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PRODUCTION FUNCTIONS, TECHNOLOGICAL CHANGE AND THE DEVELOPING ECONOMIES: A REVIEW ARTICLE*

by

Jeffrey G. Williamson, 1935-

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by

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"The objection is raised that the production function is a fiction . . . not directly measurable ... foreign to the world of common sense ... a fiction fabricated by marginalist economists. /The/ employment of production functions can be justified simply on the ground that it produces highly useful and verifiable hypotheses."

Murray Brown, 1966.

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* A joint review of Murray Brown (ed.), The Theory and Empirical Analysis of Production, (New York: National Bureau of Economic Research, 1967), pp. x, 515; Murray Brown, On the Theory and Measurement of Technological Change, (Cambridge: Cambridge University Press, 1966), pp. xii, 214; Lester B. Lave, Technological Change: Its Conception and Measurement, (Englewood Cliffs, New Jersey: Prentice-Hall Inc., 1966), pp. xx, 228. Earlier discussions with José Encarnación, Gerardo Sicat and Don DeVoretz were very helpful, as usual.

1. Introduction.

The quotation above reveals Professor Brown's impatience with the vociferous critics of the recent research on production functions. The level of criticism becomes even more intense when dynamic shifts in production functions are entertained. Combine this with a macroeconomic approach and it leads Professor Hicks [1965] in exasperation to call it an artificial construction irredeemably far removed from a static plant production relation. If such constructs are to be welded onto growth models, Hicks suggests we regard them as labor or capital functions and nothing more.

But Professor Hicks goes on, and we would be foolish to ignore him, to criticize a very specific hypothesis about aggregate production relationships: "It is very wrong to give the impression to a poor country, which is very far from equilibrium even on a past technology, that capital accumulation ... is a matter of minor importance."[1] Hicks, of course, is referring to the Golden

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1/ Hicks [1965], p. 304.

Age models and to the early research of Abramovitz [1956] and Solow [1957] in which technological improvements are disembodied. The latter are the "old school" approaches to aggregate production function analysis and the famous empirical results stemming from that hypothesis are known to all. The ensuing focus of professional research on the determinants and character of technology and its change has been intense. Much of the subsequent inquiry into forms of the production function are attributable to those startling results that capital formation explained very little of long term growth.[2] Certainly much of the work devoted to competing hypotheses, such as various forms of the embodiment model, can be explained by a common sense objection to the assumptions of the old model as well as the dismal pronouncement it made regarding the economist's favorite policy tool - his

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2/ Lave states the case with even greater strength (p. 131):

> "The interest in technological change has given rise to a derived demand for better estimates of production functions. If we are to measure technological change correctly, some general idea of the production function is necessary."

influence on the pace of capital formation. Furthermore, persistent problems of structural unemployment are certainly related to the character of technological change and the ease with which factors are substitutable for each other, and where else is the unemployment problem more severe than in Asia? Finally, a solution in the early 1950's, at least intellectually, to the problems of inadequate aggregate demand turned economists' attention once again to the classical concern with policies which optimize the returns to scarce resources. And as Murray Brown /1966, p. 2/ reminds us so well, technology sets the conditions for optimum resource use and further a change in technology alters that optimum solution. In short, the results of the early research on technical change (or, as Domar /1961/ insists, the Residual) by Abramovitz, Solow and others has initiated an enormous amount of subsequent inquiry into the theory of production in both its static and dynamic contexts. The impact, in turn, on growth and development theory has been equally profound as a quick review of Hahn and Matthews /1964/ will attest. The theory of production is, after all, at the heart of economics and when that theory "is cast in macroeconomic terms it merges imperceptibly

with the theory of economic growth."[3/]

Strangely enough, until recently almost all of the research on aggregate production functions and technical change has been applied to developed nations. Certainly very little has been done for developing Asian nations. Perhaps it might be useful to start there since it illustrates how useful the aggregate production function approach is and how we must modify the approach when applied to Asian economies. In the past few years the model has been applied to Latin America in general, Chile in particular, Greece, Taiwan, Mainland China and the Philippines.[4/] In the Latin American and Philippine cases, the Abramovitz-Solow model is exceedingly effective in attaching a crude estimate to the costs of resource misallocation associated with excessive policies of import substitution. That is, those studies persuasively argue that most of the secular variation in the Residual can be explained by the varying ability of these economies to allocate resources in a

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3/ Solow, in Brown [1967], p. 28.

