Volume Title: The Theory and Empirical Analysis of Production

Volume Author/Editor: Murray Brown, editor

Volume Publisher: NBER

Volume ISBN: 0-870-14486-3

Volume URL: http://www.nber.org/books/brow67-1

Conference Date:

Publication Date: 1967

Chapter Title: Aggregative Production Functions and Economic Growth Policy

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Chapter URL: http://www.nber.org/chapters/c1485

Chapter pages in book: (p. 479- 499)

AGGREGATIVE PRODUCTION FUNCTIONS

AND ECONOMIC GROWTH POLICY

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AS the title of my paper indicates, I have narrowed the scope somewhat from the broader topic assigned to me. I have focused on the question: What guidance for economic growth policy is provided by our present knowledge of the relationship between various aggregative inputs and the production possibilities open to an economy. Further, I shall limit myself to the situation in the United States.

By economic growth policy I mean policy concerned with influencing the rate at which potential output will grow—the speed at which the frontier of production possibilities will push ahead. There are many and diverse kinds of knowledge inputs to economic growth policy making. Complementing or competing with the formal knowledge of professional economists is knowledge held by other disciplines, and hunches and inferences drawn by policy makers from experience, analogy, and *ad hoc* reasoning. The relative influence of the economist's formal knowledge about production relations depends, and rightly so, on its strength and scope. Perhaps even more important, the way knowledge is most fruitfully employed in decision-making depends on how reliable and complete that knowledge is.

The first parts of this paper will be concerned with assessing certain aspects of the strength and scope of existing knowledge. Then I shall attempt to assess in broad terms what real assistance to policy making is provided by present knowledge, and how that knowledge might most fruitfully be used.

Uncertainties About the Aggregate
Production Function

By an aggregate production function I mean the constraint or set of constraints that determines the outer bounds of the set of production possibilities as a function of the quantity and quality of various inputs. Here I am interested in the dynamic aspects—the relationship between growth of potential output over time and increases or improvements in the inputs and changes in the nature of the constraints. Clearly, knowledge of this sort is a key input to growth policy making; it points out the variables on which to operate, and indicates the results of effecting various changes. Obviously the more complete and correct the list of influential variables, and the more accurate and reliable the estimates of exactly how and how much their changes will increase potential output, the greater the assistance to growth policy.

Several of the papers of this conference are concerned with examining the present state of knowledge regarding just this. I shall try to avoid too much overlapping or conflicting with these papers, but a brief assessment of the strength of existing knowledge about the dynamic aggregate production function is pivotal in examining the role of such knowledge in the making of growth policy.

Over the past several years the list of variables treated explicitly in aggregate production function theory has increased significantly. Not so long ago theory pointed to two variables—quantity of labor and quantity of physical capital—and wrung its hands that these alone appeared to be only half the story, increases in total factor productivity appearing about as important as increases in the variables dealt with explicitly. While sometimes increases in total factor productivity— measured as a residual or a time trend—were called technical progress, no one really believed this was all there was to it, and in any case such an implicit treatment of technological progress adds little to knowledge.[1]

Certainly today we have a much larger set of variables that we deal with explicitly. Denison works with at least a dozen variables.[2] Even

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[1] For example, R. M. Solow, "Technical Change and the Aggregate Production Function," *Review of Economics and Statistics,* August 1957, or M. Abramowitz, "Resource and Output Trends in the United States," *American Economic Review,* May 1956.

[2] E. F. Denison, *The Sources of Economic Growth in the United States and the Alternatives Before Us,* Washington, D.C., 1962.

the more modest analyses generally attempt at the minimum to treat explicitly skills and knowledge embodied in the work force and deal at least in a semiexplicit way with the constraints imposed by the limits of man's technological knowledge, as well as examining the effects of different quantities of labor and capital.[3] Assuming these variables are important, over the past few years our growing knowledge of aggregate production relations has increased in usefulness to policymaking by providing a richer set of variables that policy can try to influence.

However, our knowledge still would appear to be quite weak with respect to the functional form of the relationships, and very weak with respect to the size of certain key parameters.

