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RECENT EXERCISES IN GROWTH ACCOUNTING:

NEW UNDERSTANDING OR DEAD END?

by

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RECENT EXERCISES IN GROWTH ACCOUNTING:

NEW UNDERSTANDING OR DEAD END?*

Richard R. Nelson

The growth accounting literature has been enriched recently by several major quantitative studies, and a sophisticated technical dialogue.¹ The obvious high quality of this work may lead some economists to think that great progress has been made in our understanding of economic growth and that we are nearly home. I suggest that while recent research has increased our knowledge, studies of this sort have run into sharply diminishing returns and soon will arrive at a dead end leaving many essential open questions. In Section I, I will consider some basic difficulties with growth accounting. Several of the points raised here have been raised before but appear to have been repressed in the recent discussions; it seems important to introduce them again to the dialogue. Most growth accounting purports to rest on the neo-classical theory of economic growth. In Section II, I shall argue that this theory is more a way of looking at things than a real theory, and that neo-classical spectacles may distort or block perception of phenomena that should be at the center of a serious theory of economic growth. I shall conclude by providing a preliminary sketch of a proposed growth theory built on Schumpeterian rather than neo-classical perceptions.

Some Basic Limitations of Growth Accounting

The logic behind growth accounting appears to be simple, but appearances

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are deceiving. In this section I shall discuss two basic difficulties of growth accounting. One is the problem of distinguishing between movements along a production function and shifts in that function. The second is the problem of treating experienced growth as the sum of the contributions made by separate factors.

The problem of competing explanations. From its beginnings growth accounting has been concerned with trying to estimate how much of growth can be explained by movements along a production function, and how much can be attributed, at least in part, to advances in technological and organizational competence. The early studies recognized quite explicitly the difficulties, perhaps even the theoretical impossibility, of distinguishing between alternative explanations of observed growth patterns without rather strong a priori assumptions.² The growth patterns here refer to time series data. The a priori assumptions could come from cross section data or other empirical sources. Some of the recent studies appear to give the impression that on the basis of rather weak a priori assumptions there is a theoretically correct way of distinguishing movements along a production function from shifts in it. It seems important, therefore, to review the basic problem.

