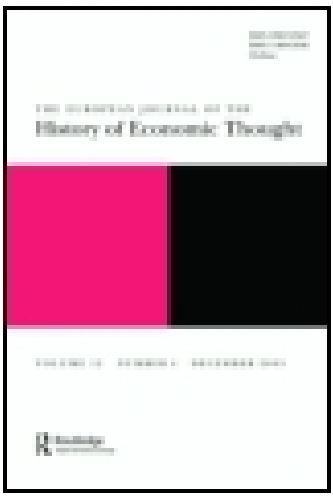

The European Journal of the History of Economic Thought

Publication details, including instructions for authors and subscription information:
http://www.tandfonline.com/loi/rejh20

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Published online: 26 Jan 2015.

Euro. J. History of Economic Thought, 2015
http://dx.doi.org/10.1080/09672567.2014.1001763

Routledge
Taylor & Francis Group

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The beliefs of economists about the history of their own discipline often reveal a failure to have read the original sources or to have considered their meaning. They are too willing to take history as having been faithfully transmitted by other economists or through the textbooks. The textbook narratives themselves more nearly reflect the self-image of the contemporary community than any compelling account of the actual history of the field. Hence, economists are apt to take positions on history that are strong nearly in proportion to their being wrong. We were brought up as economists with such a strong view of Roy F. Harrod's "growth model."

In their textbook, Robert Barro and Xavier Sala-i-Martin (2003, p. 17) write that Harrod "used production functions with little substitutability among the inputs to argue that the capitalist system is inherently unstable." Nearly every textbook that mentions Harrod in the context of growth – and many ignore him altogether – takes a similar line (cf. Besomi 1999, p. 209). Indeed, it is the story that one of the authors of this article has told himself (Hoover 2003, p. 413).[1] The common history of growth theory provides us with a narrative that presents Harrod's "Essay in

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[1] Another recent interpretation is to be found in La Grandville (2007): "[I]n the first part of the twentieth century, a fixed relationship was posited between factor inputs and output ... In one form or another this led ... Harrod (1948), and Domar (1946) to dire predictions about the future of the economy: it was bound either to a waste of resources or to ever-increasing unemployment, unless the growth rate of labour happened to be exactly equal to the savings rate divided by the fixed capital–output ratio" (La Grandville 2007, p. 16).

Dynamic Theory" as the origin of modern growth theory. Harrod, it is said, offered a model of long-term economic growth that ignored substitution of factors of production and, therefore, possessed an unreasonable "knife-edge" property in which any step away from the warranted rate of growth led inexorably to collision with a full employment ceiling or to mass unemployment and depression. The protagonist of this narrative, Robert M. Solow, corrected Harrod's knife-edge and transformed his model into the basis of modern growth theory. The now canonical story of the development of growth theory originates with Solow himself: Both the term "knife-edge" and the proposal that Harrod's instability arises from a fixed-coefficients production function – and can be cured by a flexible-coefficients production function – go back to Solow's "Contribution to the Theory of Economic Growth" (1956). He showed that the price system would guarantee that deviations of the warranted rate of growth from the natural rate (i.e., the rate consistent with the growth of population and technology) would be self-correcting. Far from being balanced on a knife-edge, long-term growth was stable.

Reading Harrod's (1939) and Solow's (1956) articles side by side shows that this potted history is a misreading: not only was Harrod interested in very different questions than Solow, he also applied a different framework of analysis and came to different conclusions. Daniele Besomi shows how the interpretation of Harrod's dynamics as a theory of growth originated in the late 1940s, was incorporated into growth models based on Harrod's idea of the interaction of the multiplier and the accelerator in the 1950s and 1960s, and, finally, was codified in the textbooks on macrodynamics and growth in the 1970s.[2] Besomi also notes that the discrepancy between the story told by textbooks and Harrod's work "does not seem to have attracted the attention of recent interpreters" (Besomi 2001, p. 80). Despite Besomi's work having been well received among historians of economics, the image of Harrod as the "founder of growth theory" is still widely spread among economists.

Where Besomi focuses on Harrod's work and develops the story of his "mistaken attribution" as an epilogue to his analysis of the development of Harrod's dynamics, we focus on Solow's interpretation and its prehistory. We start with a close reading of Solow's representation of Harrod's "Essay in Dynamic Theory," which we then compare with Harrod's "Essay." To approach the question, how it was possible for Solow to interpret Harrod in the way he did, we turn to a variety of interpretations of Harrod's work

[2] See Besomi (1998), Besomi (1999) and especially Besomi (2001). Other authors who have pointed to misrepresentations of Harrod's work are Kregel (1980) and Asimakopulos (1985).

between 1939 and 1956 in Section 3. Finally, we argue that there existed a "culture of misunderstanding": in the 1940s and 1950s, *modelling* became the dominant way to do economics and promoted the transformation of Harrod's dynamic theory into a model of long-run growth, as it was finally presented in Solow (1956). Solow's interpretation quickly became the canonical account of "Harrod's model."[3] At the end of the paper, we present unpublished material, in which Robert Solow not only acknowledges Harrod's work, but also emphasises its value for today's macroeconomics.

1. Solow's Harrod–Domar case

Tagged as "the Harrod–Domar line of thought," "the Harrod–Domar model" and "Harrod's model," Harrod's analysis provides the foil against which Solow displays the power of his own simple model of long-run economic growth. Harrod's "Essay" pointed to the pervasive instability in macroeconomic dynamics, which Solow characterised with a compelling metaphor: "The... conclusion of the Harrod–Domar line of thought is that even for the long run the economic system is at best balanced on a knife-edge of equilibrium growth" (Solow 1956, p. 65).[4] Solow frames his own model as the repudiation of Harrod's knife-edge. "All theory," Solow maintains,

> depends on assumptions which are not quite true. That is what makes it theory. The art of successful theorizing is to make the inevitable simplifying assumptions in such a way that the final results are not very sensitive. A 'crucial' assumption is one on which the conclusions do depend sensitively, and it is important that crucial assumptions be reasonably realistic. When the results of a theory seem to flow specifically from a special crucial assumption, then if the assumption is dubious, the results are suspect. [p. 65]

Solow claims to accept "all the Harrod–Domar assumptions" except fixed proportions (p. 66). We can see what Solow regards as Harrod's (and Domar's) assumptions by looking at what he himself assumes for his own model (pp. 66–68):

(i) an economy with single commodity (Y);
(ii) a constant savings rate (s), so that savings $S = sY$;
(iii) a constant-returns-to-scale production function with smooth substitution between capital and labour;

[3] It is worth noting from the outset that our concern is with Harrod and not with Evsey Domar. The bracketing of Harrod's analysis with Domar's growth model is actually part of the culture of misunderstanding that we are addressing.

[4] When no confusion arises, references to Solow (1956) and Harrod (1939) are by page number only, omitting author and publication date.

(iv) labour that grows at an exogenous rate (n);
(v) "no scarce nonaugmentable resource-like land";
(vi) flexible prices and wages;
(vii) constant full employment of factors of production;
(viii) and closely related to this last assumption, the identity of *ex ante* and *ex post* investment, which guarantees the identity of *ex ante* investment and savings, allowing the accumulation of capital to be described by the savings function alone.

Solow believes that the "crucial assumption," the one that would distinguish his stable model from Harrod's knife-edge model, is Assumption 3. Harrod's contrasting assumption of a fixed-proportions production function is not "reasonably realistic."

Let us see how Solow exposits "the Harrod–Domar case" as a special case within his framework. It is important to remember that we are explaining the connection between Solow's and Harrod's frameworks from the point of view of Solow's model and interpretation. Harrod, as we shall argue, would reject this interpretation – particularly the move of reducing the analysis to a question of the shape of the production function. Solow proposes that Harrod's analysis is grounded in a *fixed-proportions* production function.

$$Y = F(K, L) = \min\left(\frac{K}{a}, \frac{L}{b}\right), \tag{1}$$

where $Y =$ output, $K=$ capital, $L =$ labour, and a and b are production parameters. This function exhibits constant returns to scale, but it does not allow substitution between capital and labour. The production function can be recast into what has been called subsequently "intensive form":

$$y = f(r) = \min\left(\frac{r}{a}, \frac{1}{b}\right), \tag{2}$$

where $y = Y/L$, $r = K/L$ and $f(r) = F(K/L, 1)$.

Earlier in the article, Solow had worked out the dynamics of growth in any system with a constant-returns production function, yielding his "fundamental equation" (p. 69):

$$\dot{r} = sf(r) - nr. \tag{3}$$

The time rate of change of capital per worker ($\dot{r} = dr/dt$) is the difference between savings per worker ($sf(r)$) and the amount of additional

capital needed to outfit a growing number of workers with the current rate of capital per worker (nr). In terminology that gained currency later, *capital deepening* ($\dot{r}$) is the difference between additions to capital in the form of savings and *capital widening* (nr).[5]

Figure 1 – which is not Solow's own but is closely related to his Figure IV (p. 74) – uses Equations (2) and (3) to present the dynamics of growth graphically.[6] Panel A shows production, which rises as r increases at a rate of $1/a$ up to the point that $r = a/b$, where it becomes horizontal at an output $y = 1/b$. To the left of a/b, capital constrains output and there is some unemployment of labour; to the right of a/b, labour is fully employed and additional capital is redundant. The savings function $sf(r)$ is a simple scaling of the production function. It also has a kink at a/b where savings per worker $S/L = s/b$. The needs of capital widening are shown as rays from the origin with a slope n. Consider the particular growth rate of the labour force n_1. Its ray, labelled $n_1 r$, intersects the savings function at $r = \frac{s}{n_1 b}$.

