Department of Industrial Economics and Strategy
Working Paper 97-2

The New Growth Theory: Some Intellectual Growth Accounting

by

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REVISED DRAFT
January 10, 1997

Abstract

This paper discusses the reasons for the success of the New Growth Theory. Given that the NGT has a somewhat tautological quality, that its essential ideas have been known for a long time, and that it does not make contact with a large literature on institutions and economic change, its strong success is arguably surprising. At any rate, it causes problems for models of scientific change that focus on novel facts as a criterion of progress, such as Lakatos'. The success of the NGT is better explained by, for example, Laudan's focus on increased problem-solving ability as an important cause of scientific change.

Acknowledgments:

The comments of Esben Sloth Andersen, Bent Dalum, Jan Fagerberg, Henrik Lando, Keld Laursen, and Steen Thomsen are gratefully acknowledged. The usual disclaimer applies.

Introduction

This paper is in the nature of a primarily methodological reflection on what is now generally known as "the new growth theory" (henceforth, NGT).[1] It is to some extent a progress report, that is, an attempt to clarify and organize thoughts and present the results of this process. However, it is more than simply a summary of existing doctrine. For I have from the beginning of my (brief) interest in the subject been somewhat surprised by the enormous attention that proponents of the NGT have received, not only in professional circles but also in the more popular press (notably, in *The Economist*). The reasons for being baffled are many.[2] Here are some of these:

1. The most important thing about the NGT is not that it appears to put forward any "novel facts", in the Lakatosian sense (Lakatos 1970) of the word, namely some new fact about the empirical world. In fact, it is hard to find such novel facts in the NGT. It is true, of course, that in the NGT the long-run rate of growth is not explained by the growth of population – as in the traditional Solow model – but by knowledge accumulation. However, that knowledge accumulation is behind the growth process is not a new recognition in economics. Relatedly, the overall welfare and policy conclusions – for example, that investment in human capital is suboptimally low and that general education is therefore called for – can hardly be called novel either.

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[1] I shall not here consider the closely allied "new trade theory", although what I have to say may have implications for this body of theory, too.

[2] In fact, I am even more surprised by the fact that so few observers seem to be baffled! An exception is Mankiw (1993, p.300) who in a rather unimpressed discussion flatly says that the NGT "...has been oversold by its advocates".

2. There is a certain tautological quality to the NGT: If we assume that knowledge is a motor force behind growth because of the externalities that the process of knowledge accumulation creates, then it is surely comforting to be told as a conclusion that different rates of growth in knowledge accumulation leads to different rates of long-run growth!

3. The NGT would seem to obtain part of its tautological character because the institutions and firm dynamics underlying the process of knowledge-accumulation and growth is not theorized. Or, to put it more bluntly, Why don't Romer, Lucas, Rebelo, Grossman et al. read Douglass North (1990) or Rosenberg and Birdzell (1986) or even Fukuyama (1995)?

4. Related to the preceding point, we may ask – along Coasean lines – why institutions do not arise to internalize the knowledge externalities that are so important to the growth process (Weder and Grubel 1993; Richard Langlois and Paul Robertson 1996). More generally, why aren't property rights and transaction costs considered in the NGT? If there is one thing that we know is necessary to "make a miracle" (to borrow a line from Robert Lucas 1993), this is that the matrix of property rights has to be secure and relatively well-defined.

5. Stripped to their non-mathematical essentials, the basic mechanisms that are highlighted in the NGT have been known for a long time. Almost sixty years ago, Allyn Young (1928) (no, not *Alwyn* Young), to some extent building on the even older work of Alfred Marshall (1890), pointed to the centrality of externalities – pecuniary as well as technological – and increasing returns in the growth process. That it is good to include some market power so that R&D outlays can be rationalized (Paul Romer 1990) is hardly a new recognition either. Moreover, Kenneth Arrow (1962) long

ago pointed to the role of learning by doing in the growth process, a factor that has been highlighted in the NGT. Finally, Richard Nelson (1994) argues that most of the substantial content of the new growth theory was in fact presented in 1952 by Moses Abramowitz. Thus, it is doubtful whether there is nothing inherently new or revolutionary about the NGT; in retrospect, it may turn out to have been a much more incremental development than other developments during the last two decades, such as rational expectations, equilibrium business cycles, work on incentive compatibility and contract theory, etc.

In the following pages, I shall explain and expand on these points. However, I shall do something more than present a critical perspective on the NGT. For if I am right in claiming, for example, that there is a tautological quality to the NGT and, moreover, that the theory (in the light of the history of economic thought) does not really address any new theoretical mechanisms, then what is all the fuss about? Why is the NGT, along with work on incentives and contracts, the probably most prestigious sub-discipline the budding young economist can choose as her area of specialization?

