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Author(s): Robert M. Solow

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Perspectives on Growth Theory

Robert M. Solow

The current wildfire revival of interest in growth theory was touched off by articles from Romer (1986, from his 1983 thesis) and Lucas (1988, from his 1985 Marshall Lectures). This boom shows no signs of petering out. The time is not yet ripe for stock-taking and evaluation. My goal is not nearly so ambitious. All I want to do is to place the new thinking in some sort of historical perspective, and perhaps sprinkle a few idiosyncratic judgments along the way.

There have been three waves of interest in growth theory during the past 50 years or so. The first was associated with the work of Harrod (1948) and Domar (1947); Harrod's greater obscurity attracted more attention at the time (and earlier, in 1939), although Domar's way of looking at things is more relevant to some of the current ideas.¹ The second wave was the development of the neoclassical model. I think—probably inevitably—that some misconceptions remain about what that was all about, and why. The third wave began as a reaction to omissions and deficiencies in the neoclassical model, but now generates its own alternation of questions and answers.

The Harrod-Domar Impulse

Suppose aggregate output is for some reason—technological or any other—proportional to the stock of (physical) capital. There is a warrant for this in

¹Harrod's exposition tended to rest on incompletely specified behavioral and expectational hypotheses. Domar focussed more straightforwardly on the requirements for equilibrium of demand and supply in steady growth.

■ *Robert M. Solow is Professor of Economics, Massachusetts Institute of Technology, Cambridge, Massachusetts.*

the almost-trendlessness of the observed ratio. Suppose that realized saving and investment (net, for simplicity) is proportional to output and income. There is similar warrant for this assumption. It follows that investment is proportional to the stock of capital, and this fixes the trend rate of growth of both capital and output, unless the rate of capacity utilization is allowed to go wild. That rate of growth is the product of the investment-output ratio and the output-capital ratio. If we think entirely in *ex post* terms, the saving-income ratio and the investment-output ratio are the same thing. One of the defining characteristics of growth theory as a branch of macroeconomics is that it tends to ignore all the difficult economics that is papered over by that sentence.

Now suppose that the required labor input per unit of output is falling at the rate $\underline{m}$ (which is to say that labor productivity is rising at the rate $\underline{m}$), again for whatever reason. If the labor force is increasing at the rate $\underline{n}$, a sort of impasse arises. Total output must grow at the rate $m + n$ on average, or else the unemployment rate will rise indefinitely (if output growth is too slow) or the economy will run out of labor (if growth is too fast). But we have just seen that the growth rate must satisfy a quite independent condition: it must be equal to the product of the saving-investment quota ($\underline{s}$) and the output-capital ratio ($\underline{a}$). The two conditions can be reconciled only if $sa = m + n$. But there is no reason why this should ever happen, because the four parameters come from four wholly unrelated sources.

This construction seemed to have two unpalatable consequences. The first is that observed economies should spend most of their time experiencing either prolonged episodes of increasing or falling unemployment rates and/or prolonged periods of rising or falling capacity utilization. There is no reason to expect these movements to be confined to minor business-cycle dimensions or to be quickly reversed. But that is not what the record of the main capitalist economies looks like.

The second apparent consequence is this. Suppose the first problem can be evaded. This might happen, for instance, in a developing country with a large pool of rural labor. It could then have an industrial labor force growing at whatever the required rate, $sa - m$, happens to be; the consequences of a mismatch would be seen only in the waxing or waning of the rural population. Such an economy could jack up its long-term rate of industrial growth merely by increasing its investment quota. Under the influence of this model, that policy was sometimes prescribed. It makes general sense. But if economic development were that easy, it would be hard to understand why more poor countries did not follow that route to rapid growth. Even rich countries would surely want to take advantage of this possibility sometimes. Something seems to be wrong with this way of looking at long-run economic growth.

The straightforward way to avoid the first of these awkward conclusions is to recognize that at least one of the four underlying parameters is likely to be endogenous. Then the condition $sa = m + n$ may have a solution most or all of the time; and there may be a plausible adjustment process that will realize the

solution and allow uninterrupted growth to take place. Obviously the investment-income ratio quota $\underline{s}$ and the output-capital ratio $\underline{a}$ are the natural candidates for endogeneity.² Nicholas Kaldor (1956) and others tried to use $\underline{s}$ in this way, usually by emphasizing its interpretation as a saving rate, introducing different saving rates applying to different categories of income, especially wages and profits, and then focussing on changes in the functional distribution of income as the mechanism causing the aggregate saving rate to vary endogenously. (Bertola, 1992, is an interesting modern treatment of this line of thought.) It is fair to say that this way of resolving the problem did not catch on, partly for empirical reasons and partly because the mechanism seemed to require that factor prices be completely divorced from productivity considerations.

