Production and Cost Functions: An Econometric Survey

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1. QUANTITATIVE REPRESENTATION OF TECHNOLOGY AND COSTS

THE MOST general representation of technological conditions is achieved by using the language of sets. Consider a very simple case where the productive activity consists of only one homogeneous input (say labour) and one homogeneous output. Then, measuring quantities of labour used as negative values, we define the production set illustrated in Figure 1. This set represents the

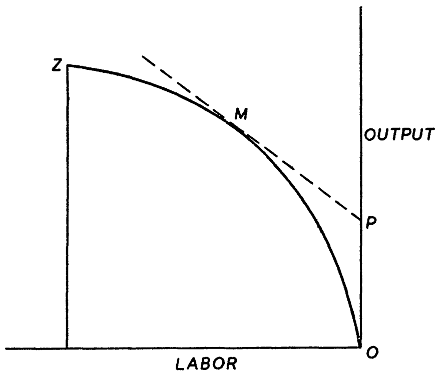

FIGURE 1

possible realizations of the outputs of the commodity in response to labour inputs. We specify that the set does not extend beyond the northwest orthant. In addition, we suppose that the set is compact, that is to say, it is closed and bounded and each point of the boundary is a member of the production set. The boundedness of the production set in our example implies that there are limits to the amount of labour available and to the quantity of output. Neither can be increased, or decreased, indefinitely. With a compact production set, a linear function (i.e., a fixed market rate of exchange between labour and output) reaches a maximum on a point (or points) of the boundary. In Figure 1, the market rate of exchange is represented by the line MP. The production set has a boundary point M at which profits OP are maximised.

Much of the theory of competitive economics depends, however, on a more crucial and restrictive condition than that of boundedness, namely, the assumption of convexity. The production set is convex if a line joining any two points of the set a and b contains only points belonging to the production set. Or, to put it informally, the set has no holes and no dents in its boundaries. The main advantage of the convexity postulates is that they are, as Koopmans puts it, "in some sense minimum assumptions ensuring the existence of a price system that permits or sustains compatible and efficient decentralized decision making" [**174**]. No assumption about differentiability is required. The maximum of the convex production set is not merely the highest value within the immediate neighbourhood; it is the maximum over the whole range of possible production patterns represented in the model.

The representation of technological possibilities as a set is the most comprehensive approach to the analysis of production. But for many practical, and for some theoretical applications, this approach is too general to be useful. The set must be restricted and specialised. One such specialised concept is the production function.

The traditional approach to the theory of the firm was to specify the production function which described the maximum output that can be obtained, with an existing state of technological knowledge, from given quantities of inputs. In terms of our figure, the production function describes the subset of points on the boundary OMZ. For a firm producing many products, the production function describes the maximum value of one particular output as a function of the quantities of inputs, with all other outputs held constant. To simplify the representation, let it be assumed that the function is continuous and differentiable with infinitely divisible inputs and outputs. Then if this technological process is repeatable, the expansion of *all* inputs by a certain fraction should lead to the same proportional expansion of output. In short the simplified production function is assumed to be linear and homogeneous [**300**].

The traditional production function describes only the *efficient* techniques, i.e., those which produce the maximum output of a desired commodity for given inputs. The process by which those techniques are discovered is not examined. For many years these processes were deemed to be management problems and so outside the range of economics. But in recent times it has been recognised that the problems of resource allocation *within* the firm are closely analogous to those *between* firms and industries. There is both economy and additional insight to be gained by pushing the domain of study back into the firm to examine its *internal* decisions.

Technological conditions have been most successfully explored in practice by linear activity analysis [**76**]. The production set is drastically simplified. We first specify that there are a finite number of homogeneous *commodities* and a finite number of *basic activities*. Each basic activity is represented by a number for each commodity—a negative value indicating an input and a positive value, an output. The technical knowledge of society is subsumed in these basic activities, and in application they are supposed to be constant and reproducible. The activities are assumed to be *additive*. This means that the activities are independent of each other; one activity can be added to any other to form an activity that consists simply of the sum of the values in the original activities. Each activity is independent of others; or, in other words, there is no interaction. The activities are assumed to be infinitely *divisible* and *proportionally reproducible*. Thus, if a basic activity is represented by 2 man-days + $2\frac{1}{2}$ spade-days = 1 hole, then 2λ man-days + $2\frac{1}{2}\lambda$ spade-days = λ holes, where λ is any nonnegative number, is also a possible activity. The λ is usually called the *level* of the activity [**174**].

So far we have not specified any restrictions on the linear activity analysis model; the set, which up to now has been open, is closed by postulating *limited resources*. Resources may be used in any nonnegative amounts up to a given upper limit. This limits the production set to those points corresponding to levels of activities which are within the bounds of these availabilities.[1] With *given* quantities of *all* resources we can illustrate the resulting set diagramatically for two outputs x_1 and x_2, and with three (effective) limitations of resource availability. At the points B and C, two of the factor limitations are binding, and the other resource is still not entirely used. For points on the lines AB, BC and CD, only one factor limitation is binding—the

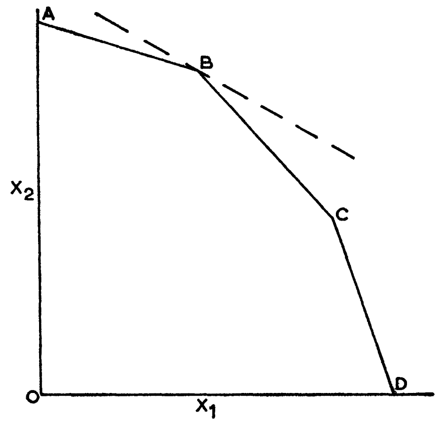

FIGURE 2

[1] Strictly formulated, two other postulates are required: one states that the origin is to be included in the production set; the other states that there exists a point where at least one desired output has a positive value (see Koopmans [**174**, pp. 78–82]) and all intermediate commodities are used up.

other two resources are still in excess of requirements. New points which lie on the boundary ABCD are obviously in some sense more efficient than interior points. We can then formally define a bundle of activities as *efficient* if there exists no other feasible collection of activities which produces more of one commodity and no less of others. (This, of course, corresponds to the concept of efficiency used in the traditional production function approach.) If, to outputs x_1 and x_2, we assign certain prices, say, p_1 and p_2 (which we assume are strictly positive) then the maximizing of the function, $\pi = p_1x_1 + p_2x_2$, necessarily leads to the choice of an efficient point; so, the broken line in Figure 2 leads to B. By suitably changing the values of p_1 and p_2, but keeping them strictly positive, we can induce the entrepreneur to trace out all points in the efficient subset. Conversely, to each efficient point there corresponds a system of prices that will induce a profit maximizing entrepreneur to choose that point [**77**].

The activity analysis result differs from the classical production function analysis in that the transformation curve of Figure 2 consists of a series of flat segments instead of a smooth surface. Consequently, the marginal rate of substitution, instead of changing smoothly as the product mix alters, "jumps" from the value of the slope of one flat segment to that of the adjacent one.[2] With a very large number of basic processes the transformation polygon of Figure 2 will consist of a large number of small segments, and so the activity analysis model will then approximate the classical production function.

In both representations of the technological opportunities we have supposed that the process or activity is constant and reproducible under similar conditions. If there are no limitations on quantities of inputs, this implies that each firm then has, by virtue of the postulates, the same production set. By the proportionality postulate, each firm experiences constant returns to scale. Under conditions of perfect competition each firm will earn zero profits whatever its scale of operations. Thus the model leaves the *long run* equilibrium level of output of the firm undetermined. This is obviously not an entirely satisfactory result, and for some purposes we must rationalise the existence of firms and their size distribution.

The neo-classical artifice to get around this problem was suggested by Kaldor [**164**]. Suppose that we define an essential element in any firm called "entrepreneurship." This cannot be bought on the market, but is a specialised and personal attribute of each individual in the community. A firm exists when an individual employs his entrepreneurial ability. But since the amount of entrepreneurial ability possessed by a man is an attribute specific to him, the production function will vary from one person to another. This hypothesis

[2] At the "corners," the marginal rate of substitution is not unique; it is, however, limited by the slopes of adjacent segments.

"explains" both the existence of firms and their distribution by size. It is not a satisfactory explanation, but, until a better one is devised, it will have to serve.

Because the activity analysis model is based on the technological processes of the firm, the introduction of entrepreneurial ability into that model involves many difficulties. It is clear that the best approach would be to regard the quantity of entrepreneurship as an inequality indicating a *long run* limitation of entrepreneurial capacity. But this is not possible since there is no generally accepted cardinal measure of entrepreneurship.[3] The general conclusion must be that the activity analysis approach is not useful for examining the problem of the long run distribution of output between firms. Fortunately the solution to this problem is not crucial or even important for the main body of economic theory.

The modern developments in growth theory contain many special hypotheses about production functions. One useful distinction, closely related to those used in classical period analysis, which has been pursued in several studies [**152**, **290**], is between the *ex post* and *ex ante* production function. The *ex ante* function contains the full opportunities of substitution open to the entrepreneur when he chooses his technique of production. The *ex post* function exhibits the opportunities available after the technique has been chosen. This distinction can be worked into the particular mathematical function used to specify the relationships.

2. PRODUCTION FUNCTIONS AND AGGREGATION

There is a wide choice of algebraic forms which can be used to represent production functions. The simplest model is a special case of the general model of activity analysis. If only one activity is available for the firm, we can represent the production function by rectangular isoquants with constant returns to scale. There is no opportunity for changing relative factor inputs, and the elasticity of substitution is, of course, zero. This is an extremely specialised production function, but its simplicity explains its extensive use in many models.

Probably the most popular production function, however, is the Cobb-Douglas [**63**]. In its best known form, with X measuring outputs, K the quantities of capital, and L the input of labour, we write:

$$X = AL^{\alpha}K^{\beta}, \qquad \begin{array}{l} X > 0\,, \ \alpha \geqslant 0\,; \\ K > 0\,, \ \beta \geqslant 0\,; \\ L > 0\,, \ A > 0\,. \end{array} \tag{1}$$

[3] An alternative approach is to give up the constancy of technological laws and introduce for each entrepreneur different basic activities. This may be a useful technique for some purposes, but it does wreck the simplicity of activity analysis.

The properties of the Cobb-Douglas function are:

(a) The α and β are the elasticities of production with respect to labour and capital, respectively.

(b) The function is homogeneous of degree $\alpha + \beta$. If $\alpha + \beta$ exceeds unity, there are increasing returns to scale; $\alpha + \beta = 1$ indicates constant returns.

(c) Marginal physical productivity of labour, for example, declines if $\alpha < 1$ as labour input is increased. Specifically, $\partial^2 X/\partial L^2 = \alpha(\alpha - 1)X/L^2$ and is negative if $\alpha < 1$.

(d) The marginal rate of substitution is $\alpha K/\beta L$, and so the elasticity of substitution is unity.

From the Cobb-Douglas production function and the prices of output (P), labour (W), and capital (I), we can derive the cost function and the supply equation for competitive market situations. Substituting the marginal productivity conditions into the production equations, the cost function is

$$(2) \qquad C = I\left(\frac{\alpha + \beta}{\beta}\right)\left[\left(\frac{\beta W}{\alpha I}\right)^{\alpha} \frac{X}{A}\right]^{\frac{1}{\alpha+\beta}}.$$

Similarly, the supply function can be obtained by equating $\partial C/\partial X$ to P and rearranging:

$$(3) \qquad X = A^{\frac{1}{1-\alpha-\beta}}\left(\frac{\alpha}{W}\right)^{\frac{\alpha}{1-\alpha-\beta}}\left(\frac{\beta}{I}\right)^{\frac{\beta}{1-\alpha-\beta}} P^{\frac{\alpha+\beta}{1-\alpha-\beta}}.$$

The cost function is linear in the logarithms of the wage rate, the price of capital, and the level of output. If $\alpha + \beta = 1$, the cost is proportional to output, and the supply curve is infinitely elastic. We can drop the assumption of competition and introduce elasticities of supply of factors of production to get noncompetitive cost functions—but the algebra becomes tedious.

The Cobb-Douglas function has had a long and successful life without serious rivals. But recently it has been strongly challenged by a new function by Arrow, Chenery, Minhas, and Solow [**9**, **8**]—which we shall abbreviate to SMAC. (Brown and de Cani have also developed this function independently [**45**]). The basic change introduced by SMAC is to allow the elasticity of substitution, σ, to be constant at a value other than unity (Cobb-Douglas) or zero (input-output). The function is

$$(4) \qquad X = \gamma[\delta K^{-\varrho} + (1 - \delta)L^{-\varrho}]^{-1/\varrho}.$$

This function is linear and homogeneous, i.e., there are constant returns to scale. The *efficiency* parameter is γ, which changes output for given quantities of input; the *distribution* parameter is δ $(0 \leqslant \delta \leqslant 1)$ which determines the division of factor income. The *substitution* parameter, ϱ, is a simple function of the elasticity of substitution, thus, $\sigma = 1/(1+\varrho)$. The marginal product of capital is $\delta\gamma^{-\varrho}(X/K)^{1+\varrho}$. The limits to the value of ϱ are derived from σ. When elasticity is infinite, $\varrho = 1$; and when $\sigma = 0$, $\varrho = \infty$.

By choosing appropriate values for σ the SMAC production function can be specialised to the input-output and Cobb-Douglas forms. As σ approaches unity (i.e., $\varrho \to 0$), the SMAC function approaches the Cobb-Douglas. This can be demonstrated as follows:

$$(5) \quad \exp\left[-\varrho\log\frac{X}{\gamma}\right] = \delta\exp[-\varrho\log K] + (1-\delta)\exp[-\varrho\log L]\ ;$$

and so,

$$1 - \varrho\log\left(\frac{X}{\gamma}\right) + O(\varrho^2) = 1 - \delta\varrho\log K - (1-\delta)\varrho\log L + O(\varrho^2)\,.$$

Dividing by ϱ and taking the limit $\varrho \to 0$, we get:

$$(6) \qquad X = \gamma K^{\delta}L^{1-\delta}\,.$$

This is the Cobb-Douglas function with constant returns to scale.