Problems probably are fewer with respect to the relationship between increased output potential and greater quantities of labor and capital inputs than with respect to other variables, but even here there are considerable uncertainties. Let me mention just a few. Denison has suggested that there is a strong negative relationship between average hours worked per week and the quality and intensity of work.[4] Clearly, the strength of this relationship (and the way it works) matters for growth policy, since one of the variables on which policy might act is trends in the average work week. Yet it is equally clear at the present time that the strength of the relationship is highly uncertain. With respect to physical capital, uncertainties still exist, for example, about the magnitude of the embodiment effect: the amount of gross investment that is really needed each year to keep the nation's capital stock from falling further out of date, and the extent to which more gross investment than this serves to achieve a fruitful updating of embodied technology.[5] The power of policies which increase the gross investment rate certainly depends on the answers to questions such as these. As another example, there is still some uncertainty with respect to the elasticity of substitution between capital and labor inputs, although over the short run this may not be very important.[6]

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[3] For example, R. Nelson, "Aggregate Production Functions and Medium Range Growth Projections," *American Economic Review,* September 1964, or Z. Griliches, "Agricultural Production Functions," *American Economic Review,* December 1964.

[4] *Op. cit.* Of course, this notion has been long-standing in some of the economic literature.

[5] See, for example, E. F. Denison, "The Unimportance of the Embodied Question," *American Economic Review,* March 1964.

[6] The most recent results would appear to be those of P. A. David and Th. van de Klundert, "Biased Efficiency Growth in the U.S.," *American Economic Review,* June 1965. However, clearly a slightly different specification of the model can lead to different conclusions.

Our knowledge is far weaker regarding the effect of increasing the skills and knowledge possessed by the work force. Let us assume, for the moment, that years of educational attainment is an adequate proxy variable for this, and focus on the question of what the effect would be of an increase in average educational attainments, without worrying that we are using an input once removed from the production function. Presently we tend to treat educational attainment as a factor that multiplies the power of labor input; education makes a man a more efficient laborer —able to do more each hour than a person with less education. Undoubtedly this is part of what education does. At least the rudiments of education enable a man to make fewer mistakes in doing almost any job and to understand and follow instructions better. For certain jobs special courses and training programs likewise perform this function. However, one wonders how sharply diminishing returns set in to increases in average educational attainment, or in the number of people who go through certain occupational training programs, or in the length of these programs. Surely there are limits to what a man need know to do a certain job well, and to the number of jobs where a great deal of education or training is needed.[7]

The suspicion about sharply diminished returns becomes stronger when a second, and quite different, role of educational attainment is recognized: that of serving as an easily observable indicator to an employer of a person's intelligence and discipline. In this role the educational system serves not so much to impart useful knowledge and skills— but rather to put a different label on different kinds of people.[8] Assuming that the more intelligent and disciplined have more advantage in some jobs than others, improved ability of employers to discriminate in advance should result in an improvement in labor allocation, which should increase potential output. But in this role almost surely there would be little return from increasing average educational attainments; indeed such increases as would result from forcing early dropouts to remain in school would make the education screen a less useful quality-signaling device.

There is a third role that educational attainment plays that suggests

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[7] For an elaboration of this and the following discussion, see R. Nelson, M. J. Peck, and E. Kalachek, *Technological Advance, Economic Growth, and Public Policy* (forthcoming).

[8] This is a point that has been much discussed orally. I have not been able to find a suitable written reference, however.

still another way it should fit into the aggregate production function. Obviously one of the principal things that education does is to increase the discriminating and the information-processing capacity of people; to enable a man better to assess the significance of different observations and to reduce the time and effort needed to comprehend additional knowledge generally (including more education).[9] In part this has its effect on the productivity of a man on a given task; thus a trained television repairman is much more likely to be able to diagnose a trouble, even if it is slightly different from those he has been trained to deal with, than someone with no knowledge of television sets. But in large part the principal effect is in adding flexibility to learn a new job fast and to understand and deal with things that are new. Thus in the early production operation of new chemical processes, chemical engineers often constitute a large fraction of the work force; their education and training gives them a comparative advantage both in rapidly acquiring any specialized knowledge needed for the job and in dealing with the unexpected things that are quite likely to happen. As the process becomes familiar and understood, special training programs can be set up to equip less well-educated persons to the task. As another example, the evidence suggests that well-educated farmers are better able to evaluate new agricultural products and processes than their less-educated peers, and tend therefore to adapt the high payoff innovations earlier.[10] Note that in this role education enters the production function as a variable determining how effectively the system adapts to change. Thus in agriculture average years of educational attainment may be an important variable determining the average lag between the introduction of productive new technology and its adaption.[11] In the chemical process industries the stock of educated people may determine how rapidly a new process can be adapted productively. This is very different than treating years of education as a variable multiplying productivity of labor.

In short, even though we may have considerable confidence that rising educational attainments have been a strong factor behind our growth,

[9] B. Wiesbrod has pointed this out in his "Education and Investment in Human Capital," *Journal of Political Economy,* October 1962.

[10] E. M. Rogers, *Diffusion of Innovations,* New York, 1962, particularly Chapter VI.

