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**Quaderni di ricerca del
Dipartimento di Scienze Economiche
“Hyman P. Minsky”**

Anno 2002 n.1

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Model-robustness in ‘old’ and ‘new’ growth theory*

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Abstract: In economics, models, rather than theories, seem to be the fundamental units of appraisal and practitioners seem to hold in high esteem the criterion of ‘robustness’. In this paper we shall try to explicate the multifarious notion of robustness, and articulate it on three different dimensions. In order to show their relevance to concrete economic practice we shall apply these notions of robustness to a particular case: the “old” vs. “new” growth theory. Special attention will be paid to the robustness of the implied causal mechanisms, due to its substantial role concerning the possibility of deriving sound policy prescriptions from models.

JEL classification: B 41, O 40

Keywords: growth, modelling, robustness

* A previous draft of this paper has been presented at the conference “Old and New Growth Theories: An Assessment”, Pisa, October 5-7, 2001. We would like to thank, with the usual caveat about responsibility, Giacomo Costa and Stephen Turnovsky for the useful comments made in Pisa. The final version has benefited from comments and suggestions by Roger Backhouse and an anonymous referee. Grants from, respectively, the Cognitive Science Laboratory of the University of Trento and the Department of Economics of the University of Bergamo are gratefully acknowledged. The present version is forthcoming as a chapter in Neri Salvadori (ed.), *Old and New Growth Theories: An Assessment*. Cheltenham: Edward Elgar.

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1. Introduction

Scientific progress is often supposed to consist of the discovery of ‘better and better’ theories within a particular field of research. How to characterise a ‘better’ theory is nonetheless far from obvious. Plausible candidates may be an increased power in the prediction, explanation, and control of real-world phenomena. In economics, where *models*, rather than theories, are the fundamental units of appraisal, practitioners seem to hold in high esteem the criterion of ‘robustness’ (as it can be detected in the literature or during seminars and workshops). A ‘model economy’, or ‘model’ for short, is a simple object that stands for (in some way to be specified) a real-world economic system. Models can be abstract or (rarely) concrete objects, which can be studied and manipulated on paper, by means of thought experiments, or concretely, with the ultimate goal of discovering interesting facts about the real-world system they are representative of¹. A ‘better’ model, then, is a ‘more robust’ one – but robust *to what?* In this paper we would like to do two things: first, we shall try to explicate the notion of robustness, and articulate it on three different dimensions. Secondly, we shall apply these notions of robustness to a historical case, in order to show their relevance to concrete economic practice. ‘Old’ and ‘new’ growth theory (OGT and NGT respectively, from now on) stimulate interesting methodological reflections, as we shall see. Special attention will be paid to the robustness of the implied causal mechanisms, due to its substantial role concerning the possibility of deriving sound policy prescriptions from models.

2. Three different notions of robustness

Robustness is a comprehensive term under which various different concepts are subsumed. However, since different kinds of progress are associated with different kinds of robustness, it is important to distinguish between them properly. In Guala and Salanti (2001), we identify three main different meanings of the term: (1) robustness to changes in the model’s ‘idealisation’s; (2) robustness to changes in the ‘background’ conditions (usually, but somewhat improperly, referred to as *ceteris paribus* clauses); (3) robustness to changes in the implied causal mechanism(s)².

¹ Cf. Morgan and Morrison (eds. 1998) for a general account of modeling along these lines.

² This categorisation owes substantially to the work of Cartwright (1989) and, more recently, Woodward (2000) on causation. Their approach, in turn, borrows from the notion of ‘structural’ relation introduced by early econometricians like Haavelmo and Frisch.

The first two notions emerge from considering that models provide simplified representations of real entities and systems in roughly two ways, by distortion and, often at the same time, by omission of some aspects of reality. The terms '*idealisation*' and '*abstraction*' are meant to refer to these two distinct facets of model-building activity respectively³.

Every model incorporates (possibly highly) idealised descriptions of some real-world feature (entity, system, or property). These descriptions capture just one of the several possible forms that each single feature can take in the real world. A typical idealisation is involved for example when a result⁴ (e.g. a steady state equilibrium with its associated properties) is proved to hold for a one-sector, or a two-sector (or even a two-class) model-economy, although we in fact know that the number of 'sectors' or 'classes' in the real world almost certainly differs from what is assumed in that model. Many other examples of idealisations within ('old' or 'new') growth theory could be easily provided. Think of, for instance: Harrod's (or Hicks's, for that matter) definition of neutral technical progress within OGT; the assumption of constant returns to scale in capital and labour; the notion of optimal consumption for an infinitely-lived (representative) household; two-period overlapping generations models; and so on.