By a similar argument we can show that the limiting form as $\varrho \to \infty$ is represented by a series of right angled isoquants with their corners on a ray from the origin. Instead of specialising the SMAC function, we may generalise it to include cases where there are increasing or decreasing returns to scale by including another parameter, ν:

$$(7) \qquad X = \gamma[\delta K^{-\varrho} + (1-\delta)L^{-\varrho}]^{-\nu/\varrho}\,, \qquad \nu > 0\,.$$

If $\nu > 1$, there are increasing returns to scale; and if $\nu < 1$, returns are decreasing [**222**].

Cost and supply functions can be derived from SMAC (with $\nu = 1$) by suitable substitutions. In the case of perfect competition in factor markets we get the long run cost curve (with capital as the independent variable):

$$(8) \qquad C = KZ$$

where

$$Z = W\left\{\frac{I(1-\delta)}{W\delta}\right\}^{\frac{1}{1+\varrho}} + I\,.$$

Substituting from the capital-output relation we get the long run cost function with output as the independent variable:

$$(9) \qquad C = \left[\frac{\delta V^{-\varrho}}{I}\right]^{\frac{1}{1+\varrho}} ZX\,.$$

Cost is a linear function of output, as we should expect with a linear and homogeneous function. The relation between cost and factor prices is, of course, more complicated than in the Cobb-Douglas case.

Choice among the various representations of production relations depends on a wide variety of conditions. One important criterion is that the model gives rise to sensible aggregate relationships which, in some sense, correspond to micro-relations.

The production function is a technological relationship confronting a firm. It is the entrepreneur who chooses factor proportions and output levels. Can we then proceed to construct useful production functions for an *industry* or for the industrial or agricultural *sector* as a whole? One difficulty is immediately apparent: those factors which we regarded as fixed for the individual firm are not necessarily fixed for the industry, e.g., entrepreneurial ability. Other factors, such as the quantity of skilled labour, which were not fixed for the individual firm, may well be an important limitation for the industry.[4] Even if all firms were enjoying increasing returns to scale, it would not follow that the industry as a whole also experienced economies of scale. An expansion of the industry would often encounter less suitable sites, limited supplies of raw materials, etc. It is convenient to put aside these difficulties of external economies and diseconomies in discussing the aggregation problem.[5] In particular, we shall suppose individual production functions of firms do not depend on aggregate production of the industry.

A systematic treatment of the general aggregation problem in production functions was developed in a pioneering paper by Klein [**167**]. He suggested that, to get an aggregate (or strictly, an average) production function and aggregate marginal productivity relations analogous to the micro-functions, we need to construct weighted *geometric* means of the corresponding micro-variables, where the weights are proportional to the elasticities for each firm. The elasticities of the macro-function are the weighted average of the micro-elasticities, with weights proportional to expenditure on the factor. The macro-revenue is the macro-price multiplied by the macro-quantity, which is defined as the *arithmetic* average of the micro-revenues; similar definitions apply to the macro-wage bill and macro-capital expenditure.

Kleinian aggregation *over firms* has some curious consequences. For example, the definition of the macro-wage rate W is:

$$WL = \frac{1}{n} \sum_{i=1}^{n} W_i L_i \tag{10}$$

[4] Formally we can allow for these effects on production functions of firms by including aggregate production of the industry in the production function of the ith firm.

[5] External diseconomies due to fixed supplies of a factor enter the production function. Other external diseconomies arise because the *prices* of inputs rise as output increases. These *pecuniary* diseconomies have no effect on the production function, but they do modify the cost curves.

where W_i and L_i are the wage rate and (homogeneous) labour employed in the ith firm, and

$$(11) \qquad L = \prod_{i=1}^{n} L_i^{\alpha_i/\Sigma\alpha_i}$$

is the definition of the macro-labour input, where α_i is the labour elasticity in the ith firm. In competition, all firms have the *same* wage rate $W^* = W_i$ for all i. So we can substitute to get

$$(12) \qquad W = W^* \Sigma L_i/n \prod_{i=1}^{n} L^{\alpha_i/\Sigma\alpha_i}.$$

Thus the macro-wage rate will almost always differ from the common wage rate of the firms. In this simple case, it is difficult to interpret W and to see why it should differ from W^*.[6] Similar comments apply to the prices of output and capital.

Much of the formal theory of aggregation was cleared up by Nataf [**226**] who proved that, for sensible aggregation, the production function must be *additively separable*. Output is then equal to a labour component *plus* a capital component.[7] This is a highly restrictive condition. Of the three production functions discussed so far, the input-output model is obviously additively separable. The Cobb-Douglas does not satisfy the condition, but when transformed logarithmically the function is additively separable; this is the rationalization of Klein's use of geometric means.[8] Certainly the Cobb-Douglas is quite well behaved for aggregation, especially within firms.[9]

[6] The natural supposition that, when the wage rate does not vary, $W = W^*$ misled Simkin [**276**] into alleging that Klein's model did not satisfy his marginal productivity conditions in the simple case.

[7] In fact the function should be of the form

$$X^{w_0} = aL^{w_1} + bK^{w_2}$$

where the w's are constant weights.

[8] Difficulties still arise basically because the macro-elasticities are arithmetic averages of micro-elasticities. Klein showed that to get suitable macro-elasticities, we must compute a weighted average where the weights are proportional to values of output. But this means that the measure of macro-input depends on the quantity of output; clearly the weights must be independent of output.

[9] In an impressive synthesis of linear programming and the Cobb-Douglas production function, Houthakker has shown that *individual* production functions of the linear programming type can give rise to a Cobb-Douglas production function for the industry as a whole [**146**]. For each firm, entrepreneurial ability is reflected in the fixed input-output ratios—an efficient businessman will have low ratios and an inefficient one high ratios. If these ratios are distributed in the Pareto form, the aggregate function is a Cobb-Douglas. Houthakker's synthesis is an excellent rationalisation of static cross section data. If we attempt to use the analysis to explain dynamic adjustments, how-

This quality can also be claimed for SMAC's production function. The function can be written in the form

$$\gamma^{\varrho} X^{-\varrho} = \delta L^{-\varrho} + (1 - \delta) K^{-\varrho} . \tag{13}$$

This is additively separable and so satisfies the Nataf conditions.

One of the main practical problems of aggregation is that the data are usually published in the form of arithmetic averages or totals, whereas our system of aggregation requires geometric averages. What are the errors introduced by this approximation? If the deviations from the mean value are relatively small we can show that the geometric mean G is approximately related to the arithmetic mean by the formula:

$$G \doteq \bar{X}\left(1 - \frac{1}{2} \cdot \frac{\sigma^2}{\bar{X}^2}\right) . \tag{14}$$

In this case, the arithmetic value gives an upward relative bias which is approximately half the relative variance. This result suggests that if the relative variances of the variables, output, capital, and labour are approximately equal, the relative biases will be about equal. This depends, however, on the assumption of there being only relatively small deviations from mean values, and this condition is not satisfied in many practical studies.

The statistical problems of linear aggregation have been examined by Theil [**307**]. With given micro-relations and a form of aggregation, Theil examines whether it is legitimate to fit a macro-relation to the aggregate values. Consider the Cobb-Douglas production function in its logarithmic form for the ith firm:

$$x_i = \alpha_i l_i + \beta_i k_i + a_i \qquad (i = 1, \ldots, n) \tag{15}$$

where $x_i = \log X_i$, $l_i = \log L_i$ and $k_i = \log K_i$. From these we seek a macro-production function:

$$x = \alpha l + \beta k + a + \varepsilon \tag{16}$$

for observations of the aggregate values for a time series. We first examine the regression of the labour input of the ith firm on aggregate labour input and aggregate capital input for the time series observations, i.e.,

$$l_i = B_{li} l + C_{li} k + D_{li} + U_{li} . \tag{17}$$

Similarly the regression for capital is also computed:

$$k_i = B_{ki} l + C_{ki} k + D_{ki} + U_{ki} . \tag{18}$$

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ever, difficulties appear since it supposes that changes in factor proportions are introduced by new firms—the old firms stick to their old fixed coefficients or die off. This is contrary to casual observation, although clearly much more evidence is required before such a fruitful hypothesis is rejected.

The regression coefficients B and C describe the systematic movements of micro-variables as macro-quantities change. The U's are random variables with the usual characteristics. Substituting these equations into the micro-equation (15) we get

$$x = \alpha l + \beta k + a \tag{19}$$

where

$$\begin{aligned} \alpha &= \bar{\alpha} + n\{\mathrm{cov}(\alpha_i B_{li}) + \mathrm{cov}(\beta_i B_{ki})\}\,, \\ \beta &= \bar{\beta} + n\{\mathrm{cov}(\alpha_i C_{li}) + \mathrm{cov}(\beta_i C_{ki})\}\,, \\ a &= n\{\bar{a} + \mathrm{cov}(\alpha_i D_{li}) + \mathrm{cov}(\beta_i D_{ki})\}\,. \end{aligned} \tag{20}$$

The aggregation bias of the macro parameters is measured by the covariance terms in these equations. Unfortunately it is difficult to press these results any farther since we cannot easily suggest *a priori* restrictions on the values of the covariances. Quantification of the bias must await statistical studies.

After surveying the problems of aggregation one may easily doubt whether there is much point in employing such a concept as an aggregate production function. The variety of competitive and technological conditions we find in modern economies suggest that we cannot approximate the basic requirements of sensible aggregation except, perhaps, over firms in the same industry or for narrow sections of the economy.

3. PROCESS AND PRODUCTION FUNCTIONS FROM ENGINEERING DATA

The development of economic theory constructed from data about technological processes has given rise to attempts to construct production and cost functions from technical information supplied by the engineer or agrarian scientist. This information is in turn collected by experiment or from the engineers' experience in the day to day operation of a technical process. There are considerable advantages of approaching the production function from its technical base because both the range of applicability is known and because it is relatively easy to incorporate the results of technical progress. Furthermore, unlike cross section and time series studies, we are not restricted to the narrow range of actual observations.

The basic unit of analysis, the engineering "process," is defined according to the analytical convenience of engineers. Similarly the inputs are often specified in terms of their engineering characteristics, e.g., B.T.U.'s. The first problem is to translate the engineers' units into quantities more suited to economic analysis. To achieve this may require specification of other processes, for example, the production of B.T.U.'s by purchasing and burning fuels. The second and most important problem is to combine these processes for a particular plant to form one plant production function. In defining and analysing process functions one must try to ensure that the functions are independent and additive. Fortunately, the engineer also tries to avoid interaction effects in specifying his processes, so that suitable aggregations from processes to plants can probably often be achieved with reasonable accuracy.

The process function and plant production functions derived from engineering data differ from the traditional production function of economic theory. First, the input of entrepreneurial capacity has not been explicitly included in the process function. The engineering data may represent the most efficiently managed process or, perhaps, "average experience" in the industry. Entrepreneurship then enters into the engineering data itself. Secondly, engineering data can encompass only technical processes. They cannot easily include, for example, the non-technical process of selling the goods. This means that the functions derived do not cover all the activities of the firms; hence it is useful to call them *process* functions or *plant* functions to distinguish them from the comprehensive production function of the firm.

This method of measuring production functions has been extensively used in agriculture. Heady [**125**] and his colleagues have derived these functions from experiments; for example,

$$(21)\quad Y = -5.68 - .316N - .417P + .63512\sqrt{N} + 8.5155\sqrt{P} + .3410\sqrt{PN}$$

where Y is bushels of corn per acre, N is pounds of nitrogen, and P is pounds of phosphate. From this function the marginal productivity of each of the fertilizers can be computed, and so the optimum combination of fertilizer can be found for various levels of relative prices. But of course the function does not tell us how much land or management should be used. This is then strictly a process function.

Chenery and Ferguson [**54**, **96**] pioneered the use of engineering data in industrial process functions. Chenery's treatment of natural gas transmission used the principles of gas engineering to produce a process and plant production function for gas pipe line operation; the result indicated increasing returns to scale. Ferguson studied the techniques of commercial air transport to find a production relation. He was concerned with measuring an aggregate process function but included only the direct inputs; for example, administration was not counted among the inputs. (He also described the course of technical progress over the years 1939-1947.) The techniques of chemical engineering have been used by Moore [**219**] to show that there are generally increasing returns to scale. This seems to be a general result of most engineering studies.[10]

Process and plant production functions are useful for a wide variety of purposes; for example, in deriving plant cost curves, in predicting raw material and labour requirements, etc. But these functions are not suitable for testing hypotheses about economies of scale of the *firm*. Even though one may find increasing returns in the process or plant functions, it does not follow that the *firm* enjoys these conditions. Increasing returns to a process may be offset by higher costs of administration, etc.

The process functions are particularly useful for the analysis of technical progress. Hirsch [**138, 139**] showed how experience with a process resulted in a reduction in labour inputs as the volume of output increased. This process function in the machine tools industry suggested that a doubling of cumulative output gave rise to a decrease of about 18–20% in labour inputs [**10**].[11]

The development of process functions from engineering data with the aid of linear programming models has been the most important development in the economics of the firm in recent years [**202, 199**]. Technological opportunities of the firm are represented by a finite number of activities, and technical limitations are introduced in the form of fixed capacities of some inputs, such as, for example, a limited floor space. Using the simplex (or other) computing methods, the economist can find the efficient combination of activities for any given system of prices.

When comparing the linear programming approach with empirical work on traditional production functions three reservations must be borne in mind. First, the linear programming model is normally interpreted as a *short run* production model with some inputs in fixed supply. The empirical work on Cobb-Douglas production functions, on the other hand, is usually concerned with long run relations where all inputs are variable. Secondly, the solution

[10] The most important survey and extension of engineering production functions is in the recent book by Vernon Smith [**284**]. Although the main purpose of his study is investment theory, he bases most of the development on engineering approaches to production relations. In particular he shows that the many "least cost solutions" developed by engineers are the same as those which we derive from the neoclassical theory of the firm. The study also includes a valuable survey of multi–facility production functions.

