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Mythical ages and methodological strictures – Joan Robinson's contributions to the theory of economic growth

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Abstract

This paper considers some methodological aspects of Joan Robinson's contribution to post-Keynesian growth theory. Joan Robinson's criticisms of equilibrium analysis, of the conflation of logical and historical time and of the uses (and misuses) of mathematical formalisation are scathing. But while many of her points are well taken, parts of her argument appear questionable. As a result, her methodological critique of equilibrium economics may be misleading. Moreover, she failed to appreciate the potential gains from mathematical formalisation. The further development of a Robinsonian analysis of economic growth calls for a reconsideration of these aspects of her legacy.

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As a student I found Joan Robinson's work exhilarating. But the excitement was combined with an element of frustration. As hard as I tried, I often reached a point where it seemed impossible to understand *precisely* what she was saying. Her writing was stimulating, and it created a legitimate space for the questioning of textbook orthodoxies. Her vision also seemed right. But there appeared to be a lot of details, both in her criticism of neoclassical theory and her constructive alternative, that still needed to be worked out. Alternatively, of course, the details might have been worked out already but deemed so obvious that the reader would not need to see them spelled out.

Re-reading her work I still find it wonderfully stimulating. But some of the problems also seem to remain and, somewhat hesitantly, I have decided to use this occasion to confront these problems, rather than to focus on aspects of her work that I fully agree with. My reason for doing so is a fear that some of her influential methodological positions may stand in the way of the further development and extension of her substantive vision of capitalist accumulation. Thus, my criticisms in this paper are motivated precisely by the fact that I share–and have been greatly influenced by–the Robinsonian vision, with its emphasis on aggregate demand, social and distributional conflict, and path dependencies.

1 Equilibrium

1.1 The impossibility claim

The irrelevance of equilibrium economics and the confusion of time and space in neoclassical economics are recurrent themes in Joan Robinson's writing. In one of her best-known articles she put it as follows:

> The neoclassical economist thinks of a position of equilibrium as a position towards which an economy is tending to move as time goes by. But it is impossible for a system to *get into* a position of equilibrium, for the very nature of equilibrium is that the system is already in it, and has been in it for a certain length of time. (Robinson 1953-4; quoted from Robinson 1960, p.120, italics in original).

Indeed, this point is at the centre of her critique of neoclassical economics:

> The problem of the "measurement of capital" is a minor element in the criticism of neoclassical doctrines. The major point is that what they offer as an alternative to the post–Keynesian theory of accumulation is nothing

> but an error in methodology– a confusion between comparisons of imagined equilibrium positions and a process of accumulation going on through history. (Robinson 1974; quoted from Robinson 1978, p 136)

Elsewhere in the same paper she seems to locate the problem in the nature of simultaneous systems of equations. Thus, she claims that “it is not legitimate to introduce an event into a system of simultaneous equations” (Robinson 1978 p. 130).

But is it *logically impossible* to get into equilibrium, and is it always illegitimate to use systems of simultaneous equations to analyze the effects of unanticipated events? The clear answer to both questions, it seems to me, is no.

Even if equilibrium is understood in a restrictive sense as a state where all expectations are fulfilled and no agent wants to change her behavior, there is no *logical* impossibility of getting into equilibrium. An “equilibrium” outcome cannot be ruled out on grounds of logic, even if the system has at some point been “out of equilibrium” and movements in time are strictly one-way. It is straightforward, for instance, to describe a simple hypothetical economy with two types of agents (berry pickers and fishermen, say) where the agents initially have mistaken beliefs about the relative price but where an equilibrium with fulfilled expectations and market clearing may come to be established.

Joan Robinson would probably dismiss the example as an idle and irrelevant thought experiment. But “irrelevance” is not something that can be decided on purely logical grounds. In other words, her real claim may not be methodological. It does not concern logical impossibilities. The real, implicit claim may be that *in the world in which we live* and *given the range of questions that she was interested in*, it can be *misleading* to represent the economy as an equilibrium system, where equilibrium is understood as a state of market clearing and universally fulfilled expectations. But if her claim is qualified in this way, most people would probably agree. Agreement with this weak claim may lie behind Paul Samuelson’s joke “that the distance between me and Joan Robinson is less than the distance between Joan Robinson and me” (Turner, 1989, p. 128).

Clearly, important disagreements do exist between Robinson and mainstream economics. But these disagreements only become apparent in relation to the substantive, and much more difficult, questions about the adequacy and relevance of various theories and models with respect to carefully defined issues. Substantive questions of this kind cannot be settled by sweeping statements about logical impossibilities. All models involve simplifications. As Robinson has noted herself, we do not want road maps at a scale of 1:1, and it is neither controversial nor interesting to point out that conditions of strict equilibrium in the sense of fulfilled expectations will never be met in the real world. Thus, logical strictures about the impossibility of getting into equilibrium are largely beside the point–besides being, strictly speaking, wrong.