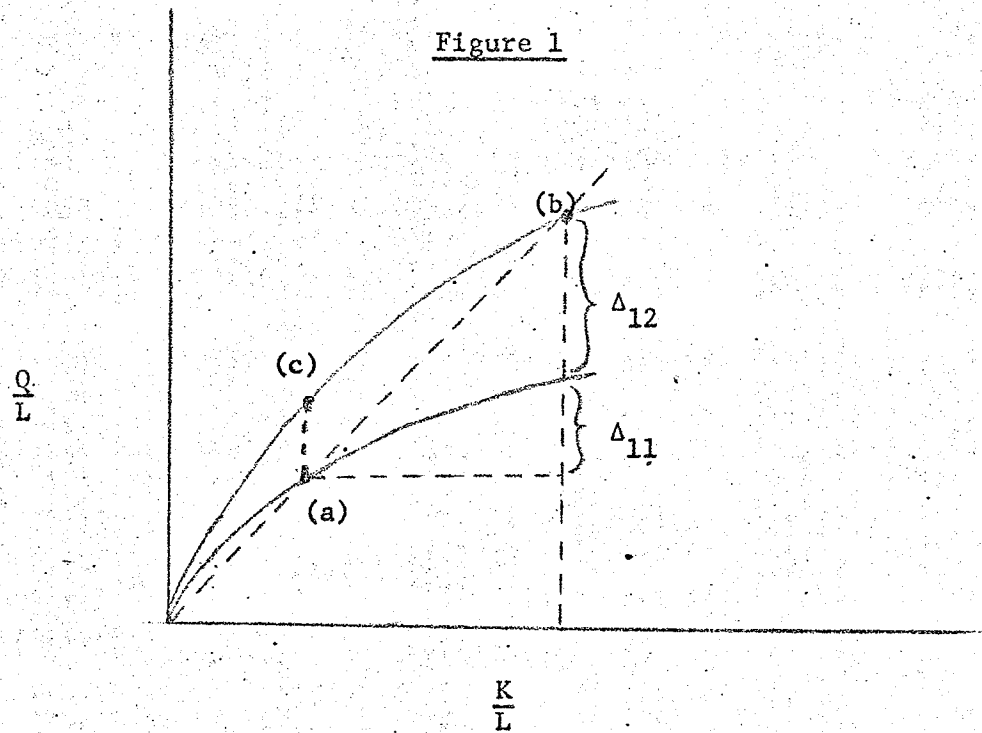
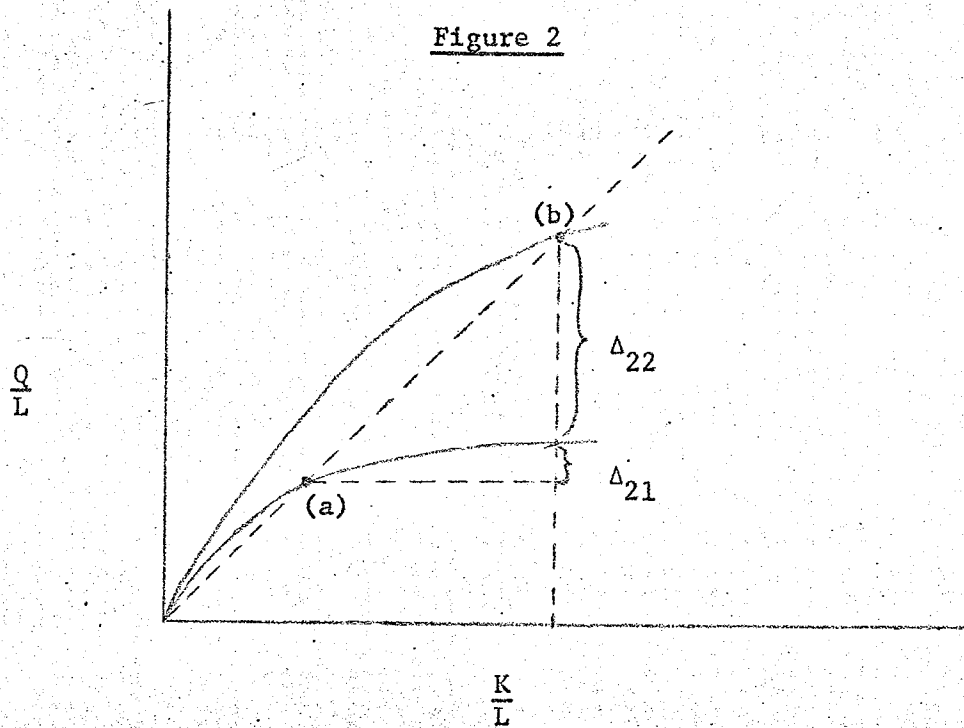
The discussion here will not be focussed on any particular study or set of numbers but on the general problem. The difficulty can be seen sharply if one assumes the following stylized aggregative facts.³ Output (GNP) has been growing at the same rate as capital and at a faster rate than labor; hence the capital output ratio has been constant and output per worker and the capital-labor ratio have been rising. Factor shares have remained constant;

thus the rate of return on capital has been constant and the wage rate has risen. These "facts" very roughly characterize the U.S. growth experience that the accounting exercises seek to explain. Consider the following two competing explanations, both consistent with the time series data. One is that the underlying production function is Cobb-Douglas (of unitary elasticity of substitution) and technical change has been neutral in the sense of Hicks. The second is that the underlying production function has an elasticity of substitution less than one, and that technical change has been labor saving.

The differences between the explanations can be seen in terms of how they explain growth of output per worker. The first interpretation is depicted in Figure 1, the second in Figure 2. Points (a) and (b) in the two figures are identical and the slopes of the curves (the marginal productivity of capital) at those points also are identical. However the curve in Figure 1 that goes through point (a) shows a greater tendency to diminishing returns than the curve of Figure 1 (the elasticity of substitution between capital and labor is lower). Also, the curve through (b) in Figure 2 does not represent an equal proportional increase in output per worker for each capital-labor ratio compared with the curve through point (a). Rather the proportional increase is greater for a high capital-labor ratio than for low (technical change has been labor saving).