[11] The formal incorporation of progress functions into the framework of the economics of the firm was attempted by Charnes, Cooper and Mellon [**53**], who worked in terms of the cost curves rather than the production function. They postulated a family of cost curves—one for each time period—corresponding to the learning function. Entrepreneurs plan to expand their output through this family of cost curves until they reach equilibrium output. Arrow [**9**] has recently worked out many of the implications of integrating progress functions into the theory of production. In Arrow's model, however, learning takes place only in the capital goods industry; no general learning effect due to progressively better use of existing equipment is incorporated.

with the linear programming model tells us what firms *ought* to do to maximise their net revenue; in a sense it improves on the existing production function of the firm by giving a higher level of efficiency. The linear programming approach does not merely describe production relations, it usually changes them!

The use of engineering and technical data for production functions is still in its infancy. But for many years such information has been used in the analysis of cost, particularly by cost accountants and more recently by economists. These techniques and results are reviewed in Section 7.

4. STATISTICAL MODELS: IDENTIFICATION AND ESTIMATION

The formal specification of models of production vary from the simple single equation model to the "complete system" approach. All models, whether used for aggregate or micro-data, whether adapted to cross section or time series, involve similar problems of specification and interpretation; to these we now turn.

The explicit treatment of probabilistic aspects of simultaneous equation models of production functions was first introduced in an important paper by Marschak and Andrews [**203**]. They wrote: "the production function (1) will change, even within the same industry, from firm to firm and from year to year, depending on the technical knowledge, the will, effort, and luck of a given entrepreneur: these factors can be summarised as 'technical efficiency,' and may be represented by one or more random parameters." They suggested the simple hypothesis:

$$Y_0 = A_0 Y_1^{\alpha_1} Y_2^{\alpha_2} U_0 \tag{22}$$

where Y_0, Y_1, and Y_2 are output, labour, and capital, respectively, and U_0 is a random variable with a mean equal to unity. (This new notation with subscripts is more useful for further development.) Marschak and Andrews interpret the disturbance as analogous to an experimental error. But there is more to it than that. The variation in output of a particular firm from year to year may be due to exogenous random causes (such as the weather); but the differences between one firm and another due to the "ability" of the entrepreneur will be constant over time for a particular firm [**224, 229**]. Clearly different interpretations of the disturbances are required for cross section and time series data.

The marginal productivity relationship also would normally not be satisfied exactly; and the deviation could be described by a random variable. Suppose that the real wage rate is P_1/P_0, where P_1 is the money wage and P_0 the price of output; then

$$(23) \qquad \frac{\partial Y_0}{\partial Y_1} = \alpha_1 \frac{Y_0}{Y_1} = \frac{P_1}{P_0} U_1$$

where U_1 is a random term with a mean equal to unity. Marschak and Andrews argue that "not all entrepreneurs may have the same urge or ability or luck to choose the most profitable combination of production factors: even if entrepreneur A be technically as efficient as B, he may have smaller '*economic efficiency*'."

Again care is required in interpreting the role played by the disturbances. First consider the cross section interpretation of U_1. The disturbance might be thought to be an attribute of a particular entrepreneur—a reflection of his "ability."[12] But can we describe variations in "economic efficiency" from one entrepreneur to another as a random variable? Clearly for one particular cross section sample, this is admissible. Samples of the same firms taken over time, however, will reflect the ability of the entrepreneur more appropriately as a *constant*. The efficient entrepreneur will have a mean of U_1 near to unity and a small variance over time. Inefficient entrepreneurs may have a mean value of U_1 quite different from unity and perhaps also a large variance over time.[13] The prior specification of the interrelation between the U's is very important, but very little is, in fact, known about these disturbances.

The system of random simultaneous equations for the competitive model can be represented as follows:

$$(24) \qquad y_0 - \alpha_1 y_1 - \alpha_2 y_2 = u_0 + a_0 ,$$

$$(25) \qquad y_0 \qquad\qquad - y_1 = u_1 + a_1 ,$$

$$(26) \qquad y_0 \qquad\qquad - y_2 = u_2 + a_2 ,$$

where a_0, a_1, and a_2 are constants and u_0, u_1, and u_2 are random variables with zero mean. For a model of an industry in *imperfect* competition Marschak and Andrews suggest the hypothesis:

$$R_0 = B_0 Y_0^{\beta_0}$$

where R_0 is revenue and B_0, β_0 are constants, so that $r_0 = b_0 + \beta_0 y_0$ where, as before, lower case letters indicate natural logarithms. This hypothesis can be extended to the wage bill R_1 and the annual capital cost R_2. The equation system can be expressed in terms of r_0, r_1, and r_2 or some mixture of y_i and r_i according to the data available. In practice, the data usually consist of net values of output, net values of capital employed, and numbers of employees. Consequently the most useful equation is that in the variables r_0, y_1 and r_2.

[12] We must be careful how we characterise high and low efficiency. The best entrepreneur will have $U_1 = U_2 = 1$.

[13] There are obvious difficulties here since an inefficient entrepreneur may make the same mistake each period, and so have a low variance of U_1. Cf. [**321**].

$$(27)\qquad r_0 - \alpha_1 y_1 - \alpha_2 \frac{\beta_0}{\beta_2} r_2 = u' + a' ,$$

$$(28)\qquad r_0 - \beta_1 y_1 \qquad\qquad = u_1' + a_1' ,$$

$$(29)\qquad r_0 \qquad\qquad - r_2 = u_2' + a_2' .$$

In this system there are, of course, only endogenous variables.

It is natural, however, to consider other models which do have exogenous variables. In short-run cross-section models of production the obvious candidates for exogeneity are quantities of fixed factors of production [**224**, **229**]. If we add the logarithms of the quantities of these fixed factors to the existing equation systems, we get a linear system which can be solved for each of the endogenous variables in terms of the exogenous variables. These are the reduced form equations of the employment of variable factors for the given quantities of fixed factors. In long-run cross-section studies, unless the firms have separate capital and labour markets, the only exogenous variable is "entrepreneurial ability."

With time series studies there is more latitude in the choice of exogenous variables. In particular, prices of factors and of outputs vary over time so it is possible to use these prices as exogenous variables. Consider, for example, the simple competitive system of equations (24), (25), and (26) above. If we regard the prices as variables, we get the first equation of the reduced form:

$$(30)\qquad y_{0t} = \frac{\alpha_1}{1 - \alpha_1 - \alpha_2} (p_{0t} - p_{1t}) + \frac{\alpha_2}{1 - \alpha_1 - \alpha_2} (p_{0t} - p_{2t}) + c + v_t ,$$

where c is a constant (a function of α_i and $\log \alpha_i$) and v_t is given by

$$v_t = (u_{0t} + \alpha_1 u_{1t} + \alpha_2 u_{2t})(1 - \alpha_1 - \alpha_2)^{-1} .$$

This equation then shows the reaction of output to changes in the relative prices of inputs. Unfortunately, the equation is not suitable for the case where there are constant returns to scale.

It can be argued that it is useful to regard the prices of factors as exogenous only if the industry (or firm) is small in relation to the supply of factors. If this is not the case then the elasticity of supply of factors will have to be taken into account in the above equations. We might then regard the supply price as determined endogenously within the system and specify supply and demand curves for factors in terms of other exogenous variables such as wage rates in other industries, etc. This approach leads to a more complete model for a sector of the economy, such as that of Hildreth and Jarrett for livestock production and marketing [**137**]. At the highest level of aggregation, the general model of the economy includes an aggregate production equation among the many other relations.

The choice between the single and many equation models must depend on the purposes for which the estimates are required, the availability of data, and relative errors. The results of empirical research in other fields of econometrics suggest that it is dangerous to be pedantic about the superiority of simultaneous equations or single equation methods. It is likely that, if the purpose of the model is to predict output for given quantities of input, the single equation approach will be best. Estimation of the structural coefficients is not necessary for this purpose. If, on the other hand, we wanted to predict the effect on output and quantity of capital of a payroll tax then we do require estimates of the structural parameters of an equation system. If the reduced form is available it is probably best to estimate the structural parameters by first computing the coefficients of the reduced form. This also provides a direct calculation of the effect of the exogenous variables on the output and factor employments. The estimates derived from the reduced form can then be compared with estimates obtained by estimating the parameters directly [**224**]. This provides a consistency test of the statistical specification.

There remains the problem of identification. The production equation is not identified if the scatter of observations measures some equation (e.g., the marginal productivity equation) other than the production function. With exogenous variables in the system, a necessary condition for identifying (or overidentifying) all parameters of the production function is that the number of variables appearing in the system, but excluded from the production equation, be at least equal to one less than the number of equations (i.e., endogenous variables) in the system [**224, 229**]. In most models of the production function, however, this criterion is of no use since there are no exogenous variables in the system. We must seek some other criteria of identification.

Consider the equation system (24), (25), and (26) above. The constant a_1 in equation (25) is identified since no linear combination of the other equations can produce an equation of the same form as (25) unless y_2 is zero. Similarly, the constant a_2 in equation (26) is identified. Let us now suppose that we do not postulate any *a priori* relationship between the constants a_1 and a_2 and the coefficients α_1 and α_2 in the production equation. Under what circumstances may we be confident that the scatters of the y's will enable us to measure α_1 and α_2? Clearly, we can get another equation of a similar form as (24) by taking linear combinations of all three equations. But a linear combination of the disturbances will produce a term of the form $\lambda_0 u_0 + \lambda_1 u_1 + \lambda_2 u_2$. The meagre emprical evidence (to be reviewed below), suggests there is no support for the less stringent hypothesis of zero correlation between u_0 and u_1 and u_2 [**203, 331**]. In cross section models, it seems unlikely that the coefficients α_1 and α_2 will be identified.

The search for further assistance in identification may take many forms.

Perhaps the best procedure is to attempt to construct and use dynamic models of the production function [**229**]. With a system of lags and proportional adjustments built into the model, the problem of identification is considerably easier. Probably the best extension of the model is to specify that the stock of capital is not changed every year to conform to the marginal productivity condition, but is adjusted gradually through time to the desired level. Models of this kind, using distributed lags and "adaptive expectations," have had considerable success in investment analysis as well as in demand and consumption studies. This suggests that their use in the analysis of production might well be worth the additional complications introduced thereby.

The problems of estimation vary considerably according to the specification of the model. Systems and equations which are linear (or logarithmically linear) have received considerable attention in the literature. The properties, and especially the asymptotic tendencies, of estimators in linear systems have been well explored. Much less is, however, known about estimation with nonlinear systems. For the SMAC function, for example, the presence of the exponent ϱ means that no simple method of classical linear estimation can be used to estimate the coefficients. *Ad hoc* methods must be employed. For these reasons we restrict the following survey to linear systems.

The main methods of estimation used for the production function are (a) single equation least squares, (b) the covariance matrix method, (c) factor shares, and (d) instrumental variables. We review these methods in turn.

(a) *Single Equation Least Squares*

This is quite the most popular method of estimating the parameters of the Cobb-Douglas function. Early studies [**63**] began by restricting the sum of the coefficients to unity: so the equation took the form

$$\frac{Y_0}{Y_2} = A_0\left(\frac{Y_1}{Y_2}\right)^{\alpha} U_0\,, \tag{31}$$

or

$$y_0 - y_2 = a_0 + \alpha(y_1 - y_2) + u_0\,. \tag{32}$$

In later studies [**39**, **79**], the sum of the coefficients was not restricted, and the three variable equation.

$$Y_0 = AY_1^{\alpha_1}Y_2^{\alpha_2}U_0\,, \tag{33}$$

was used.

The attractive properties of the least-squares single-equation method are the simplicity of computations, the small standard errors of the coefficients, and the high level efficiency in predicting output for given inputs.[14] The main disadvantage is that, if the random simultaneous equations of the model are of the form set out in (24), (25), and (26) above, the estimates of α_1 and α_2, as estimates of the structural coefficients, will be both biased and not consistent. To judge from the meagre empirical evidence [**203**, **331**], the lack of consistency is quite important, and it is probably wise to use some other method. In the special case of a controlled industry where the firms have to minimise costs for a given quantity of output and where the prices of production vary from firm to firm and where the ratios of marginal productivities are exactly equal to the ratios of factor prices, least squares can be applied to the reduced form of the system. As Nerlove [**230**] has shown, one of the reduced form equations is simply the cost function,

$$C_i = \gamma Y_{0i}^{1/r} P_{1i}^{\alpha_1/r} P_{2i}^{\alpha_2/r} U_{0i}^{-1/r} \tag{34}$$

where $r = \alpha_1 + \alpha_2$, and γ is a constant. This cost equation is linear in the logarithms of output and the prices of factor inputs. Least squares methods will, if the conditions be satisfied, give estimates of the structural coefficients and of the order of returns to scale [**230**]. The success of this method in practice depends on getting accurate error-free data on prices of factors of production with suitably large dispersions.

A more specific criticism of single-equation least-squares estimates, interpreted as estimates of the structural coefficients, has recently been advanced by Hoch [**139**]. Assuming first that the disturbances in each equation of (24), (25), and (26) are independently distributed, he showed that least-squares estimates tended to sum to unity, i.e., they were biased towards indicating constant returns to scale. When the assumption of independence is dropped, however, the tendency to sum to unity was not nearly so apparent. Since both theoretical speculation and empirical evidence suggest that the disturbances are not independently distributed, Hoch's critique loses some of its force [**229**, **223**, **331**].

An important difficulty with most practical studies is that all factor inputs cannot easily be measured or can be quantified only very rougly. Consequently, some factors of production are omitted from the analysis. Grilliches

[14] With fixed inputs, the sampling variance of the coefficients is given by the usual formula. In many cases, particularly in agriculture, the marginal product is the value required. Carter and Hartley [**50**] have shown that the usual formula underestimates the sampling variance, since $\hat{Y}_0$ is a random variable and not a fixed value. The correct version includes the prediction error of $\hat{Y}_0$. When inputs are random variables (and we can suppose that they are distributed independently of u_0), the usual estimate of marginal product is biased (but consistent). If it were thought important to do so, much of the bias could probably be removed easily and efficiently by using Quenouille's transformation; see Durbin [**83**].

[**105**] has examined the bias in least squares estimates when the specification of the list of factors is wrong. In particular, if the nth factor is omitted, the estimate of the jth coefficient is biased by $c_{nj}\alpha_n$ where c_{nj} is the coefficient of y_j in the regression of the omitted input on the included ones.