4/ Bruton [1967], Harberger and Selowsky [1966], Bowles [1966], Liu [1967], Williamson [1967, 1968] and Lampman [1967].

nearly optimal fashion. Here again little of output growth is explained by capital formation. But, contrary to the apparent results for advanced economies, we are not observing variations in the rate of "pure" technical change in these developing economies. We are instead observing considerable variety in their ability to approach an optimal allocation of resources. To the extent that these tentative results are confirmed by other more detailed studies, then it clashes sharply with the conventional conclusion that the gains associated with a movement towards optimal resource allocation are small. It also suggests that a great deal more research on technical change and production functions needs be done for the developing economies. Finally, it illustrates quite dramatically some of the weaknesses in the recent work on production functions as it relates to the environment of the developing economy. Basically, there are four main shortcomings revealed in the above research and the volumes presently under review. These shortcomings are recognized by all practioners in this area but they seem especially worth emphasizing in the developing economy context. First, the problems of aggregation are unusually difficult in the developing nation and the process

obscures much of what is interesting in these economies. Second, competitive conditions in goods or factor markets do not even come close to being satisfied there. Third, much of the recent research assumes instantaneous adjustment by entrepreneurs to exogenous price changes. This assumption is especially questionable when applied to the developing economies of Asia and Latin America. Fourth, not enough research has been done on short run production functions which explicitly allow for departures from full capacity utilization. We return to each of these points in detail in the course of this review.

2. The Present State of Production Economics.

The three books on review here certainly yield a variegated harvest from a field which has had little difficulty attracting abundant resource inputs over the past decade. The flow of new inputs into the area has been so rapid, in fact, that a review of these books published between 1966 and 1967 is already hopelessly out of date. The NBER volume represents the careful editing of papers presented at an October meeting in 1965 and thus already

leaves us three years behind in an industry where capital obsolesces at enormous rates.

The books complement each other in many ways. Professors Lave and Brown are concerned specifically with technological change, while the NBER volume is eclectic in its approach. Professor Brown carefully develops a theoretical framework and has a particular point of view while the others do not. Lave's book maintains an annotated bibliography approach with heavy emphasis on reporting the empirical results of others. The NBER volume is excellent as a reference and as an indicator of present research trends.

Professor Brown's book, On the Theory and Measurement of Technological Change, should have a fairly long life. Until his book appeared, only Salter [1960] had made a serious theoretical attempt to redress the neoclassical under-emphasis on technology. The result was a book full of insights and which now appears on everyone's reading lists. The need for a textbook in this field is obvious as more and more of our professional efforts are devoted to a quantification of the sources of economic growth. The book is broken up into three parts. Part I is in many ways

the best as it presents a very careful development of the basic theory. It begins with an elementary classification of four characteristics of the production function an understanding of which are crucial to discussions of technological change: (1) efficiency of the technology, (2) the degree of economies of scale that are technologically determined, (3) the degree of capital intensity of a technology, and (4) the ease with which capital is substituted for labor. From there Brown proceeds to specify a definition of non-neutral technological change. This step is critical since Brown wishes eventually to develop neo-classical production theory within which an unambiguous measure of non-neutral technical change is possible. The remainder of Part I deals with changes in technology and output in the Cobb-Douglas and CES worlds, deals with the distinction between long-run, short-run, and secular production processes, and finally confronts models of embodiment. Throughout the mathematical properties and economic interpretation is made both understandable and elegant. Part II contains a lucid review and critique of the methods of measurement associated with the names of Kendrick, Abramovitz, Solow, and Salter in terms of the coherent theory developed in Part I. (Many