The two interpretations are different in the following "growth accounting" sense. In the case of Figure 1 output per worker would have grown by Δ_{11} if capital per worker had grown as it did, but the production function had not shifted. Δ_{12} represents the increase in output per worker not explained by growth of the capital-labor ratio and hence due, in some sense, to technical

Alternative Interpretations of Productivity Growth

Figure 1Figure 2

change. In Figure 2, Δ_{21} can be attributed to growth of capital per worker and Δ_{22} to technological change in the sense above. File for future reference that the "contribution" of technical change is estimated by subtracting the contribution of other factors. This aside, under the first interpretation a larger fraction of productivity growth is attributed to growth of capital intensity. In the latter interpretation the lower elasticity of substitution means that less of productivity growth can be attributed to growing capital intensity, hence more must be attributed to improved technology. As Diamond, McFadden, and Rodriguez (among others) have pointed out, since both interpretations are equally consistent with the data there is no way to choose among them, without a priori assumptions. Thus the growth accounting is arbitrary.

The discussion above has not dealt explicitly with an important characteristic of many recent growth accounting exercises; the attempt to take into account increases in factor quality as well as quantity. But exactly the same issues are involved. The way quality changes are handled (in principle at least) in the recent literature is to divide gross factors into subgroups of different quality and estimate the expansion of each. Thus the quality problem is translated into a disaggregation of quantity problem.⁴ All of the preceeding discussion applies.

In fact the growth accounting exercises have not proceeded by attempting to specify a particular "production function" and estimate its parameters. Rather the strategy is somehow to build up an input "index" that measures the contribution of input growth to output growth without explicit commitment to a particular production function. There is a semantic problem here.

The "input index" really is an estimate of output under the assumption of constant technology. In any case this research methodology does not avoid the problem but simply evades it.

The use of a particular weighting or index scheme for input growth is the growth accountant's de facto assumption about the shape of the production function. A starting place for all growth accounting is the assumption that, if the neo-classical theory holds, at any time factor prices equal marginal productivities. Thus it is natural to weight factor inputs by their prices. But prices when? They are likely to vary over the period in question. One could use initial price weights. If one did this one would be in effect assuming that the production function followed the tangent at point (a) in Figures 1 and 2. If there were any curvature at all to the function this procedure would lead to an overestimate of the contribution of input growth and an underassessment of the contribution of technological change. Or one could weight percentage input growth by the initial "share" of income. In effect this would be assuming that the production function was Cobb-Douglas. Both of these assumptions obviously are arbitrary and lead to arbitrary growth accounting.

There seems to be a belief that the use of Divisia indices gets around this problem.⁵ The Divisia index in theory weights inputs at any moment of time by their prices at that moment. While more traditional indices use (arbitrary) fixed weights, the Divisia index uses continuously changing weights. There are many reasons why the Divisia index is appealing. However the use of the Divisia index does not resolve the problem.

Ideally in using the Divisia method one would estimate the instantaneous

rate of production function shift by the method proposed by Solow--percentage output increase minus factor share weighted percentage input growth.

$$1) \frac{dA}{A} = \frac{dQ}{Q} - S_L \frac{dL}{L} - S_K \frac{dK}{K}$$

Integration then would yield a moving index of total factor productivity. In practice factor price weights cannot be re-estimated continuously but on a yearly (or other periodic) basis. This amounts to a de facto assumption that within periods the production function is Cobb-Douglas. Thus this procedure means that within each sub-period the contribution of input growth and technology shift is estimated on the basis of interpretation 1a. This clearly is arbitrary, but can be rationalized by appeal to a "Taylor's series" argument, and in any case is not the basic problem.

If time intervals are short the difference between the intra-period interpretations shrinks. In the limit, for infinitesimally small proportional input changes (and changes in technology), we cannot distinguish between the different interpretations; they yield the same attribution. This is so because we are moving along curves with initially the same slope, and for small changes in inputs even large differences in curvature (elasticities of substitution) will not show up. But if the total attribution is over a finite period of time, the fact that the overall period is divided up into a large number of very short periods does not help at all. As the sub-time periods shrink and the intra-period differences get smaller, a larger number of these need to be added up over the total period. The problem does not go away. Nor would the problem disappear if we didn't have to worry about the practical reality of finite sub-periods. It is the finiteness of the total

comparison period that causes the difficulty.

The dashed lines between points (a) and (b) in Figures 1 and 2 are identical. They are meant to represent a smooth growth of output and input between two discreetly different points of time. Both time paths will yield the same Divisia index series of inputs, since along the path inputs and factor prices (shares) are the same. The Divisia index series for inputs is, in several recent studies, the implicit specification of output growth, had technology not changed. Under our stylized data assumptions capital's share is constant. This means that the Divisia index of inputs moves like a Cobb-Douglas with constant output elasticities, or along the curve through point (1) in Figure 2.⁶ The use of the Divisia index for inputs thus will yield the attribution of growth to increased factor inputs and to technical change of Figure 1. But the data are consistent as well with Figure 2.

