpreted Harrod’s problem, Keynes proposed a formulation we have to consider carefully, for it provided some suggestions that Harrod held in due account while writing the “Essay”.

Keynes’s formulation, in fact, matched the ‘fundamental equation’ of Harrod’s dynamics in many respects: it was based on the same elements connected by an equivalent functional relationship. In both cases we have an inverse relationship between a rate of growth, and the product of the multiplier and the accelerator. On the other hand, Keynes’s formulation differed from the analytical framework of \textit{The Trade Cycle} both in its logical structure and in a few details. Moreover, both Keynes’s equation and \textit{The
Trade Cycle differed from the “Essay” with respect to the same features. Comparing these three different interpretations of the interaction of the multiplier and the Relation can therefore provide some clues for understanding the evolution of Harrod’s dynamic theory.

Keynes suggested that the fundamental equation expressing the rate of growth of capital should be

\[ y = \frac{1}{M \cdot R - 1}, \]  

(5)

where \( M \) stands for the average value of the multiplier while \( R \) is the acceleration coefficient\(^{11} \) (Keynes to Harrod, 12 April, 1937, in Keynes CW XIV, p. 171). In his letter to Harrod, Keynes did not specify how he obtained this equation. However, a note of calculations he preserved among his papers indicates that he reasoned as follows.

Let\(^{12} \) \( M \cdot I_t = Y_t \) and \( R = \frac{K_t}{Y_t} \).

If in a unit period of time capital grows at a constant rate \( y \), we have \( K_t = (1 + y)K_{t-1} \).

The value of \( y \) can be easily calculated just by substituting from the definitions given:

\[ y = \frac{\Delta K_t}{K_{t-1}} = \frac{I_t}{\frac{Y_t}{M}} = \frac{R \cdot Y_t}{1 + y} = \frac{1 + y}{M \cdot R}, \]

from which \( y \cdot M \cdot R = 1 + y \) and finally Keynes’s equation (5) is derived\(^{13} \).

Moving from equation (5), Keynes deduced some hints for interpreting Harrod’s theory. First, he noticed that a necessary condition for a constant growth rate is that the product \( M \cdot R \) is constant. That is to say, steady growth requires that changes in some of the dynamic determinants exactly match variations in the others, according to an inverse proportionality relationship (Keynes to Harrod, 12 April, 1937, in CW XIV, p. 171).

Incidentally it must be observed that by adopting a formulation analogous to the one suggested by Keynes, the multiplier and accelerator acquired a different epistemic status. On the one hand, in the “Essay” the three dynamic determinants were substituted by the variables \( s \) and \( C \) (or \( M \) and \( R \), in Keynes’s formulation); the qualitative discussion of the resultant of the dynamic forces in \textit{The Trade Cycle} thus gave place to a quantitative link\(^{14} \). On the other hand, Relation and multiplier were no longer considered as the mechanisms of two interacting processes, but as independent coefficients codetermining the growth rate. The coefficients \( s \) and \( C \) and the growth rate \( G \) were entirely new concepts, thus needing interpretation.

In the second place, Keynes pointed out that \( M \) probably tends to fall as income increases; a constant growth rate thus requires steady growth of \( R \) and therefore a continuous decrease in the rate of interest (\textit{ibid.}, p. 171-2. Only in one of the following letters Keynes made explicit the relationship between \( R \) and the interest rate. This point will be examined in § 4 below).

Third, “it is only by a miracle or a careful design that the values of \( M \) and \( R \) will be such as to be consistent at the same time with steady growth and full employment”
steady growth and full employment do not provide the same criterion. As between them I do not favour steady growth. On the contrary I say several times that, starting with the slump, to damp growth down to what could be steadily maintained would involve perpetuating existing unemployment, which would be intolerable” (Harrod to Keynes, 15 April 1937, in Keynes CW XIV, p. 176).

In the “Essay” Harrod faced this point more directly, acknowledging that the warranted growth rate is not uniquely determined once for all, but depends, among other things, on the level of activity.

The fourth of Keynes’s remarks was the most painful for Harrod and influenced the further development of both Harrod’s theory and the debate with Keynes. According to Keynes, The Trade Cycle was not an appropriate title for Harrod’s book: “I do not see that the theory has an application worth mentioning to the trade cycle”. On the one hand, Keynes maintained that growth is a problem pertaining to long-period analysis (Keynes to Harrod, 12 April 1937, in Keynes CW XIV, p. 173; for the time being, Keynes did not give further details). On the other hand, Keynes attributed to Harrod the assumption that the boom is a phase of steady growth (ibid., p. 171). Keynes then rejected this hypothesis, asserting that “the maintenance of steady growth is at all times an inherent improbability in conditions of laissez-faire”: “both the boom and the slump, that is to say the whole of the cycle, are characterised, I should have thought, by none of the conditions of steady growth which you are assuming to be present” (ibid., p. 173).

Harrod reacted vigorously: “in spite of what you say, I still think that my book concerns the trade cycle; nay more, subject to further criticism, that it contains the essence or germ of the theory of the trade cycle” (Harrod to Keynes, 15 April 1937, in Keynes CW XIV, p. 174). In his reply, Harrod emphasised the lack of symmetry between growth at a constant or increasing rate justifying entrepreneurs’ decisions, and any fall in the pace of growth causing a ruinous depression: “it is always possible to accelerate, but it is not possible to decelerate without starting again from the zero line” (ibid., pp. 174-5). In other words, Harrod was thinking of the upward phase of the cycle in terms of a growth pace (temporarily) feasible for the economy. This notion did not strictly coincide with that of equilibrium, since it also included more than justified investment. Keynes’s criticism indicates that he did not understand that Harrod was less concerned with equilibrium strictly than with the instability of the equilibrium state15.

Harrod’s reply to the crucial question whether growth is a matter of long or short run consideration, eventually channelled the following debate and constituted the main point of disagreement between Harrod and Keynes. Still, as I shall try to show, this was not at all obvious even for them.
4. The Accelerator: actual, normal and equilibrium values

For Harrod the problem was that of divorcing the component of global investment dependent on long run consideration from the part depending on short-period perspectives:

You think I am wrong in making investment a function of current growth only. Granted. Suppose only half were governed by current growth, the rest by long-period planning. My theory is substantially intact. It remains true that the growth of consumption cannot slow down without producing a great recession; but in this case the recession would only have to be such as to reduce savings to half their usual level.

Personally I believe by far the greater part of investment rests on an immediate prospect of an increase of demand. People do not build new factories for use some years hence nor houses that will remain unwanted. Why should they? They increase equipment at the last feasible moment to save interest. Moreover if you try looking more than a year or so ahead everything becomes so violently uncertain (Harrod to Keynes, 15 April 1937, in Keynes CW XIV, pp. 175-76).

To understand Keynes’s viewpoint, it is necessary to examine carefully his interpretation of the fundamental equation $y = \frac{1}{M \cdot R - 1}$. First, Keynes discussed which variables had to be considered as dependent and which as independent: “I should hold that $y$ is determined by the rate of interest and the state of expectations, that $M$ is determined by individual psychology and institutions, and that, in the short-period, $R$ is dragged at the chariot wheels of $y$ and $M’$. Thus in the short run $R$ has to be considered as a dependent variable: if in consequence of better expectations the rate of growth of capital should increase, there would follow a change in the relative prices of capital and consumption goods such as to reduce $R$ to a level compatible with the new $y$, given the value of the multiplier (see Keynes to Harrod, 20 April 1937, in Keynes CW XIV, p. 178).

Keynes raised then a second point: “on the other hand, the strength of your approach lies in the idea that $R$ has, so to speak, a normal long-period value which is a function of the rate of interest” (ibid., p. 177). This was the source of the first of Keynes’s criticisms relative to trade cycle and long run: “the factors determining the normal value of the relation have little or no bearing on the trade cycle; and it was those I thought you were discussing in much of your book” (ibid., p. 178).

The distinction introduced by Keynes had far-reaching consequences. To appreciate them, it is worth considering how he applied the new concept of ‘normal value of the Relation’ (which will be denoted by $R_n$) to the cycle. Consider a state of the economic system in which $R = R_n$, and suppose that there occurs a change in the expectations or in the value of the multiplier. Being a dependent variable, $R$ should then adapt to the new condition, thus diverging from $R_n$ which depends instead on long-period considerations. There would follow, for instance, $R < R_n$; this would stimulate economic activity, $y$ would increase causing a further fall in $R$ and thus widen the gap between $R$ and $R_n$ (ibid., pp. 177-8).
Harrod could not remain indifferent to this statement of the instability principle, and indeed we find traces of it in the “Essay”. Rather than pointing out the similarities between these two approaches, I shall concentrate instead on the differences, which seem to reveal some peculiarities of the evolution of the ideas of accelerator and investment from *The Trade Cycle* to the “Essay”.

Let me thus compare Keynes’s equation

\[ y = \frac{1}{M \cdot R - 1} \]  

with Harrod’s equations describing the actual and warranted rates of growth:

\[ G = \frac{s}{C_p} \]  

and

\[ G_w = \frac{s}{C} \]  

As Keynes himself pointed out, if the Relation was assumed to be constant the rates of growth of capital \( y = \frac{I}{K} \) and of income \( G = \frac{\Delta Y}{Y} \) would be equal (Keynes to Harrod, 12 April 1937, in Keynes *CW XIV*, p. 171). On the other hand, both Keynes and Harrod agreed that trade cycle analysis could not overlook the possibility and indeed the actual occurrence of changes in \( R \). It was therefore inherently different working with one or the other variable, in spite of the similarity of the respective equations. Entrepreneurs and consumers decided in fact how much to invest and save, not what the level of income would be. \( y \) was therefore an expression of the process of decision, and had to be interpreted as an independent variable. \( G \) on the contrary represented the global effect of the saving and investment decisions and was thus a dependent variable. More accurately, \( G \) was the result, registered *ex-post*, of the decisions actually made, while \( G_w \) expressed the result of the decisions that the entrepreneurs ought to make in order to be exactly satisfied\(^{16}\).

Regarding the multiplier, opinions fundamentally agreed. Keynes maintained that for trade cycle analysis \( M \) had to be considered an exogenous variable, though its value probably fell as \( y \) increased. In his “Essay”, Harrod treated \( s \) alike: he considered it as given in the short run, but subject to fluctuations due to variations in \( Y \) in the course of the cycle; the changes in this variable were themselves part of the cycle mechanism.

Regarding \( R \) and \( R_n \), and \( C \) and \( C_p \), however, opinions radically diverged. First, Keynes distinguished \( R \) from \( R_n \) for the length of the reference period. In the short run \( R \) was dependent on \( y \) and \( M \), while \( R_n \) was independent of the present state of business and only referred to the long-period rate of interest. On the contrary, Harrod’s \( C \) and \( C_p \) both concerned a single instant, a period of infinitesimal length during which they had to be considered as data (this point is of fundamental importance, and will be discussed below in § 6). The second difference to be mentioned is that \( R \) stood for the ratio between
capital and income, while \( C \) was its marginal value \( \frac{\Delta K}{\Delta Y} \). The last aspect to be examined is the most interesting, since comparison of \( R, C \) and the Relation shows how Harrod’s notion of investment developed in those few years.

The meaning of the Relation in *The Trade Cycle* was the following. Given the technique, the rate of interest, the degree of utilisation of capital (though Harrod did not mention this factor) and any other relevant considerations, to increase the production of consumption goods it was necessary to employ a correspondent additional quantity of machinery, labour, etc. In Harrod’s view, investment thus depended on the prospective increase in the demand of consumption goods, in the ratio defined by the third dynamic determinant. The Relation therefore expressed a *determination nexus*, converting entrepreneurs’ expectations and the technical conditions of production into investment decisions. *The Trade Cycle* provided a theory of the determinants and the consequences of the interrelations of two distinct but mutually interdependent orders of decisions, those regarding investment and those regarding saving (see Harrod 1936a, especially pp. 160-63, and for a discussion see Chapter V, §§ 1 and 9 above).

For Keynes investment depended on the state of expectations. Given the multiplier, the rate of increase of capital \( y \) determined how the ratio of \( K \) to \( Y \) changed. Keynes’s idea seems to run as follows. Investment was an addition to capital stock, but on the other hand, through the multiplier, it also generated income. Given an initial capital \( K_0 \), after investment occurred capital would amount to \( K_i = K_0 + I_i \). Income, at the end of the period, would be \( Y_i = M \cdot I_i \) (remember that \( M \) represented the average and not the marginal multiplier). \( R \) was residually determined by the new values of capital and income, \( K_i \) and \( Y_i \). Since the alterations that investment induced in capital and on income were independent of each other, nothing could guarantee that they were consistent with a constant \( R \). Investment decisions were not represented by \( R \), which included instead a multitude of other factors: in Keynes’s view, the acceleration principle was not a determinant of investment.

To clarify things, let me calculate the new value \( R_i \) for the Relation, corresponding to \( K_i \) and \( Y_i \):

\[
R_i = \frac{K_i}{Y_i} = \frac{K_0 + I_i}{M \cdot I_i} = \frac{I}{M \left( 1 + \frac{K_0}{I_i} \right)} = \frac{I}{M \left( 1 + \frac{1}{y_i} \right)}.
\]

This result obviously matches the one that can be obtained by manipulating Keynes’s fundamental equation (5).

In his “Essay” Harrod stated that a community’s increase of income was an important determinant of its investment decisions, and translated this axiom in the equations \( I = C \cdot \Delta Y \) and \( I = C_p \cdot \Delta Y \) (I have introduced the subscript \( e \) to represent equilibrium values). Harrod, like Keynes, treated \( C_p \) as a residual magnitude, determined by actual increases of income and investment (the latter including increments of capital planned by entrepreneurs and undesired variations of stocks). In other terms, \( C_p \)
summarised (given s) the ratio of investment decisions and their actual effect on income and stocks. \( C \) was merely a particular value \( C_p \) could take, in the special case when the combined effects on income of the expenditure in capital goods and of the decisions of saving do not give rise to unplanned changes in stocks.

Hence -in spite of Harrod’s claim to the contrary (1939a, p. 14)- not even in the “Essay” was the accelerator a determinant of investment. \( C \) in fact was not a ratio of investment to increase in income that entrepreneurs would find particularly satisfying or desirable. It is actually true that the warranted \( C \) would be preferred to any actual \( C_p > C \), but it is also true that entrepreneurs would prefer to the warranted rate any \( C_p < C \), which would make *ex-post* investment more than justified.

Equations \( I_e = C \cdot \Delta Y_e \) and \( I = C_p \cdot \Delta Y \) therefore did not really represent the causal link the axioms claimed for them. What determined the investment in the “Essay”, then? In Harrod’s growth equations there was no term representing the quantity of capital goods that entrepreneurs actually ordered, and after all, this was a matter that did not bother Harrod. While Keynes treated \( y \) as dependent on the rate of interest and the state of expectations, for Harrod’s instability argument only the *ex-post* registration of undesired accumulation or shortage of stocks mattered, which the entrepreneurs would consider as an indicator of excess or deficit of investment and production. Obviously also the difference between \( R \) and \( R_n \) considered by Keynes entailed undesired variations of stocks or of the degree of utilisation of plants. Yet in Keynes’s view this would influence expectations, while Harrod’s entrepreneurs interpreted \( C \neq C_p \) directly as a signal of inadequate production and as an instruction to increase (or decrease) investment.

Harrod’s interpretation of the fundamental equation thus excluded expectations, which were at the core of both Keynes’s analysis and *The Trade Cycle*\(^{19}\). While in his book Harrod compared the actual result of the entrepreneurs’ actions with the result they expected at the moment of deciding the amount of investment, in the 1939 article Harrod compared actual and warranted effects of some unexplained decisions independent of expectations.

The difference in the interpretation of the acceleration principle in Harrod’s book and in his article was mirrored in the new nomenclature adopted in the “Essay”\(^{20}\). Harrod no longer used ‘equilibrium’ but preferred ‘warranted’ instead. This word had a precise meaning: “I apply warranted to the unknown variable, rate of growth, the value of which is found by solving the equation”\(^{21}\) (Harrod to Keynes, 7 Sept., 1938, in Keynes *CW* XIV, p. 337).

The very concept of *dynamics* had changed together with the notion of investment. In *The Trade Cycle* dynamics was defined as the study of the set of forces (interpreted as *causes*: see above, Chapter V, § 2) determining the pattern of economic growth. In the “Essay” it was reduced to the formulation and resolution of a system of equations “in which a rate of growth appears as an unknown variable” (Harrod 1939a, p.17) and from which all causal nexuses were eliminated\(^{22}\). Harrod’s dynamics thus
turned from a science of causes into a science of effects that ignored what determined them.

This change of perspective led to a paradoxical result. In spite of being the unknown variable in the fundamental equation, \( G_w \) was absolutely redundant for the argument of the “Essay”. The only function of this variable was that of providing a term of comparison for \( G \), but the difference \( G - G_w \) was logically and formally equivalent to the difference \( C_p - C \). In discussing his fundamental equation, Keynes did not feel the need to introduce a term equivalent to \( G_w \); \( R_n \) had rather a different methodological status than \( C \), not only because the former referred to the long run while the latter was instantaneous, but also because \( R_n \) was positively defined as the ‘normal’ value of the variable \( R \) while \( C \) was negatively defined as that particular value of \( C_p \) that does not register unwanted accumulation of stocks.

The exchange of letters on the first draft of the “Essay” reveals more details about these aspects and therefore deserves a more detailed examination.

5. The topic Harrod and Keynes actually discussed: the validity clause for the fundamental equation

The first draft of the “Essay” was sent to Keynes on August 6, 1938. Keynes’s first detailed comments on the paper were dated August 17. Keynes asked for some restatements and for the explanation of a few terms, congratulated Harrod on his interpretation of the Treatise on Money, and formulated a criticism that was going to attract most of their attention in the course of the subsequent discussion.

The whole affair appears quite curious, since many misunderstandings on both parts confused the terms of the debate and in the end concealed from Keynes and Harrod themselves the true problem lying behind Keynes’s criticism.

The problem is as follows. Keynes argued that the solution of Harrod’s model is not independent of the value of the parameters, but that its instability is subject to the condition that \( C > s \) (Keynes to Harrod, 17 Aug., 1938, in Keynes CW XIV, p. 324). Harrod was never able to understand this comment of Keynes, while its implication first escaped the attention of Keynes himself, who only in his last letter affirmed to have finally realised that the above mentioned condition does not regard the stability, but the very existence of an equilibrium rate of growth.

Because of the reciprocal misinterpretations, the discussion often looks quite confused. Moreover, the modern reader of the correspondence is even more puzzled by a few slips in the transcription of some of the formulae and by the fact that the symbolism to which Harrod and Keynes referred in the correspondence was different from that of the “Essay”. And in Keynes’s opinion, to which I subscribe, it anyway remained a “very muddling symbolism” (Keynes to Harrod, 19 Sept., 1938, in Keynes CW XIV, p. 340), “so contrived as to lose sight of the dimensions of your quantities,
which makes it very difficult to handle”; “I have found it practically impossible to work things out for myself in terms of your symbolism” (ibid., p. 339).

Yet it is not difficult to identify the cause of the inability to communicate if one accepts Keynes’s suggestion to pay due attention to the dimensions of the magnitudes27.

C and s were not homogeneous quantities, s being a pure number and C depending on the length of the period, ∆t. Keynes thus correctly pointed out that “it is dangerous to drop [the length of the period] out in the first instance, because it confuses the dimension to drop the time dimension out of sight by defining it as a unity. (This is a point about which a mathematician is very particular)” (Keynes to Harrod, 26 Sept., 1938, pp. 347-8)28. In what follows, I shall thus insert a reference to the length of the period. The reader should take notice that s, being a number, is not associated to a unit of measure; C is measured in terms of a time unit. Income Y, savings S and investment I are fluxes, and are therefore to be measured in some money unit divided by a time unit (obviously the same time unit used for C). The rates of growth G, Gw and y are measured in (time unit )−129.

The origin of the misunderstandings seems to me to reside in the fact that Harrod and Keynes attributed a different degree of importance to the flowing of time in economic behaviour. However, in the course of their discussion this difference was concealed by their practice of omitting the time indices. I shall now reintroduce these indices, to show that their reasonings were based on different assumptions. This will provide the instruments for discussing Harrod’s notion of instantaneous analysis, whose comprehension is essential for understanding the “Essay” and the debate with Keynes.

Harrod repeatedly stressed that his analysis only concerned a single instant (as to the origin and rationale of such procedure, see Chapter VI, § 3 above). He obviously did not feel the need to specify at what time his variables referred, since they all belonged to the same time section. Moreover, if ∆t was sufficiently small, the difference between, e.g., Yt and Yt−∆t would be irrelevant. Harrod could thus explicitly declare that in defining the rate of growth of income it did not matter whether it referred to the income of the preceding or the current period: \[ G = \frac{\Delta Y_t}{Y_{t-\Delta t}} \equiv \frac{\Delta Y_t}{Y_t} \] (Harrod 1939a, pp. 16 and 20).

Having made this premise, the axiom that “the rate of increase [of a community’s] income is an important determinant of its supply of savings” can be formulated in two ways, either

\[ S_t = s \cdot Y_{t-\Delta t} \] (1a)

or

\[ S_t = s \cdot Y_t. \] (1b)

Assumption (1a) presupposes that people save a fraction of the income of the preceding period. This is Robertson’s hypothesis, which Harrod had already employed in *The Trade Cycle* to account for some peculiarities of the business cycle’s turning points (cf. Harrod 1936a, pp. 128 ff.; for a discussion see Ch V § 7). Using (1a), the
calculation of the rate of growth of $Y$ simplifies to the highest degree; but it implies a drawback: at time $t$, consumption and saving do not add up to total income. This happened for instance in the case Harrod considered in his letter to Keynes of 21 August (in Keynes CW XIV, p. 328). The alternative formulation (1b) although introducing some complications in the calculation of the rate of growth is free of the above mentioned difficulty; it therefore implies the strict equivalence of investment and saving, the first regarded as the constituent of income other than consumption and the latter defined as the excess of income over consumption.