I shall suggest that the explanation of the apparent success of the NGT should be sought, not in the ability to say something new and interesting about empirical reality *per se*, but primarily in the apparent expansion of the problem-solving capacity of general equilibrium theory that the NGT undeniably represents. This in itself is an important lesson about neoclassical economists' criteria for theory choice: Success is not primarily a matter of putting forward and explaining some novel fact (although this may help) – it is a matter of bringing general equilibrium theory closer to empirical reality by endogenizing something that has hitherto been treated as exogenous.[3]

Assuredly, it is possible to explain the breakthrough of NGT in other, more externally oriented, ways, or by pointing to seemingly "irrational" motives. For example, at the end of the 1980s enthusiasm in macroeconomics and business cycle theory apparently had faded somewhat as more "structural" policies came increasingly into focus. Moreover, we should not be blind to the existence of fads in economic research. These latter observations may, however, be consistent with the above, more "internal", explanation.

As the reader has no doubt become aware of by now, there is a strong negative quality to the argument of this paper. And critique – at least of the more encompassing sort – without any alternative is in a sense rather pointless. However, I do think that there exists theoretical alternatives to the NGT, even if these cannot conform to the formal standards imposed by proponents of the NGT. Future work will be concerned with arguing that, for example, Douglass North's (1990) work on institutional change and economic growth in fact constitutes a rival and – in a substantive (if not formal) sense – superior body of work.

[3] Thus, the methodological perspective I here adopt owes more to Larry Laudan (1977) than to Imre Lakatos (1970).

The New Growth Theory

> "Technological advance, resting in new knowledge and occurring accidentally or mechanically, seems to be the only possible offset to this 'natural' tendency to diminishing returns" (Frank Knight 1944).

In this section, I provide a brief conspectus of the key contributions to the NGT, and present some critiques of these. I only focus on three contributions – Romer (1986, 1990) and Lucas (1988) – out of an already very large literature.[4] My justification for being so narrow is that these are arguably the seminal contributions, and that the critical points I wish to make presumably also apply to the rest of the literature. The critical points are mainly those mentioned in the Introduction.

A. Background and Key Contributions

The story begins, not with Allyn Young, Schumpeter and their lot, and not even with Harrod, Domar or Kaldor, but with Robert Solow (1956, 1957). First, Solow made economic growth understandable in terms of neoclassical economics. The ambition of both Harrod and Domar, in contrast, apparently was to provide a long-run counterpart to Keynes' macroeconomics (Graham Hacche 1979, p.3). Solow's (1956) early work is often referred to as "the neoclassical model" to differentiate it from the NGT (e.g., Romer 1994)[5]. Second, he presented a puzzle, namely the nature of the famous "Solow residual": If increases in the stock of labor and capital only explain a small part

[4] Barro and Sala-i-Martin (1995) provides a well-written and accessible overview of the field.

[5] Of course, this is something of a misnomer because the NGT is quintessentially neoclassical.

of the observed growth in industrial countries, what explains the rest (Solow 1957)?

The nature of puzzle did not really concern what was underneath the residual, for even then there was agreement that human capital accumulation and technological progress were the main drivers behind the productivity advances of labor and capital. The puzzle was more in the nature of precisely accounting for these mechanisms – of endogenizing them. Arrow's (1962) introduction of learning by doing effects was one important advance in this directions as was later models on embodied capital.

In the beginning of the 1970s interest in growth theory petered out, however, in favor of macroeconomics and business cycle theory. Monetarism, equilibrium business cycle theory and rational expectations became all the rage for some one and half decade. Gradually, however, interest in business cycle theory shifted away from monetary factors towards an appreciation of real factors (e.g., Finn Kydland and Edward Prescott 1982) and policy-makers' interests shifted toward more long-run and "structural" issues. Arguably all this helped pave the way for the wildfire of interest in growth theory that hit the profession at the end of the 1980s.[6]

The initiating event clearly was the publication of Paul Romer's 1986 article on "Increasing Returns and Long-Run Growth". At least three, and perhaps four, slightly different streams of research – or more precisely: modeling strategies – within the NGT are identifiable. The first is that associated with Romer's initial paper (Romer 1986); the second one is associated with the work of Robert Lucas (1988, 1993); the third, and arguably dominant, modeling strategy is that pioneered by Romer (1990) (this is the one

[6] This is pure speculation on my part. One tiny piece of evidence, however, concerns the centrality of Lucas and Barro in both efforts.

that is often referred to as "endogenous growth theory"); and a possible fourth approach is that represented by Aghion and Howitt (1992) that is differentiated by drawing on the patent-race literature.

