Expectations, Lags and Particular Parameter Values in Harrod's Dynamics

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Abstract: Two recent books on Harrod's work and Harrodian themes mentioned two articles on Harrod that I published in the early 1960s. Harrod himself wrote a letter to me commenting on the articles. This letter throws some new light on how Harrod, at least at the beginning of the 1960s, regarded the role of expectations, lags and the extent, if any, that his results depend on particular parameter values. The most startling thing in the letter is Harrod's admission that his fundamental instability principle may depend on the sizes of the multiplier and acceleration coefficient falling within certain ranges.

In recent years, there has been a renewed interest in the work of R.F. Harrod and especially in his work on economic dynamics. Two recent books, Besomi (1999) and Rampa, Stella and Thirlwall (1998) mention my work on Harrod published in the early 1960s. Both Nevile (1960) and Nevile (1962) are correctly included in the literature of the time which recognised that Harrod's dynamic analysis had implicit assumptions about expectations and tried to make these explicit in a way that was congruent with the character of Harrod's analysis. Then the stability of the growth rate in the resulting models was examined.

However, one of the articles went further than this and examined the effects of relaxing Harrod's assumptions about entrepreneurial and consumer behaviour. Harrod, himself, wrote a letter to me commenting on the articles.[1] These comments throw light on some of the issues that have been much discussed in the literature of the last quarter of the twentieth century. They give a new insight into his position, at the beginning of the 1960s, on the role of lags and reinforce one existing interpretation of his views on expectations. The most exciting point is a surprising window into his views on the need, or lack of it, to constrain the values of the parameters in the model in order to produce instability and/or the trade cycle. Each of these three points will be discussed in turn. Other comments in his letter set out some of his well-known views and are not discussed in this paper.

To set the scene, let us remind ourselves of the context in which Harrod was writing and the essence of what he thought was his contribution. In the middle of the twentieth century there had been a gap of about 100 years since dynamic economics had been part of the main corpus of economics. Trade cycle theory had certainly been significant, but it had been ad hoc with little connection with mainstream economics. Moreover, there was no widely accepted theory of the trade cycle, but a multitude of theories, until Hicks's (1950) contribution became the dominant textbook explanation in the 1960s. Nevertheless, the resurgence in interest in dynamic economics started with trade cycle explanations, not the growth theory which had loomed large in the writings of the classical economists.

Two approaches emerged in the 1930s and 1940s. Both flowered by combining the multiplier and accelerator, but otherwise were very different.[2] They even had different definitions of dynamic economics and were inclined to regard

the other as still being static in important ways. On the one hand, mathematical economists and econometricians such as Kalecki, Frisch, Samuelson and Tinbergen constructed what were essentially difference equation models which, if suitable values of the parameters were chosen, produced cycles around an equilibrium level of income. They, implicitly or explicitly, adopted Hicks's definition of dynamic economics as analysis in which there are lags so that it is necessary to date variables.

On the other hand, for Harrod the essence of dynamic economics was that the variables to be explained were rates of change and that the trade cycle had to be viewed as an oscillation around an equilibrium rate of growth. The following quotations sum up the differences nicely.

In a League of Nations volume on trade cycles, later reprinted by the United Nations, Haberler commented on models that are based on the interaction of the multiplier and accelerator. He said: 'The technique of the theoretical analysis of these relationships has been greatly improved in recent years. The analysis has become more explicitly dynamic, that is to say the relationships in question are all interpreted so as to imply time lags; the magnitudes are being carefully "dated"…' (1946, p. 473). And, in a footnote to the above, 'Mr Harrod's system is incompletely dynamized; he introduces the dynamic acceleration principle but he still interprets the multiplier as an instantaneous relationship'.