Increasing robustness to changes in idealisations is taken as an indication of progressiveness on the basis of the presumption that a given result has been rigorously demonstrated with respect to a very specific model-economy only for reasons of simplicity, clarity, or mathematical tractability, but the validity of the result itself should not depend on any of the specific idealisations used in the proof. This independence may be self-evident to the specialist or hinted at by the author. In any case, what characterizes idealizations is that they are explicitly (and rigorously) enunciated and very often they are also highlighted as such⁵.

³ This terminology is by no means standard in philosophy of science or methodology of economics. The lack of a shared convention can be appreciated for example by looking at the essays in Hamminga and De Marchi (1994). Here we adopt Cartwright's convention (1989, Ch. 5).

⁴ The term 'result' is here employed according to common scientific jargon, i.e. somehow loosely. It may indeed refer to theorems, predictions, existence or non-existence proofs, but also to the demonstration of the existence of a (causal) relation between modelled entities.

⁵ This does not mean, however, that all the idealised features have the same status. With different terminology, several authors have pointed out the distinction between 'core relations' and 'boundary' conditions (indeed, the distinction goes back to at least Machlup 1955). As Roger Backhouse pointed out to us, ideally we would like a result to be completely independent from boundary assumptions, but to a certain extent dependent on the core assumption of the model. If a result holds independently of a

At the same time, models usually depart from reality also by deliberately omitting some features (abstractions). The crucial point to be noted is that the abstracted features do not appear in the model-economy, although sometimes they may be mentioned in the informal interpretation that goes with it. The omitted aspects are kept ‘in the background’ as conditions, factors, etc. which are supposedly irrelevant for the purpose at hand⁶. Sometimes successive approximations (or ‘concretisations’) permit to add the omitted feature back and thus test the robustness of the result to changes in the degree of abstraction. If carried on, however, eventually the process of concretisation will lead from the theoretical to the empirical realm. This is why to establish the robustness (in the previous senses) of a model is often supposed to be a preliminary step towards (but sometimes even a partial substitute for) identifying a structural relation in the real world.

A third kind of model-robustness we may encounter in economics is robustness to changes in the implied causal mechanism, where progress is identified with the proliferation of models featuring *different* causal mechanisms⁷. Perhaps economists rely on an argument of the same sort as the one used for ‘robustness to changes in idealisations’: if conclusions do not change very much under different assumptions, this means that that particular detail is not so important after all. In some cases, this may be true. If the only aim is to predict, assuming that not much will change in the institutional set-up, an instrumentalist attitude may be justified. In this case, other words, robustness may refer to the conclusions but by no means to the explicative power of the various models that have been employed to justify them⁸. But in many other circumstances, particularly when the possibility of deriving policy prescriptions is

core assumption, then that assumption seems pretty useless. (When the assumptions are variables which can take different numerical values, the issue becomes more complicated. We shall bracket such subtleties here, but see Guala and Salanti 2001.)

⁶ Economists often express this idea by saying that a theoretical result is valid *ceteris paribus* (other things being equal). It would be perhaps more appropriate to say that such a result holds *ceteris absentibus* (other things being absent) or *ceteris neglectibus* (other things being neglected). With reference to economic theory, Musgrave (1981) was the first to point out the difference between these kinds of assumption (although the terminology he used was different). On such distinctions see also Mäki and Piimies (1998) and Boumans and Morgan (2001).

⁷ It is far from easy to define precisely what is a “mechanism”. For the present purposes, we shall define it as a path through a sequence of causal links, connecting one or more parent variables to one or more descendants. For an application to a specific economic issue (based on a slightly different definition of the concept), see also Salmon (1998) and the following discussion.

concerned, reliance on such a kind of ‘robustness’ seems to be seriously misplaced. When this is the case, the proliferation of models is just evidence of our inability to detect the best unique representation of the situation we want to investigate and/or the true mechanism at work. For each real-world situation, indeed, at most only one of these models can tell the true causal story; the fact that we have many alternatives converging on the same result does not, *per se*, imply that we are getting closer to the truth at all⁹.