The origin of the misunderstandings seems to lie in the fact that Harrod calculated using (1a), but argued with Keynes supposing (1b)\textsuperscript{30}. He obviously felt justified in doing so by his assumption that $\Delta t$ was small enough. Ignoring the time indices, Harrod thus wrote (1), thought (1b), and calculated with (1a). Keynes instead consistently used only (1b) - although he sometimes referred to Harrod’s result obtained on the ground of (1a) -, since he could not believe $\Delta t$ to be irrelevant\textsuperscript{31}.

Before examining the consequences of this, let me calculate the growth rates corresponding to the two assumptions. The equations representing investment and the equality between saving and investment must be taken into account:

\begin{align}
I_t &= C_{\Delta t} \cdot \frac{\Delta Y_t}{\Delta t} \\
I_t &= S_t \tag{6}
\end{align}

Combining (1a), (2) and (6), and (1b), (2) and (6), it is easy to obtain respectively the rate of growth of the “Essay” and the rate of growth of which Keynes was thinking\textsuperscript{32}:

\begin{align}
\hat{G}_{\Delta t} &= \frac{s}{C_{\Delta t}} \tag{7a} \\
\tilde{G}_{\Delta t} &= \frac{s}{(C_{\Delta t} - s \cdot \Delta t)} \tag{7b}
\end{align}

These two equations mean that, if at time $t-\Delta t$ the system was in equilibrium and the parameters $s$ and $C$ remained constant, then corresponding to alternative assumptions (1a) and (1b) the only increases of income in period $\Delta t$ that would not entail unexpected accumulation of stocks would be respectively $\Delta Y_t = \hat{G}_{\Delta t} \cdot Y_{t-\Delta t} \cdot \Delta t$ and $\Delta Y_t = \tilde{G}_{\Delta t} \cdot Y_{t-\Delta t} \cdot \Delta t$.

It is now possible to verify Harrod’s premise that if $\Delta t$ were small the two solutions would differ by much:

\[
\lim_{\Delta t \to 0} \tilde{G}_{\Delta t} = \lim_{\Delta t \to 0} \frac{s}{C_{\Delta t} - s \cdot \Delta t} = \frac{s}{C_{\Delta t}} = \hat{G}_{\Delta t}
\]

Yet Keynes too was right in claiming that the terms on the right hand side of (7b) must not be overlooked. If $s \cdot \Delta t > C_{\Delta t}$, the denominator would be negative and $\tilde{G}$ would not describe growth but a cumulative downfall, or even alternation of positive and negative values of $Y$ if the values of $s$, $C$ and $\Delta t$ were such as $\tilde{G} < -1$. Harrod probably never bothered solving the second set of equations and could therefore not imagine that such a difficulty would emerge, nor understand Keynes’s remark. As a matter of fact, equation
(7a) is free of this problem, both terms on its right being positive\textsuperscript{33}. Keynes’s suggestion that in practice $C_\alpha > s \cdot \Delta t$ was not a sufficient condition also seems legitimate, since for the rate of growth to be feasible $C$ must be much larger than $s \cdot \Delta t$ (about forty times as large, in order to have $\tilde{G} \equiv 2.5\%$) (Keynes to Harrod, 26 Sept., 1938, in Keynes CW XIV, p. 349).

Unfortunately, Keynes’s and Harrod’s attempts to settle this matter ultimately diverted their attention from the peculiarity of Harrod’s method which was the real cause of incommunicability. Their debate on the questions of stability and existence of equilibrium relegated as a secondary matter the problem of instantaneous analysis, which was the real core of dissent. In other words, it seems to me that Keynes had not been able to force Harrod to state explicitly the implications of his method. This would have been possible if only Keynes had adopted an accurate formulation of his fundamental equation\textsuperscript{34}.

6. The topic Harrod and Keynes ought to have discussed: instantaneous analysis

In Chapter VI, § 3, I have discussed the distinction Harrod operated between the first stage of dynamic analysis, in which the network of relationships was studied in its simultaneity, the second stage in which laws determining the succession of events were determined and the final stage in which policy matters were discussed. This method was consistently applied in both \textit{The Trade Cycle} and the “Essay”, although the two approaches have to be distinguished.

In \textit{The Trade Cycle} ‘simultaneity’ was intended as a \textit{logical} relation, while in the “Essay” Harrod literally confined his analysis to a single \textit{instant}. In his 1936 book in fact Harrod was concerned with processes whose causes (the determinants) had to be distinguished from their effects. He examined causes and consequences not only in terms of their logical links but also in their time structure: entrepreneurs take their investment decisions with the prospective, \textit{future} uses of capital goods in mind (see for instance Harrod 1936a, pp. 88 and 160). \textit{The Trade Cycle} described a world where past and future were not symmetric, where therefore expectations were all important\textsuperscript{35}. In 1939, on the contrary, “the analysis relates to a single point in time” (Harrod 1939a, p. 24), and if the temporal horizon is reduced to a single instant there is no room for the distinction between past and future. The alteration in the interpretation of ‘simultaneity’ reflected both the change of role of the acceleration principle and the shift of emphasis from the causes to the effect of investment. (I have already said something about this in § 4, but I will add a few considerations in § 7 below.)

On the other hand, these were not the only reasons favouring the adoption of a new view of simultaneity, for the formal treatment of the “Essay” imposed some additional analytical constraints. In particular, to determine the warranted rate of growth and to explore the consequences of any deviation from the equilibrium state it was
necessary that the coefficients $C$ and $s$ remained constant. Given his decision to define dynamics as the formulation and resolution of a fundamental equation whose unknown variable was a rate of growth, Harrod was then bound to choose between two alternatives: either to regard $s$ and $C$ as constants, or to restrict his analysis to a domain where they could possibly be treated as if they were constant. Harrod was conscious of this requirement and refused to assume that the multiplier and the accelerator remain unchanged during the cycle. But how could he account for their variations? Here it becomes evident why and how Harrod applied the methodological distinction worked out in his 1938 Presidential address. The cartographic stage, that really defined economic dynamics, consisted in the determination of the equilibrium growth rate in one single instant and the study of its stability in its neighbourhood. The analysis of the cycle followed: only by considering the sequence of events far from equilibrium was it possible to account for the changes in $s$ and $C$, the parameters that defined equilibrium itself.

This was thus the rationale of Harrod’s decision to confine his analysis to a period $\Delta t \to 0$. Strictly speaking, only an interval of infinitesimal length assured the constancy of the coefficients. On the other hand, one had to ask oneself what the meaning of $\Delta t$ was, and in the end Harrod agreed with Keynes that “the relevant period is as you say that within which orders are readjusted” (Harrod to Keynes, 14 Sept., 1938, in Keynes CW XIV, p. 338).

In Keynes’s view, time truly flowed, determining a symmetry break between the past and the future, the former given and known, the latter uncertain and indeterminate. Keynes would not even dream of considering a vanishing interval: $\Delta t$ could not tend to zero, but was of a finite and not negligible length. Therefore in his view during this interval something relevant could happen to $s$ or $C$. For instance, if $\Delta t$ were long enough any positive $G$ would entail perceptible increases of the level of income that might in turn cause an increase in the propensity to save; it could thus happen that a casual deviation from equilibrium (say $G > G_w$) generated, through the combined effect of $\Delta Y$ and $\Delta s$, an increment of total savings capable of financing the extra investment. In that case Harrod’s principle of instability would not hold: the new rate of growth would be as warranted as the old, and the corresponding equilibrium would be neutral or even stable.

The discussion of this case was long and painful. Harrod agreed that this could be the case, but he was doubtful of its relevance: “if you think the point sufficiently important in practice or theory I will insert a footnote” (Harrod to Keynes, 14 Sept., 1938, in Keynes CW XIV, p. 338). At first Keynes insisted that this was not a footnote matter, but Harrod finally proved that even an unrealistically high marginal propensity to save could entail only a minimal effect, if any, on the average propensity. Eventually, Harrod also managed to persuade Keynes that in his model only the average $s$ was important, since it was the total saving that had to equal aggregate investment.
Harrod however was not convinced that this proof was necessary to his argument, but he supplied it only to convince (or to please) Keynes. In the course of the entire correspondence Harrod contended that it was necessary to keep distinct the three problems of equilibrium, of its stability and of the cycle, and that the proper procedure was to analyse the situation instant after instant. Before showing that his "proposition regarding instability, rigidly proved for $s$ constant, also holds with $s$ variable save in unlikely circumstances", Harrod confirmed

the formal point that it is not to be supposed that $s$ changes in response to changes in the rate of growth, but only to changes in the level of income. A changed rate of growth will have to endure some time before an appreciable consequential change in the level of income occurs (Harrod to Keynes, 22 Sept., 1938, in Keynes CW XIV, p. 342).

And again: the analysis being referred to a single instant,

$s$ is regarded as likely to vary with a change in the size of income, but a change in the rate of growth at a given point of time has no effect on its size” (Harrod 1939a, 24-25).

Harrod’s proof convinced Keynes “that the average, not the marginal, propensity is relevant”, and therefore “it is not worth bothering about a possible change in $s$, corresponding to a change from $Y$ to $Y + \Delta Y$“ (Keynes to Harrod, 26 Sept., 1938, in Keynes CW XIV, p. 346). But changes in the other coefficient, $C$, had to be considered as well.

7. **Ex-ante and ex-post: more about the accelerator**

Harrod did not assume that in the long run $C$ was constant; on the contrary, his explanation of the cycle presupposed changes in $C$ as well as $s$. Discussing stability, Harrod referred again to the methodological argument of instantaneous analysis: “$C$ may also be expected to vary with the size of income, e.g., owing to the occurrence of surplus capital capacity from time to time, but the same argument for regarding it as independent of the rate of growth at a particular point of time applies” (Harrod 1939a, 25). From this point of view the debate on the nature and variability of $C$ did not therefore differ much from the discussion of $s$ just examined. It is nonetheless interesting to dwell upon it since it reveals new insights into Harrod’s notion of investment as compared to Keynes’s emphasis on uncertainty and expectations in the analysis of investment decisions.

In § 4, I argued that Harrod’s definition of $C$ was negative. In fact $C$ was thought to be the coefficient describing the level of investment “which producers regard as ideally suited to the output which they are undertaking in that period” (Harrod 1939a, 19), where ‘ideally suited’ did not refer to the entrepreneurs’ desires but only to the absence of ex-post unplanned variations of stocks. $C$ was therefore not considered for what it was, but for what it was not: it was only that particular ratio $\frac{I}{\Delta Y}$ involving no residuals in the form of stocks. $C$ was not defined on its own, but only as a particular value of $C_p$; it was nothing else than a watershed between two domains where instability prevailed.
Keynes on the contrary treated investment as the result of decisions in conditions of uncertainty. All of his comments on the notion of $C$ reflected this viewpoint and give cause to recall the discussions on the nature of $R$ which occurred the preceding year.

In the first place Keynes disliked Harrod’s attempt to condense into $C$ all the factors determining the appropriate size of investment:

You are wrapping up the influence of the rate of interest in the state of technology, and you do in passing mention this. This is certainly convenient so far as the long-period trend of the rate of interest is concerned, though not so convenient when applied to short-period interest changes. I suggest, however, that it would be advisable to emphasise more clearly what you are doing. Moreover, you are making no reference to the ‘state of confidence’. Presumably that also is wrapped up in the state of technology. But that is not nearly so convenient, since it is essentially a short-period phenomenon. On the whole, my preference would be not to wrap up the rate of interest and the state of confidence in the state of technology, but simply to say that you were, at this stage of the argument, taking them for granted and that the influence of changes in the rate of interest or the state of confidence have to be superimposed on the present argument. Changes only really have to be brought in just the same sort of way as you have brought in a change in the propensity to save [...]. If you are to get your own specific point clear, it is better to segregate changes in all these factors (Keynes to Harrod, 17 Aug., 1938, in Keynes CW XIV, pp. 321-22).

Harrod took no notice of Keynes’s point, and candidly replied: “I have inserted a paragraph about confidence” (Harrod to Keynes, 7 Sept., 1938, in Keynes CW XIV, p. 337); a comparison between the first draft and the published version of the “Essay” reveals that the addition consisted of a clause specifying that, among the “other conditions” he mentioned, the rate of interest and the state of confidence were to be included alongside technology. In the published version of the “Essay” we thus read that $C$ is “the amount of capital per unit increment of output required by technological and other conditions (including the state of confidence, the rate of interest, etc.)” (Harrod 1939a, p. 18; see Harrod 1938*, § 4)142. It is scarcely surprising that Harrod did not accept Keynes’s suggestion. According to him, $C$ was not a coefficient accounting for the factors influencing investment decisions, but rather an *a posteriori* summing-up of the conduct the entrepreneurs ought to have followed to be ‘satisfied’ with their action. Moreover, in the “Essay” decisions were never discussed: Harrod only considered the consequences whose causes did not concern his analysis. There were only two parameters referring to investment: $C_p$, that represented the actual increment of capital per unit of output, and $C$, a particular case of the latter; neither of them was concerned with economic actions.

To distinguish between them, Harrod used the term *ex-ante* for $C$ and *ex-post* for $C_p$. The potential deceptiveness of this choice did not escape Keynes’s attention. His remark promptly characterised the peculiarity of Harrod’s use of the new terminology, which related investment not to the entrepreneur’s decisions, but to what, *ex post*, would be discovered to be the right thing:
$C$ is most certainly not ex ante strictly speaking. Ex ante $C$ is the investment entrepreneurs actually plan to make. Your $C$ is the addition they ought to plan to make. $C$ is the planned investment which would equate ex ante and ex post investment (Keynes 1973, 322).

Harrod’s reply again dismissed Keynes’s point, for instead of questioning his own notion of investment he objected that the use of the term was not yet well established, and claimed his right to use it in a different sense. Nevertheless, he introduced the following change: while in the first draft of the “Essay” ex ante $C$ was defined as

that quantity of capital goods, which, if producers foresaw the total development during the period, they would produce, or that which, if they do produce it, makes them feel satisfied that they have neither exceeded nor fallen short of the mark. For convenience $C$ in this sense will be referred to as ex-ante $C$ in this article (Harrod 1938*, § 6)

in the final version it was defined as

that addition to capital goods in any period, which producers regard as ideally suited to the output which they are undertaking in that period. For convenience the term ex ante when employed in this article will be used in this sense (Harrod 1939a, p. 19).

Rather than stressing the difference with Lindahl’s formulation, Harrod had thus rephrased his reference to the difference between the equilibrium and the actual investment, which, as Keynes acutely remarked, was Harrod’s true concern. While Keynes was pointing out that Harrod’s notion of investment did not account for the actual plans, Harrod did not feel bound to modify it. The entrepreneurs’ plans and desires did not pertain to his dynamics, which was only concerned with equilibrium. When he wrote that $C$ is the increment of capital per unit of income “which is desired” (Harrod 1939a, p. 22), Harrod did not mean that it was desired by capitalists (who would rather prefer a lower ratio, that would more than justify their investments), but only that it is necessary for growth being in equilibrium.

Third, Keynes raised a point that has been overlooked by Harrod’s interpreters who have transformed the “Essay” into a theory of economic growth. $C$ must not be interpreted as the capital-output ratio, but it is rather “the marginal rate of investment corresponding to additional income, which is significant” (Keynes to Harrod, 19 Sept., 1938, in Keynes CW XIV, p. 342): “I stress that it is $\frac{\Delta K}{\Delta Y}$ which is relevant, not $\frac{K}{Y}$” (Keynes to Harrod, 26 Sept., 1938, in Keynes CW XIV, p. 346).

If growth represents a growth of population and not of standard of life, there is, indeed, no reason for much difference between $\frac{K}{Y}$ and $\frac{\Delta K}{\Delta Y}$ [...]. But let us suppose a stationary population in an old country, the rate of growth relating solely to the standard of life; or, even worse, a declining population. Let us suppose that the assumed rate of interest has been in force for a long time, so that all the capital which can be advantageously employed at that rate of interest with the existing level of income is already employed. Let us suppose that the transport system and public utilities and housing are all on a satisfactory standard, and no foreign investment. Let us suppose disarmament, sinking funds rampant, distribution of profits conservative and belated, incomes unequally
divided, fairly good employment so that there is large dissaving on this head (*ibid.*, p. 348).

Keynes argued that if such a state of affairs prevailed, one would have to expect that marginal investment would be much lower than the capital-output ratio, and that correspondingly there might arise some further difficulties in proving the instability principle (*ibid.*, pp. 347-349).

There is a further consideration that might accentuate the bias between marginal and average values of $C$, that is the fact “that there are large fluctuations in unused capacity (or in capacity employed below its maximum) or in inventories or in both”. If there was all-round surplus capacity, production could be increased without employing additional capital equipment. “Thus you are assuming that there is no surplus capacity”\(^{46}\), or anyway that the degree of utilisation of capital remained constant.

Once again Harrod did not change his mind. In his viewpoint, the quantity of capital required or proportion of income saved are data, analogous to the demand schedules of static theory. (Data, the values of which, like those of demand schedules, change from time to time) (Harrod to Keynes, 7 Sept., 1938, in Keynes *CW* XIV, p. 337).

8. The case of a failure

In spite of the fame of his “Essay in Dynamic theory”, Harrod failed to convert his fellow economists to his dynamic viewpoint. By ‘Economic Dynamics’ Harrod meant a theory of the determination of the instantaneous rate of growth of an economic system\(^ {47}\), for which “a new method of approach -indeed, a mental revolution- is needed” (Harrod 1939a, p. 15, but also see Harrod 1938a, pp. 402-405). Both the features Harrod advocated for dynamics have been neglected or misinterpreted by scholars in the field, who almost unanimously accepted Ragnar Frisch’s conception\(^ {48}\) (for Harrod’s attitude towards the econometricians’ notion of dynamics, see Chapter VI, § 3). It is interesting to notice that the difficulties that prevented the mental revolution advocated by Harrod were already inherent in the debate with Keynes.

A first aspect that initially escaped the attention of Keynes and his contemporaries is the fact that the dynamic analysis concerned the determination of the equilibrium rate of growth. This had not been noticed at all when *The Trade Cycle* and the ‘Essay’ were published; only after the war, when Harrod restated his fundamental equation in a book\(^ {49}\) was it paid due attention (for a survey of this literature see Besomi 1996a). Hicks’s remark represents the situation well:

> I could kick myself for not having seen it before. After all, the essential ideas which I am taking from Mr. Harrod are not new ideas, put forward by him the first time in 1948; if one had eyes to see, one could have seen them nearly a decade ago. It is, however, quite clear […] that neither I myself, nor (as far as I know) anyone else, seems to have seen them\(^ {50}\) (Hicks 1950, p. 7).

In the second place, students of economic dynamics did not favourably receive the method of instantaneous analysis: according to Ragnar Frisch’s (and the generally
accepted) view, a system is dynamic only if at least one of its equations refers to different points in time (Frisch 1936).

Harrod’s idea of equilibrium growth met with a strange fate. In *The Trade Cycle* it was a consequence of the application of the instability principle to static equilibrium, and it was meant to express the possibility of economic growth without the need of postulating the existence of exogenous forces causing it. For traditional theory this was an outrageous upshot: if motion becomes theoretically conceivable on its own, so does crisis, a behaviour of the system which on the contrary the stability of static equilibrium wanted to deny unless determined by causes alien to the normal functioning of economic activity. In the ‘Essay’, the fundamental equation represented a particular state of the system, characterised by no undesired accumulation of stocks or variations in the degree of utilisation of machinery, that is by the absence of incentives to adjust the level of activity. Such a state was not given once for all but was referred to a single point in time; the growth rate describing it could (and indeed, does) fluctuate in the course of the cycle.

Modern readers of Harrod, ignoring this specificity and interpreting his result in the light of Frisch’s notion of dynamics, have understood the fundamental equation as a definition of a growth path. Some of Harrod’s own expressions have indeed contributed to misguide the attention of scholars from the methodological aspect of his considerations on instantaneous analysis. Yet such expressions like “line of output”, “line of advance”, “path of growth” and “line of growth” have to be considered in their context. Harrod used them to specify that the warranted growth was a *moving* equilibrium, and to distinguish it from the static equilibrium he was discussing in those pages (Harrod 1939a, pp. 22-23). Further on, Harrod fully specified that “there is no unique warranted rate; the value of warranted rate depends upon the phase of the trade cycle and the level of activity” (1939a, p. 30). Interpretations of Harrod’s theory as dynamics of trajectories (as in the manner of Frisch’s dynamics) omit to distinguish the two stages of analysis to which Harrod dedicated his methodological essay in 1938, that is, the cartographic study of the system instant after instant, and the subsequent integration of these points in time as a sequence of events (see Chapter VI § 3, and above § 6).

Harrod proposed to keep these stages distinct in order to provide foundations for his dynamics on the same methodological principles as statics. Transposing this procedure to dynamics, Harrod treated the coefficients $s$ and $C$ as the data from which the fundamental proposition about the growth of the economic system is derived; only afterwards, in studying the cycle, were changes in the data considered.

Determining the rate of growth and its stability, Harrod thus transposed to dynamics the *ceteris paribus* clause, referring it to his fundamental conditions $s$ and $C$. Harrod was certainly conscious that strictly speaking they were not parameters but endogenous variables; but since his unknown term (the rate of growth) monopolised his only equation, Harrod needed to treat them as data to be able to solve the equation. His
only way out, given Harrod’s limited mathematical toolbox, was to confine the domain of his analysis to a period of time so short that nothing had time to change, i.e., to a single instant. In other words, Harrod knew that neither $G$ nor $C$ and $s$ were independent variables, but he was forced to ignore it. This seems to be the gist of Keynes’s protest that it is not possible to “assume absolute rigidity of $s$ and $C$ and a departure from warranted growth. You have to make some assumptions as to the changes in $s$ and $C$ in unwarranted conditions” 53 (Keynes to Harrod, 29 Aug., 1938, in Keynes CW XIV, p. 334).

Keynes thus noticed the weakness of the analytical structure of the ‘Essay’. But instead of locating its causes in the peculiarity of Harrod’s method, he thought Harrod was implicitly assuming constancy for his coefficients. However, Harrod explicitly refuted this hypothesis, and while he desperately tried to prove to Keynes that his analysis, in the domain he chose for it, had no need nor desire to adopt it, the sense of Keynes’s criticism escaped his attention. During the entire correspondence, Harrod firmly believed it was not his concern.