readers will prefer these pages in Brown to most of Lave's book. Lave's book, Technological Change: Its Conception and Measurement, suffers from an unwillingness to present the basic theory underlying the empirical results. Brown makes no effort, on the other hand, to summarize other people's numbers for the reader.) At this point, Professor Brown departs from the textbook format and passes on to a summary of his econometric work already published with de Cani, Popkin, Conrad and others. Utilizing the Cobb-Douglas production function and hypothesizing disembodied technical progress, Professor Brown in Part III estimates the sources of growth in the American non-farm private sector. The vision is Schumpeterian where intervals of time are isolated in which production function parameters are stable but different from contiguous intervals. This "epoch" approach will be familiar to those who have read Murray Brown's contribution to the literature over almost a decade. The decomposition of sources of output growth differs from the Griliches or Denison approach which emphasizes proper input measurement. Brown decomposes American growth into that attributable to increases in inputs, non-neutral technological progress, neutral

technological progress, and changes in the degree of returns to scale. The pace of neutral and non-neutral technical progress accelerated from 1890-1921 to 1921-1960. A switch from labor-saving to labor-using technical change took place between the two periods. An increase in the degree of returns to scale was beneficial to growth only in the second of the two periods. And so on.

Quite an impressive result but this reader, at least, is left unconvinced. First, as Brown himself points out, his form of the production function requires an arbitrary separation of economies of scale effects and non-neutral technical change effects. Those parameters of the production function which shift in response to non-neutral technological change are also utilized to estimate the economies of scale effect. Second, identification of the epochs themselves is absolutely crucial and the epochs are very sensitive to the procedures used to isolate them. The parameters of the production functions certainly vary enormously with the choice of epochs. For these reasons and others, Brown's results must be viewed with considerable skepticism. The book will be read primarily for its first two parts.

The NBER volume edited by Murray Brown, The Theory and Empirical Analysis of Production, contains papers of varying quality and interest. Judging by the published comments and by the editor's summary, the conference papers triggered discussion even more exciting than the papers themselves. Somewhat fittingly the volume begins with some reminiscing remarks by ex-Senator Douglas. The first two papers to follow, one by Solow and one by Nerlove, are probably among the most rewarding for the non-specialist.

Professor Solow offers an excellent review of the recent past and indicates the directions in which future research on production economics might go. First, Solow introduces factor-augmenting technical change. Embodiment of technical progress in successive vintages of gross investment is an extremely useful idea, for which Solow himself is largely responsible. But as Tobin points out, "its appealing simplicity does depend on the aggregation of all investment of one vintage into one homogeneous productive factor. The model becomes very complicated if a variety of capital goods, obsolescing at different rates, is allowed - plant, equipment, inventories, houses, consumer

durables."[5/] Shades of Joan Robinson. Nevertheless, empirical work and growth theory has found the assumption that technical progress augments one input or another very useful. Both Solow and Tobin point out that improvements embodied in a factor need not augment it. As we are learning in Southeast Asia, education of farmers may be land-augmenting not labor-augmenting. Furthermore, Nelson [1964] has emphasized that education may enlarge the choice of technology rather than simply stretch man-hours, as assumed by Denison, or in the developing economies case in the work of Williamson [1967, 1968] and Harberger and Selowsky [1966]. Suppose we let

$$Q = F[a(T)K, b(T)L]$$

where T is a parameter representing a level of technology and a change in a and b are K and L augmenting, respectively (but they need not be K or L specific). Obviously, if $a(T)/b(T)$ = constant, then we have Hicks neutrality, while it has been shown that Harrod neutrality results when $a(T)$ = constant, e.g., when technical progress

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5/ Tobin's remarks are on page 52. Mrs. Robinson [1962] made similar criticisms of Solow's [1957] model. Samuelson [1962] came to Solow's rescue but in the process had to struggle with some very restrictive assumptions.

is purely labor-augmenting. Now we can avoid the old restrictive notions about neutrality and talk in terms of the rate of technical progress and its "bias" which becomes a function of the rate of change in a and b and the elasticity of substitution (the latter being kept constant).

