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THE THEORY AND EMPIRICAL ANALYSIS OF PRODUCTION

INTRODUCTION

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SOME conferences are constructive, not in having solved any major outstanding problems, but in producing dissatisfaction with received knowledge, by articulating the unresolved problems, and by airing new approaches. The conference on production functions which provided the papers for this volume was of that type. For it did provide a forum for new ideas, yet a significant part of it—indeed for some participants, the memorable part—was characterized by dissatisfaction and dissidence.

A stock-taking of the developments in the field of production since the mid-1950s could not have been the direct source of this dissatisfaction. For in that period, we have witnessed the breakthrough of Abramovitz, Solow, Kendrick, and Schmookler, who startled us by describing the enormous range of our ignorance concerning the sources of economic growth. Then, Solow, in the first part of the 1960s, taught us some of the implications for growth and production of dealing with layers upon layers of fixed assets. About the same time, the CES production function was discovered. All of these developments stimulated sufficient research to justify the belief that the field had progressed relatively far since the previous way station, which can be dated in 1948 with Paul Douglas's classic presidential address to the American Economic Association. Indeed, if these made the sum of the recent developments in production economics, then one could justifiably question the vexation expressed at the conference.

But more had occurred; there was real dissatisfaction as a result, and it was intensely expressed—in fact, the day and a half conference seemed like a winter of discontent with production economics. I would say that its sources were the following: the inability to derive in the recent past acceptable production function estimates from engineering data and, hence, in some people's opinion, to obtain structural or stable estimates; the wide and unreconciled disparities of estimates of the elasticity of substitution taken from intercountry, interregional, interindustry, cross-section, and time series data, and between small differ-

ences in time periods, thus casting doubt on the specification and results of production studies; the uncertainty that the elasticity of substitution is even pertinent to growth analysis in the short and medium run; the role of two new "impossibility" theorems which place certain restrictions on the estimation of production functions and on their interpretation; the inability to evaluate from empirical studies of production functions whether an increase in technological change since World War II has actually occurred; and the asserted lack of usefulness of the empirical results for policy purposes. We shall return to these in a moment.

In itself, dissatisfaction or controversy is not sufficient to justify a conference. The problems must be important. These issues are, for not only do they lie close to the heart of production economics, they affect as well the areas of economic growth, the distribution of income, and employment. The payoff of effective results would be quite substantial, which probably explains the considerable efforts devoted to the field in spite of the inherent difficulty of coming up with something that will satisfy scientific requirements.

Paul Douglas, in his contribution to this volume, tells us of the efforts he devoted to production economics and the various types of difficulties, professional as well as intellectual, he encountered. He recalls the very beginning; this occurred even before his collaboration with Charles Cobb that resulted in the development of the celebrated and frequently used production function bearing their names. Indeed, these experiences were the beginning of the subject itself, since he was the first to explore the problems in theory and estimation that are associated with contemporary production function analysis. Ostensibly, it may appear that we have not come very far from that period in which Paul Douglas's work generated intense controversy and unsympathetic reactions. But that is not the case, for though the controversy may be as intense as ever, it proceeds on a different basis, on the assumption that the task Paul Douglas began is salient work, and that we require more rather than less efforts toward obtaining acceptable empirical and theoretical results.

There were three sessions at the conference, the first dealing with a review of the past and the other two containing papers which presented newly developed work. It is not feasible to summarize the contents of the papers in either neat categories or succinct conclusions. In what

follows, I shall merely try to highlight the contents of the papers and the more important points of dissent and to indicate where additional work is required, though this is done at the risk of belaboring the obvious.