What about simultaneous systems? Surely it is correct that few real-world interactions are strictly simultaneous and that the introduction of an unanticipated event must lead to a *sequence* of causal changes rather than simultaneous interactions. This claim may be true but it is not very helpful. Even if the world is completely sequential, this real-world sequentiality does not imply that theories must also eschew simultaneity. Theories simplify, and as soon as there is simplification, the unconditional claim of sequentiality must go. Putting it differently, theories typically aggregate in time (events that do not occur at exactly the same moment are treated as if they did) as well as among goods and agents. Thus, there is no *a priori* reason for imposing strict sequentiality between the variables considered by the theory. In many cases a good theoretical approximation to the behavior of the real world may involve simultaneous equations. Here again, a purely methodological claim takes us nowhere. We need to spell out precisely where certain theories and models go wrong and why the simplifications relating to the treatment of time and dynamics in those theories may be particularly misleading. Better still, we need to formulate alternative theories that better capture the salient features of the real world. Methodological claims do little to settle these substantive issues.[1]

The mixture of methodological and ontological considerations shows up in Robinson's explicit discussion of equilibrium in chapter 6 of *The Accumulation of Capital*.[2] After suggesting that the metaphor of equilibrium is treacherous, she proposes a new set of concepts to highlight the remoteness of simple model-economies from actual economies. An economy, she suggests, may be characterized by *tranquility* "when it develops in a smooth regular manner without internal contradictions or external shocks, so that expectations based on past experience are very confidently held, and are in fact constantly fulfilled"; by *lucidity* if "everyone is fully aware of the situation in all markets, and understands the technical properties of all commodities, both their use in production and the satisfaction that they give in consumption"; and by *harmony* if "the rules of the game are fully understood and accepted by everyone and no one tries to alter his share in the proceeds of the economy". These concepts describe substantive characteristics of (hypothetical) economies, and Robinson concludes this short chapter by suggesting that it "is only necessary to describe these conditions to see how remote they are from the states in which actual economies dwell" (Robinson, 1969, p. 60). Well, yes, but these very general statements

[1] Joan Robinson's instrumental use of (seemingly) methodological arguments has been noted by Salanti (1996, p. 286).

[2] Kaldor (1972) is another prominent critique of equilibrium economics. But in Kaldor's case it is relatively clear that the term equilibrium economics is used as a short-hand for Walrasian general equilibrium theory. Joan Robinson's criticism often gives the appearance of having a much broader target.

still form an insufficient basis for a blanket rejection of "neoclassical economics", if for no other reason than that not all neoclassical theories seem to claim or require the fulfillment of these conditions of tranquility, lucidity and harmony. A sequence of Hicksian temporary equilibria, for instance, need not involve expectations that are constantly fulfilled. Her comments point in the right direction–that is, to the need for a careful assessment of the relevance of different theories with respect to features of the real world–but they still leave the task undone.

1.2 Equilibrium methodology[3]

Robinson's impossibility claim has been highly influential. Indeed, it may be the main inspiration behind the emphasis in much of the heterodox literature on the need to replace "equilibrium" economics with a "disequilibrium analysis" that is a truly dynamic and historically relevant. It is difficult to disagree, of course, with the desirability of dynamic and historically relevant theories. But the equilibrium / disequilibrium focus may have obscured the real issues.

What do we mean when we talk about equilibrium? There appears to be a range of different definitions, and the resulting ambiguity is a fundamental problem for methodological critiques that have "equilibrium" as their focal point. Sometimes constancy or absence of inherent tendencies to change is implied by the term, but there are plenty of growth equilibria in which quantities change. Thus, "constancy" will not work as a general definition. "Fulfilled expectations" will not work either: the fulfillment of expectations characterize some equilibria but clearly not all (e.g. Marshallian ultra-short run equilibrium or Lucas's theories of equilibrium business cycles). Market clearing, finally, is a non-starter if one wants to talk of Keynesian equilibrium models, including formal IS-LM models with excess supply in the labor market.

If there is a unifying element in the various equilibria, it appears to be this: each equilibrium (or set of equilibria) corresponds to the predictions of the underlying theory. Putting it differently, associated with any theory–whether phrased in the form of mathematical equations or described verbally– there exists a set of combinations of initial conditions and outcomes that are consistent with the theory. This set of theory-consistent outcomes constitute the equilibria of the theory. Some theories predict stationary outcomes, and the associated equilibria therefore will be characterized by constancy; some theories predict market clearing but others do not, and the associated equilibria accordingly exhibit market clearing or non-market clearing, as the case may be.

[3] The argument in this section draws on Skott (1984).

Using this definition of equilibrium as *theory-consistent outcomes*, it makes no sense to criticize “equilibrium theory”. To be against equilibrium–on this definition–is to be against theory. Equilibrium becomes a purely methodological term: we analyze the world using theories that, in order to be internally consistent, must have a non-empty set of equilibria. A theory without equilibria describes a system in which no outcomes can ever be consistent with the regularities posited by the theory.

Consider a couple of examples. An Arrow-Debreu-McKenzie theory posits a world in which all agents have well-defined preferences and in which their choice sets are limited only by budget constraints. The internal consistency of this theory requires the existence of a set of “equilibrium prices" that make the desired actions of all agents mutually consistent. In the absence of such equilibrium prices it is a matter of pure logic that some agents must face additional constraints, aside from the budget constraints considered by the theory. Analogously, simple Keynesian models posit a certain relation between consumption and income and state that investment is determined by animal spirits. This theory predicts an “equilibrium" level of income and employment but, of course, some workers typically will be involuntarily unemployed at this Keynesian equilibrium. In fact, if the predicted employment level violates a binding full employment constraint, there is no Keynesian equilibrium and the theory ceases to be internally consistent.