According to Equation (3), the difference between the savings function and the capital-widening ray determines the time rate of change of capital, which is shown as the phase diagram in panel B. The difference reaches a maximum at $r = a/b$, falls to zero at $r = \frac{s}{n_1 b}$, and then becomes negative to the right of $r = \frac{s}{n_1 b}$. The arrows indicate that, for any value of r less than $\frac{s}{n_1 b}$, r increases and, for any value greater, r decreases. While the phase diagram is cast in terms of the time rate of change of r, the growth of output per worker ($\dot{y}/y$) is itself completely determined by the time path of r.

The growth rate of the labour force may be high enough (or, equivalently, the savings rate may be low enough) that the nr ray never intersects the savings function. The ray $n_2 r$ illustrates such a case, and panel B shows the corresponding phase diagram. In such cases, r collapses toward zero, no matter what positive value it takes at the start.

Solow relates his model to Harrod's in the following way. First, for Harrod (1939, p. 30), the natural rate of growth is given by the maximum rate permitted by the rates of growth of population, technology, and

[5] The term "capital deepening" is not mentioned in Solow's "Contribution," but he mentions it in a letter to Eisner the same year: "From a different point of view, if we imagine a society committed to maintain full employment at every moment of time, there will still be a multiplicity of ways this can be accomplished, some with much investment and some with little. Now Harrod is a subtle enough man to see this. He does make a few remarks to that effect in his book, as your quotes show. (By the way, the word 'deepening' goes well beyond this literature to Hawtrey and perhaps earlier.)" (Solow to Eisner, 14 June 1956, Solow Papers, Box 54, File E: 2 of 2). Domar, in the context of his work on economic growth and employment, also speaks of "deepening of capital" (Domar 1946, p. 142).

[6] Wan (1971, Figure 2.5, p. 41) presents a similar figure.

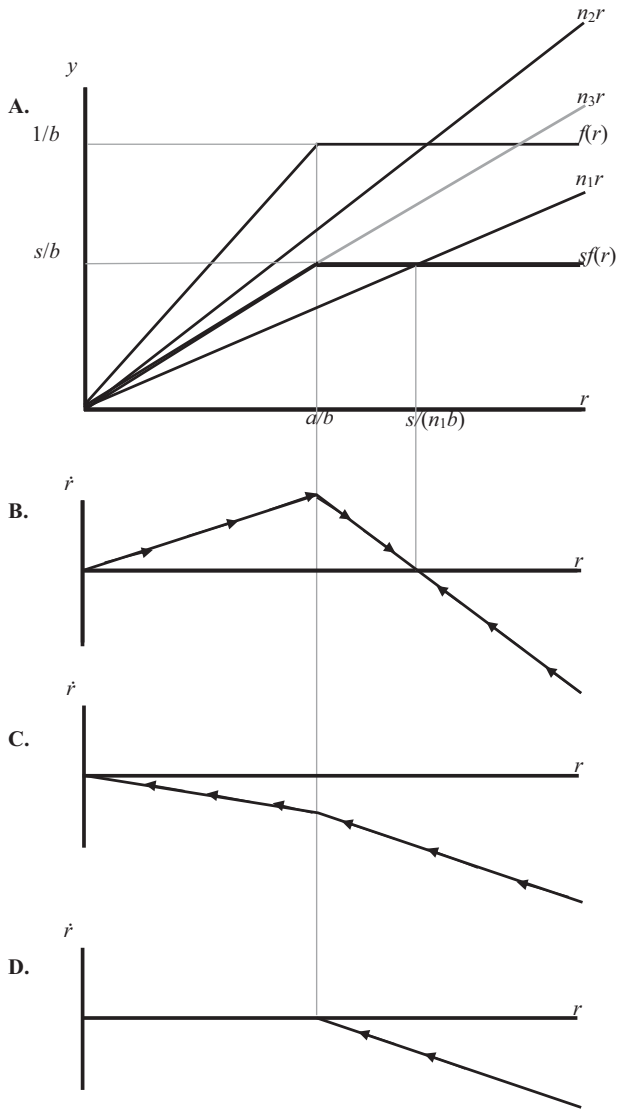

Figure 1 Solow's interpretation of the "Harrod-Domar Case"

labour–force participation. Thus, in a model without technical progress and a constant participation rate, the natural rate of growth $G_N = n$ unambiguously, and the slope of the nr ray is, in fact, the natural rate of growth. Solow interprets Harrod's warranted rate of growth as the growth rate

dictated by the production function and the savings propensity. From Equation (1), the time rate of change of output $\dot{Y} = \frac{\dot{K}}{a}$. Since $\dot{K} = sY$, the warranted rate of growth $G_W = \dot{Y}/Y = \frac{sY/a}{Y} = \frac{s}{a}$. Solow identifies Harrod's C as the incremental capital–output ratio, which is the inverse slope of a ray from the origin to the point of production.[7] Thus, $C = a$ for the production function (1). Substituting yields Harrod's own "fundamental equation":

$$G_W = \frac{s}{C}. \tag{4}$$

The slope of the savings function to the left of a/b in Figure 1, panel A, therefore, gives us the warranted rate of growth.

To understand Solow's analysis of the knife-edge, consider a case in which ray $n_3 r$ coincides with the upward-sloping portion of the savings function as shown in Figure 1, panel A (the corresponding phase diagram is in panel D). Start with the economy at a full employment, moving equilibrium in which it is growing at a warranted rate exactly equals to the natural rate ($G_W = G_N$). Any change of parameters that breaks the equality of the warranted and natural rates results in divergent movements of r. For example, an increase in the growth rate of labour to, say, n_2 (equivalent to a decrease in s) moves the economy from the phase diagram in panel D to that in panel C, which indicates the inexorable collapse of r, as the economy *relatively* disinvests.[8] On the other hand, a decrease in the growth rate of labour to, say, n_1 moves the economy to phase diagram B. The growth rate of output is unaffected, but *redundant* capital accumulates until the economy comes to rest again at $r = \frac{s}{n_1 b}$. In either of these extreme adjustments, the warranted and natural rates are permanently pulled apart. And any step away from their initial equality results in one of the extreme adjustments, which, according to Solow, define the knife-edge.

Solow attributes the knife-edge phenomenon to the fixed-proportions production function. To see why, consider the analogous diagram to Figure 1 when capital and labour are smoothly substitutable. In Figure 2, panel A, neither the production function nor the savings function has a kink, and both are concave to the r axis (compare to Solow's Figure I (p.

[7] Harrod (p. 16) himself does not use the term *capital–output ratio*, but refers to "the value of the capital goods required for the production of a unit increment of output." His use of the plural "goods" and the qualification "value," already indicates that he does not have a single-good, physical production function in mind. Where it causes no confusion, we will continue to refer to Harrod's C as the "capital–output ratio."

[8] Disinvestment is only *relative* to the size of the labour force because capital is permanent and there is no depreciation in the model.

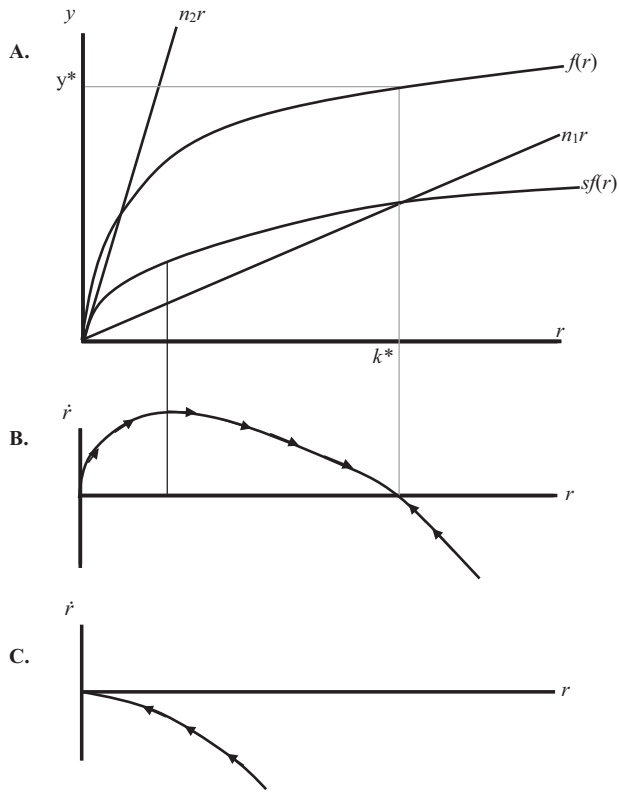

Figure 2 The Solow model

70)). The capital-widening ray $n_1 r$ is shown cutting the savings function from below. The positive intersection at $r = r^*$ corresponds to a growth rate n_1. The warranted rate of growth is given by the slope of a ray from the origin to the intersection of *sf(r)* and $n_1 r$. Since this ray must coincide with $n_1 r$, the warranted and natural rates of growth are equal. The phase diagram in panel B shows that any small deviation of r from r^* will reconverge on r^*; and, although the warranted and natural rates of growth may temporarily diverge, they cannot be pulled permanently apart. Any small change in n or s changes the location of r^*. Again, warranted and natural rates of growth would temporarily diverge, but would reconverge on n over time. There is no *excess* capital at r^*.