The problem lies in the failure of the Divisia formula for an index of technology to face up to the basic problem. Integrating the Solow instantaneous technical change equation yields:

$$2) \log A(T) - \log A(0) = \int_0^T \left[\frac{d \log Q(t)}{dt} - s_L(t) \frac{d \log L(t)}{dt} - s_k(t) \frac{d \log k(t)}{dt} \right] dt$$

However the time path of factor shares is what it is because of both changes in factor ratios and technological change. The Divisia formula fails to distinguish between alternative explanations of factor shares. Thus under the interpretation of Figure 2 the capital share would have fallen but for the fact that technical change was capital using. If one wants to attribute

to factor growth only what output growth would have been had technology been constant, then one must use in equation (2) not actual "shares," but the time path of shares as they would have been had technology not changed.⁷ But to do this requires that one be able to specify the original production function which was the original *impass*.

The route out of the *impass* requires more specification based on other data. For example, one could attempt to estimate the elasticity of substitution from engineering design data. Or, if one had access to cross-section data on outputs and inputs as well as time series data, under certain assumptions one might be able to sort out the shape of the production function from shifts in that function. Assumptions about momentum or independence can facilitate discrimination. One can assume that variation in the rate of change of the capital-labor ratio is large relative to variation in the pace and character of technical advance, or that movements in the two are independent of each other. Then if one found that when the capital-labor ratio increased rapidly there was a fall in capital share, but when the capital-labor ratio increased by the same amount but over a longer period of time (more slowly) there was no fall in the share, this would be evidence that the elasticity of substitution was less than one and that technical change was labor saving. This is Fellner's approach in a recent paper (1971). In any case, in order to do growth accounting in a non-arbitrary way we need knowledge that goes beyond the data that are used in the growth accounting.

The meaning of growth attribution. Let us assume that the problem described above is solved. Then it would be possible to pose the following question: how much growth would we have experienced had only technology

changed, or only capital, or only labor, or only capital and labor, or various other combinations of factors. However it is uncertain what meaning one would give to the answers. If by attribution to a particular factor we mean the growth that would have occurred had it alone changed, the relative attribution to growth of different factors is not independent of the time period in question, even if all factors are changing at a constant rate. Further, while using this meaning of attribution the sum of the attributions adds up to total growth for very small time periods, they may not add up to total growth if finite time periods are considered. These points were raised earlier by Levine and Massell, but seem to have been ignored in the recent discussion. The recent literature appears to get around the problem by posing the attribution problem in a different way. How much of the average yearly growth rate that we have experienced would we have attained if during an average year technology alone had advanced at its average rate, or capital alone, etc.? While this resolves the technical problem it of course does not solve the basic problem that the very meaning of a growth attribution is obscure.

Assume that interpretation 2 is known to be correct, that the production function is Cobb-Douglas and technical advance is neutral, and that all factors were growing at constant rates. Assume that by attribution to a factor we mean the amount of output growth that would have occurred had that factor alone changed. Measure the instantaneous growth rates associated with the yearly growth rates of capital and labor. Label these λ_K and λ_L . Estimate the instantaneous rate of technical progress by the Solow method using the instantaneous rate of output growth. Call this λ_A . In the case of infinitesimally small changes the attribution to technical advance

relative to capital growth can be expressed as follows:

$$3a) \frac{\text{Attribution to Technical Advance}}{\text{Attribution to Capital Growth}} = \frac{\lambda_A}{\lambda_K S_K}$$

And the following expression tells how much of total growth was contributed by technical change:

$$4a) \frac{\text{Attribution to Technical Advance}}{\text{Total Growth}} = \frac{\lambda_A}{\lambda_A + \lambda_L S_L + \lambda_K S_K}$$

All this is nice and neat, with attributions to individual factors adding up to total growth.

All the neatness goes when finite time periods are considered. For capital and technological change the attribution ratio is as follows:

$$3b) \frac{\text{Attribution to Technical Advance}}{\text{Attribution to Capital Growth}} = \frac{e^{\lambda_A T} - 1}{e^{\lambda_K S_K T} - 1}$$

As $T \rightarrow 0$ the ratio refers to very small changes and should asymptotically yield expression 3a. Since both numerator and denominator go to zero, l'Hôpital's rule must be applied. Then it is seen that as $T \rightarrow 0$ the attribution ratio does approach $\frac{\lambda_A}{S_K \lambda_K}$. However assume finite T . Then the ratio is different. Indeed as T increases the expression increases toward infinity or falls to zero as λ_A exceeds or falls short of $\lambda_K S_K$. Relative attributions to different factors are sensitive to the time period in question.