The Neoclassical Response

The standard neoclassical model, of course, resolves the problem by making the output-capital ratio a the endogenous variable. Then labor productivity growth m will have an endogenous component too, as capital-intensity changes; but there may remain an exogenous component, loosely identified as technological progress. This has several related advantages. It fits in well with the rest of economics; the possibility of increasing the output-capital ratio by substituting labor for capital is a comfortable and sensible device, especially on a longish time scale. The implied adjustment mechanism is plausible and familiar. If $sa - m > n$, so that labor is getting scarce relative to capital, one might naturally expect the wage-rental ratio to rise; cost-minimizing firms would naturally substitute capital for labor. The output-capital ratio would fall and the economy would move closer to satisfying the consistency condition. Similarly in reverse. (There the habit of ignoring aggregate-demand considerations might grate a little. In periods of high unemployment firms face weak product markets; lower wages could make things worse.) The assumptions about diminishing returns that are required to make this mechanism work come easily to most economists. Substitution along isoquants is routine stuff. That does not count as evidence in favor of the traditional assumptions, but it explains why the model feels comfortable to economists. Besides, there is quite a bit of evidence to support

²In principle there is no reason to exclude the endogeneity of m and n . But induced changes in population growth, although an important matter in economic development, seemed not to figure essentially in the rich countries for which these models were devised. The idea of endogenous technological progress was never far below the surface. In those days it would have seemed rash to conjure up some simple connection between the allocation of resources and the *rate of growth* of productivity. Kaldor and Mirrlees' "technical progress function" (1962) was an attempt that apparently did not seem plausible. I would recommend Karl Shell's papers (1966, 1967, 1973) as an indication of how far a technically-sophisticated and well-read economist of the time would have been willing to go. There has been some progress since those papers, but not a whole lot.

the traditional assumptions, considerably more than there is in the opposite direction.

Notice that I have not mentioned constant returns to scale. That is because the model can get along perfectly well without constant returns to scale. The occasional expression of belief to the contrary is just a misconception. The assumption of constant returns to scale is a considerable simplification, both because it saves a dimension by allowing the whole analysis to be conducted in terms of ratios and because it permits the further simplification that the basic market-form is competitive. But it is not essential to the working of the model nor even overwhelmingly useful in an age of cheap computer simulation.

Everybody knows that fixing up the first awkward implication in this way (the implication that economies should be experiencing prolonged swings in unemployment and capacity utilization) also takes care of the second awkward implication (that growth by raising an investment quota seems somehow too easy an approach). Diminishing returns to capital implies that the long-run rate of growth is completely independent of the saving-investment quota. A closed economy that manages to raise or lower the fraction of output invested, and sticks to the program, will experience a rise or fall in its aggregate rate of growth, but only temporarily. Eventually the rate of growth relapses back to its long-run value. This underlying rate of growth is the sum of n and the technological-progress component of m . The only permanent effect of the maintained change in investment will be an upward or downward shift in the level of the trend path, but not in its slope. Increasing the rate of *per capita* growth is not only not easy in this model, it is impossible unless the rate of technological progress can be altered deliberately.

This reversal of conclusions has led to a criticism of the neoclassical model: it is a theory of growth that leaves the main factor in economic growth unexplained. There is some truth in that observation, but also some residual misconception. First of all, to say that the rate of technological progress is exogenous is not to say that it is either constant, or utterly erratic, or always mysterious. One could expect the rate of technological progress to increase or decrease from time to time. Such an event has no explanation within the model, and may have no apparent explanation at all. Or else it might be entirely understandable in some reasonable but after-the-fact way, only not as a systematic part of the model itself.

Secondly, no one could ever have intended to deny that technological progress is at least partially endogenous to the economy. Valuable resources are used up in pursuit of innovation, presumably with some rational hope of financial success. The patent system is intended to solidify that hope, and thus attract more resources into the search for new products and precesses. It would be very odd indeed if all that activity had nothing to do with the actual achievement of technological progress. The question is whether one has anything useful to say about the process, in a form that can be made part of an aggregative growth model. I will suggest later on that this is probably the most

promising aspect of the current third wave of growth theory, even if much that has been written on the subject so far seems simplistic and unconvincing.