While there is no knife-edge in Figure 2, Solow does not over-claim: "There *may not be* – in fact in the case of the Cobb–Douglas function there never can be – any knife-edge" (p. 73, emphasis added). A knife-edge

could still occur if the capital-widening ray rose faster at the origin than the savings function as shown with the ray $n_2 r$ in Figure 2 (compare to Solow's Figure III (p. 72)). The phase diagram in panel C shows that for any initial r, progressive collapse toward zero capital and output is the only possible outcome. Such a knife-edge cannot occur with the Cobb–Douglas production function: since it has an infinite slope at the origin, any ray with a finite, positive slope must cut it from below.[9]

2. Harrod's dynamics

Where does Solow go wrong?

In one sense, everywhere. Solow claims that his model differs from Harrod's *only* in not assuming a fixed-proportions production function. In fact, one could make a case that Harrod does not subscribe to *any* of the assumptions numbered (i)–(viii) in Section 1 above. The literature pointing out differences between Harrod's dynamics and its neoclassical interpretation is vast.[10] Hence, we concentrate on the most fundamental disparities between Harrod's concept of instability and Solow's knife-edge (Section 2.1). Even though Harrod did consider a relationship similar to Solow's knife-edge, we argue in Section 2.2 that this does not lend support to Solow's interpretation of Harrod.

2.1 Harrod's instability

The *first* fundamental difference is that Harrod and Solow address different conceptual problems. Solow's article is entitled "A Contribution to the Theory of Economic Growth," and that title accurately conveys Solow's preoccupation with long-run economic growth. Solow self-consciously distinguishes between Keynesian pathologies and a world of perpetual full employment:

> Everything [in my model] is the neoclassical side of the coin. Most especially it is full employment economics – in the dual aspect of equilibrium condition and frictionless, competitive, causal system. All the difficulties and rigidities which go into modern Keynesian income analysis have been shunted aside. [Solow, p. 91]

[9] The "Inada conditions" later provided a set of sufficient regularity conditions to guarantee the existence, uniqueness, and stability of a well-behaved steady-state equilibrium in which no knife-edge phenomena can occur (Inada 1963).

[10] See, for instance, Hahn and Matthews (1964), Wan (1971), Hacche (1979), Pugno (1998), the contributions in Rampa *et al.* (1998) as well as the already mentioned Kregel (1980), Asimakopulos (1985), Besomi (1999) and Besomi (2001).

Solow singles out r^* as the "equilibrium," where r^* is the point at which, after all adjustments are done, the economy achieves a steady rate of growth equal to the natural rate – a long-run equilibrium – even though at every point along the adjustment path supplies equal demands and saving and production plans are all satisfied, so that there is never any disequilibrium in a wider sense.

Harrod's article, in contrast, is entitled "An Essay in Dynamic Theory." The implicit contrast is not between short-run and long-run or between full employment (neoclassical) and less than full employment (Keynesian) but between dynamic and static:

> Static theory consists of a classification of terms with a view to systematic thinking, together with the extraction of such knowledge about the adjustments due to a change of circumstances as is yielded by the "laws of supply and demand...". [Dynamic] "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 static theory for the study of rest. [Harrod, p. 14]

Harrod defines "dynamic" broadly "as referring to propositions in which a rate of growth appears as an unknown variable" (p. 17).[11] Thus, dynamics includes more than models of economic fluctuations, such as formal multiplier–accelerator models in which dated variables and explicit lags play a crucial role. He regards such models as perhaps explaining oscillations about trends, but he also suggests that oscillations in the trend itself are a crucial part of dynamics (p. 15). In contrast to Solow's equilibrium at r^*, in which growth had settled in to a steady rate, Harrod holds that

> [t]he line of output traced by the warranted rate of growth is a moving equilibrium, in the sense that it represents the one level of output at which producers will feel in the upshot that they have done the right thing, and which will induce them to continue in the same line of advance. [Harrod, p. 22]

The equilibrium is moving, not only in that the economy on a warranted path is growing, but also in that the parameters that govern the warranted rate itself may change frequently without any sense of convergence to a steady-state "equilibrium" of Solow's type. Indeed, Harrod's object is not to analyze a particular path *ceteris paribus* for the economy, not to compare the values of variables at different periods, but to elucidate the forces that may systematically drive the economy away from its equilibrium,

[11] There was some ambiguity with regard to Harrod's definition of dynamics: At another place in the "Essay," Harrod defines dynamic theory as dealing with situations in which "certain forces are operating steadily to increase or decrease certain magnitudes in the system" (Harrod 1939, p. 14).

warranted path *at any particular time* (pp. 17, 24–25). Growth is a central concern, since growth and change are essentially synonyms, but *long-run* economic growth of the type that animates Solow simply does not define the agenda.

The *second* fundamental difference between Solow's interpretation and Harrod's essay is that Harrod makes no explicit assumptions about a production function, and his implicit assumptions do not warrant a fixed-proportions, constant-returns-to-scale production function, such as Equation (1) or (2):

> [aggregate output is] compounded of all individual outputs. I neglect questions of weighting. Even in a condition of growth, which generally speaking is steady, it is not to be supposed that all the component individuals are expanding at the same rate. [Harrod, p. 16]

Harrod saw aggregate output as a summary statistic and not as a single commodity. This might appear to be a small difference. Solow did not literally believe that the economy produced a single commodity; rather he made a strong, simplifying assumption. The importance of the difference becomes clear in Solow's derivation of the knife-edge from the sharply defined parameters of the production function. The incremental capital–output ratio (C) is a fixed parameter (a in Equations (1) and (2)) for Solow. It is not fixed for Harrod:

> The value of C depends on the state of technology and the nature of the goods constituting the increment of output. It may be expected to vary as income grows and in different phases of the trade cycle; it may be somewhat dependent on the rate of interest. [p. 17]

> 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... [p. 25]

While the capital–output ratio is not constant for Harrod, it is importantly independent of the actual rate of growth. Its independence is related to the distinction between the actions of the individuals and their aggregate consequences, a well-known Keynesian trope that frequently appears in discussions of fallacies of composition (Harrod, pp. 22–25). While the aggregate balance is governed by the warranted rate of growth (and, therefore, by C and s), the actual rate of growth is governed by the reactions of many individual producers to particular conditions of over- or under-production. Individually, rational responses to these particular conditions drive aggregate actual growth away from the warranted path. *Harrod's instability* is, then, *pace* Solow, not an unstable relationship

between the warranted and natural rates of growth but an unstable relationship between the warranted and *actual* rates of growth.[12]

Where Solow's own model involves an endogenous adjustment of the capital–output ratio to that required by the natural rate of growth, endogenous adjustment is not critical for Harrod. It is not, however, that he denies the possibility. C is not a constant; it may respond over time to excess capacity or other factors. Still, what matters to Harrod is its value at a point of time. He is not primarily interested in the specific paths of output nor in the long-run consequences of some change in parameters or policy action. Rather he wants to point out the difficulty in staying on a warranted path at any moment. Where Solow emphasises the ultimate consequences of the knife-edge, Harrod emphasises only how hard it is to stand on such a narrow support. Solow (p. 91) introduces the metaphor of a tightrope as an alternative to the metaphor of the knife-edge.[13] As Solow conceives of the problem, the question of interest is where does the tightrope ultimately lead; as Harrod conceives of the problem, the question of interest is how difficult it is for the tightrope walker to keep his balance.

The second fundamental difference between Solow's interpretation and Harrod's essay is closely related to a *third* – and probably most fundamental – difference: for Solow *ex ante* savings and investment are always equal to *ex post* savings and investment. "A remarkable characteristic of the Harrod–Domar model," Solow writes "is that it consistently studies long-run problems with the usual short-run tools" (p. 66). This is not a characteristic of Harrod's analysis – remarkable or otherwise – because, as we already showed, Harrod is not concerned with long-run problems, but with the instability of the actual growth rate relative to the moving equilibrium (warranted) growth rate – a short-run problem for which he believes short-run tools are appropriate. Solow contrasts the warranted and the natural growth rates; Harrod mainly contrasts the warranted and the actual rates. It is 17 pages into a 30-page essay before Harrod so much as

[12] The survey of Hahn and Matthews (1964, pp. 804–805) already drew a distinction between instability in the sense of a failure of equilibrium convergence to the steady-state path ("equilibrium dynamics') and instability in the sense of nonconvergence from a nonequilibrium point to an equilibrium, but not necessarily steady-state, path ("disequilibrium dynamics"). Hahn and Matthews (p. 810) see Harrod as noting both kinds of instability, but they reserve the term *knife-edge* for the second kind of instability. This is puzzling since "knife-edge" is Solow's coinage and, as they note, he is concerned only with the first kind of instability.

[13] Besomi mentions Yeager's (1954) "nervous tightrope walker" as the forerunner of Solow's knife-edge (Yeager 1954, p. 59, cited after Besomi 1999, p. 204).

mentions the natural rate of growth, and, by that point, the core analysis is complete.

Harrod defines C 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... [T]he term *ex ante*... will be used in this sense. [p. 19]

Harrod proceeds in the standard Keynesian manner: when *ex post* investment falls short of *ex ante* investment, output is stimulated as producers react to unexpected and undesired shortages of "stock [i.e., inventories] and equipment," and conversely when *ex post* investment exceeds *ex ante* investment (e.g., p. 21). Harrod's principal mechanism is "a marriage of the 'acceleration principle' and the 'multiplier' theory" (p. 14). It is not, as Solow suggests, the wrong tool for the job; it is a different tool for a different job.