(b) The Covariance Matrix Method[15]

The general approach of this method is to postulate *a priori* the correlation coefficients between disturbances and then to calculate the equations relating the moments of observations of Y_0, Y_1 and Y_2 to the parameters α_1, α_2, etc.

With a two-factor model, the estimating equations are conveniently thrown into matrix form. If A is the matrix of coefficients of the equation system and y and u are column vectors of variables and disturbances, respectively, we have

$$Ay = u \,, \tag{35}$$

$$Ay(Ay)' = uu' \,,$$

$$Ayy'A' = uu' \,. \tag{36}$$

If we denote the population covariance matrix of the y's by $V(y)$ and that of the u's by $V(u)$, the equations become

$$AV(y)A' = V(u) \,.$$

The estimating equation is

$$\hat{A}\hat{V}(y)\hat{A}' = \hat{V}(u) \,, \tag{37}$$

where the $\hat{V}(y)$ denotes the sample values of the covariance matrix. These values are consistent estimates of the population matrix [**192**]. With the simple perfect competition model we have eight unknowns[16] and six equations. In order to obtain estimates of α_1 and α_2, Marschak and Andrews postulated values for the two coefficients of correlation between disturbances ϱ_{01} and ϱ_{02}, and then represented the equation system by plotting ellipses and conics from the covariance matrix of observations.

The covariance matrix method has not been used at all extensively,[17] probably because the equations are nonlinear in the coefficients. There are, however, other serious disadvantages of the method; in particular that (a) the requirement that the covariance structure of the disturbances must be specified, and that (b) little useful information is available about the small sample properties of these estimators.

[15] Marschak and Andrews [**203**].

[16] The two coefficients α_1 and α_2 and the six terms in the covariance matrix.

[17] I can find only one application: Verhulst [**321**].

(c) *The Factor Shares Method*

By far the simplest method of estimating the structural coefficients is from factor shares. In the perfect competition model, the estimate

$$\widehat{\log \alpha_1} = \frac{1}{n} \sum_{i=1}^{n} (r_{1i} - r_{0i}) \tag{38}$$

is a best linear unbiased estimator of $\log \alpha_1$ (similar estimates $\widehat{\log \alpha_2}$ have these properties for $\log \alpha_2$). Unfortunately, $\exp(\widehat{\log \alpha_1})$, is not an unbiased estimate of $\exp(\log \alpha_1)$. As Wolfson and Nerlove have shown, the estimate, although consistent, is biased downwards because of the convexity of the logarithmic transformation. However, it seems that in practical cases the bias may be very small because of the low relative variation in the α_{1i}'s.[18]

It is curious that the factor shares method was neglected for so long;[19] even in the extensive and penetrating discussion of Marschak and Andrews [**203**], the marginal productivity relation was not used in estimation. The reason for this neglect was probably the desire of early authors to *test* their estimates of the production function against the known facts about the distribution of income among factors of production. To have incorporated parameters estimated from factor shares would have turned their test into a tautology. One argument against the factor shares method is that it does not enable one to test hypotheses about economies of scale. If the factor shares method is used to estimate *all* the parameters, then the sum will be unity.[20] When the industry can be supposed to be near enough to perfect competition and there is no point in testing for the existence of increasing (or decreasing) returns to scale, the factor shares method will probably give good estimates of α_1 and α_2.

(d) *Instrumental Variables*

In estimating Cobb-Douglas functions, instrumental variables have hardly ever been used. To get consistent estimates of the parameters of the production function, we must use two instrumental variables, z_1 and z_2, which are

[18] Ignoring terms of order $1/n^3$ the relative bias is $\sigma^2/2n$. Dhrymes [**74**] has recently developed unbiased estimators.

[19] Klein [**169**] was the first author to make extensive use of this method of estimation. See also Solow [**286**].

[20] Klein [**169**] ingeniously avoids this criticism by estimating the ratios α_1/α_2 by factor shares methods and then using least squares methods to complete the estimation. But it must also be recalled that Klein's model was designed for the regulated railroad industry; the ordinary conditions of minimum cost, maximum profit were modified. Solow [**286**] also used factor shares methods. Effects due to economies of scale were collected in the residual term $A(t)$, which Solow attributed to technical progress.

distributed independently of u_0. It is not easy to find variables with the characteristics of z_1 and z_2 in cross section studies. The variable u_0 reflects the technical efficiency of the entrepreneur. All the obvious candidates for z_1 and z_2 seem to be likely to be related to u_0. In time series analysis, the position is perhaps a little better. Hoch [**141**] and Mundlak [**224**] have recently suggested that, with joint time series and cross section data, a transformation (or covariance analysis) can be introduced to make the disturbance in the marginal productivity equation independent of that in the production function. Mundlak then proposes that the two disturbances in the marginal productivity equations be used as instrumental variables in the production equation. If the transformation is successful in securing independence, this will solve the problem of biases due to varying entrepreneurship in cross section studies and due to simultaneous equation conditions. The efficiency of these methods of estimation has yet to be fully tested with empirical data. Hoch has, however, applied the method of covariance analysis to a time series cross section sample of farms. The evidence suggests that the new methods did reduce simultaneous equations bias.

5. TIME SERIES STUDIES

Early studies of the production function all used time series data. The easiest series to measure was that for labour input. This variable should be measured in terms of a standardised unit, such as the "equivalent man hour," and by using the geometric aggregation methods suggested by Shepherd and Klein [**274, 167**]. Two practical problems are, first, to aggregate different kinds of labour weighted according to skill, sex, and age, and, second, to allow for changing weights over time. In practical studies these problems have received little attention, partly because of the lack of data. For all the time series studies of aggregate data, simple measures were used, such as average number of persons employed or the number of man hours worked. Clearly these empirical correlates introduce bias into the estimates of the labour input since we know that, in all countries, systematic changes have taken place over time in the age-sex-skill structure of the labour force. Over the usual periods we should normally expect an increase in age and a growing fraction of skilled employees. One would thus get a downward bias over time in the measure of labour input.

The measure of output should ideally be in terms of homogeneous physical units. In practice, the best possible indicator is probably real net domestic product. Early investigators used indexes of physical production. Later studies have tended to use deflated gross or net national product. All the problems here, however, are essentially those familiar conundrums of index number theory.

The most intractable difficulties are involved in measuring capital. In a mythical world where all machines are the same over time, the ideal measure would appear to be the number of homogeneous machines. Unless this world were strictly classical we should, however, find that, over slump periods, some machines were not employed and others were underemployed. This suggests that the ideal measure, which should enter into a production function, is the volume of capital services.[21] There is no available measure of capital services; such services are rarely sold by one firm to another. Solow [**286**] has used the technique of reducing the measured capital stock by the percentage of the total labour force employed. Unfortunately this adjustment probably goes too far since, to the individual firm with its existing capacity, the cost of using the surplus machines is less than the cost of hiring unemployed labour; so we should expect the fall in labour employment to be proportionately greater than the decrease in the use of capital.[22]

The concept of capital we should employ in production functions should correspond to the capital services provided. Thus net capital is obviously the wrong measure since it reflects the age of the equipment; clearly the machine that gives identical capital services over its ten years of life should have the same value each year whatever the net worth of the machine. Gross capital is more satisfactory; however it should be amended to take account of, not the decline in value, but the decline in efficiency of a piece of equipment as it ages. In practice most authors have used net capital concepts. The bias introduced thereby is probably not large unless the age distribution of the capital stock is extraordinarily irregular.[23]

But the main difficulties of measuring capital arise from the fact that the stock consists of various kinds of machines, buildings, and land at different stages of their life cycles. Combining these into a monetary measure involves not only all these usual index number problems but also difficulties which are peculiar to capital. As Mrs. Robinson has pointed out, the unit of measurement varies with the rate of profit; the relative prices of equipment are determined by future profit expectations.

The normal procedure is to measure capital value and then deflate by an

[21] See, for example, Solow [**289**]. This is not entirely satisfactory since we are then ignoring the very important economic activity of *producing* capital services.

[22] There are, of course, many other serious difficulties connected with the measurement of the available labour force or working population. Massell [**204**] has examined these problems and suggests additional reasons for supposing that Solow's method underestimates the capacity in use. Instead of using labour employment, Massell fitted a trend for the capital-output ratio for full employment (excluding the war years) and used these to give more satisfactory results than the approach of Solow.

[23] Anderson [**4**] has recently shown that there is a considerable trend in the ratio of net to gross values of fixed assets in the U.S.A. It seems likely that the ratio varies cyclically, but there is no direct evidence on this point.

index of prices. The ideal price index would reflect the changing value of capital services. Unfortunately such a series is not available, and, instead, investigators have deflated by an index reflecting the changing *costs* of labour and other inputs used in making capital equipment. As Anderson [**4**] has shown, increased productivity of labour, etc., in the industries producing capital goods is not incorporated in the price index. Thus the increase in the quantity of capital is considerably understated.

Some of the practical problems of capital measurement would disappear if we knew more about obsolescence. It would be useful if there were an extensive market for second hand capital goods. But data on second hand prices are notoriously scarce. Statisticians have normally ignored such information. The usual technique of measuring fixed capital is the "perpetual inventory method." An inventory is maintained of the capital stock at base year prices by adding gross investment and by deducting capital consumption annually.[24] Other methods of estimating gross fixed capital include, first, collecting the valuations for fire insurance [**17**], second, multiplying valuations for estate or inheritance duties by the reciprocal of suitable specific mortality rates, and, third, multiplying the property income stream by the reciprocal of the estimated rate of return (the Giffen method).[25]

Two of these methods give rise to net worth concepts and only the fire insurance method [**17**] provides a gross capital concept. But even allowing for differences in definition, the measures in practice give quite different values.[26] While the ultimate test of a statistic is whether it is useful in practice, that is, whether one gets better predictions with the capital figure than without it, critics of empirical production functions are justified in expressing some skepticism of results which depend on such unsatisfactory foundations.

The exploratory paper of Cobb and Douglas was concerned with aggregate series for manufacturing and mining industry in the U.S.A. This pioneering work drew trenchant criticism. Mendershausen argued that Cobb and Douglas had measured a confluent relation, and that the particular results obtained depended critically on the errors in the variables [**211**]. He suggested an alternative hypothesis where the empirical estimates of the coefficients can be interpreted as indicators of differences between time trends in output, labour, and capital. The coefficients measured technical progress and not a production function.

Attempts to estimate technical progress as a separate term in the aggregate production function were first made by Tinbergen. The simplest hypothesis

24 For an account of this method see Goldsmith [**103**].

25 See [**8**].

26 See [**17**].

is that the Cobb-Douglas function has an exponential trend, i.e.,

$$Y_0 = AY_1^{\alpha_1}Y_2^{\alpha_2}e^{\alpha_3 t}U_0 . \tag{39}$$

This hypothesis specifies that technological progress is, on the average, neutral in both Hicks and Harrod senses. In a time series analysis for 1870–1914 of aggregate data, Tinbergen estimated the percentage increase of output due to neutral technical progress as 1.5 for Germany, 0.3 for Great Britain, 1.1 for France, and 1.1 for the United States [**309**]. A similar analysis by Aukrust and Bjerke [**13**] for Norway, 1900–1955, showed that 1.8% of the 3.4% annual increase in output was due to neutral technical progress. Niitamo has estimated a function for Finland with a variable indicating the growth of education. Solow [**286**] has analysed technical change in the U.S.A. by estimating the value of α_1 for each year 1909–49 from the proportion of total value added accruing to capital. This enabled Solow to divide the increase in output between capital or labour intensive or neutral technical progress, and those parts which are due to the increases in the quantities of labour and capital. Neutral (or offsetting non-neutral) technical change accounts (on average) for about 90% of the annual increase in output per unit of labour and increases in capital intensity for 10%.[27] Arrow, Chenery, Minhas, and Solow have reinterpreted these results, using the SMAC form of the production function by fitting:[28]

$$\log\left(\frac{P_1^*Y_1}{P_0Y_0}\right) = a_0 + a_1 \log (P_1^*) + a_2 t \tag{40}$$

where $a_1 = 1 - \sigma = .431$ and $a_2 = -\lambda(1 - \sigma) = .003$. This assumes that technical change is neutral and is reflected in a geometrical rate of increase. SMAC found that the elasticity of substitution σ was much less than unity, i.e., only .569. The resulting annual productivity increase of 1.8% compared closely with the Cobb-Douglas estimate of 1.5%.[29] Solow [**291**] has recently analysed the U.S. time series with the hypothesis that *all* progress is embodied in new equipment. He calculated an "efficiency standardised" capital series, taking account of the increasing efficiency of new equipment, and after allowing for the unemployment of capital, he calculated the coefficients of the restricted Cobb-Douglas function. These were then used for projections of capital requirements.

[27] As amended by W. P. Hogan [**142**]. In a more recent paper, Solow [**289**] has developed this model to take account of the improvement in efficiency of capital equipment over time.

[28] The asterisk indicates that the wage rate is measured with output as numeraire.

[29] These results for the U.S.A. have been confirmed by Kendrick's analysis of "total productivity."