These approaches differ, for example, with respect to whether a competitive equilibrium or a monopolistic competition equilibrium is used as the basic set-up, although they are of course all built on some notion of economies of scale as the ultimate driver of persistent growth.[7] Romer's 1986 model was a competitive equilibrium model as are Lucas' (1988, 1993) models; however, building on the work of Dixit and Stiglitz (1977), Romer (1990) – and with him a number of new growth theorists – have preferred to work with monopolistic competition models.

However, a more convenient way to put in perspective some of these different modeling approaches is to focus on the aggregate production postulated in these models (Howard Pack 1994). The standard production function postulated in Solowian growth model takes the form: $Y = Ae^{\mu t} K^{\alpha} L^{1-\alpha}$, where Y is gross national product, K is the stock of capital (both physical and human), A is a constant that reflects the technological start position of the relevant economy, L is unskilled labor, e^{μ} represents the exogenous rate at which technology evolves, and α represents the percentage increase in gross national product from a 1 % increase in capital.

Empirically, α is derived simply from capital's share in the national income account, which means that implicitly capital is taken to be paid its marginal product and to yield no externalities (Pack 1994). It is this aspect which is changed in many models of the NGT. Importantly, this helps

[7] In fact, it would appear to be axiomatic in this literature that economies of scale are necessary to any theoretical understanding of the growth process. However, as Milgrom, Qian and Roberts (1991) point out, (Edgeworth) complementarities can do the job independently of scale considerations.

addressing the obvious deficiency of the standard formulation: The determinants of μ, the rate of growth of income per capita, are left unaddressed in the model.

A compact overall formulation that captures the main ideas of the different modeling strategies in the NGT is the following aggregate production function: Y = F (K, L, H, A) (we do not need to care here about specific functional forms), where we now explicitly makes a distinction between physical and human capital. Moreover, A is no longer a constant.

For example, *Romer (1986)* considers a simplified version of the above production function, namely $Y = A(R)\, F(R_i, K_i, L_i)$, where R_i is the stock of results from expenditure on R&D by firm i and where spillovers from private research efforts lead to improvements in the public stock of knowledge, A(R). Because Romer's set-up is a competitive equilibrium, he assumes F to be homogenous of degree one in all inputs. Thus, there are, in Marshall's (1890) words, no "internal economies" in the model, only the "external economies" represented by A(R). One may say that these externalities offset the propensity to diminishing returns to the other inputs

Underlying this is a view of technological knowledge as having, to some extent, public good characteristics, that is, it is not fully excludable and it is non-rival – a view that characterizes all of the NGT. As Romer (1986, p.1003) explains: "The creation of new knowledge by one firm is assumed to have a positive external effect on the production possibilities of other firms, because knowledge cannot be perfectly patented or kept secret". A mechanism behind this effect is that the marginal cost of using new knowledge is assumed to be zero or close to zero. The low costs of using existing knowledge is also assumed to lower the costs of producing new knowledge, thus causing dynamic scale economies in knowledge accumulation. Because of the assumption of only partial excludability, there are intertemporal positive externalities from private knowledge accumulation efforts.

The presence of knowledge externalities is not only the driver behind increasing returns, but also where the model makes contact with welfare issues. The conclusion, of course, is the one that has generally characterized neoclassical work on the welfare properties of the innovation process, namely that firms when investing in research will only take private and not social benefits into account, thus causing a suboptimal level of investment in knowledge.

Lucas (1988) essentially also reaches this conclusion, albeit with a different set-up. He begins with an aggregate production function of the form: Y = A(H) F (K,H), which corresponds to two different sectors and two different types of investment, namely in physical capital and in human capital. As in Romer (1986), the propensity to diminishing returns in an input, in this case H, is offset by externalities, in this case caused by the input itself: "...let the *average* level of skill or human capital...contribute to the productivity of all factors of production" (Lucas 1988, p. 18). As in Romer's (1986) model, the competitive equilibrium is suboptimal. Of course, in Lucas' model, it is the accumulation of human capital that is suboptimal.

Romer (1990) is the perhaps most important single contribution to the NGT. It is a three-sector model that allows for internal economies, which means that price-taking competitive equilibrium cannot be supported. Monopolistic competition is introduced to reflect the presence of market power. The aggregate production function may be written as Y = F (K, L, H, A), where the stock of knowledge about technology A is assumed to be non-rival, L is measured by the number of people in the workforce, K is disaggregated into a continuum of producers' goods, and human capital, H, represents on-the-job training and education. This functional form suppress, however, the complex structure of the model.