The *fourth* fundamental difference between Solow's interpretation and Harrod's essay is that Solow's knife-edge and Harrod's instability along the warranted growth path are almost completely unrelated ideas. As Solow describes it:

> Were the magnitudes of the key parameters – the savings ratio, the capital-output ratio, the rate of increase of the labour force – to slip ever so slightly from dead center, the consequence would be either growing unemployment or prolonged inflation. [p. 65]

Solow's analysis of the knife-edge concerns the failure of the warranted rate to adjust to the natural rate, where Harrod's instability concerns the divergence of the actual rate of growth from the warranted rate. The radical difference between them is clear from the fact that they work in opposite directions.

As we have shown in Section 1, if an economy with a fixed-proportions production function starts with equality between the warranted and natural rate and if that equality is upset, for example, by a fall in the savings rate (s), which lowers the warranted rate (s/C), then the economy as described in the phase diagram in Figure 1, panel C, crashes toward zero output. This is one side of Solow's knife-edge.

Consider Harrod's analysis of the same fall in the savings rate. The fall in the savings rate reduces the warranted rate of growth below the actual rate of growth. "Savers will find that they have saved more than they would have done had they foreseen their level of income... Consequently they will be stimulated to expand purchases, and orders for goods will consequently be increased" (Harrod, p. 21), which widens the divergence

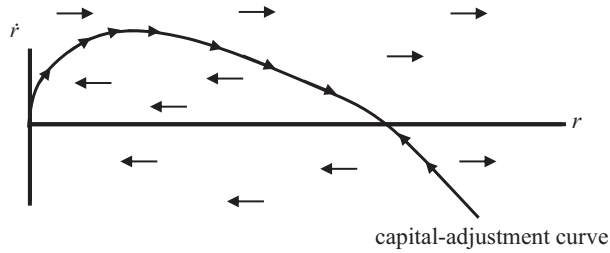

Figure 3 The phase-space of the Solow model

between the actual rate of growth G and the warranted rate G_W. While in the case of Solow's knife-edge, a fall in the savings rate directs the economy toward lower levels of output, in the case of Harrod's instability, it directs the economy toward higher levels of output. Solow's knife-edge and Harrod's instability concern the relationship of different growth rates (G_W and G_N for Solow; G and G_W for Harrod); and, for any change in parameters (s, n, C), they work in opposite directions. They address nearly orthogonal issues.

However, Harrod's instability can arise in Solow's own model provided that *ex ante* and *ex post* investment is allowed to diverge. If we interpret the capital−adjustment curve in Figure 3 (corresponding to panel B in Figure 2) as we did in Section 1 as governing the transient movements in the warranted rate of growth as it adjusts toward the natural rate, then Harrod can be said to have directly described the situation analyzed using Figure 3 in terms that accurately reflect the way it is drawn. A fall in the savings rate is a Keynesian stimulus. Harrod writes:

> Suppose that one of these stimulants begins to operate when the actual rate is equal to the warranted rate. By depressing the warranted rate, it drags that down below the actual rate, and so automatically brings the actual rate into the field of centrifugal forces, driving it away from the warranted rate-that is, in this case, upwards. Thus the stimulant causes the system to expand. [p. 31]

2.2 The constitutional weakness of the macroeconomy

So far, we have argued that Solow's criticism of Harrod misfired because the key difference between them was found not in assumptions about substitutability in the production function but in the assumption that *ex post* and *ex ante* quantities are always equal. Harrod's instability is, therefore, the instability of the actual growth rate relative to the warranted rate, whereas Solow's knife-edge is the instability of the warranted rate relative to the natural rate. Yet, Harrod does consider the relationship of the

warranted and natural rates; does that not lend some support to Solow's analysis? We think not.

As mentioned before, Harrod considers the natural rate only late in the paper, after his main analytical conclusions have been established. The message of Figure 3 is that, even if over-time warranted and natural rates converge as in Solow's model, any change in parameters that temporarily drives them apart sets up Harrod's instability – with or without a fixed-proportions production function.

Harrod's concept of full employment is unlike recent macroeconomic analyses that relate full employment or potential output to something like Milton Friedman's natural rate of unemployment. The popular concept allows output to run above, as well as below, full employment or potential. In contrast, Harrod's conception of full employment is the typically Keynesian notion of a ceiling. An economy is fortunate if it operates close to the ceiling, but it can never operate above it. The economy cannot grow faster than the natural rate allows in the long run – that is, it cannot operate above full employment, even though it may grow faster than potential output itself grows when it starts below full employment. Harrod introduces a fourth rate of growth to his famous three: actual, warranted, and natural. The *proper warranted rate of growth* is "that warranted rate which would obtain in conditions of full employment" (p. 30) – that is, the rate of growth of potential output.

Harrod conjectures that the relationship between the proper warranted rate and the natural rate determines the *likelihood* of the economy operating below full employment for any length of time. He writes:

> The system cannot advance more quickly than the natural rate allows. If the proper warranted rate is above this, there will be a chronic tendency to depression; the depressions drag down the warranted rate below its proper level, and so keep its average value over a term of years down to the natural rate. But this reduction of the warranted rate is only achieved by having chronic unemployment.
>
> The warranted rate is dragged down by depression; it may be twisted upwards by an inflation of prices and profit. If the proper rate is below the natural rate, the average value of the warranted rate may be sustained above its proper level over a term of years by a succession of profit booms. [p. 30]

Harrod's conjecture amounts to a claim that when the warranted rate of growth exceeds the natural rate of growth, the economy displays a *constitutional weakness*: a tendency toward frequent recessions.

Harrod's thinking can be understood with the help of Figure 4, which shows the time path of output (Y) on a logarithmic scale, so that growth rates correspond to the slopes of the curves. In panel A, up to time t_0, the

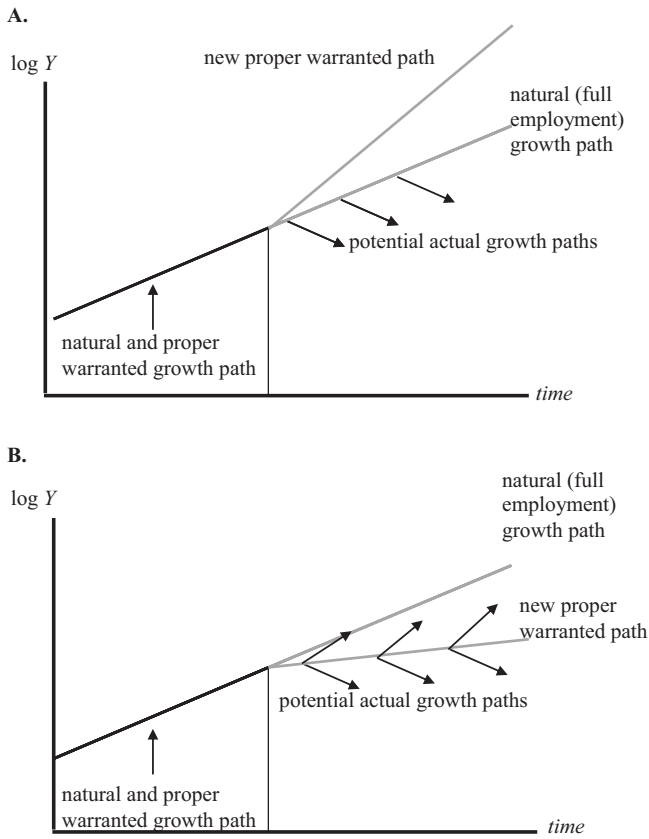

Figure 4 Instability on the proper warranted growth path

proper warranted rate and the natural rate coincide. At t_0, the proper warranted rate increases (s increases or C falls) as shown by the upper, grey path. Since the natural growth path is the full employment path, actual output cannot move into the area between the natural and warranted growth paths. Harrod's instability can manifest itself only in the downward direction as shown by the arrows.

In contrast, panel B shows a case identical up to t_0, in which the proper warranted rate falls at t_0 (s falls or C rise). Harrod's instability can still manifest itself downward, but the region above the proper warranted growth path and below the natural growth path is now feasible, so that it may manifest itself upwards as well. Instability can, in this case, drive the economy toward full employment and keep it there. Recessions are possible in both

cases; but, when the proper warranted rate exceeds the natural rate, they are inevitable; and, when the natural rate exceeds the proper warranted rate, full employment may prove a common outcome. Harrod's conjecture is that when the proper warranted rate exceeds the natural rate, the economy will be constitutionally weak in the sense that recessions are more frequent; whereas when the natural rate exceeds the proper warranted rate, the economy will be stronger, with fewer recessions and more time spent near full employment.[14]

Harrod's analysis of the possibility of a constitutional weakness in the economy owing to a proper warranted rate of growth above the natural rate has, to our knowledge, not been discussed in the economics or history of economics literature.[15] It is critical to distinguish our account of it from survey and textbook accounts of Harrod's analysis of the long run. Hahn and Matthews (1964, p. 810) and others see Harrod as pointing out two distinct problems. The first is the "knife-edge" as they (but not Solow) interpret it as the disequilibrium dynamics associated with a divergence between the actual and the warranted rates of growth. The second is Solow's original "knife-edge," which arises because there is no mechanism to bring the natural and warranted rates together, which is supposed to result in the progressive collapse of the economy into mass unemployment or uncontrollable inflation and excess capacity. Our view is that this second problem, which Hahn and Matthews see as a problem in equilibrium dynamics (see fn. 12), is *not* any part of Harrod's analysis. In fact, Harrod saw no reason for the factors that determine the warranted rate to stay continuously aligned with those that determine the natural rate or to adjust smoothly to divergences. But he did not predict radical collapse and argued that both C and s would change in the face of recession or inflation. The constitutional weakness is not a distinct process from Harrod's short-run instability, as Hahn and Matthews suggest. Rather the constitutional weakness is the possibility (or even the likelihood) of there being standing conditions that over time make it likely that the short-run

[14] In a related unpublished paper, Hoover (2008) provides a preliminary test that favours Harrod conjecture for US data.

