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CONTENTS

On Measuring Technical Efficiency
C. Peter Timmer

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C. PETER TIMMER

ON MEASURING TECHNICAL EFFICIENCY*

CHAPTER 1. INTRODUCTION

Types of Efficiency

Economics is devoted to understanding the production and consumption of man's material needs and desires. In order that the understanding be more than purely descriptive, economists have postulated that all economic decision-makers want more of whatever it is they seek. Firm managers want more net revenue. Consumers want more satisfaction. From this encompassing description of human motivation flow a remarkable number of models purporting to explain the production and consumption processes. All of these models force the economic agent to maximize some function subject to constraints. The example of immediate relevance is the firm manager maximizing net revenues subject to given factor and product prices and his technical production function.

There are at least two very important ways in which this maximizing process might fail in the real world. The whole core of economic theory is concerned with one of these: the marginal revenue products of some or all factors might be unequal to their marginal costs. If this is true the *allocative* decision is said to be inefficient.

The second important source of failure in the maximizing process has received far less theoretical treatment in the economic literature but is potentially more important quantitatively (in terms of wasted resources). This is the extent to which firms actually produce on the technical production function that yields the greatest output for any given set of inputs. A failure in this regard means the firm is *technically* inefficient.

In a sense, technical efficiency is not an economic problem at all, for economics has traditionally assumed that the internal maximizing process in the firm is always completed (in a static world). Thus all firms achieve the same amount of output when they use identical amounts of (traditional and measurable) inputs. That this is patently not so in the real world has only recently intruded into eco-

* This is a revised version of my Ph.D. dissertation, "On Measuring Technical Efficiency," Harvard University, November 1969. A number of people provided stimulation, criticism, direction, and restraint. I would particularly like to thank Walter P. Falcon, Carl H. Gotsch, and Christopher Sims at Harvard, and Ben Massell, W. O. Jones, Scott Pearson, Don Keesing, and Bill Comanor at Stanford. My research for this study started a fruitful and continuing dialogue with Pan Yotopoulos on the meaning and measurement of economic efficiency. His comments have been extremely helpful. I would also like to thank the Food Research Institute for its generous staff and financial support.

nomics. The advent of linear programming opened for the first time the door to the firm's management office. Internal allocative decisions became subject to as much scrutiny by economists as the assumed external results of those decisions.[1] The result has not just been to show that many firms are not technically efficient when judged by best practices of the industry. "Everybody" knew that. A further result has been to give some rationale for the inefficiency. Nontraditional and frequently nonmeasurable costs are paid for "correct" decision-making. Competitive conditions may dictate that the costs be paid. Alternatively, they may allow sufficient leeway for substantial productive inefficiencies to exist with no efforts made to eliminate them.[2]

Another major intellectual development in economics also contributed to the interest in technical or productive efficiency. In the 1950s steady growth rather than the stationary state became the desired equilibrium in economists' models. It had long held this position in popular sentiment, but its formal acceptance into basic economic theory started a round of searching questions. How does growth start? What determines its speed? How much is enough? Do the economists' traditional factors of production account for it? These questions are almost entirely empirical. The process of answering them sent economists into the firm to study the diffusion of new knowledge and technology, to industry and national aggregate data to estimate production functions and shifts in production functions, and back to the firm to see what embodied and disembodied change really meant.

The research is beginning to indicate that technical efficiency is important. The reasons a firm uses "best" rather than "average" practices are closely related to the rate of acceptance of technical change and growth in output. The importance of technical efficiency has caused few ripples in the theoretical literature. And without solid theoretical support, the measurement of technical efficiency has been somewhat ad hoc. With this in mind, this monograph has the following goals: (1) to understand the concept of technical efficiency within the context of received economic theory; (2) to devise a theoretical measure of technical efficiency consistent with this context; and (3) to use the theoretical measure in an empirical test of the quantitative significance of technical efficiency. The test will be for U.S. agriculture from 1960 to 1967, with each state's average performance in each year assumed to be that of a representative farm firm.

A road map for what follows would show only a few departures from a straight path between "building the model" and "explaining the real world coefficients." The remainder of this chapter briefly discusses some of the welfare significance of the allocative and technical production decisions. Some reasons why these decisions might not always be "correct" are also listed. The role of management in

[1] "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 recognized 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" (*48*, p. 2).

[2] One model that allows costs to rise above minimum levels is described in *9*. The welfare effects of a positive relationship between competitive pressures and degree of technical efficiency are discussed in *8*, pp. 304–309.

particular is discussed, although the aggregate nature of the agricultural data used in the empirical part of this monograph precludes any judgments about the function and efficiency of management in U.S. agriculture.