Robinson’s historical-time models also have equilibria, on this definition:

> The other type of argument [historical-time models, PS] specifies a particular set of values obtaining at a moment in time, which are not, in general, in equilibrium with each other, and shows how their interactions may be expected to play out. (Robinson 1962, p. 23)

The “particular set of values of the variables” may not be in equilibrium with respect to some (unspecified) restrictive theory, but the historical model obviously must permit these initial values, and the model–we are told–can be used to derive “how their interactions may be expected to play out”. In other words, the interactions specified by the historical model generates a set of model-consistent equilibria.

Methodological critiques of equilibrium economics, to summarize, are of limited value in the absence of a clear definition of the term equilibrium. Various, conflicting definitions coexist in the literature, and many discussions of equilibrium are characterized by a profound lack of clarity. Several, usually incomplete, definitions of equilibrium can be found in Robinson’s writings, but supporters of equilibrium theory also fail to provide clear definitions of the theory they defend. Blaug (1980, p. 101), for instance, argues that

> The idea of equilibrium is, surely, nothing more than the prediction that the real-world observable counterparts of the endogenous variables of economic models will remain constant so long as the real-world counterparts of the exogenous variables remain constant.

This definition contains no less than three different components: (i) equilibrium as theoretical prediction, (ii) the correspondence of theoretical variables to real world counterparts and the direct applicability of the theory and (iii) a constancy requirement. The second requirement effectively restricts the equilibrium concept to theories intended for direct empirical testing. Theories of, say, closed economies without public sector, which no one would want to apply directly to actual economies, apparently have no equilibria. The third requirement rules out, say, growth equilibria or inflationary equilibria, or, alternatively, raises questions of how much and what kind of constancy is required.

My very inclusive definition of equilibrium as theory-consistent outcomes is motivated by this absence of a narrow, generally accepted definition.[4] The broad notion of equilibrium makes a methodological critique of equilibrium economics meaningless. I view this implication of the broad definition as a virtue: adopting the broad definition may contribute toward a re-focusing of the debate on the underlying issues, that is, on the adequacy of particular theories or theoretical approaches with respect to some specified class of issues.

The notion of equilibrium has been discussed by many writers. A survey of this literature is beyond the scope of this paper. It should be noted, however, that some writers have taken a position that is similar to the one advocated in this paper. Estrup (1977, p. 241; emphasis in original), for instance, argues that a "model without an equilibrium is no model at all - there must be something wrong in its assumptions or in its deductions ...

[4] With relatively slight modifications, this broad notion of equilibrium can provide a general, descriptive definition of what economists mean when they talk of equilibrium:

> Associated with any non-vacuous and internally consistent theory is a *full* set of *weak* equilibria. This set is defined as the set of configurations of the conceptual variables of the theory which are feasible when the influence of all the postulated regularities are taken into account and the influence of all other conceivable disturbing factors is excluded. A subset of the full set of weak equilibria may be obtained by imposing additional regularity conditions and the term equilibrium is sometimes used in a stronger and more restrictive sense to describe those configurations which belong to the full set of weak equilibria and which also satisfy such additional conditions. A *restricted* set of equilibria in this *stronger* sense can be viewed alternatively as the full set of equilibria associated with a more restrictive, special theory. Skott (1984, p. 336-337)

The classical equilibrium analysis, therefore, is not an analysis *of* an equilibrium, but an analysis *by means* of an equilibrium". Katzner's (1998) analysis also has affinities with the argument in the present paper. Katzner identifies four methodological approaches: equilibrium analysis, disequilibrium analysis, non-equilibrium analysis and mutually interactive analysis. Although his terminology may be different than the one suggested in this paper, the substantive differences seem minor. Thus, "disequilibrium analysis may still be viewed as 'equilibrium analysis' with an altered system of equations" (p. 18), and non-equilibrium analysis (his preferred approach and the one associated with historical time) must be "coherent":

> A model is *coherent* if it is internally consistent in the sense that, at every t, its relations representing simultaneously occurring behavior have unique solutions (i.e. solution vectors) and, where those solutions represent inconsistent planned activity, they are resolved into unique realized variable values according to appropriate allocative and other rules of market operations. The realized values themselves are sometimes called *outcomes,* and a (necessarily kaleidic) sequence of such outcomes, one for each period t, is a realized kaleidic time-path generated by the model. Thus, a model is coherent whenever it produces unique outcomes at each t, and its coherence is an attribute of the structure of a model rather than of its outcomes. (p. 332; emphasis in original)

The kaleidic time-path of Katzner's non-equilibrium model represents the equilibrium time-path associated with the theory, using my terminology. Katzner imposes a uniqueness requirement as part of his definition of coherence. I do not see anything incoherent or internally inconsistent in the non-uniqueness of equilibrium and therefore included no similar condition in my definition of theory-consistent equilibrium. But this, as far as I can see, is the only substantive difference.

The argument in this paper, finally, shares some common ground with Cohen's (1993) analysis of Robinson's methodology. Following Bliss (1975), Cohen suggests that equilibrium be viewed as simply an analytical stepping stone. Equilibrium analysis, in this sense, he argues, represents one of the stages in Robinson's historical approach. The distinctive characteristic of this approach, according to Cohen (p. 233), is the recognition that "complications have to be reinserted before conclusions drawn from the model can be confronted with evidence from reality" (Robinson 1969, p. 64). The problems associated with reinserting the complications are discussed in section 3 below.