Harrod was equally black and white in his view: 'In Dynamics, the fundamental conditions will themselves be changing, and the unknowns in the equations to be solved will not be rates of output per annum but increases or decreases in the rates of output per annum' (1948, p. 4). And in discussing Parts III & IV of Hicks's *Value and Capital*, which are entitled 'The Foundations of Dynamic Economics' and 'The Working of the Dynamic System', Harrod says that they only 'allegedly deal with dynamic economics' (1948, p. 9).

Lags: Inherently Misleading or Just Misused?

Harrod spent a great deal of energy pointing out the essential difference between the two approaches and arguing that his definition of dynamic economics and type of analysis was more fruitful than the rival one. Harrod (1948) Chapter 1, (1951) passim, and (1960) paragraph 7 are all examples of this sort of activity but the list is by no means exhaustive. However, the fact that Harrod sharply distinguished his model from both that of Hicks (in *Value and Capital*) and that of Samuelson (1939) and other similar difference equation models, did not necessarily mean that lags had no part to play, even in his basic instability theorem.

He certainly often wrote as if lags belonged to a second stage of the analysis, after the basic cause of the instability in capitalist economies had been established. For example in Harrod (1948) he said: 'It is far from my purpose to give a finished theory of the trade cycle. Lags, psychological, monetary and other factors, no doubt play their part. I should suggest that no theory can be complete which neglects the fundamental causes of instability expressed in the equations which have been set out' (p. 89). Moreover, at various places (e.g. Harrod 1960, p. 279) he maintained that the 'fundamental concept in dynamic economics … is the rate of increase that obtains at *a given point in time*' (emphasis in the original). Nevertheless, as his critics pointed out, he was not consistent. He employed lags himself, not just in detailed trade cycle theory, but in establishing his 'fundamental dynamic theorems'. In early work he explicitly acknowledged his was a form of

period analysis, for example defining G as the difference in output in two successive years divided by the level in output in one of those years and stating that 'we suppose the period to be short' (1939, p. 16) – short not instantaneous. In later work he used dated variables even more, and explicitly related what happened in a period to the outcome of the previous period (see, for example, his 1959 *Economic Journal* article, p. 459).

The first substantial comment in his letter to me is one approving my difference equation formulation of his theory which was later published in the June 1962 issue of the *Economic Journal*. This suggests that it was not lags *per se* but the way they were used to which he objected. There are several reasons why Harrod might be expected to look more favourably on my model than on most of those seeking to give a formal mathematical expression of his theory. First, the variable to be explained is a rate of growth. As was pointed out above, Harrod thought that the essence of dynamic economics was using rates of change as the dependent variables. Secondly, the model published in 1962 not only had the usual rigid accelerator version of Harrod's model but also a flexible accelerator version. It seemed to me at the time, and still does, that the discussion in Harrod (1948) implied that he was using a flexible accelerator. Along with a statement about his assumption relation to expectations, the use of a flexible accelerator was made explicit a few years later.

> In my analysis I assumed that on the line of 'warranted' advance the existing condition of stocks and equipment was satisfactory and that the size of the current order was based on an extrapolation of the rate of increase of put-through in the recent period. But in the fields of centrifugal forces lying on either side of the warranted line, I assumed that orders are also influenced in the upward field by a shortage of stocks and equipment and in the downward field by their redundancy. (Harrod 1951, p. 273)[3]

Thirdly, as the preceding quotation shows, my assumption about expectations was the same as that made by Harrod, i.e. that 'entrepreneurs expect the most recent past rate of growth to continue in the next period' (1962, p. 368). However, Harrod (1951) did point out that this expectations assumption was only a sufficient condition, not a necessary one, for his results to hold.