[15] The proper warranted rate has, as far as we can tell, been mentioned only twice in the journal literature by Wright (1949, p. 326) and by Yeager (1954, p. 62, fn. 17). Neither appears to grasp its analytical importance and each treats it as a reflection of Harrod's confusion and a lack of clarity. Kregel (1980) and Besomi (2001) do discuss Harrod's related view that the warranted rate can be dragged up or down by extended divergence from the actual rate, but not the asymmetry that Harrod highlights as the constitutional weakness in the economy. Besomi (1999, p. 137) briefly mentions the proper warranted rate – but, again, only in the context of adjustment of the warranted rate.

instability is asymmetrically biased toward recession. The problem is not as radical as the calamity conjured by Solow: Harrod envisages not exponential collapse but recurring recessions and extended unemployment and operation of the economy below potential.

3. Cultures of understanding

We have argued that Solow's Harrod is a "misreading" in that Harrod did not aim at a model of long-run growth and did not assume a constant capital coefficient. How was such a misreading possible? Looking only at Solow's and Harrod's articles side by side, one might well wonder whether Solow's interpretation of Harrod reflects a reading of Harrod at all. In what follows, we would like to suggest that there are, in effect, different interpretive communities, each with its own interpretive practices and methodological and substantive presuppositions. Within each interpretive community, there are common practices of doing research and "cultures of understanding" that implicitly guide its work. Thus, what was "in the air" with respect to Harrod was a specific *image* of his work suggesting a certain culture of understanding that led to Solow's reading. The canons of an interpretive community may be explicit, but, more often, they are tacit and hardly noticed by its members.[16] In order to get a grip on the culture of understanding that guided Solow's reading of Harrod, we will first reflect on the "Harrod–Domar literature" that developed right after the Second World War, and then shed a light on Solow's modelling work before his 1956 "Contribution."

3.1 The "Harrod–Domar line of thought"

Besomi demonstrates that the interpretation of Harrod's work as a theory of growth developed in the 1940s, when the discussion was fuelled by the

[16] The notion of cultures of understanding bears a family resemblance to Fleck's (1935) concept of thought collectives, Kuhn's (1962) paradigms, and Lakatos's (1978) methodological research programmes. While we accept the genuine resemblance of these ideas to our own use of a "culture of understanding," we reject any extreme reading of them that suggests insuperable incommensurability – rather than intrinsic difficulty in communication – across communities. "Misreadings" and "misunderstandings" as described above are an intrinsic and prevalent feature of science as a cultural and social enterprise. Depending on the closeness and overlapping of communities and their practices difficulties in communication might be overcome more easily or pose serious difficulties of grasping the meaning of products of a different community. See Fish (1980) for the notion of interpretive communities and Hoover (1991, 1994) for arguments against insuperable incommensurability.

publication of Harrod's *Towards a Dynamic Economics* (1948) and of Domar's (1946) article in *Econometrica*. Domar stressed the similarity of Harrod's fundamental equation to his own (Besomi 2001, p. 79). Harrod belonged to the English Keynesian school. Its central concern in the decade before the Second World War had been with the problem of the trade cycle, motivated, of course, by the experience of the Great Depression. Harrod, like Keynes, pursued a Marshallian macroeconomics. Methodologically, it favoured using mathematics in limited ways to isolate the qualitative causal structure of the economy. Keynes understood dynamics to be a matter of isolating and cataloguing the casual forces that changed important economic variables (Hoover 2006). His *General Theory*, however, downplayed the forces of change in favour of articulating a new architecture for thinking about the economy as a whole. Harrod's "Essay" is, in part, a reintroduction of the explicit dynamic concerns of Keynes's earlier *Treatise on Money* (1930, p. 120) into the new macroeconomic framework. Growth was simply not the central issue.[17]

Solow's approach is also distinguished from Harrod's by its commitment to the practice of formal modelling. It is easy to forget that *models* as primary instruments of economics are largely a post-Second World War phenomenon. A crude measure of the rise of modelling is given by the fact that the frequency of the use of "model" or "models" rises in economics journals by 300% between the ten years before and the ten years after the Second World War. A smaller, but similar, increase also shows up in science journals.[18] The transition of economics into a modelling science had, of course, already begun before the war, and was epitomised in the work of Frisch, Tinbergen, and Samuelson.[19] Harrod was aware of this work and explicitly sought to distinguish the questions that he wanted to address in his "Essay" from the new emphasis on models consisting of formal systems in which the dynamics were driven by an explicit lag structure.

[17] For a treatment of the Harrod–Keynes correspondence, see Kregel (1980) and especially Besomi (1995); on Harrod's 1938 draft of the "Essay," see Besomi (1996); and on the changes he added reacting to Keynes' and Marschak's criticism, see Sember (2010). Young (1989) provides a detailed account of Harrod's work on the trade cycle leading up to the "Essay" (see pp. 48–50 on the importance of Keynes's *Treatise*).

[18] Data are drawn from searches on *JSTOR* archive, executed on 25 November 2012, for the use of "model" or "models" for 173 economics journals and 29 general science journals, counting the number of articles using one of the search terms as a percentage of all articles. For economics journals: 1930–1939 = 14%; 1946–1955 = 42%. For general science journals: 1930–1939 = 14%; 1946–1955 = 23%.

[19] Frisch (1933), Tinbergen (1939) and Samuelson (1939); also see discussions in Louçã (2007), Boumans (2005) and Morgan (2012, ch. 6).

Although the American growth economics of the 1950s represents a distinct community from the English trade-cycle economics of the 1930s, it took some time for the distinctions to emerge. Over the period between the late 1930s and the early 1950s, economists – the older Marshallian macroeconomists as well as the new formal modellers – investigated the implications of combining the consumption multiplier and the investment accelerator. And it is in this literature that we find the first traces of Solow's representation of Harrod.

When reading the papers about growth and dynamics between 1939 and 1956 in which Harrod is mentioned, it is particularly striking that almost all of them express some kind of doubt as to "what Harrod actually meant" with regard to various topics. Thomas Schelling (1947, p. 867) for example notes that "whether [Harrod] meant to imply that [non-growth-induced investment] actually would be proportionate to income, or only meant to give it formal expression, is not clear." Elsewhere, Schelling (1947, p. 868) mentions that Harrod's warranted rate of growth appears "to us as two definitions whose equivalence is seriously in question ... [O]n the one hand, they feel that they have done the right thing; on the other hand, they are induced to 'continue in the same line of advance.'" In a similar vein, Baumol (1948, p. 507, fn. 1) notes that "Mr. Harrod himself sometimes employs the one assumption [of saving as a constant proportion of this period's income] and sometimes the other [of savings as a constant proportion of the preceding period's income]." Wright (1949, p. 326) shows Harrod using six independent qualifications regarding the warranted rate of growth.

These doubts are also to be found in Solow's correspondence: Three years before the publication of his "Contribution," Solow "confesse[s] to a certain uneasiness in this Harrod maybe-maybe land of equilibrium growth, in which one never knows anything about the behaviour dynamics of the system. But as long as we work in this genre, we're stuck" (Solow to Johnson, September 28, 1953, Solow Papers, Box 56, File J: 1 of 2). Here, Solow demands a more clear-cut analysis, something he also recalls in an unpublished introduction to Harrod's 1946 lectures: "[Harrod's] 'theorems' will seem odd to a modern economist. They left Harrod's contemporaries confused too; there was considerable discussion at the time about what they really meant" (Solow 2009, p. 3).

We do not want to support Solow's reading by saying that Harrod's work was unclear to post-war American economists. Rather, we regard the commonly uttered doubts about "what Harrod really meant" as an indication for a specific culture of reading Harrod that was to develop at the time: several authors introduced different notations and formalisms to "clarify" Harrod's work and/or to represent it in a "more general" way. A common

practice was to combine Harrod's "Essay" with Domar's "Capital Expansion, Rate of Growth, and Employment" (1946) and to use Domar's "clearer" notations and formalisms for representing Harrod's dynamics. The first of these combinations is to be found in an article by Thomas Schelling, just one year after the publication of Domar's (1946) article. Schelling was at this time a teaching fellow at Harvard, where Solow was then a student. Describing Harrod's theory as a system of difference equations expressed in Domar's notation, Schelling implied that Harrod's savings rate is consistent with Domar'sα, thereby neglecting that α is the "marginal propensity to save" (Domar 1946, p. 140). In contrast, Harrod took into account the "fraction of income saved," i.e., the average propensity to save (Harrod 1939, p.29, cf. Harrod 1959, p. 454). By further equating Domar's σ, the "potential social average investment productivity" (Domar 1946, p. 140), with the reciprocal of Harrod's capital coefficient, Schelling investigated under which circumstances a "required rate of growth," which he referred to as the "Harrod–Domar $\alpha\sigma$," tends to maintain itself (Schelling 1947, p. 872).[20] Even though Schelling acknowledged many aspects of Harrod's work that, later, Solow neglected, he still derived the value for the "Harrod–Domar $\alpha\sigma$" to maintain full employment. Similar to Domar's, but not to Harrod's approach, Schelling looked at the requirements for a full employment equilibrium along a growth path – he did not address the instability of Harrod's moving equilibrium.[21] And so, despite explicitly recognising some of the differences between Harrod and Domar ("It should be added here that Harrod considers the equilibrium represented by this warranted rate of growth as essentially unstable," Schelling 1947, p. 868), Schelling still defined Harrod's work as equilibrium analysis, since "Mr. Harrod begins directly by inquiring what rate of income growth would tend to maintain itself" (Schelling 1947, p. 866).