The first substantive point of controversy of the conference arose in connection with the use of engineering data in the estimation of production functions. Robert Solow notes in his paper that if we ignore some programming applications, little progress was made in using that type of data after the initial attempts of Chenery and Clark. Then the Kurtz-Manne study appeared, which implemented in an ingenious manner a relatively new body of engineering data for production function estimation. But Furubotn's criticism of that study, to which Solow adds his agreement, holds that the production function estimates are unstable, that they depend on relative factor prices. Though the Furubotn and Solow criticisms went unchallenged at the conference, some participants—the oral remarks of Chenery, Easterlin, and Leontief could be interpreted in this way—voiced the feeling that even though previous efforts to estimate engineering production functions encountered aggregation and other unresolved problems, researchers should attempt to obtain such estimates. Indeed, notwithstanding these very difficult problems, severe skeptics of the aggregate production function approach maintained that we would succeed in obtaining structural estimates of production functions only with engineering-type data. In his paper, Solow takes a compromise position between the engineering and aggregate approach. Also, a compromise is offered by the Gort-Boddy and Eisner papers, which employ individual-firm data. But no consensus was reached as to the desirable research strategy, though it is safe to say that engineering production function studies would receive wide support and encouragement.

Solow reviews a number of theoretical devices—some old, some new—which he believes offer promising research leads. Underlying the new device, the invention possibility frontier, is the notion of factor-augmenting technical change. This assumes that technical knowledge augments the efficiency of one or the other inputs within a stable production function. Solow emphasizes that the advantage of this scheme is that it gives "something" to estimate or talk about. But there is a cost in its use; the scheme does not encompass changes in the elasticity of substitution or in the exponents of the Cobb-Douglas production function as a result

of changes in technical knowledge. Both Solow and Tobin indicate that the factor-augmentation representation of technical change, though perhaps useful in empirical work as well as in growth theory, may be misleading if improvements apparently embodied in a factor are identified as augmenting it. The idea turned up again in empirical discussions at the conference when it was noted that education probably augmented both capital and labor; hence, in the specification of production functions, education should not only multiply the labor input, but other factors should be adjusted as well for quality change in labor.

In Kennedy's pioneering paper on induced technical change and in the subsequent work by Drandakis-Phelps, Samuelson, and Weizsäcker, the factor-augmentation scheme and the invention possibility frontier are used to great advantage. For, given the frontier, a choice can be made of the degree of augmentation of each factor, and hence the degree of technical bias is determined within the system. If a saving function and an assumption concerning the growth of labor is added, factor shares and growth paths are determined. This is a considerable bounty of results. Yet, though Solow noted some shortcomings, Tobin asked the really basic question—essentially, how does it work for the individual firm? Is it operationally meaningful in a microdecision setting?

Additional research openings are discussed by Solow—namely, Kaldor's technical progress function, Arrow's learning-by-doing model, extensions of the CES model, and Houthakker's aggregation theorem. Tobin contributes to the list by pointing to two omitted problems that he feels merit further work, the treatment of depreciation and the role of short-run fluctuations in production analysis.

Marc Nerlove commences his review of the recent empirical studies of production with a discussion of the proposition, derived by Richard Nelson, concerning the implications of the elasticity of substitution for medium-run growth. Zvi Griliches also cites the Nelson result. The result can be stated as follows: No matter what reasonable values the elasticity may be taken to be, increases in the capital-labor ratio explain only a small fraction of the growth in productivity in the postwar period. If this is indeed the case, then for growth analysis one may not need to worry himself about specifying and estimating the relatively complex CES production function; one need only use the Cobb-Douglas function, which is considerably simpler. Nerlove challenges this from two points of view: Over long periods, it is certainly important to know the value

of the elasticity of substitution; and, moreover, when capital and labor are measured in efficiency units one cannot say that elasticity is *a priori* irrelevant. Brown goes even further and attempts to show that the elasticity of substitution is really a first-order parameter and hence is relevant to medium-run growth analysis. As with most of the controversies that took place in the conference, this had no conclusive ending.