This leads us quite naturally to Solow's second point, the new theories of induced innovation where the factor-augmenting scheme and the invention possibility frontier play crucial roles. Since there is no accounting for resources used up in research, technical change still might be viewed as autonomous. (At this point, a large share of our colleagues may get up and leave. If we have learned anything in the past few years, it is that technical change can be "expensive." This is certainly the message of the Brown and Conrad paper which was read at the same conference. Brown and Conrad emphasize R & D and education as an explanation for discrepancies in intra- and inter-industry efficiency. The allocation of education and research resources among alternative industrial uses then becomes crucial - a point that must be emphasized with planners in Asian and Latin American economies. It is a

point that receives considerable attention in modern textbooks on development economics, especially regarding the R & D resources required to adapt imported machinery to indigenous relative factor prices. Bruton [1965], Chapter II.) The new theories of induced technical progress assume that a choice is being made in the aggregate among all possible combinations of a and b growth. These combinations trace out a frontier

$$I(g_a, g_b) = 0$$

where g_a and g_b are growth rates. Only a limited improvement in a or b is possible at one point in time. The best pair (g_a, g_b) is chosen to maximize the rate of "technical progress", and that depends upon parameters in the production function. Given competitive assumptions, the factor shares are crucial. At this point, Tobin again raises a critical question: how does it work for the individual firm? Silence. To the underdeveloped economy purchasing machines abroad, these issues are obviously important and some concrete answers are urgently needed. The present writer, at any rate, feels that adequate answers can be found only by a return to Salter's [1960] best-

practice-technique approach formulated at the firm level. In any case, it would be more helpful to emphasize the resource costs and returns to alternative technological adaptations in the developing economy to best-practice-techniques embodied in machinery produced abroad.

Let us retrace our steps for a moment back to the embodiment issue. Solow's notion was, of course, that "improvements in technology affect output only to the extent that they are carried into practice either by net capital formation or by the replacement of old-fashioned equipment by the latest models, with a consequent shift in the distribution of equipment by date of birth."[6] The "new view" of investment apparently put capital formation back in a prominent position as a vehicle for growth: it rejected the conclusions of the "old view" that, assuming disembodiment, little of output growth could be explained by capital accumulation. Because Solow's model yields difficult estimation problems, Nelson [1964] offered a promising approximation to it. The effective stock of capital, $J(t)$, can be approximated by a function of the gross capital stock, $K(t)$, its average age, $\bar{a}_t$, and the historical rate of

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6/ Solow [1960], p. 91.

productivity improvement of new capital goods, λ :

$$J(t) = B(1 + \lambda)^{t} K(t) \left[1 + \lambda (\bar{a}_{t-1} - \bar{a}_{t})\right].$$

Nelson finds that the variations in American 20th century productivity growth, the behavior of the Residual, can be explained almost entirely by variations in the average age of the capital stock - rather than by variations in λ , even if we assume full embodiment. The implications here are quite straightforward. In the American case, and if we accept the embodiment model in toto, then the rate of capital accumulation becomes the key explanatory variable of output growth since the average age of the capital stock is an inverse function of the rate of investment. We should indicate that a similar experiment performed with postwar Philippine data suggests quite the opposite. (Williamson [1968].) In the Philippine case, full acceptance of the embodiment model still requires an explanation of variations in λ . Very little of the variation in the rate of total factor productivity improvement would have been due to different rates of growth of capital and labor and different trends in the average age of capital.