Notice also that under our concept of attribution, over a finite period of time total growth may not be attributable to the separate factors. Indeed in the Cobb-Douglas case the percentage of total growth explained by growth of any particular factor, with the others held constant, shrinks to zero

as the time period increases. For technical advance, for example:

$$4b) \frac{\text{Attribution to Technical Advance}}{\text{Total Growth}} = \frac{e^{\lambda_A T} - 1}{e^{(\lambda_K S_K + \lambda_L S_L + \lambda_A)T} - 1}$$

This expression clearly goes to zero as T increases.

At first glance all this seems strange, but it is not. Growth accounting simply does not get at what some practitioners seem to claim it gets at-- relative contributions to growth of different factors. It does not get at that question because the question has no answer if we are interested in finite changes over a finite period. The problem here is the same one that plagued the profession many years ago when it was trying to attribute total product (rather than growth) between the different factors. We learned then that this was impossible. We could attribute at the margin. But there was no way of attributing shares of the total.

To see the problem from another perspective look again at Figure 1 and assume the time period is one year. Δ_{11} measures how much output would have grown had technology remained constant. It is analogous to the numerator of equation 3b. Δ_{12} is the measure of the contribution of technological change measured as a residual, but it does not measure how much output would have grown over the year had capital remained constant. It is not analogous to the denominator in equation 3b. The distance between points (c) and (a) measures that. Call this Δ_{12}^* . This is the same as Δ_{12} only for very small changes. For finite changes, even a year, Δ_{11} plus Δ_{12}^* do not add up to total growth. To get total growth one must add an "interaction term."

The standard growth accounting appears to get around this problem,

but it does not. In the first place the conversion to an average yearly change rather than an instantaneous change means that for that average year the "contribution of technical change" overestimates what growth would have been had factors actually remained constant over that year; Δ_{12} exceeds Δ_{12}^* . But this discrepancy is very small. The real problem is that the contribution of technical change to growth, or of expansion of any particular factor, during any year is not independent of what happened to the other factors prior to that year. Let us continue to assume interpretation 1 and constant rates of change of the factors of production and technology. Consider a year toward the end of the accounting period. The contribution of say capital during that year is as large as it is because labor and technology had advanced during the prior periods as much as they did.⁸

The attempt to get numbers for the contribution of different factors to the growth process rests on misspecification of the process. Experienced growth is not the simple sum of the contributions of separate factors. In the Cobb-Douglas neutral technical change case all factors are complements. A finite increase in labor increases the output expansion that will result from a finite increase in capital, and vice versa. Technological advance and factor increase are also complementary, the first increases the marginal productivity of the second, the second increases the gain from a given percentage increase in total factor productivity. In this Cobb-Douglas neutral technical change case one certainly can go through the technical operations of attributing average yearly growth to a sum of average yearly contributions. But the meaning of such an attribution is quite unclear for considering finite growth. One could say that over a finite period the contributions multiply,

rather than add. But this is to admit that the "accounting" or "adding up" metaphor is misleading. And unless one knows that the production function is Cobb-Douglas one doesn't know that the contributions "multiply." The "division of credit" flavor of the growth accounting becomes even more obscure when one recognizes stronger forms of complementarity, like the requirement for educated people to do research and development, and of new physical capital to embody new technology.

One could take the position that the degree of interaction among the factors is small, and that the separable contributions of the different factors are like the first terms of a Taylor's expansion. This is a plausible position but rests on an assertion about the nature of the production function and about technical change. The approximation might be good, and it might be poor.

The thrust of these remarks is not that growth accounting is unilluminating. Growth accounting has been extremely useful in knocking down simple-minded notions. The early work of Abramowitz, Solow, and Kendrick demonstrated that there almost surely had to be more to growth than simple augmentation of physical capital and labor. Denison and Griliches mapped out a list of possible factors, and some plausible rough estimates of their importance. Griliches and Jorgenson have contributed significantly to our knowledge of the time path of certain of the factors behind growth. Changes over time in the estimated average yearly contribution made by different factors, in particular variations in the residual, are interesting and suggestive facts to know about, as are cross country and cross industry differences. Assumptions about momentum and independence permit some rough inference

drawing. But some of the recent studies seem to imply that we can get more than this from growth accounting, that somehow the growth accounts really explain growth. I do not see how they can. We cannot get to a tested theory of growth through growth accounting alone, and that particular route strikes me as now at a stage of very low marginal return.