It was Nerlove's review of the recent attempts to estimate the elasticity of substitution from various types and bodies of data that detonated the most resounding reaction of the conference. For there are significant differences among these many estimates, and there is no clear-cut explanation of their diversity. With respect to the cross-section estimates (interindustry, intercountry, and interregion), Nerlove presents the Liu-Hildebrand generalization of the CES model, employing it in his search for an explanation of the diversity of the estimates. It does not turn the trick. However, Griliches suggests an alternative interpretation of that model, which, if valid, implies that it may not be the appropriate model, after all, on which one should base such an explanation. Nerlove takes note, as does Mansfield and many others, of the sensitivity of the results to variations in the specification of the equation fitted and to the data used.

No relief from unreconciled discrepancies is forthcoming from Nerlove's review of time series studies of the elasticity of substitution, either. This section is prefaced by Arrow's formulation of the Diamond-McFadden "impossibility" theorem which sets out conditions for estimating the elasticity of substitution and the bias of technical change from time series. In Arrow's formulation these conditions are not excessively restrictive, although Nerlove criticizes some studies for violating them. Brown relaxes their restrictiveness even more, the result being that on most reasonable assumptions it is not "impossible" to estimate these two characteristics.

One strong impression received from the review of time series and cross-section studies is that biases and technical difficulties in the various studies make it difficult to square the results with each other. Nevertheless, a pattern emerges of differences between cross-section and time series estimates of the elasticity of substitution—namely, the former are generally larger than the time series estimates. This is reinforced if one considers the elasticities computed by Griliches in the introduction to his paper for the conference. After reviewing his cross-section estimates

and those contained in time series studies, Griliches throws his support to the cross-section side of the controversy. Boddy addresses some pertinent remarks to the discussion. For him, both time series and cross-section estimates are biased, but the latter type are probably more so. The bias in cross-section elasticities derives from the relationship used in the estimation procedure, which specifies fixed assets constructed in the past (and based upon factor prices in the past) as a function of current factor prices. Clearly, there are some open questions here, but one can say, on the basis of the participants' reactions to the disparate estimates and to the controversy—some "shuddered," some "despaired," and one observer to the conference quipped to me in the hall: "You should go into a respectable field, like astrology"—one can assert that these problems represent a major obstacle that must be removed before significant progress can be made.

Mansfield's comments are really complementary to the Nerlove paper; he discusses Jorgenson's new "impossibility" theorem, focuses on the consistency between estimated rates of technological advance in various industries, and whether it has increased from the pre- to the post-World War II period. Intransigence appears to characterize this important area, also. In particular, about half of the industries Mansfield reviews showed a higher rate of technological change in the postwar period than the pre-war period and about half showed a lower rate. No unambiguous conclusion can be drawn.

The paper contributed by the Canadian authors—Lithwick, Post, and Rymes—served to introduce the newly developed work presented at the conference. As noted by their discussant, Derek White, this may well be a landmark on the recent economic scene in Canada; but no provincial conclusions should be drawn from this, since the theoretical and empirical results, especially with respect to their work on total factor productivity, transcend the Canadian economy. With the aid of new capital stock data, similar in concept and scope to the U.S. capital data produced by the Office of Business Economics, the authors compute total factor productivity, measured in two ways: by means of proportionate rates of changes in the prices of inputs and outputs and by the conventional real method. The former method constitutes an innovation in productivity measurement. They find that as in the United States and many other countries, most of the change in output per man-hour is attributable to some residual influence. In another part of their paper the authors review the

Cobb-Douglas production function studies of Canadian data and derive a major conclusion: Intersectoral movements of resources are relatively more important in Canada than in the United States in promoting growth. The authors enter the controversy centered around the use of net or gross stock in production functions—a controversy that reappears in somewhat different guise in the discussion of the Griliches paper—and come down heavily on the net stock side. Their statement of the problem and their rationale for selection of the appropriate stock concept is an excellent representative of the traditional approach.