We should also point out that Brown finds no conceptual difference between the old and new views of investment.7/ Under the usual stringent competitive assumptions, the value of vintage capital declines relative to the value of new capital in the same fashion that vintage capital's marginal product declines relative to new capital's marginal product. Thus, the obsolescence component in the declining balance depreciation is clearly equal to Solow's productivity improvement factor and depreciation rate. As long as asset valuations reflect perfect foresight, the old style model, which utilizes a properly estimated net stock concept, would be accurate. The net stock would be measured by the real market value of those machines including the discount to reflect the obsolescence rate. Under these conditions, the disembodied and embodied models are two different ways of conceptualizing the same problem. In practice, however, they yield different results since net stock figures do not conform to theoretical requirements nor do competitive conditions prevail in developing economies. Thus an interesting econometric issue still

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7/ The relevant pages in Brown are pp. 77-92. A somewhat different theme is developed in Phelps [1962].

remains. Not enough research has been devoted to it utilizing data drawn from the underdeveloped economies where λ may be determined exogenously (the rate of productivity improvement in foreign capital goods sectors) and where $\bar{a}_t$ is undergoing dramatic secular changes.

Third, and here we move quite smoothly into growth and development theory, Solow reviews Kaldor's [1957, 1962] technical progress function as it appears in his recent models of growth.[8] Kaldor's technical progress function competes directly with the vintage model of production. Kaldor's early argument was that productivity would increase through general technological drift, but that an increase in capital-labor ratios would generate even greater productivity increases but at a decreasing rate. Very little empirical testing has been applied to this hypothesis. It is of some interest to note that the technical progress function can be expanded by iteration to illustrate that "production possibilities at any one

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8/ The concluding pages of the Kaldor [1957] article are addressed specifically to the developing countries undergoing changes in economic structure and tending towards inflation and inequality.

point of time depend on the whole path the firm or economy has followed in the past."9/ Like so many modern theories of growth and production, this one makes us prisoners of our past and devotees of economic history. Arrow's learning by doing model shares this characteristic as do other models currently in vogue.

Fourth, Arrow's /1962/ learning by doing model has been offered to articulate the sensible idea that technological change grows out of experience and that experience can be measured either by cumulative gross investment or output. In Arrow's model, then, technical knowledge is not assumed to be autonomous. At the micro level, the hypothesis predicts that the longer the production run the lower the average costs. The Arrow model has profound consequences for growth and development theory as well as policy. The infant industry argument, of course, can be derived from the learning by doing model. Perhaps more interesting, however, are the consequences for the literature on optimal saving especially as it applies to

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9/ Solow, in NBER /1967/, p. 36.

the developing economy. The Arrow production function exhibits increasing returns to scale. All factors are rewarded their private marginal product but accumulated gross investment increases technological efficiency and thus raises the social marginal product above the private marginal product. Since the discrepancy may be substantial, the rate of investment (saving) under competitive conditions may be far lower than the optimal rate or one which might prevail under a centrally planned regime. The hypothesis is certainly an appealing one which can easily be formulated at the micro level but very little empirical exploration has been done with it. It seems especially imperative to explore the hypothesis with data from developing economies but as yet the results for both advanced nations (Sheshinsky [1967]) and developing nations (Williamson and Sicat [1968]) are quite mixed. Certainly the hypothesis is deserving of more critical scrutiny than has yet been forthcoming.

Following Professor Solow's stimulating remarks on the state of production theory, the rest of the NBER volume is devoted primarily to empirical problems.

it should be clear that the output rate should always rise. "If technological progress permits the substitution of the relatively cheap factor for the relatively expensive one with greater ease, the same rate of output can be maintained at lower unit costs: therefore, with a fixed budget outlay a higher rate of output can be obtained."10/ Presumably, the developing economy can increase its growth rate simply by raising σ . The question is: by how much (and at what cost)?