Keynes’s misdirected attack led Harrod to reformulate his “Essay” by shifting the emphasis away from the second stage of his argument, relating to the trade cycle, so that the role of the fluctuations in the fundamental conditions $s$ and $C$ in the course of the cycle was overlooked by interpreters who did not have access to the first draft. In fact, although in the final version it was stated quite clearly that the warranted rate of growth is not given once for all but changes in the course of the cycle, this feature was not given much weight. From the manuscript, on the contrary, the non-linearity of the mechanism postulated by Harrod is striking. In Section 15, for instance, Harrod discussed three “forces at work all tending to depress the warranted rate while income falls”, while in Section 16 he considered the forces tending to influence $G$ during recovery 54 (Harrod 1938*, §§ 15-16). Incidentally, it must be noted that the non-linearities were so evident that Marschak, the econometrician, could undoubtedly go further than Tinbergen himself did in his review of the Trade Cycle, where Harrod’s mechanism -interpreted as a linear first order difference equation- was criticised for not being capable of producing fluctuation but only cumulative growth (Tinbergen 1937). Marschak, on the contrary, besides pointing out to Harrod that a linear approximation for his supply and demand functions for saving could not give rise to oscillations, also recognised that there was involved a non-linear mechanism responsible for the periodicity of the solution. He only asked Harrod to explicitly state his postulate regarding the changes of $C$ as a function of actual growth or of the divergence between actual and warranted growth rates 55. As regards the interpretation of Harrod’s cycle mechanism, from the original draft of Harrod’s article Marschak could thus draw a deeper insight than subsequent commentators.

As to Keynes’s criticism regarding the constancy of the fundamental dynamic conditions, Harrod was aware that it brought to the fore some sort of difficulty.
Nevertheless, he thought he could deal with it in the second part of his analysis, that dedicated to the study of long-run changes. Anyway it must be noticed that variations in the coefficient were not an analytical problem for *The Trade Cycle*, but only for the later developments of Harrod’s theory. The quest for an equilibrium state in the 1936 book aimed to show how the combined effect of the two sets of independent decisions regarding saving and investment could justify the expectations on which these decisions depended. Harrod was therefore merely interested in showing the existence of some range of dynamic forces capable of causing such a result in the course of the period of finite length during which the multiplier and the accelerator unfolded their effects.

The main result of the ‘Essay’, the instability proposition, was obtained by comparing the situation of two adjacent instants. If at any point in time entrepreneurs ascertained undesired changes in the utilisation of machinery or in the volume of their stocks, they would slow down the pace of investment, which in turn entailed further accumulation of stocks (or conversely, if stocks at first decumulated). Yet in Harrod’s paper there was no clue suggesting how the two instantaneous states were related, nor any precise suggestion regarding changes in the coefficients $s$ and $C$ due to the variations in the level of activity. In Harrod’s theoretical set-up it is therefore impossible to integrate in a unique picture the entire course of events: one can only recalculate everything instant after instant, or at most try to identify some domains in which changes are qualitatively homogeneous. The instability principle as stated by Harrod does not admit to rigorous proof, but can only be intuitively grasped.

It thus seems to me that the misfortune met by Harrod’s dynamic conception mainly depends on the fact that the interrelations and feedbacks between Harrod’s chosen variables are much more entangled than his equation could possibly represent (and Harrod himself seemed from time to time to have been aware of that), even after expectations disappeared from the scene. This seems to explain why both Keynes and the econometricians could praise Harrod for some of his ideas, but at the same time be dissatisfied by his procedure as a whole.

The econometricians, who were aiming at formulating complete sets of functional equations capable of representing the time path of the unknown magnitudes were, for many years after the publication of the ‘Essay’, incapable of dealing with the intrinsic non-linearity resulting from the feedback between variables. Only since techniques of non-linear analysis became available in economics (as from Goodwin’s 1951 paper), were Harrod’s ideas integrated within the notion of dynamics developed by Frisch. The principle of instability was formalised in the form of the well-known ‘knife-edge’. This approach definitely dismissed Harrod’s dynamic conception, since in the new context the rate of growth of the variables was a by-product of the solution of the functional equation: it was deprived of any analytical value, and was reduced to a component of a trajectory that Harrod refused to calculate (and that his formulation would not allow him to calculate anyway).
On the other hand, Keynes was interested in the problem, deeply rooted in the classical and Marxian tradition and revived by Harrod after many decades of neglect by the neoclassical theory, of whether and under which conditions there exists some form of self-sustained economic growth. The economic system object of Keynes’s concern was one where taking decisions and recording their results took time: in his view, the new decisions actually reflected the newly formed expectations and the disappointment of the old ones. Hence his predilection for the assumption (1b) on the determination of savings, and his concern for the question of the length of the relevant period $\Delta t$.

While in the successive versions of his dynamic theory Harrod remained consistent with the approach outlined in the ‘Essay’, in his discussion with Harrod Keynes preferred to reappraise the problem (and the approach) of *The Trade Cycle*. Faced with the contrasting notions of a dynamics outside of time -because time, when reduced to an instant, disappears-, and the dynamics of the econometricians describing a “changeless change […] in timeless time”58, Keynes could not choose. His discussion with Harrod shows that he was rather inclined towards a dynamics “full of the natural give and take of a world where people must find out, compare, decide, before they act; then register results and make fresh plans and decisions”59. What Keynes held against Harrod in the course of their debate, and what Harrod did not understand, is that in dynamics time must be the ordinary time, full of hopes and disenchantment, enabling one to recognise the importance of the uncertainty regarding the future course of events as distinct from the certitude of the past.

Notes

1 In a letter to Sisam, Harrod described the “Essay” as “an article carrying further my Trade Cycle ides which I think is very good” (14 Aug., 1938).

2 Other motives for misunderstanding or non understanding arose in the subsequent years. However, I am not concerned with them here. See, for a detailed discussion, Besomi 1996a.

3 For this correspondence, see Keynes *CW* XIV, pp. 295-305. For a discussion of the methodological debate, see Chapter VI § 2 above.

4 Harrod 1938a, pp. 402-405; the actual quotation is from p. 403. But already in October 1936 Harrod had presented an address to a meeting organised by the Econometric Society, in which he considered the main problem of dynamics to discuss “what rate of growth can continue to obtain, so long as the various surrounding circumstances, including the propensity to save, remain the same”; the “distinguishing feature of the dynamic theory will […] be […] that it will embody new terms in its fundamental equations, rate of growth, acceleration, de-celeration, etc.” (published as Harrod 1937a, p. 86).

5 On the same day, Harrod also announced to Robertson that the article was “in the hands of the typist”, and that he thought that “it is jolly good” (Harrod to Robertson, 3 August, 1938).

6 In the letter to Keynes accompanying the typescript, Harrod wrote that he was “at the moment more interested in it than in [his] wretched Ad. Presidential” (Harrod to Keynes, 6 August, 1938).

7 The “number of [Marschak’s] detailed suggestions” Harrod incorporated in his first revision are indicated in the editorial footnotes to the first draft of the “Essay” (Harrod 1996).

8 In the first draft of the “Essay”, Harrod referred to a different notion instead of that of the ‘proper’ warranted rate. Both Keynes and Marschak complained that the notion of the ‘normal warranted rate’ was too obscure, and their criticism led Harrod to reformulate his discussion quite radically. I have discussed the implications of the debate on this point in Besomi 1996, where I have also tried to indicate the possible sources of Harrod’s notion of ‘normality’.
The origin of Keynes’s misinterpretation is discussed in Chapter V, note 38.

The analogies are quite easy to spot. In spite of that, most of the recent studies on the evolution of Harrod’s dynamics or on the correspondence between Harrod and Keynes tend to ignore it, for some reason I frankly cannot understand. I refer in particular to Kregel 1980, Varri 1990 and Young 1989. Young in particular preferred to look for unlikely similarities between Harrod’s formula and Kalecki’s or Lange’s formulations, that “may even have catalyzed the discovery of Harrod’s ‘fundamental equation’” (Young 1989, pp. 150-158; emphasis mine).

I do not want to deny that Harrod might have incorporated influences other than Keynes’s. I only affirm: that Keynes’s suggestions are a source with which Harrod was familiar; that before Keynes’s formulation Harrod had not thought of such a representation of The Trade Cycle; that Harrod spent some time thinking about and experimenting with it. “Your algebraic formulation is extremely helpful”, wrote Harrod to Keynes the 15th of April 1937 (in Keynes CW XIV, p. 175).

As far as I know, the only author who did not ignore the similarities between Keynes and Harrod’s formulas was Pugno 1992. Unfortunately, after correctly identifying the analogies between the two equations, Pugno did not inquire into the different analytical and methodological role of the terms of the two equations, and was therefore led to draw quite a different conclusion from the one presented here.

Keynes was always very meticulous about measure. It might therefore seem surprising that he proposed a formula where non-homogeneous magnitudes appear. However, it has to be firmly kept in mind that Keynes’s equation was not meant to summarise Harrod’s book, nor did Keynes disregard the multi-dimensionality of his magnitudes. This results from Keynes’s own specifications provided as a postscript to the same letter in which he suggests the fundamental formula:

\[ y = \frac{100}{M \cdot R - 1}. \]

Moreover, Keynes disregarded the time indices; I have inserted them, for reasons which will soon become apparent.

Keynes 1937*. It is not clear whether Keynes referred the Relation to the ratio of investment and capital on consumption or on income. The former would be more in accordance with Harrod’s use in The Trade Cycle, but the latter would better interpret Keynes’s formalism. I have chosen the latter for it enables me to underline the formal similarity between Keynes’s approach and that of the “Essay”.

To be precise, Keynes expressed his formula in percentage, rather than in absolute terms. It thus read

\[ y = \frac{100}{M \cdot R - 1}. \]

Moreover, Keynes disregarded the time indices; I have inserted them, for reasons which will soon become apparent.

De facto Harrod had thus ignored Keynes’s warnings about the heterogeneity of the magnitudes involved. This was to have further consequences (see § 5 below).

In the “Essay” Harrod explicitly shifted the emphasis on stability. But a price had to be paid for favouring the symmetry of the cumulative process of the widening gap between actual and warranted growth over the asymmetry between boom and crisis: Harrod had to renounce his explanation of the difference between “the gentle nature of the revival” and the destructive character of the crisis.

The exact meaning of the latter term will be discussed below, in § 7.

Here I am only considering a unit period, and therefore I will leave out for simplicity the time index. Later on, however, I will show that such an omission in general can result in unfortunate consequences.

Harrod had thus completed the shift of his concern from the growth of consumption to the growth of income. (On the terminological confusion generated by this shift see below, note 26). This gave ground for Keynes’s criticism that Harrod had not discussed “the distinction between over-production in consumption and over-production in capital goods” (Keynes to Harrod, 17 and 23 Aug., 1938, in Keynes CW XIV, pp. 323 and 330 respectively). In his reply, Harrod acknowledged the importance of the distinction, but explained that in order to introduce it the “Essay” he would have had to run into a book. Keynes agreed that it was possible to ignore it, but only subject to certain qualifications, and therefore suggested to Harrod

\[ y = \frac{100}{M \cdot R - 1}. \]
Harrod only partially followed Keynes’s suggestion, since he omitted to specify that the only changes of output considered were those preserving the balance. Harrod claimed, in fact,

that the ‘acceleration principle’ works in precisely the same way with regard to each category of capital, fixed and liquid, considered separately; … we may lump them together always remembering that a lack of balance between them may set up forces not yet taken into account (Harrod to Keynes, 21 Aug. 1938, in Keynes CW XIV, p. 329)

(And in fact in the first draft Harrod found it convenient to separate fixed from circulating capital in his analysis of the slump; the accelerator was applied to both; see Harrod 1938*, § 15).

In the final version, Harrod thus added the following passage:

It may be well to emphasize at this point that no distinction is drawn in this theory between capital goods and consumption goods. In measuring the increment of capital, the two are taken together; the increment consists of total production less total consumption. Some trade-cycle theorists concern themselves with a possible lack of balance between these two categories; no doubt that has its importance. The theory here considered is more fundamental or simple; it is logically prior to the considerations regarding lack of balance, and grasp of it is required as a preliminary to the study of them (Harrod 1939a, pp. 18-19).

19 Regarding expectations in The Trade Cycle see below, § 6.

20 Harrod had in fact already changed terminology in his methodological essay submitted in 1938 as Presidential address to the Economic Section of the British Association, thus between the two exchanges of letter with Keynes regarding respectively The Trade Cycle and the “Essay” (see Harrod 1938a, 403; see Chapter VI § 3 above).

21 The new terminology was also consistent with Harrod’s new notion of dynamics, defined “as referring to propositions in which the rate of growth appears as an unknown variable” (1938*, §4, and 1939a, p. 17), but not with the notion of moving equilibrium that emerged from The Trade Cycle. There, in fact, the upward and downward phases of the cycle were treated asymmetrically, since Harrod did not feel the need to consider the cumulative aspect of the divergence between “steady growth” (that is, equilibrium growth) and “more than justified” growth, assuming that in both cases entrepreneurs would just feel happy with their past action. At first, Keynes was deceived by the new definition; after having received Harrod’s explanations, he suggested it should be specified that “by warranted you mean exactly warranted” (Keynes to Harrod, August 23, 1938, in Keynes CW XIV, p. 331).

In reply to Keynes’s suggestion, Harrod actually added a qualification that went well beyond a mere restatement, and in the final version of the “Essay” added an arbitrary postulate on entrepreneurs’ behaviour in equilibrium conditions. For a discussion of this alteration and its unfortunate consequences see Besomi 1996.

22 In the paragraphs of the first draft of the “Essay” relating to business fluctuations, Harrod still referred to forces and “determinants” in discussing the variations of the warranted rate of growth in the course of the cycle (see 1938*, especially § 15, where Harrod listed the forces tending to influence the warranted rate during depression, and also referred to the “shift away from profits”, the Trade Cycle’s second dynamic determinant). However, the determination of the warranted rate, that is, “dynamics proper”, did not involve the study of causes. On Harrod’s distinction between these two stages in dynamic theory, see below, § 6, and Chapter VI, § 3.

23 In particular, Keynes was initially misled by Harrod’s use of the term “warranted”; see note 21 above.

24 In one of his last letters to Keynes, Harrod wrote: “You seem to have some substantial objection to my argument which I am quite unable to see […] I cannot comprehend the force of your criticism” (Harrod to Keynes, 22 Sept., 1938, in Keynes CW XIV, p. 342).

25 See especially Harrod’s letters to Keynes of September 6 and 22, 1938. As it appears in Keynes CW XIV, p. 345, the latter is quite perplexing, due to some confusion in Harrod’s formalism and a few mistakes in the transcription of Harrod’s original formulas. The reader of the Harrod-Keynes correspondence might thus appreciate to have some of the formulas rewritten in a somewhat clearer form:

\[
eq 2: \quad c(x+1) = \left[ \frac{x+1}{x} \frac{y}{y+1} \right]_{s_{a1}} (y+1)
\]

\[
eq 3: \quad c(x+1) = s_{a1}(y+1)
\]

\[
eq 4: \quad s_{a1} = \left[ \frac{x+1}{x} \frac{y}{y+1} \right]_{s_{a1}}
\]

(combining 4 and 5):
\[
\frac{x+1}{x} y_s a = y_s a_1 + s_m
\]

\[
s_m = y_s a_1 \left[ \frac{x+1}{x} - 1 \right].
\]

26 The confusion was generated by the fact that in the transition from the Trade Cycle to the “Essay” Harrod shifted the attention from the growth in consumption to the growth in income (see § 4 above). In his very first letter to Harrod on the “Essay” Keynes felt the need to resume the formulation of the acceleration principle that Harrod used in his Trade Cycle, and to introduce into the debate a new notion, the ratio of investment per unit increase of consumption, to be compared with the propensity to save (Keynes to Harrod, 17 Aug., 1938, in Keynes CW XIV, p. 323). After Harrod had clarified that he was reasoning only in terms of total output, Keynes adapted the meaning of his term to that implied by Harrod’s new set-up. Unfortunately Keynes maintained the same symbol, and in the course of the exchange Harrod adapted his symbolism to Keynes’s usage. This has created some difficulties in interpreting the exact terms of the debate, since it has been believed that \( t \) was the forerunner of \( C \) (Kregel 1980, p. 110). It is therefore worth stressing that the symbolism of the new version corresponded to that of the first draft of the “Essay” (except for some symbols appearing in equations that had subsequently been deleted: see Harrod 1938*).

27 Unfortunately too often economists, whether they are mathematicians or not, are not cautious enough about the dimensional nature of their objects. Consider for instance Kregel’s essay on the Harrod-Keynes correspondence presently under examination (Kregel 1980). Having ignored Keynes’s suggestion, he could not have noticed that Harrod and Keynes unknowingly referred to different formulae (as will be explained in the text). Kregel thus somewhat obscured the accurateness of other parts of his insights by confusing, as Keynes and Harrod did, the problems related to instantaneous analysis and stability of equilibrium.

28 Strangely enough, only at the end of the exchange of letters did Keynes give due attention to his own advice. The instability condition \( C > s \) that he had identified since the first letter compared the magnitude of two non-homogeneous quantities. Only in his last letter, when he was already dealing with the existence of equilibrium rather than its stability, did Keynes introduce in his calculations \( \Delta t \), that is “the interval elapsing between new sets of entrepreneurs’ decisions” (Keynes to Harrod, 26 Sept., 1938, in Keynes CW XIV, p. 346).

29 Harrod was conscious of the fact that these magnitudes had different dimensions: see Harrod 1939a, pp. 16-17. However, Harrod did not seem to be able to make appropriate use of this insight, for he soon got confused on how the magnitudes should be composed with respect to time. In fact, he maintained -against the opinion of a mathematical student attending his lectures (this fact is mentioned in a letter to Marschak, 7 Sept., 1938) and of Marschak (Marschak 1938*)- that “the annual increment measured as a fraction of the existing output (rate of growth) is 12 times the monthly increment, because the denominator of the fraction, i.e. existing output is 12 times as great if you take an annual period as if you take a monthly period” (Harrod to Marschak, 7 Sept., 1938, loc. cit.; see also Harrod 1939a, p. 16). This argument was of course wrong, for rates of growth are typically compound magnitudes, obeying to exponential rather than linear laws. His explanation to Marschak and the footnote to p. 17 of the “Essay” reveal that Harrod confused between absolute magnitudes and relative rates of increase; this would of course not have happened if the appropriate time-indices were appended to the magnitudes involved.

30 This suspicion was already raised by Baumol 1948, p. 507n.

31 Keynes had considered assumption (1b) already when proposing his fundamental equation. His formula (5) in fact presupposes that \( \frac{\Delta K}{K_{\Delta t}} \neq \frac{\Delta K}{K} \); in his calculation he uses the first of these rates of growth (cf. § 3 above); the second one would have led to \( y = \frac{1}{M \cdot R} \).

32 The procedure is the following.

Consider first the equation (1a), (2) and (6), and take \( s \) and \( C \) as warranted quantities. By definition of rate of growth, the following equalities hold:

\[ \Delta Y_t = G_{\Delta t} \cdot Y_{t-\Delta t} \cdot \Delta t \]

and

\[ Y_t = Y_{t-\Delta t} + \Delta Y_t = Y_{t-\Delta t} \cdot (1 + G_{\Delta t} \cdot \Delta t) \]

Let \( \hat{G}_{\Delta t} \) be the solution of our system of equation. Substituting (i) in (2), then (1a) and the equation so obtained in (6) there results

\[ \frac{1}{\Delta t} \cdot C_{\Delta t} \cdot \hat{G}_{\Delta t} \cdot Y_{t-\Delta t} \cdot \Delta t = s \cdot Y_{t-\Delta t}, \]

from which
\[ \hat{G}_M = \frac{s}{C_M} \]  \hspace{1cm} (7a)

which is the equilibrium solution found by Harrod. Equation (7a) means that, if at time \( t - \Delta t \) the system was in equilibrium and the parameter \( s \) and \( C \) do not change, then the only increase of income that does not entail unexpected accumulation of stocks is \( \Delta Y_t = \hat{G}_M \cdot Y_{t-\Delta t} \cdot \Delta t \).

Substitute now (1a) with (1b), following the same steps and denoting by \( \hat{G}_M \) the corresponding equilibrium rate. The result is

\[ \frac{1}{\Delta t} \cdot C_M \cdot \hat{G}_M \cdot \Delta t = s \cdot Y_{t-\Delta t} \left( 1 + \hat{G}_M \cdot \Delta t \right), \]

that is

\[ \hat{G}_M \cdot (C_M - s \cdot \Delta t) = s \]

and finally

\[ \hat{G}_M = \frac{s}{C_M - s \cdot \Delta t} \]  \hspace{1cm} (7b)

33 Keynes repeated his criticism many times, and the following seems to me one of its clearest formulations. From equations \( S = s \cdot Y \), \( I = C \cdot \Delta Y \) and \( I = S \), Keynes formulates the equilibrium equality as \( C \cdot \Delta Y_w = s \cdot Y \). “Suppose [...] that the ex-ante growth of output exceeds the warranted growth in the assumed unit of time by an amount \( X \), then (i) say you are assuming that

\[ C \cdot (\Delta Y_w + X) > s \cdot (Y + X) \]  \hspace{1cm} (i)

where \( C \) and \( s \) have the same values as before, i.e. that \( C > s \) (for that unit of time). The longer a unit of time, the less likely is this to be true” (Keynes CW XIV, p. 333).

Here Keynes repeated Harrod’s mistake, that is to say he thought in terms of (1b) but used Harrod’s result based on (1a); Again, the use of time indices would have avoided this error. Equation (i) should be restated as:

\[ \frac{1}{\Delta t} \cdot C_M \cdot (\Delta Y_w + X) > s \cdot (Y_{t-1} + \Delta Y_t + X_t) \]  \hspace{1cm} (ii)

Now the role of the period length \( \Delta t \) is really visible.