[20] Harrod himself pointed to the difference between his capital coefficient and Domar's σ a few years later: "[H]e [Domar] designates the potential increase of output per unit of new investment by the symbol σ. I, on the other hand, make no explicit reference to this increased productivity, but ... considered how many units of new investment are required, on the assumption that the new investment is properly utilised, to produce an extra unit of output; this I designated C_r" (Harrod 1959, p. 452; cf. Hagemann 2009, p. 70).

[21] A similar argument was made by Hamberg (1952, p. 446, fn. 2) who investigated the consequences of the full employment growth rate exceeding the rate of growth required for the full utilisation of capital. He reproached Harrod for discussing only "the Keynesian case" – i.e., for not considering the full implications of the situation in which "the full employment growth rate exceed the full capacity one ..." and thinking only about the inflationary aspects of this situation." Hamberg, as Solow later, recognised only the possible divergence between the warranted and natural rates, thus ignoring Harrod's instability principle.

Schelling's reinterpretation of Harrod in the light of Domar laid the groundwork for the later treatment of Harrod and Domar as offering a common "model" and for interpreting Harrod's "Essay" as a "theory of growth."[22]

Furthermore, Schelling related Harrod's work to Samuelson's dynamics by supposing that Harrod proceeded from the assumption that investors as a whole projected the current rate of growth into the future "with such confidence as to guide their decisions by that criterion alone." Hence, Harrod "did go too far in attaching motivational significance to the [warranted] rate" (Schelling 1947, p. 870).[23] Schelling tied this interpretation of Harrod firmly to the formal dynamics of the 1930s that Harrod had explicitly eschewed: Schelling (1947, p. 872) observed that Samuelson (1939) "accepts as a postulate the very investment behaviour which Harrod tried to establish" (Schelling 1947, p. 872).

A year later, Baumol published an article that specifically addressed the connections between Harrod's "Essay" and Samuelson's (1939) article on multiplier–accelerator interactions. Baumol (1948) described Harrod's work as a "dynamic model" (p. 506), regarding it as a "normative study... indicating the sort of conditions which must be satisfied by the course of the level of the national income through time in order for investment demand to be satisfied" (p. 516). According to Baumol, Harrod's instability principle would not follow necessarily from his premises, as it required implicit assumptions about producer's expectations and their subsequent effects on production plans. Hence, "there appear to be possible alternative situations which will be characterised by considerably less instability than Mr. Harrod's argument might lead us to expect" (Baumol 1948, p. 514). Following a similar tactic to Schelling, Baumol reinterpreted Harrod's theory in a new notation "in the interest of ready comparability with the system of Prof. Samuelson" (Baumol 1948, p. 507).

The next year, Baumol (1949) formulated a "complete model," in which he made explicit the assumptions about entrepreneurial expectations that

[22] See Kregel (1980) for an earlier treatment of the differences between Harrod's work and the "Harrod–Domar" model.

[23] This point is also made by Asimakopulos (1985): "For Harrod's purposes, this moving equilibrium should have the characteristics of a trendline – it should be one that the economy would follow in the absence of disturbances. The development of the model within a Keynesian framework where uncertainty over future conditions prevailed made it difficult to explain why the entrepreneurs' investment decisions would lead the economy along such a path. Harrod, when challenged by Alexander, saw the need for an explanation, but his resort to a 'representative entrepreneur' [in Harrod (1951)] was an evasion of a real problem for his theory, not a solution" (Asimakopulos 1985, p. 633).

he had previously argued were implicit in Harrod's theory. Specifically, he assumed that entrepreneurs plan to increase their output at the warranted rate unless there is excess demand, in which case they plan to depart from the warranted rate by an amount sufficiently great to make up for excess demand. Baumol was convinced that it is an assumption like this that is required to make "Mr. Harrod's system work the way he says it does" (Baumol 1949, p. 629).

Once Harrod's theory had been subsumed into a formal model, it was a short step to reinterpreting it as embodying a fixed-proportions production function. Harold Pilvin (1953) offered perhaps the first such explicit interpretation in an article that is one of the few mentioned in Solow's 'Contribution.'[24] Pilvin (1953, p. 546) represented Harrod's and Domar's work by introducing a production function in order to provide a general treatment of "the growth problem" and, as with Baumol, to make explicit certain implicit assumptions in Harrod's formulations. Pilvin (1953, p. 548) referred to a "constant capital coefficient as employed by Domar and Harrod." He did not mention instability or cycles, and his major aim was to construct "a growth model of considerable generality" (Pilvin 1953, p. 546). Pilvin appears to be the first person in this literature to employ a graphical apparatus, similar to the one found in Solow's "Contribution," to represent the Harrod–Domar model.

Pilvin thanked Domar for his comments on his work (Pilvin 1953, p. 545), and Domar, in turn, considered Pilvin's explicit production function as very useful by showing that "the existing growth models" are an extreme case "of the more 'normal' one – the former does not allow for substitution between factors, the latter does" (Domar 1953, p. 562). By saying that "as an analytical device, a constant input coefficient is God-sent, but it is quite a simplification and it should be used with care, particularly over longer periods of time when it is known to be subject to change," he implied that Harrod actually assumed such a constant coefficient (Domar 1953, p. 561; cf. Hagemann 2009, p. 81).[25]

[24] Solow thanked John Chipman for a remark on Harrod's answer to Pilvin's article (Solow 1956, p. 83).

[25] In his answer to Pilvin's article, Harrod criticised the use of a production function and especially the accompanying assumption that the rate of interest "will move in such a way as to change the productive process employed, causing it to move in an appropriate fashion along the curve of the production function" . Harrod brings forward several arguments that, although the rate of interest "plays some part" in changing the capital–labour ratio, it is not as important as Pilvin thinks (Hagemann 2009, p. 83 and Besomi 1999, p. 204).

The American modellers of the immediate post-war years found it essential to translate Harrod's theory into a new and formal language, assimilating it to their own modelling practices. But to the degree that Harrod's central concerns – e.g., to identify the forces that would render explicit dynamic equations irrelevant or to isolate the constitutional weakness of the macroeconomy that made repeated slumps more likely – were omitted, a vital element of his work was lost in translation and the interpretive practices of the growth modellers unwittingly promoted a culture of misunderstanding that laid the groundwork for Solow's treatment of Harrod's "growth" model.

3.2 Solow and growth before the "contribution"

In the early 1950s, Solow worked on the aggregation of Leontief's input–output tables and dynamic linear programming.[26] Solow (1952) applied Samuelson's stability framework to linear systems, studying the relationship between the existence of an equilibrium and the stability of a dynamic system (cf. Boumans 2009; Assous 2013). Samuelson's concept of stability had been defined with respect to static equilibria. The next year, Samuelson and Solow applied concepts derived from P.H. Leslie's population mathematics to work out the concept of "balanced growth." They also "replace[d] the hypothesis of fixed proportions by the more general one that productions functions are homogeneous of first degree" (Solow and Samuelson 1953, p. 412).

Having worked out dynamic versions of Leontief's input–output system, Solow (1953–1954) proceeded to identify "the Leontief and Harrod dynamic models" (cf. Boumans 2009, pp. 138ff). Referring to Leontief's system in matrix form, he suggested to "banish the thought that a and b are matrices and x_t is a vector and think of these quantities as ordinary numbers. Then Equation (1) looks suspiciously like Mr. Harrod's system. In fact, deep down, it is Mr. Harrod's system" (Solow 1953–1954, p. 74). Solow offered three reasons to identify the "Leontief and Harrod dynamic models": both traced out moving equilibrium output paths; both defined equilibrium by the *ex post* justification of investment plans (perpetual appropriateness of the existing capital stock to the current level of output); and neither had anything to say about the actual time-path or contemplates in any detail the possibility or consequences of disequilibrium. Although Solow conceded that Harrod was "of course, quite interested in the consequences of departure from equilibrium growth, and in the instability of the

[26] For the contexts and practices Solow's (1956) model emerged from, see Halsmayer (2014) and Backhouse (2012).

latter" (Solow 1953–1954, p. 75, fn.1), he saw it as Harrod's fault not to have provided explicit causal dynamics. As a result, he regarded Harrod's "model" as a special one-commodity case of the Leontief dynamic system.

Solow's first representation of Harrod's dynamic theory equated it with a dynamical Leontief input–output system. Aggregated to a one-commodity economy with a fixed-proportions production function, Harrod's dynamic theory was transformed into a model of long-run growth. Solow interpreted Harrod in a way that made sense for him within the emergent work on formal models of growth. Yet, it is an interpretation that could only be regarded as a deep misunderstanding and, even inexplicable misrepresentation, from the point of view of Harrod. Solow presented Harrod having developed a moving-equilibrium output path; Harrod did not talk about long-run equilibrium. Solow took Harrod to address trend growth; but Harrod principally addressed the trade cycle.[27]

The advantage of Solow's interpretation is stated clearly at the end of the paper: his formal system provides the basis for a "more complete causal dynamics of the kind usual in business cycle theory. In these terms, questions beginning 'What would happen if' would have exact answers" (Solow 1953–1954, p. 79). The disadvantage is that Solow's model sets aside the questions and their suggested answers that were the focus of Harrod's analysis. The loss is clear in a letter to Frank Hahn in which Solow opines that "[t]he Harrod–Domar legacy of paying attention only to equilibrium paths is by now an obstacle. All these ad hoc stability statements about what happens off such path are useless without an explicit causal dynamics." According to Solow, such explicit causal dynamics have to include "a theory of uncertainty – and therefore,... a theory of investment" (Solow to Hahn, March 23, 1959, Solow Papers, Box 55, File H: 1 of 3).