Chapter 2 presents a number of techniques for measuring the efficient production function. The technique ultimately chosen for estimation—the single-signed "least lines" fit to a Cobb-Douglas production function—is justified on the basis of its conformity to most of economic theory, its ease of estimation with linear programming methods, and the facility with which comparison can be made to more conventionally estimated production functions. Chapter 3 briefly sketches the usual least squares approach to estimating Cobb-Douglas production functions and concludes that the Hoch-Mundlak technique of pooling time series and cross-section data in order to use analysis of covariance provides the most meaningful "average" function with which to compare the "frontier" function of Chapter 2.

A theoretical comparison of "average" and "frontier" functions is carried out in Chapter 4, and the two are used to examine in some detail the nature of technological change and the diffusion of innovations. The theoretical part concludes with a suggested approach to evaluating the impact on output of the physical environment.

Chapters 5–7 provide the empirical meat, with the discussion following the natural progression from data description to estimation of "average" and "frontier" production functions to an explanation of differences in technical efficiency. The quality of the data is such that much of the discussion in these three chapters is devoted to explaining awkward or inconsequential results.

The one major digression occurs in Chapter 8, where biased and unbiased versions of the "average" production functions are used to examine the extent of allocative inefficiency (marginal revenue product unequal to marginal cost) and economies of scale in U.S. agriculture.

Chapter 9 supplies the usual conclusions.

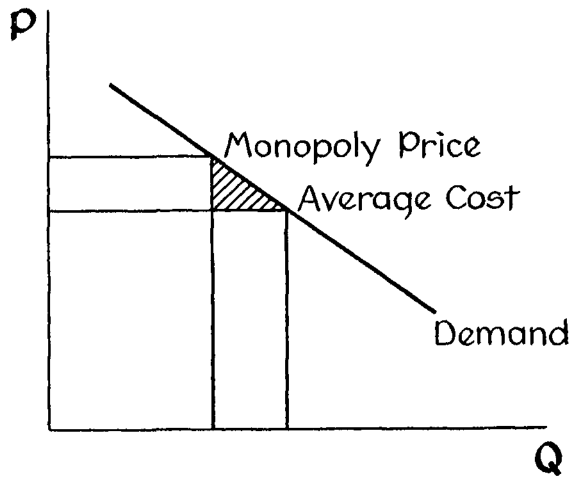
Welfare Effects of Efficiency

Allocative efficiency is the central issue in microeconomic theory. The model of perfect competition developed in the two centuries since Adam Smith determines the allocation of society's scarce resources to meet insatiable desires in such a way that no one can be made better off without someone else becoming worse off. In short, society will reach a Pareto optimum if all the assumptions of the competitive model are fulfilled.

But what if all the assumptions are not fulfilled? In particular, what if firms have a substantial degree of market power and exercise considerable influence over the price received for the goods they produce? Harberger and Schwartzman have provided the empirical answer: not much is lost—in fact, less than 1 per cent of GNP for the United States economy (*19*, pp. 79–92; *41*, pp. 727–29).

The losses are so small because the resources that are not used because of monopolistic output restriction are used in other (competitive) industries to increase output. Only the marginal distortion, measured by the shaded triangle in Harberger's familiar diagram (Chart 1–1), is welfare loss. But another produc-

CHART 1-1



tion decision underlies this, with far greater welfare implications. The model assumes that each firm "*purchases and utilizes* all of its inputs 'efficiently.'"[3] A failure of this assumption, for whatever reason, causes total and not marginal welfare losses. Comanor and Leibenstein assume that monopoly power may cause this assumption to fail (*8*, p. 305).

> Now let us suppose that a shift from monopoly to competition not only lowers price but also lowers costs (i.e., increases X-efficiency). What are the welfare losses under these circumstances which are due to monopoly? . . . The important implication of our result is that the actual degree of *allocative inefficiency* may be very much larger than the level as heretofore calculated. Furthermore, to this larger sum must be added the volume of X-efficiency for the monopolistically used inputs to obtain the total welfare loss from monopoly.

Thus if firm A buys 10 per cent more of society's scarce resources than firm B but produces exactly the same output, those extra resources are lost to society. If they had been employed in an efficient firm they would have produced a tenth more output than was actually achieved. Some of Leibenstein's admittedly diffuse and scanty data indicate that some firms do as much as 100 per cent better than others, or that gains to simple changes in organization and incentives can increase output by 50 per cent without any change in capital and labor (man-hours) inputs.