[11] R. Nelson and E. S. Phelps have developed such a model in "Investment in Humans, Technological Diffusion, and Economic Growth," *American Economic Review,* May 1966.

we must be quite modest about predicting the quantitative effects of policies which would increase educational attainments still further.

Of course, the greatest difficulties with the theory still lie in its treatment of technological progress. Clearly the treatment of technological progress as a residual or a time trend intrinsically is unsatisfactory, dealing neither with what technological advance is nor with its determinants. Technological advance is an increase in knowledge—an enrichment in the set of goods and processes man knows how to produce or use. In terms of its treatment in a production function, a technological advance is a relaxation of a constraint on what a given set of inputs can produce, often taking the form of a new activity that is open for use. It is true that technological advance in this sense will show up, as advances are implemented, as an increase in the productivity of various inputs, but increases in the productivity of inputs can result from a lot of other factors as well (in particular increases in other inputs not specified in the production function). Nor is incorporation of a variable like the rate of research and development (R&D) expenditures really very satisfactory, for the achievement of ouput increase through the expansion-of-knowledge route certainly involves both more and less than R&D inputs conventionally measured. A lot of technological progress results more from learning while doing—as a by-product of other activities—than from conscious R&D.

Further, even when a new product or process is largely the result of an R&D effort aimed at that objective, a lot more than R&D generally is required. Griliches may be on the right track when he incorporates both R&D and extension expenditures into his analysis of the factors behind productivity changes in agriculture, but he certainly would not claim he has the right functional form.[12] In a very real sense here the problem is similar to the use of years of educational attainment as a proxy for the skills and knowledge possessed by the labor force—the use of an input once removed whose connection with the variable which directly affects production potential is highly uncertain.

Relating to problems of dealing with technological progress, but actually a more general problem, is the basic inadequacy of our measurement of potential output. The production possibility frontier certainly does not shift out uniformly, and we have the familiar problem of try-

[12] *Op. cit.*

ing to characterize a changing vector with a scalor—the index number problem. I certainly do not want to open up a lengthy discussion of that general topic, but there is one point that is important in the context of this paper. The use of growth of GNP, as presently measured, probably leads to an understatement of the relative contribution of technological advance to economic growth.

Much of the economic growth we have experienced has involved the introduction of new consumer goods, like the airplane, penicillin, and the telephone. These goods are new in the sense that they enabled wants to be satisfied that could not be satisfied before. While in some situations the airplane is simply a less costly way to travel (counting time) than a train, prior to the modern airplane it was impossible to travel across the country in less than two days, much less five hours; not more difficult or expensive—impossible. Penicillin has made it possible—not just less costly—to save the lives of many people with certain infections. New products are measured in the GNP calculations by the amount people spend on them, but clearly many people would be willing to pay far more than that. If airplane service were eliminated from the spectrum of final products many consumers would require a significant increase in income to achieve a comparable level of satisfaction. For those for whom penicillin is the only thing that blocks death, the value of the new product clearly vastly exceeds what they pay for it. In general, a given increase in measured GNP tends to mean more in terms of increased utility when it is accompanied by an expansion of the range of consumer-produced choice than when it is not. Even without technological progress the near tripling of GNP per person we have experienced since 1900 would have been a boon. But it would have been a far less powerful liberating force if it had meant largely wider carriages, more coal for the kitchen stove, and more kerosene for the oil lamp. To the extent technological progress is the principal element behind the expansion of the kinds of product and services we know how to provide, its relative contribution to growth is underestimated by analyses using growth of measured GNP as an indicator of growth of potential output.

Again, it would appear that we are in much the same position as with respect to education. We have strong confidence with respect to the powerful role played by technological advance, but are in quite a weak position to estimate with confidence the impact of more or less rapid technological advance in the future.

The Scope of Theory; the Weak Links to Policy Instruments and Objectives

Of course, even if knowledge of aggregate production relations were very strong, this in itself would not provide full guidance for policy. This knowledge must be supplemented by a linkage to instruments under the control of government, by which the variables of the production function can be manipulated. Further, it must be possible to assess the welfare implications of different growth patterns and policies.

Presently the linkage downward to policy instruments is far from adequate. This is most striking in the case of technological progress. The delineation between the variables that enter the production function and the variables that influence these variables is, of course, somewhat arbitrary. Although I would prefer not to consider accumulated R&D expenditures as an input to the production function but rather as a variable once removed, this is in part at least a matter of taste. Further, I do not want to quibble here about the uncertain link between the pace of technological advance and R&D spending.