Harrod realised that in Keynes’s formulation (i) the term \( \Delta Y_t \) was missing on the right hand side, and thus replied: “In any warranted position \( C > s \), since

\[ C \cdot \Delta Y = s \cdot (Y + \Delta Y) \]  \hspace{1cm} (iii)

\( C \) here stands for the required increment of capital” (Keynes to Harrod, 29 Aug., 1938, in Keynes CW XIV, p. 335).

It was now Harrod’s turn to go on the right track: from (iii) -which presupposes assumption (1b)- he could easily have obtained equation (7b), but he soon got lost, reviving instead the result (7a).

Rather than proclaiming that in equilibrium \( C > s \) is always satisfied, the correct procedure would have been to complete equation (iii) with the appropriate time indices

\[ \frac{1}{\Delta t} \cdot C_M \cdot \Delta Y_t = s \cdot (Y_{t-1} + \Delta Y_t) \]

and then to transform it into

\[ \Delta Y_t \cdot (C_M - s \cdot \Delta t) = s \cdot Y_{t-1} \cdot \Delta t \]  \hspace{1cm} (iv)

The right hand side of (iv) being positive, the two factors on the left must have the same sign, i.e., if \( \Delta Y_t > 0 \) then \( C_M > s \cdot \Delta t \), and if \( \Delta Y_t < 0 \) then \( C_M < s \cdot \Delta t \).

The debate rested anyway on very confusing terms, and neither part could be convinced. In the following letter, Keynes saw the way out of the dilemma (but unfortunately it was too late):

I quarrel with the first sentence in your letter [...], where you write that in any warranted position \( C \cdot \Delta Y = s \cdot (Y + \Delta Y) \). This seems to me to wrong in its dimensions. I suggest that the equation should run \( C \cdot \frac{dY}{dJ} = s \cdot (Y + \Delta Y) \), or if you like \( C \cdot \Delta Y = s \cdot \Delta J \cdot (Y + \Delta Y) \), where \( \frac{dY}{dJ} \) is the rate at which income has increased. \( J \) stands for time (Keynes to Harrod, 9 Sept., 1938, in Keynes CW XIV, pp. 337-338).

34 Stated correctly, rather than \( y = \frac{1}{M \cdot R - 1} \) equation (5) should read

\[ y_{\Delta t} = \frac{1}{M \cdot R - \Delta t} \]  \hspace{1cm} (5a)

Let me remind the reader of Keynes’s claim that if the Relation was constant, the rate of growth of capital would be equal to the rate of growth of income (Keynes to Harrod, 12 April, 1937, in Keynes CW XIV, pp. 336-337).
and its consequences on

\( \Delta = \),  \( i \)

(ii) be the warranted growth rate corresponding to assumption (1b). Then, by definition, the

and eliminating the common terms, the condition for

explicitly determined

might be relevant (even though only to disprove

was and what determined its duration (although in the “Essay”, when taken to task

by Keynes, he considered it to be about six months: Harrod 1939a, 28). Harrod probably thought he was

not concerned with that problem, since equation (7a) \( \hat{G}_{\Delta t} = \frac{s}{C_{\Delta t}} \) is valid independently of \( \Delta t \) (a different

choice of \( \Delta t \) would involve changes only in \( C_{\Delta t} \) and \( \hat{G}_{\Delta t} \), but not in their product. Cf. Harrod to Keynes, Aug. 1938, in Keynes CW XIV, p. 332, and Harrod 1939a, p. 17); he could therefore not imagine any obstacle in reducing it to an infinitesimal.

In Keynes’s result (7b) on the contrary \( \Delta t \) explicitly determined \( \hat{G} \), and he therefore had to ask himself on what it depended: “I am not quite clear what determines the relevant value of \( \{\Delta t\} \) in practice. But it seems to me to depend on the period of production” (Keynes to Harrod, 26 Sept., 1938, in Keynes XIV, p. 347).

Harrod’s proof in the “Essay” provides a further example of the confusion surrounding the choice of the assumptions on savings. Since Harrod allowed that \( \Delta t \) might be relevant (even though only to disprove it), he ought to have distinguished between (1a) and (1b). But, as usual, Harrod reasoned as if the saving equation was (1b) while using the result descending from (1a). His conclusion was qualitatively correct, but the argument needs amending.

Let \( G_{\Delta t} \) be the warranted growth rate corresponding to assumption (1b). Then, by definition, the warranted increase of income \( \Delta \hat{Y} \), is

\[ \Delta \hat{Y} = \hat{G}_{\Delta t} \cdot \hat{Y}_{t-\Delta t} \cdot \Delta t \]  

(i)

Suppose now that income increases more than the equilibrium amount, i.e. that

\[ \Delta Y = \Delta \hat{Y} + X \]  

(ii)

where \( X \) is the absolute value of an experimental increase that occurred as a proportion \( \Delta G_{\Delta t} \) of the old level of income. Equation (ii) might thus be rewritten as

\[ \Delta Y = \hat{G}_{\Delta t} \cdot Y_{t-\Delta t} \cdot \Delta t + \Delta G_{\Delta t} \cdot Y_{t-\Delta t} \cdot \Delta t = Y_{t-\Delta t} \cdot \Delta t \left( \hat{G}_{\Delta t} + \Delta G_{\Delta t} \right) \]  

(iii)

According to investment equation (2), its equilibrium value is calculated as follows:

\[ I = \frac{1}{\Delta t} \cdot C_{\Delta t} \cdot \Delta Y = C_{\Delta t} \cdot Y_{t-\Delta t} \left( \hat{G}_{\Delta t} + \Delta G_{\Delta t} \right) \]  

(iv)

while total saving at the end of the period is

\[ S = s \cdot \hat{Y} + s_m \cdot X \]  

(v)

here \( s_m \) stands for the marginal propensity to consume. Substituting (i) and (iii), equation (v) becomes

\[ S = Y_{t-\Delta t} \left( s + s \cdot \hat{G}_{\Delta t} \cdot \Delta t + s_m \cdot \Delta G_{\Delta t} \cdot \Delta t \right) \]  

(vi)

Equating saving (vi) and investment (iv), after simplifying the following equation results:

\[ C_{\Delta t} \left( \hat{G}_{\Delta t} + \Delta G_{\Delta t} \right) = s + s \cdot \hat{G}_{\Delta t} \cdot \Delta t + s_m \cdot \Delta G_{\Delta t} \cdot \Delta t \]  

(vii)

Substituting for \( \hat{G} \) its value \( \frac{s}{\left(C_{\Delta t} - s \cdot \Delta t\right)} \) and eliminating the common terms, the condition for neutral equilibrium is that
This means that if the marginal propensity to save $s_m$, equaled $\frac{C_{\Delta t}}{\Delta t}$, the total savings would exactly finance total investment, i.e. the experimental increase of income $X$, which would have been a disequilibrium value if $s$ remained constant, would be warranted by the higher marginal propensity to save it triggered.

Equation (viii) is similar to the condition Harrod found in the “Essay” (Harrod 1939, §10), save for the fact that it refers to $\Delta t$ and thus provides the additional information that instability is more unlikely the longer the period, as Keynes maintained (Keynes to Harrod, 26 Sept., 1938, in CW XIV, p. 347) and Harrod recognised (without being able to state it formally).

It is more interesting to compare $s_m$ to $s$, rather than to $C$. To obtain such result it is enough to solve (7b) for $C$ (rather than to express $C$ in form (7a), as Harrod did), and substitute it in (viii). The neutral stability condition then reads

$$s_m = s \cdot \left(1 + \frac{1}{\tilde{G}_{\Delta t}} \cdot \Delta t\right).$$

(ix)

For a reasonable value of $\tilde{G}$, say 2.5 % per year, to invalidate the instability principle the marginal propensity to consume in the period $\Delta t$ should be about forty times larger than its average value, which is decidedly unrealistic.

40 If Harrod’s proof is considered separately from the letters showing he was reluctantly modifying his text as a concession to the editor of the Economic Journal, it might indeed appear that Harrod was not consistent. This is how G.L.S. Shackle interpreted it, in his book on The Years of High Theory (which was written before these letters were published). There we read that it is “when he seeks to analyze the nature of consequences of a departure from warranted growth that Harrod finds the absence of an explicit apparatus of time-lags most inconvenient” (Shackle 1967, p. 261). A few years earlier, Baumol as well was mislead by the lack of an explicit assumption in Harrod’s argument (Baumol 1948, pp. 509-11).

41 It must be observed that not only a changed rate of growth, but any non-zero rate of growth (the warranted one included) entails changes in the absolute amount of income and thus in the propensity to save. The dynamic equilibrium itself thus causes disequilibrium.

Further on, while introducing long range capital outlays in his fundamental equation, that is to say increments of capital not directly dependent on increases of income, Harrod quoted again the elements suggested by Keynes: “there are doubtless numerous factors, including the state of confidence and the rate of interest, affecting the volume of such outlay” (Harrod 1939a, pp. 26-27). Whereas Keynes’s remark was relative to the short run, the distinction Harrod introduced was only meant to reduce the influence of the acceleration principle due to long-period plans.

42 “with regard to ex-ante I do not think it matters using this in a somewhat different sense of Lindhal, since the definition is given” (Harrod to Keynes, 7 Sept. 1938, in Keynes CW XIV, p. 337).

43 Harrod specified that the same definition also applies to ex-ante saving (compare 1938*, § 8, and 1939a, p. 20); this specification was also introduced to meet Keynes’s remark that ex ante saving is surely a chimera, which it is much better not to mention. [...] You are concerned, not with ex ante saving but with the time lag in ex post saving before ex post saving fully adjusts itself to change. Here again I should stick to the phrase ‘warranted’. What you are dealing with is the difference between warranted and actual saving (Keynes to Harrod, 17 Aug. 1938, in Keynes CW XIV, p. 323).

44 This point was also raised by Asimakopulos, who compared Harrod’s 1939 “Essay” with subsequent versions of his dynamics (Asimakopulos 1985, 623).

45 Keynes to Harrod, 29 Aug. and 26 Sept., 1938, in Keynes CW XIV, pp. 334-335 and 349 respectively; see also Tinbergen’s criticism of the rigorous form of the acceleration principle in 1938, p. 166.

46 In the “Essay”, Harrod did not pause to stress that the aim of examining a cross-section was to represent the mutual consistency of the relationships among variables and their rates of change in a steady advancing economy as clearly as he did in his earlier and successive writings (see e.g. Harrod 1934a, p. 287; 1934d, p. 478; 1938a, p. 404; 1959a, p. 451-2; 1960, p. 277; 1963, p. 405). In the light of this continuity and in spite of the shift of emphasis, there is no reason to doubt that this scope was on the background of his argument.
A notable exception - but himself quite an oddball - was Hawtrey. In his comment on Harrod’s “Essay”, he agreed that 

Time is the essence of dynamics. “Time lags” do not adequately express what is involved. A dynamic theory must take into account the respective rates of progress of the various tendencies set up by a state of disequilibrium (Hawtrey 1939, p. 471).

The title itself of this book, *Towards a Dynamic Economics* (Harrod 1948), alleges both the necessity of a new approach to the subject and the general and provisional nature of Harrod’s theory. It is interesting to notice that Harrod originally thought of calling his book *Is Interest Obsolete?*, giving prominence to the content of the last Chapter (Harrod and Macmillan, preliminary Royalty Agreement, February 1947, in HP-IV-E, Box 33: Harrod to Macmillan, 24 February, 1947); only at a later stage Harrod shifted the emphasis from the popular appeal towards his more academic claims on dynamic theory (Harrod to Macmillan, 19 April, 1947).

Domar also stressed that “Harrod’s now famous creation of 1939 […] had to wait for almost a decade and to be repeated in his book to receive its deserved recognition” (Domar 1957, p. 18).

This definition, if broadly interpreted, comprehends not only difference or integral equations but also differential equations.

Like Harrod, Hicks followed a different path from that traced by Frisch, but his definition also differs from that proposed by Harrod. According to him, in economic dynamics all variables have to be dated (Hicks 1950, p. 10).


Analogously, after reminding Harrod that his equation in linear form could not give rise to oscillations, Marshak noted:

I presume that you postulate a change in the form of the functions themselves; in fact, you did discuss changes in the parameter C; could the postulates as to the behaviour of C (viz., its relations to changes in G) be stated explicitly? (Marshak 1938*).

With regard to Marshak’s attempt to represent graphically the various rates of growth, Harrod specified that 

I find it hard to depict warranted growth since it may have various ups and downs within one cycle, being high in the early revival, for instance, because of surplus capacity, and again later on because of high saving (Harrod to Marshak, 7 Sept., 1938).

Marshak 1938*, pp. 4 and 7 (these passages are paraphrased in Young 1989, p. 169; unfortunately, in Young’s rendition the emphasis was shifted from ‘linearity’ to ‘simplicity’, so that it does not emerge that Marshak correctly identified in the assumption of linearity the cause of the incapacity of a system of differential equations to give rise to oscillations). Marshak’s observation is really important, since it shows that the econometricians were already aware of the limits of linear analysis. This suggests that Le Corbeiller’s 1933 *Econometrica* article on the importance of non-linear analysis did not pass unnoticed. However, one may doubt that Harrod’s mathematical knowledge would have allowed him to appreciate the point.

In spite of Marshak’s remark, only privately communicated to Harrod, the non-linear character of Harrod’s trade cycle theory was only noticed much later. Only in 1950, in his review of Hick’s book on the trade cycle, did Goodwin lay the foundations for considering Harrod’s theory as a forerunner of the non-linear cycle models; but the main non-linearity was found in the ‘ceiling’ and ‘floors’ that limited the working of the instability principle (Goodwin 1950: for a more explicit statement, see e.g. Goodwin 1953). Goodwin’s interpretation was at first referred to Hicks’s theory; but on his part, Hicks had recognised that his model characterised by explosive behaviour of the variables limited by a “ceiling” and a “floor” was largely inspired by Harrod’s dynamic theory (Hicks 1950, pp. 6-10). On Goodwin’s interpretation of the history of formal economic dynamics as a cumulative process of progressive introduction of non-linear components, see Besomi 1992.

See e.g. Medio 1979, § 1.3, or Baumol 1948, pp. 509-511, and 1949 for a suggestion of how to make explicit Harrod’s tacit assumptions as to the behaviour of entrepreneurs in face of a disequilibrium. It might be interesting to notice that this criticism was already raised by Marshak in his “Remarks”:

The reaction to the divergence $G - G_w$ as postulated in the “Essay” (the “cumulative process”) may be written, I suggest, as $\frac{dG}{dt} = \lambda(G - G_w)$, where $\lambda$ is some increasing function. Am I right in
understanding this as an empirical assumption of the boom (and depression) rather than a conclusion following from the other equations. In fact, one could imagine the opposite case: a shortage of capital equipment being met by reduction of output rather than leading to an increase. It seems that the definition of the “unstable equilibrium” implies certain empirical postulates regarding the psychology of entrepreneurs’ reactions to current profits made in capital good production. Could this be stated explicitly?

Further, what postulates do you make to conclude that $G$ increases faster than $G_w$?

(Marschak 1938*)

57 It is well known that the possibility of integrating functional equations is restricted to a very limited and non representative set of them, namely linear equations and a few others; in general, solutions are indeed calculated point after point by the use of computers. This remark is therefore not meant to provide a criticism of Harrod’s principle in itself, but only of Harrod’s claim that his result is “rigorously proved”.

58 This forceful expression was originally used by Alexandre Koyré (1965, p. 11) to describe the notion of time in classical mechanics, but it adapts perfectly to the dynamics of the econometricians; the very notion of Frisch’s dynamics, in fact, refers to the analytical instruments which took part in the definition of the object of classical mechanics. For a discussion of this aspect see Besomi 1991.

59 These words were originally used by Shackle for characterising The Trade Cycle (Shackle 1967, 270), but they seem more appropriate to describe Keynes’s conception. For Keynes’s remarks about the limits of Harrod’s notion of ‘expectation’, see footnote 14.
Chapter VIII

Beyond the Traditional Economics

This final Chapter is dedicated to a comparison of Harrod’s dynamics with traditional theory. There are several reasons for concluding a monograph on the making of Harrod’s dynamics with such an operation.

In the first place, Harrod himself was very keen on stressing the points of convergence and of divergence of orthodox economics with his own approach. This was not accidental. On the one hand, it answered the necessity of making Harrod’s thesis understandable to his fellows economists by characterising it with respect to the context defined by the problems it was called to solve and to the alternative solutions proposed by other authors. On the other hand, it satisfied the need for Harrod himself to make the nature and the extent of his departure from tradition clear.

The latter aspect provides the reason for concluding this study with a comparison between Harrod’s dynamics, traditional theory, and the other lines of attack on orthodoxy: Harrod’s own references to the aspects of traditional theory to be preserved or to be rejected seems to me to provide the guiding thread linking his most important decisions concerning the epistemic, methodological and analytical nature of economic dynamics. This Chapter is thus meant to provide the logical complement (along the lines indicated in the discussion on the pre-analytical premises of Harrod’s dynamics in Chapter I § 4.) to the chronological picture given in Chapters II to VII.

A second, and most obvious, reason for devoting a Chapter to the relationship between Harrod and traditional theory lies in the fact that Harrod’s dynamics, like any other theoretical system, was not suddenly born out of nothing, but co-evolved with its intellectual environment. This consisted in the first place of the tradition of thought which nurtured Harrod’s infancy as an economist, and which gave rise to the ideas that ‘ramified in every corner of the minds’ of his contemporaries. The ‘orthodoxy’ to which Harrod referred so often may be identified, in broad terms, with the Marshallian tradition as was currently interpreted in Cambridge in the 1920s, although of course other traditions found their way to Britain and to the current economic and policy debate. In the second place, Harrod’s intellectual environment was populated with the positive contributions of which those years of high theory were so generous, and to which Harrod himself contributed to a great extent. Finally, Harrod also had to face, and indeed reacted to, some critical remarks addressed to traditional theory. I do not think historical research, even if directed to the development of an individual theory, can limit its domain of interest to the internal logical development of the theory and ignore its permeability towards the environment surrounding it.
The intellectual environment which contributed to the shaping of Harrod’s dynamics was in part outlined in the preceding Chapters. The backbone of Harrod’s approach consisted of the methodological principle that the causes of the level of output should be studied before the determinants of its variations, and of the epistemic principle that some destabilising factor ought to be considered at the outset in order to develop a proper theory of economic movement and of the cycle (Chapter I, § 4). This seems to have been conceived quite independently of the state of current debates and of the other theoretical breakthroughs which characterised the early 1930s. But the contrary is true of the genesis and evolution of Harrod’s analytical tools and his dynamic mechanism. Harrod’s interest for the trade cycle was revived in 1933 by the urgency of providing a theoretical explanation (and, of course, a remedy) for unemployment. His first line of approach privileged a reasoning in terms of prices and money rather than output and quantities. So far this was not strange, since the debates on economic policy at the time were normally conducted in these terms (Chapter II, §§ 1 and 2). But from this point on, and for another couple of years, Harrod was left behind in the theoretical developments occurring in the Keynesian field. This emerges in all evidence from the exchanges of views in print and in private correspondence Harrod had in the second half of 1934 with Robertson, Haberler and Kahn on saving and investment. At that stage, Harrod was still arguing on the presupposition that economic advance is governed by the amount of saving and made possible by a sufficient quantity of money in circulation, so that he found Kahn’s approach in terms of quantities and the Keynesian reversing of the relationship between saving and investment extremely paradoxical (Chapter III).

While discussing economic progress in terms he was to abandon by the end of 1935, Harrod had nonetheless laid out his reasoning on the assumption that a mode of regular progress is possible, for which he attempted to set out the conditions. This idea had far reaching implications for Harrod’s dynamics, and eventually ended up at its core, both as providing an alternative to static (motionless) equilibrium and -because of its instability- as a state of the system from which oscillating motion starts off and is kept alive. Harrod, however, acquired and developed this notion in the course of the debates on the economic policy for a socialist government which took place in the bosom of the New Fabian Research Bureau between 1932 and 1934. The young economists engaged in these discussions, in fact, shared the presupposition that the economic system is progressing, in spite of their disagreement on theoretical grounds (broadly speaking, those based at the LSE embraced an Hayekian framework of thought, while the Oxonians upheld Keynesian positions). Therefore, although the ultimate theoretical foundation of this idea for these people probably lay in Robertson’s Banking Policy, so far as Harrod’s dynamics is concerned its specific origin is political, rather than economic (Chapter II § 3).

Harrod acquired the analytical tools for assembling his trade cycle mechanism during 1935. Having learned from the first draft of Haberler’s Prosperity and Depression
of the possible uses of the accelerator for trade cycle theory, during February and March 1935 Harrod experimented with it, raising interesting questions regarding the relationship between investment, the rate of increase of consumption, and technical progress (Chapter IV, § 1). Later on, having read *The General Theory* in proof, Harrod finally became acquainted with the principle of effective demand and the multiplier (Chapter IV, § 2). The mechanism based on the interaction between the multiplier and the accelerator was conceived by the end of 1935, and in January 1936 Harrod had already drafted the core of *The Trade Cycle*.

In this book, the methodological and epistemic principles devised in 1925 and 1934 respectively finally found their way into a theory of the cycle. The structure itself of the book reflects the requirement that the causes of the level of output should be studied before the causes of its variations, the first being studied in Chapter I and the second in Chapter II. Harrod adopted the same procedure in both cases, identifying two sets of independent forces, one inducing entrepreneurs to increase (or reduce) the amount they produce, and the second acting on the system as a whole and determining the amplitude of the mutual repercussions of variations in investment and income. The instability principle was brought into play twice: in the static system, a de-stabilizing force was evoked to neutralise the traditional stabilising factors, in order to make endogenous *movements* of the level of output possible. In the dynamic system, the instability of the moving equilibrium was meant to make endogenous *fluctuations* possible. In the logical structuring of his theory, Harrod explicitly gave priority to the epistemic and methodological principles over the analytical consistency of his model: at places, when he knowingly failed to provide a strict analytical connection between the various compartments of his theory, he evoked a ‘methodological break’ which referred to the determination nexuses entailed by the epistemic (pre-analytic) link between stabilisers and destabilisers (Chapter V).

