Endogenous growth theory: The most recent "revolution" in economics?

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In his very interesting and provocative book, David Warsh (2006) offers that there has been a recent "revolution" in economic science, "a new economics of knowledge." While Warsh is sensitive to the history of economic thought and offers some critical insights into how it "progresses," a 1990 essay by Paul Romer is credited with precipitating this recent revolution. As Warsh writes: it was not until 1990, when "Romer published a mathematical model of economic growth in a mainstream journal that the economics of knowledge at last came into focus, after more than two centuries of informal and uneasy presence in the background" (2006: xv). But Warsh is ambivalent on the question of a whether there really has been a revolution. On the one hand, he has a chapter entitled "The Invisible Revolution" which seems to imply that few have noticed what should be a revolutionary shift in thinking. Moreover, as regards his "tale of how one paper in technical economics" precipitated "a new economics of knowledge," he asks, "What has changed as a result" and answers "not much—at least not yet" (Warsh, 2006: 408). We need to consider this provocative ambivalence in what follows. Part of the answer resides in considering the considerable continuity in the history of economic thought, and part of the answer resides in clarifying the "technical question" which Romer's paper addressed. This question regards solving a problem which first appeared in Adam Smith, runs throughout the history of economic thought and then recently surfaced as the problem of explaining growth "endogenously," that is, in a competitive market can a firm employ new knowledge to achieve increasing returns to scale?[1]

In Smith's famous pin factory example there was an unresolved contradiction: The specialization which comes with the division of labor allows for remarkable increases in productivity. But up to a point at least, the bigger the pin factory the greater the possibilities of specialization and thus of increased efficiency—technically, increasing returns to scale. But since larger firms can achieve a larger scale than smaller firms, there will be a tendency toward monopoly—as Marx had insisted. Smith's more famous metaphor, the invisible hand, however, requires many competitors in which no firm can achieve market control: In this condition, returns to scale will be diminishing rather than increasing. Since growth occurs all the time, how then to resolve this paradox?

We need a bit of history here, but my approach will be different than Warsh's. Warsh's history is strikingly whiggish, arguing that "economics makes progress over time," and that, fortunately, there is "technical economics—mathematical, empirical, free to make its own mistakes—and an indispensable guide to the modern world" (2006: 408). I am not convinced of this. My suggestion is that economics remains caught in a set of assumptions which not only serve enormously important ideological purposes, but also offers little help in understanding the modern world.[2] To see this, we need to notice that economics is a discipline which in fundamental ways has changed little since its early articulation, that

[1] In his review, Krugman (2006) expresses doubts both whether the Romer essay can bear the weight Warsh puts on it, and, more important, whether, "increasing returns really did transform our understanding of economic growth."

[2] The ideology is neo-liberalism, of course. For an insider's view, see Joseph E. Stiglitz, (2002). For a provocative history of economic analysis which shares much with my sketch, see Roncaglia, (2005).

despite a series of putative "revolutions" which greatly changed the character of economic analysis, the discipline has maintained what Schumpeter termed a "vision" of the economic process. This vision is well captured by Smith's powerful metaphor of the "invisible hand." Assuming what is sometimes termed "methodological individualism," market outcomes could be explained as the joint product of the actions of self-interested and more or less atomized persons interacting in society. The stunning feature of this interaction is that in a perfectly competitive economy, all this behavior adds up to order—indeed, to an efficient allocation of resources and perhaps also, to a just distribution of these. The idea that explaining outcomes required a focus on individuals was sound, even if Smith (and the other classicists) were unclear about what had to be assumed and even if some of the more obvious assumptions were obviously not true, including doubts about whether perfect competition could reasonably be assumed. That is, part of the power of the vision is the fact that it makes good sense to try to explain, for example, a market price in terms of the decisions of households and firms. But important problems arise in what is left out and in what is imputed to these actors, whether, for example, they are rational in the sense of the theory--whether they have the requisite information and capacities, and, for example, whether theory can ignore processes that may or may not enter into the constitution of markets. The strength of these problems is to throw doubt on the entire enterprise of mainstream economics.

So-called "classical theory" from Smith to J.S. Mill, gave way to "neo-classical" theory, constituting what is usually termed "the marginalist revolution." W.S. Jevons, Carl Menger and Leon Walras each quite independently arrived at the main ideas, the heart of what today is called "micro-economics." While the idea was implicit in the metaphor of the invisible hand, Walras, along with Pareto and then Pigou, specifically introduced into this body of theory the idea of general equilibrium—a condition in which the prices and quantities of all products and factors that would be bought, given pure competition, is completely determined. As an amplification of the invisible hand, this is also a powerful idea. On this view of the matter, what might seem to be a very messy process or, worse, a very messy set of processes turns out to be a fully understandable and rigorously theorized and generalized market process.

Still, as Schumpeter has argued, the "marginalist (neo-classical) revolution" was not a revolution if by that one means that the neo-classicists offered important changes in the fundamental assumptions of "classical" theory (Schumpeter 1954: 892-944; Chapter 7), or in the conclusions that the theory generated in terms of government policy. Rather, by generalizing the idea of the "marginal" to cover both production and consumption, these writers were able to develop a "model" of the economic process which was far superior, in terms of both its clarity and its power, to the one offered by the classicists. But the idea of "the invisible hand," the question of what had to be explained, and how it was to be explained was essentially the same as that of the classicists. (*Neo*-classical theory was indeed a *new* version of classical theory.) Thus, for example, "rational bchavior" could now be unpacked in terms of optimizing "marginal utility," and production decisions unpacked in terms of minimizing "opportunity costs. And despite Marshall's "glance" at descending cost curves, pure competition remained the taken-for-granted "normal" case (Schumpeter 1954: 892).[3]

