***Very** provisional draft*

On the robustness of models within old and new growth theory*

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Introduction

Scientific progress is often supposed to consist, among other things, of the discovery of better and better theoretical and/or empirical relations within a particular field of research. How to characterize a better explanation is nonetheless far from obvious. In this respect practitioners have been usually inclined, in order to solve the problem of choice between different theories or between different models pertaining to the same theoretical perspective, to resort to some conventionalist criteria such as simplicity, generality, falsifiability, and so on. Each one of these criteria, however, raises the problem of infinite regress[1], while all together they pose the problem of potential conflicting evidence between conclusions, which might be possibly drawn on the basis of different criteria.

More recently, as it can be either detected in the literature[2] or frequently heard during presentations at seminars and workshops, practitioners seem to becoming increasingly prone to hold in high esteem the rather different criterion of *robustness* . Among the most telling examples of such an attitude, I count: (1) the widespread habit of speaking about classes of models , instead of single models

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1 As Boland (1997, p. 107) aptly points out, although those who practice conventionalism usually deny that a theory is true or false (a theory is either ¨better¨ or ¨worse¨, they presume their criteria can be true. Each of the criteria listed above presumes something about *the* true theory of the real world .

2 Gibbard and Varian (1978) is one of the few philosophical accounts of economic modelling that makes explicit use of the idea of robustness (for a more recent discussion, see also Sugden 2000). The authors characterise models (or at least one important class of them) as caricatures . In their own words: A caricature differs from an approximation not only in its simplicity and inaccuracy, but also in its deliberate distortion of reality. When a model is applied as an approximation, the goal is to distort as little as is compatible with a given degree of simplicity and tractability. A caricature involves deliberate distortions of reality for other reasons to isolate the effects of one of the factors involved in the situation, or to test for robustness under changes of caricatures (p.676).

receiving the consensus of the profession; (2) the general belief that theoretical propositions concerning economic mechanisms are more reliable if repeatedly demonstrated within different classes of models (that is to say that the multiplicity of models for a very same phenomenon is perceived as a virtue, instead of a sign of utter confusion or at least of disturbing uncertainty); (3) the common practice of building models at different levels of abstraction and simplification without being able of showing their reciprocal connections in a rigorous way[3].

Growth theory, either old or new , does not seem to make an exception to all this. The following sections are thus devoted to appraise the robustness of models within both the traditional theories of growth and new growth theory (NGT) according to the different senses in which a model can be said to be robust. Special attention is paid to the robustness in the implied causal mechanisms, due to its substantial role concerning the possibility of deriving sound policy prescriptions from (either theoretical or empirical) models.

Three different notions of robustness

Robustness, like other normative terms, is often taken as a comprehensive notion 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), three main different meanings of the term have been identified: (1) robustness to changes in the model s idealisations ; (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.

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[3] There are at least two other competing explanations that may appear equally compatible with such a state of affairs. One has been proposed by Faverau (1997, p. 122ff), in the form of the conjecture that the role played by models in economics is close to that played by experiments in hard science . The other, to which Morgan and Morrison (1999) have recently drawn economists attention, looks at models as mediators . As mediating entities they are said to have three main characteristics: they are *partly independent* both from high theory and from system they are supposed to explain, they *stand for* some real-world systems of interest (which may be regarded as target system), and they can be *manipulated* in order to learn something about the real world.
Each of these two approaches would deserve a detailed discussion that cannot be pursued here any further. Note, however, that at first sight viewing models as caricatures (cf. note 2), experiments or mediators does not seem reciprocally incompatible. After all, if a caricature displays the previous three characteristics it might well serve as a mediator. On the other hand, if experiments may be interpreted as being mediators themselves (cf. Guala 1999), it becomes apparent that models may play the same role as the experiments.

The first two notions emerge from considering that models provide simplified representations of real entities and systems in roughly two ways, by distortion and, commonly at the same time, by omission of some aspects of reality. The terms *idealisation* and *abstraction* are meant to refer to these two distinct implications of model-building activity respectively.[4]

Every model incorporates (possibly highly) idealized descriptions of some real-world features. They are usually intended to 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 result[5] (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 that in that model[6].

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 can 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 are clearly highlighted as such[7].

Moreover, at the same time any model usually departs from reality also by deliberately omitting some features. 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

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4 It must be noted that 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 the distinction should be understood in the same sense as in Cartwright (1989, Ch. 5).

5 The term result is here employed according to the common scientific jargon, i.e. somehow ambiguously. 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.

6 Many other examples of idealisations within (old or new) growth could be easily provided. Think of, for instance: the different definitions of neutrality of (exogenous) technical progress within old growth theory; 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.

7 This does not mean, however, that all the idealized features have the same status. With different terminology, several authors have pointed out the distinction between core relations and boundary conditions (indeed, it goes back to at least Machlup 1955). It does exist, therefore, an implicit hierarchy between types of robustness: the robustness of the core relations to changes in the value of their variables comes first in the (lexicographic) scale of appraisal criteria.

aspects are kept in the background as conditions, factors, etc. which are supposedly irrelevant for the purpose at hand.[8] Sometimes successive approximations permit to add the omitted feature back and thus test the robustness of the result to changes in the level 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 to (but sometimes even a substitute for) establish 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 mechanisms[9]. If robustness is obtained in that way, however, it may at most refer to the conclusions but by no means to the explicative power of the various models that have been employed to justify them[10]. When this is the case, the proliferation of models cannot but be regarded as evidence of our inability to detect the best 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 all. Perhaps economists rely on an argument of the same sort as that 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 nothing much will change in the institutional set-up, an instrumentalist attitude may be justified. But in many other circumstances, particularly when the possibility of deriving policy prescriptions is concerned, reliance on such a kind of robustness seems to be seriously misplaced.