Nelson /1965/ argues that it's not much. He approximates output growth by

$$\frac{\dot{Q}}{Q} = \frac{\dot{A}}{A} + b_o \frac{\dot{L}}{L} + (1 - b_o)\frac{\dot{K}}{K} + \frac{1}{2} b_o(1 - b_o) \frac{\sigma - 1}{\sigma} \left[\frac{\dot{K}}{K} - \frac{\dot{L}}{L}\right]^2$$

where b_o is the initial labor share, $\dot{Q}/Q$ output growth and $\dot{A}/A$ disembodied technical change. The last term on the right-hand side of this expression is the CES adjustment. If $\sigma = 1$, as with the Cobb-Douglas function, then

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10/ Brown /1966/, p. 24.

the expression reduces to a more familiar growth equation of the Solow [1957] type. If however $\sigma > 1$, then the last term becomes positive and increasingly so as σ increases and/or the rate of capital deepening increases. The greater does σ exceed unity, the larger will be the underestimate of output growth given the rate of disembodied technical change: or given the growth rate of output, the more will the rate of technical change be overestimated. But Nelson goes on to illustrate that under reasonable assumptions about b_o, capital stock growth, and labor force growth, the last term drops to insignificance. Nelson's argument is quite clear: the more complex CES function doesn't predict growth rates any better than the simpler Cobb-Douglas function, at least in the medium-run.

Nerlove however reconfirms our faith in the importance of the research which searches for the "true" value of σ. This reviewer comes away convinced that the argument is even more persuasive for the developing Asian or Latin American economy. First, there is some evidence that rates of capital formation are higher in many developing economies which begin from low capital

stock bases. This increases the relevance of σ (and its behavior over time) as a first order parameter. Second, if we accept some form of the embodiment thesis then rates of capital stock growth should be far greater than those predicted by the naive "old" disembodied model (which uses imperfect measures of the net stock of capital). Even investment in human capital may be, in part at least, capital-augmenting. Third, and most telling, the aggregate σ in the developing economies is likely to undergo far more dramatic secular variations, even in the medium term, than that of the advanced nations. Currently developing economies are undergoing sharp secular changes in economic structure associated with their efforts toward industrialization and diversification. With a given growth of factor supplies, resource shifts to different industries may have a profound effect on the overall σ if there exist significant industry differences in σ. Accumulated research confirms that such variety does in fact obtain. All of this emphasizes the importance of empirical production function research for the developing economies and experimentation with forms of the CES function.

The fireworks started at the conference when Nerlove indicated the enormous variety of results for similar data drawn from similar industries under various estimation techniques. The discrepancies among these estimates certainly must be reconciled before significant progress can be made. Mansfield's remarks on Nerlove's paper is complementary in the sense that there is little consistency in the various American estimates of rates of technological change either. Apparently we cannot even agree whether the rate of technological change increased between the pre- and post-World War II periods! The Nerlove and Mansfield results must have accounted for much of the gloom generated by the conference.

The remainder of the NBER volume deals with more recent research on production relationships. From the point of view of economists interested in the developing economies, the results of the Lithwick, Post and Rymes paper on postwar Canada should be most interesting. They emphasize the tremendous importance of interindustry shifts in resource use as a source of aggregate productivity growth in Canada. The importance of such intersectoral

shifts cannot be stressed strongly enough in the case of developing economies, and it raises issues of aggregation all over again. Earlier in this paper it was pointed out that Bruton [1968] and Williamson [1967, 1968] both argued on the basis of aggregate production function analysis that most of the variation in Latin American and Philippine total factor productivity growth could be explained by changing deviations from optimal resource use. In the Philippine case, it was asserted that the decline in aggregate technical progress could be explained by increasingly poor factor use. A more explicit test of this hypothesis has been performed by Williamson and Sicat [1968] utilizing the Massell [1961] disaggregated model.[11] The results for the Philippines show that interindustry technical change, attributable to labor and capital shifts within manufacturing, was negative over the period 1957 to 1962 when an excessive import substitution policy was still being implemented. Quite

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11/ This type of analysis has also been successfully applied to regional resource allocation and growth problems by Borts and Stein [1964]. Their analysis was limited to the American economy. We suspect that the approach would bear even greater fruit when applied to developing economies.

obviously, more research along these lines could be done for other Asian and Latin American economies.