These three points are probably enough to explain Harrod's approval of my 1962 model, but I like to think that one can use that approval to support a particular interpretation of Harrod's views on difference equation models. Kregel (1980) and Besomi (1998) both stress that Harrod was concerned with what was happening at a single point in time. Besomi has set out a number of arguments Harrod used to reject the difference equation models of Samuelson and others. He gives considerable weight to three in particular. One is that the most fundamental part of dynamics is concerned with the analysis of a system of mutual relationships and especially the determination of the equilibrium rate of growth (Besomi 1998, pp. 115, 118-19). The second is that discussion of this equilibrium growth rate requires 'an examination of the state of the system at a single given instant' (Besomi 1998, p. 115). Thirdly, for Harrod, the true cause of the cycle was not to be found in errors or frictions (which he considered were the cause of lags) but in the instability of equilibrium which meant that after any disturbance the economy did not quickly return to equilibrium (Besomi 1998, pp. 112-15).

There can be no doubt that Harrod held strongly to the first and the third of these reasons for rejecting the work of Samuelson, Frisch and others. For example,

the first is why he considered the dynamic sections of *Value and Capital* only 'allegedly dynamic' and the third he stated again and again was a 'fundamental dynamic theorem'. However, I would argue that the second point was adopted as much as a matter of convenience as a fundamental point.[4] It has already been noted above that Harrod himself actually used a period, not an instant in time, in establishing his fundamental theorems. He argued that there was some force in the view that the acceleration principle itself implied a lag, but he 'deliberately neglected' the study of lags 'to get the clearest possible view of the forces determining the trend and its influence as such' (Harrod 1939, p. 20) and that 'where you get steady movement, a lag has no meaning' (Harrod 1948, p. 132). The interpretation that Harrod concentrated on simultaneous rather than lagged relationships as a matter of convenience is strengthened by his approval of a fully-fledged third-order non-linear difference equation model which established that Harrod's assumptions, properly understood and carefully formalised, led to an unstable equilibrium rate of growth.[5]

Expectations

In his 1939 *Economic Journal* 'Essay' and his 1948 book, Harrod was rather vague, to say the least, about expectations and entrepreneurial decision-making. In the book, for example, the closest he comes is to say that when 'goods in the pipe-line or the equipment [are] insufficient to sustain existing turnover … orders will be increased' (Harrod 1948, p. 85). He even makes it explicit that he is not sure whether this is because entrepreneurs expect next periods' income and demand for their output to be the same as this period's or whether, as is more fitting in a dynamic analysis, they expect next period's growth rate in output to be the same as this period's (Harrod 1948, p. 86).

This vagueness was seized upon by critics and in 1951 Harrod set out much more precise assumptions. As indicated above Harrod's normal assumption was that entrepreneurs made investment decisions on the basis of 'an extrapolation of the rate of increase of put-through in the recent period' (Harrod 1951, p. 273). However, he made it quite clear that this only related to induced investment. In the letter to me his reaction to my suggestion that entrepreneurs might take a longer view is to say that, if this occurs, the effect is to shift some investment from the induced to the autonomous category. This repeats what he said in Harrod (1951, p. 267) where the proportion of autonomous investment is represented as a variable varying continuously from one in an extremely short period to zero in an extremely long period. This reinforces the view that concentrating on an instant in time was adopted for convenience since if it is taken to its logical extreme it implies zero induced investment, whereas induced investment is at the heart of Harrod's dynamic analysis.

In his later writings Harrod used expectations to make what he thought an important point. Not surprisingly, many thought that the equilibrium rate of growth derived in the 1939 'Essay' and the 1948 book was extremely unstable. Harrod's exposition made this very likely. Consider, for example,

> G [the actual growth rate of output] is a quantity determined from time to time by trial and error, by the collective trials and errors of vast numbers of people. It would be great luck if their collective appraisals caused them to hit *precisely* on the value Gw [the equilibrium growth

> rate]. But if they do not their experience will tend to drive them farther and farther from it. (Harrod 1948, p. 86; emphasis added)

The idea of great instability, if not the word knife-edge, was probably inevitable.[6]

However, Harrod objected strongly to the knife-edge terminology, and with good reason since such extreme instability is not a characteristic of economies in the real world (Harrod 1973, pp. 32-33). He maintained that because of frictions in the system a very small deviation would not produce instability. Harrod (1970) identifies these frictions as 'degree of conservatism, sensitivity to current changes day by day, uncertainties about the future, sensitivity to changes, changes of expectations, the kind of phenomena that affect expectations etc' (p. 740). The view that Harrod lumped expectations together with lags as a complication to which one does not have to pay too much attention when deriving his fundamental theorems is correct. He regarded both as frictions or imperfections. However, expectations did become important in Harrod's thinking as a way of reducing the instability in the system to a realistic level. In the early 1960s, this idea appears still to have been embryonic but it was there. A decade later it was fully developed.