For the question of policy remains as to how the government can obtain increases in R&D spending. For certain kinds of R&D this is a simple matter; the government simply can increase its own R&D spending. But for other kinds of R&D—in particular applied research and development in most consumer goods fields and in many producer goods industries—the government presently does not spend any money. Experience of the past few years with proposals for the government to provide funds for R&D in housing and machine tools suggests that there are very great political obstacles to the direct approach. And there are questions as to whether direct provision of R&D grants and contracts would be the most efficient route. Rather, policy will, in large part, have to work through stimulating increases in private R&D spending. But how to do this? Theory of the determinants of private R&D spending is weak and at the present time provides little guidance in suggesting means of stimulation.[13] A tax credit has been proposed, and several suggestions have been made for increasing the incentives provided by the patent

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[13] Pieces of a theory have been developed by E. Mansfield, "Industrial R&D Expenditures; Determinants, Prospects, and Relation to Size of Firm and Inventive Output," *Journal of Political Economy,* August 1964, and R. Nelson, "The Impact of Arms Reduction on R&D," *American Economic Review,* May 1963.

system. But it is quite uncertain how much effect these measures would have.

Similar questions exist with respect to how the government can reduce the gap between best and average practice through provision of better technical information services (like the extension service on agriculture). We shall examine this case in some detail later.

The situation is somewhat better for getting a policy handle on ways to influence growth of physical capital and educational attainments. While with respect to physical capital the government is even more limited than in the case of R&D regarding what it can procure directly (clearly there are very strong barriers to the government directly investing in industrial capital), existing theory of the determinants of private capital formulation, although weak, is better than existing knowledge of the determinants of private R&D spending. Further, some of these variables—in particular the tax rate on profit, allowable depreciation schedules, and interest rates—are directly at the influence of government. In stimulating education, the government is able to spend funds directly. With respect to higher education, to the extent the quantity and quality of the education provided is limited by the funds available to colleges and universities, and the number of talented people going to school is limited by the cost of attaining education, direct subsidies and scholarship assistance should work. Until just recently direct provision of federal funds for primary and secondary education was impossible politically, but now that obstacle seems to have been removed.

However, I doubt if we should have great confidence in our ability to predict exactly the effect of various government policies on the rate of physical capital formation and on growth of real educational attainments. Certainly the effect of interest rates and tax rates on private investment decisions is still uncertain quantitatively. With respect to education, the question remains: Will simply more money provided to educational institutions result in a real increase in relevant education imparted to students? The situation is better than the weak knowledge of the relationship between R&D input and output of technological advance, but we scarcely can be confident of our knowledge of the production function for education.

I could push the point further, but there should be no need; it is clear that present understanding of how to influence the variables that enter the aggregate production function is far from as strong as it might be.

Even if we know exactly how to influence growth, our knowledge still would not be complete. I do not wish to dwell here on the present state of theory and thinking with respect to the payoffs and costs of different growth patterns. Suffice it to say there still is some question even as to whose benefits and costs should be counted, and with what weights; I refer to the whole set of issues with respect to the treatment of future generations.[14] And it is evident we still are not quite clear with respect to the very nature of the benefits to be obtained from increased production potential. Let me recall again the problem of valuing new goods. And to what extent is utility a function of the difference between what one aspires to and what one has, rather than a function simply of increased consumption? What is the effect of an increase in one's neighbor's consumption upon one's own utility? What is the relationship of growth to a nation's prestige or security, and how should this be valued? And how about the process of growing; it is intrinsically unsettling, requiring changes in almost all aspects of the patterns of life. Is this a minus or a plus? Do, or should, people value or disvalue change per se?

These are very difficult questions. But so long as we cannot answer them with any real confidence we also cannot define with any confidence even what an optimum growth policy really means.

The Role of Theory in Growth Policy

It is clear that at the present time the role that growth theory can play in guiding growth policy is somewhat limited. In comparison, say, with employment theory, we can place less confidence on the basic model of the economic mechanisms involved, have less knowledge of how to influence the mechanisms in a predictable way, and are far less clear about what we want policy to achieve for us anyway. At the present time to formulate a Theil-like model and to use it to search for an optimal growth policy is utopian.

What use, then, is growth theory in policy making? In the first place it serves to identify relevant variables and the relevant directions of

[14] For example, S. Margolin, "The Social Rate of Discount and the Optimum Rate of Investment," *Quarterly Journal of Economics,* February 1963. E. S. Phelps, "The Golden Rule of Accumulation," *American Economic Review,* September 1961, and J. Tobin, "Economic Growth as an Objective of Government Policy," *American Economic Review,* May 1964.

change. When, for better or for worse, the United States joined with the other nations of the Organization for Economic Co-operation and Development in setting a growth rate target for the 1960s, theory pointed toward at least three variables that could be stimulated: physical investment, education, and technology. This is an important and useful function, assuming that the theory is correct about the variables. Recall how for many years unemployment policy was sorely handicapped by the belief that proper policy called for balancing the budget, rather than for providing an added fiscal stimulus. While identification of variables and their qualitative impact is far less useful than a more quantitative knowledge, this kind of knowledge is still worthwhile.