Newer Alternatives

The direction taken at first by the newer growth-theoretic models was not toward a direct approach to the economics of technological progress. It was something much simpler: a straightforward abandonment of the idea of diminishing returns to “capital” (now interpreted as the whole collection of accumulatable factors of production, one of which might be labelled human capital or even the stock of knowledge). This stage of the revival could be described as a return to generalized Domar, but with sophisticated bells and whistles. Among the bells and whistles were allowance for substitutability between capital and labor and between various forms of capital, allowance for only asymptotic absence of diminishing returns, the adoption of a representative-agent set-up with infinite-horizon intertemporal optimization to determine investment (in everything), and the introduction of monopolistic competition as the underlying market form.

Here I would like to interject two comments. The modelling of imperfect competition was made necessary by the appearance of increasing returns to scale. I have already mentioned that the presence of increasing returns to scale is not the essence of these newer approaches. It is perfectly possible to have increasing returns to scale and preserve all the standard neoclassical results. What is essential is the assumption of constant returns to capital. The presence of increasing returns to scale is then inevitable, because otherwise the assumption of constant returns to capital would imply negative marginal productivity for non-capital factors. Anyway, I register the opinion that the incorporation of monopolistic competition into growth theory is an unambiguously good thing, for which the new growth theory can take a bow (along with a derived curtsy to Dixit and Stiglitz).

I cannot say the same about the use made of the intertemporally-optimizing representative agent. Maybe I reveal myself merely as old-fashioned, but I see no redeeming social value in using this construction, which Ramsey intended as a representation of the decision-making of an idealized policy-maker, as if it were a descriptive model of an industrial capitalist economy. It adds little or nothing to the story anyway, while encumbering it with unnecessary implausibilities and complexities.

Now I return to the question of constant returns to capital. It may not be generally recognized how restrictive this assumption is. There is no tolerance for deviation. Lucas emphasized in his 1988 article that a touch of diminishing returns to capital (human capital in his case) would change the character of the model drastically, making it incapable of generating permanent growth. He did not notice that a touch of increasing returns to capital would do the same, but

in a quite different way. Since I have not seen this acknowledged in the literature I will spell it out here.

Suppose that the production function is $f(K, L)$, with non-decreasing returns to capital. Treat L as constant for the moment, so we can think of this as just $f(K)$. Let net investment be the fraction s of output so that the time path of K is determined by $dK/dt = sf(K)$. It is obvious on the face that there is potential for fairly explosive behavior if $f(K)$ increases more and more rapidly with K . For instance, if $f(K)/K$ increases with K , the rate of growth of K gets faster as K gets larger. Then the time path for this growth model has the property that the stock of capital becomes infinite in finite time. (It is one thing to say that a quantity will eventually exceed any bound. It is quite another to say that it will exceed any stated bound before Christmas.) It takes a little calculus to show that “fairly explosive” puts it mildly.³

The fragility of the constant-elasticity case is worth pursuing further. I will choose $h = .05$ to represent a fairly small degree of increasing returns to capital. If $Y = K^{1.05}$, increasing K by 20 percent will increase Y by a bit more than 21 percent. This is already a fairly weak dose of increasing returns, and might even be empirically undetectable. Anything more would have even more drastic consequences. The capital-output ratio is of order of magnitude about one, to be conservative. A straightforward calculation shows that output will be infinite in about $(1/sh)$ years.⁴ If s is about 0.1 and h is as small as 0.05, a country like Germany or France will achieve infinite output in about 200 years, or even a shorter time from “now.” They should live so long, one is inclined to say.