Parameter Values

The last point to be noted in Harrod's letter is very straightforward and can be dealt with most briefly. Yet it is the most surprising of all his points. One of the reasons Harrod gave for the superiority of his approach to dynamic economics over the difference equation approach was that it did not rely on any particular parameters to produce instability. For example, in comparing his approach to that of Samuelson, he said:

> In Professor Samuelson's model there may be a run-away movement towards infinity or an explosive cycle or a damped cycle or just a once-over movement to a new level; which of these happens depends on the coefficients assigned to the propensity to consume and to the capital requirement induced by the increment of output (or the accelerator…). On my system there will be a run-away movement to infinity whatever the values of these coefficients. (Harrod 1951, p. 263)

More generally, in Harrod (1948) he stated in the lead-up to his fundamental dynamic theorems: 'I believe that we are on the way to certain basic truths, which are independent of complications that have to be introduced when we seek to build up a more detailed picture of the whole process' (p. 80). The values of particular parameters (in this case the size of the accelerator) along with lags are the most important of these complications. These views were not just expressed in his early work on dynamics. For example they were put at greater length in Harrod (1960, p. 277).

Yet, in January 1962, Harrod wrote, 'I still like to think that my formula provides the starting point for considering these matters and that on probable estimates of the values involved there is likely to be instability'. This statement comes at the end of the letter. Harrod had argued that, although each of the three points I raised in the 1960 article reduces the degree of instability, none by itself will eliminate it. Now at the end he admitted that it is theoretically possible that the combined effects of all three could do so. Nevertheless, he considered it unlikely that any realistic estimates of the parameters would be such as to remove the stability altogether.

At one level this is a major change in his position, but the practical effect is small. The change in what is theoretically possible does not affect Harrod's equilibrium rate of growth and he can keep his conviction that in a capitalist economy, of the type existing since the industrial revolution, this equilibrium growth rate is unstable.

Conclusion

Harrod's letter gives interesting insights into two aspects of his analysis: the difference between his dynamic analysis and that of the mathematical economists and econometricians; and the universal nature of that analysis. Kregel and Besomi argue that Harrod thought his fundamental dynamic equations were about rates of growth in an economy at a point in time, and therefore it was completely different from the use of periods, lags and difference equations that was the predominant form of dynamic analysis from the mid-1930s to the mid-1950s. There is no doubt at all that Harrod thought his analysis 'radically different' (1951, p. 271). But this was not because period analysis was involved in one case but not in the other. His approval in the letter of a third-order non-linear difference equation as embodying a formal version of his analysis shows that. The difference was that Harrod was concerned to establish that there was an equilibrium rate of growth in a capitalist economy but that, unlike a static equilibrium, it was inherently unstable. This instability was a necessary cause of the cycle. Establishing this came first.

Harrod's comments on expectations in the letter were basically the same as those he had made a decade earlier. However, in 1961, Harrod was more open to the implications that this approach to expectations reduced the instability of the equilibrium growth rate.