2 Stability analysis and historical time

Sometimes theories are nested. Marshallian short-run equilibrium is a special case of ultra-short run equilibrium; full-employment equilibrium is a special case of Keynesian equilibrium; multisectoral models may include special cases with balanced growth trajectories. The nesting of theories in this way makes it possible to examine the *stability* of the nested theory, that is, one can examine whether the more general theory generates an outcome that can be approximated by–or perhaps converges to–the outcome predicted by the special case. This stability analysis is conditional on the more general theory, just as the statistical test of a special hypothesis H_1 against a more general hypothesis H_0 is conditional on H_0. Hence the stability analysis (the significance test) is of little value if one has no faith in the more general theory (the more general hypothesis).

Robinson's frequent criticisms of traditional stability analysis make sense within this terminological framework. She has been scathing in her comments on the "stability analysis' to be found in general equilibrium theory, and for good reason. *Tatonnement* processes do *not* qualify as serious stability analysis for the simple reason that no outcomes are specified during the *tatonnement* process. This is a fundamental methodological problem. A *tatonnement* process quite simply does *not* constitute a more general theory of decentralized market economies: the process does not allow for the possibility of outcomes without market clearing. In case of instability we are left without any predictions about the outcome. The theory allows for no trade, no production and no consumption to take place until the process has been brought to a successful end and an equilibrium has been established.

The *tatonnement* process, in Robinson's terminology, takes place in "logical time". By contrast, a proper stability analysis must take place within a framework–a broader theory–which allows outcomes that are "out of equilibrium" from the perspective of the theory whose equilibria are being tested for stability.[5] Or as she put it in *History vs. Equilibrium*:

> If we construct the equations for a single self-reproducing system and then confront it with an unforeseen change, an event taking place at a particular date, we cannot say anything at all before we have introduced a whole fresh system specifying how the economy behaves in short-period disequilibrium. (quoted from Robinson 1978, p. 128)

[5] Since only nested theories can be examined for stability, it follows that there is no way to decide the stability properties of equilibria associated with the most general theory that has been specified; by definition, this general theory is non-nested.

This analysis gives a different perspective on the methodological problem in neoclassical theories of market clearing. The problem is not the "impossibility of getting into equilibrium." The problem arises from a failure to specify a coherent, more general theory which allows for non-clearing markets and within which the stability issue can be raised. The *methodological* error is the pretence that *tatonnement* processes in logical time may provide the required theory.

The unsatisfactory nature of *tatonnement* processes does not imply that stability analysis is impossible. In fact, the key message of Keynesian economics concerns stability: in the *General Theory*, Keynes developed (the outlines of) a theory which allows for outcomes with unemployment as well as for classical full employment. According to this theory the full employment position fails to be stable. The groundwork for this radical message is done in chapters 1-18, which effectively present an analysis of a fixed-money-wage economy; the (somewhat sketchy) stability argument follows in chapter 19.

Robinson (along with other post Keynesians and old Keynesians) has echoed Keynes's position and presented similar instability arguments (e.g. Robinson 1969 p.79; 1962 p.73-4; 1971 p.91). In this light, moreover, the criticism of neoclassical analysis in Robinson (1974) is straightforward: without a demonstration that, contrary to Keynesian claims, market adjustments will in fact generate full employment, neoclassical analysis based on assumptions of market clearing is no "alternative to post-Keynesian theory of accumulation ... but an error in methodology—a confusion between comparisons of imagined equilibrium positions and a process going on through history".

As Robinson has also pointed out, Keynes had to "fudge" the stability argument in various ways (e.g. Robinson 1979, p. 113). He treated expectations as well-defined, certainty-equivalent point expectations and, rather than dump the economy at some arbitrary initial position, he assumed that a fix-wage short-run equilibrium had been attained. Thus, despite Robinson's protestations, it is hard to disagree with Hicks's comment that Keynes's analysis was only partly in historical time.[6]

But perhaps being partly in historical time is all we can hope for. All analysis is theory-driven. Explicitly or implicitly it stipulates and relies on various linkages and

[6] See Robinson (1977, p. 1325). Keynes himself commented that in the *General Theory* he was "more classical than the Swedes, for I am still discussing the conditions of short-period equilibrium" (Keynes 1973, p. 183). At a methodological level, moreover, Keynes expressed a belief in the usefulness of short-run equilibrium models:

> The main point is to distinguish the forces determining the position of equilibirum from the technique of trial and error by means of which the entrepreneur discovers where the position is. (Keynes, 1973, p. 182)

dynamic effects between variables, and non-vacuous theories must impose restrictions on the set of possible initial patterns. If a theory states that workers' consumption is equal to their wage income, for instance, an analysis based on that theory cannot consider the implications of initial patterns in which consumption differs from wage income. The real question therefore is not whether an analysis is fully in historical time, it never is, but whether it is "sufficiently historical".

How does one decide whether a theory is sufficiently in historical time?[7] If, following Robinson (1962, p. 23), we define a historical-time model as one which specifies "a particular set of values obtaining at a moment in time ... and then shows how their interaction may be expected to play out", the criteria seem straightforward: A theory can be insufficiently historical if the set of permissible initial values is too restrictive, *given the issues addressed by the analysis.* Thus, the ability of markets to generate full employment cannot be analyzed in a satisfactory way by a theory which presumes universal market clearing and which therefore does not allow initial conditions that involve unemployment. Keynes's assumption of "short-run equilibrium" with fulfilled short-run expectations also raises questions. Expectations sometimes go wrong–except in a mythical golden age–and mistaken expectations must involve a divergence between outcomes and expectations in some short period. My own analysis in Skott (1989) therefore built up the analysis as a sequence of "ultra-short run equilibria" in which expectations need no be fulfilled.