Of course this kind of calculation should never be taken literally, but it teaches an important lesson. The knife-edge character of the constant-returns model can not be evaded by the obvious dodge: oh, well, so it blows up in finite time—that time could be a million years from now, by which time we will have evolved into God knows what. For the Land of Cockaigne to be a million years away, $1 + h$ would have to be so close to 1 that we would never be able to

³The solution of this differential equation is given by $\int_{K(t_0)}^{K(t)} dx/f(x) = s(t - t_0)$. Now suppose that the improper integral $\int_{K(t_0)}^{\infty} dx/f(x)$ converges to a number J (which will depend on $K(t_0)$ though this is not significant). Indeed the capital stock approaches infinity as t gets closer and closer to $t_0 + (J/s)$. If the production function will generate infinite output from infinite capital (as with Cobb and Douglas or a better-than-unit elasticity of substitution between labor and capital) then aggregate output and income become infinite at that time too. Allowing employment to increase can only hasten the date of the Big Bang. If output is finite even with infinite capital, the economy will achieve its maximal output in finite time. That is what I meant by saying that the model changes its character in a different way. What will make that improper integral converge? Clearly it is more likely to do so if $f(K)$ increases very rapidly with K . It can not do so if $f(K)$ is concave or linear. There are convex functions $f(K)$ for which the integral diverges. But increasing returns to capital helps a lot. It is easy to see that the integral converges if $f(K) = K^{1+h}$ for any positive h , no matter how small.

⁴When $f(K) = K^{1+h}$, the number J is, $\int_{K(t_0)}^{\infty} x^{-(1+h)} dx$, which is $K(t_0)^{-h}/h$. Since $Y(t_0) = K(t_0)^{1+h}$, $K(t_0)^{-h} = K(t_0)/Y(t_0)$. Thus the date of the Big Bang satisfies $s(t - t_0) = h^{-1}K(t_0)/Y(t_0)$. Solving for t shows that the date of the Big Bang (the end of scarcity as we know it) occurs at $t_0 + (K(t_0)/Y(t_0)(sh))^{-1}$.

discern the difference. The conclusion has to be that this version of the endogenous-growth model is very un-robust. It can not survive without *exactly* constant returns to capital. But you would have to believe in the tooth fairy to expect that kind of luck.

This branch of the new growth theory seems unpromising to me on straight theoretical grounds. If it found strong support in empirical material, one would have to reconsider and perhaps try to find some convincing reason why Nature has no choice but to present us with constant returns to capital. On the whole, however, the empirical evidence appears to be less than not strong; if anything, it goes the other way.

A particular style of empirical work seems to have sprung from the conjunction of growth theory and the immensely valuable body of comparative national-accounts data compiled by Summers and Heston (1991). It rests on international cross-section regressions with the average growth-rates of different countries as the dependent variable and various politico-economic factors on the right-hand side that might easily affect the growth rate if the growth rate were easily affected. I had better admit that I do not find this a confidence-inspiring project. It seems altogether too vulnerable to bias from omitted variables, to reverse causation, and above all to the recurrent suspicion that the experiences of very different national economies are not to be explained as if they represented different "points" on some well-defined surface. These weaknesses are confirmed by Levine and Reinelt (1992) and Levine and Zervos (1992), who find that these cross-section regressions are not robust to the choice of explanatory variables and are otherwise statistically unprepossessing. More strictly focussed studies—I am thinking especially of Mankiw, Romer and Weil (1992) and Islam (1992)—seem to favor some extended version of the neoclassical model.

The temptation of wishful thinking hovers over the interpretation of these cross-section studies. It should be countered by cheerful skepticism. The introduction of a wide range of explanatory variables has the advantage of offering partial shelter from the bias due to omitted variables. But this protection is paid for. As the range of explanation broadens, it becomes harder and harder to believe in an underlying structural, reversible relation that amounts to more than a sly way of saying that Japan grew rapidly and the United Kingdom grew slowly during this or that period.

I think that the real value of endogenous growth theory will emerge from its attempt to model the endogenous component of technological progress as an integral part of the theory of economic growth. Here too the pioneer was Romer (1990). Many others have followed his lead: my short list includes Grossman and Helpman (1991), Aghion and Howitt (1992), Stokey (1992) and Young (1991, 1993), but there are others.

This is a very hard problem for a number of reasons. For one thing, there is probably an irreducibly exogenous element in the research and development process, at least exogenous to the economy. Fields of research open up and

close down unpredictably, in economics as well as in science and technology. This is reflected, for instance, in the frequency with which research projects end up by finding something that was not even contemplated when the initial decisions were made. There is an internal logic—or sometimes non-logic—to the advance of knowledge that may be orthogonal to the economic logic. This is not at all to deny the partially endogenous character of innovation but only to suggest that the “production” of new technology may not be a simple matter of inputs and outputs. I do not doubt that high financial returns to successful innovation will divert resources into R&D. The hard part is to model what happens then.