TABLE I

PRODUCTION FUNCTIONS FOR AGGREGATES OF INDUSTRIES: TIME SERIES ESTIMATES

Reference	Period	Country	$\hat{\alpha}_1$	$\hat{\alpha}_2$	$\hat{\alpha}_1+\hat{\alpha}_2$		Reference
[79]	1899–1922	U.S.A.	.81	.23	1.04		Douglas (1948)
			.78	.15	.93		
			.73	.25	.98		
			.63	.30	.93	(trend eliminated)	
[117]	1907–29	Victoria	.84	.23	1.07		Handsaker and (Douglas (1937)
[78]	1901–27	New South Wales	.78	.20	.98		Director Douglas *Theory of Wages*)
[79]	1915–16, 18–25	New Zealand	.42	.49	.91		Brown in Douglas (1948)
[339]	1923–40		.54	.46	(restricted)		Williams (1945)
[324]	1920–40	U.S.A. mining and manufacturing	1.34	.93	2.27	(linear trend)	Wall (1948)
			1.46	.49	1.95	(parabolic trend)	
[289]	1909–49	U.S.A.	.65	.35	(restricted)		Solow (1957)
[332]	1909–49	U.S.A.	.99	.23	1.22		Walters (1962)
[13]	1900–55	Norway	.76	.20	.96	(exponential trends)	Aukrust and Bjerke (1959)
	1917–55	Norway	.51	.28	.79		
	1917–39	Norway	.62	.72	1.34		
	1922–39	Norway	.39	.62	1.01		
[233]	1925–52	Finland	.78	.22	(restricted)		Niitamo (1958)

TABLE II

PRODUCTION FUNCTIONS FOR INDUSTRIES: TIME SERIES ESTIMATES

Reference	Country and Period	$\hat{\alpha}_1$	$\hat{\alpha}_2$	$\hat{\alpha}_3$ (raw material)	$\hat{\alpha}_1+\hat{\alpha}_2$ $(+\hat{\alpha}_3)$		
[125]	U.S. Agriculture 1920–41	1.70	.81		2.51 with exponential trend	1.6% p.a.	Tintner (1952)
[181]	U.K. Agriculture 1924–47	.18	.37		.55	1.03% p.a.	Lomax (1949)
[186]	U.K. Cotton industry 1948–52	.33	.70 (loom hours)	.26	1.29	.45% p.a.	Lomax (1953)
[183]	U.K. Coal	.79	.21		(restricted)	—	Lomax (1950)
[280]	Canada-Automobiles 1918–30	.96	.41		1.37	exp. trend $-.034t$ $+.00134t^2$	Smith (1945)

These results of Solow and SMAC depend on the assumption of constant returns to scale in the aggregate production function of the U.S.A. Murray Brown and Popkin [**46**], however, have recently used a generalised SMAC function to measure economies of scale and the nature of technical progress in four epochs from 1890 to 1958. Their results show that non-neutral technical progress was important. The results of Brown and Popkin and the conclusions of other recent studies tend to discredit the hypothesis of constant returns [**306, 332**]—although they do not shake the conclusion that technical progress was still a far more important cause of growth than capital intensity. But the large tribute to neutral technical progress is partly due to the fact that Solow started by analysing that part of the increase in output per head which could be directly credited to increased capital intensity; the remainder was then labelled "technical progress." But, as Levine has pointed out [**179**], this residual includes all interaction terms and there is no reason why all interaction effects should be attributed to technical progress. If, in Solow's results, we first extracted technical progress, Levine has shown that 19% would be attributed to capital intensity.[30]

The results of time series regression studies are set out in Tables I and II. Most of the studies refer to the period preceding the depression of the 1930's; the exceptions are Wall's and Walters' studies for the U.S.A. and Aukrust and Bjerke's work on Norway. If we exclude these last three sets of data, we find two important results. First, the sum of the coefficients usually approximates closely to unity.[31] The linearity of the production function seems to be a remarkably consistent finding between one country and another. The second important result is the agreement between the labour exponent and the share of wages in the value of output. These two findings have been interpreted as confirmation that the aggregate production function has constant returns to scale and that the marginal productivity of labour is equal to the wage rate.

These interpretations, however, depend on the estimates being good approximations to structural parameters. Whether the values in Table I are good estimates of structural parameters remains doubtful. Simple single-equation least-squares techniques were used, and none of the authors tested to see if there was simultaneous equation bias or whether the production function was identified. Furthermore, it seems that the estimates of the parameters are not stable over time. Few of the studies cover the years of the slump in the 1930's; the coefficients of Wall and some of those of Aukrust and Bjerke which apply to the 30's are clearly not consistent with the other values, nor with the traditional hypotheses. Furthermore, it is certain that

[30] This figure would be increased even more if we took into account the underestimate in the increase in capital stock [**4**].

[31] Large standard errors were characteristic of interwar studies.

other investigators have got regression results for the interwar years which have been disappointing.[32] Part of the explanation, as Douglas observed, may be due to the fact that the capital input data do not reflect the immense unemployment of equipment in the 30's. This view is partly confirmed by Solow's results which were obtained from series in which the capital input has been adjusted by the fraction of labour employed.

The estimates for individual industries vary much more than those for countries. Some of the results are simply unacceptable—such as Tintner's values for U.S. agriculture and the estimates of Lomax for U.K. agriculture. Probably the explanation of these strange values can be found in the effects of trends and of the elimination of trends. On the other hand, the economies of scale which Smith found in the automobile industry do conform to one's expectations, and Lomax's values for cotton cannot be dismissed as impossible.

One cannot conclude from this survey of time series estimates that the simple single-equation Cobb-Douglas specification has been uniformly successful. The results suggest that the most important reason is the unsatisfactory state of the data—and, in particular, of the capital series. But one also suspects that the specification is itself too simple. We know from other evidence that the adjustment of the capital stock takes time—usually longer than a year—and that the technological efficiency of new capital is greater than that of old [**283**, **291**]. Probably the adaptation of production function analysis to include these phenomena would produce more stable and satisfactory results.

6. CROSS SECTION AND INDUSTRY STUDIES

The use of cross section data for the analysis of production functions was pioneered by Bronfenbrenner and Douglas in 1939 [**39**]. Since that date these studies have been much more numerous than time series estimates. Cross section studies can be divided into three groups according to the type of basic data. First, the observations may be aggregates for industries giving an "interindustry" production function. Second, the figures may relate to individual firms. Third, the estimates of capital, labour, and output may cover aggregates for industries in different countries, giving an "inter-state" estimate of the parameters of the production relation.

The raw material of these studies is derived largely from censuses of manufactures, and usually cross section studies are restricted to years when census material is available. With both inter-industry and inter-firm studies one of the great difficulties encountered with time series data disappears; there is no need to deflate the value data. This is not true, however, for in-

[32] See, for example, Douglas' remarks on pp. 21–22 of [**79**], Bronfenbrenner [**37**, p. 43, note 12], and Felsenthal's results referred to by Smith [**281**].

ternational cross sections since the figures for different countries must be deflated to a common monetary unit.[33] Thus the "value added" data used to measure output,[34] and the value of net physical capital, used to measure capital stock, can be usually employed directly in the computations. This is, however, not the case with labour which is, of course, normally measured in quantity terms. While changes in age-sex composition over aggregate time series are usually small, such variations over the cross section are large and should not be ignored; few investigators, however, have reduced the labour figures to equivalent male adults.

Again the main headaches occur in measuring capital. One of the troubles with time series data was the changing utilization of capital. An advantage of cross section studies is the fact that variations in the amount of idle capacity are probably less over the cross section than for time series. There have, however, been few attempts to adjust estimates of cross section capital stock for the percentage employment of capacity. Klein [**169**] achieved this result by using the number of train hours as a measure of the input of capital services on the railways.

The quality of the data on capital has been recognised by most authors to be extraordinarily poor. In the U.S.A. the compilers of the census of manufactures thought that the capital data were so unreliable that they ceased publication after the first world war. The main reason was that the firms returned the book value of assets, and this figure is not usually closely related to the market value.[35] The gap left by the official statisticians has been partly filled by various expedients. For example, in their study of industry aggregates SMAC used a version of the Giffen method. The net value of the capital stock of an industry was computed by first finding the net rate of return on capital from the balance sheets of firms in the industry, and then multiplying the net value added (less the wage bill) by the reciprocal of the net rate of return. The disadvantages of such a measure stem from the artificial valuation of assets in balance sheets and from the variability of profits over time.[36] Indeed, net profits on operations might be zero or negative!

While the inadequacies of the statistics are very serious, it seems likely

[33] One also has some time-deflation to do since the countries usually have different years for the census.

[34] Again the correct measure of output is net value added, but many authors prefer to avoid the arbitrariness of accountants' depreciation and so use gross value added instead.

[35] For a complete criticism, see [**39**]. Anderson [**5**] has shown that in one industry, the undervaluation was about 50%.

[36] This was particularly important in [**8**] since samples over different years and different countries were used. Some years were mild depression periods, and some were boom times.

that, in cross section analyses, there are fewer causes for worry than with aggregative time series studies. With this rather chilly comfort, let us now turn to the problems of specification. Consider first the inter-firm production function. The set of observations that are generated by a cross section of firms depends on how near the firms are to their equilibrium outputs, on the price structure of inputs, and on the conditions of competition. If we make the assumption, with the Cobb-Douglas equation, that there is perfect competition and that the marginal productivity conditions are *exactly* satisfied (i.e., $u_1 \equiv u_2 \equiv 0$), then the observations will measure the variation of output with the inputs, capital and labour, in fixed proportion and with the residual, "entrepreneurial ability." There is no way of measuring the separate effects of capital and labour unless there are disturbances in the marginal productivity equations. This implies that the suitable model for a competitive industry is the basic simultaneous equation system (24), (25), and (26). Alternatively, it is, of course, possible to get variations in the capital-labour ratio, with marginal productivity conditions satisfied exactly, if there is imperfection in at least one of the two factor markets. But to estimate the parameters of the production function, we must know the elasticity of supply of the factor; clearly this approach is of little practical use. Another way out of this difficulty arises if we can choose firms which are faced with considerably different factor prices. Within one country where there is reasonable mobility of resources, this does not seem a likely situation.[37] But between one country and another there are substantial differences in factor prices, and this provides the variation in external circumstances required to generate observations of the production function [**8**].

The upshot to this discussion of the specification problem is that particular attention must be paid to the simultaneous equation problem and to the bias due to the role of entrepreneurial ability. The "disturbances" must play an important role in the model, and their specification, intercorrelations, and variances have an important bearing on the interpretation of the results. It is, however, unfortunate that in practice little attention has been paid to these aspects of empirical production functions. The coefficients in industry studies have usually been obtained by single-equation least-squares methods.

The results for analyses of individual industries cannot easily be summarised in a convenient table. Inputs and outpus are usually defined and measured in a particular way from one industry to another. Industry studies also differ according to the way in which they allow for inputs of raw material. Bearing these variations in mind, we have set out in Tables III and IV examples of the sort of results which have been derived from these studies. This is in no sense a comprehensive list, but it does include most of the important work

[37] See, however, Nerlove [**229**].

TABLE III

INTER-FIRM STUDIES OF INDUSTRIES

VALUES OF COBB-DOUGLAS COEFFICIENTS

			Labour α_1	Capital α_2	Raw Materials α_3	Total $\alpha_1 + \alpha_2 + \alpha_3$	Reference
		UTILITIES, RAILWAYS					
		France					
[**321**]	1945	Gas	.83	.10		.93	Verhulst (1948)
			.80	.14		.94	
		U.S.A.					
[**169**]	1936	Railroads	.89	.12	.28	1.29	Klein (1953)
		OTHER EXTRACTIVE AND MANUFACTURING INDUSTRIES					
		U.K.					
[**183**]		Coal	.79	.29		1.08	Lomax (1950)
[**176**]		Coal	.51	.49		1.00	combined cross-section time-series, Leser (1955)
		U.S.A.					
[**39**]	1909	Clothing	.98	–.07		.91	Bronfenbrenner and Douglas (1939)
		Foods	.72	.35		1.07	
		Metals and Machinery	.71	.26		.97	
					Share of Wages	Total	
		India					
[**225**]	1951	Cotton	.92	.12	.63	1.04	Murti and Sastry (1957)
	1952	Cotton	.66	.34	.75		
	1951	Jute	.84	.14	.60	.98	
	1952	Jute	.91	.34	.71		
	1951	Sugar	.59	.33	.30	.92	
	1952	Sugar	.24	.94	.32		
	1951	Coal	.71	.44	.57	1.15	
	1952	Coal	.58	.58	.55		
	1951	Paper	.64	.45	.41	1.09	
	1952	Paper	.59	.49	.39		
	1951	Basic	.80	.37	.37	1.17	
	1952	Chemicals	.82	.40	.48		
	1951	Electricity	.20	.67	.30	.87	
	1952	Electricity	.02	1.00	.30		

in this field. Again we find that (a) the sum of the coefficients is near unity, and (b) the labour coefficient is approximately equal to the share of wages in value added. Even if estimates of structural parameters have been obtained,

TABLE IV

AGRICULTURE

VALUES OF COBB-DOUGLAS COEFFICIENTS

		Labour	Land	Fertilizer	Units of Productive Livestock	Total	Reference
	International						
[23]	1949 General agriculture	.28	.39	.29	.05	1.01	Bhattacharjee (1955) (International industry cross section)
	Australia			(feed)	(other)		
[270]	1955 Milk	.23		.13	.62	.98	Schapper and Mouldon (1957)
	India						
[269]	1955 Arable	.56	.08	.25		.89	Sarkar (1957)
	Japan						
[117]	1939 Rice	—.07	.73			.66	Kamiya (1941)
	,,	—.53	1.30			.77	
	South Africa						
[126]	1950–1951	.18	.21	.54		.93	Heady and du Toit (1954)
	1951–1952	.13	.24	.60		.97	
	Cow and Calf ranches	.19	.19	.52		.90	
	Growing and fattening ranches	.13	.28	.55		.96	

TABLE IV (continued)

AGRICULTURE

VALUES OF COBB-DOUGLAS COEFFICIENTS

	Labour	Land	Capital	Live-stock	Feeder Cattle	Machinery and Draft Animals	Total	Reference
U.S.A. Arable and various								
[341] 1939 Cotton	.45	.29	—	.033	—	.13	.90	Wolfson (1958)
Corn-Hog	.37	.35	—	.095	.011	.17	.99	Wolfson (1958)
Wheat	.41	.23	—	.045	.013	.31	1.00	Wolfson (1958)
[119] 1939 Iowa (various)	.03	.23	(equipment) .08	.48		(other) .03	.850	Heady (1946)
[311] 1942 Iowa (various)	.16	.29	.42				.87	Tintner (1944)
[122] 1950 Crop	.33		.38		(Fertiliser) .07	.15	.93	Heady (1952)
[125] Montana	.04	.50	.58				1.12	Heady and Dillon (1962)
N. Iowa	.08	.91	.17				1.16	Heady and Dillon (1962)
S. Iowa	.09	.80	.39				1.38	Heady and Dillon (1962)
Alabama	.32	.39	.46				1.17	Heady and Dillon (1962)
[123] 1950 Iowa Crop Share	.12	.77	.32				1.21	Heady (1955)
Cash Leases	.10	.97	.31				1.38	Heady (1955)
1951 Iowa Crop	.09	.97	.15				1.21	Heady (1955)
1951 Iowa Crop Share	.10	.73	.20				1.03	Heady (1955)
[141] 1946 Minnesota (mixed)	.04	.20	.19		(feed etc.) .29	(current expenses)	0.73	Hoch (1962)
1951	.06		.32		.29	.17	0.83	Hoch (1962)

	Labour	Capital	Other	Total	Reference
U.S.A. Livestock and Dairy					
[81] 1950 Dairy and General (milk)	.15	.25	.54	.94	Drake (1954)
[125] 1950 Livestock (Montana)	.08	.94		1.02	Heady and Dillon (1962)
N. Iowa	.08	.91		.98	
S. Iowa	.12	.98		1.10	
Alabama	.23	.74		.97	
[122] 1950 Livestock (Midwest)	.25	.68		.93	Heady (1952)

these results do not necessarily imply that the production function is linear and homogeneous and the wage rate is equal to the value of the marginal product. For most of the cross section studies, value figures were used to measure both output and capital. Thus the first result (a) implies only

$$\alpha_1 \beta_0 + \alpha_2 \frac{\beta_0}{\beta_2} = 1 , \tag{41}$$

and the second result (b) becomes:

$$\alpha_1 \beta_0 = \frac{P_1 Y_1}{P_0 Y_0} . \tag{42}$$

We cannot say whether the production function has constant returns or not—unless, of course, we know the values of β_0 and β_2. The marginal productivity condition corresponding to the second result is

$$\alpha_1 \cdot \frac{\beta_0}{\beta_1} = \frac{P_1 Y_1}{P_0 Y_0} , \tag{43}$$

and this combined with (42) means that there is no evidence of labour monopsony, i.e., $\beta_1 = 1$. In order to sharpen discussion, however, we shall suppose that, in the cross section studies, $\beta_0 = \beta_1 = \beta_2 = 1$. Thus when the sum of the coefficients is unity, this is interpreted as evidence of no economies or diseconomies of scale.