As pointed out in the contributions to this volume by Harris and Dutt, significant progress has been made since *The Accumulation of Capital.* Robinson's analysis used golden-age models as stepping stones for an examination of internal contradictions in the growth process and for rather informal considerations of how the trajectory might be altered by various events. Subsequent work by post-Keynesians and writers in the structuralist tradition has pushed further in the direction of historical-time modeling of capitalist accumulation.[8] Less well-known among post-Keynesians is the research project carried out by Carl Chiarella, Peter Flaschel and various coauthors (see e.g. Chiarella and Flaschel (2000)). They insist on building models that are integrated (in the sense of including the major markets in a capitalist economy: labor, goods and financial), that are consistent (in the sense that all accounting restrictions are seen to be satisfied at all times) and that trace the dynamic implications of arbitrary non-market clearing initial positions.

[7] The common metaphors used to illustrate the irreversibility of time are largely irrelevant in this respect, it seems to me. If we want to understand precisely what happens when an egg is cracked, it is a strength rather than a weakness that video recordings of the event can be re-run at will. It is hard to see, moreover, how the possibility of running the recording backwards could count as a strong argument against the use of recordings.

[8] The structuralist modeling strategy is discussed by Gibson (2003).

Robinson might have taken issue with some of the substantive assumptions built into the models they have analyzed so far. But the methodological approach should have met her approval–except, that is, for their heavy use of mathematics, an issue that I now want to turn to.

3 Mathematical models

3.1 Powerless or redundant?

Robinson seems skeptical about the contribution of formal reasoning when it comes to stability analysis or, more generally, to the analysis of situations where expectations fail to be fulfilled. In Robinson (1962), for instance, she concludes a verbal analysis of the effects of changes in wages and prices with the following comment:

> These various effects of changes in money-wage rates play across the various real movements discussed above. This makes the operation of the model complicated and confusing. That, however, is a merit, not a defect, since it corresponds to reality. (Robinson, 1962, p. 74)

This is a curious argument. The effects of changes in money wages clearly can be complex but that is no excuse for a vague analysis ("plays across") that makes the operation of the model "confusing". Why not try to make the analysis of the effects of changes in money wages as precise as possible?

Mathematical formalizations–including Tobin (1975), Dutt (1986-87), Chiarella and Flaschel (2000) and Flaschel and Franke (2000)–have clarified many of the dynamic interactions associated with changes in money wages. Formalization has made it possible to specify both the conditions for Keynesian instability and the types of instability that may arise much more precisely than would have been possible using verbal reasoning. None of the models, to be sure, capture the full complexity of the real world. But why not accept the formal models as helpful tools? If stylized models depicting mythical ages can be helpful, why not expect that by using simplified dynamic models we can "hope to be able to gain insight into the behavior of the actual, complicated economy" (Robinson, 1969, p.64)?

Robinson's reservations concerning the use of mathematics may be related to her general critique of "equilibrium economics". Even if directed primarily at specific theories of perfect foresight and universal market clearing, a seemingly methodological criticism of "equilibrium economics" has a rather diffuse target, and Robinson and some of the post-Keynesian tradition that she inspired may have been deceived by their own rhetoric. Having (implicitly) identified mathematical modeling with equilibrium analysis and equilibrium analysis with a particular kind of theory, neoclassical theories of general equilibrium, they may have concluded that any kind of formal modelling should be avoided. Arguing along similar lines, Katzner (2001, p. 55) suggests that although formalization may appear to be the target of much criticism, it is an unhappiness with the kinds of questions being analyzed and the substantive assumption content of the analysis that constitutes the real source of dissatisfaction with the current state of economics.

In any case, a number of quotations can be found in which Joan Robinson suggests that, when not directly harmful, mathematical formalization may be largely redundant. Thus, Robinson (1979, p. 117) argues that

> [m]athematical logic is a powerful tool of thought, but its application in economic theory generally seems to consist merely of putting circular arguments into algebra. (Robinson 1979, p. 117)

Alternatively, formal analysis may be powerless:

> disturbing events occur on disequilibrium paths. The resulting turbulence is beyond the skill of model builders to analyze. Historical analysis can be made only in very general terms. (Robinson 1962, p. 27)

It may be true, as argued by Harris (this volume, p.) that Joan Robinson was not "averse to the use of mathematical modeling as such, if designed to eliminate errors in "thinking"". Indeed, one can find quotations along these lines in her work. The overall impression from her writings, however, is one of deep skepticism with respect to the usefulness of mathematical modeling, and indeed this skepticism seems widespread within the post-Keynesian tradition.[9] The following extended quote from the survey of Joan Robinson's economics by Gram and Walsh (1983, p. 534, n.14) expresses the reluctance to formalize the analysis:

[9] Formalizations of post-Keynesian theories do exist, of course. Indeed, Robinson's analysis of the conditions for a 'golden age' (or other mythical ages) can be and has been presented in simple mathematical terms; early formalizations include Worswick (1959) and Findlay (1963). Robinson appears to have accepted Worswick's formalisation; her main objection concerns the incompleteness arising from Worswick's treatment of the real wage as exogenously fixed (Robinson 1959, p. 142). Robinson (1975) also cites Harris' (1973) formalization approvingly.