A second difficulty, no doubt related to the first, is the large uncertainty surrounding many research projects. It is possible that some of this uncertainty is not probabilistic: if “Knightian uncertainty” shows up anywhere, it could be here. If so, then appropriate analytical techniques are lacking. Third, it is not clear how you would know if you had a promising model. Surface plausibility is one criterion, but hardly a sufficient one. The best source of empirical material may be historical case studies, but then the test of truth is bound to be fuzzy.

There are, of course, historians and sociologists, as well as economists, who study the R&D process in contextual detail. Their insights and conclusions are usually not in a form that can be used by a macroeconomic model-builder, and they may even regard the necessary abstraction and codification as a kind of violation. Even so, there is no excuse for ignoring the generalizations that emerge from other styles of work. Models of innovation can be constructed out of thin air, but it is surely better to use more durable materials if they are available. The best bet, no doubt, would be collaboration between model-builders and those who use informal methods, to compromise between one side’s need for definiteness and the other side’s sense of complexity.

All the difficulties notwithstanding, it seems to me that the body of work I have just cited has an air of promise and excitement about it. Aghion and Howitt (1992) manage to give some precision to Schumpeter’s vague notions about “creative destruction.” They make a formal model in which each innovation kills off its predecessors. It is obvious that some innovations reduce or wipe out the rents that might otherwise have accrued to previous innovations, and this fact of life has to be taken into account in any understanding of the process. But sometimes—who knows, maybe just as often—innovations are complementary with predecessors and add to their rents. This possibility matters too. Is there any non-mechanical way to take both contingencies into account? (Schumpeter is a sort of patron saint in this field. I may be alone in thinking that he should be treated like a patron saint: paraded around one day each year and more or less ignored the rest of the time.)

It seems to me that there is great merit in Alwyn Young’s (1993) project of treating learning-by-doing as one mode of productivity increase, but not the only one. It is an important fact of life that many instances of product improvement and cost reduction have little to do with the R&D activity, but

originate in some other way, for instance from the cumulation of small suggestions coming from production workers, process engineers, and even customers. Categorical R&D spending may be an inadequate measure of the resources devoted to increasing productivity. How to understand and model that other way is an important question. Growth theorists might profit from picking the brains of informed observers of industry.

This is a good place for me to insert a few more idiosyncratic criticisms of the new wave. Much of the advanced literature uses the “new product” as a universal metaphor for innovation. Even cost reduction is often supposed to come about via the invention of new intermediate goods. The development of new products is certainly a prominent feature of the technological landscape, but one is permitted to wonder if that is the only way to go, or even the best way. Any particular metaphor can impose a bias on subsequent trains of thought.

The 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. Suppose that the production function is $Af(K, L)$ where A carried (Hicks-neutral) technological progress. (The neutrality is just for clarity; it is inessential.) Successful innovations make A larger. But how much larger?

For this purpose, take it for granted that there is something meaningful called “an innovation” and a stream of these innovations occurs as a result of decisions made by firms. It is easy to agree that the flow of innovations per unit time depends on the amount of resources devoted to creating them. If an innovation generates a proportionate increase in A , then we have a theory of easy endogenous growth. Spend more resources on R&D, there will be more innovations per year, and the growth rate of A will be higher. But suppose that an innovation generates only an absolute increase in A : then greater allocation of resources to R&D buys a one-time jump in productivity, but not a faster rate of productivity growth. I do not know which is the better assumption, and these are only two of many possibilities. But merely to adopt the more powerful assumption is no more than to assume the more powerful conclusion.

Ideally, such modelling decisions should be made in the light of facts. Unfortunately there are not a lot of usable facts to be digested. One could hope for some enlightenment from case studies of industries, technologies, and R&D decisions. Even that is not easy: it takes two to tango and the authors of case studies do not like to see their insights reduced to terms in a highly-simplified equation. Nevertheless I think the best candidate for a research agenda right now would be an attempt to extract a few workable hypotheses from the variegated mass of case studies, business histories, interviews, expert testimony, anything that might throw light on good ways to model the flow of productivity-increasing innovations and improvements. Finally I would like to call attention to an interesting paper by Caballero and Jaffe (1993) who made

an ingenious start on exploiting whatever data there are. I am not necessarily endorsing all their conclusions, but rather their willingness to sift through a lot of data looking for reasonable generalizations.

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