Professor Griliches offers a paper summarizing his research on production functions in manufacturing. His basic results deserve emphasis here. First, he finds very little evidence of increasing returns in American manufacturing. Second, he confirms the importance of proper measures of inputs, a research approach missing elsewhere in both the NBER and Brown volumes. Labor quality improvements are very important in explaining output growth. Perhaps more interesting are Griliches' attempts to improve our estimates of capital service flows in the production function. Conventional measures of capital suffer from enormous shortcomings: (1) they assume that services derived from a machine deteriorate too rapidly with age (Brown, of course, would strongly challenge this assertion); (2) they measure the stock of capital rather than the flow of services from it; and (3) they use the wrong price indices to deflate it. Griliches would argue for the "one-horse shay" view it seems. He certainly attacks with vigor the relevance of the conventional observation

that the value of machines declines rapidly as they age. His view is that this rapid decline reflects only a decline in their expected life span not because the relative quality of their services has declined. (The present reviewer is left unconvinced since we have yet to accurately capture the effects of obsolescence.) At any rate, Griliches finds these adjustments having enormous effects on the residual estimate of technical change. These results have been confirmed in a more recent Jorgenson and Griliches [1967] paper and thus they give impetus to more extensive research on the quality of labor in the developing economies and its improvement.

The remaining empirical papers in the NBER volume (namely those by Brown and Conrad, Gort and Boddy, and Eisner) raise far more issues than answers. This reviewer has thought it more useful simply to raise three of the more important points which appeared in conference discussion and which, in our judgment, have special relevance for research on the developing economies. The three issues selected are (1) competitive assumptions, (2) capacity utilization, and (3) lagged response at the firm level.

All of us who have worked in this area feel considerable discomfort after having admitted to serious market imperfections in the real world but then pursuing analysis which requires a fulfillment of the traditional competitive assumptions. Nowhere does this make the econometrician more uneasy then when confronted with data from developing nations whose outstanding characteristic is one of structural disequilibrium. In fact, there is a whole library full of development literature which begins with the assumption that labor, capital, and foreign exchange markets are far displaced from equilibrium. Weisbrod's comments on the Brown and Conrad paper confront this issue with special eloquence. Weisbrod emphasizes that it simply won't do to suggest instead that the degree of imperfection is roughly the same from industry to industry in both input and output markets. Furthermore, it is very difficult to reconcile the perfect competition, profit maximization assumptions with the remarks made earlier in this review that improved resource allocation could account for large rates of technical change (total factor productivity improvement) in the less developed economies. "If perfect competition did prevail we would

expect each resource to move until the values of its marginal product were equalized in all uses (and equalized with factor price). Why, then, would any misallocation remain?"[12] Part of the answer, of course, may be lags in adjustment and we shall have shortly more to say on this. And yet very little research has been done along these lines. A recent exception to this general rule is Bruno's [1968] work on Israel[13] which attempts to estimate factor contribution to growth (using a CES production function assuming disembodied technical progress) postulating structural disequilibrium in factor markets. It appears that Bruno's model gives more reasonable results than the simple competitive model. Not only does the model sharply raise the estimated contribution of capital formation to growth, but it yields estimates of the discrepancy between labor's marginal product and its wage, and thus the marginal product of capital and the actual rate of return. One hopes to see further research

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12/ Weisbrod, in NBER [1967], p. 380.

13/ Hoch [1962] and Mundlak [1963] have dealt with estimation methods applied to production models which test for these equilibrium conditions.

along these lines applied to the economies of Asia and Latin Amerida.