Using human capital and the existing stock of knowledge, the research sector produces “designs” for producers’ goods under increasing returns to scale. Specifically, there are increased marginal productivity to human capital in the research sector. Private firms in this sector invest in R&D activities and produce and commercialize a new widget. But the knowledge developed on a private basis spills over into the stock of general knowledge which can be applied by other firms in the research sector in the production of “wedgets”, “wudgets” and “wodgets”. Each new product in turn contributes to stock of non-depreciating general knowledge, which implies that the marginal productivity of the human capital employed in the research sector is increasing over time.

The producers’ goods sector then uses these designs for producing producers’ goods, which are then applied in the consumers’ goods sector by firms with market power. Solving the model, Romer finds that the rate of growth is determined by the stock of human capital that is employed in research, and that this stock will increase with the total stock of human capital in the population.

B. Critique

Given that the NGT has been in existence for about a decade – provided, as seems reasonable, that Romer (1986) is taken to be the founding contribution –there are surprisingly few contributions that are more radically critical of the approach (except for Nelson 1994; Pack 1994; Solow 1994; Weder and Grubel 1993; Langlois and Robertson 1996).[8] Most debate seems to take place between

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[8] This may be contrasted with the advent of the new classical macroeconomics which immediate provoked heated debate. (There are many overall similarities between the NGT and

contributors to the NGT and to concern the specific modeling strategies employed (Romer 1994). In contrast, the following points are more in the nature of "external" critiques.

Tautological reasoning. Robert Solow (1994, p. 53) noted that "[t]he idea of endogenous growth so captures the imagination that growth theorists often just insert favorable assumptions in an unearned way; and then when they put in their thumb and pull out the very plum they have inserted, there is a tendency to think that something has been proved". In other words, Solow accused the proponents of the NGT of tautological reasoning, although he did not provide examples.

However, these are easy to find. At the broadest level, they characterize the whole explanatory approach of the NGT. This basically argues that if knowledge is a peculiar good that implies increasing returns because of externalities, then the level of knowledge and the accumulation of knowledge can explain, for example, differences in growth among nations (Lucas 1988, 1993). By thus designing the assumptions we make with respect to knowledge to reflect the phenomenon we wish to "explain" (inserting "favorable assumptions in an unearned way"), we never really get to know whether it actually *is* knowledge that drives the growth process, whether there are some deeper determinants that perhaps warrant scrutiny, etc.

A more specific example of tautological reasoning can be found in Robert Lucas' 1993 paper, "Making a Miracle".[9] This is essentially a follow-up

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the new classical macroeconomics, such as the use made of representative agent constructions, dynamic programming, the attempt to model in equilibrium terms what had hitherto often been taken to be disequilibrium phenomena, etc.). The difference may be a matter of the NGT not presenting the harsh policy conclusions that characterized the new classical macroeconomics, but on the contrary supporting standard neoclassical conclusions on optimal investment in knowledge.

[9] This example is from Matthias Kelm (1995, p. 18).

on his 1988 paper, and the made miracles he refer to are the East Asian economies. This motivates asking “How did it happen? Why did it happen in Korea and Taiwan, and not in the Philippines?” (Lucas 1993, p. 252). Lucas argues that accumulation of human capital is underneath the successful growth experience of Korea and Taiwan, and that the Philippines’ failure to sustain a human capital accumulation on the same level explains why it has lagged behind. Lucas then concludes by saying that his modeling efforts have “...the virtue of being consistent with the recent experience of both the Philippines and Korea. It would be equally consistent with post-1960 history with the roles of these two economies switched. It is a picture that is consistent with any individual small economy following the East Asian example” (Lucas 1993, p.271). As Kelm (1995, p.18) sarcastically points out, Lucas “...celebrates as a virtue his model’s failure to answer his own question: ‘Why in Korea and Taiwan, and not in the Philippines?’”.

Transaction Costs, Property Rights, etc. Both Weder and Grubel (1993) and Langlois and Robertson (1996) criticize the NGT on Coasean grounds. As we have seen, the stock of knowledge (whatever that means) is considered in the NGT to be a non-rival factor production; this is what creates the basis for dynamically increasing returns to scale. Knowledge spillovers ensure that what comes out of one research lab is instantaneously available to other research labs at no cost. However, as Weder and Grubel (1993) and Langlois and Robertson (1996) point out, in real economies there are numerous institutions that internalize these externalities, such as industrial associations and industrial clusters.