The principal purpose of Zvi Griliches' paper is to explain the major sources of productivity growth in United States manufacturing in the postwar period. He employs the Cobb-Douglas production function, and presents cross-section estimates of the CES productivity equation to show that the assumption of unitary elasticity of substitution is not inappropriate for his data. Two important conclusions are drawn: Labor quality emerges as a significant explanatory variable, and mild increasing returns to scale with respect to capital and labor appear to be present. Bodkin takes up a point noted earlier in the conference—that the factor-augmenting properties of the labor quality variable, which is essentially educational attainments of the labor force, imply that this variable may not enter multiplicatively with raw labor (especially after Griliches introduces industry and state dummy variables into his cross-section analysis). For a different reason—namely, the measurement of the labor quality variable itself—Popkin feels that the effect of labor quality is overestimated. This, together with the discussion of the same issue by Weisbrod and by Brown and Conrad, leave one with the decided opinion that the measurement and specification of labor quality in production function analysis requires considerably more work.

Bodkin raises a very interesting point with respect to the calculation of the effect of economies of scale upon productivity measures. Griliches, in his calculation, corrects for the changing number of firms, but Bodkin suggests that this may be unjustified if the aggregate production function measures Marshallian external economies rather than economies of scale specific to the individual firms. Clearly, not only does the technique of measuring gross economies of scale require additional attention, but the external-internal returns problem is ripe for examination.

It is difficult for a paper that has the audacity to use a capital stock

concept to escape challenge, and Griliches' paper is no exception. Neisser and Brown maintain, for different reasons, that his treatment of capital is inconsistent, that his concept of rent is incompatible with a service concept of capital, and that the use of depreciation allowances introduces an element of embodied technological change via the obsolescence component which also is inconsistent with his service concept of capital. Griliches' reply adds several interesting dimensions to the discussion, but this problem and the one covered by Lithwick-Rymes-Post deserve more extensive treatment. Finally, a general and valuable point raised by Domar is that one must be careful to adjust the output side when one adjusts inputs for embodiment or quality change; Griliches notes that this does not apply to his study.

Though the main objective of the Brown-Conrad paper is similar to the Griliches study—namely, the explanation of the sources of productivity in postwar United States—they differ in several respects. The former study uses the CES framework, it employs a covariance analysis on pooled time series and cross-section data, and it includes a research variable as well as education as a source of productivity change. Its principal conclusions are: A given percentage increase in education and research in durables goods industries produces a substantially larger percentage increase in productivity than does the same percentage increase in research and education in the productivity of nondurable goods industries; there is a suggestion that a malallocation occurred in the period under consideration, that the system did not allocate education and research resources between the two types of industries in an efficient manner.

The discussants challenged the data and its transformations. Terleckyj focused on the author's input-output specification of research, which was an attempt to quantify the external benefits to an industry of the research conducted in other industries. The rationale behind the author's specification is that research resulting in new or improved products is not reflected in output as measured in the national income accounts. Terleckyj felt that Brown and Conrad should have separated the internal and external benefits of research in their estimation procedure. Weisbrod raises some additional questions concerning the possibility of biases in the treatment of this variable, but his main attack is reserved for the education variable. There is no reason to reproduce the arguments against, and in defense of, the treatment of education, for as noted

above in the discussion of Griliches' paper, this is an area in which a consensus is quite a way off and certainly requires more detailed study.

A serious technical criticism is raised by Griliches to the Brown-Conrad analysis, which employed a modified stepwise estimation procedure. This procedure is inefficient and inconsistent (in the statistical sense), but the authors use the reply to include additional tests which do not have those properties and find that both sets of results yield the same conclusions. Other criticisms were directed to the Brown-Conrad study, perhaps the one of note being their assumption of competition (see Weisbrod's discussion). Although the authors do not accept the implied criticism, it opens up a very difficult area in the field of production which must be added to the list of those that require exploration.