[3] Neo-classical theory has never been without its critics. These begin, perhaps, with Durkheim (see Lukes, 1974), and in Germany with the *Methodenstreit*, conveniently dated from the 1893 publication of Carl Menger's *Untersuchungen über die Method de Sozialwissenshaften und der de Politischen Ökonomie insbesondere.* Weber, of course, played a key role, too often misunderstood. One then needs to include Thorstein Veblen and a long line "institutionalists," from John R. Commons to John Kenneth Galbraith to many contemporary "economic sociologists." Useful anthologies of essays by representative writers include: Etzioni and Lawrence 1991; Granoveter and Swedberg 1992; Swedberg 1993; Smelser and Swedberg 1994; Biggart 2002; Dobbin 2004. See also Dugger 1992. We exclude here any discussion of Marxist criticisms. See also the Progressive

One should note also that none of this model building required much mathematics. Some writers showed a preference for equations, but others did not. [4]

But with or without the mathematics, if one is seeking explanations, the fact that most of the assumptions of the model are acknowledged to be false is a serious problem. Thus, for example, the Bohr model of atom gives us an understanding of chemical outcomes just because we have good reason to believe that real world atoms are properly represented by the model.[5] The classic response to the problem of the reality of the model in economics was made by Milton Friedman in 1953. Taking an unequivocal position in an ongoing debate in the philosophy of science, he argued: "…theory is to be judged by the predictive power for the class of phenomena which it is intended to 'explain'" (1968: 512). As he says:

> …the relevant question to ask about the 'assumptions' of a theory is not whether they are descriptively 'realistic,' for they never are, but whether they are sufficiently good approximations for the purpose at hand. And this question can be answered only by seeing whether the theory works, which means whether or not it yields sufficiently accurate predictions (517).

On this test, it is hardly clear that neo-classical theory succeeds. But put this aside—at least for the moment. Predictive power does give a theory usefulness but can a theory whose assumptions are not "descriptively 'realistic' explain? Friedman is here assuming an account of explanation, sometimes termed "instrumentalist" such that explanation and prediction are symmetrical, that the capacity to predict gives *also* the capacity to explain—and conversely. The idea is widely taken for granted by economists. Indeed, as we notice momentarily, it is critical to the assumption that one needs a mathematical model if one is doing real science. Still, a moment's thought suggests that prediction and explanation are not symmetrical and that to explain, the model must be at least an approximation of reality. A good correlation, for example, gives one good predictive capacities, but much more is needed if we are to have an explanation. We need, for example, to know that the correlation is not a fluke, that there is a real causal mechanism at work. Indeed, the mechanism explains the correlation. For example, there is a strong correlation between exposing iron to salt air and rusting. We know why this happens precisely because molecular chemistry gives us an understanding of the (real!) properties of Fe and NaCl. Moreover, as seems plain, we are often in a position to explain when we could not have predicted. We easily explain the rusting once it has occurred, but to predict that it will occur we need to know also that nothing will prevent anticipated oxidation. Neo-classical theory is quite correct to seek

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Economics Forum (www.web.ca/~pef), the Post-Autistic Economic Review, pae_news@btinternet.com , and the writings of various "heterodox" economists, to be discussed below.

For some exceptional doubt offered by the discipline's most leading lights, see the AEA Presidential Addresses of Leontief 1971, Tobin 1972, and Solow 1980. Similar themes have been expressed by other notable insiders, for example, Thurow 1983, Balough 1982, Hirshman 1985, and Sen 1977.

[4] Schumpeter notes that "nobody denies that, numerous differences in detail notwithstanding, Jevons, Menger, and Walras taught essentially the same doctrine" and that "the most important differences in technique turned on the use or the refusal to use the calculus and systems of simultaneous equations" (952f.). See below.

[5] For a full fledged account, see my *A Realist Philosophy of Social Science* (Cambridge: Cambridge University Press, 2006).

mechanisms for market outcomes, but if the mechanisms are not true of the real world, then even if there is predictive power, we lack an explanation.[6]

A second "revolution" highly pertinent to the problem of explaining growth was said to occur in the 1930s with the publication of Joan Robinson's *The Economics of Imperfect Competition* (1933) and E.H. Chamberlin's *The Theory of Monopolistic Competition* (1933). It was a response to the criticism of the unreality of the model of pure competition. According to Brakman and Heijdra, Chamberlin's offered "a radical analysis" which was "the first to answer the question that was raised in 1926 by Sraffa: is it possible in a market characterized by monopolistic competition and declining average and marginal costs to reach an equilibrium?" (Brakman and Heijdra, 2004: 8). Notice first that Chamberlin's analysis (like Robinson's) was fully within the general equilibrium paradigm. That is, properties of "rational action" necessary to achieve equilibrium were all assumed, and, often unnoticed, Chamberlin also assumed free entry and exit of firms—a defining feature of pure competition. On his view, the trick to seeing the monopolistic elements of the competitive model –and hence the possibility of increasing returns to scale, was to notice that firms could introduce "product differentiation" --uniqueness of location, brand names, qualitative differences, etc. which alongside price competition (as in the pure competitive model) allowed for forms of non-price competition. This varying degree of monopoly, accordingly, allowed for increasing returns to scale.

Chamberlin noticed that this model gave a new and important role to advertising which in the model of pure competition could only be informational. He insisted that "selling costs"-- which could not appear in the pure case, must be distinguished from "production costs." Critically, the efforts of sellers alter the shape of the demand curve for a product. The implications of this are of enormous importance and, as noticed by Joan Robinson, include total rejection of the idea of "consumer sovereignty." On the pure competitive model, demand curves are outcomes of autonomous choices by rational consumers and suppliers are not in a position to manipulate demand.[7] But as Chamberlin noted, "…the art of the advertiser is akin to that of the hypnotist. Control of the buyer's consciousness must be gained, and while it is being gained additional expenditure yields increasing returns" (1956: 133).