This means that implicitly the NGT assumes (in an unwarranted way?) the presence of some transaction costs that hinder the internalization of the relevant technological externalities. Clearly, in fact, the presence of those costs are crucial to the argument; otherwise there would not be any uncompensated knowledge spillovers. However, we should recognize that underneath all transaction cost problems are defective knowledge of some sort (Carl Dahlman 1979). But the implicit admission of transaction costs into the model is hard (if perhaps not impossible) to align with the fact that the NGT assumes that research labs have perfect absorptive capacity (they can immediately internalize the results of other labs).[10] More generally, if we stand ready to acknowledge that knowledge for exchange purposes is not perfect, then why assume that knowledge on the domain of production is perfect (Langlois and Foss 1996)?[11]

At any rate, it is apparent that the NGT simply does not make contact with the whole corpus of neo-institutional economics, with its emphasis on property rights, measurement problems, transaction costs, and how these translate into the institutional fabric of society and thereby crucially influence the wealth of nations (Rosenberg and Birdzell 1986; North 1990; Thráinn Eggertson 1990). At best these determinants are all hidden underneath specific functional forms, at worst the theorist simply doesn't consider them. Little wonder, then, that Douglass North can think of growth theory as "begging all the important questions" (as quoted in Langlois and Robertson 1996, p.24-25).

Now, from one perspective this is completely defensible, for we are here talking about different levels of analysis, and we cannot apriori condemn

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[10] Of course, this sort of inconsistency is very common in neoclassical economics, also in supposedly "rigorous" theory. For example, in standard, non-transaction cost price theory, the monopolist is assumed to sell at a single price and induce a deadweight welfare loss, when none of these phenomena can in fact take place unless some transaction costs are assumed to exist.

[11] Doing this would bring us into differential capabilities and a completely different microfoundation for growth theory. This theme will not be pursued here, but see Nelson (1994), Pack (1994), Langlois and Robertson (1996) and Thomsen (1996) for some stimulating discussions.

a theory for not incorporating a specific level of analysis. This is a view that Romer (1996, p. 203) clearly endorses:

> "What theories do is [to] take all the available complicated information about the world and organize it into [a] kind of hierarchical structure.
>
> In building this structure, good theory indicates how to carve a system at the joints. At each level, theory breaks a system down into a simple collection of subsystems that interact in a meaningful way".

This may be granted, as may Romer's accompanying statement that "...explanation operates on many levels that must be consistent with each other" (idem.).

What is noteworthy here is the emphasis on consistency between explanation that move on different levels of analysis. For there is, in fact, no guarantee that an approach to growth that begins from the individual transaction or the exchange of property rights will yield the same predictions or welfare conclusions as the much more aggregate NGT analyses. In the context of the general neoclassical property rights model (Thráinn Eggertson 1990), the NGT, because of its emphasis on knowledge externalities, is implicitly making *specific* assumptions about the enforcement and exchange of property rights, and therefore about transaction costs.

This is not *necessarily* to say that these assumptions are unwarranted. But it is to say that they need fuller justification and that there is a latent conflict between neoclassical (property rights) analysis conducted at a very low level of analysis and neoclassical analysis conducted at higher levels of analysis. For example, it is not at all clear that an analyst that began from the lower level of analysis would reach the same welfare conclusions (or frame the problem in the same way) as an analyst that began from a higher level.

Finally, if the NGT really is (as Romer (1996) argues) what we may call "*history-friendly*", in that it explicitly leans on economic history for inspiration, then why aren't key insights from economic history incorporated? We do know that a regime of well-defined, alienable and extensive property rights that are cast within the rule of law is not a bad recipe for creating growth. This is not the prejudices of a libertarian; it is an empirical fact (where is the counter-example?) that finds substantial support in theory (Peter Boettke 1995). Isn't *this* the sort of "stylized fact" that should serve as a foundation for growth theory?

Old hat? If Nelson (1994) is right, Abramovitz (1952) presented the rudiments – perhaps even the essential mechanisms – of the NGT. However, we can go further back in doctrinal history. As Langlois and Robertson (1996) point out, the idea of knowledge externalities as a motor of growth and dynamism was employed by Alfred Marshall. His often quoted analysis of the industrial district in *Principles* (1890) is a case in point, as is his major study on *Industry and Trade* (1923). In the Marshallian tradition, Allyn Young (1928) powerfully restated the classical theory of the progressive division of labor and placed increasing returns – from both pecuniary and technological externalities – centerstage in the growth process. And even earlier, the idiosyncratic, but occasionally brilliant, Thorstein Veblen (1914) had made knowledge accumulation central to the growth process.[12]