Do We Have a Growth Theory of a Plausible Kind?

In order to do non-arbitrary growth accounting, and to know if growth accounting is a meaningful summary approximation to the sources of growth, we need additional knowledge. Ideally what we need is a tested growth theory with confident estimates of parameter values. If we had such a theory we could do everything that growth accounting can do (although not all that growth accounting purports to do). And without such a theory we really can't do growth accounting. Almost all of growth accounting claims its intellectual justification as the neo-classical theory of economic growth. But does such a theory exist? To the extent that it does exist in part, is it believable?

Neo-classical theory as a point of view, not a theory. What would a theory do for us, if we had one? In the first place we would expect the theory to give an explanation, an account (if not an accounting) of past growth. This immediately poses the question--an account of what phenomena? What needs to be explained? Certainly the aggregative time series data (at an economy or sector level) on output, input, and prices. The data show that beneath the aggregate (mean) figure for say labor productivity or the profit rate there is a considerable dispersion of firms around the mean.

I would argue that the theory ought to be at least consistent with, or better explain, the disaggregated data. I would argue that the theory also ought to be consistent with, or better explain, what we observe about process, a point with which many might disagree, and to which I will return later.

What do we mean by an explanation? I assume we mean computational ability of the theory to replicate reasonably closely the phenomena to be explained, given estimable parameter values. But generally we want more of a theory than just ability to replicate the past. There may be alternative explanations that it is interesting or important to distinguish among. I assume that an acceptable "explanation" does not leave unanswered questions that economists find interesting, like the two probed in the preceding section. How much of the economic growth we have experienced would have been possible in the absence of technological advance? To what extent can the explanation of growth be in terms of the contribution of different factors, or is this misleading because growth involves a strongly complementary package of factors? It is apparent that an interest in distinguishing among alternative explanations of growth is highly influenced by our hope that the theory may be useful to policy. The two questions above have obvious significance to growth policy.

Limiting the present discussion to ability to explain the "macro" data, I suggest that the neo-classical theory isn't really a theory. In particular different versions of the grab bag of things called neo-classical theory answer these questions in different ways.

I suggest that the spirit of most of the growth accounting exercises indicates de facto acceptance of a model that presumes considerable sustained growth is possible without technical advance, and that factor complementarity

is not particularly important. The implicit theory is that output is a function of technology, effective capital, and effective labor. The elasticities of substitution among capital of different qualities, and among labor of different qualities, is assumed to be infinite.⁹ The elasticity of substitution between effective capital and effective labor implicitly is assumed to be relatively high, probably in the neighborhood of unity, and certainly not close to zero. There is considerable ambiguity regarding the connection between technical advance and capital and labor quality. Basically, however, growth accounting as practiced makes sense only if it is assumed that the generation and incorporation of new technology requires only modest amounts of new capital and is not particularly associated with labor of a particular kind or quality.¹⁰

Under these specifications growth of output per worker can continue so long as the capital labor ratio grows. Actually the critical value of the elasticity of substitution in a CES model is unity; if it is below this output per worker for a constant technology is bounded. However for analysis of periods of a couple of decades at the rates of factor growth we have experienced, little deceleration of growth of output per worker would be experienced at a constant growth of capital per worker, even for an elasticity of substitution of as low as one half. Similarly the growth accounting interaction term would not be particularly important over such a time period. Growth of output could be explained quite well as the sum of the separate contributions of improved technology, and increases in effective capital and labor.

Consider the following alternative model which is at almost an opposite extreme regarding the two questions. Solow, Tobin, Von Weizacker and Yaari have proposed a model in which any productive increase in the capital

intensity of production requires new technology. If technological advance stopped today output per worker would grow for a while as firms that had been working with older capital shifted over to new and more productive machines. But once they had done this growth of output per worker would cease. In this model new technology is needed in order to permit productive increase in capital intensity but new technology cannot get into practice without new capital. Let me add to this structure the following. The production and installation of new technology requires educated workers; further in the absence of technological advance educated workers would be doing nothing different than uneducated workers and would not be more productive.