[6] Most economists acknowledge that their assumptions are false, but take for granted that the model is justified in terms of its putative predictive value. But, sadly, neither do we have predictive power! Economists, like weathermen and stock market analysts, are never without an explanation of a failed prediction. On the other hand, and perhaps remarkably, even among economists, there is doubt as regards the capacity of theory to understand the real world. Davis (2004) concluded that "a majority of AEA members" who responded to a survey he conducted, admitted, "at least privately, that academic research mainly benefits academic researchers who use it to advance their own careers and that journal articles have little impact on our understanding of the real world and the practice of public policy" (359). See also the very pertinent comments by Samuelson , Romer and Pack, below. Warsh's book is full of wonderful insights into the sociology of academic economics suggesting that, following the work of Thomas Kuhn, the search for Truth is powerfully constrained institutionally.

[7] But this provokes the question, raised by Thorstein Veblen, of the rationality of the system: He wrote:

> The producers have been giving continually more attention to the saleability of the product, so that much of what appears on the books as production-cost should properly be charged to the production of saleable appearances. The distinction between the workmanship and salesmanship have been blurred in this way, until it will doubtless hold true now that the shop-cost of many articles produced for the market is mainly chargeable to the production of saleable appearances, ordinarily meretricious (quoted from Baran and Sweezy (1968: 133).

Indeed, if market capitalism is to be reproduced, new needs must constantly be created (Baran and Sweezy 1968; Galbraith 1968). As Schor put the matter, "consumerism is not an ahistorical trait of human nature, but a specific product of capitalism" (Schor 1992: 117).

The work of Robinson and Chamberlin represented a major forward step, but as Brakman and Hiejdra argue, "by the 1960's most (but not all) leading economists had come to the conclusion that the Chamberlin/Robinson revolution had essentially failed" (Brakman and Heijdra: 2). A number of reasons can be adduced which explain this.

First, it was hard to give up on the model of pure competition. In Chamberlin's terms, the invisible hand was less visible. Indeed, it might not be there at all! Second, Stigler, Friedman and others argued that the "predictions" were not very different than one's available in the pure competitive model.[8] Third, and very much related, as Schumpeter pointed out: once we allow for monopolistic elements through product differentiation, there is literally "an infinite variety of market patterns between pure or perfect monopoly and pure or perfect competition." This makes for a very, very messy world which is not obviously amenable to anything like the highly simplified model of pure competition (Schumpeter, 1954: 975). Indeed, Samuelson hit the nail squarely on the head when he wrote:

> If the real world displays the variety of behavior that the Chamberlin-Robinson models permit—and I believe that the Chicago writers are simply wrong in denying that these important empirical deviations exist—then reality will falsify *many* of the important qualitative and quantitative *predictions* of the competitive model. Hence by the pragmatic test of prediction adequacy, the perfect-competition model fails to be an adequate approximation…The fact that the Chamberlin-Robinson model is 'empty' in the sense of ruling out few empirical configurations and being capable of providing only formalistic descriptions, is not the slightest reason for abandoning it in favor of a 'full' model of the competitive type *if reality is similarly* 'empty' and 'non-full' (Samuelson, 1967, cited by Brakman and Heijdra, p. 11f.).

What, after all, is theory about, if not to give us an understanding of real world processes?

Brakman and Heijdra conclude with two additional observations which help to explain the failure of Chamberlin's work to revolutionize the discipline. First, the timing was bad. The Great Depression and Keynes "revolution" were of immediate concern. While Keynes's rejection of Say's law allowed him to show that one could have a stable equilibrium with less than full employment, Keynes, like Chamberlin (who nowhere appears in Keynes's work) was firmly within the paradigm of neo-classical theory. Indeed, throughout he assumed perfect competition. While the point cannot be pursued here, Samuelson's neo-Keynesian model managed to "'square the circle" "by means of 'wage stickiness' and 'the money illusion'" (Boettke 1997: 37). But here again the assumptions were not only ad hoc, but implausible, leaving the model vulnerable to the Chicago school's "hyperformalist attempt to purify the synthesis by purging it of its Keynesian contaminants" (38). The Neo-Liberalism of the recent past was the consequence. Boettke well summarizes matters:

> Samuelson's reconciliation of the micro-economic ideal type with involuntary unemployment was repudiated, along with Keynesian prescriptions, in favor of a view that there could be no involuntary unemployment , hence that government action was unnecessary. The result was a doctrinaire derivation of the laissez-faire conclusions that had been overturned by the formalist revolution; economics was now cleansed of

[8] The thrall of the instrumentalist model is clear here along with the relative absence of capacities to falsify on the basis of "predictions." As we shall see, these arguments recur as regards endogenous growth theory.

Keynesian impurities that had been introduced in the interest of realism (Boettke, 1997: 38). [9]

Brakman and Heijdra offered "perhaps a more important" reason for the failure of Chamberlin's effort to revolutionize mainstream theory. "… [P]erhaps more importantly, Chamberlin and coworkers failed to come up with a canonical model embodying the key elements of the theory. It was not so much Chamberlin's ideas that were rejected but rather his *modelling approach* that was deemed unworkable" (2). This says a great deal about economics as a science. The "formalist revolution" had by then occurred. Presumably,
in the absence of a mathematical model, the ideas could be ignored—even if they were essential to understanding economic reality. We would like to think that reality drives theory, but it surely seems here that we have good example of how fundamental assumptions, including assumptions about technique, are decisive. To see what is at issue as regards the construction of a "canonical model," we need here another bit of history.

Mathematics has long been a part of economic theorizing, but there is no agreement on dating the birth of mathematical economics partly because its essential features are contestable. Debreu offers that 1838, the publication of Augustin Cornot's *Researches sur les Principes Mathematiques de la Thèorie des Richesses* is a proper "symbolic date" for its birth. But as Schumpeter noted, the appearance of mathematics in an economic treatise does not make it mathematical economics. It may be that already familiar ideas can be represented by a mathematical term with nothing further at stake. So for example, "a production function" was merely the mathematical expression of the relationship between inputs and outputs in the same manner as it had been understood. Calculus could easily represent the mechanism which shows that firms pay wages equivalent to the marginal product of the worker. But, as Warsh notes, the mathematics conceals many assumptions. Indeed, Marshall was not alone in thinking that the mathematics might, indeed, *mislead* inquiry and easily could be avoided.[10]

Almost everybody agrees that there was significant break after World War II. Debreu (1984), for example, says that the work of von Neuman and Morgenstern in 1944 "announced a profound and extensive transformation of economic theory." This work was followed with work by Samuelson whose 1947 *Foundations of Economic Analysis* is usually credited with bringing together this new approach to economic model building.[11] In a mathematical economics, then, mathematics does not merely represent economic mechanisms mathematically; it becomes (in contrast to Marshall's advice) an engine of inquiry. Thus, one needs *also to* employ mathematics deductively, to generalize and integrate systematically the underlying processes.