Seemingly, research markets are not fully efficient (as Stigler (1982) argues); significant ideas are not just smoothly absorbed in existing theory; the profession forgets. Or, so it would seem. For it is entirely likely that the profession knew of Marshall, Young and Veblen's ideas – but simply did not

[12] In fairness, it should be noted that contributors to the NGT are aware of the contributions of Marshall and Young.

know what to do with them, because existing methods did not allow the incorporation of the phenomena that these theorists had identified into the core of economic theory, namely general equilibrium. Of course, it is not just that Young and Veblen were outsiders – as they clearly were – relative to the neoclassical mainstream and its core of general equilibrium theory, for Solow, Nordhaus, Arrow and many others were critically aware of the importance of knowledge accumulation and knowledge externalities in the growth process. The difficulties must have been somewhere else.

One problem, of course, was to reconcile a notion of economies of scale – namely what Marshall called "external economies" – with competitive equilibrium. This problem was essentially solved by Masahiko Aoki in 1970, and his model is very close to the micro-foundation of Romer (1986). Later developments, notably by Dixit and Stiglitz (1977), helped put the theory of monopolistic competition on a rigorous footing and stimulated its application in macroeconomics, and later in growth theory. In the eyes of the profession, the charm of the latter approach was that macroeconomic modeling was no longer wedded to competitive equilibrium but could come to grips with real-world phenomena such as product diversity and internal economies of scale.

This suggest that while the NGT is in some substantive sense old hat, the role of more technical developments in the core of general equilibrium theory, and how these developments helped formalize older ideas (or, as formal economists say, "intuition"), is crucial to understanding the development and the success of the NGT. In the following section, I shall make an attemp to account for the apparent success of the NGT.

Accounting for the Growth of the NGT:
A Methodological Perspective

It is beyond dispute that the NGT has met with considerable success in the economics profession and is generally hailed as an important example of theoretical progress, arguably the most important, in the development of economics in the last decade. Given what has been said above, this is surely surprising: how can a theory that incorporates a significant amount of tautological reasoning, neglects almost completely those societal institutions which an impressive number of economists have seen as absolute essential to economic growth, and basically restates in formal terms older ideas, be so successful? In short, which factors account for the growth of the NGT?

In order to successfully address this question, we obviously have to turn to an analysis of the criteria that economists use to evaluate new theories, that is, to issues of methodology. Here, in fact, we are assisted by a large, and largely Anglo-Saxon, literature in the philosophy of science, often called "the growth of knowledge literature", which specifically addresses this issue, and which has been extensively used by economic methodologists. The key names in this literature are Karl Popper, Thomas Kuhn, Imre Lakatos and Larry Laudan, and I shall be concerned with the latter two here.[13]

In contrast to Popper and Kuhn, Lakatos (1970) distinguishes several levels at which scientific change may occur. To *Popper*, scientific change is a matter of the fate of bold conjectures that are formulated as individual theories: are they falsified? Or, are they corrobated, adding to our stock of scientific knowledge? To *Kuhn* scientific change is a matter of the fate of much more aggregate phenomena, what he calls "paradigms": do these scientific belief systems prosper or are they steadily accumulating anomalies that will eventually lead to their demise? The attraction of *Lakatos*' work is that he incorporates in his "methodology of scientific research programmes" both

[13] Lakatos' work in particular has attracted attention among economic methodologists, beginning with Latsis (1976). Hands (1985) is an excellent, critical discussion of Lakatos.

levels, that is, both the level of the individual theory and the more aggregate level. Moreover, while Popper's model of scientific change is essentially normative and Kuhn's is essentially descriptive, Lakatos' model has both a normative and a positive component.

The important organizing category in Lakatos' model is "*the scientific research program*", which is clearly a more detailed analogy to Kuhn's concept of paradigm. Specifically, it should be thought of as a series of theories that comprise a continuous because they share some so-called "*hard core propositions*". The research programme changes by modifying propositions in the "protective belt" (a "*heuristic*" informs the researcher about how this should legitimately be done), while keeping intact the hard core. This is so far a descriptive model of scientific activity. But Lakatos adds a normative dimension by introducing notions of *progression* and *degeneration*:

> "Let us take a series of theories, T_1, T_2, T_3,.... where each subsequent theory results from adding auxiliary clauses to...the previous theory in order to accommodate some anomaly, each theory having at least as much content as the unrefuted content of its predecessor. Let us say that such a series of theories is *theoretically progressive (or 'constitutes a theoretically progressive problemshift')* if each new theory has some excess empirical content over its predecessor...Let us say that a theoretically progressive series of theories is also *empirically progressive (or 'constitutes an empirically progressive problemshift')* if some of this excess empirical content is also corrobated...Finally, let us call a problemshift *progressive* if it is both theoretically and empirically progressive, and *degenerating* if it is not" (Lakatos 1970, p.118).