Related to the issues raised by Weisbrod are the distributed lag models applied to production. Models of production normally are supposed to represent equilibrium production processes. Obviously, the firm cannot substitute up to σ in the short run, but the question is never seriously pursued in the NBER volume. Once capital is built or installed substitution possibilities are limited. This commonplace observation has been frequently invoked in arguments against the neoclassical economics of smooth isoquants. Nerlove [1958] and Koyck [1954], of course, appeal to institutional, technical and psychological factors in developing distributed lag models but it seems to this reviewer that considerably more work along these lines is warranted. The estimation of short-run substitution possibilities requires special attention in underdeveloped economies where large unsystematic relative factor price changes occur with disturbing frequency. In developed economies, these changes are likely to be more predictable.

Brown points out that the short-run σ depends upon two factors: (1) the constraint which existing technology places on the long-run σ (thus setting an upper limit on short-run substitution possibilities) and (2) the degree of rigidity of capital to vary in response to current changes in relative factor prices. The degree of rigidity (δ) is influenced by the average economic life of capital equipment, the rate of investment, the range of variation in L that can be applied to existing K, institutional constraints, and psychological factors. Obviously, too, the greater the role played by the past history of relative factor prices (p_t) in determining current technique, the more resistant will current capital-labor ratios be to current changes in $\underline{p}$. Thus, for example, suppose

$$p = p_o \, p_{-1}^{\delta} \, p_{-2}^{\delta^2} \cdot \cdot \cdot p_{-n}^{\delta^n}$$

where p is the factor price ratio which determines current relative factor prices.[14] In this multiplicative version, δ is the rigidity parameter influenced by age,

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14/ The relevant pages in Brown [1966] are pp. 63-72.

durability, rate of investment, flexibility of installed equipment, etc. In this fashion, we arrive with Brown at a point where the current factor combination depends upon (1) the relative capital intensity parameter, (2) the elasticity of substitution, (3) the rigidity parameter, and (4) the current and historical factor price ratios.

The point here is that we can postulate any form of distributed lag model, but δ implies some cost of inflexibility. Given a change in p_o, a high δ implies a large discrepancy between actual and potential output. Where the historical changes in p_t are smooth and gradual, the costs may not only be small but future price ratios may be predicted (and anticipated wherever profitable) by entrepreneurs with considerable accuracy. But how about less certain and more unstable circumstances? How long does it take firm decision-making units in a developing economy to adjust to new minimum wage legislation, decontrol and devaluation, changes in government lending policy, and so on? These seem to the present writer to be extremely important problems requiring more attention than they are currently receiving. In spite of

Griliches' /1967/ recent warnings on the use of distributed lag models, the present writer has applied a partial adjustment model of production (CES) to Philippine manufacturing sectors over the late 'fifties and early 'sixties. Interestingly enough, the results suggest that entrepreneurs adjust far more rapidly to relative factor price changes than is normally the case for developed economies.

One final problem remains. A great deal more work needs to be done on short-run capacity utilization problems. Both the NBER and the Brown volumes are critical of research which ignores short-run fluctuation in output and factor utilization. Such adjustments are probably all the more problematical for developing economies where data of this sort is thin, and time series are so short. The adjustments are further compounded by the fact that departures from full capacity are not due to variations in aggregate demand over a business cycle. Instead they are determined by random harvest conditions, external conditions in primary product markets, foreign exchange constraints, abrupt changes in commercial policy, and domestic supplying bottlenecks of all kinds. Thus the

reader is well advised to look with great suspicion on production function research in developing economies which fails to come to grips with the short-run utilization problem.

3. A Concluding Remark.

In summary, these three volumes are excellent surveys of the enormous theoretical and empirical strides the profession has made in production economics over the past decade. They also reveal quite starkly how much work is left to be done. Professor Chenery recently pointed out that almost every problem in economics could fruitfully be re-examined under the conditions prevailing in underdeveloped countries, especially those nations which differ most strikingly from the advanced countries. "Since the first requirement of development policy is to raise total output, a better understanding of production relations in both the traditional sectors of agriculture and the newer industrial sectors is needed."15/ Further justification for econometric work on production functions in the developing economies is unnecessary.

15/ Chenery /1967/, p. 11.

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