3. ‘Old’ vs. ‘new’ growth theory

Growth theory has traditionally been based on highly idealised, caricatured models (in the sense of Gibbard and Varian 1978). Because of this feature – which is generally recognised by its proponents as well as by its critics – it constitutes an appropriate case study for our purposes. We would like first to separately appraise OGT and NGT according to the different criteria of robustness we have previously summarised, and then try to figure out in which respects exactly we can speak of a progressive shift from the old to the new ‘programme’¹⁰.

OGT represented from its very outset an explicit attempt to extend the domain of macroeconomics to the analysis of long-period mechanisms of growth. Its first important results were pessimistic conclusions about the stability of growth paths, grounded on the ‘knife-edge’ properties of equilibrium path as emerging from the Harrod-Domar model. This result, however, was quite soon shown to be fragile: small changes in the idealisations and abstractions (concerning either the form of technology as in the neoclassical approach, or the form of the saving function as in the neo-Keynesian formulation) suffice to produce much more favourable conclusions about the stability (convergence to steady states) of equilibrium paths.

⁸ Cf. Rosenberg (1978, p. 683 and 1992, pp. 84-85).

⁹ Cf. Cartwright (1991) for a very similar argument. In the terminology of Backhouse (1997, pp.100-101), we may say that robustness to the causal mechanism increases the *generality* of a result, but diminishes its *scope*. (Where the ‘scope’ is the class of *identifiable* situations of applicability.)

¹⁰ The problem of what exactly is scientific progress has been widely debated in the philosophy of science literature. For interesting discussions focused on economics cf., for instance, Boland (1989) and Backhouse (1997). By focussing on “robustness”, we try in this paper to propose yet another set of criteria for progress, which we feel is more in line with economists’ practice and intuitions. As we shall see, to establish robustness in a model is regarded as a preliminary step to (but sometimes even a substitute for) establishing a structural relation in the real world. Robustness, in other words, may be regarded as a property of either theoretical models or empirical generalisations.

Both these approaches (the neoclassical and the neo-Keynesian), separately considered, seem to be fairly robust according to our first two definitions of robustness. Within each of them we may find indeed a great variety of models (based on different idealisations and abstractions¹¹), leading to similar conclusions. But our third robustness criterion (namely, robustness to changes in the implied causal mechanism) provides some food for thought. If the ultimate goal in growth theory is the discovery of some effective set of policy recommendations suited to promote economic growth, then robustness to changes in idealisations or abstractions is only part of the story. It also matters, eventually, how much reliance we can place on the actual functioning of the mechanisms represented in our models. The problem is that, even if it is not always acknowledged, different idealisations and abstractions often involve different causal mechanisms as well. This is what happens, in neo-classical models, when the assumption of an exogenously fixed propensity to save in the Solow-Swan model is replaced either by a representative household maximising utility over an infinite horizon, or by an overlapping generations mechanism (and, incidentally, it is not clear at all how such particular mechanisms might be exploited in order to enhance the rate of growth in actual economies). Moreover, neoclassical and neo-Keynesian approaches envisaged deeply different causal mechanisms behind the growth process. In fact, the fierce debates during the 1960s and the 1970s between the advocates of the two approaches involved a fundamental disagreement about one of the most important causal mechanisms embodied in those models, i.e. the causal link between accumulation and distribution. However, even neo-Keynesian models of growth, by leaving virtually unexplained the determinants of distribution, were unable to offer secure prescriptions on policy matters.

The highly idealised character of (all) models within OGT, together with a significant amount of uncertainty about the ‘true’ causal mechanisms, prompted many scholars to regard those models as no more than a first step towards a satisfactory explanation of economic growth. During the 1960s and 1970s, caveats about the limitations of growth theory were indeed very common. It was usually maintained that its content was neither a satisfactory description of actual growth processes or development experiences, nor a useful starting point for policy recommendation, but simply a first step towards a better understanding of some fundamental

¹¹ The neoclassical version of OGT supports, for instance, models with one or two sectors, with vintage or non-vintage capital goods, and so on: cf. Burmeister and Dobell (1970) or Wan (1971). On models in the neo-Keynesian tradition, see Harris (1978) or Marglin (1984).

mechanisms (primarily the accumulation of capital) affecting economic growth. As Frank Hahn (1971, p. vii) puts it:

The theory of growth is not a theory of economic history. It is of no help in answering Max Weber's famous question and only of marginal use in understanding, say, Industrial Revolution. Where the theory is to be taken descriptively, it takes the institutional setting for granted and highly idealises it. The parts of the theory which are to be understood as prescriptive have hardly anything to say on either the actual problems of 'control' or on the society to be controlled.