By "excess empirical content" is here meant that the relevant theory predicts some "*novel fact*" (this is the Popperian legacy that raises its head).

When research programmes degenerate, it is rational for the individual scholar to leave it in favor of some alternative and competing research programme, provided this has a better track record. This overall structure of reasoning – which has been only extremely briefly dealt with here – has been employed in numerous studies in economic methodology. How does it fare as an explanation of the breakthrough of the NGT?

It is rather immediately apparent that the NGT belongs to “the neoclassical research programme”: although proponents of the NGT fancy referring to Solowian growth theory as “neoclassical” in order to distinguish it from their own work (e.g., Romer 1994), there can be no doubt, of course, that the NGT is neoclassical and should be seen as a set of theories in a broader neoclassical research programme. The emphasis on casting all decision problem as maximization problems and casting all interaction among agents in equilibrium terms are what immediately qualifies the NGT as neoclassical. Specific NGT theories should be considered as belonging to the protective belt of the larger neoclassical research programme.

The problem, however, is that while the NGT may neatly be characterized as part of the neoclassical research programme, the Lakatosian approach does not appear to help us understand its success. This is essentially because the NGT does not predict any novel facts, not even relative to the background knowledge existing in the neoclassical research programme.[14] That knowledge accumulation is a crucial driver behind economic growth is, as

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[14] Henrik Lando has pointed out to me, however, that models based on the NG, but used for other purposes, have yielded novel insights. For example, it has recently been demonstrated that long-run neutrality of money does not necessarily obtain in such models. However, notice that 1) many Keynesians have consistently denied long-run neutrality, and 2) the “novel fact” in question could hardly have been anticipated by those who proposed the first NGT models ten or so years ago. More to the point, perhaps, Jan Fagerberg has pointed out that the NGT put forward at least one novel fact, namely that there need not be convergence between rates of growth among nations, and that this partly accounts for the success of the NGT.

previously mentioned, no new recognition at all. The welfare and policy conclusions of the NGT do not seem to imply any novel facts, either (as Romer (1994, p.21) admits).

We may quite sensibly argue that Lakatos' criteria for scientific criteria are simply too harsh. In fact, the Lakatosian philosopher, John Worrall (1978) long ago suggested that we modify Lakatos' notion of novelty and his criteria of acceptance of a theory in the following way:

> "The methodology of scientific research programmes regards a theory as supported by any fact, a 'correct' description of which it implies, provided the fact was not used in the construction of the theory" (Worrall 1978, p.50).

The problem here, however, is that it is precisely the case with the NGT that "the fact" is "used in the construction of the theory"; specifically, that the "fact" that knowledge is a motor of growth is both an assumption and a prediction in the theory. Therefore, on Lakatosian standards, the NGT would appear to be a degenerating theoretical development, which rational scientists should stay away from. But they don't. Clearly, something must be wrong here.

It would seem to be the case, therefore that Lakatos' model cannot help us understand the success of the NGT: the theory has met with success, but it essentially has very little new to say. Some other mechanisms must be at work. In my view, we obtain a better understanding of the issue under consideration here if we turn instead to the work of Larry Laudan (1977).

Like Lakatos and Kuhn, Laudan argues that scientific theories normally belong to some ongoing scientific tradition, but idea of the "research tradition" is more flexible than Kuhn's "paradigm" or Lakatos' "scientific research programme". For example, he drops Lakatos' idea of the hard core, in the sense of a set of core propositions that unite all theories in the program, in favor of an argument that research traditions are animated more by certain methodological and even metaphysical commitments. Thus, what makes a theory belong to, for example, the neoclassical research tradition is perhaps more the theoretical style with which the argument is constructed than the specific underlying assumptions.[15]

However, what is more important in the present context, Laudan supplies a concept of scientific progress that is considerably more flexible and realistic than Lakatos'. The key to understanding Laudan's view lies in his conception of scientific activity as continuous problem-solving relative to pre-defined motivating puzzles and questions, and situational constraints dictated by the research tradition within which the problem-solving efforts take place. It is the framing of puzzles and questions in the context of the constraints dictated by the research tradition that makes questions and puzzles into well-defined problems. Successful scientific activity consists in solving the puzzles by problem-solving that has been framed in this way, it is not a matter of putting forward "bold conjectures" or presenting "novel facts" *per se.*