Some comments Harrod received on his book -Keynes’s, in particular- set him to rethink his approach, and eventually led to the writing of the “Essay in dynamic theory”. This, however, also incorporated the results of Harrod’s reflections on the scientific status of his theory. On the one hand, Harrod had several occasions to discuss the logical status of scientific theories with Frank Ramsey at first, and with Alfred Ayer since 1933. Harrod was therefore aware of the tautological nature of purely deductive theory, and argued that in order to give content to scientific theories it is necessary to provide the basic axioms with an inductive foundation. Accordingly, he was greatly impressed with Mitchell’s visit to Oxford in 1932, and he later participated with keen interest in the empirical research of the Oxford Economists’ Research Group. On the other hand, Harrod borrowed from Lindemann, the Christ Church physicist, the geographical metaphor of the scientific theories as ‘maps’ providing a simultaneous conspectus of the field as a whole, suitable to ‘reduce chaos to order’. The structure of the “Essay” reflects both aspects of this approach. On the one hand, in fact, the axioms from which the theory
develops deductively are either definitory or empirical generalizations, and on the other hand Harrod claimed rigour for his analysis only in so far as it was confined to an instant, admitting it had a tentative character when it speculated on ‘the succession of events’. On this ground, Harrod counterposed his notion of dynamics and the scope of his theory to the approaches ‘based on time-lags’ (Chapter VI).

In the “Essay”, Harrod reformulated his cyclical mechanism shifting the emphasis from the *determinants* of movement to the *effects* of saving and investment decisions. His fundamental equation for the warranted rate of growth describes the instantaneous rate which, *ex-post*, would be found not to give rise to undesired variations of stocks. Such variations, in spite of being logically equivalent to a divergence between saving and investment decisions, were interpreted as causing a further widening of the gap. Harrod was aware that the coefficients in his equation were not given once and for all, but were dependent on the level of income. But he was incapable of dealing with such nonlinearity in formal terms, and he therefore solved the problem by confining the analysis to a single instant (during which coefficients can be treated as constant), while the study of the development in time should proceed by iterating the instantaneous states under the new conditions. Harrod claimed that he had *proved* the instability of the moving equilibrium, which was the fundamental cause of the cycle. However, the lack of an explicit law of connection between successive instantaneous states, and the confusion lying behind a causal interpretation of a logical equivalence, show that the instability principle retains its place as a premise, rather than as a result, of Harrod’s theory of the cycle (Chapter VII).

The preceding Chapters have thus shown how Harrod’s dynamics evolved under multiple influences, plunged into current debates, was stimulated by the needs of economic policy, incorporated assumptions underlying political action, assimilated analytical tools worked out in different traditions and reflected some aspects of a view of science which was currently discussed by philosophers. However, I have not yet discussed the background against which Harrod’s dynamics developed. In Chapters I and V, I have pointed out that Harrod interpreted his methodological premise as indicating the necessity of founding dynamics on the analysis of the individuals’ motives of production. So far, Harrod accepted the traditional viewpoint as to the scope, method and tools of economics. On the other hand, his epistemic criticism of the assumption of stability of equilibrium implied a radical detachment from tradition. It is therefore necessary to explore the relationship between Harrod’s ‘conservatism’, which led him to place statics at the methodological foundation of dynamics, and his ‘revolutionism’, which induced him to think that he went beyond the traditional theory and also beyond Keynes.

This Chapter therefore discusses Harrod’s view on orthodoxy, on the background of the other lines of criticism to the traditional theory which were advanced in those years. This Chapter thus offers a viewpoint on Harrod’s dynamics which complements the previous discussion, and provides the link between Harrod’s early research on imperfect competition and his later reflections on the relationship between statics and
dynamics. For this, I proceed as follows. Section 1 introduces to the atmosphere of criticism which in the 1930s complemented the rich production of high theory and prospects the general lines of Harrod’s reaction to it. The following two Sections are dedicated respectively to an examination of Sraffa’s and Keynes’s attacks on the traditional theory, and to Harrod’s response which consisted in reinterpreting these criticisms as directed to the relevance rather than to the logic of the orthodox analytical structure. Sections 4 and 5 discuss the constructive counterpart of Harrod’s attitude towards Sraffa’s and Keynes’s criticism: the notions of both statics and dynamics were in fact developed in order to define, by a procedure of generalisation, the domain where the traditional toolbox could be applied without running into logical difficulties. This allowed Harrod to place statics at the foundation of his dynamics.

Section 6 considers the role of a criticism of traditional economics Harrod developed of his own, concerning its impossibility of giving rise to an endogenous theory of the cycle, and illustrates Harrod’s 1934 to 1939 attempts to escape this limit. The final Section aims at understanding the ground on which Harrod claimed that he was working towards a truly revolutionary development of economics and provides an evaluation of the conservative elements which his dynamics preserved.

1. Years of high criticism

Harrod’s economic thought did not evolve only by interacting with the positive contributions on the part of his fellow economists and academicians, but was certainly also influenced by the radical attacks launched against the corpus of orthodox theory. The 1930s, in fact, besides being years of high theory, also opened a new era in the criticism of neoclassical economics. Before the 1920s, only a few heretics challenged the established doctrine, but their assaults were quite ineffective. As Keynes noted, either they claimed that observed facts did not support the theory, but forgot that facts were theory-dependent, or they rejected the conclusions of classical economics while maintaining its premises, thus facing the charge of inconsistency (cf. Carabelli 1991, pp. 105-109).

In 1925 an entirely new line of criticism was launched by Piero Sraffa, who attacked the Marshallian theory on its favourite ground, questioning the logical consistency of its core, namely the theory of supply (Sraffa 1925 and 1926). In 1936, Keynes provided a new challenge to traditional economics on logical and methodological grounds. He maintained that the superstructure of orthodox theory rested on some implicit premises that either restricted its generality or were inconsistent with the explicit premises. To clarify his thought and “to persuade economists to re-examine critically certain of their basic assumptions” (Keynes CW VII, p. xxi), in the General Theory Keynes continually stressed the points of difference with traditional theory, making his book a highly controversial one.
Keynes knew that his task was an arduous one, “the difficulty [lying], not in the new ideas, but in escaping from the old ones, which ramify, for those brought up as most of us have been, into every corner of our minds”. Time justified Keynes’s worries. Certainly, economists could not remain indifferent to Sraffa’s and Keynes’s attacks on the traditional doctrine; but instead of renouncing the orthodox approach, they have incorporated in the traditional frame the positive suggestions implied in Keynes’s and Sraffa’s works. Therefore we had on the one hand the theory of imperfect competition, developing Sraffa’s suggestion that the limits to the growth of firms must be sought not in the structure of costs, but in the structure of demand. On the other hand we had the IS-LM approach, reducing the under-employment described in the General Theory to a particular case due to some sort of friction.

Roy Harrod was one of those most impressed by Sraffa’s and Keynes’s criticisms, both of which stimulated new directions for his research, leading in the end to new pieces of high theory. On the one hand, Harrod provided significant contributions to the theory of imperfect competition (for a discussion see Chapter I), which in turn became an important ingredient of his 1936 book on The Trade Cycle. On the other hand, his dynamic theory was deeply influenced by the discussion with Keynes on the General Theory’s criticism of the traditional theory of investment, saving and interest.

To understand Harrod’s ideas correctly, especially those connected with dynamics, it thus seems necessary to examine carefully his reaction to Sraffa’s and Keynes’s assaults on the citadel of nineteenth century orthodoxy. Harrod recognised that their criticism expressed the need to clarify somewhat the foundations of the partial equilibrium approach. But an attack on logical grounds inflicts wounds that could not be healed; Sraffa and Keynes therefore explicitly abandoned the traditional approach to develop one of their own. Harrod did not follow this line, but thought that the traditional approach could still be of use if applied to the appropriate domain, and that the principles on which it was based could be taken as the foundations on which to develop the treatment of the complementary domain. But in order to avoid discarding the whole theory, Sraffa’s and Keynes’s destructive attacks had to be interpreted as applying to the relevance of the assumptions of traditional theory rather than to its logical consistency. This re-interpretation (tacit in the case of Sraffa, explicit in the case of Keynes) was the methodological counterpart of Harrod’s positive analytical construction.

It is not the purpose of this Chapter to provide a detailed examination of Sraffa’s and Keynes’s criticisms, nor to summarise the previous discussion of Harrod’s positive treatment of imperfect competition and dynamics. However, in order to show how Harrod’s notion of marginal returns and his distinction of statics and dynamics were developed as a response to Sraffa’s and Keynes’s attacks to traditional economics, and to examine how Harrod’s general project reflected on the method of his dynamics, I shall proceed to a cursory examination of Sraffa’s and Keynes’s radical attacks.
2. Sraffa’s Criticism and Harrod’s Imperfect Competition

Sraffa’s criticism was directed at the logical consistency of the partial equilibrium approach, for he maintained that the required independence between supply and demand curves for the determination of equilibrium prices and quantities was not respected. Sraffa reasoned in the following terms: in most productive conditions small increases of production entail variations of both the costs of the good considered and of the other commodities, and therefore of the demand of both. This applies to decreasing as well as to increasing returns. On the one hand, “the imposing structure of diminishing returns is available only for the study of that minute class of commodities in the production of which the whole of a factor of production is employed” (Sraffa 1926, p. 539). On the other hand, increasing returns are obviously incompatible with perfect competition, since individual firms would tend to increase indefinitely the quantity produced. Marshall’s solution, based on economies of scale external to the firm but internal to the industry, contemplated the condition most rarely met; “the only economies that could be taken into consideration would be such as occupy an intermediate position between these two extremes; but it is just in the middle that nothing, or almost nothing, is to be found” (Sraffa 1926, p. 540).

Sraffa’s argument showed that in most cases, the interdependence between conditions of production and the demand for a commodity with respect to small changes in the quantity produced was not of the second order of magnitude, but that “a variation in the quantity produced by the industry under consideration sets up a force which acts directly, not merely upon its costs, but also upon the cost of other industries”.

In such a case, the conditions of the “particular equilibrium” which [the assumption of independence] was intended to isolate are upset, and it is no longer possible, without contradiction, to neglect collateral effects (Sraffa 1926, p. 539)

The consequence was straightforward, and Sraffa explicitly stated it in 1930: in a symposium on “Increasing Returns and the Representative Firm”, in reply to Robertson’s conservative attempt to partially rehabilitate the Marshallian orthodoxy, Sraffa concluded as follows:

I am trying to find what are the assumptions implicit in Marshall’s theory; if Mr. Robertson regards them as extremely unreal, I sympathise with him. We seem to be agreed that the theory cannot be interpreted in a way which makes it logically self-consistent and, at the same time, reconciles it with the facts it sets out to explain. Mr. Robertson’s remedy is to discard mathematics, and he suggests that my remedy is to discard the facts; perhaps I ought to have explained that, in the circumstances, I think it is Marshall’s theory that should be discarded (Sraffa, 1930, p. 93).

In his 1926 article, however, after having examined the difficulties of the notion of supply curve in conditions of perfect competition Sraffa suggested to abandon this assumption and to consider monopoly instead. By dropping the restriction of competition being perfect, he allowed increasing returns back on the scene. In this case output was
restricted not by increasing costs, but by increasing difficulty in marketing, and descending demand curves had to be considered for the product of individual firms rather than for the whole market for a commodity (Sraffa 1926, p. 543). Sraffa’s positive proposal found a more fertile ground than his “destructive criticism”, for within a few years some young and brilliant economists had already started exploring the consequences of the assumption of imperfect competition in the full spirit of the partial equilibrium approach (see Moss 1984).

One of the most important bricks in the new building was the notion of marginal revenue that Harrod suggested as early as 1928, which enabled him to state in formal terms the difficulty of marketing increments of produce (for a detailed discussion of Harrod’s contributions on imperfect competition, see Chapter I). In fact he understood that if the demand for the individual source of production is downward sloping rather than horizontal, it is not the average but the marginal revenue that matters in determining the equilibrium production maximising the profits of the firm. Harrod thus propounded the derivation of the marginal curve from the corresponding demand curve.

Harrod was fully aware that the new notion was consistent with Marshall’s partial analysis, and also with Cournot’s treatment of monopoly. Indeed, he had meant it to provide a generalisation of both schemes, since it enabled one to treat the cases of monopoly, perfect competition and all the intermediary conditions by the same method. In Harrod’s eyes, far from being logically inconsistent with its premises -as Sraffa’s critical contention suggested- the traditional approach in terms of demand and supply curves, if properly interpreted was perfectly suited to deal with decreasing as well as increasing returns. In fact if the demand curve was inclined, the marginal revenue curve could intersect the marginal cost curve in correspondence with the decreasing section of the average cost curve, therefore defining equilibrium at a point below the technically optimal capacity of plants.

According to Harrod then, the inadequacy of the Marshallian approach did not lie in its analytical method, but in assuming a restrictive condition as to the structure of the market. Harrod’s solution enabled the removal of this unnecessary hypothesis and the treatment of the entire domain from monopoly to perfect competition by means of the same analytical instrument. There was therefore, in Harrod’s view, continuity in a double sense between these two extremes: a theoretical continuity, and continuity in the reality of the market, since the elasticity of the demand curve could assume any value between the maximum defined by perfect competition and the minimum corresponding to perfect monopoly (see Chapter I § 1). Sraffa’s criticism was thus neutralised: far from invalidating the Marshallian approach, in Harrod’s view imperfect competition and monopoly rather provided a new and broader domain to which its toolbox could be successfully applied.

It is, however, necessary to observe that Harrod could resolve the situation in favour of the traditional approach only by changing the methodological status of Sraffa’s
argument. Harrod recognised the existence of a hidden assumption implicit in Marshall’s own treatment, but in his view this only restricted its validity to the particular case where there was no need to distinguish between marginal and average revenue (for they were the same), and by no means implied logical fallacy. In other words, Harrod’s solution to Sraffa’s criticism consisted in generalising the traditional theory of value: Marshall’s treatment of perfect competition and Cournot’s approach to monopoly were interpreted as particular and extreme cases for economic analysis, just as monopoly and perfect competition were the extremes to be observed in the reality of the market\(^8\). Harrod’s notions of equilibrium, profit and optimal employment of plants did not differ from the orthodox ones, but rather amended them: the marginal principle was made universally true (see Harrod 1928*, and Harrod 1930, p. 239), and ready to be put at the foundation of any further inquiry regarding the determination of the level of output on the basis of the maximising behaviour of entrepreneurs.

But before discussing the implications of this conclusion for Harrod’s notions of statics and dynamics, it is necessary to examine his reaction to Keynes’s criticism to the traditional theory of interest.

3. Harrod and Keynes on Saving, Investment, and the Rate of Interest

The main topic of concern in the debate between Harrod and Keynes on the galley proofs of the *General Theory* was the orthodox notion of the rate of interest as the price equating the supply and demand of capital. Keynes discovered in the orthodox theory a tacit premise\(^9\) in the assumption that income is constant, and in the *General Theory* claimed that this hypothesis “is inconsistent with the assumption that these two curves can shift independently of one another”, and thus the theory “involves formal error” (Keynes CW VII, p. 179, italics mine).

While Harrod appreciated the “positive doctrine” of books III and IV, he did not agree with the violence\(^10\) and the content of Keynes’s attack on traditional theory:

the view that I object to lies in the argument that because saving must always and necessarily equal net investment (which I accept) there is ‘no sense’ in the view that interest is a price which equates the demand for saving in the shape of investment to the supply which results from the community’s propensity to save (Harrod to Keynes, 1 Aug., 1935, in Keynes CW XIII, p. 530).

Harrod contended that the classical theory of interest was not nonsense -that is, in Harrod’s words, “inconsistent or confused on its own premises” (Harrod to Keynes, 21 Aug., 1935, in Keynes CW XIII, p. 546)- but rather simply erroneous, being founded on a false premise: “the classical theory […] is invalid but not nonsense” (Harrod to Keynes, 12 Aug., 1935, in Keynes CW XIII, p. 540 and passim.). In fact Harrod agreed with Keynes that the orthodox theory implicitly assumed that income was constant. But Harrod interpreted this hypothesis as one of the *ceterisparibus* clauses that are necessary for determining the equilibrium price level (in this case, interest) from
demand and supply curves (namely, investment and saving). He thus concluded as follows:

Now you are perfectly justified in saying that the amount of saving is so clearly related to the level of income that to cover the level of income by the cet. par. clause is to refuse to examine the problem. But when you do examine the functional relation all sort of funny things (including the very important one that variations in the propensity to save may be offset by variations in the level of income in such wise that they have no effect on the rate of interest) appear. But you are not justified in denying to classical theory a logical and water-tight view, albeit one which neglected the most important features in the situation (Harrod to Keynes, 1 Aug., 1935, in Keynes CW XIII, p. 532).

Harrod implied that the traditional theory maintained its cogency if one supposed that the level of income was constant, while Keynes maintained that within the framework of traditional theory it was not possible to find a remedy for the omission to consider changes in the level of income:

The fault of the classical theory lies, not in its limiting its terrain by assuming constant income, but in its failing to see that, if either of its own variables (namely propensity to save and schedule of marginal efficiency of capital) change, income must also cet. par. change; so that its tool breaks in its hands and it doesn’t know and can’t tell us what will happen to the rate of interest, when either of its own variables change.

I say, therefore, that it is nonsense to assume at the same time that income is constant and that the propensity to save and the schedule of the marginal efficiency of capital are variable (Keynes to Harrod, 15 Sept., 1935, in Keynes CW XIII, p. 559).

Given that Harrod agreed with Keynes as regards the existence of a hidden assumption in the neoclassical interest theory, what was the nature of the quarrel between the two men? The strife regarded the place of this premise in the orthodox analysis. For Keynes, its presence was essential for the classical analysis, yet it was in contradiction with other assumptions. Keynes maintained that saving and investment functions were not in pari materia, i.e., they belonged to different planes in space and thus could not intersect. Only “if one assumes that income and employment cannot change” (Keynes to Harrod, 14 Aug., 1935, Keynes CW XIII, p. 541) could the two functions be flattened on the same plane; but

the assumption that income is constant is inconsistent with the assumption that these two curves can shift independently of one another. If either of them shift, then, in general, income will change; with the result that the whole schematism based on the assumption of a given income breaks down (Keynes CW VII, p. 179).

Whereas Keynes’s criticism regarded the logic of the orthodox theory, Harrod accepted the details of Keynes’s argument but recombined them with a view to attacking the relevance of the traditional analysis. In Harrod’s view, the supply and demand analysis has to be discarded not because the two curves are not independent, but because it fails to satisfy the ceteris paribus condition with regard to the level of income (Harrod to Keynes, 1 Aug., 1935, in Keynes CW XIII, pp. 531-2). For Harrod therefore the
problem with the classical theory was only its lack of generality, and not its consistency. He repeatedly stressed this point in the course of the correspondence:

If there was some mechanism for securing constancy of income, which the old classical doctrine assumes (i.e. it implicitly assumes constancy of income and does not envisage the rate of interest as the mechanism for keeping incomes constant) then the classical doctrine that it is the rate of interest which equates the propensity to invest to the propensity to save would not only make sense but also be true (Harrod to Keynes, 12 Aug., 1935, in Keynes CW XIII, p. 540; cf. also pp. 553 and 554).

Here of course the analogy with Harrod’s reaction to Sraffa’s criticism is precise. In both cases Harrod recognised the existence of undeclared assumptions in the orthodox analytical apparatus, but refused to admit that the tacit premises were inconsistent with the other hypotheses; he only admitted that the presence of such assumptions restricted the validity of the theory to a special case. The final parallel to be drawn regards the positive part of Harrod’s contribution. In the case of the theory of prices, he laid the foundations of a general theory of value of which Marshall’s analysis represented the special case corresponding to the assumption that competition was perfect. As regards the theory of investment, saving and the rate of interest, the development of Harrod’s thought led to a redefinition of statics and dynamics.

4. Statics as a Generalisation of Traditional Theory

In Harrod’s interpretation, Sraffa’s and Keynes’s criticisms pointed out two intrinsic limits of traditional theory: on the one hand its failure to account for the restriction to the marketing of goods due to the rigidity of demand, on the other hand its incapacity to account for saving, investment and the rate of interest. Both Sraffa and Keynes saw this failure on the logical and methodological ground, and thus inferred that the partial equilibrium analytical apparatus had to be discarded. Harrod, on the contrary, having shifted these criticism to the domain of relevance, in both cases implied instead that by removing the limiting assumptions the logical consistency of the traditional theory would not be undermined.

Accordingly, Harrod operated two distinct but interconnected operations of generalisation. As a first step (and this will be the purpose of this Section to consider) he re-defined the domain of orthodox theory so as to include the whole range of possible market conditions from perfect competition to monopoly. After considering Keynes’s criticism, Harrod also confined the possible scope of orthodox analysis to a sphere where changes in the level of output were not admitted. Within these limits, Harrod thought the traditional analytical toolbox to be capable of application without generating inconsistencies. The next step (to be examined in the following Section), was that of exploring the world beyond the limits of application of strict traditional theory, the generalisation consisting in allowing income changes and considering its consequences and conditions.
Having established the domain where the traditional toolbox could be applied, Harrod had to adapt them to his scope, for he also wanted the factors considered by traditional theory to allow (by being consistent with) fluctuations in output; therefore he also reformulated the traditional analysis in terms of ‘forces’ and imposed the neutrality of equilibrium (for a discussion see Chapter V, § 2). In this revised but still clearly recognisable form, traditional theory was ready to be renamed as *statics*, and as such included in *The Trade Cycle*’s global analytical framework. There, its place was that of accounting for the individual desires, plans and constraints in the determination of the level of output and for the permanence of these motives and their consequences when other forces determined a general economic advance or recession:

It is the purpose of what follows to explain the shift of prices and profits in the course of the cycle in a way which is conformable with the general theory of value without introducing hypotheses of time-lags, miscalculations, errors of judgement, or inflation (or deflation) on the part of the banks, &c. (Harrod 1936a, p. 75)\(^\text{17}\).

Of course the treatment of these problems was not strictly necessary as a premise of the dynamic analysis: the multiplier-accelerator model could work satisfactorily without referring to the individual desires and cost functions. In fact, Harrod himself in the “Essay” centred his attention on the dynamic part of his theory disregarding statics. In his book, however, Harrod attributed pride of place to statics, undoubtedly because the treatment of motives satisfied his methodological belief that *before* discussing the causes of change of output one ought to determine the causes of its level (for a detailed discussion of this point see Chapter I, § 4), and his pre-analytical opinion regarding what economics was all about.