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[9] Rational expectations theory also played a role. Human capital theory (invented by Gary Becker) was the effort to place labor back within the price-auction framework. See Thurow 1983: Chapter 7.Worth mention, perhaps, Keynes was not, in contrast to Samuelson, a formalist who was committed to mathematical economics. Keynes wanted models, but for him, building them required "a vigilant observation of the actual working of our system." Indeed, "to convert a model into a quantitative formula is to destroy its usefulness as an instrument of thought" (Keynes 1984). Of course, it was Samuelson's use of mathematics which allowed him to reconcile the micro economics of neoclassical theory with involuntary employment.

[10] Warsh (2006: 77) quotes Marshall's 1901 letter to Bowley:

(1) Use mathematics as a short hand language rather than as an engine of inquiry;
(2) Keep to them till you have done.
(3) Translate into English;
(4) Then illustrate by examples that are important in real life;
(5) Burn the mathematics;
(6) If you can't succeed in (4) burn (3).

[11] Worth mention, perhaps, Wassily Leontief and Joseph Schumpeter were early advocates of mathematics and came to change their minds.

Ideally, the model should be axiomatized so that assumptions can be clarified, theorems can be drawn, and consistency can be proven (Schumpeter: 1954: 954-55). As Warsh writes: "… instead of the unstructured , unnumbered, and insoluble series of equations that Walras had envisaged, Samuelson created a system strongly influenced by the new macroeconomics…No longer was it sufficient to say that everything depended upon everything else. Now it was necessary to break the economic world into subsystems and demonstrate how the big spending categories depended on one another" (119). The approach was quickly appropriated by the leaders of the new generation of economists. We need to emphasize here also that the mathematics in use, a mathematics that assumed linearity and convexity, was easily fitted to the neo-classical model of perfect competition, even if it meant ignoring features of the world which did not fit the mathematics.[12] That is, simultaneous equations of n variables were prefect to represent the ideas of general equilibrium.

An important feature of mathematical model building was on its side. I noted earlier that Friedman's classic defense of the problem of unrealistic model building assumed that explanation and prediction were symmetrical. This is a consequence of the account of explanation which dominated thinking in the philosophy of science for many decades. Sometimes called "the covering law" model or the D-N (Deductive/Nomological) model, it holds that explanation *is* deduction. That is, if the explanadum was an entailment of premises, then one had an explanation (period). In Hempel's classic formulation:

$$\begin{array}{l} C_1, C_2, \ldots C_k \\ \underline{L_1, L_2, \ldots L_r} \\ E \end{array}$$

The "explanans," C_1, $C_2,\ldots C_k$ are statements describing the particular facts invoked, sometimes called "the initial conditions," and L_1, L_2, $\ldots L_r$ are general laws. The event to be explained (the explanandum), E, is a logical consequence of the premise set.

To take the simplest case, if one has the "principle," "If you put salt in water, it dissolves," you can then predict that in particular instance, a consequence of putting salt in water will be that it will (probably) dissolve. Similarly, on this view, one can explain why some instance of salt dissolved by appeal to the same "law." Of course, it is part of the explanation of its dissolving that the salt was put into water. Science may well begin by identifying regularities in the world. But a scientific explanation does not stop with the discovery of "law-like" correlations. Rather, it comes with identifying the causal mechanisms at work such that, in this example, when salt is put in water, it tends to dissolve-- and not (say) to explode or turn the water to gin! Indeed, the covering law model obscures the critical role of model building in real science.

Formalization gives economists enormous deductive power. Unfortunately, as just the time that economics was achieving the status of a science which had the appearance of physics, philosophers were providing fatal objections to Hempel's deductive-nomological

[12] That is, it is not true (as asserted by, for example, Debreu (1984) that "commodity space has the structure of a real vector space"—a critical assumption for formalization. Mirowski (1991) relates the story of the shepherd who agreed to accept two sticks of tobacco for one sheep but became confused when given four sticks for a second sheep. For the economist, this shows that the shepherd does not understand arithmetic. But indeed, it shows that the economist does not know sheep! The example is especially pertinent to Chamberlin's argument regarding product differentiation. Krugman offers: "Economics understandably and inevitably follows the line of least resistance" (quoted by Warsh, p. 59). But Mirowski (1991) is correct in insisting that there was nothing inevitable about this process.

account of explanation.[13] Indeed, despite much talk to the contrary, no real theory in the physical sciences can be fully expressed as a deductive system, with axioms and deductions there from. Moreover, and critically, while for some theories, mathematics will play an important role, as noted, scientific theories explain by providing "an account of the constitution and behavior of those things whose interactions with each other are responsible for the manifested patterns of behavior" (Harre: 1970: 35). These scientific theories identify "things"—molecules and atoms, consumers and firms, how they are structured and how they interact with others. They are, of course, representations, but they are meant to represent reality—as it is in-itself (Manicas, 2006). Wassily Leontief well represents the problem of mathematical economics (and econometrics):

> Page after page of professional economic journals are filled with mathematical formulas leading the reader from sets of more or less plausible but entirely arbitrary assumptions to precisely stated but irrelevant theoretical conclusions…Year after year economic theorists continue to produce scores of mathematical models and to explore in great detail their formal properties; and the econometricians fit algebraic functions of all possible shapes to essentially the same sets of data without being able to advance, in any perceptible way, a systematic understanding of the structures and the operations of a real economic system (Leontief 1982: 104).