Both the Gort-Boddy and Eisner studies utilize the Cobb-Douglas production function (though Eisner does experiment with direct estimates of the CES function in an appendix), and focus on individual-firm data. The latter aspect of these studies is an auspicious one, for, as noted above, it represents a compromise between the aggregative approach and the requirements of engineering production functions. An innovation of the Eisner paper is the explicit introduction of a capacity utilization variable that is measured independently of the output which is to be explained. This plays a major role in the time series tests and in reconciling the disparate cross-section and time series estimates he obtains. With respect to the cross-section tests, Eisner finds that constant returns to scale are present (compare this with Griliches' findings of slightly increasing returns to scale and with Hickman's comments), and that the labor and capital elasticities of production are precisely those found by Paul Douglas in his pioneering studies for the United States. This is emphasized by Jorgenson, who feels that Eisner's model, by and large, explains the data.

Hickman does not concur with his fellow discussant. He challenges the time series experiments performed by Eisner, the utilization variable used in the study, and the particular specification of that variable. In general, he would be unwilling to accept at face value the economic implications of the Eisner study. He has praise for the general approach though, and joins Tobin in calling for more work on the problem of short-run production movements. With Domar, he also notes that Eisner uses gross output as a dependent variable but fails to include materials among the factors of production. However, since Eisner's paper is a

progress report on a continuing research project, it may well be that he will confirm in time Douglas's major findings, provided that it can be shown that the elasticity of substitution is approximately unity, and also that one can reconcile the disparate estimates in the manner he indicates.

Gort and Boddy pose a difficult and fundamental problem concerning the effects of interactions between past and present investment. Suppose, they say, that each vintage capital item is associated with a unique production function so that their aggregate is a production function of the embodied type; this may be a serious misspecification if the different vintage capital items are interdependent, i.e., if the investment today depends, say, on the capital items put in place yesterday. In short, they assert that to a large extent a firm's investment decisions depend on its investment biography. The authors present a scheme for handling the interdependence of vintage capital items which specifies a production function (for simplicity, they use the Cobb-Douglas type) in which the capital variable is a distributed lag in investment flows. This permits a representation of the interactions between successive investments, since the coefficients on the vintage investment terms are allowed to differ. The main results of their tests of a simplified version of the model on cross-section data for the electric power industry tend to support the interaction hypothesis. There were several expressions of support for pursuing the type of analysis proposed by Gort and Boddy, and though Anne Carter voiced serious reservations about the empirical results, both discussants endorsed it. There is little doubt that the micro-orientation of the Eisner and Gort-Boddy studies, and the interaction property of investment in the latter study will receive considerable attention in future work.

Richard Nelson's paper ventures to initiate a dialogue between the fields of policy and production functions. Though little formal work in effectuating a dialogue has been attempted, Nelson's paper and the discussion by Knowles and Chenery indicate that this is a legitimate research field in its own right. After critically examining the conventional methods of formalizing and measuring technological change in a production function framework, Nelson discusses the instruments that can influence production, with special reference to research activities and education. He maintains that, even abstracting from the indirect and perhaps tenuous connection between research expenditures and production, it is unlikely that the government can significantly increase research

spending in nonmilitary areas. The situation is somewhat better with respect to education because the government can spend directly in this sphere. Yet, Nelson argues that simply providing more money to educational institutions may not result in a real increase in relevant education imparted to students. He is slightly more encouraging with respect to theoretical developments and policy formation, for, at the minimum, these developments have articulated the list of relevant variables that influence production and that can be employed as instruments.

The discussion of Knowles and Chenery is complementary to Nelson's paper. Both discussants suggest other fields in production function research that have important policy implications. Knowles urges additional work on the relation of average hours worked to productivity and between education training programs and productivity. Chenery points out that sector production functions may indicate differences in wage policy between sectors if the elasticities of substitution differ between them. He also emphasizes the important insights for the growth policies of the United States and the developing countries that can derive from intercountry analyses of production functions.

As a final note to this introduction, it might be said that the conference unequivocably demonstrated that we really know very little about production relative to what we would wish. This gap between attainments and desiderata, a natural state of affairs in all research fields, is particularly large here. But when we consider the amount of dissatisfaction with the present condition of production economics, the commitment to the belief that it is a fundamental and important area, and the growing number of people working on its problems, we must view our position, perhaps not quite as a beginning but certainly not as an end.