The evidence of constant returns to scale in industry studies is quite strong. The most important exception to this rule is for U.S. railroads where Klein's results imply increasing returns. Perhaps the most strange exception is the Indian coal industry which Murti and Sastry found to be enjoying *increasing* returns to scale. For studies of U.S. agriculture, the sum of the coefficients did not usually differ significantly from unity, and in Western Australia and South Africa, the industry enjoyed constant returns to scale. But in pre-war Japan not only were there decreasing returns, but because of the excess of family labour, Kamiya argued that the marginal labour product was negative!

In industry cross section studies, there seems to be less agreement between the share of wages and the labour coefficient. Examining the Murti and Sastry data for Indian industries in 1951, we see that the shares of wages are, on the average, lower than the coefficients. In sugar and chemicals, the share of wages is only about half the value of the coefficient. Sampling errors may account for part of this difference, but it is unlikely to explain all these deviations. Comparing the results for Indian industry in 1952 with those of 1951, the most remarkable feature is the variation in the coefficients from one year to another. The movement in the coefficient for the most reliable industry group (cotton) is in the *opposite* direction to the movement in labour's share. Indeed the Murti and Sastry results cast considerable doubt on both the stability of least squares estimates of the coefficients and the equality of α_1 to labour's share. Part of the instability may be explained if industries were in a process of adjustment at this time. The period 1951–52 was one of boom and rapid adaptation following the outbreak of the Korean War in 1950. Clearly the evidence would have to be examined in much greater detail in order to clear up these points. But it does suggest, however tentatively, that a dynamic lag model, with adjustments built in, might shed more light on these curious results.

A more general source of specification trouble may be the simultaneous equation effect since all the results, except those of Klein, Verhulst, Wolfson, and Hoch, were estimated by least-squares single-equation methods. Klein used a variant of the factor shares method of estimation for some parameters and then inserted these estimates in a second-stage least-squares regression. Klein was the first to test the normality and independence of the logarithmic disturbances in the simultaneous equation system; in the case of U.S. railroads, the hypothesis was not rejected. Verhulst, in his study of the French gas industry, used the coveriance matrix method. He chose values of α_1' and α_2' that gave a minimum estimate of the variance of the disturbance in the production equation, combined with maximum values for the correlations between the disturbance in the production equation and the disturbance in the marginal productivity equations, because "the firms which have the best technical efficiency have at the same time the best economic efficiency." Certainly, plausible results appear to require high positive correlations between the disturbances in Verhulst's data. But from the argument above, this throws doubt on Verhulst's conclusions. Hoch used covariance analysis with considerable success in eliminating nuisance variables and simultaneous equation bias.

There remains the problem of identification. Apart from Hildreth and Jarrett and Klein, empirical research on intra-industry production functions has not been much concerned with formal questions of identification. There appears to be a general presupposition that the observations were distributed randomly about the surface of the production function. With the exception of Verhulst, no author gives the original observations, so it is impossible to check hypotheses about identification. As argued in Marschak and Andrews, Nerlove, and Walters [**203**, **229**, **331**], *a priori* reasoning does not lead to the hypothesis of the independence of disturbances. Generally much more work is required in testing empirically the identification of the production function before we can place any considerable confidence in the results interpreted as estimates of structural coefficients.

The inter-industry studies of the production function seem, on *a priori* grounds, to be much less satisfactory than the inter-firm studies. In the basic specification it appears to violate one's common knowledge of industrial structure to suppose that each industry has the same α_1, α_2 and A. The results for inter-firm functions suggest that there are significant and important differences between industries. To ignore these variations in setting up the specification seems to invite the risk of serious biases in the results.[38]

One of the other main criticisms of inter-industry studies is that the Cobb-Douglas equation simply measures the structure of industry.[39] We cannot use the resulting coefficients to predict the effect of increased factor inputs

TABLE V

INTER-INDUSTRY PRODUCTION FUNCTIONS

CROSS SECTION ESTIMATES

Reference	Year	Country	Labour α_1	Capital α_2	Sum $\alpha_1+\alpha_2$	
	1889	U.S.A.	.51	.43	.94	Douglas (1948)
	1899		.62	.33	.95	
[79]	1904		.65	.31	.96	
	1909		.63	.34	.97	
	1914		.61	.37	.98	
	1919		.76	.25	1.01	
	1912	Australia	.52	.47	.99	Gunn and Douglas (1941)
	1922–3		.53	.49	1.02	
[110]	1926–7		.59	.34	.93	
	1934–5		.64	.36	1.00	
	1936–7		.49	.49	.98	
[109]	1910–11	Victoria	.74	.25	.99	Gunn and Douglas (1940)
	1923–4		.62	.31	.92	
	1927–8		.59	.27	.86	
	1933–4	N. S. Wales	.65	.34	.99	
[79]	1937–8	S. Africa 1.	.66	.32	.98	Browne (1943)
		2.	.65	.37	1.02	
	1923	Canada	.48	.48	.96	Daly and Douglas (1943)
[65]	1927		.46	.52	.98	
	1935		.50	.52	1.02	
	1937		.43	.58	1.01	
[339]	1938–9	N. Zealand	.46	.51	.97	Williams (1945)
[182]	1924	U.K. (industry)	.72	.18	.90	Lomax (1950)
	1930		.75	.13	.88	
[306]	1946	India	.66	.31	.97	Tewari (1954)
[84]	1947		.57	.50	1.07	Dutt (1955)
[225]	1951		.59	.40	.99	Murti and Sastry (1957)
[203]	1909	U.S.A. (industry)	.74	.32	1.06	Marschak and Andrews (1944)

[38] On the other hand, the manipulation of models with variable coefficients is fraught with difficulties.

[39] Just as the intra-firm function evaluates the pattern of entrepreneurial ability.

on production. Again these criticisms parallel those of inter-firm studies. The important task is to analyse the residuals to see if they conform to a specification which will enable consistent estimation and identification.

The results of inter-industry studies are set out in Table V. The figures are similar to those for time series and inter-firm studies, and the main conclusions are repeated, i.e., (a) the sum of the coefficients is about unity, and (b) the labour coefficient is nearly equal to labour's share in value added. Since value data were invariably used for measuring both capital and output, these results do not necessarily imply constant returns to scale in industry as a whole.[40] But this is probably of little importance. The result, $\alpha_1 + \alpha_2 = 1$, simply reflects the fact that variations in the size of industries as measured by their inputs is matched by proportional differences in their values added. Large industries are not more or less productive than small industries. This is an interesting, but not perhaps an unexpected result. It tells us, however, nothing at all about the economies of scale of the firm since large industries may consist of small firms and small industries of large firms.

Marschak and Andrews demonstrated, with data for the U.S.A. in 1909, the critical role played by the simultaneous system of equations. They showed that the data were consistent with a large number of values of the coefficients.[41] The least-squares single-equation solution was not in any sense superior to alternative estimates. Indeed if we put "reasonable" *a priori* conditions on the correlation matrix of the residuals, the least squares values are inferior to another set of solutions. The main concern of critics, however, has been the identification of the production function. The early commentators pointed out that the data may be explained by what Bronfenbrenner called the interfirm function, $P_0Y_0 = P_1Y_1 + P_2Y_2$. Evidence has been adduced by Phelps-Brown [**244**] to show that the scatter of the observations of Australia in 1909 can be explained in terms of this simple linear relation.[42] Thus, in fitting a Cobb-Douglas function (with $\alpha_1 + \alpha_2 = 1$),

[40] See above.

[41] See, however, Nerlove [**229**].

[42] From the linear relation we get

$$\frac{R_0}{R_2} = P_1\left(\frac{Y_1}{R_2}\right) + 1$$

i.e.,

$$d\left(\frac{R_0}{R_2}\right) = P_1 d\left(\frac{Y_1}{R_2}\right),$$

and from the log-linear Cobb-Douglas function with $\alpha_1 + \alpha_2 = 1$, we get

$$d\left(\frac{R_0}{R_2}\right) = \alpha_1 \frac{R_0}{Y_1} d\left(\frac{Y_1}{R_2}\right) = P_1 d\left(\frac{Y_1}{R_2}\right) \qquad \text{if } \alpha_1 = \frac{P_1Y_1}{R_0},$$

which is the same form as the linear equation.

we merely measure the share of wages in the value added. This result does not provide a test of the marginal productivity law.

The inter-industry results give, I think, the most unsatisfactory estimates of the production function. But aggregate industry data have been used with considerable success in *interstate* (or *international*) studies of the SMAC function. The authors used observations of the same industry in different countries to estimate the parameters.[43] Given that the industry has the same production function in each country, the different ratios of factor prices will generate observations which should trace out the production function [**8**].

The first task of SMAC was to estimate ϱ from the marginal productivity of labour equation. If the production function is the same for all countries, the regression of the logarithm of value added per worker on the logarithm of the wage level provides an estimate of σ or $1/(1 + \varrho)$. For all but one of the industries surveyed by SMAC, the coefficients were less than unity, implying that ϱ exceeds zero. This suggests that the Cobb-Douglas assumption that there is unit elasticity of substitution is discredited by the data. The low wage countries employ more capital, and the high wage countries less capital, than is predicted by the Cobb-Douglas function. This result is reinforced by the fact that all countries have different production functions. The most efficient are the high wage countries, and this is reflected by a varying λ with constant distribution parameters, δ. Thus the estimates of the regression coefficients give *over*estimates of the elasticities of substitution. But, on the other hand, the Cobb-Douglas hypothesis is not discredited by the detailed estimates from Japanese and American industry nor by one of the time series tests on American data. It can be concluded, therefore, that SMAC have adduced some evidence to suggest that the elasticity of substitution is less than unity—usually in the region of 0.6 to 0.9—but, in view of the contradictions in the results, the simple Cobb-Douglas function should not be confidently rejected at this stage.

The tests used by SMAC are similar to the factor shares approach in Cobb-Douglas estimation, except that the exponent $(1 + \varrho)$ enters. The first test was to see whether each country had the same production function and whether there were "constant returns to scale."[44] This stringent hypothesis was not consistent with the data. The second hypothesis was that variations in efficiency were neutral, i.e., that the λ varies from one country to another

[43] This method had been used by Bhattacharjee [**23**] to get a Cobb-Douglas function for agriculture.

[44] This refers to industries and not to firms; again large firms may exist in a small industry and small firms in a large industry. To some extent SMAC contradicts previous results by Chenery which suggest that economies of industry size are important.

but the value of δ does not. This implies that the ratio of the wage bill to the capital cost is given by

$$\frac{P_1^*Y_1}{P_2^*Y_2} = \frac{1-\delta}{\delta}\left(\frac{Y_2}{Y_1}\right)^{\rho}. \tag{44}$$

To test this hypothesis, data are required on capital, Y_2, and the price of capital, P_2^*. Unfortunately, the net capital figures obtained by using the Giffen method are very unreliable as indicators of true gross capital. But this evidence, however rough, does not refute the hypothesis of neutral variations in efficiency.

This simple result enabled SMAC to explore a rich seam of subsidiary hypotheses—with great ingenuity. But, with the possible exception of the hypothesis that ϱ is positive, the statistical foundations of the other SMAC hypotheses are rather shaky.[45] SMAC have used tentative tests by predicting the time path of the ratio of the wage bill to total capital returns. One such test applied by SMAC showed that the SMAC function with $\varrho > 0$ would give *worse* results than the Cobb-Douglas.[46] But this is, of course, only one result among others where the hypothesis, $\varrho > 0$, was not discredited, and where the SMAC function gives rather better results than the Cobb-Douglas.[47] Whether the SMAC complication is the best way of improving the Cobb-Douglas function, or whether some other complexity, such as factor disaggregation, lag systems, etc., would provide a better approach remains on the programme of research workers. Certainly Arrow, Chenery, Minhas, and Solow have infused production analysis with a new and exciting prospect.

7. COST FUNCTIONS

Instead of measuring the production function we can estimate directly the cost function. This can be derived from the reduced form of the production and marginal productivity relations. In practice, measuring cost curves is often more convenient than estimating the production function since the available accounting data are normally cast in money terms.