It might be supposed that even if this is true of those equations which represent the model of purely competitive markets, reality was restored with what Brakman and Heijdra call the "second monopolistic competition revolution," powerfully promoted in a 1977 paper by Avinash Dixit and Joseph Stiglitz. In contrast to the Chamberlin revolution, this revolution was a success—at least within the world of professional economicsts. As Brakman and Heijdra write: "the reason for [their] success is that Dixit and Stiglitz managed to formulate a canonical model of Chamberlinian monopolistic competition which is both easy to use and captures key aspects of Chamberlin's model." And even if one may argue over whether it left out "key aspects" of Chamberlin's mode, the Dixit/Stiglitz model, in contrast to Chamberlin's, was mathematical. In any case, Brakman and Heijdra note that while it is "somewhat unrealistic, it has nevertheless become the workhorse model" (12).

But indeed, the power of the idea of the "invisible hand" is nowhere clearer than in Stiglitz's acknowledgement of the consequences of one of the most typical and obviously unrealistic assumptions of their model. "The most crucial assumption" is that "all individuals are identical." To be sure, this assumption "simplifies the analysis." Unfortunately, there is a price to be paid: making this assumption "might give us a false sense of how well markets work" Indeed, "when the simplifying assumptions are dropped, it becomes apparent that the invisible hand is partly invisible because it is simply not there" (2004: 146). One may suspect that Leontieff had very much in mind a host of such unrealistic, but "workhorse models" inspired by Samuelson 1947-- including the influential 1977 paper by Dixit and Stiglitz.

This takes us, at last, to the featured player in Warsh's "tale." Endogenous growth models, following the lead of Romer (1990) are currently *de rigour*. But it is easy to show that while "imperfect competition" now has a place in mainstream theory, the new growth models are still well within mainstream theory and, accordingly, still very much lacking a significant touch to reality. A quick review of Romer will show this.

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[13] The critical literature is huge. For a review, see Manicas (2006), Chapter 1.

Romer's model has three major premises. First, technological change is at the heart of economic growth. Second, technical change is "endogenous"—it is explained in terms of "the intentional acts of individuals." More specifically, it is explained in terms of the familiar mechanism of neo-classical price theory, for example, that individuals are "rational" in the sense of neo-classical theory and respond to market incentives, in particular here, the competitive drive to reduce the costs of production. Third, technological change, for example, the design for a new product, or more generally "knowledge," involves non-rival, incompletely excludable goods. A non-rival good can be used by many others. A good is excludable if "the owner can prevent others from using it." The excludability, even if partial or temporary, of a non-rival good is essential as regards covering fixed costs of production and thus of increasing returns to scale.

The production process in the Romer formulation is described as a function F (A, X) where A is a non-rival input, for example, a production process that can be used in many settings, and X is a rival input, for example, physical or human capital, for example, the ability to add. Thus, "the ability to add is rivalrous because the person who possesses the ability cannot be in more than one place at the same time; nor can this person solve many problems at once." Like most economic goods, "human capital can be privately provided and traded in competitive markets" and is thus subject to rivalrous competition (S75). In Romer's model, there are four inputs: capital, labor, human capital and an index of the level of technology. If these are to function in the mathematics, of course, they must be quantifiable. Here one encounters all the usual problems with indexing.

Romer notes that prior to his work, "most models of aggregate growth, even those with spillovers or external effects, rely on price-taking behavior.[14] "But once these three premises are granted, it follows directly that an equilibrium with price taking cannot be supported" (S72). Moreover, all these efforts employed a mathematics which assumed convexity.[15] But because of its peculiar properties, nonrivalry needs to be represented by a mathematics of non-convex sets. Dixit and Stiglitz (1977) had provided the canonical mathematical model of monopolistic competition, and Romer had the mathematics to represent an equilibrium with non-rivalry as its key feature. (S71).

Like his predecessors in axiomatic mathematical theorizing, Romer's model operates at a very high level of abstraction and while he did overcome some of the limits of the older mathematics, he had to make a number of "simplifying assumptions" if the mathematics was to be manageable and produce results. One is that the population and supply of labor is constant, a second is that the "the total stock of human capital is fixed and that the fraction supplied to the market is also fixed" (S79). It is plain enough that neither of these are true. Other simplifying assumptions include "extreme assumptions on factor intensities"(*ibid*.) —not to be developed here, that "the output of designs is linear in each of H_A and A when the other is held constant" (S84), that "devoting more human capital to research leads to a higher rate of production of new designs," that "the higher the total stock of designs and knowledge is, the higher the productivity of an engineer working in the research sector will be" and finally,

[14] Price taking is the rule in perfect competition since neither buyers nor sellers can determine the price. In price-making markets, price is determined by sellers, oligopolists or monopolists. Roughly, spillovers are "externalities" which can be bad, e.g., pollution, or good, in particular here, the *unpaid* side effects of economic activity.

[15] The distinction, convex/non-convex, is a geometrical (topological) distinction which asks whether for any pair of points within an object, any point on the straight line segment that joins them is also within the object. In cutting edge mathematical economics, the calculus gave way to set theory/topology. Warsh identifies some of the key figures, Tjalling Koopmans, a physicist turned economist, Gerard Debreu, Kenneth Arrow and others.

but confusedly, that "the equilibrium here is based on the assumption that anyone engaged in research has free access to the entire stock of knowledge" (S83), that is, knowledge is non-rivalrous although the capacity to use it is. Several of these assumptions are merely simplifying, some are quite plausible, and several of them lead directly to policy: for example, expanding sums devoted to research should promote growth. The last, "free access," is puzzling because as Romer says, non-rival goods may be at least partly excludable. To protect their investment, firms do all that they can to exclude knowledge from potential competitors. But the assumption seems to be needed to assure an equilibrium solution.[16]

But having been thoroughly socialized into the paradigm of neo-classical theory, Romer also takes for granted a number of other assumptions which are even more critical. Thus, as already noted, actors are rational in the required sense. Labor markets are competitive, and there is still an equilibrium price of commodities since producers can determine their marginal costs and "the resulting monopoly price is a simple markup over marginal cost, where the markup is determined by the elasticity of demand" (S87)—however this is determined.