According to Laudan, scientific problems come in two overall categories: empirical and theoretical. The latter category primarily refer to internal consistencies and shall not be further discussed here. The category of empirical problems, however, is directly relevant, for a research tradition confronts an empirical problem when its assumptions "forbid" the occurrence of a specific real-world phenomena. For example, economics for a long time "forbid" the existence of vertically integrated firms. That the modern theory of the firm is considered as an instance of theoretical progress is not a matter of its putting forward a new novel fact – firms have been known to exist for quite

[15] One may argue that the increased interest in bounded rationality in neoclassical economics in recent years is a case in point: Kreps, Hart, Aumann, Hahn and others are interested in bounded rationality because they see problems in maximizing rationality, but does this make them any less neoclassical?

some time – but a matter of its rationalizing a hitherto ill-understood empirical phenomenon, that is, solving an empirical problem by “assimilating” it under the neoclassical research tradition (for example, Sanford Grossman and Oliver Hart 1986).

This, then, is the explanation for the apparent success of the NGT, a theory that does not appear to predict any novel facts or incorporate key insights of other approaches to growth: the NGT successfully solved an empirical problem – endogenous technological change – and assimilated it into neoclassical economics, conforming to the constraints dictated by this research tradition. It thus demonstrated the continued viability of the neoclassical traditions and hindered a crisis situation provoked by an inability to come grips with an important empirical problem. The fact that growth and structural change would appear to have emerged as important problems on the agenda of economists at about the time of the emergence of the NGT further confirms that this is a case of theoretical progress in Laudan’s sense: not only did the NGT solve an empirical problem, it solved an *important* empirical problem.

This view of theoretical progress also means that it is not directly relevant (at least to the proponent of the NGT) to assess her work against the work of neoinstitutional or evolutionary economists. Her work should be assessed against what came before within the research tradition, specifically relative to the neoclassical predecessor, the Solow model. While the NGT does not present any “novel facts” in the Lakatosian sense, it does solve problems that could not be solved within the confines of the Solow model.

Is this scientific progress? In the eyes of most mainstream economists, it certainly is. What is their justification for thinking so? I my view, the implicit working methodology of many mainstream economists is not, as has often been asserted, Friedmanite instrumentalism (Milton Friedman 1953), but rather some sort of “modified essentialism” (Popper 1983), according to which the aim of science (economics) is to probe deeper and deeper into the structure of the (social) world. The realist idea of the aim of science as the uncovering of the workings of the mechanisms that operate underneath observable events really seems to enjoy a broad, but implicit, acceptance.

In such a view, scientific progress occurs when a theory (such as the NGT) is formed that provides a truthlike (and, we may add, more formal) description of a deeper essential layer (endogenous technological change) of the object (economic growth) than its predecessor (Solowian growth theory) (cf. Uskali Mäki 1991, p.87). This is not exactly the same as Laudan's perspective, but it would seem to be consistent with it.

From a new institutionalist (North 1990) or evolutionary (1982) perspective, the perspectives that have implicitly shaped the arguments of this paper, the NGT would only appear to constitute a case of scientific progress in the context of the neoclassical research programme. For proponents of these research programmes will maintain that they have uncovered even deeper essential layers – such as property rights institutions and firm capabilities – than have NGT economists. From such a perspective, the NGT hardly constitutes a case of significant scientific progress.

Concluding Comments

This paper has had both a substantial and a methodological orientation. The substantial contribution was to present a critique of the NGT. I here argued that the NGT is characterized by a certain tautological quality, that it suppresses what many economists would regard as the ultimate determinants of growth and growth differentials, namely institutions, and that it is largely a formalization of insights that have been well-known to economists for decades.

The methodological contribution was to use this critique to discuss alternative explanations of how economic theorists make choices with respect to the acceptance of new theories. I believe that my discussion is one further small nail in the coffin of Lakatosian methodology (if that is really needed after Hands 1985): the NGT has been successful, not because it presented any "novel facts", or because it was in any way "theoretically and empirically progressive", but because it carried further general equilibrium theory and demonstrated how this could be extended so that important empirical problems could be solved within the constraints dictated by the neoclassical research tradition.

An important final consideration here concerns the amount and allocation of research effort: the NGT did not prosper until the profession's attention had turned considerably away from macroeconomic issues and until this shift of interest released the required resources. This simple consideration suggests a final point that can only be tentatively presented here: economic methodology should pay much more attention to genuinely economic factors when accounting for theory choice, change of research programmes, etc. The economics of the allocation of research resources and which problems get top priority may be decisive for the evolution of economics. Instead of playing underlabourers to both philosophers and economists, economic methodologists should assert themselves as economists and to a much larger extent use economic tools in the analysis of the choices that theorists make.

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