> It is easy enough, at this stage in the development of dynamic models of capital accumulation, to write down the equations characterizing a steady-state growth path ... But one hesitates to cast Robinson's golden age models in this mold, for there is more in her verbal descriptions than such formal structures adequately portray (which is not to say that we eschew or denigrate formal analysis). Perhaps the best one can do is to say that, even in a golden age, *decisions* in Robinson's argument are being taken (as one of our referees put it) in a state of Marshallian confusion (owing to Keynesian uncertainty). It just happens to be the case that events are unfolding in such a way as to be consistent with what are, in any case, very sketchy outlines of steady-state growth paths offered by formal growth theory. Marshallian firms, as it were, are leaving behind steady-state growth tracks, but those tracks do not in any very adequate way tell us why they are behaving in a manner consistent with these requirements of a golden age, and they are certainly not to be enshrined as a complete description of what is going on.

It is true that simple mathematical models do not provide a complete description of what is going on, but neither does simple verbal reasoning. Neither type of theory, in any case, is meant to give a complete description, and theories, whether presented verbally or mathematically, can and should be supplemented by historical and empirical studies. The advantage of formalization is that in many cases it will enable one to present and analyze more complex and more satisfactory theories and to do so with greater clarity and transparency. The use of mathematics therefore becomes particularly helpful if one wants to go beyond the golden age. And although an important stepping stone, the analysis of golden ages has limited value:

> if steady growth is proposed as a hypothesis, it sinks at the first step but ... it is useful in what Kornai describes as intellectual experiments, which are necessary to sort out the questions involved in analyzing complicated processes. ... I intended my golden age (which has often been mistaken for a hypothesis) to be used in this way. (Robinson 1977, p. 1330)

As she put it in *The Accumulation of Capital*,

> It is important to remember, however, what complications have to be reinserted before conclusions drawn from the model can be confronted with evidence from reality. (Robinson 1969, p. 64)

3.2 Reinserting the complications

How does one go about "reinserting the complications"? Both *The Accumulation of Capital* and *Essays in the Theory of Economic Growth* present complex verbal arguments. Golden-age relations are discussed alongside modifying influences, and matters are not helped by the fact that often the modifying influences and the interactions are only implicitly or partially specified. One example, referred to above, is the Keynesian analysis of the effects of changes in money wages. Here I shall briefly consider an example from Robinson's (1962, pp. 22-87) analysis of accumulation.

The choice of technique may have figured prominently in much of Robinson's writings but for present purposes nothing is lost by using a one-sector framework and assuming a fixed-coefficients technology.[10] Given the one-sector assumption, we have a well-defined accumulation function and Robinson's verbal argument (p. 47) implies that it takes the form

$$g = \frac{I}{K} = f(r^e) \tag{1}$$

where I is investment, K is the capital stock, g denotes the rate of accumulation,r^e is the expected future rate of profit on new investment, and $f' > 0$ Saving, S, is proportional to total profits and, normalizing by the capital stock, we have

$$\frac{S}{K} = sr \tag{2}$$

where r is the current rate of profit and s the saving rate out of profits.

The current rate of profit is determined by a market-clearing condition for the product market. In a closed economy without public sector, this condition requires the equality between investment and saving, that is,

$$sr = g \tag{3}$$

In order to close the model, the specification of investment and saving is supplemented by assumptions concerning the formation of expectations. Most of the analysis focuses on the case of static profit expectations,

$$r^e = r$$

[10] Aside from a few remarks on the effects of different types of restraints in a "restrained golden age", the choice of technique plays no role in the essay on "A model of Capital Accumulation" in Robinson (1962). In the preface to *The Accumulation of Capital*, moreover, Robinson's comments on the intricacy of the choice-of-technique problem, noting that "the difficulty of the analysis is out of proportion to its importance" (Robinson, 1969, p. ix).

Assuming that the investment function f is strictly concave, we may now get the well-known "banana diagram" with two equilibrium solutions: an unstable solution with low growth and low profit rates and a stable solution with high growth and high profit rates.

Two points should be noted. The stability analysis (p. 48-49 and p. 63-64) would seem to fall foul of Robinson's own strictures: she has not specified a more general model which allows one to consider the trajectories following an initial position in which there is a discrepancy between desired investment and saving. Thus, it is hard to see how one can be sure that when lags are introduced "near-enough stability is realized" (p. 64). The notion of static expectations with respect to the profit rate, secondly, seems peculiar. Under conditions of imperfect competition, firms' expected profit rate, r^e, cannot be independent of their production and investment decisions and thus, implicitly, we have an assumption of price-taking behavior (perfect competition). As a corollary, the utilization of capital should be at the desired rate. This latter assumption is stated explicitly on p. 46: in the analysis of short-period equilibrium "let us suppose that competition (in the short-period sense) is sufficiently keen to keep prices at the level at which normal capacity output can be sold" (Robinson, 1962, p. 46). Algebraically,

$$u = u^* \tag{4}$$

where u is the rate of utilization and u^* is normal (or desired) utilization. Using this notation, the rate of profit can be written $r = \pi uk = \pi u^* k$ where k is the maximum output-capital ratio as determined by the fixed-coefficient production function.