Warsh suggests that until Romer achieved a formalization, important ideas regarding the role of knowledge in growth economics could be ignored. But this says a great deal about the discipline of economics—noticed by Warsh but not pressed to its conclusion. On the present view of the matter, we have here an excellent example of how method stands in the way of gaining understanding. The "invisible hand" idea was powerful, no doubt, and it easy enough to see how it came to dominate nineteenth century thinking about markets. And the subsequent formalizations were indeed elegant, beautifully legitimated by a philosophy of science which though false, remained dominant (Manicas, 1989). One would need good reason, presumably, to abandon either the idea that one could explain economic outcomes in terms of the interactions of atomized, rational and self-interested persons or the highly sophisticated mathematical model building approaches which rested on these assumptions.

The equations in Romer's paper give a rigor to the argument and allow for strict deductions from premises. But as is clear enough, their relation to reality depends upon the reality of assumptions which are required by the formalism. But perhaps, as regards the revolution in economic theory, this is not the worst of it. The augmented Solow model has not been driven from the field. Its defenders not unreasonably insist that the competitive model, suitably modified, would still be preferable to the Romer model. Indeed, given its set of special assumptions, why bring in a shaky monopoly model when so much of the traditional model has served its purposes? Echoing Friedman, as Mankiw noted, "the issue at hand is not whether the neo-classical model is exactly true… The issue is whether the model can even come close to making sense of international experience" (Quoted by Warsh, p. 274).

Romer was himself fully aware of these problems. In his perhaps even more important 1994 essay, "The Origins of Endogenous Growth," Romer tells two "stories," "equivalent to creation myths, simply stories that economists tell themselves and each other to give meaning and structure to their current research efforts" (3). The first "story" regards "the convergence controversy," "whether per capita income in different countries is converging" (4). His conclusion is very important. "[I]f you are committed to the neo-classical

[16] According to Warsh, Joseph Stiglitz was irritated by the Romer model. "We knew how to construct models that 'worked' but we felt uneasy making these special assumptions" (Quoted by Warsh, p. 301). There may be some irony is this since Dixit and Stiglitz, of course, shared in accepting the "non-special" but manifestly untrue assumptions of the competitive model.

mode, the kind of data in Figures 1 and 2 cannot be used to make you recant." Of what kind is this data? They are regressions of cross-cultural data.[17] The problem is precisely that "many different inferences [from the competing models] are consistent with the same regression facts" (10). Worse, with suitable assumptions about evidence and the tasks of theory, "we can thereby enshrine the economic orthodoxy and make it invulnerable to challenge" (20). As suggested in the foregoing, it is perhaps *already* invulnerable to challenge.

For Romer the convergence controversy was a wasteful divergence since it took attention away from the real causes of growth—a divergence he admits contributing to in his 1987 essay, "Crazy Explanations for the Productivity Slowdown." He says: "Looking back, I suspect that I made this shift toward capital and away from knowledge partly in an attempt to conform to the norms of what constituted convincing empirical work in macroeconomics." "If you want to run regressions, investment in physical capital is a variable that you can use, so use it I did" (20). But indeed, if one wants to evaluate the competing theories of growth, Lucas's observation (1988) "that people with human capital migrate from places where it is scarce to [a] place where it is abundant, is as powerful a piece of evidence as all the cross-country regressions combined" (19). To be sure, this sounds like a healthy infusion of sociology and history.

Indeed, on Warsh's account, Romer seems to have himself despaired of the limits of mathematical economics, despite his early enthusiasm, and has turned his attention as an entrepreneur to "how to educate people in a world of global competition." His 1994 paper, "New Goods, Old Theory, and the Welfare Costs of Trade Restrictions" suggests a powerful reason. He wrote: "[I]n our post-WWII enthusiasm for distilling 'the miracle of the market' down to its mathematical essence, economists have generally been willing to push issues aside. Decentralized markets could be shown to get everything right but only by assuming that half of our economic problem…had already been solved" (Quoted by Warsh, p. 326).

The problem here is the very idea of "equilibrium." With evident acknowledgement of critics of general equilibrium theory—Romer offered that only a sparse amount of goods exist and thus that genuinely new goods are always added.[18] There may well be no equilibrium since entrepreneurs are always seeking to upset the conditions that are posited to generate it. This was long ago recognized by business school professionals who argue that micro-economic models are useless for business decision-making. For example, "market models admit time considerations only in a limited and contrived manner...But investment represents the concern of major executives, rather than clerks, for the very reason that markets are dynamic and are buffeted by many forces that vary over time...In other words, executives who are estimating of the pattern of revenues and costs over the life of an investment--and the length of its life--get relatively little help from market models of price theory" (Oxenfeld (ed.)

[17] The problem becomes a defense of the competitive model in the hands of Howard Pack (1994). He asks, "But have recent theoretical insights succeeded in providing a better guide to explaining actual growth experience than the neo-classical model?" and answers: "This is doubtful." (55). For him, not only it difficult to use aggregate data to distinguish between standard neo-classical theory and endogenous growth theory, but "most of the empirical work has utilized observations across countries and imposed extremely strong assumptions about international production functions" (*ibid*.).

[18] As Hayek insisted: "[E]conomic theory sometimes appears at the outset to bar its way to a true appreciation of the character of the process of competition because it starts from the assumption of 'given' supply of scarce goods. But which goods are scarce, of which things are goods, and how scarce or valuable they are—these are precisely the things which competition has to discover" ("Competition as a Discovery Procedure," cited by Paul Lewis, 2006). Critical here is the idea that knowledge is distributed and not anybody can have it all. Markets offer a solution to this.

1963: 63). Indeed, as Hayek and the Austrians have insisted for some time, the problem of knowledge far outreaches the rather limited role that it plays in endogenous growth theory (Lewis 2006).