Harrod would surely reject the idea that his principal concern was with equilibrium paths and not causal dynamics. In fact, he acknowledged the role of uncertainty in a theory of investment as implicit throughout his analysis. It is only through the lens of a formal model, in which these features are not easy to treat realistically, that Harrod can be regarded as having ignored them. Where Harrod had seen himself as having taken a first step toward addressing these issues, Solow sees them as issues that have become pressing only at a later stage in the evolution of growth models. For Solow, they are the next new thing; for Harrod, they are the reinvention of the wheel.

It is important to recall that interpretive communities are not hermetically sealed, but overlap, interpenetrate, communicate, and trade. Solow's

[27] Harrod (1973, ch. 3) shows Harrod's battle against the interpretation of his instability as a "knife-edge" problem.

interpretation of Harrod was not uncontested at the time and criticism led Solow to defend his interpretation vigorously:[28]

> I'm damned if the impression you leave [Harrod's "Dynamic Economic Theory"] with is one of flexibility. Harrod achieves this by first saying that perchance the rate of interest can't be driven down far enough . . .then giving the impression that what is needed is an eternally falling profit-interest rate, and then *assuming* that the rate of interest is constant and with it the capital coefficient. For my money this is the moral equivalent of fixed proportions. No doubt Harrod can't be held responsible for what his followers have made of the doctrine, but he certainly gave them a head start. [Solow to Eisner, May 14,1956, Solow Papers, Box 54, File E: 2 of 2]

Answering Kaldor, Solow admitted that "I was probably undesirably elliptical in referring to Harrod in terms of fixed coefficients. *In effect* that's where he comes out, but he does it by saying that V depends on the rate of interest, but the rate of interest is stuck" (Solow to Kaldor, November 9, 1959, Solow Papers, Box 57, File K: 1 of 4). Also in his correspondence with Harrod Solow insisted that Harrod used fixed proportions:

> [I]t still seems to me that even when the equation is deduced on other grounds it is dependent on the tacit assumption that the marginal productivity of capital (or the capital coefficient) is a technical constant... No matter how I take it, I seem to find your equation to be dependent on the constancy of the social yield or capital. [Solow to Harrod, September 23,1960, Box 1, File 1960][29]

Throughout these debates about growth and dynamics Harrod's theory was interpreted as a *model* of growth – an attribution that Harrod himself vigorously rejected:

> many years after I had made certain formulations in the field of growth theory and after Professor Domar had made similar formulations, there began to be references to the 'Harrod–Domar model.' I found myself in the position of Le Bourgeois Gentilhomme who had been speaking prose all his life without knowing it. I had been fabricating 'models' without knowing it. [Harrod 1968, p. 173]

As Besomi (2001, p. 88) shows, Harrod had consistently insisted that he was providing the "outline of a theory" and not a complete model, which

[28] For example, Solow's misplaced emphasis on the nature of the production function has not escaped notice. As already observed (see fn. 12 above), Hahn and Matthews (1964) point to different notions of instability. Similarly, Burmeister and Dobell (1970, p. 41) are unusual among textbook authors in noting the issue : "studying the Harrod position as if it were based essentially on a technological hypothesis about the production function, namely that it shows fixed proportions, misses the essential feature of Harrod's analysis.

[29] The authors are thankful to Roger E. Backhouse who brought this letter to their attention.

he saw as requiring "special postulates and assumptions in regard to lags and coefficients, which can only be accepted subject to statistical verification" and providing a full explanation of growth. Still, the interpretation of Harrod's work as a model of growth fits the emerging practices of the economics profession from the 1940s on. Economics became a modelling science, and modelling became the "natural way" to do economics (cf. Morgan 2012, ch. 1). Seen in the light of the emerging modelling practice, Harrod's dynamic theory was understood as a model of growth.

As E. Roy Weintraub (2005, p. 149) has noted, Harrod's ideas were initially thought to be quite wrong, then were thought to be quite difficult to interpret, and finally were quite ignored as a new generation of theorists rewrote economic dynamics from a mathematical perspective. From this perspective, Solow offered a "formalisation" of Harrod's theory. Formalisation is commonly regarded by the community of economic modellers as the replacement of verbal and mathematical theorising by models and typically regards such translation as possible while preserving the equivalence of concepts between the original and the translation (cf. Boumans 2005, ch. 1). Such a view of formalisation fundamentally underestimates the role of models. Reformulation of Harrod's "dynamic theory" in terms of a model – that is, translation between the practices of different interpretive communities – involved serious losses. Because they were not easily modelled through a closed system of differential or difference equations, Harrod's concerns with the stability of a dynamic path were not understood by the growth modellers. In identifying Harrod's instability with Solow's knife-edge, Harrod's major longer-term concern with the constitutional tendency of the economy to slip into recession when savings rates were relatively high was omitted from the research agenda: the formal growth models of the 1950s and 1960s had no natural mechanism for articulating the issue that takes up much of the latter half of Harrod's "Essay." Consequently, not only Harrod's dynamic theory, but also its ultimate purpose, providing an analysis of those factors that determine the trade cycle, got lost. His work came to be defined by Solow's "Contribution"– namely, a model of the long-run with a unique stable growth path and a knife-edge (Besomi 1999, p. 205; Kregel 1980, p. 118).[30]

[30] Years later, Solow recognised that Harrod had in mind two kinds of instability, but stuck to the notion that Harrod's work implied a constant capital coefficient: "One can mean two different things by the phrase 'Harrod's knife-edge.' One is the effective-demand instability argument that you elucidate. The other is an existence question: the idea, also to be found sometimes in Harrod, that equilibrium growth need not be possible if n, S, and the capital–output ratio are independently given constants. In 1956, I was mainly discussing this second meaning" (Solow to Nikaido, 29 March 1976; cf. Boianovsky and Hoover 2009, pp. 7–8).

4. Harrod's fate

Economic modelling became the dominant research practice of the mainstream of the economics profession. The mainstream generally accepted Solow's model, as well as his reading of Harrod, with the result that Harrod's analysis has been consigned to the dustbin of history. Textbooks present the history of their science as a linear story of progress, and rarely reflect on what might have been lost. Barro and Sala-i-Martin (2003, p. 17), for example, write of Harrod's supposed fixed-proportions growth model: "Although these constructions triggered a good deal of research at the time, very little of their analysis plays a role in today's thinking." This is an understatement.

Figure 5 shows the relative number of articles in the *JSTOR* archive that cite "Harrod" and "growth" relative to "Solow" and "growth," starting in 1939.[31] It is easy to spot the high tide of growth economics around 1970. Citations to both Harrod and Solow fall substantially after that. What is less striking, but true nonetheless, is that even as citations to both fall, citations to Solow *relative* to Harrod rise substantially. After 1985, growth-related citations to Solow enjoy a revival, probably related to the rise of endogenous growth models and to the incorporation of the Solow growth model into real-business-cycle models (see Hartley *et al.* 1998, ch. 1). By the end of the period, growth-related citations to Solow outnumber those to Harrod from ten to one. Harrod has gone down in the memory of the economics profession as builder of the first modern model of economic growth, although in the eyes of his readers the model with fixed proportions was at best a first step toward an adequate model of growth.

One of the ironies of the history of modern macroeconomics is that real-business-cycle models use Solow's long-run growth model, in which the economy never deviates from the warranted rate of growth, to explain short-run fluctuations. For Solow, this is precisely the opposite of what he sees as Harrod's error: using long-run tools for a short-run problem. Solow, however, is a short-run Keynesian, who agrees that savings and investment decisions need not be coordinated *ex ante*. To not address this

[31] Data are the number of articles in economics journals in the *JSTOR* archive in which ("Harrod" and "growth") or ("Solow" and "growth"), scaled by the total number of articles and indexed so that the maximum value (177 for Solow in 1970) = 100. Harrod data run 1939–2005; Solow data, 1952–2005. Data were collected from 79 journals in *JSTOR* on 8 May 2008.

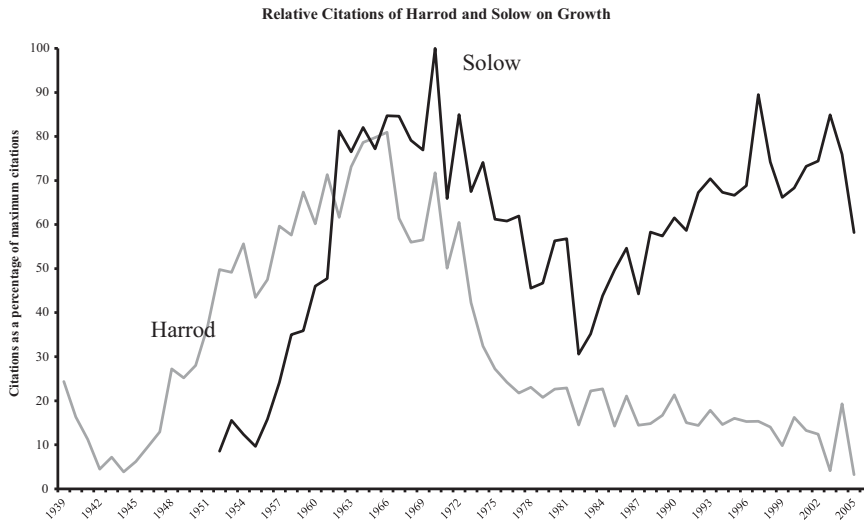

Figure 5 Relative citations of Harrod and Solow on growth

issue in his growth model was an analytical choice, not a claim that such problems do not arise in the world:[32]

> It is not my contention that these problems don't exist, nor that they are of no significance in the long run. My purpose was to examine what might be called the tightrope view of economic growth and to see where more flexible assumptions about production would lead a simple model. Under-employment and excess capacity or their opposites can still be attributed to any of the old causes of deficient or excess aggregate demand, but less readily to any deviation from a narrow "balance." [p. 91]

Solow served advanced notice that he was unsympathetic to what, after the 1970s, amounted to the collapsing of short-run into long-run analysis through appeals to rational expectations: "No credible theory of investment can be built on the assumption of perfect foresight and arbitrage over time" (p. 93).