In this theory it is natural to think of technological advance as the binding constraint on the system; certainly growth would be impossible without technical advance. Traditional growth accounting would be nonsense, because of the strong complementarity among technological change, capital growth, and education. To estimate the contribution of technical change by subtracting an estimated contribution of increases capital and education clearly would be absurd.

The differences between the models involve not only interpretation of past experience, but prescription of how to improve future performance. In the first model it is natural to think of a number of different, and roughly separable, factors that might increase the growth rate. Choice among say more R and D, education, and more physical investment can be made on the basis of rate of return, or cost benefit calculations. While in the long run the complementarity among the factors means that the rate of return on one is not independent of the level of the others, for shorter run calculations this

can be ignored. The second model forces policy thinking in terms of complementary packages. Thus a policy in support of R and D is thought of as needing support by a policy of training scientists, and as being made effective through policies to facilitate physical investment. An interesting example of a policy which, to be successful, required a rather complex package is the so called green revolution, as described by Hayami and Ruttan.

To repeat the argument of the earlier sections, we cannot confidently distinguish between these two opposite extremes on the basis of growth accounting exercises and time series data alone. Yet the differences clearly are very important. I suspect that, if we limit ourself to neo-classical formulations, the right model is somewhere in between the two cases discussed above, but for many sectors may be closer to the second model than the first. We economists tend to be far too facile with our chalk (or equations) in drawing isoquants into regions of factor proportions that never have been experienced.¹¹ I would bet that in the absence of considerable research and development reconnaissance of the terrain, firms venturing into technologies with significantly higher capital labor ratios than actually have been experienced will tend initially to make mistakes, and will experience a considerable amount of learning costs before achieving significant gains in output per worker. Either research and development (learning before doing) or learning by doing (certainly also a form of R and D) is required to make the isoquant more elastic beyond the experienced range. Similarly I believe that economists have been much too mechanical in their treatment of the returns to education. It seems a safe bet that a large share of the returns to higher

education are tied up with the processes of technical advance and the chain of economic adjustments there-by set in train.¹²

These conjectures can be read as a bet in favor of one of the models in the neo-classical grab bag rather than another. However until the conjectures are proved one way or another, I would argue we really don't have much of a theory. One might take the position that we have a theory but no firm knowledge of the parameter values. However if range of possible specifications of the model is as great as it seems to be this point of view seems close to meaningless.

Further, if technical change is important I suspect that the kind of growth theory that we need is not in the current grab bag of neo-classical models as described by Solow. Our existing growth theory represents a rather straightforward dynamizing of the very statical firm and industry of Schumpeter's circular flow. All that growth theory adds is smooth and predictable growth of inputs and technology. Sooner or later we will need to encompass the world of Schumpeter's Chapter 2. Innovation and change are not predictable. All technologies purchased now are not the best. Some firms make better choices, others worse. There are leaders and followers. Competition is a dynamic process not a static condition. Nordhaus and Tobin comment pessimistically on the chances of developing a Schumpeterian theory of growth.

Many economists agree with the broad outlines of Schumpeter's vision of capitalist development, which is a far cry from the growth models made nowadays in either Cambridge, Massachusetts, or Cambridge, England. But visions of this kind have yet to be transformed into a theory that can be applied to everyday analytical and empirical work.

I suspect it will not be hard once we put our minds to it.

One of the reasons we have not put our minds to it is that economists

appreciate that, for all its difficulties, the neo-classical vision (I suggest it really is not a theory in any meaningful sense) contains some important germs of truth. It contains the notion that firms are not unresponsive to profit opportunities. It is built around the notion that outputs require inputs, and that part of what increases labor productivity is increase in resources (principally capital) per worker. Surely these we want to preserve. But we do not need a full blown neo-classical theory to preserve these. We can have them with a theory based on Schumpeterian foundations.

What might a neo-Schumpeterian theory of growth look like? The outlines seem reasonably clear. In the first place the theory must avoid the representative firm in competitive equilibrium allegory which demarks neo-classical theory. In a Schumpeterian growth model, at any time firms can be operating using different technologies, with different unit costs, some making profits, others making losses. Competition is a process in which profitable firms expand and are imitated, unprofitable ones drop out of business or find better ways. Such a model can, under certain assumptions have the equilibrium steady state characteristics of neo-classical theory. Winter has developed such a model. But the "motion" of this kind of a model in a regime where new technology is being introduced is Schumpeterian. I have employed a simple model in this spirit to examine growth over time in a less developed country.¹³ The model generates Schumpeterian profits for the firms using the better technologies, which provides the funds (savings?) for their expansion relative to the less efficient firms, as well as the motivation.