It is therefore no mistake, as pointed out by William Baumol, that "ideas of entrepreneurship, institutions, property rights, and freedom have almost no place in the textbooks of core classes and industrial organization classes" (*Economic Journal Watch*, January 2006).[19] Baumol provided two powerful reasons: First, "entrepreneurial activities do not incorporate the homogeneous elements that lend themselves to formal mathematical description" and second, as suggested by Romer, "equilibrium models exclude the entrepreneur by their very nature…[S]ustained equilibrium," as insisted by Schumpeter and the Hayekians, " is something that the entrepreneur does not tolerate" --exactly because entrepreneurs are constantly seeking genuinely new goods. [20]

Indeed, this takes us back to the vision laid out by Smith and the classicists. I noted that there is good sense to trying to explain economic outcomes in terms of the decisions of the relevant actors. But actors cannot be restricted to "firms" and "households." And more important, they need to be situated. Contrary to the assumptions of neo-classical theory, individuals are not identical: They have very different values, aims and goals, and more important, very different capacities and powers—critically a function of their place in social relations. CEOs of corporations are in a situation which is very different from the situation of a family run Chinese restaurant; unionized workers are not in the same situation as immigrant farm workers—one can easily go on here. Accordingly, not only are markets concretely different—exhibiting varying degrees of monopoly, but as part if this, market outcomes are also shaped by decisions of governments, unions and certifying boards, among others.[21] Unfortunately, reality *is* messy. Indeed, as Schumpeter and the Hayekians see, it is too messy for the inevitably static models of general equilibrium theory –whether or not it seeks to move away from the assumptions of pure competition (Schumpeter, 1954: 1160-61; Herrara, 2006: 249).

So, what of Warsh's question: "What has changed as a result?" and his answer "not much—at least not yet." First, it is still not clear whether the new models of imperfect competition will displace the pure competitive model or whether the new growth models can indeed make sense of growth—as raised by Krugman, among others. One may doubt both. One might hope, optimistically, that at least the new models do thwart much of the ideological core of neo-liberalism. Perhaps, its ideological hold on the economics profession may be on

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[19] This was established in a study by Dan Johannson in EJW (2004). The socialization of mathematical economists begins in the introductory course. As regards public thinking on economic matters, Samuelson wisely pointed out: "I don't care who writes a nation's laws—or crafts its advanced treatises—if I can write its economic textbooks" (Quoted by Warsh, p. 384).

[20] But Baumol is nevertheless unwilling to give up general equilibrium theory. On his account, it is dealing with subjects for which the entrepreneur is irrelevant! "Static analysis has offered many valuable insights and its body of theory is an admirable accomplishment" (p. 135). Similarly, economists cannot leave to historians the "greatness mystery" that we face: why have "the relatively free economies in the past two centuries been able to outstrip…the performance in terms of growth and innovation, of all other forms of economic organization?" (*ibid*.). But as the Austrians insisted, *dis*equilibrium prices, not equilibrium prices are the key since they alert entrepreneurs to new profit opportunities. See Kirzner 1984.

One should emphasize here that the problem is not whether or not markets do function to allocate, distribute, and promote innovation, but whether general equilibrium theory provides an adequate explanation of outcomes. See my 2006*,* Chapter 6 and Appendix D.

[21] In addition to my 2006, see also Boettke (1997), Lawson (1997), Lewis (2004, 2006).

the wane. Still, even if the pure competition model is replaced, one must assume that growth (and other critical issues) can be explained within the framework of general equilibrium theory. Not only may this be doubted, but, worse, it may well be that because perfect competition has been compromised, the new growth paradigm will conquer genuinely competing heterodox alternatives. The problem with Warsh's account is just that he does not see the extent to which endogenous growth theory is well within the neo-classical paradigm, that there are deep criticisms of that paradigm, and even more important, that there are genuine –revolutionary—alternatives. If, then, these alternative accounts—still very much in the making--can provide the best hope for an understanding of growth--and many other features of capitalist market societies, and they get buried, the new growth theory will have made another "revolution" which sustains a considerable portion of Adam Smith's vision of the invisible hand. [22]

References

Baran, Paul A. and Sweezy, Paul M. 1968. *Monopoly Capital: An Essay on the American Economic and Social Order.* New York: Monthly Review Press.

Balough, Thomas 1982. *The Irrelevance of Conventional Economics.* New York: Liveright.

Baumol, William J. 2006. "Textbook Entrepreneurship: Comment on Johansson,"*Econ Journal Watch*, Vol. 3, No. 1.

Biggart, Nicole Wesley 2002. *Readings in Economic Sociology*. Oxford: Blackwell

Boettke, Peter J. 1997. "Where Did Economics Go Wrong? Modern Economics as Flight from Reality," *Critical Review,* Vol. 12.

Brakman, Steven and Heijdra, Ben J. (eds.) 2001.*The Monopolistic Competition Revolution in Retrospect*. Cambridge: Cambridge University Press.

Chamberlin, E.H. 1962 [1932]. *Theory of Monopolistic Competition: A Re-Orientation of the Theory of Value.* 8th Edition. Cambridge, Ma.: Harvard University Press.

Davis, William L. 2004. "Preference Falsification in the Economics Profession," *Economic Journal Watch*, Vol. 1.

Debreu, Gerard 1984. "Economic Theory in the Mathematical Mode," *American Economic Review*, Vol. 74, No. 3.

Dobbin, Frank (ed.) 2004. *The New Economic Sociology: A Reader.* Princeton: Princeton University Press.

Dixit, Avinach K. and Stiglitz, Joseph E. 1977. "Monopolistic Competititon and Optimum Product Diversity, *American Economic Review,* Vol, 67, No. 3.

Dugger, William M. 1992. *Underground Economics: A Decade of Institutionalist Dissent*. Armonk: M.E.Sharpe.

Dyck, Charles F. 1981. *Philosophy of Economics*. Englewood Cliffs: Prentice-Hall.