His last comment, that expectations along with particular values of some coefficients could remove this instability, is the bombshell in the letter. But, since it is immediately qualified by the statement that such values are unlikely, the resulting damage is not great. Even without the qualification, the bombshell leaves untouched the most basic difference between Harrod and Samuelson, and Frisch and Hicks about the essential characteristics of dynamic economics. Harrod dismissed their work as only pseudo-dynamic economics because:

> what we ought to be looking for, beyond or beneath the oscillations, as the proper or normal effect of continuing changes, is a steady rate of change in each of the dependent variables. It may be that in fact in an advancing (or declining) economy there is a persistent failure to achieve those steady trends of increase which the changing fundamental conditions require, just as in a generally static economy there may, owing to the continued impact of detailed changes or some oscillation, be persistent failure to achieve the stable equilibrium which fundamental conditions indicate. But just as it is important to know what the stable equilibrium would be, even if it is not achieved from moment to moment, so in the dynamic field it is necessary to know what the steady lines of advance would be, as a basis for analysing why actual lines of advance depart from them and behave as they do. (Harrod 1948, pp. 9-10)

The existence of an equilibrium growth rate is not challenged. The demonstration that it is unstable now requires more empirical assumptions, though only ones which Harrod believed would be 'safe', a word he used in a very similar

context. The change, from a theoretical model which holds irrespective of the values of the parameters to one which holds for plausible values of the parameters, is a major change in theoretical structure. However, it may not be of great consequence in analysing specific economies and formulating policy advice. Harrod was not interested in theorising about hypothetical economies that might not bear any relationship to those that actually existed. He believed that his analysis was 'of urgent and vital relevance to the immediate problems' of specific economies (Harrod 1948, p. vi). The comments in his letter do not contradict that belief.

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Notes

1 The text of the letter is contained in the appendix to this paper.
2 See Besomi (1998) for a longer description of the differences between the two.
3 Harrod uses the word 'put-through' because he is talking about the expectations of a representative entrepreneur, whereas I make an assumption at the macro level with the representative entrepreneur approach only implicit.
4 Pugno (1998) takes a stronger position. He argues the lags are a necessary part of the mechanism which produces instability and that Harrod knew this. Pugno points to the 1951 article in which Harrod both said that current and recent conditions determined induced investment in his model and accepted the proof in Alexander (1950) that 'there will be a run-away movement to infinity whatever the value of [the] coefficients' (Harrod 1951, p. 263).
5 Harrod's revision of his treatment of expectations discussed in the next section also supports this view that he adopted a simultaneous approach as a matter of convenience.
6 Actually, in the 1939 'Essay' (p. 26), Harrod suggests that a deviation from the equilibrium rate of growth could have to last as long as 6 months before a divergence from that equilibrium rate of growth occurred, but this was generally overlooked.

Appendix

15/1/62

Dear Mr Nevile,

I was so interested and gratified to learn that you have been doing this work on my equation. I like your mathematical formulation (in typescript). [This was published unchanged as Nevile, 1962.]

As regards your article [Nevile, 1960] I think that I cannot quarrel with your statement of the tendency of any relaxation in the three assumptions to lead us nearer stability.

I am not sure, however, that I take your point that likely changes in the rate of growth being less than a trebling (p. 482 towards bottom) affects my argument. My point is that to get stability people must consume less out of a higher income,

unless s is not higher than G and in that case they must save the *whole* of the increase of income. This is independent of whether the increase involves a trebling of G or a rise in G by 50%, or any other amount, I think that you miss the point that a smaller change in G entails a smaller increase in income. I think that my argument is independent of how great the change in G is.

If, as you rightly suggest, s is flexible and may rise with an increase in G this reduces the centrifugal force but does not, I think, dominate it, as you suggest in the last paragraph ending on p. 483.

You are right in thinking that the greater k the less the centrifugal force. But the presence of k only serves to eliminate the centrifugal force if s-k is less than G and one has to consider in what circumstances this, on a realistic appraisal, is likely to be so.

Taking a longer period view by entrepreneurs (your third point) operates in the same way as increasing k. Thus the drift of your argument is right.

I still like to think that my formula provides the simplest starting point for considering these various matters and that on probable estimates of the values involved there is likely to be instability.

Yrs sincerely,

Roy Harrod

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