Second, the model should distinguish between the kinds of capabilities that are important in the routine steady state operation of equilibrium,

and the kinds of capabilities that are required for effective innovation or perceptive imitation. In part the mix of capabilities possessed by a firm may be a matter of luck; but certainly in part it is a matter of decision. Firms can decide to hire scientists and engineers to try to develop new technologies and to watch the developments of other firms, or they can decide not to do that. If the technology is amenable to innovation and the firm or its competitors finds something new and better, these capabilities will pay off. If technology is not tractable the firm with an R and D establishment will be saddled with costs but no benefits. And the firm that did not hire the R and D capability will make the profits. Undoubtedly R and D fortunes fluctuate and so therefore do the capabilities of firms that are associated with being profitable.

Sidney Winter and I are developing a model which incorporates these elements.¹⁴ Growth, profits, and capital formation are all generated largely by innovation. The industry at any time is characterized by a distribution of firms using different technologies, having different profitabilities, and expanding or contracting at different rates. Firms also differ in the probability that they will create an innovation, or adopt better technology used by others, over a given time period. Not all innovations are superior to existing technology, so the selection process is a key part of the model. Better technology, when it is created, is spread through the system both by expansion of the innovating firm and by imitation. Rising capital intensity is induced in the model through the effects on the dynamic selection system of increases in the wage rate, which makes profitable more capital intensive technology, if it is created.

Obviously this is a much more complicated theory than the neo-classical theory. What are the advantages? Note first that this theory can explain aggregate data at least as well as can the neo-classical theory; Winter's demonstration that this kind of a model can generate competitive equilibrium guarantees that neo-classical results can be replicated. This kind of a model may do better with aggregate data but its real advantages lie in ability to be consistent with, and perhaps to explain, disaggregated data and "to square with" observed process. It is our bet that real understanding of how growth occurs, and of how to influence it, can be won only after one has stripped off the surface level of aggregate data and looked at the individual units, and understand what they really are doing.¹⁵

Footnotes

1. In particular there is the important study of Griliches and Jorgenson (1967), discussion of this work by Denison (1969), and the attempt at reconciliation by Griliches and Jorgenson (1970). Nadiri recently has presented a general discussion of the growth accounting literature.
2. See for example the cautious remarks of Kendrick.
3. Solow makes use of these same stylized facts.
4. This is stressed in the work of Griliches and Jorgenson.
5. Griliches and Jorgenson seem to claim this. The original statement of the correctness of the Divisia formula seems to be Richter's.
6. Thus under the stylized data assumption assuming a Cobb-Douglas initially, using initial factor shares to weight percentage factor increases, and using the Divisia index, all amount to the same thing.
7. That is the "shares" need to be written explicitly as a function $S_i(\frac{K}{L}(t), A(t))$. Equation (2) needs to be specified with $S_i(\frac{K}{L}(t), A(0))$. The Richter specification of Equation (2) does not do this.
8. Consider for example a Cobb-Douglas of the form:

$$Q = AK^{1/2}L^{1/2}$$

Assume that over a half century A doubled, and K and L both quadrupled. Q then would increase by a factor of eight. Each factor, had it alone changed, would have caused a doubling of output. However given that capital and labor grew as they did, output would have increased only four-fold had technology not changed. Thus technical change would account for a four-fold increase in growth if its contribution was estimated as a residual over the total period. From another perspective, average yearly growth rates of A, K and L, and Q would have been 1.4%, 2.8%, and 4.2%. The sum of the "average yearly contributions" of each factor would add up to total average yearly growth. But note that in the final year the contribution of any of these factors taken alone would have been only one fourth as much as it actually was had the other factors remained constant over the entire period.

9. This is so for models that aggregate capital and which "quality adjust" labor. Not all of the neo-classical models are of this kind.
10. For a similar discussion see Fellner (1970).

11. Fellner (1970) makes a similar point.
12. For models in this spirit see Nelson and Phelps, and Welch.
13. Nelson (1968).
14. For a discussion in more detail of certain aspects of our modeling see Nelson (1971).
15. Relatedly we believe that progress toward a theory of growth will require that different sectors be studied and treated separately.

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