To address the first point, explicit lags may be introduced. Thus, to accommodate lags in a simple continuous-time setting, the investment function in equation 1 may be replaced by

$$g^* = f(r) \tag{5}$$

$$\dot{g} = \lambda(g^* - g) \tag{6}$$

where $\lambda > 0$ is an arbitrary adjustment parameter and the dot indicates a time derivative; g^* is the desired rate of accumulation and g is the actual rate of accumulation. Equation 6 expresses the gradual adjustment of actual accumulation toward the desired rate and may be re-expressed using equations 5 and 3 as:

$$\dot{g} = \lambda[f(\frac{g}{s}) - g]$$

Thus, a locally asymptotically stable equilibrium at g^* satisfies the condition

$$f'(\frac{g^*}{s}) < s$$

This is the "Robinsonian stability condition", using Marglin and Bhaduri's (1990) terminology: investment must be less sensitive than saving to variations in the profit share.[11]

Turning to the second point, the assumptions of price-taking and utilization at the desired rate may be logically consistent but they are unattractive, both theoretically and empirically. Robinson acknowledges as much. She notes that "in reality, of course, markets for manufacturers are highly imperfect, prices are fairly sticky and changes in investment are generally accompanied by changes in output and employment"(Robinson, 1962, p. 65). But no real attempt is made to analyze the implications of these features. We are left with the assertion that despite these and other complications "all the same, in a broad way, our analysis of long-run growth remains cogent. ... the characteristic features of restrained and limping golden ages or of platinum ages can be discerned under the restless surface of unstable growth" (Robinson, 1962, p. 69).

One way to relax the desired-utilization assumption in equation 4 is to introduce a finite adjustment speed for prices and profit margins. Thus, let the profit share, π, adjust to the difference between actual and desired capacity utilization

$$\dot{\pi} = \nu(u - u^*) \tag{7}$$

where $\nu > 0$ is again an arbitrary adjustment coefficient. With slow price adjustment it is now instantaneous movements in the utilization rate u (that is, in output normalized by the predetermined level of capacity output) that ensure the equalization of saving and investment: both the rate of accumulation, g, and the profit share, π, are predetermined at any given moment, and from the saving-investment balance in equation 3 and the definition of the profit rate, $r = \pi uk$, we can write

$$u = \frac{g}{s\pi k}$$

Thus, we can define a new function ϕ and express capacity utilization as

$$u = \phi(g, \pi) \tag{8}$$

where $\phi_g = u/g > 0$ and $\phi_\pi = -sku^2/g < 0$.

The assumption of slow price adjustment may be combined with slow adjustment of investment. Actual capacity utilization, however, should be allowed to influence investment

[11] The inequality is cast in terms of the sensitivites to changes in the profit *rate,* but in this model the output-capital ratio is assumed constant, and movements in the profit rate are therefore proportional to movements in the profit share. Multiplying both sides of the inequality by the value of the ouput-capital ratio gives the condition in terms of sensitivities to movements in the profit share.

and, as a simple extension of 5, let

$$g^* = f(\pi, u) \tag{9}$$

with partial derivatives $f_\pi > 0$, and $f_u > 0$.

Equations 6 and 7, with equations 8 and 9, constitute a two-dimensional dynamic system in the growth rate of the capital stock and the profit share

$$\dot{g} = \lambda\{f[\pi, \phi(g, \pi)] - g\}$$

$$\dot{\pi} = \nu[\phi(g, \pi) - u^*]$$

The Jacobian is given by

$$J(g, \pi) = \begin{bmatrix} \lambda(f_u\phi_g - 1) & \lambda(f_\pi + f_u\phi_\pi) \\ \nu\phi_g & \nu\phi_\pi \end{bmatrix}$$

and, evaluated at the stationary point, we have

$$\det(J) = -\lambda\nu(\phi_\pi + f_\pi\phi_g) = \lambda\nu\frac{u^*}{g^*}(sku^* - f_\pi) \tag{10}$$

$$tr(J) = \left[\lambda\left(f_u\frac{u^*}{g^*} - 1\right)\right] - \left[\nu\frac{u^*}{g^*}sku^*\right] \tag{11}$$

The Robinsonian stability condition–desired investment being less sensitive than saving to changes in the profit share–ensures that $\det(J)$ is positive. Saddlepoint instability can be ruled out, and local asymptotic stability depends on the sign of the trace. In the expression for the trace, the first term in square brackets may be either positive or negative. The second term, however, is negative and it follows that local stability obtains if the adjustment speed for prices is fast (relative to the adjustment speed of investment). Moreover, if the first term in square brackets is positive, complex eigenvalues and damped fluctuations must characterize the system for a range of $\nu-$values above the critical value that ensures local stability. Thus, Robinson's conclusion may be vindicated: a chance disturbance may generate fluctuations in the growth rate but "the wobbles around the desired rate grow less and near-enough stability is realized unless a fresh disturbance intervenes" (Robinson, 1962, p. 64). The conclusion, however, is conditional on certain parameter values.[12]

These simple exercises in stability analysis could be extended and generalized in various directions.[13] Of course, no generalization will give us the final word on the dynamics of capitalist economies. But that does not invalidate formal analysis of (selected) dynamic interactions outside steady growth. The models bring clarity and enhance our understanding. The simple example above demonstrates the logical coherence of Robinson's conjecture that movements in the profit share *may* keep actual utilization close to desired utilization. But it also demonstrates the possibility of other outcomes and, more importantly, help to identify the conditions that give rise to the different outcomes.