In the second place, theory provides a starting place for hunting for policy instruments. Thus the knowledge that investment decisions are, to a degree at least, sensitive to after-tax profit rates led to a focusing on corporate tax rates as a variable that might be manipulated. Knowledge of the importance of R&D expenditures and of information dissemination services to technological progress led to a search for various programs and policies by which the government could spend effectively on R&D, or stimulate companies to spend more. The idea of an industrial extension service evolved. Knowledge that many persons with low incomes were being deterred from college by high costs led to the search for an effective scholarship and fellowship program. Again, it would have been more helpful if theory had specified instruments and their effectiveness, but broader qualitative knowledge at least provides a convenient starting point.

Finally, theory provides a rough idea of the nature of some of the benefits and costs of more rapid growth and, also, helps to point out the kinds of objectives with which growth has little to do or which can be achieved without growth. This last role is quite interesting, I think. In the popular discussion of growth in the early 1960s, there was a tendency to argue that more rapid growth was needed if we were to solve the unemployment problem or the poverty problem; indeed, more rapid economic growth seemed to be thought of as the universal solvent. It was only with some difficulty that economists were able to get people to distinguish between growth of potential output and growth of effective demand, and to understand that, with sensible fiscal and monetary policies, unemployment could be reduced whether or not we decided to push for rapid growth of potential output. It took considerable work to

show that by and large today's poor require special policies; growth alone is not enough. Theory has led us to stress that the issues of growth policy hinge largely around how much people were willing to give up today to get something more tomorrow. The nagging question remains: Are we right?

The Strategy of Policy Making in Situations Where Knowledge Is Mostly Qualitative

In short, existing growth theory seems capable of providing qualitative, but not quantitative, guidance; it points to the right variables and directions, but does not specify the details of what should be done, nor quantitatively how much should be done. Yet policy actions require specification of details and of quantity. How should we proceed under such conditions?

I suspect that the only sensible way to proceed is to acknowledge partial ignorance explicitly, and to view policy making as a sequential experimental decision process. What do I mean by this? The approach I am recommending has the following elements:

1. In assessing the promise of a particular proposed program the studies should not attempt to determine whether the expected return from the policy is high when the uncertainties are very great. Rather, the studies should ask: Is it quite possible that the rate of return will be high, what are the key uncertain variables that will determine whether the rate of return will be high or not, and how is it possible to find out more about these variables without very great cost? In a large number of instances I suspect the only way to get a significantly improved fix on the key uncertain variables is to try out an experimental program—this is the only way that history can be teased into creating the relevant data.

2. New programs should, where possible, be instituted on a modest scale and should be viewed as having two objectives—some alleviation of the problem the program is designed to deal with, and the creation of the data necessary to make a better analysis of the merit of the program. In other words, the program should be initiated as an experiment and should be organized so as to have a high chance of providing relevant data. A large part of the design of the program should involve a data-collecting scheme.

3. At the end of a specified number of years the experiment should be evaluated by a group who have no connection with the running of the program and no interest in it; and a study should be undertaken of the promise of a larger-scale program, utilizing the knowledge won in the experimental program. While it is quite possible that it still will be very difficult to estimate rates of return on a larger-scale program with any confidence (or even to estimate exactly what the rate of return was on the pilot program) it certainly should be possible to have a much better idea of the likely range of payoff: whether the idea is as promising as it seemed initially, whether some of the conditions which could cause the program to have a low rate of return materialized or not, what aspects of the design of the program proved most troublesome, etc.

An Example—The Proposed Industrial Extension Service

To make the discussion concrete let me cite an example—the proposed industrial extension service now undergoing hearings before Congress.

The basic policy idea is suggested by growth theory broadly defined. Recognition of the fact that the adoption of new technology takes time and that there is a gap between best and average practice leads rather naturally to a search for possible measures to increase the efficiency of the diffusion process. The data shows clearly that the rate of diffusion, even for extremely productive innovations, sometimes is extremely slow.[15] The significant differences in productivity which often exist between firms in the same industry, one would suspect, are in large part the reflection of the fact that the more productive firms are using more modern techniques.[16] While theory would suggest that there are added costs, as well as added benefits, from more rapid diffusion, the suspicion remains that in many cases the slowness of diffusion is inefficient; that if relatively low-cost measures could be found to accelerate diffusion the rate of return could be substantial.