[32] As Solow mentions a few years before the publication of his "Contribution": "[S]uppose that people are endowed with perfect foresight as to the future of prices, output, and interest rate. This is always a hard assumption to swallow, and it turns up often in economics. In the present context, it is perhaps not so bad. For one thing, we are not interested in deducing the implications of any particular method of forming expectations about the future; we might just as well assume the future to be known. Second, we are concerned with a long-run equilibrium situation, which is hardly compatible with consistently false expectations" (Solow 1953–1954, p. 76).

Our contention has been that, however valuable Solow's exploration of flexible assumptions has proved for the theory of long-run economic growth, Harrod was not the right target. His analysis was not of long-run economic growth, but of short-run or medium-run economic dynamics. He made no explicit assumptions about the flexibility, or lack thereof, of the production function, and gave substantial reasons to reject the idea that he thought that a fixed-proportions production function with constant parameters governed the economy. His instability was not Solow's knife-edge (i.e., a divergence of the warranted from the natural rate of growth), but rather forces that would drive the actual rate of growth away from the warranted rate. His main concern in the long run was not how the warranted rate of growth adapted to the natural rate of growth, but rather that a warranted rate in excess of the natural rate implied constitutional weakness in the macroeconomy, a tendency for recessions, to be more frequent and more enduring.

Solow's "Harrod–Domar case" represents a specific reading of Harrod's dynamics that was framed by Solow's research practices, the questions he asked, and hence by the communities of which he was a part. Interpretive communities are not isolated and impermeable; rather they overlap and interpenetrate – both intellectually and temporally. Solow and Harrod were both part of several (intra- and extrascientific) communities, they shared specific ways of thinking, and were separated by others. Contributing to the emerging practices of economic modelling, Solow integrated Harrod's work into his own framework. It was, perhaps, not inevitable that Harrod was misunderstood, that, in the translation of his ideas into the language of formal models, his key concerns were overlooked and his causal mechanisms were misrepresented. But it was natural. From the point of view of the post-war community of modellers, Solow's interpretation of Harrod's theory was a way of making it comprehensible – a requirement of the culture of understanding in which they practiced economics.

Even though it was an aim of the Harrod–Domar literature to figure out "what Harrod really meant," these attempts were not a historian's or anthropologist's way of investigating the *meaning* of Harrod's "Essay." Instead of trying to make Harrod's work *plausible*, to use Clifford Geertz's words, the authors of the Harrod–Domar models reformulated and transformed Harrod's equations in terms of a model. In these processes of reading and interpretation, essential parts of Harrod's dynamics were lost, and it is in this sense that we speak of Solow's "misreading" of Harrod's work.[33] We argued that it is possible to even speak of a "culture of

[33] Geertz (1973) defines culture as a "historically transmitted pattern of meanings embodied in symbols, a system of inherited conceptions expressed in symbolic

misunderstanding" owing to changing dominant practices in the economics profession. As member of different interpretive communities, Solow interpreted Harrod along the lines most congenial to the practices of the emerging community of economic modellers in the 1950s. Later, he was to acknowledge Harrod's concept of instability in his Nobel Memorial Prize lecture.[34] More recently, in an unpublished introduction to Harrod's 1946 lectures, Solow mentions, that

> Harrod is actually trying to address important issues that later growth theory has neglected or assumed away. So there is food for thought here for a 21st century reader, particularly in the aftermath of the financial crisis and recession of 2008–2010. [Solow 2009, p. 3][35]

Solow acknowledges that Harrod's instability concerns the divergence between the actual and warranted rates of growth. Moreover, he emphasises its value for today's economics, which – to his great concern – relies too strongly on equilibrium mechanisms:

> This part of Harrod's theory is worth serious thought... because it is at least taking up a problem that later growth theory has never solved nor even carefully considered. We know as a matter of fact that modern industrial economies are often off their equilibrium growth paths, sometimes in booms and probably more often in recessions. The source of these deviations usually lies in the behaviour of aggregate demand.... Recent growth theory, if it considers the question at all, seems to rest entirely on confidence–maybe excessive confidence–in price-guided markets to restore equilibrium growth. But that is generally a poor way to deal with episodes of excess or failing aggregate demand. [Solow 2009, pp. 5–6]

Cultures of understanding in economics are not the exception, but the rule. Like Solow, who presented Harrod as a growth modeller, textbooks have for decades now told and retold the same success story of growth theory. Mainstream textbooks are introductions to the dominant practices of economists, instruments for forging students' identities as economic modellers – and for steering them away from "unclear" and "unscientific" economics. Economists hardly ever question the potted stories about the

form" (p. 89). Geertz develops the concept of "thick description" as his method of doing ethnography. With regard to scientific cultures, the basic idea is to make scientific practices plausible in a manner that would allow outsiders to get a grip of the meaning of the participants' procedures and problems.

[34] "I may not have been as clear then as I am now about the distinction between the two notions of instability" (Solow 1988, p. 310). The authors thank one of the referees for pointing them to that statement.

[35] Assous (2013) documents Solow's efforts after 1956 to integrate Keynesian short-run and neoclassical long-run factors into a "medium-run macroeconomics."

development of their field. Their focus is on doing economics, not doing history, as William J. Baumol argued: "later writers are responsible only to be careful to assert that they have merely taken off from what Harrod's writing suggested to them, not that they are correctly reporting his ideas" (Baumol 2000, p. 1037). The "main achievement of [Harrod's] model lies in the ideas it inspired in those who did not fully understand it" (Baumol 2000, p. 1039). Baumol's view makes the actual development of a discipline excessively linear and, even while ostensibly promoting an instrumental or utilitarian view of the history of economics, in fact saps that history of many of its instrumental resources as well as of its rationale, which is to tell true stories (see Hoover 2004, Section I). The power of such stories nonetheless unfolds on a meta-level: they convey an image of economics as a linear accumulative development of formalisation. Against the implicit picture of the textbooks and their potted stories, the history of economics – just as the history of any other social and cultural enterprise – is complex and messy: the creation of stories is always a process of highlighting and neglecting, valuing and devaluing, and translating and passing over what seems to be incomprehensible. The gains of a coherent story are offset by the loss of content and a systematic misunderstanding of the past. One job – among others – of the historian is to reimagine earlier cultures of understanding, to recover what is lost, to translate what appeared to be untranslatable, and to tell coherent stories from now unfamiliar perspectives.

Acknowledgements

We thank Robert Solow for providing us with his unpublished manuscript (Solow 2009). The paper originates from each author's independent paper, both presented at the 10th annual Summer Institute for the Preservation of the History of Economics, University of Richmond, 19–22 June 2009. Verena Halsmayer is indebted to the participants of the workshop "History of 'Economics as Culture'" 2012 (University of Cergy-Pontoise), the Spring School "Geschichte und Soziologie der Sozial- und Kulturwissenschaften" 2011 (University of Graz) and the HISRECO 2012 (University of Porto), where she presented a related paper. Kevin Hoover thanks the many people who commented on various versions of his precursor paper ("Was Harrod Right?") at numerous conferences and departmental seminars, and particularly to Pedro Garcia Duarte, who was the discussant at the Second International Symposium on the History of Economic Thought, 5–6 August 2013 at the University of São Paulo. We would also like to thank the referees of this journal for their many helpful and thorough comments.

Funding

Verena Halsmayer's work was supported by the Austrian Science Fund [grant number W 1228-G13].

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Abstract

Modern growth theory derives mostly from Solow's "A Contribution to the Theory of Economic Growth" (1956). Solow's own interpretation locates its origins in his view that Harrod's growth model implied a tendency toward progressive collapse of the economy. He formulates his view in terms of Harrod's invoking a fixed-coefficients production function. We challenge Solow's reading of Harrod's "Essay in Dynamic Theory," arguing that Harrod's object in providing a "dynamic" theory had little to do with the problem of long-run growth as Solow understood it, but instead addressed medium-run fluctuations, the "inherent instability" of economies. Solow's interpretation of Harrod was grounded in a particular culture of understanding embedded in the practice of formal modelling that emerged in economics in the post-Second World War period. Solow's interpretation, which ultimately dominated the profession's view of Harrod, is a case study in the difficulties in communicating across distinct interpretive communities and of the potential for losing content and insights in the process. Harrod's objects – particularly, of trying to account for a tendency of the economy toward chronic recessions – were lost to the mainstream literature.

Keywords

Economic growth, Roy Harrod, Robert Solow, dynamics, dynamic instability, knife-edge, warranted rate of growth, natural rate of growth

JEL Codes: B22, O4, E12, E13, N1, B31