In this respect, new growth theorists display a different and much more ambitious attitude. Consider, for instance, the following quotation from Aghion and Howitt (1998, pp. 6-7), a recent and widely acclaimed advanced textbook on NGT:

Because of its explicit emphasis on structural aspects of the innovation process, endogenous growth theory makes it possible to bridge the gap between theory and various strands of empirical and historical literature. [...] Thus one of our primary motivations in developing the model [...] with capital accumulation and population growth is to show that when these other important aspects of growth are taken into account, our approach becomes broadly consistent with the empirical observations that have been adduced to refute it.

Behind this sharp change in perspective¹² lies a firm belief in the possibility of deriving some reliable prescriptions of policy designed to rise the growth rate of actual economies¹³. Accordingly, one of the most apparent differences between OGT and NGT is that the latter places much more emphasis than its precursor upon its descriptive adequacy and, consequently, the reliability of policy recommendations coming from it.

Moreover, while OGT had to resort to some form of what we may now recognise as piecemeal theorising, the most recent approaches are more akin to the neoclassical view on theory-making, according to which all economic models must be built on the common basis of a few basic premises (individual maximising rationality and subjectivistic equilibrium being the best-known). We find here, incidentally, paradigmatic examples of the two principal ways in which the term 'ad hocness' is used in economics. Both are fairly different from the standard philosophical definition of 'being able to account for the empirical data only after the

¹² For an intermediate position on the applicability of (old) growth theory, see Solow (1970).

¹³ The same attitude may be found, for instance, in Shaw (1992) and Crafts (1996).

latter have been observed'¹⁴. OGT was 'ad hoc' in the sense that it relied on a number of non-robust assumptions (primarily the view of technical progress as exogenous), a change in which was sufficient to make the whole theoretical structure crumble. It also appeared inadequate because the explanation of some assumptions about exogenous variables was implicitly or explicitly deferred to other pieces of theory.

NGT, in contrast, supports a different choice of what is to be regarded as exogenous, probably more coherent with its own methodological premises. The price to be paid, however, is twofold: (i) the old distinction between 'development' and 'growth' virtually vanishes, but the underneath problem reappears under the form of the heterogeneity of structural parameters when cross-section analyses are performed; (ii) the usual economists' list of 'fundamentals' does not seem sufficient for an adequate explanation of growth and a rapidly growing number of other exogenous variables is necessary. Because some of these do not fall within the traditional boundaries of the discipline, in addition to the problem of their selection, we have to face the even bigger question of the demarcation of 'economic' from other generically 'social' phenomena. More than that, the proliferation of possible determinants of growth brings with it a proliferation of possible causal mechanisms that is beginning to cause some embarrassment among practitioners.

A possible 'ecumenical' solution would be this: to acknowledge that different causal mechanisms actually *are* at work in real-world economies and that observed rates of growth are a (possibly very complicated) combination of all their effects. A proliferation of models could be considered progressive in so far as it approximates real-world diversity and pluralism. The problem here is that to recognize the contemporary presence of different causal mechanisms at work is more typical of historical than economic interpretations. As we may find in a recent review of two books on NGT (Agliardi 2001, p. F773):

Endogenous growth theory has tried to understand the interplay between knowledge and structural characteristics of the economy, and how such interplay results in economic growth. A somewhat disturbing feature of the recent work has been a proliferation of special models, each with its own assumptions. This proliferation is, for the most part, a healthy thing, an indication that old assumptions are being challenged and that innovation is taking place.

¹⁴ Cf. for instance Popper (1963, ch. 10), Laudan (1977, p.115). For a survey of the notion of ad hocness in the Popperian tradition, see Hands (1988).

However, it becomes necessary a synthesis that defines the common elements in the variety of new models and, at the same time, re-establishes some continuity with older traditions.

Given the ubiquitous claim that NGT provides for better interpretations of the relevant facts about growth and development, what the reviewer points out as the most urgent need within this field of research simply misses the point. What is needed, indeed, is not so much a theoretical ‘synthesis of the common elements’ as a selection, among all the available ones, of the effectively operating mechanisms.

When it comes to choosing between mutually exclusive mechanisms, it is empirical evidence, obviously, that becomes decisive. This opens the problem of appraising the body of empirical research related to NGT. The shift from OGT to NGT can be regarded, at most, as theoretically progressive in the light of the first two of the robustness criteria identified above, but it came with a price. In particular, it made the business of empirical testing much more complicated than it used to be. First, it must be noticed that with the advent of NGT the facts (whether stylised or not) to be explained have changed, particular emphasis being placed on the growth of some per capita magnitudes as a result of possibly different sources of increasing returns.