However, existing knowledge is far from strong enough to point un-

[15] See, for example, E. Mansfield, "Technical Change and the Rate of Innovation," *Econometrics,* October 1961; Z. Griliches, "Hybrid Corn—an Exploration on the Economics of Technological Change," *Econometrica* 1957.

[16] This would appear to be the implication of the study by A. Grosse, "The Technological Structure of the Cotton Textile Industry," in L. Leontieff *et al., Studies in the Structure of the American Economy,* Oxford, 1953.

equivocally to particular policy instruments. While Mansfield and Griliches [17] have shown that such variables as the profitability of the innovation and its costs affect the rate of diffusion, our knowledge still is limited, and does not indicate clearly how policy can influence the process. One route would be to try to subsidize the purchase of new equipment or the adoption of a new technique through some sort of a special tax credit, but problems of differentiating between adoption of something new and more routine investment decisions appear insuperable; further, such a policy has the strong risk of encouraging the adoption of uneconomic new technology.

Another route seems more promising. It would appear that the information dissemination system affects the diffusion process, at least in some degree, by determining the time between the availability of new technology and when various business firms feel they have enough reliable information to decide whether or not to try it out. [18] And information services are a variable on which federal policy can operate rather directly. One potentially interesting approach might be to try to beef up the information services tailored to the needs of firms which do not possess a strong in-house scientific and technical staff; it is these firms that would appear to lag most in the diffusion process. A firm without a strong in-house capability, to be sure, does at present have many sources of technical and managerial information available to it; other business firms (particularly its suppliers), trade journals, professional consultants, contract research organizations, libraries, the Department of Commerce, and the Small Business Administration, etc. But a firm without an in-house capability often is untrustful of the sales pitch of its suppliers, unable to understand and evaluate the technical literature, unfamiliar with and unable rationally to choose among professional consultants. In agriculture this same problem was recognized long ago and a federal-state extension service designed to meet it. It might be very interesting to try out a similar service for small business firms.

This is a reasonably accurate description of how growth theory, *ad hoc* reasoning, and analogy played a role in generating a policy idea—at least to the extent that economists at the Council of Economic Advisers contributed. Without going into further elaboration, I think a case can be

[17] Mansfield, *op. cit.*, and Griliches, *op. cit.*
[18] Much of this notion is implicit in Rogers, *op. cit.*

made that the returns to such a service could be quite high. However, it would appear completely impossible to provide a quantitative estimate and, further, there clearly are a large number of uncertainties. It is quite possible that such a service would be valueless. Also there are a number of different kinds of service that might be provided and it is extremely risky to judge in advance which would be the best.

The following are just a small sample of relevant questions:

1. To what extent will business firms using backward practice be willing to listen to and be persuaded by an industrial extension agent from the state university? Can the intercourse take place largely in short courses and conferences to which business firms are invited to send representatives, or must the extension service more aggressively reach out and force its attention on the firms most in need of help—if the latter, the cost of the service per firm influenced will be greater.

2. To what extent does useful advice depend on rather intimate knowledge of the specific problems of a particular firm, in contrast with the more general knowledge of best practices and typical practice in the industry? If the former, again a much more costly service will be required, or the advice will be of much less value and relevance.

3. Will the costs, in terms of increased need for personal consultation, be significantly larger for the really backward firm with almost no in-house technical competence than for the average or just subaverage firm, with perhaps a greater facility to learn from more impersonal and formal presentations, if a somewhat smaller amount to learn? If so, then perhaps the service should focus its efforts on the average firm rather than the most backward ones.

The answers to these rather specific questions, and others like them, will in large part determine whether any kind of an industrial extension program will have a high rate of return, and if so, what kind of a program. While a limited amount of knowledge regarding them can be obtained from the experience of the several industrial extension programs that presently exist, unfortunately these programs by and large have not been collecting the relevant data. However, it should be possible to get a much better feel for the answers by running pilot programs—with statistical collection controls—in several states. Each service could be asked to keep a complete set of case records. Changes in practice of these firms contacted personally could be compared with those not contacted.

Clearly, it would be no easy job to design an appropriate data collection scheme, but some of what should be collected is obvious.

The industrial extension idea is not presented in advocacy, but rather as an example of an approach to policymaking that I suspect is fruitful quite widely in the field of economic growth policymaking. While in some cases it will not be as easy to proceed by the experimental route as in the example above—even for such policies as a tax credit on investment or for R&D or the establishment of a petit patent with a short patent life for minor inventions—it is possible both to avoid posing the issue as a once and for all decision, and to establish a data collection and evaluation procedure which will facilitate re-evaluation of the issue at some later time. As professional economists our influence and our effectiveness should be enhanced, not diminished, if we explicitly recognize and communicate that while our present knowledge provides useful guidance for growth policy, much of our knowledge enables us more to identify the relevant uncertainties than to resolve them.