Many attempts have been made to establish the shape of cost curves by theoretical arguments. In particular, the shape of the *short run* cost function was thought to be evident from the principle of diminishing returns; and this law was either accepted as axiomatic or proved by *reductio ad absurdum*

[45] My colleague, Dr. John Wise, argues that even the hypothesis $\varrho = 0$ is not convincingly refuted by the data.

[46] Since the "best-fit" value of ϱ was negative.

[47] With an additional free parameter (ϱ) to fit to the data, the SMAC function should always give a "closer fit" than the Cobb-Douglas function.

kind of arguments. Menger, however, showed that the proposition of diminishing marginal returns does not necessarily follow from the assumption that the production function is (a) bounded and (b) nondecreasing. Even with the strong assumptions of a production function which is subhomogeneous and increasing with respect to one factor, the law of diminishing marginal product does not necessarily follow; strictly, we can deduce only the law of diminishing average product. Most economists, however, still regard diminishing marginal returns as a hypothesis which is rather more than promising.

An alternative hypothesis is that short run marginal cost is constant over wide ranges of output. This theory is supported by investigators who have studied business enterprise intensively [**6**]. It is also the basic presupposition in the work of cost accountants, and so it appears to be the most popular hypothesis in practice. There has been no attempt to show that this form of the short run cost function is a necessary consequence of some set of fundamental and self evident postulates. Supporters of this hypothesis have been content to argue either that this is what many businessmen think is their cost function [**86**] or that this hypothesis is the only one consistent with a large body of emprical evidence [**162**].

The traditional theory of the firm is not so helpful in suggesting the shape of the *long run* cost function as for the short run counterpart. There is general agreement that, with given factor prices, long run average cost falls for low ranges of output. Economies of scale arise first because of the ease of dealing with large quantities. The second reason is alleged to be the spreading of risks and reduction of the costs of uncertainty [**334**]. The third, and probably most generally accepted, reason for falling costs is the existence of indivisibilities in both men and capital equipment. Large machines are usually more efficient than small ones. The optimum size of machine for each process may differ so that high multiples of machines are required to reduce average costs to a minimum. Certainly, average costs at first decline with size; but there is very little agreement on the shape of the curve as output goes on increasing. E.A.G. Robinson argued that the coordination of management and control becomes increasingly less efficient and so rising cost of management gives rise to increasing long run average costs. Sargent Florence and others have criticised this rationalisation on the grounds that the propositions have not been tested in any systematic empirical study. It might also be urged that recent developments in computors and other managerial techniques have *increased* the relative efficiency of large managements.

To summarise, the theoretical arguments suggest that the short run average cost curve has the typical U shape, although Menger has added several reservations. Theory is reasonably clear on the proposition that long run average costs may be expected to decline at first with increasing scale. But for high outputs the theoretical arguments do not seem to be so convincing.[48] Choice between the alternatives must depend on the empirical evidence.

Economic theory defines costs to the firm as those payments which have to be made to induce factors of production to continue in their employment with the firm. Payments to owners of productive services which are specific to the *firm* and which are worthless if not employed by that particular firm are rents or, as Stigler calls them, non-cost outlays. Expenditures on services which are specific to the industry are costs to the individual firm, but they are non-cost outlays as far as the industry is concerned. It is also convenient to divide costs into fixed and variable costs. Fixed costs have to be met whatever the level of output, whereas variable costs increase or decrease with the quantity of output. The variability of costs also depends on the period of adaption. In short periods, the "plant" cannot be varied. Existing machines can either be used or left idle. In the long period all factors of production can be varied, except those which are specific and fixed for the firm, such as entrepreneurial ability.

The definition of cost adopted by theorists naturally involves grave difficulties in application. Consider first the *long* period concept. The alternatives considered include changing *all* factor inputs, except those which are fixed for the firm. All disbursements of the firm, except for returns to fixed inputs specific to the firm, must be considered as costs. The appropriate definition of cost is then total revenue *less* the returns to entrepreneurial ability. But, of course, we have no empirical measure of entrepreneurial returns. There is a way out of this impasse. We have supposed that the long run choice of the entrepreneur is to produce either *nothing* or some quantity of the *particular* commodity of his industry. But, obviously, he may also transfer his entrepreneurial ability to other industries. If his expected entrepreneurial income in the most "suitable" alternative industry is only slightly below his expected earnings in his present industry, this must be regarded as a *cost* of output. Consequently, the appropriate empirical definition is total disbursements, which is identical to total revenue. Average long run cost is then equal to price [**101**]. Data on prices are usually available, and one is strongly tempted to use this perfect competition model where it seems applicable [**56**].

With an industry or firm in a markedly monopolistic situation, the "price"

48 Into the midst of this conjecture Alchian [**1**] has recently launched a suggestion for a substantial revision of the propositions on cost. Instead of describing costs as a simple function of the time rate of output, Alchian argues that costs are a function of the total planned output, the period before the first delivery, and, as usual, the rate of production at each moment. Average costs diminish as total planned output increases but marginal costs rise as the output *rate* expands. Alchian's proposals may go far towards reconciling engineers' and economists' conceptions of cost variations.

definition of long run average costs will not usually be suitable. Price includes elements of monopoly profit per unit of output. Clearly those specialised factors, controlled by the firm, which are not marketable, are recipients of rents, and these expenses should be excluded from costs. These "factors" may include a very specialised entrepreneurial skill which is a natural endowment, exclusive local monopoly rights granted by the State, public carrier licences, Fellowships of the Royal College of Surgeons, etc.; none of these attributes can be bought on the market. The residual income to these factors is pure rent. The main difficulty is in obtaining statistics to estimate these non-cost outlays. In practice we have to use accounting data. Most studies have excluded accounting profits from cost. Probably in the vast majority of cases this results in an *under*estimation of costs. But one does not know whether this bias invalidates the shape of empirical cost curves. A thorough study of accounting conventions is necessary before a judgement can be formed.

In the *short* period, there are some factors, typically capital inputs, which give rise to fixed costs for the individual firm. Their returns are *quasi*-rents and should not be considered as costs in the short period. Only costs which are variable in the short run should be included. It is difficult to find an empirical correlate to this cost concept. In principle, we must either estimate quasi-rents as well as rents proper, or we must identify and value short run variable costs directly. In practice, investigators have often used some concept of "direct cost" which excludes "overheads" [**73**]. But this is not usually satisfactory since it excludes many costs (e.g., labour costs in head office, which are variable) and it probably also includes some costs (e.g., a share of the cost of some equipment) which are not variable.[49] Another common procedure is to use total accounting costs. Accounting conventions vary and it is difficult to say whether it will underestimate or overestimate total short run variable costs. But one may well find that movements in accounting cost are closely correlated with changes in true cost. One would expect, for example, that changes in quasi-rents would be reflected in accounting profits and not in accounting cost. On these grounds, accounting data might be expected to give good estimates of marginal cost. The deficiencies in accounting practices, however, must be reviewed in some detail.

Conventions adopted by the accounting profession vary from one country to another; but, broadly speaking, the following deficiencies are characteristic of most accounting data:

(a) The unit period for accounting purposes usually differs from the unit economic period. Generally the financial year is longer than the short period of traditional theory.

[49] It must be observed that not all capital gives rise to fixed costs (e.g., vehicles and machines can be hired by the day or week) and, in some cases, payments to labour may be a fixed cost (e.g., when the employee has a long term contract).

(b) The distribution of the depreciation of an asset over its life cycle is usually determined by the taxation authorities rather than by economic criteria. Typical laws of taxation require the firm to practise "straight line" depreciation which, even if it gives the same length of life as the optimum, ignores "user costs."

(c) The valuation of capital services is at historical cost rather than current prices.

(d) The valuation of stocks is often based on some routine (e.g., in the United Kingdom "at cost or current value whichever is the lower").

These deficiencies are not disastrous for the estimation of cost functions; some of the inadequacies can be overcome by suitable processing [**161**]. Accounting data for the multiproduct firm pose many problems. It is tempting to use the cost accountants' allocation of overhead and joint costs. Because of the arbitrary methods used in accounting allocations, most cost analysts have resisted this temptation. However, much more might be done with cost accounting data.

Engineering data have not been frequently employed by economists to estimate cost functions. The results of engineering studies are not often formulated in such a way that they can be readily translated into the language of economics. The first systematic attacks by economists on this problem were made by Chenery and Ferguson [**54, 94**]. Chenery proposed to write the production and cost functions in terms of the observed (or designed) engineering variables. In his example of pipe line transportation, Chenery found cost as a function of the diameter of the pipe, the thickness, the compression ratio, and the horse power. Ferguson examined the "marginal fuel costs" of aircraft by converting the engineering production functions into cost functions.[50]

The main limitations of engineering data are similar to those of cost accounting figures. Indeed, the two sets of data are often very closely related. Cost accountants form estimates of the cost of a commodity by splitting up the production into separate operations and assess the cost of each operation by valuing the inputs of labour, etc. Engineering data, like cost accounting figures, relate to *processes*. One of the difficulties of translating these results into cost functions for some industries is that processes, and the cost of processes, may interact with one another and may not be additively separable. In practice, however, it may be a good enough approximation in a wide variety of industries. A second main difficulty is the arbitrariness in the allocation of joint costs. This appears to be the reason why there have been few synthetic or engineering cost studies of multiproduct firms. The most

[50] Recently Moore [**219**] and Alpert [**2**] have attempted to translate the engineers' so-called ".6 rule" into economic terms. Engineers' estimates of the "capacity" of plant and the associated capacity costs were related log-linearly.

important application of the synthetic cost approach by economists in recent years was the transportation study by Meyer, Peck, Stenason, and Zwick [**216**]. They allocated railroad joint costs between passenger and freight by a statistical cross section analysis. The expenditure on a particular joint productive service was expressed as a linear combination of passenger and freight traffic. The coefficients estimated from these equations were used to allocate joint expenditures. This study was an important advance in the field of engineering or synthetic cost studies.

In spite of all the objections which can be levelled against the synthetic cost approach, it does seem to be the only method available in many cases. It is probably the only practicable approach for a firm which produces a large number of outputs. Economic theory together with new computing techniques can be used to improve the estimates—and in particular to provide more suitable allocations of joint cost.

One of the most important distinctions in empirical cost functions is between cross section and time series studies. With time series data the variations of demand generate different levels of equilibrium output. Existing firms and the number of firms in the industry adjust their outputs to new levels of demand. For a particular firm, time series of output and total cost should, ideally, reveal a cost function for the firm. In cross section studies of competitive industry, on the other hand, there is no observable independent force, such as demand, which generates different levels of equilibrium output. The price of output is the same for all firms in the cross section. As Friedman has argued, if we define total costs as identical to total receipts, the cross section will give apparently constant average costs [**101**]. Friedman contended that in a competitive industry variations in output from one firm to another must be due to either "mistakes" or the existence of specialised resources controlled by the firm. The association of such variations in output with constant cost cannot be interpreted as evidence of the existence of constant long run average costs.[51]

This critique by Friedman is so powerful and damaging to empirical results that further examination is required. Suppose we redefined costs to exclude returns accruing to factors specific to the *firm* (e.g., entrepreneurial ability) and then calculated the regression of these costs on output; could the results be interpreted as some sort of average of the long run average cost curves of individual firms? The regression curve would in fact tell us how entrepreneurial ability varied with output. This is a valuable result but it has no

[51] This criticism can be put in the form of a naive question: why should an entrepreneur produce at average costs greater than the minimum? For a firm to be on the upward sloping part of the long run average cost curve, we have to postulate either monopoly power or a regulated industry, or continuous long run mistakes on the part of entrepreneurs.

close relationship to the cost curve of the individual firm. These criticisms, of course, do not apply to studies of firms in monopolistic conditions [**161**]. But the problem of valuation of costs and assets then becomes most important. And with product differentiation as the cause of varying outputs, the concept of a cost curve becomes most hazy.[52] Friedman has not killed off the cross section approach completely, but he has given it a severe purge.

The equation relating total cost to output used by most authors is usually a quadratic:

$$C(Y) = \gamma + \gamma_1 Y + \gamma_2 Y^2 + \varepsilon\,, \tag{45}$$

where Y is current output and ε is a disturbance term which is supposed to be distributed independently of Y. This gives rise to a marginal cost function which is linear with respect to output. Klein [**170**] has recently suggested that the ogive of the log-normal distribution may be used for a traditionally shaped total cost curve. Only Lomax and Nerlove have used the linear logarithmic cost curve. This is rather surprising in view of the fact that a logarithmic cost function is derived from the simultaneous equations of the Cobb-Douglas form. If we suppose there are no mistakes in the marginal productivity equations, the cost curve can be expressed as follows:

$$C(Y) = K\left(\frac{Y}{U_0}\right)^{\frac{1}{\alpha_1+\alpha_2}} = K\hat{Y}^{\frac{1}{\alpha_1+\alpha_2}}\,, \tag{46}$$

where K is a constant and Y is output. In a time series, the random variable U_0 would reflect the effect of the business cycle, weather, etc. In a cross section analysis U_0 would measure mainly entrepreneurial ability. Costs will then vary largely according to the ability of entrepreneurs. The quantity $Y/U_0 = \hat{Y}$, might be called planned or average output, according to whether the model is applied to time series or cross section data. Taking logarithms, we see that log U_0 is never distributed independently of log Y_0 so that a consistent estimate of $1/(\alpha_1 + \alpha_2)$ cannot be obtained by simple least squares. But in the case of public utilities, where we may treat output as an exogenous variable, this objection does not apply [**230**].

There have been very few attempts to combine short period and long period observations to get estimates of both the short and long run curves [**162**]. A simple model can be developed as follows:

$$\frac{C}{Y} = \gamma_0 + \gamma_1(Y - Y_m)^2 + \gamma_2 Z \tag{47}$$

with $Y_m = \delta Z$ where Y_m is the output at which average cost is a minimum on the short run average cost curve and Z is a measure of the size of plant.

[52] "Anything other than a perfectly even spatial distribution of demand involves having firms of various sizes . . . product differentiation . . . will make it feasable for firms of various sizes to exist profitably side by side": Johnston [**162**].

The second term in equation (47) refers to the short run average cost for a given plant, whereas the third term describes the variation in plant costs. One of the main difficulties with using an equation of this kind is to get a suitable measure of the scale of plant [**162**].