The reasons for this choice, in truth, were only hinted at in *The Trade Cycle*\(^\text{18}\); however, Harrod paused on his methodological premise on many occasions in private correspondence. To Hubert Henderson —to whom Harrod had sent the first draft of his book—, who was “fundamentally unsympathetic” to the assumption that “each entrepreneur is endeavouring to maximize his ‘monopoly net income’” since “as an instrument of trade cycle analysis, it is too unreal to be legitimate” (Henderson to Harrod, 21 Feb., 1936), Harrod explained that “the parts about elasticity of demand merely attempt to provide the static background in terms of most up-to-date theory” (Harrod to Henderson, 21 Feb., 1936, emphasis mine):

My section on profits and prices [Harrod 1936a, Chapter II, Sect. III]. I want to emphasize that this is independent of the general theory of the cycle advanced in the book. None the less I attach considerable importance to it. It accounts in a coherent way for a number of observed facts on the assumption that entrepreneurs endeavour to maximize their profit (which of course includes maximizing the prospect of future profit).

You say -entrepreneurs dont endeavour to maximize their profit. Of course they dont. That is a fact we must take account of. But we cant take account of it to much purpose until we have reduced the ways in which they deviate from the path of self-interest into some sort of systematic form. The maximizing profit formula is only a first approximation. I should urge that it is
that -that by trial and error and by all sorts of funny rules of thumb and mystic formulae that is the leading thing that entrepreneurs are trying to do. And that to work out what will happen if they do that is not a wholly useless occupation. And when the consequence of doing this leads to results which conform in a number of respects with observed facts, one may be encouraged that the first approximation method is not wholly fruitless. I am all in favour of getting a closer approximation. But that is not possible without further sifting, analysis and sorting of the facts. I see no reason to put the results of my first approximation in the waste paper basket, especially when they work out so nicely (Harrod to Henderson, 23 Feb., 1936).

To Keynes, who could not understand why Harrod called “those forces stabilisers which [...] are those which cause prices to fluctuate” (Keynes to Harrod, 12 April, 1937, in Keynes CW XIV, p. 173), Harrod explained:

The reason why I call the forces which make prices to fluctuate stabilizers, is that I want to bring level-of-output theory into relation with the system of cost and utility equations on which the orthodox general theory has -in a way rightly- reposed. The level of output ought prima facie to depend on the desire for goods, for leisure etc. In the Crusoe economy it would so rest. Now that in a growing and capitalist society the level of output depends on the interaction of the Relation and the Multiplier, what has happened to those fundamental forces? What has happened to the utility functions? They are still there, I say. But they are overcome by price fluctuations. The factors having agreed to bargain in money, their natural inclination to relate their work to certain fundamental desires is counteracted by the fluctuation of prices. The amount by which prices have to fluctuate to secure given changes of output measures the force of the stabilizing forces. The static system of equations, including the money equation, provides a field of neutral equilibrium within which output may fluctuate in accordance with the laws of growth (Harrod to Keynes, 15 Apr., 1937, italics added).

Finally, Harrod also argued out the matter with Robertson. Harrod’s letter of Dec. 25, 1936 is particularly important in this respect, for he discussed the relative places of statics and dynamics with explicit reference to Keynes’s *General Theory*. Having reiterated the point that “the doctrine of what governs the volume of saving and the rate of interest must be thrown out bag and baggage” from static theory, Harrod maintained however that “the static system retains its place as a foundation” (emphasis added). He then stressed that in spite of the limits of statics, “among other things I have wanted in my book to throw out a salvage line in the situation. It is for this reason that I attached a certain importance to my chapter one, which Joan R[obinson]. finds so fanciful”19.

5. Dynamics as a Complement to Statics: A further Generalisation

In order to understand how Harrod’s statics, in the form of a ‘general theory of value’20, could be placed at the foundation of dynamics, it is necessary to pause on the overcoming of the second assumption limiting the domain of application of the orthodox toolbox.

The problem was that of identifying the conditions implying constancy of income. Harrod found the reply in the *General Theory* itself, for he accepted the doctrine of the multiplier which he intended in the sense that new net investment was the main determinant of the increase of income. In the negative, if there was no investment, income would not change. Since investment is equal to saving, the requisite for income to
remain constant was simply that in the economic system there were no net saving and investment. If this condition was satisfied, according to Harrod there would be no objection to the application of the traditional analysis. In this case, of course there did not remain much scope for trying to determine the rate of interest from saving and investment curves, and in fact Harrod maintained that it had to be determined otherwise. What is important to note here is that Harrod had identified a realm where Keynes’s attack on the logical consistency of the orthodox toolbox did not apply, and named it statics:

The static system provides an analysis of what happens where there is no increase which entails […] that saving = 0 (Harrod to Keynes, April 1937, in Keynes CW XIV, p. 164).

This analysis is appropriate to a society that does not accumulate (Harrod 1936a, p. 150. See also Harrod 1937a, p. 497).

Although in the domain of statics any reference to economic growth had to be avoided, it was still of some interest to inquire into the system of relationships and laws characterising it:

static theory will be elaborated on the assumption that there is no growth and no saving. The assumption that people spend the whole of their income will be rigidly maintained. On this basis it will be possible to evaluate the equilibrium set of prices and quantities of the various commodities and factors, excluding saving (Harrod 1937a, p. 86).

Statics was thus concerned with the determination of the equilibrium level of income. Harrod’s analysis then proceeded by identifying the motives that would stimulate or inhibit production given certain circumstances, and next by considering “the way in which these forces hold that level of activity in equilibrium” (Harrod 1936a, p. 5 and passim).

On the other hand,

saving necessarily involves growth; for an increase in the amount of capital goods involves an increase of productive power. In order to determine an equilibrium volume of saving, it is necessary to take this factor of growth explicitly into account. It is no longer appropriate to ask -as in the case of a particular commodity- what amount of saving will be justified on the assumption that the surrounding circumstances remain the same within the period in which the equilibrium is established. For the saving itself entails a change of no little importance in the surrounding circumstances, viz. a growth of productive power (Harrod 1936a, pp. 166-167).

Thus, alongside static theory, according to Harrod a second department of economics had to be developed, whose subject matter should be the equilibrium rate of growth of the economy:

In the second department, dynamic theory, growth and saving will be taken into account. Equilibrium theory will be concerned not merely with what size, but also with what rate of growth of certain magnitudes is consistent with the surrounding circumstances (Harrod 1937a, p. 86).

Harrod’s early analytic definition of dynamics, jointly with and opposed to the notion of statics, therefore originated in the reflections stimulated by Keynes’s criticism of the orthodox theory of the rate of interest, as Harrod himself implicitly acknowledged
in the Preface to his book\textsuperscript{22}. \textit{Statics} in fact was defined as the domain in which the orthodox analysis could be applied, for the conditions that assure constancy of income held, while \textit{dynamics} was conceived as the \textit{complementary} and more \textit{general} domain, where changes of income could be considered.

In order to complete the picture of Harrod’s notion of dynamics as a generalisation of statics, it is necessary to stress that after 1938 Harrod referred the economic notions of statics and dynamics to the use of the terms in physics\textsuperscript{23}. He suggested -almost parenthetically, at first\textsuperscript{24}- that by analogy statics should be concerned with a state of rest, while dynamics should consider steady rates of increase of demand:

There is no law, save that of expediency, governing the use of terms. Statics and dynamics, however, are borrowed from another science, and analogy may give some clue to the usage which will put us on the best line of advance. Statics is concerned with a state of rest, and in economics this is achieved when the determinants of the equilibrium, tastes, resources and expectations, retain constant values. The system is at rest because prices and quantities produced remain constant. In dynamics, I suggest, we should postulate first a constant rate of change, and then, when our theory becomes strong enough to stand it, a changing rate of change, in the fundamental determinants. The resultant continued movement of prices and quantities produced under the influence of these forces would then be demonstrated. Statics would become a special case of the more general theory, namely, when the rate of change in all determinants was zero. Such a usage would not only preserve the analogy with mechanics, but would, in my judgment, be useful in directing investigation in a profitable direction (Harrod 1939f, p. 299).

During the same year, Harrod expressed the same opinion also in an essay, published in French, heading “Towards a Dynamic Theory”:

L’analogy entre les relations de la statique et de la dynamique en économie et celles de la statique et de la dynamique en mécanique serait alors précise. Les conditions fondamentales dans notre étude seraient les taux positifs ou négatifs de croissance de certain variables, et l’état de repos serait un cas spécial, tout aussi bien que l’état nul de vélocité. […] Si l’accroissement uniforme d’une variable donné était considéré comme analogue à la vélocité en physique, et que pussent être formulées une série de lois gouvernant la vélocité, alors il est probable que des propositions relatives aux accélérations et aux ralentissements s’ensuiveraient comme corollaires. Si l’on puovait montrer qu’un système positif de cette espèce pourrait être construit, sa prétention à s’approprier le terme ‘dynamique’ serait inexpugnable (Harrod 1939c, pp. 164-5).

[The analogy between statics and dynamics in economics and in mechanics would then be precise. The fundamental subject of our study would be the positive or negative rates of growth of certain variables, while the state of rest would be a special case, exactly as it is in the case for the state in which velocity = 0. […] If we considered the uniform rate of growth as analogous to velocity in physics, and if it were possible to formulate some laws governing velocity, then it would be likely that some propositions regarding accelerations and decelerations would ensue as corollaries. If it could be shown that such a system may be constructed, it could claim the full right to be called ‘dynamic’] (my translation).

These remarks may suggest that Harrod’s reflections on the possible analogy with mechanics may have contributed to the formulation of his notions of statics and dynamics: after all, some forms of this analogy were already evoked in Harrod’s 1934
essay on credit in a progressing community and in the Trade Cycle. I submit, however, that this conclusion should be considered with some care. In the first place, the concern of both the passages cited above and the 1934 analogy with a train travelling at constant velocity and a train accelerating from rest (Harrod 1934d, p. 478) was the methodological opposition of steady vs. per saltum change. In the Trade Cycle, ‘velocity’ and ‘acceleration’ were mentioned as the fundamental objects of inquiry in opposition to the econometricians’ attention for lags, compared to ‘frictions’ (Harrod 1936a, p. viii). Secondly, from the passages mentioned above (which are the first fully developed expression of the analogy) it is far from clear whether Harrod’s suggestion, that the constant rates of change should be considered as a first step and changing rates of changes as a second step, was developed on the grounds of an analogy with the successive moments of the variables, or rather on the grounds of the intrinsic analytical difficulty of the operation. In either case, Harrod never explained the link he saw between the derivatives of the variables considered in mechanics, and the rates of growth he considered himself. Finally, Harrod implicitly linked the necessity of a notion of dynamics in terms of steady rates of variations to the notion of the ‘advancing community’ which constituted the background of the 1934 debates on credit (see Chapter II § 3):

Such a development of theory is suggested not only by analogy with mechanics, but also by the facts of our economy. For we are concerned with societies in which population is continuously increasing (or decreasing) and capital accumulating. It is rather absurd that we should continue to consider these phenomena in our analysis as successions of discontinuous changes. Professor Hicks has a chapter on capital accumulation, but here again his analysis is in the terms of a once-over injection, tracing its successive repercussions, rather than in terms of a steadily continuing increase (Harrod 1939f, p. 300).

Similarly, in the accompanying essay:

Mais dans notre théorie générale, pourquoi devrions-nous nous confiner dans la considération d’un montant invariable de demande, pour les chaussures mettons; pourquoi ne considérons-nous pas également une demande constamment croissante de chaussures? L’accroissement de la population suggère qu’il serait tout à fait légitime de considérer cela comme une condition normale. Il serait alors de notre devoir de considérer les mutuelles relations de variables qui augmenterait conformément à quelque loi.

[But why should we confine our general theory to considering a given amount of demand, e.g. for shoes? Why do we not consider analogously a steady growing demand for shoes? Population growth suggests that it would be legitimate to consider such a state as a normal condition. We should then consider the mutual relationships among variables growing according to some law.

... Population, the volume of capital and our power over nature all increase. Growth is sometimes irregular; but a conceptual system considering as normal a
uniform rate of growth would provide a better approximation to reality than one considering rest] (my translation).

Unfortunately, it is rather difficult to evaluate the precise extent of the influence of the analogy with mechanics on the development of Harrod’s notion of statics and dynamics. Certainly, since his first hints in 1934 Harrod invested some energy trying to give full development to this analogy, other aspects of which, namely the formulation of both statics and dynamics in terms of forces, were given pride of place in the *Trade Cycle*. Nonetheless, I suspect that it was more the result of an *ex post* rationalisation of a development which took place along a different line, rather than the originating cause of this development, although the possibility of economic dynamics paralleling physics may have helped Harrod in giving shape to certain of his decisions. Unfortunately, the surviving evidence does not help to decide in favour of either alternative, for the rare references to physics in Harrod’s unpublished writings and correspondence regard the state of the discipline rather than analogy of notions or analytical instruments.

The fact that the analogy with physics was fully developed only relatively late, when the first draft of the “Essay” was practically ready, suggests not to give it too much weight as a factor directly determining the development of the notion of a *general* theory including statics as a special case. This in fact was fully recognisable (and for the first time) since the *Trade Cycle* and the paper on “Mr. Keynes and Traditional Theory”, that is shortly after the debate with Keynes on the logical tightness of the orthodox theory of interest.

It is now time to turn to our main theme, and examine a further criticism to traditional theory which Harrod developed himself since 1934.

6. Instability as a Premise of Harrod’s Dynamics

Harrod’s considerations on traditional theory were not exhausted by the ‘salvage line’ he threw out to rescue the aspects of the partial equilibrium approach that could be saved from Sraffa’s and Keynes’s attacks, and by his posing the ‘generalised theory of value’ at the methodological foundation of dynamics. In fact, besides coping with the rigidity of the demand curve and with the assumption of constant income, Harrod had also developed a line of criticism of his own, regarding the incapacity of the traditional approach to equilibrium economics to give rise to a proper theory of the cycle.

Harrod’s criticism concerned the association between equilibrium and rest. On the one hand, he argued that if equilibrium was stable, no movement would be possible, unless one assumed that the ‘fundamental conditions’ (individual preferences, costs, etc.) regularly fluctuated in the course of the cycle, and caused it. But such an approach would be incorrect, for it would simply shift the need for an explanation of the causes determining the oscillation in the fundamental conditions. On the other hand, Harrod also refused to draw the conclusion that rational economic behaviour would be incompatible with output changes (for a detailed discussion see Chapter I, §§ 3 and 4). Harrod was
thus facing the epistemic necessity to provide a further re-interpretation of the orthodox principles of maximisation of utility, so that they could possibly give rise to some sort of instability and thereby allow output fluctuations.

In 1934, he thought that the factor responsible for the stability of equilibrium was the assumption of perfect competition, and therefore suggested that consideration of imperfect competition could solve the problem of the theoretical possibility of fluctuations (for a discussion of this aspect, see Chapter I, § 3). This early approach, however, failed to account for the necessity of the cycle.

In *The Trade Cycle*, Harrod tackled the problem by superimposing on the determinants ‘causing the level of output to be what it is’ some additional and distinct factors determining output variations. Harrod argued that at any time the two sets of determinants had to be compatible with each other, and concluded that in the whole field of variation the entrepreneur’s considerations inducing (or deterring) further expansion (or contraction) of output had to balance each other. Harrod recognised in the monetary factor the determinant compensating the resistance to change exerted by cost and utility considerations. He maintained that the dynamic forces, besides determining variations of output, also induced a change in the price level which would exactly balance the stabilising effect exerted by the other static determinant. In order to be able to represent in a comprehensive scheme all these different sorts of considerations, Harrod renounced the formulation in terms of demand and supply curves, and resorted to a different analytical device. The choice fell on a representation in terms of independent vectorial forces, some — the static determinants — acting on the individual entrepreneur’s decisions and others — the dynamic determinants — regarding the working of the economy as a whole. Harrod could thus examine the stability of static equilibrium in terms of the resultant of the static forces, and extend by analogy the same procedure to dynamics. The dynamic (moving) equilibrium was characterised by the steady rate of growth which resulted from the balancing of the dynamic forces; oscillations departing from this state were permitted by its instability.

A similar mechanism was preserved in the “Essay”: the trade cycle mechanism consisted in cumulative departures from equilibrium growth, which were allowed by its instability. But while in *The Trade Cycle* Harrod recognised that the requirement of instability was a premise which guided both the analysis of equilibrium growth and the construction of his trade cycle mechanism, in the “Essay” he rather seemed to believe that instability was an analytical result of his model. In fact, in correspondence with Keynes, Harrod insisted in maintaining that he had “rigidly proved” his proposition regarding the instability of the instantaneous warranted growth rate (Harrod to Keynes, 22 Sept., 1938, pp. 342-3), and in the “Essay” referred to his “demonstration of the inherent instability of the moving equilibrium” (1939a, p. 24). In reality, as I have pointed out in Chapter VII, §§ 4 and 7, the proof was far from rigorous. Harrod in fact maintained that a divergence between actual and warranted (equilibrium) rates of growth brought as a
consequence a discrepancy between actual and desired increases in capital goods and thus an undue depletion (or accumulation) of stocks, while in analytical terms a difference between $G$ and $G_w$ is logically equivalent to a difference between $C_p$ and $C$.

The pivot of Harrod’s argument was that the unplanned variation in the volume of stocks induced further expansion or contraction of output, thereby giving rise to a cumulative accretion of the original divergence between actual and equilibrium growth rates. On the one hand it must be stressed that, as Keynes’s comment implied (see Chapter VII, § 6) and several commentators have noticed, this assumption was not explicitly incorporated in the model, which therefore lacked a mechanism connecting one state of the system to the next. On the other hand, it is necessary to remark that Harrod was mistaken in treating the divergence between $G$ and $G_w$ as the cause of the undesired accumulation of stocks, because it was -in virtue of Harrod’s definitions of the warranted quantities- the undesired accumulation of stocks.

The reader will surely have noticed that the latter comment recalls the criticism aroused by Keynes’s *Treatise on Money*, that the divergence between saving and investment was not the cause of the divergence between prices and costs, but it was the divergence between prices and costs. This is by no means an accident, for Harrod’s argument not only resembled Keynes’s, but was explicitly meant to rescue the *Treatise’s* reasoning in terms of the disequilibrium between saving and investment.

The whole argument of the “Essay” was in fact based on the distinction between *ex ante* and *ex post* notions of saving and investment. By definition, *ex post* saving is equal to investment. This equality between aggregate values is ensured, independently of the particular decisions of individual consumers and entrepreneurs, by the unwanted accumulation (or depletion) of stocks (which, by bookkeeping definition, are included in investment) in the hands of the entrepreneurs and shopkeepers. But Harrod believed that undesired variations of stocks are interpreted by entrepreneurs as a signal that their decisions are in some way inadequate; they therefore provide a stimulus for a change in their expenditure policy. He thus saw in these unjustified variations of stocks a factor of disequilibrium, and by contrast he defined ‘equilibrium’ as situation in which there is no undesired accumulation of stocks. He labelled the value of investment giving rise to such a situation (given the decisions to save) as ‘*ex ante* investment’. Obviously there is no reason why *ex ante* and *ex post* investment should coincide, nor therefore why saving should equal *ex ante* investment. Harrod interpreted this divergence as giving expression to the *Treatise’s* mechanism explaining the cumulative deviation from equilibrium in terms of the difference between saving and investment:

saving is necessarily equal to [...] *ex post* investment. Saving is not necessarily equal to *ex ante* investment in this sense, since unwanted accretions or depletions of stocks may occur, or equipment may be found to have been produced in excess of, or short of, requirements.

If *ex post* investment is less than *ex ante* investment, this means that there has been an undesired reduction of stocks or insufficient provision of productive equipment, and there will be a stimulus to further expansion of
output; conversely if \textit{ex post} investment exceeds \textit{ex ante} investment. If \textit{ex post} investment is less than \textit{ex ante} investment, saving is less than \textit{ex ante} investment. In his \textit{Treatise on Money} Mr. Keynes formulated a proposition, which has been widely felt to be enlightening, though experience has led him subsequently to condemn the definitions employed as more likely to be misconstrued than helpful. He said that if investment exceeded saving, the system would be stimulated to expand, and conversely. If for the definitions on which that proposition was based, we substitute the definition of \textit{ex ante} investment given above, it is true that if \textit{ex ante} investment exceeds saving, the system will be stimulated, and conversely. This truth may account for the feeling of satisfaction which Mr. Keynes’ proposition originally evoked and the reluctance to abandon it at his behest. In many connections we are more interested in \textit{ex ante} than in \textit{ex post} investment, the latter including as it does unwanted accretions of stocks. Mr. Keynes’ proposition of the \textit{Treatise} may still be a useful aid to thinking, if we substitute for “Investment” in it \textit{ex ante} investment as defined above\textsuperscript{34} (Harrod 1939a, p. 19).

Harrod was aware that his definition of the actual growth rate, \( G = \frac{s}{C_p} \), in terms of the fraction of total income which is saved and of the increment in the stock of capital per unit increment of output, was “a \textit{truism}, depending on the proposition that actual saving in a period […] is equal to the addition to the capital stock” (Harrod 1939a, p. 18, emphasis mine). Nonetheless, he believed that if \( C_p \) assumed the particular value \( C \) indicating the increment of capital per additional unit of output not giving rise to undesired accumulation of stocks, the corresponding formula \( G_w = \frac{s}{C} \), would become “\textit{the fundamental equation} […] which determines the warranted rate of growth” (ibid., p. 18, italics added). Harrod, however, did not discuss the logic of the metamorphosis of his truism into an equation\textsuperscript{35}. This consisted in the fact that an identity is in itself incapable of expressing causal relationships, while the fundamental equation was interpreted as determining the equilibrium rate of growth of the system, appearing “as an unknown variable” (ibid., p. 17), in terms of certain ‘fundamental conditions’. Harrod did not explicitly justify the introduction of a causal nexus, nor did he point out where it occurred. However, the analogy with the \textit{Treatise} seems to go on, for the overall argument of the “Essay” indicates that Harrod was thinking of the piling up of undesired stocks (which is logically equivalent to a difference between \textit{ex ante} and \textit{ex post} investment, or between saving and \textit{ex ante} investment) as the behavioural variable giving meaning to his equation\textsuperscript{36}. Similarly, Keynes saw in “the departure of profits from zero” -which, in the terminology of the \textit{Treatise}, is equivalent to a difference between saving and investment- “the mainspring of change” which “saves [the fundamental equations] from the character of being mere identities”\textsuperscript{37} (Keynes \textit{CW V}, p. 141).