COMMENT

HOLLIS B. CHENERY

It is perhaps unfortunate that so much of the recent empirical work on production functions has dealt with aggregate time series for the United States economy. Because of the relatively steady growth in labor, capital, and productive capacity, we observe only a very limited range of input combinations. The growth of output is explained almost equally well by a great variety of functions.

The main policy conclusion drawn from these studies is the importance of factors other than the increase in the quantity of labor and capital. As Nelson rightly points out, the quantitative results of testing functions with additional inputs are useful mainly in providing a plausible listing of the other elements responsible for growth.

In limiting himself to aggregate functions and U.S. experience, Nelson has chosen the type of production function research whose policy implications are perhaps the least interesting. Since I have little quarrel with his conclusions, I should like to suggest a few samples of the much richer range of results that may be obtainable when these limitations are dropped.

Sector Production Functions

Although there is little to choose between Cobb-Douglas and more general production functions as applied to aggregate data, there are important differences on a sector basis. Variations among industries in the elasticity of substitution, for example, can lead to substantial changes in relative factor proportions as the price of labor rises in comparison to the price of capital. These changes in turn affect relative prices, international trade, and the distribution of employment by sector. All of these are important in planning for future growth.

In this area, too, research has been hampered by excessive attention to the United States, where the relative prices of labor and capital differ relatively little among regions. Intercountry analysis seems to hold much more promise of being able to estimate substitution possibilities more reliably, based on the much greater variation in relative factor prices.

Intercountry Analysis

The problems of using U.S. time series to identify and interpret the influence on output of various inputs would be greatly reduced if more attention were given to intercountry analysis. The U.S. studies, for example, suggest that the growth of capital plays a relatively small part in the growth of output, even under favorable assumptions about embodied technological change. A very different conclusion emerges from studies of rapidly growing economies such as Japan, Israel, or Greece, where the growth of the capital stock can be shown to account for a much higher proportion of the total growth than in the United States.

History is not likely to perform enough experiments on one economy to permit us to deduce all the relevant properties of production functions that are needed for policy purposes. I suspect that a comparison of the growth of the several inputs and corresponding output in the United States to comparable estimates for other countries may be the best way to acquire better insight into growth policy for the United States itself. The case for intercountry analysis becomes even stronger when we consider substantial departures from patterns of output and mixtures of inputs that have been experienced in the past. Intercountry analysis therefore becomes essential to the design of growth policies for

underdeveloped countries, whose own experience is of very limited value for this purpose.

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Nelson's paper provides an exceptionally appropriate conclusion for the program of this conference on research into production functions. So far, we have been discussing what research has revealed about the theory of production functions and about the statistical determination of their parameters. This is all very well, but Nelson asks, "So what? What do we do with this knowledge?" He also asks what guidance can be derived from this knowledge that will help policymakers formulate public policies, particularly those aimed at improving the growth rate. By this he means what change in the outer limits of the production possibilities of the economy is set by the quality and quantity of inputs available or what can be done about the rate of growth in potential gross national output. He asks two questions about this: First, does our knowledge of the production function point to variables which public policy can work on to influence the rate of growth in potential output; second, does research enable technicians to predict well enough for policy purposes what the results will be of any changes that are made in public policies?

Nelson concludes that the present state of our knowledge of the production function provides us with qualitative knowledge; i.e., a sort of list of relevant variables, but knowledge is very incomplete about the form of the production function, the size of the parameters, and the links between policy instruments and the operation of the economy. He then goes on to give us some examples of how this qualitative knowledge has been applied in the Kennedy-Johnson administrations with some suggestions as to how it can be further applied.

Nelson's paper is excellent and deserves high praise as a pioneering effort. But some of its strengths are also its weaknesses, and a few comments upon these would perhaps be helpful to future workers. First, he is very correct in stressing the weakness of the growth of GNP as a measure of the growth in potential output and, further, in stressing that

there are many things about the growth of the economy that cannot be comprehended in such a measure. This has been a matter of perennial concern, for example, to members of the Joint Economic Committee, as is evident to those who have attended our hearings. But the stress put upon this reveals, at the same time, a weakness among analysts. The incompleteness of GNP as a measure of growth is confused with the related problem of measuring the changing social utilities. In general, these are aspects of dynamic change in the economy, including changes in social valuations. They are not necessarily directly correlated with changes in output of goods and services.