Secondly, while empirical applications associated with OGT were scarce and almost limited to estimating Solow’s residual and/or total factor productivity (and cross-country data were regarded as highly suspicious if not totally useless), with the advent of NGT the amount of applied research (together with the number of supposedly relevant variables) has enormously increased. As a consequence, NGT markedly departs from the classical tradition, to which the OGT was still somehow linked, of trying to focus on the fundamental mechanisms capable of fostering growth or, in other words, on the fundamental laws of motion of industrial economies¹⁵.

The problem here is that the empirical evidence¹⁶, which according to its advocates should support NGT, at a closer scrutiny appears to be far from decisive. Steven Durlauf (2000a) has recently pointed out the inconclusiveness of much recent empirical work intended to exploit a number of insights provided by NGT. He goes beyond some criticisms previously set forth in

¹⁵ When we see studies on the relation between, say, personal distributions of income, levels of democracy, or different tax systems, and growth, can we still legitimately speak of ‘fundamental’ laws?

¹⁶ For a recent survey of such a ‘new growth evidence’, see Temple (1999).

Durlauf and Quah (1988)¹⁷, with an admittedly “provocative purpose — namely to argue that the econometric component of the new growth literature has done little to adjudicate leading growth questions” (p. 249). In his opinion, because almost all variables employed to explain growth in real per capita income are probably endogenous (and possible instrumental variables, due to the open-endedness of growth theories, are likely to be correlated with the error term), we cannot easily jump from finding statistical correlations to claiming stable causal links. Moreover, discriminating between competing explanations of growth would require reliable criteria for sorting out the relevant variables, but statistical robustness does not seem to be attainable through the actually available procedures of variable selection¹⁸.

4. Final remarks

Despite the (sometimes bold) claims in favour of NGT, this approach has failed so far to detect the fundamental determinants of growth and to provide firm empirical evidence of the relevant links among variables. Consequently it has also failed to provide precise and reliable prescriptions about policies intended to foster economic growth (an issue whose practical importance can hardly be underestimated).

NGT offers a number of possible explanations of mechanisms generating (more or less directly) cumulative processes, but in this case (assuming the possibility of deriving sound policy prescriptions as the pre-eminent target) robustness to changes in the implied causal mechanism(s) is more a problem than a virtue. A problem, unfortunately, which still has to be solved. Of course, growth theory remains a field of research worth of serious attention. After all, in the long run it is not so much the optimal allocation of given resources or the effectiveness of counter-cyclical policies, as the rate of growth of per-capita magnitudes, that, given a reasonable degree of inequality in the distribution of individual incomes, determines people’s standard of life.

¹⁷ See also Durlauf (2000b) and (2001).

¹⁸ More technical objections concern, on one hand, models with multiple steady-states, for which the usual specification of empirical models based upon linearisation may lead to misleading conclusions about convergence issues. On the other hand, assuming an invariant statistical model when performing cross-section analyses of growth amounts to completely disregard the important question of parameter cross-country heterogeneity (while both econometrics and historical analyses lead to regard heterogeneity as a key feature of country-specific patterns of growth).

Our main point is that the three notions of robustness highlight some of the methodological problems within growth theory (and other sub-fields in economics, although we cannot show it here) that traditional philosophical accounts leave somewhat unaccounted for. Take, for instance, the classic Popperian notion of ‘ad hocness’ as post-hoc accommodation of previously observed evidence. If taken seriously, such a criterion would condemn almost all of what economic theorists usually consider progressive in economic history¹⁹. At a closer scrutiny, economists’ attempts to refine growth theory display a more articulated situation: the development of growth theory was progressive on the first two robustness dimensions (robustness to different idealisations and abstractions), but problematic on the third dimension (due to the proliferation of alternative causal mechanisms). Such a trade-off between different methodological criteria (robustness criteria, in this case) is the price to be paid for abandoning ‘monistic’ methodological frameworks such as the Popperian one. But it pays off by reducing the gap between economists’ practice and abstract philosophical theorising.

¹⁹ This is indeed the conclusion forcefully stated in Blaug (2000), where NGT is portrayed as nothing but a further — albeit brilliant — exercise in neoclassical theory, lacking any convincing explanation of empirical evidence and bringing no real improvement on OGT.

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