The whole of the argument of the “Essay” thus rested on the assumption as to the entrepreneurs’ behaviour in the face of unexpected variations in their stocks. This is true in particular for the instability proposition, which Harrod believed to have proved while in reality it was nothing else than a \textit{premise} for the possibility of his dynamics.
7. Beyond Traditional Theory

Harrod’s discussion of the limits of traditional economics provides the clue for understanding how far he departed from the received orthodoxy, but also how deep the links were between his dynamics and traditional theory. On the one hand, in fact, Harrod’s approach was extremely conservative with regard to the method and the analytical instruments of traditional economics, but on the other hand he lay the epistemic premises for the overcoming of the classical optimistic view as to the tendency of the system to remain in equilibrium, and he saw in this a truly revolutionary achievement for economics.

Harrod attempted to preserve the ideas underlying the apparatus of cost and demand curves and to apply them, in their generalised form, to the static problem of determining the level of output. The conservative character of this approach is quite apparent, and does not require further discussion. The same holds for the definition of the notion of statics, meant to delimit the domain in which the difficulty highlighted by Keynes did not apply and where therefore the orthodox procedure remained appropriate. It is however interesting to reflect upon the aspect of Sraffa’s and Keynes’s criticisms to traditional theory that Harrod decided to ignore when he interpreted their attack as concerning the domain of application of the orthodox approach, rather than its logical consistency.

Both Sraffa’s and Keynes’s critical remarks related to the assumption of independence of the demand and supply curves. Sraffa, being concerned with demand and supply of goods, had observed that changes in the level of production, entailing additional demand for raw materials and intermediate products, often bring as a consequence variations in both costs and demand, which in most conditions cannot therefore be considered as independent. Keynes insisted instead on the mutual dependence of demand and supply of capital, for investment entails an increase of income which in turn determines a variation in the amount of available saving. In both cases, the lack of independence between supply and demand was exactly the aspect whose general consequences Harrod had refused to accept.

As to the theory of value, in an isolated passage Harrod had admitted that a case could occur in which the demand and supply curves were not independent. Discussing the marketing expenses of a firm, Harrod noted that they surely depended on the quantity produced, but in conditions of imperfect competition they are also affected by the state of demand. In fact if the latter should change and induce variations in the market shares of the firms, these should modify their marketing expenses to make up for the lost share. “If this is so”, Harrod concluded,

a complete reconstruction of the notion of a supply schedule becomes necessary. In the usual analysis supply and demand schedules are regarded as independent of one another. On the new view every demand schedule has its own appropriate supply schedule. To determine equilibrium after a change in the former, the latter also must be changed. The customary graphical
representation of supply is no longer possible. Any given supply schedule of the old type is only valid while the demand remains constant. To draw a single supply schedule to be valid for all states of demand, it is necessary to use three dimensions. Cost becomes a function of two independent variables quantity of output and state of demand. Thus the traditional analysis breaks down\(^{(40)}\) (Harrod 1931, pp. 567-568).

Here Harrod seemed to have caught the spirit of Sraffa’s criticism, and indeed this statement suggested to some commentators that Harrod was critical to the logic development of Marshall’s analysis (Lee 1981, pp. 343-344). But again it must be noted that while Sraffa questioned the independence of demand and supply in general, Harrod’s remark only concerned the domains extraneous to Marshall’s chosen terrain\(^{(42)}\). Harrod’s criticism thus regarded once more the realm of validity of orthodox analysis, and not its logical cogency. Indeed, Harrod only questioned the appropriateness of the choice of the number of variables to be considered, rather than repudiating the traditional approach itself: while the traditional graphical representation “breaks down”, the theoretical problem of independence was left untackled. Moreover, marketing expenses are but one of the components of the firm’s cost, and in subsequent works Harrod simply ignored them: to express it in Shackle’s words, “suddenly marketing costs are thrown overboard” (Shackle 1967, pp. 33-34). Third, Harrod himself found an escape from the difficulty he had just pointed out. In his later papers he only considered uniform changes in production -defined as those variations that leave unaltered relative prices-, and therefore he only discussed total (and not relative) changes in the elasticity of demand (see Harrod 1936b, pp. 85-86, and later 1936a, pp. 14-15). Harrod’s extemporaneous remark therefore does not seem to be enough to impugn the conclusion that Harrod remained substantially loyal to the traditional line of thought in its generalised form, for eventually he concluded that in the appropriate domain of traditional analysis the difficulty did not apply.

To Keynes’s remarks on the lack of independence between saving and investment curves, Harrod simply did not respond: in spite of Keynes’s insistence that this was the point at issue, there was not a single word on independence in any of Harrod’s letters. It is not difficult to understand the reason for this attitude: both Harrod’s general theory of value (i.e., the doctrine of imperfect competition) and his approach to economic dynamics required the same assumption.

On the one hand, even if demand was downward sloping, the determination of equilibrium prices and quantities still presupposed the independence of cost and revenue curves. On the other hand, the level and rate of growth of output as discussed in The Trade Cycle were seen as resulting from the composition of different forces (the four static and three dynamic determinants, respectively): equilibrium states corresponded to the exact balancing of forces acting in opposite directions, while the tendency prevailing far from equilibrium depended on the single magnitude represented by the resultant of the relevant forces. But again, it was possible to add the separate effects of distinct causes
into a joint quantity only provided that independence among the determinants was assumed. Although in his 1939 “Essay in Dynamic Theory” Harrod abandoned the approach in terms of static and dynamic determinants, the assumption of independence was still implicit. In fact, Harrod treated the propensity to save $s$ and the acceleration coefficient $C$ as given and independent from the changes in income triggered by them. Keynes, in the exchange of letters with Harrod on the first draft of this article, promptly remarked that it was not possible to “assume absolute rigidity of $s$ and $C$ and a departure from warranted growth. You have to make some assumptions as to the changes in $s$ and $C$ in unwarranted conditions” (Keynes to Harrod, 29 Aug., 1938, in Keynes CW XIV, p. 334), but Harrod assumed the difficulty away by confining the domain of validity of his growth equation to a single instant. Instead of regarding traditional theory, this time Keynes’s attack pointed out the existence of an independence assumption in Harrod’s dynamics; his criticism remained nonetheless of the same methodological nature. Harrod’s reply similarly repeated his preceding defence of orthodoxy by looking for a sphere where the criticism did not apply.

The object of Harrod’s restoration in the face of Sraffa’s and Keynes’s destructive criticisms was thus the orthodox analytical toolbox, which Harrod applied to both his statics and dynamics, after having reformulated it in terms of independent forces. But he also preserved the traditional method of analysis, which he did apply not only to the static department, but also to dynamics. In both cases, the approach to the problem of the determination of the level and rate of growth of output was distinct in analogous stages: firstly, the instantaneous determination of the equilibrium state, secondly, the study of the consequences of the variations of the fundamental conditions, and finally, the policy implications (see Chapter VI, § 3). Of course there are some specificities characterising the two different problems, for statics was suitable in order to study changes occurring once and for all while dynamics was appropriate in order to consider continuous variations. In Harrod’s view, however, their general method was the same:

Ce qui est nécessaire, c’est que les vagues mécontentements et les idées mi-formulées qu’exprime l’usage courant de la dynamique, soient mises en relation avec la structure méthodique de la doctrine existante par une extension de celle-ci appropriée à l’usage en question. L’objet de la présente étude est de suggerer une méthode d’extension de cette espèce (Harrod 1939c, p. 161, my italics). Si l’on pouvait établir les éléments d’un système de conception dynamique vraiment analogue à la théorie statique, on réaliserait un grand progrès (ibid., p. 167)

[It is necessary that the vague dissatisfaction and the roughly outlined ideas represented by the current use of the term dynamics are put in relation to the methodological structure of the current doctrine, which ought to be extended to fit the appropriate requirements. The purpose of the present essay is to suggest such a method of extension. […] Establishing the elements of a dynamic conception truly analogous to static theory would amount to an immense progress. (my translation)].

The process of redefining the domain of application of the classical analytical instruments in their generalised form compelled Harrod to renounce some of the
analytical results of traditional economics. However, Harrod seemed to maintain that it was not necessary to renounce the ‘pieces’ of the traditional theory, provided that these were re-arranged in a new logical order. Such a conclusion is suggested by Harrod’s own comparison of the structures of Keynes’s *General Theory* and of traditional theory, which can easily be paralleled with the structure of Harrod’s book on the *Trade Cycle* which at the time was hot off the press (see Besomi 1993-94). Harrod’s “version of the relation of Keynes to orthodoxy” (Harrod to Sisam, 14 Aug., 1938) was submitted in September 1936 at the Oxford Meeting of the Econometric Society (and later published in the January 1937 issue of *Econometrica*: Harrod 1937a). Its aim was that of identifying which elements of traditional theory were maintained, which were expelled and which were modified in Keynes’s treatment. It therefore provides precious information on Harrod’s attitude to the comparative value of traditional and Keynesian theory for contemporary economic analysis.

I have summarised Harrod’s opinion on Keynes’s rearranging of the pieces of traditional theory in Chapter V § 4; I have stressed in particular that in the first place Harrod identified the marginal efficiency of capital with the old marginal productivity, that in the second place he thought the multiplier to be nothing else than the old supply schedule of capital with the level of income treated as a variable, and that finally he thought the liquidity preference as supplying a piece missing from traditional theory. Now it is interesting to notice that the *Trade Cycle* was based on the same elements, save that the accelerator was substituted for the marginal efficiency of capital, due to the fact that the latter, according to Harrod, had to be calculated on the basis of a given level of output while the dynamic problem presupposed a continuously changing output. Accelerator and multiplier together constituted the basic mechanism of the dynamic section of Harrod’s theory, while the criticism of the quantitative theory of money implicit in the third piece provided the link between static and dynamic forces. As explained in Chapter V, § 5, in Harrod’s book it was the general level of prices that actually provided the destabilising element maintaining the level of income in neutral equilibrium and thus enabling income to fluctuate, given the static and dynamic forces. But how could the price level fluctuate in such an accommodating way? According to Harrod, those fluctuations were not a consequence of changes in the quantity of money, as the quantity theorist would have argued, but of changes in the velocity of circulation triggered by income variations, which “enable money to act as the arch-destabilizer” (Harrod 1936a, p. 126). The velocity of circulation therefore was determined by the interplay of static and dynamic forces, via the general level of prices. Thus, even if somewhat differently from Keynes’s solution, in the *Trade Cycle* the monetary factor filled the gap of the missing piece presupposed but not defined by the classical theory (Harrod 1937a, p. 85; see also 1936a, p. 126).

The pieces considered by Harrod and Keynes thus corresponded to those considered by traditional theory, albeit rearranged in a different logical order. However,
the preservation of the traditional pieces in Keynes’s work suggested to Harrod that his friend had not affected a true revolution in economic thinking, while on the contrary he thought his dynamics to have constituted the first step towards “a new method of approach -indeed, a mental revolution” (Harrod 1939a, p. 15). In what did Harrod’s radical novelty consist?

Harrod saw the differentia specifica of his dynamics with respect to the corpus of traditional doctrine in the instability proposition:

The significance of what follows should not be judged solely by reference to the validity or convenience of the particular equations set forth. It involves something wider: a method of thinking, a way of approach to certain problems. It is necessary to “think dynamically”. The static system of equations is set forth not only for its own beauty, but also to enable the economist to train his mind upon special problems when they arise. For instance, an economist may pose to himself the question, What would be the effect on the system of an increase of exports or of a labour-saving invention? By reference to the static equations, he then proceeds to work out the new equilibrium position supposing the new higher level of exports to be maintained in perpetuity or the labour-saving invention to be incorporated in the productive technique once for all.

But let the question be: suppose the level of exports begins and continues to increase steadily, or suppose its rate of increase to increase, or suppose labour-saving inventions begin to be made in a steady or growing stream; then the static method will not suffice. The static theorist may hope to reduce this supposed steady increase to a succession of steps up, each having the same effect. But if the following argument is correct, the effect on the moving equilibrium of advance may often be in the opposite direction to the effect on the static equilibrium produced by each of the steps considered singly (ibid., emphasis mine).

Harrod thus saw in the incapacity of traditional theory of providing an explanation of states different from equilibrium, the main obstacle opposing the construction of a genuine dynamic theory; and in the overcoming of this limit lay, in his view, the passage for proceeding beyond the traditional economics.

The latter expression was Harrod’s own, for it was the title he suggested to his publisher towards the end of 1938 for a collection of his essays. Conveniently, it summarises his feelings as to the necessity of overcoming the limits of the traditional approach that he has strenuously betokened since 1934, but also reveals his intention to find a solution to this problem within the principles of orthodoxy itself:

The book would be divided into 3 main blocks of articles. One concerns method, the other the economics of growth and the third the economics of imperfect competition. They have this in common that they are all feeling out towards new developments of economic theory. I thought at first for a title of “Towards a new economics”, but I don’t quite like this because it suggests something too subversive, namely a scrapping of the old, which is not at all my intention. What do you think of “Beyond the traditional economics”? (Harrod to Sisam, 26 Sept., 1938).

The upshot of Harrod’s epistemic considerations on the principle of instability and of the redefinition of the domain of application of the traditional analytical instruments was a general theory which included as special cases the traditional theory, Harrod’s own dynamic theory, but also Keynes’s theoretical systems and the theory of imperfect
competition stemming out of Sraffa’s suggestion, which Harrod redefined in such a way as to make them fit within the more comprehensive scheme. Harrod thus felt that this general project was going to be the most important achievement for economic theory and was going to constitute the true mental revolution needed in economic thought. Here originates his judgement of Keynes’s *General Theory* as not truly revolutionary, but only as a first step towards the identification of the limits of statics, and not yet its overcoming.

In evaluating Harrod’s achievement in its historical context, on the one hand it must be considered that his dynamics was worked out in the full spirit of the neoclassical tradition, which his achievements thereby rehabilitated and strengthened. But on the other hand, two other aspects of Harrod’s work must be appreciated. First, his considerations have introduced into orthodox analysis some concepts that were truly unthinkable before his book and articles. Although the traditional approach could not neglect the existence of cycles, the notion of business fluctuations (for instance) only admitted an explanation in terms of causes exogenous to the proper working of the economic system. On the contrary, devoting a branch of economics to the study of endogenous changes of income entailed recognising that equilibrium and stability are unlikely states of affair and that the economy is far from being in conditions of permanent optimum.

Harrod’s effort had been that of reconciling cycles and imperfection with a system of thought excluding such phenomena from its own premises. To achieve this result, he had to modify the premises and complement the traditional approach with a new theoretical construction based on the new extended set of assumptions. But he could not renounce certain fundamental assumptions of the old approach. For instance, both his static and dynamic equilibrium states were defined with respect to the satisfaction of the individuals operating in the economy: statics considered

whether individuals, having the tastes and needs that they have in those circumstances, can improve their position by altering their line of conduct (Harrod 1936a, p. viii),

and dynamics inquired into

what sort of action we must suppose individuals to take in certain circumstances, so that, having regard to the circumstances and the factor of growth which their action entails, they will not be able to improve their position otherwise than by continuing to act as they do (Harrod 1936a, p. ix).

This is the reason why Harrod insisted on the question of how the fluctuations of output are consistent with the ordinary behaviour of people (i.e., the compatibility of static and dynamic forces), and why he used the term “justification” and “warranted” for characterising the states of static and dynamic equilibria. Equilibrium thus still implied profit maximisation, and hence some sort of optimality under the prevailing conditions.

As a second example of a premise that could not be radically modified we have Harrod’s treatment of expectations. I have already quoted a passage where he maintained
that traditional theory could conveniently deal with the state of confidence (Chapter V, § 4); to this it may be added that in *The Trade Cycle* expectations played a very restricted and peculiar role, while in the subsequent 1939 article they were not even mentioned (for a discussion see Chapter VII, § 7). It is not surprising then that Harrod could not understand Keynes’s notion of investment as an autonomous process rather than as dependent on some sort of calculus, and that he concluded that the *General Theory*’s “treatment of the trade cycle was merely fragmentary for lack of a theory as to what governs the volume of investment” (Harrod to Keynes, 15 April, 1937, in Keynes CW XIV, p. 175). In fact, Harrod’s approach was worked out in terms of the analogy with the forces of classical mechanics, just as traditional theory was. This required expression of all the magnitudes involved as functions of some other variable, so that Harrod could not find any workable determinant of investment in Keynes’s work.

The second aspect of Harrod’s writings to be noted is the skilfulness by which he originally developed the *pars construens* of Sraffa and Keynes’s contribution, injecting into economics fresh ideas full of consequences. Unfortunately Harrod’s theory, together with other economists’ contributions in the same direction, have often been interpreted as the sole and necessary outlet of Keynes and Sraffa’s analysis. Harrod’s *Trade Cycle* is thus often seen as one of the dynamic versions of the *General Theory*, in that it implements it with an explanation of the genesis of investment, while Keynes and Sraffa’s attacks are simply ignored. But Keynes himself, in a letter to Harrod, warned that “acceptance of my constructive parts can only be partial if you do not accept my critical sections”. Keynes’s conclusion applies both to Harrod and his above mentioned exegetes:

I am frightfully afraid of the tendency, of which I see some signs in you, to appear to accept my constructive part and to find some accommodation between this and deeply cherished views which would in fact only be possible if my constructive part has been partially misunderstood (Keynes to Harrod, 25 Aug., 1935, in Keynes CW XIII, p. 548).

**Notes**

1 It is worth repeating at this point that throughout this book, and this Chapter in particular, by the expressions ‘orthodox’ and ‘traditional’ theory I therefore mean, in broad terms, the Marshallian theory of value as it was commonly taught in Cambridge in the mid-twenties. For further discussion see Introduction, note 12.

2 On Harrod’s cultural memory see Introduction, § 4.

3 This list is of course far from exhaustive: all these developments taking place within the economic discipline were of course plunged into a political and philosophical environment, themselves rapidly evolving. Some of these were quite relevant for the development of Harrod’s ideas, and are thus discussed elsewhere in this book. I refer in particular to the notion of a ‘progressive society’ (Chapter II § 3) and the neo-positivist currents in philosophy (Chapter VI, § 1).

4 It is not clear whether Harrod’s original 1928 contribution sprang from Sraffa’s 1926 article: Harrod did not quote it, and cited rather a 1928 contribution by Pigou. However a few years later he explicitly dealt with (and claimed to have solved) Sraffa’s problem, which was re-stated in the following terms:
where the product is not completely standardized or the market not organized, the individual producer may, although quite small, have increasing difficulties in marketing increments of produce (Harrod 1931, p. 566).

5 The reader will notice that the marginal cost curve considered by Harrod still retained the traditional U-shape.

6 On Harrod’s concern with the “legitimacy of the conception of increasing returns industries”, see Chapter I, § 3).

7 At the time of writing the Trade Cycle, Harrod still held the same opinion: “I still think that the way in which the behaviour of competitors and monopolists is often contrasted is quite fallacious” (Harrod to Henderson, 23 Feb., 1936).

8 It must be noted that the difficulty originally highlighted by Sraffa was left unanswered. For, if on the one hand it is true that Harrod’s notion of marginal returns allowed a generalisation of the application of demand curves in the partial equilibrium approach (and thus also enabled one to consider the possibility of increasing returns), on the other hand for the validity of this very approach it was still necessary to assume the independence of the two curves. But this hypothesis was far from being universally satisfied. In fact, in the particular (Marshallian) case of perfect competition, in the majority of occurrences this assumption is still subject to the logical difficulties pointed out by Sraffa. In the general case of imperfect competition, the problem is by no means more simple. For Harrod maintained that if the firms are subject to decreasing cost, the resulting equilibrium level of output is unstable (see above, Chapter I § 3); this implied that a small change in output may entail consequences of a much higher order of magnitude. In such conditions, the cost and demand curves could not be assumed to be given, nor to be independent of each other. Therefore the logical conditions for the extension of the partial equilibrium approach were not automatically warranted by the more general interpretation of demand curves, but needed further discussion of which there is no trace in Harrod’s writings.

9 Keynes’s method of criticism of traditional theory was based on the recognition of tacit assumptions that either were incompatible with the explicit premises or restricted the generality of the theory to special cases. Most of these premises unwarrantedly assumed independence between variables. On this aspect see Carabelli 1991.

10 See e.g. Harrod’s letter to Keynes, 22 Aug., 1935, in Keynes CW XIII, p. 546 and passim. Harrod stressed in many occasions his dissent from Keynes’s attack and especially from his manners, e.g. in correspondence with Henderson (“I admit that his manners, where his own theories are concerned, are impossible”, letter of 28 March, 1936; and “I agree that Maynard’s manners are provoking”, letter of 9 April, 1936) and with Robertson, to whom Harrod wrote:

I share your feeling entirely about his attacks. I have attacked him for them, but have only succeeded in getting the most offending chapter printed in smaller type as an appendix. I regret them, I feel they will raise un-necessary dust -but there it is, that is his way (Harrod to Robertson, 7 Oct., 1935).

11 As to the methodology of criticism, there is a big difference between Harrod’s and Keynes’s attitudes. Keynes indicated the source of the classical economists’ mistake in the assumptions that warranted the possibility of treating the economic system as composed of isolated and independent ‘atoms’, to which he opposed his view of a complex and organically interdependent system (see Carabelli 1991). Harrod treated it instead as a mistake due to repeated carelessness through generations of economists:

Have I at all succeeded in making plain why I conceive the orthodox theory [of interest] to be definitely fallacious? It seems to me to contain one piece of definitely wrong thinking, an intellectual mistake, which now that it is exposed must be admitted. And if you ask how it is that so many minds have been guilty of it, I reply precisely because each mind received it with the authority of <many> minds and did not pause to examine it (Harrod to Robertson, 8 Oct., 1937).