Again, Nelson quite correctly stresses the importance for policy of differentiating between the growth of effective demand and the growth of potential output. Clearly, these are not the same thing. But Nelson makes an error which is common in the profession of assuming that this confusion is solely an error of nontechnical people which the economists have belatedly, and with some difficulty, tried to correct. I am embarrassed to say this, but the economists were major offenders in confusing changes in effective demand with changes in potential output, partly by stressing changes in the GNP as a measure of growth. I am afraid economists themselves have been lax at times in their policy advice by not distinguishing between demand and supply as our older mentors were wont to do.

Third, I think Nelson has fallen into a familiar trap in his praise for the state of so-called employment theory, or the theory of aggregate demand as developed in the Keynesian economics which he contrasts with the rather less satisfactory state of production theory. I think this is, unfortunately, a very general belief in the profession. As a policy adviser myself, I have been appalled at times to find that the state of this so-called employment theory is such as to lead to some very poor policy advice. For example, it has been the basis for some quite false and improper emphasis on the greater growth stimulation to be obtained from changes in public expenditures compared with that from tax changes. This stems from the emphasis on the so-called balanced budget theorem in modern Keynesian economics which rests on some very unlikely assumptions, among which is the unstated assumption that changes in the structure of the economy via tax changes have quite similar results to the changes in flows of incomes or expenditures within the existing structure.

It may be of some interest for me to give some other examples from my own experience at the Joint Economic Committee of the use of the production function in formulating economic policy advice. The committee's staff has made use of this tool for a number of purposes, the majority of which may be grouped together and called examples of its use as a tool to separate long-term from short-term influences in the economy. These studies revolved around using the production function to develop a long-term full employment estimate of potential output and then using the difference between this potential and the actual GNP as a measure of short-term changes in effective demand. With this as a basis, studies have been made of such issues as differences between short- and long-term marginal tax rates; differences between the short-term and long-term consumption functions; explorations into the role of money variables; and, finally, the relative role of long-term and short-term factors in determining the profitability of private enterprise.

From the studies we have made, many of which were published in connection with the committee's reports, we have learned some lessons which may be of value to others. There is much discussion, for example, about the accuracy with which we can determine production functions and whether the results are good enough. Nelson voices some doubt on this point. We made some tests to determine the answer to this question. We made a series of projections for a seventeen-year period between 1958 and 1975, using a number of different aggregate production functions, both with generally sensible parameters and then with some that incorporated ridiculous estimates such as inverted coefficients implying that an increase in labor input reduced output. The results showed that the maximum difference after seventeen years in the estimates of the potential output was only about 12 per cent, even using the most ridiculous functions. Within the calculation period 1909 to 1958, the difference for any of the more sensible functions was insignificant for practical policy purposes. The truth was that the most important factors were the assumptions about the input of labor and capital, how they would grow in the future, and what system of measurement was used.

Another lesson we learned was connected with what may be called the Denison effect, to which Nelson refers; that is, the negative correlation between average hours of work and labor productivity. Nelson indicates that the mere hypothesis of this negative relationship is in itself useful. In the early 1950s, I made some tests for the committee

staff and found that we had to know fairly precisely the shape of this function, not just its slope. The reason is that it is almost certain that this curve is highly curvilinear. When average hours of work are sixty or eighty a week, a reduction in hours raises output per man-hour more than proportionate to the reduction of hours. But when hours of work get down to around forty per week, there seems to be barely a one-to-one effect, and at some lower point the relation becomes less than one-to-one. If this research could ever be confirmed, it would have substantial implications as to how far you could carry this policy, and probably would imply that it has been carried about as far as feasible in the United States.

I would like to close my remarks by a comment on the application of the production function to policy which is relative to a large area to which Nelson refers. The staff and members of the Joint Economic Committee have long been interested in the connection between education and training programs and the productivity of our economy. We have been particularly interested in whether or not substantial increases in the resources allocated to these programs would increase the rate of growth in potential output. However, large increases in the usual types of educational programs, such as elementary, secondary, and college education, if they are successful in increasing productivity, seem likely to bring about increments in aggregate output per man-hour twenty or thirty years after the program is started. I wonder how well an educational program could be sold now on the basis of a rather vague and indefinite estimate as to the benefits in the form of productivity increases that might be expected in 1980 or 1990? On the other hand, there must be a number of programs to train existing workers and managers which would have rather large effects within a much shorter time.

This emphasizes a point that our staff has run into time and time again in applying both production theory and other forms of economic analysis to public policy. It is of the utmost importance carefully to analyze, in each case, the sequence of events over time through which the proposed policy shifts will have their effects. It is very possible, if not indeed probable, that the lags involved between action and its consequences for the economy as a whole may be just as critical, if not more critical, than the magnitude or character of the policy change.