3.3 Dangers of formalization

Before closing this section, it should be noted that an emphasis on formal modelling techniques may involve significant dangers. Worswick (1959, p. 121) prefaced his formalization of *The Accumulation of Capital* with the warning that

> It is sometimes thought that the objection to the use of mathematics in economics is that it is too hard. The more serious objection, however, is that it is much too easy.

The ease of mathematical manipulation may lead to a neglect of those issues that are hard to formalize or, equally dangerous, to a distortion in the way we look at those problems that are being analyzed. Formal mathematical analysis may also lead to a focus on mathematical elegance and a refusal to deal with the messiness of real history.[14] In

[12] If u deviates from u^*, the assumption of perfect competition needs to be reconsidered. This in turn raises questions concerning the determination of expected profitability since, under imperfect competition, a firm's future profit rates cannot be independent of its pricing and investment decisions. The model, finally, treats output adjustments as infinitely fast while prices adjust slowly. The existence of production lags make this ranking of adjustment speeds questionable.

To avoid these and other problems, Skott (1989) reverses the adjustment speeds for output and prices in a model which includes labour and financial markets as well as an explicit analysis of firms' interrelated output and investment decisions. This model generates limit cycles around a locally unstable steady growth path.

[13] The model with slow adjustment in prices and investment is closely related to Steindl's (1952) argument. Flaschel and Skott (2004) analyse various extensions of the model, taking into account the interaction of the product market with labour and financial markets.

[14] One clearly should not "praise the logical elegance of a system which becomes self-contradictory when it is applies to the questions it was designed to answer" (Robinson 1978 p. 127-128). The prime example, of

some cases, undoubtedly, a lack of mathematical tools can impose a focus on less complex, but perhaps more relevant interactions. Indeed, casual observation suggests that many important insights can be expressed in relatively simple terms and that there may be strongly diminishing returns to the application of ever more sophisticated techniques.

One may also note the paradoxical usage of mathematics in much of mainstream economics. Over the last 30 years, in particular, macroeconomists have struggled to solve sophisticated problems of intertemporal optimization. These optimization problems represent grossly simplified and stylized versions of real-world optimization problems, the implicit presumption being that agents in the real world have already solved (or act as if they had solved) these more complex problems. All interactions between agents, on the other hand, are brushed aside by representative-agent assumptions. This is precisely how not to use mathematics. It may sometimes be relevant to include optimization in economic models as a stylized representation of goal-oriented behavior, but mathematical models are useful primarily because they allow a clear analysis of complex interactions between agents, each of whom may follow relatively simple (but possibly changing) behavioral rules.

These dangers of a technical fixation notwithstanding, a refusal to formalize arguments that *can* be formalized carries its own dangers. And arguably these dangers are even more serious. Without formalization the analysis easily gets bogged down in verbal reasoning that leaves the logical implications unclear. Moreover, the tortuous process of sorting out the logic of various systems using purely verbal reasoning may leave little or no resources for the task of piecing together and evaluating the relevance of the different theories.

4 Conclusion

Robinson's relentless critique of capitalist inequities and her "hatred of injustice" (Harcourt, 1992) cannot fail to impress and inspire her readers. She clearly was right also to stress both the apologetic role of much economic theory and the pervasive influence of ideology on the way economic questions are being addressed by the discipline, a point which remains as pertinent as in her time, despite significant changes in mainstream economics over the last twenty years. The main elements in her substantive contribution to the analysis of accumulation, finally, have stood the test of time: the evolution of capitalist economies cannot be understood, in the short or the long run, without an analysis

course, is the application of Walrasian general equilibrium systems that presume universal market clearing to an examination of the ability of a decentralized market system to generate full employment.

of Keynesian issues of effective demand; social and distributional conflict, moreover, is central to the dynamics of the system, and path dependencies abound.

It is when it comes to her methodological positions that I have reservations. She may be right that much growth theory involves “a confusion between comparisons of imagined equilibrium positions and a process of accumulation going on through history”. But, as argued above, some of her detailed methodological arguments seem less than convincing. *Tatonnement*-based stability analysis is a mockery but perhaps this point could have been made differently and more clearly. Path dependence, furthermore, does not imply the absence of theoretical explanation. Agreement with her view that “in most economic reactions the path the market follows, while it is adapting itself to change, has a long-persisting effect upon the position that it reaches” (Robinson, 1969, p.58) is fully compatible with equilibrium analysis, as defined above. Path dependence will be reflected in the theory and the character of the associated equilibria.

The biggest problem, however, concerns Robinson’s legacy when it comes to formal modelling. She surely was right that “it is high time to abandon the mainstream and take to the turbulent waters of truly dynamic analysis” (Robinson 1978, p. 125). But without powerful analytical tools it is unlikely that the expedition will get very far. We should have no illusions that we can develop a “complete theory”. A complete theory "would be only another box of tricks. What we need is a different habit of mind, to eschew fudging, to respect facts and to admit ignorance of what we do not know." (Robinson 1979, p. 109, 119) But there is no conflict between the use of formal models with well-defined equilibria and the development of habits of mind that eschew fudging, respect facts and admit ignorance of what we do not know. Critical economists cannot afford to leave the powerful tools of mathematical analysis to mainstream economists.

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