This rather trivial argument, which failed to attribute the fallacy of the orthodox theory of interest to the features of the traditional approach itself, could obviously not provide Harrod with any cogent reason for rejecting the ‘classical’ approach as a whole.

12 Harrod felt that the difference between him and Keynes was not to be found in the substance but in the exposition of the argument: “You may wonder why I lay such stress on a point that merely concerns formal proof rather than the conclusion reached” (Harrod to Keynes, 1 Aug., 1935, in Keynes CW XIII, p. 533).

13 Accordingly, Harrod and Keynes attributed a different methodological status to the tacit premise of constant income. Harrod treated it as a ceteris paribus clause, that is a condition that might or might not
be satisfied; Keynes never referred to that notion in this connection, neither in the correspondence with Harrod nor in the Chapters of the *General Theory* dedicated to the classical theory of interest. On the contrary, Keynes qualified the assumption specifying that “income and employment cannot change”, (Keynes to Harrod, 14 Aug., 1935, in CW XIII, p. 541), implying that

the assumption of a fixed income [...] is too all-embracing and does not allow for those things to vary which the classical theory assumes to vary. For, if either the propensity to save or the marginal efficiency curve changes, the level of income changes; so that the assumption of a given level of income involves us in assuming that neither of the classical’s theory own chosen variables is capable of change (Keynes to Harrod, 10 Sept., 1935, in Keynes CW XIII, p. 558).

14 Here Harrod seemed to accept Keynes’s attack to the classical theory of interest only to the extent that it was led on the same methodological ground as Keynes’s criticism to the orthodox theory of employment, which was meant to show that its tacit assumptions restricted its generality:

15 Having perceived that the point of disagreement lay in the different status and place they attributed to the hidden premise, Keynes also guesses the possible upshot of Harrod’s line of thought:

16 Such exclusion was motivated in terms clearly recalling Harrod’s interpretation of Keynes’s criticism:

17 Harrod stressed the same point while commenting on the *General Theory* as compared with the *Treatise on Money*:

He also concluded his article on “Imperfect Competition and the Trade Cycle” in similar terms:
Its claim for consideration rests on its attempt to rescue the theory of output in the cycle from excessive dependence on the hypothesis of time lags. It gives certain broad reasons for supposing that the entrepreneur’s variations of output within the cycle may be represented not as maladjustments, but as conformable to the postulates of the general theory of value (Harrod 1936b, p. 88).

With reference to his “study of the Crusoe state”, Harrod specified that the analysis of “the stabilizing elements in a modern society” was not irrelevant nor “a device to hold the reader’s attention, by directing his suspicions to a wrong quarter”, but “is indispensable to the general theory” (Harrod 1936a, p. 170).

In her review of *The Trade Cycle*, Joan Robinson failed to understand the role of the preliminary, static analysis reformulated in terms of forces, which she considered an odd masquerade (Robinson 1936, p. 693).

For Harrod’s use of this term to indicate the partial equilibrium approach under general conditions as to competition, see Chapter I § 2

in *The Trade Cycle* there was not much room for the rate of interest. Harrod maintained that changes in this variable could only be effective if their magnitude was quite substantial and if they occurred within a short lapse of time (the ‘breathing space’), after the dynamic determinants decreed the downturn but while outstanding orders still held the recession within fairly narrow limits (Harrod 1936a, pp. 104 and 168-169). Harrod therefore did not feel the need to discuss the determination of the rate of interest, but just noticed that the liquidity preference approach was not necessary to, nor incompatible with, what he had to say (ibid., p. 120). On the other hand, the traditional theory of interest had to be discarded for the reasons I have mentioned in Chapter V § 4 above, so that to complete the theoretical set-up some alternative formulation would be anyhow necessary. Harrod therefore expressed the opinion that Keynes’s theory has to be accepted, provisionally at least. He exposed his view to Robertson (who did not accept Harrod’s criticism of traditional theory) in the following terms:

It really does shock me to see you standing there absolutely naked and imagining yourself fully decked out. Maynard offers you a temporary shift in which you can run for shelter while you make yourself something more substantial.

But it is no light task that lies before us. My particular system of dynamic determinants, a first feeble attempt in this field, also assumed the rate of interest as a known. So there is the poor interest rate with no friends either in the static or dynamic camps. That is why I look with a kindly eye on Maynard’s theory, because here at least is one friend for him in the static camp. He may be a poor one, but beggars can’t be choosers (Harrod to Robertson, 4 Oct., 1937).

The ultimate reason for the provisional acceptance of liquidity preference was stated in a letter to Hubert Henderson, 28 March, 1936:

I don’t feel altogether happy about the liquidity preference theory of interest, as a complete theory, for reasons which I have stated in my book. But if you get rid of that you are one equation short. It means that the true theory of interest is still to seek, not that there is any theory in existence, that makes sense!

Describing the method of static analysis, Harrod restated what he believed to be the valuable part of Keynes’s criticism:

The weak point in the static theory is that, in order not to be too remote from the facts, it is often assumed that one line of action, which individuals take, is to save so and so much. An attempt is made to demonstrate what determines the equilibrium price for this saving, viz. the rate of interest. Yet really the supposition of saving is inconsistent with the pre-requisites of a static analysis, for, if any net saving is occurring, the quantity of capital and the income-earning capacity of the community must be growing, and the factor of growth does not appear among the static assumptions (Harrod 1936a, p. viii. See also Harrod 1938a, pp. 403-404).

Such attempts have always been quite popular, undoubtedly because of the prestige of physics which was hoped to transfer, in part at least, to economics, by adopting similar procedures. Ragnar Frisch, for instance, proposed his definition of dynamics with an eye on the notions (‘stationary state’, ‘rest’, ‘equilibrium’, ‘disturbance’, ‘stability’, etc.) and the analytical instruments (the family of functional equations, with particular reference to difference, differential, mixed and integro-differential equations) developed in mechanics to deal with the motion of bodies (Frisch 1936, pp. 100-102). This example is particularly important, for Harrod’s analogy with mechanics was also meant to provide further ground for the opposition to the econometricians’ notion of dynamics. It would also be interesting to know if Harrod
was aware of Knight’s essay on “Statics and Dynamics. Some queries regarding the mechanical analogy in Economics”, translated in English in 1935 (Knight 1930).

24 The suggestion was originally confined to a few lines in his methodological essay and in the first footnote to the “Essay”. In the Presidential Address to the British Association, Harrod wrote:

I conceive the analogy between the relation of dynamics to statics in mechanics and that of this branch of economics to the static theory to be much closer than that implied in recent uses of the word dynamics in economics. While the equilibrium price determined by the maintenance of a steady flow of demand and supply corresponds to a state of rest, new equations would be formulated to determine regular movements in the economic magnitudes under the influence of growth of population, savings, inventions, etc. (Harrod 1938a, pp. 402-403).

The “Essay”’s footnote seems to have been written to specify Harrod’s dissent with the use of the term “acceleration principle” in economic literature:

The study of the condition in which demand and supply are flowing at an unaltered rate has long been known as Static Theory: this implies that the equilibrium of prices and quantities resulting therefrom is regarded as analogous to a state of rest. By analogy, therefore, a steady rate of increase of demand, which is our first matter for consideration in dynamic theory, and a major effect of which is expressed by the ‘Relation’, should be regarded as a velocity. Acceleration would be a rate of change in this (Harrod 1939a, p. 14, n. 1).

In truth, there was also a reference to the analogy with mechanics in Harrod 1937 review of Lundberg’s Studies in the Theory of Economic Expansion, but it was limited to the following statement: “I suggest that it may be possible to construct a method of dynamic analysis more closely analogous to the dynamics of mechanics [than Lundberg’s]” (Harrod 1937f, p. 496); Harrod did not explain what the analogy consisted in.

25 I wonder whether Harrod ever developed in his own mind the details of the analogy. I suspect, in fact, that Samuelson’s comment may extend to Harrod’s case:

Some writers attempt to distinguish between statics and dynamics by analogy with what they understand to be the relationship in theoretical physics. That this is a fruitful and suggestive line of approach cannot be doubted. But it is too much to suppose that very many economists have the technical knowledge necessary to handle the formal properties of analytical dynamics (Samuelson 1943, p. 58).

It is however possible that Harrod discussed this and other aspects of the analogy with mechanics with his friend the physicist Lindemann in the course of the long nights they spent together dialoguing (for a description of these events, see Harrod 1959).

26 For a detailed exposition of the Trade Cycle’s argument in terms of forces, see Chapter V, § 2; for the specific mechanism of transmission between the forces determining the movement of output as a whole and the individual’s considerations on his own level of production, see Chapter V § 6.

27 After having pointed out that the static equilibrium is normally stable while dynamic equilibrium is unstable, in the “Essay” Harrod stressed that “this gives a prima facie reason for regarding the dynamic analysis as a necessary propædeutic to trade-cycle study” (Harrod 1939a, p. 21). And again, two pages below, having confirmed that the moving equilibrium is highly unstable, Harrod commented: “Of interest this for trade cycle analysis!” (ibid., p. 23)

28 See for instance Marschak 1938*, headings D and E; Baumol 1949, pp. 512-514; Medio 1979, Sections 1.3 and 1.4; Shackley 1967, pp. 261-2

29 See in particular Hawtrey 1932, pp. 343-4. Haberler had raised this point in a letter to Harrod dated 2 November, 1934:

Keynes has not made any real discovery. As Hawtrey has shown, a difference between savings and investments is ex definitione (according to Keynes’ definitions) a loss, if savings are greater than investments, and a profit, if investments surpass savings. It is wrong to say that differences between S and I cause or lead to losses or profits; they are losses or profits. I have worked this out in detail and I thought that after Hawtrey’s brilliant criticism, there would be no need to publish my paper. But perhaps I shall do so. Keynes’ explanation of the business cycle in terms of differences in savings and investment comes therefore to this: the prosperity phase is characterized or caused by profits; the depression by losses. That we knew before.
As to the difference between Harrod’s formulation and the Swede’s notion of *ex ante* and *ex post*, see Chapter VII, § 7.

It is interesting to notice in this connection that the interpretation Harrod offered of his *Trade Cycle* in terms of the interaction between saving and investment, suited the original notion of *ex ante* much better than it was the case in the “Essay”. The argument of the *Trade Cycle* regarded the interplay of the entrepreneur’s decisions and their results (Harrod 1936a, pp. 162–4), while in the “Essay” Harrod was concerned with the actual course of events as compared with the equilibrium state (see Chapter VII, §§ 4 and 7).

As to the probable origin of this interpretation, see Chapter III § 5 note 28.

Harrod did not have any “clear view as to the possible causes likely to operate in a systematic way to distort *ex post* from *ex ante* saving, or of the probable importance of such distortions”, and therefore neglected these possible divergences (Harrod 1939a, pp. 20-21).

Warren Young stressed that Harrod found the approach of the *Treatise* more convenient than that of the *General Theory* as a starting point for his “dynamisation of Keynes” (1989, pp. 195-198). However, Young did not think his book to be “the place for a detailed discussion of Harrod’s approach to the *Treatise* concept of saving or of the way in which he dynamised” it” (ibid., pp. 196-7), and consequently his conclusion was left unspecified.

This interpretation of the *Treatise* obtained Keynes’s imprimatur:

> Your interpretation of my *Treatise* here is very happy. I am not sure that your interpretation would work out quite consistently all through. But what you say is certainly what I had confusedly in mind (Keynes to Harrod, 17 Aug., 1938, in Keynes *CW* XIV, p. 322).

On the logical ambiguity of this “axiom” of Harrod’s dynamics, see Chapter VI § 1.

This aspect was already remarked by Alexander and by Kregel. The former stressed that Harrod’s “argument is merely definitional”, and pointed out that the notion of ‘justification’ depends on the particular assumption on the behaviour of the entrepreneurs (Alexander 1950, pp. 727-8), while Kregel stressed that

> The ‘fundamental relation’ is, however, as Harrod emphasises, merely a truism; it has no analytical use until behavioural relations are specified for the variables. Under different specified conditions and assumptions, the variables take on different meanings. The analytical aspects of Harrod’s analysis of growth are thus based more on the various possible economic definitions of the variables [i.e., actual, warranted and natural rates of growth] than on the form of the growth equation itself. (Kregel 1972, p. 38).

Unlike Harrod, Keynes was aware of the necessity of providing a causal interpretation of his fundamental equations -“mere identities”, “truisms which tell us nothing in themselves”- “by the introduction of extraneous facts from the actual world” (Keynes *CW* V, p. 125).

For an historical sketch on the assumption of independence between variables in the Cambridge economic tradition, see Rotheim 1992.

Keynes was explicit in pointing out to Harrod the target of his criticism:

> I still maintain that there is ‘no sense’ in the view that interest is a price which equates saving and investment; or at any rate that if one could invent a sense for it, it would be quite remote from anything intended by the classical theorists. Perhaps the clue is to be found where you allege that I am doing great violence to the accepted and familiar when I maintain that ‘two independent demand and supply functions won’t jointly determine price and quantities’, for my whole point is that the functions in question are not independent (Keynes to Harrod, 9 Aug., 1935, in Keynes *CW* XIV, p. 538).

Harrod repeated this argument a year later in a letter to Robertson, who had criticised Harrod’s claim that in the presence of marketing expenses the supply price is a function not only of the quantity of output, but also of the state of demand (Robertson 1932*). After stating that “if we are convinced that in fact cost is a function of two independent variables, amount of output and ‘state of demand’, we must require more than two dimensions”, Harrod specified in a note:

> The use of the word independent here is vital, vital both to secure my conclusion mathematically and to interpret the economic facts correctly. It also differentiates between the case in which demand may be implicitly represented in the cost curve and where it may not. The amount of
output no doubt depends on demand; productive costs depend on amount of output and indirectly on state of demand. Thus making cost a function of amount of output makes it indirectly a function of state of demand. But marketing costs unlike productive costs depend on state of demand directly and this dependence is not mediated by the amount of output. Thus the marketing cost of the same amount of output, \( x_0 \) will change if the state of demand changes. This proves that the influence of the state of demand on marketing cost is not mediated by amount of output. \( \therefore \) we must take state of demand as an independent variable of which cost is a function directly. It may also be a function of state of demand indirectly via the other variable. Per contra a change in the state of demand will have no effect on productive cost, if there is no change in output. Consequently where there are no marketing costs (as in case one) the need for a second independent variable is eliminated, all variations in the state of demand necessarily acting through the variable you already have, viz. quantity of output. But this is not so in case 2. (Harrod to Robertson, 3 Sept., 1932).

41 In his essay on Harrod and Keynes on increasing returns, theory of employment and dynamic economics, Kregel too seemed to overrate Harrod’s comments on the interdependence of supply and demand curves (Kregel 1985). This led to an interpretation of the debate between Harrod and Keynes on traditional analysis which diverges from the one presented here.

42 After having stressed the importance of the difficulty highlighted by Harrod, Kahn observed that it is not “legitimate to ascribe conditions of perfect competition to an industry which is subject to Harrod’s marketing costs”:

Surely the very fact that these marketing costs are incurred demonstrates that the market is imperfect? The assumption made by Mr. Harrod […] that the demand for the individual firm is perfectly elastic is hard to reconcile with Mr. Harrod’s statement that “any attempt to push out into the competitor’s territory is attended with rising marketing costs per unit of sales” [Harrod 1931, p. 567]. If a firm can increase its sales to any desired extent by an infinitesimal reduction of its price, why should it go to the expenses of incurring Mr. Harrod’s marketing costs? (Kahn 1932, pp. 660-61).

43 Harrod was of course conscious of this necessity, for he explicitly discussed the conditions for maintaining the assumption of independence of the static forces: see Harrod 1936a, p. 3.

44 For a discussion of Harrod’s debate with Keynes and of the methodological peculiarities of his instantaneous analysis, see Chapter VII, in particular § 6.

45 I have already observed (see Introduction, §3) that a few years before being capable of formulating a trade cycle theory himself, Harrod maintained that new theories ought to be expressed in terms of marginal analysis, in order to allow communication between theoretical systems. However, when he had read Keynes’s book he praised as an “immense improvement” the fact that “marginal analysis comes into its own again” (Harrod 1936a, p. 71) on the grounds of a rather different reason: after having criticised the Treatise on Money for conceiving entrepreneurs never to be in temporary equilibrium (ibid., p. 66), in Harrod’s eyes the General Theory eliminated any reason to suppose that the price-level diverges during boom and slump in any notable degree from the marginal cost of production (perfect competition) or that the entrepreneur misjudge their proper course of action throughout the boom and slump (ibid., p. 71).

Harrod thus brought his argument back to his epistemic tenet that fluctuations must be compatible with the maximising behaviour of entrepreneurs rather than be supposed to result from systematic errors of judgement.

46 For a discussion of this distinction with respect to the Trade Cycle, see Chapter V § 3. As to the “Essay”, see Harrod 1939a, p. 15.

47 The same term was also used in the review of Lundberg’s Studies in the Theory of Economic Expansion, where Harrod considered dynamics as “a natural expansion of the static theory, appropriate to an expanding economy” (Harrod 1937f, p. 496).

48 Pugno (1992, Chapter. 1) interpreted Harrod’s imperfect competition theory as aiming to preserve the results of traditional theory, rather than its method as it is suggested here; he thus concluded that Harrod missed, in part at least, his scope: see Chapter I. § 1, note 8.

49 In this specific occasion, Harrod claimed that he used the expression ‘traditional theory’ to refer to Bowley’s Mathematical Groundwork of Economics, rather than to Marshall or to the partial equilibrium approach as taught in Cambridge in the late 1920s (Harrod to Robertson, 25 Dec., 1936). Although
Bowley’s book introduced Pareto’s system to English language readers, Harrod’s admittedly scarce understanding of mathematics (as recognised in the same letter to Robertson), together with Bowley’s own introductory remark that he was presenting “the main part of the mathematical methods used by Cournot, Jevons, Pareto, Edgeworth, Marshall, Pigou, and Johnson” (Bowley 1924, p. v), have probably induced Harrod to think that Bowley was actually giving an algebraic formulation of the partial equilibrium approach. I submit, therefore, that in spite of Harrod’s disclaimer the expression ‘traditional theory’ has here the same meaning as it has elsewhere in Harrod’s writings. In any case, both Robertson and Pigou -by no means two naïve readers- were deceived by Harrod’s rendition, which they interpreted to refer to Marshall (Robertson to Harrod, 9 Nov., 1936, and Pigou 1936*).

Harrod described his paper as “I think the best account of Keynes by any other than himself. When I sent him a proof a week before he was due to lecture in Stockholm, he said he would gladly read my paper instead of his lecture as giving a new line on his views” (Harrod to Sisam, 14 Aug., 1938). The correspondence with Keynes on this paper is partly published in Keynes CW XIV, pp. 83-86.

Harrod’s discussion of this aspect was somewhat confused. I have however tried to analyse it in some detail in Besomi 1993-94, where I argue that Harrod’s paper at the Oxford meeting was more of an attempt to qualify The Trade Cycle with respect to the General Theory and orthodox analysis rather than an exposition of Keynes’s thought.

For a discussion see Chapter V, § 4. Years later, Harrod restated that Keynes had only “proposed a reclassification in part of the field”: 1951, p. 143.

The proposal to gather some of Harrod’s essays in a book came from Sisam of the Oxford University press at the end of July, 1938. Harrod liked the idea, for on 14 August suggested a list of “original contributions to modern theory and to branches thereof that are now under discussion”, including the forthcoming “Essay”, the 1934 article on “Doctrines of Imperfect Competition”, the 1937 paper on “Mr. Keynes and Traditional Theory”, and the 1938 methodological essay. Harrod originally meant to have the book published by Spring 1939, but the OUP delegates insisted that it had to contain some new material. In November 1938, Harrod was planning “to write two or three new papers for it”; in December Sisam mentioned “one or both of your papers on Gold”, which were however published in two parts as Harrod 1938i (Sisam to Harrod, 20 Dec., 1938). After this letter, the correspondence on the book seems to have broken down; surely the outbreak of the war induced both parts to postpone the plan, which was never resumed in its original form

For a discussion of expectations in the Trade Cycle, see Chapter V § 3.

On Harrod’s analogy of the methods and conceptions of science of economics and natural sciences, see his correspondence with Keynes on Tinbergen’s method, in Keynes CW XIV, pp. 295-305, and Chapter VI, § 1; on the analogy with the forces of classical mechanics see also Shackle 1967, p. 253 and passim. On the notion of statics and dynamics in analogy with physics, see above, Section 5.

Discussing the General Theory with Keynes, Harrod claimed the right of expressing savings and investment as functions of different determinants: see the passage from Harrod’s letter to Keynes of Harrod to Keynes, 30 Aug., 1935 (in Keynes CW XIII, p. 554), cited in Chapter IV § 2.
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Abbreviations
CH Archives of the Royal Institute of International Affairs, London: Chatham House.
DHR Dennis Robertson Papers, Trinity College, Cambridge.
FKP Frank Knight Papers, University of Chicago.
FS Fabian Society, Nuffield College, Oxford.
HKNM Harrod and Keynes, Notes and Memoranda, Tokyo University.
HHP Henderson Papers, Nuffield College, Oxford.
HP Harrod Papers, Chiba University of Commerce, Ichikawa (Japan).
HPBL Harrod Papers, British Library, London.
HTRY Hawtrey Papers, Churchill College, Cambridge.
JMK John Maynard Keynes Papers, King’s College, Cambridge.
JVR Joan Robinson Papers, King’s College, Cambridge.
KP Richard Kahn Papers, King’s College, Cambridge.
MaP Jakob Marschak Papers, University of California, Los Angeles.
MoP Moore Papers, Cambridge University Library.
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OUP Oxford University Press Archives.
TP Tinbergen Papers, Erasmus University, Rotterdam.

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1 An autograph “footnote to be appended to page 1 of article by R.F. Harrod” is attached to the TS, saying that “This article was written in mid-1933 when the up-swing of the trade cycle has not proceeded so far as it has now”.

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