Now to the results. The two main conclusions of empirical cost functions have been generally thought to be, first, that the long run average cost is L-shaped and not U-shaped, and, second, that the short run marginal cost is constant. Critical comment has normally been directed towards discrediting these findings and reestablishing traditional hypotheses. The critics have alleged that biases arise in the following ways:

(a) from the accounting data,
(b) from the statistical treatment or processing of the data,
(c) from the regression fallacy.

It has been argued that these biases account for the failure of the measured long run average cost curve to rise at high outputs and for the linearity of empirical short run average cost curves.

Critics of cost studies were quick to point out the defects of depreciation figures. Accounting allocations of depreciation were alleged by Staehle [**295**] to introduce a spurious linearity. But Johnston rightly argues that a bias will be introduced only if user depreciation is in fact nonlinearly related to output. If, for example, user cost rises sharply for high output levels, the estimated cost curve will be flatter than the true cost curve.[53]

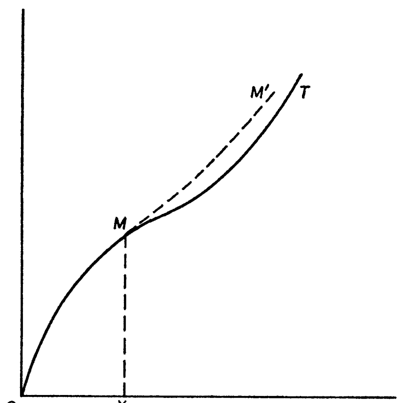
Accounting valuation of assets is usually based on historical cost. Consequently, the slope of the long run cost function calculated from a cross section will have a downward bias if large firms have old equipment and small firms have new machines and buildings. One way out of this difficulty is to revalue all equipment at current prices, but in practice revaluation is not an easy task if equipment has a long life. This bias is not so important for the short run cost function since only "user cost" of capital should be included in the measured function.

A third criticism is that short run cost theory is framed in terms of a unit economic period whereas accounting data are usually collected for longer periods. Smith [**278**] argued that variations in cost for economic periods would be averaged out in the accounting data so that one should not be surprised by the near-constancy of (accounting) marginal cost. For the unit economic period marginal costs would be rising much more steeply. This point has been accepted by most commentators, but Johnston [**162**] has ingeniously argued that the use of data for accounting periods may well *increase* the power of the usual t test in rejecting the hypothesis of linearity.

[53] Johnston [**162**] thought his data on capital costs for the electricity industry had "severe defects"; and in most other industries one would expect the situation to be even worse.

He remarks that "even in the case of random outputs, it is quite possible for the accounting data to have greater power of detecting curvature than do the unit data." The explanation of the pardox lies in the fact that Johnston compares data for N unit periods with data for N accounting periods. Since the latter are aggregates of the former, N accounting periods may contain as much, or more, information than N economic unit periods, depending on the distributions over time.

The main criticism of the statistical processing is that the correction for factor price changes in time series studies of the short run cost function gives rise to a bias towards linearity. Laspeyres' indexes of factor prices will generally *over*state the increase in factor prices since it takes no account of substitution. Deflation by such an index will overcompensate for the increase in price. If high prices of factors are associated with high outputs over the time series period, there will be a downward bias in cost for high ranges of output. One can get round this problem by constructing other kinds of index numbers. Johnston [**162**] has shown that with a wrongly weighted index, biases towards linearity or curvilinearity are both possible. The other method of eliminating factor price changes is to revalue costs at the levels of factor prices in a base period. Staehle [**295**] seems to suggest that this will bias the results towards linearity. But Johnston [**162**] has argued that since the bias is nonnegative, this method of allowing for factor price changes will distort the measured results towards *curvilinearity*, not linearity. This contention is correct only if the base period output is somewhere near the centre of the distribution of outputs. But if base period output is high and on the rising section of the short run average cost curve, and if all other observed outputs are below the level of the base period, the bias *may* be towards linearity. Similarly if the base period output is on the falling section of the average cost curve and all other outputs are above it, the bias may be linear. Again the precise result turns mainly on the choice of a base period.[54] In any case one



[54] A diagram will make this clear. Let T be the true total cost curve associated with a certain plant, and X be base period output. Observed outputs are all in excess of X. Then since the method overstates costs at outputs other than X, we get a measured cost curve of MM. And over this range of output, MM′ may be closer to a straight line than MT.

feels that any bias imparted to statistical cost functions by either of these deflating techniques is quite small compared with the distortions due to other factors.

From a consideration of the literature, the "regression fallacy" appears to be the most important criticism of the cross section method. It is argued that the output produced (and sold) by each firm is usually a random variable and the variation of output about the mean value is not controlled by the firm. The firm will find the best way of producing this distribution of outputs. If there are no variable costs, to quote Friedman, "a cross section study would show sharply declining average costs. When firms are classified by actual output, essentially this kind of bias arises. The firms with the largest output are unlikely to be producing at an unusually low level; on the average, they are clearly likely to be producing at an unusually high level, and conversely for those which have the lowest output" [**101**]. This criticism has been widely accepted. Attempts to avoid the regression fallacy have taken the form of classifying firms by plant and testing the significance of the within-plant and between-plant regressions [**31**]. On the other hand, it has been argued that, if output is a random variable, the relevant cost curve for decision making purposes is the *expected* cost curve and not the cost curve generated by the random variations of output [**327**]. Expected cost curves will be flatter than the original cost curves. Since the accounting period usually includes many unit economic periods, the data actually available will generally approximate to expected cost and expected output. If this point

TABLE VI

RESULTS OF STUDIES OF COST CURVES

GENERAL INDUSTRY STUDIES

Reference	Name	Industry	Type	Period	Result
[**16**]	Bain (1956)	Manufacturing	Q	L	Small economies of scale of multiplant firms.
[**88**]	Eiteman and Guthrie (1952)	Manufacturing	Q	S	MC below AC at all outputs below "capacity."
[**115**]	Hall and Hitch (1939)	Manufacturing	Q	S	Majority have MC decreasing.
[**178**]	Lester (1946)	Manufacturing	Q	S	Decreasing average variable cost to capacity.
[**219**]	Moore (1959)	Manufacturing	E	L	Economies of scale generally.
[**305**]	T.N.E.C. Mon. 13	Various industries	CS	L	Small or medium size *plants* usually have lowest costs. Blair (1942) draws different conclusions.

TABLE VII

RESULTS OF STUDIES OF COST CURVES

INDUSTRY STUDIES

Reference	Name	Industry	Type	Period	Result
[2]	Alpert (1959)	Metal	E	L	Economies of scale to 80,000 lbs/month; then constant returns.
[162]	Johnston (1960)	Multiple product	TS	S	"Direct" cost is linearly related to output. MC is constant.
[66]	Dean (1936)	Furniture	TS	S	MC constant. SRAC "failed to rise."
[69]	Dean (1941)	Leather belts	TS	S	Significantly increasing MC. Rejected by Dean.
[70]	Dean (1941)	Hosiery	TS	S	MC constant. SRAC "failed to rise."
[71]	Dean (1942)	Dept. store	TS	S	MC declining or constant.
[73]	Dean and James (1942)	Shoe stores	CS	L	LRAC is U-shaped, (interpreted as *not* due to diseconomies of scale).
[143]	Holton (1956)	Retailing (Puerto Rico)	E	L	LRAC is L-shaped. But Holton argues that inputs of management may be undervalued at high outputs.
[91, 92]	Ezekiel and Wylie (1941)	Steel	TS	S	MC declining but large standard errors.
[344]	Yntema (1940)	Steel	TS	S	MC constant.
[85]	Ehrke (1933)	Cement	TS	S	Ehrke interprets as constant MC. Apel (1948) argues that MC is increasing.
[234]	Nordin (1947)	Light plant	TS	S	MC is increasing.

is valid, it would go part of the way towards explaining why there is approximate linearity in measured cost curves and why we should take the results of these studies more seriously.

In Tables VI, VII and VIII the results of a broad, but not comprehensive, selection of cost studies are tabulated. It is unfortunately impossible to convey the limitations of each study and the precise interpretation which should be placed on these results. One can only hope that the short description of the results in the last column does not distort unduly the authors' conclusions. The type of data is indicated in the third column; questionnaire studies are denoted by Q, engineering data by an E, while TS and CS describe time series and cross section, respectively. Under the column "Period" we indicate whether the object of the study was for the short (S) or long (L) run cost curve.

TABLE VIII

RESULTS OF STUDIES OF COST CURVES

PUBLIC UTILITIES

Reference	Name	Industry	Type	Result
[184]	Lomax (1951)	Gas (U.K.)	CS	LRAC of production declines (no analysis of distribution)
[104]	Gribbin (1953)	Gas (U.K.)	CS	" "
[185]	Lomax (1952)	Electricity (U.K.)	CS	" "
[162]	Johnston (1960)	Electricity (U.K.)	CS	" "
[162]	Johnston (1960)	Electricity (U.K.)	TS	SRAC falls, then flattens tending towards constant MC up to capacity.
[190]	McNulty (1955)	Electricity (U.S.A.)	CS	Average costs of administration are constant.
[230]	Nerlove (1961)	Electricity (U.S.A.)	CS	LRAC excluding transmission costs declines, then shows signs of increasing.
[162]	Johnston (1960)	Coal (U.K.)	CS	Wide dispersion of costs per ton.
[162]	Johnston (1960)	Road passenger tpt. (U.K.)	CS	LRAC either falling or constant.
[162]	Johnston (1960)	Road passenger tpt. (U.K.)	TS	SRAC decreases.
[162]	Johnston (1960)	Life Assurance	CS	LRAC declines.
		RAILWAYS		
[29]	Borts (1952)	U.S.A.	CS	LRAC either constant or falling.
[31]	Borts (1960)	U.S.A.	CS	LRAC increasing in East, decreasing in South and West.
[40]	Broster (1938)	U.K.	TS	Operating cost per unit of output falls.
[197]	Mansfield and Wein (1958)	U.K.	E	MC is constant.

One of the remarkable aspects of the tables is the preponderance of public utility and railroad studies. This is probably because of the availability of data for nationalised or regulated business. These industries have either falling or constant long run average cost with the exception of the eastern railways in the U.S.A. [31]. The few studies of the short period cost curves of public utilities suggest that short run average cost decreases up to capacity and that, over the observed ranges of output, marginal cost is constant. These results do not discredit what traditional theory says about the shape of the cost function. Public utilities and natural monopolies have always been considered an exception to the normal hypotheses of U-shaped cost curves. And the shapes that appear in empirical work are similar to the diagrams described on academic blackboards.

The hard bone of contention is in the finding of constant marginal cost in industries in Table VII. But such a result is not unanimous—Apel's [**7**] interpretation of Ehrke [**85**], Nordin [**234**], and perhaps Dean [**69**] are exceptions. It is not difficult to see why Yntema and Ezekiel and Wylie [**344, 92**] got declining or constant marginal costs. Nearly all the observations were for output levels well below the "capacity" of the plants; the regression results are clearly valid only for these below-capacity outputs. There are then three of Dean's studies (furniture, hosiery, and department stores) and Johnston's study of food processing, which still need to be explained.[55] However, it is clear, even at this stage, that the evidence in favour of constant marginal cost is not overwhelming. Certainly the revision of theory to include this phenomenon is not an urgent matter.

The evidence for the existence of economics of scale, i.e., that long run average costs decline as output increases, is again only clearly established for public utilities. Apart from questionnaire and engineering studies, the only study of long run costs in manufacturing business is the Dean and Jones account of costs in shoe stores. They found a U-shaped cost curve but argued that it was due largely to the inferior factors the firm had to buy at the same prices, and not to diseconomies of scale. The questionnaire studies are methodologically suspect, and very little weight can be placed upon their conclusions.[55] Studies based on engineering data are also deficient since they measure only the relationship between expenditure on capital and the "capicity" of that equipment. We do not know what happens to other costs (e.g., administrative expenses) when capacity is increased; they may more than offset the increasing returns due to plant.[56] The empirical results clearly show that public utilities and railways have declining (or at least constant) long run

[55] Briefly, the opinion of a business man is not good evidence of his objective cost function since he is unlikely to think in the economists' language. Often he would tend to summarise the evidence obtained from his accountants. Of course, it is not necessary for the business man to know the objective cost function for him to obey the marginalist rules; see Friedman [**100**]. A particularly powerful objection to Lester's results is that he did not define the concept of "capacity."

Generally, I believe it is impossible to draw up a questionnaire which could be used to elicit information about the shape of the cost curve. The questionnaire technique is useful to discover answers to simple questions of fact or opinion. Questions that involve considerable interpretation and analysis will rarely produce useful answers.

[56] In Chapter 12 of Wiles [**330**] there is an extensive survey of other evidence on the shape of the cost function. Most of the "cost curves after total adaption" tend to fall into the L-shaped pattern. In only 32% of the cases was there evidence of a U-shaped curve. Some of the data reviewed by Wiles have been included in the summary table, e.g., Blair [**27**]. Almost all the results in Wiles are derived from cross section samples. Much of the data refer to plants rather than firms. Definitions of cost apparently vary

average cost, just as economists have long predicted. For these industries, established theory is not even faintly discredited. On the other hand, for "competitive" industries, the U-shaped hypothesis does not inspire great confidence. But this is *not* because it has been refuted by direct empirical evidence. On the contrary, this lack of faith is occasioned by the very few *opportunities* for collecting evidence to refute the theory directly. Instead we are driven to indirect and circumstantial evidence. But at least there is no large body of data which convincingly contradicts the hypothesis of a U-shaped long run cost curve and the fruitful results which depend on it.

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considerably, but the majority of studies seem to use some version of accounting cost.

I do not believe that too much weight should be placed on Wiles' results. There are serious doubts about the quality of the data which Wiles admits. But as evidence, even if we had no such reservations, the statistical L-shaped or constant cost conditions would not be inconsistent with a U-shaped long run cost curve. Using the "naive" argument, why should any competitive entrepreneur expand and stay on the rising part of the cost curve? This *may* be an ideal output for a monopoly or for a regulated industry, but then we must be very careful in interpreting the concept "cost."

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