Etzioni, Amitai and Lawrence, Paul R. 1991. *Socio-Economics: Toward a New Synthesis*. Armonk: M.E. Sharpe.

Friedman, Milton 1968 [1953]. "The Methodology of Positivist Economics," in *Essays in Positivist Economics*. Chicago: University of Chicago Press.

Galbraith, John Kenneth 1967. *The New Industrial State*. Boston: Houghton Mifflin.

Granoveter, Mark and Swedburg, Richard 1992. *The Sociology of Economic Life* Boulder: Westview.

Granoveter, Mark and Tilly, Charles 1988. "Inequality and Labor Process," in Smelser, Neil (ed.). *Handbook of Sociology* Beverly Hills, Ca.: Sage.

Harré, Rom 1970. *Principles of Scientific Thinking*. Chicago: University of Chicago Press.

Hayek, F.A. 1978. *New Studies in Philosophy, Politics, Economics and the History of Ideas*. Chicago: University of Chicago Press.

[22] See Herrera's excellent overview (2006). As he points out, "the endogenous growth models are not *neutral*: their endogenization means *marketization* (252).

Herrera, Rémy 2006. “The Hidden Face of Endogenous Growth theory: Analytical and Ideological Perspectives in the Era of Neoliberal Globalization,” *Review of Radical Political Economics*, Vol 38, No. 2, Spring).

Hirshman, A.O. 1985. "Against Parsimony," *Economics and Philosophy* Vol. I .

Johansson, Dan 2004. “Economics Without Entrepreneurship or Institutions: A Vocabulary Analysis of Graduate Textbooks,” *Econ Journal Watch*, Vol. 1, No. 3.

Kirzner, Israel M. 1984. “Prices, the Communication of Knowledge and the Discovery Process,” in

Leube, K.R. and Slabinger, A.H. (eds.), *The Political Economy of Freedom: Essays in Honor of F.A. Hayek,* Munich: Philosophia Verlag.

Krugman, Paul 2006. “The Pin Factory Mystery,” *New York Times*, May 7.

Lawson, Tony 1997. *Economics and Reality*. London: Routledge.

Lazonik, William 1991. *Business Organization and the Myth of the Market Economy*. Cambridge: Cambridge University Press.

Leontief, Wassily 1982. Letter in *Science* 217.

Lewis, Paul 2004. “Structure and Agency in Economic Analysis: The Case of Austrian Economics and the Material Embeddedness of Socio-Economic Life,” in Davis, J.B., Marciano, A., and Runde, J.H. (eds.), *The Elgar Companion in Economics and Philosophy*. Cheltanham: Edward Elgar.

Lewis, Paul 2006. “Hayek: From Economics as Equilibrium Analysis to Economics as Social Theory,” in Barry, N.P. *The Elgar Companion to Hayek*. Cheltanham: Edward Elgar.

Lukes, Steven 1972. *Emile Durkheim: His Life and Work.* New York: Harper and Row.

Manicas, Peter T. 1989. *A History and Philosophy of the Social Sciences*. Oxford: Basil Blackwell,

Manicas, Peter T. 2006. *A Realist Philosophy of Social Science: Understanding and Explanation.* Cambridge: Cambridge University Press.

Mirowski, Phillip 1991. "The When, the How and the Why of Mathematical Expression in the History of Economic Analysis, *The Journal of Economic Perspectives*, Vol. 5, No. 1.

Oxenfeld, Alfred R. (ed.) 1963. *Models of Markets.* New York: Columbia University Press. Pack, Howard 1994. “Endogenous Growth Theory: Intellectual Appeal and Empirical Shortcomings,” *Journal of Economic Perspectives*, Vol. 8, No. 1.

Robinson, Joan 1969, 2nd Edition. *The Economics of Imperfect Competition*. London: Macmillan.

Romer, Paul M. 1987. “Crazy Explanations for The Productivity Slowdown,” in Fischer, S (ed), *NBER Macroeconomics Annual.* Cambridge: MIT Press.

Romer, Paul M. 1990. “Endogenous Technological Change,” *Journal of Political Economy,* Vol, 98.

Romer, Paul M. 1994. “The Origins of Endogenous Growth,” *The Journal of Economic Perspectives,* Vol. 8, No,. 1.

Romer, Paul M. 1994b. “New Goods, Old Theory, and the Welfare Costs of Trade Restrictions,” *Journal of Developmental Economics*, Vol. 43, No, 2

Roncaglia, Alexander 2005. *The Wealth of Ideas: A History of Economic Thought* (New York: Cambridge University Press, 2005).

Samuelson, Paul 1947. *Foundations of Economic Analysis* (Cambridge, MA.: Harvard University Press.

Schor, Juliet B. 1992. *The Overworked American* . New York: Basic Books.

Schor, Juliet B. 1999. *The Overspent American: Why We Want What We Don't Need.* New York: Perseus Books.

Schumpeter, Joseph A. 1954. *History of Economic Analysis*. New York: Oxford University Press.

Sen, Amartya K. 1977. "Rational Fools: A Critique of the Behavioral Foundations of Economic Theory," *Philosophy and Public Affairs,* Vol. 6.

Smelser, Neil and Swedberg, Richard (eds) 1994. *The Handbook of Economic Sociology* Princeton: Princeton University Press.

Solow, Robert 1980. "On Theories of Unemployment," *American Economic Review,* Vol. 70, No. 1.

Solow, Robert 1988.“Growth Theory and After,” *American Economic Review,* Vol 78, No. 3.

Stiglitz, Joseph 2004.“ Reflections On the State of Monopolistic Competition,” in Brakman and Heijdra (2004).

Stiglitz, Joseph 2002. *Globalization and Its Discontents.* New York: W.W. Norton.

Swedberg, Richard 1993. *Explorations in Economic Sociology*. New York: Sage.

Thurow, Lester 1983. *Dangerous Currents*. New York: Random House.

Tobin, James 1972. "Inflation and Unemployment," *American Economics Review*, Vol. 62.

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