Old vs. new growth theory

One of the most discernible and apparent differences between old and new growth theory is that the latter places much more emphasis than its former precursor upon its descriptive adequacy.

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[8] Economists often express this idea by saying that a theoretical result is valid *ceteris paribus* (other things being equal). With reference to the structure of models it would be perhaps more appropriate to say that such a result holds c*eteris absentibus* (other things being absent).

[9] To define precisely what is a mechanism is far from easy. For the present purposes I may refer to the illustration given in Elster (1998). For an application to a specific economic issue (based on a slight different definition of the concept), see also Salmon (1998) and the following discussion.

[10] Cf. Rosenberg (1978, p. 683 and 1992, pp. 84-85).

During the 1960s and 1970s, caveats about the limitations of growth theory were 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 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 convey a different and much more ambitious attitude. Consider, for instance, the following passages taken 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 of perspective we may easily detect a firm belief in the possibility of deriving from (new) growth theory some reliable prescriptions of policy designed to rise the rate of growth of actual economies[11].

According to what has been said in the previous section, however, such a claim should imply that new growth theories could be regarded as more robust than the old ones. This is just what, in my opinion, should be denied. In order to sustain such a conclusion a much more detailed discussion than the one provided below would be in order. Unfortunately, I can offer here only a few hints, but I hope in the right direction.

About the robustness of models in the old growth theory, the main points can be summarized as follows:

- The knife-edge properties of the equilibrium path as emerging from the Harrod-Domar model have been subsequently shown to be far from robust: as is well known, small changes in the idealisations and abstractions involved (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 reach much more

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

favourable conclusions about the stability (convergence to steady states) of equilibrium paths;

- Both the subsequent approaches (the neoclassical and the neo-Keynesian), separately considered, seem to be fairly robust according to our first two definitions of robustness. Within each approach we may find indeed a great variety of models (based on different idealisations and abstractions), leading to similar conclusions12;
- Even if it is not usually acknowledged, different idealisations and abstractions may imply different causal mechanisms as well. This is what happens, for instance, when the assumption of an exogenously fixed propensity to save in the Solow-Swan model is replaced either by a representative household maximizing utility over infinite horizon or by an overlapping generations mechanism;
- The fierce debates during the 1960s and the 1970s between the advocates of the two approaches involved, among other things, a fundamental disagreement about one of the most important causal mechanisms embodied in those model, i.e. the causal link between accumulation and distribution.

When compared with the old one, new growth theory is usually regarded as progressive for at least three reasons. First, with the advent of NGT the facts to be explained (stylized or not) have undoubtedly changed, particular emphasis being placed on the growth of some per capita magnitudes and on possible sources of increasing returns. Second, while old growth theory had to resort to some form of what we may now recognize as piecemeal theorizing (due to theorists reliance on a number of *ad hoc* assumptions about exogenous variables whose explanation was implicitly or explicitly deferred to other pieces of theory), the most recent approaches are more akin to the neoclassical view on theory-making all economic models must be built on the common basis of a few basic premises (individual maximizing rationality and subjectivistic equilibrium being the most well-known). New growth theory, therefore, supports a different choice of what is to be regarded as exogenous, probably more coherent with its own methodological premises13.

12 See, for instance, Burmeister and Dobell (1970) or Wan (1971) and Harris (1978) or Marglin (1984), respectively.

13 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 seems relevant. 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 phenomena.

Finally, while empirical applications 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 new growth theory the number of relevant variables has enormously increased. As a consequence, new growth theory markedly departs from the classical tradition, to which the old growth theory 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 economies[14].

Behind this sharp change of perspective we may easily detect a firm belief in the possibility of deriving from growth theory some reliable prescriptions of policy designed to rise the rate of growth of actual economies. The ultimate scope of this research, as in other fields of economics, should be to discover some effective set of policy recommendations suited to promote economic growth. In this respect, however, robustness to changes in idealisations or abstractions is not so important. What really matters, indeed, is obviously how much reliance we may place on the actual functioning of the mechanisms we try to represent in our models and empirical evidence, therefore, becomes decisive.

The problem, in this respect, is that the empirical evidence[15] grounded on the new growth theory appears to be far from decisive. Steven Durlauf (2000) has recently pointed out the inconclusiveness of much recent empirical work intended to exploit some of the insights provided by new growth theory. He goes well beyond some criticisms previously set forth in Durlauf and Quah (1988) 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). His main remarks can be summarized as follows:

- because almost all variables employed to explain real per capita income growth are likely to be 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;
- for models with multiple steady-states the usual specification of empirical models based upon linearization may lead to misleading conclusions about convergence issues;

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[14] When I see studies on the relation between, say, personal distributions of income, levels of democracy, or different tax systems and growth, I think that we cannot speak anymore of fundamental laws.

[15] For a recent survey of such a new growth evidence , see Temple (1999).

- discriminating between competing explanations of growth would requires reliable criteria for sorting out the relevant variables, but statistical robustness does not seem to be attainable trough the actually available procedures of variable selection;
- both econometrics and historical analyses lead to regard heterogeneity as a key feature of country-specific patterns of growth;
- assuming an invariant statistical model when performing cross-section analyses of growth amounts to completely disregard the important question of parameter cross-country heterogeneity.

Final remarks

Despite the (sometimes bold) claims in favour of new growth theory, this approach ultimately fails to detect the fundamental determinants of growth and to provide firm empirical evidence of the relevant links among variables.

New growth theory offers us 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 by no means a virtue.

However, growth theory remains a field of research surely worth of serious attention. If the hope of deriving from growth theory some reliable prescriptions of policy designed to rise the rate of growth of actual economies could actually be accomplished, its importance could be hardly underestimated. 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.

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