Responsibility for Efficiency

The reasons for such potentially large degrees of inefficiency are still unclear unless the physical environment differs markedly and its impact is not removed in measuring efficiency. If the environment is the same or differences are allowed for, then the responsibility for inefficiency, in corporate fact as well as economic

[3] Leibenstein's meaning of "X-efficiency" is approximately the same as what is called technical efficiency here (*28*, p. 392).

theory, rests with management. "Managers determine not only their own productivity but the productivity of all cooperating units in the organization. It is therefore possible that the actual loss . . . might be large" (*28*, p. 397). Ultimately then, the goal of this monograph should be to relate technical efficiency to management. The aggregate nature of the data washes out most management effects in the sense of individual differences in decision-making ability. So the differences in technical efficiency measured here cannot be systematically related to statewide managerial differences. Even with a suitable data set a number of difficult questions would first have to be answered: how does management appear in a production function, what form of interaction is there between management and other factors of production, can a management variable be constructed to overcome "management bias" in estimating production functions? Some discussion of these questions is in order even though few answers emerge.

Assume that five primary factors of production appear in an agricultural production function: land, labor, capital, management, and intermediate inputs (which might be split further into noncomplementary inputs). Any industry that is competitive and not subject to rapid and continuing technological change could subtract intermediate inputs from both sides of the output $= \phi$ (inputs) equation with all arguments remaining the same. This is a poor procedure for agriculture because of the continuing disequilibrium with respect to "modern" inputs —fertilizer, herbicides, pesticides, etc. Since the marginal revenue productivity of these inputs remains well above cost, the results of any estimated production function would be biased if these inputs were subtracted using farmers' costs as a measure of productivity.

All five factors of production can be treated symmetrically when formulating a production function. This supposes that all five factors are "produced" means of production and no single factor or group of factors has a claim to being more "primary" than the others. The general production function $Y = \phi\,(D, L, K, M, I)$ is then subject to a set of five factor supply functions (as well as the usual profit-maximizing constraints). The form of these functions depends on a great many things, but particularly on the level of aggregation being considered. At the firm level in a competitive environment the factor supply functions do not constrain physical quantities of the factors available to each firm but serve to identify quality and the means of aggregating disparate components into a single index.

Assume the production function is of the form

$$Y = A \prod_{i=1}^{n} X_i^{a_i}. \tag{1.1}$$

Suppose that each X_i is in fact a weighted linear sum of the individual components, i.e.,

$$X_i = \sum_{j=1}^{m} \omega_{ij} Z_{ij}, \tag{1.2}$$

where Z_{ij} is the j^{th} type of component comprising the i^{th} factor of production, and ω_{ij} is the weight used in aggregation.

Then the production function is of the form

$$(1.3) \qquad Y = A \prod_{i=1}^{n} \left(\sum_{j=1}^{m} \omega_{ij} Z_{ij} \right)^{\alpha_i}.$$

Once expressed in this fashion, the function leads inevitably to a more general (and more meaningful) form.

$$(1.4) \qquad Y = A \prod_{i,j=1}^{n,m} Z_{ij}^{\alpha_{ij}},$$

or, suppressing the i and j subscripts and allowing a k subscript to carry over both i and j,

$$(1.5) \qquad Y = A \prod_{k=1}^{nm} Z_k^{\alpha_k}.$$

This form allows *each* component of each factor of production to have its own elasticity of production (α_k). Carried to its logical end, Equation (1.5) assumes that each worker is a separate factor of production. Stated this way, the difficulty of working empirically with Equation (1.5) is obvious. There will always be more factors of production than firms in the sample, so Equation (1.5) can never be estimated. To be empirically relevant the production function must always take, to a greater or lesser degree, the undesirable form of Equation (1.3).

Not much explicit attention has been given to estimating the different forms of Equation (1.2). Frequently it is assumed that the weighting system (set of ω_{ij}) is quite simple—all "bodies" are equal, or a dollar spent on one machine is equal to that spent on a different machine, or on a building or drainage system. Alternatively the weighting system may be very complex and involve a substantial amount of estimation itself. The work of Griliches, Denison, Tostlebee, Kendrick, and Goldsmith on the proper specification and measurement of different factors of production falls somewhere between these two extremes.

But so far no effort has been made to treat management in this fashion, and for obvious reasons—". . . there is no generally accepted cardinal measure of entrepreneurship" (*48*, p. 5). There is not even an a priori physical quantity to build around, as there is for man-hours, acres, pounds of fertilizer, or tractor-hours. The management function must thus be built on rather an ad hoc basis. Since it is a nonobservable, nonmeasurable input, management is judged by the results of its decisions, i.e., by the degree of efficiency achieved in production. If this can be estimated consistently—and the next two chapters will attempt to present means by which it can be—then a firm-specific index of efficiency (managerial performance) will have been generated. The variable X_i will be known, and all that remains will be to find any Z_{ij} that seem relevant to managerial performance and to estimate the ω_{ij}.[4] For agriculture in particular the Z_{ij} might include education and age of the farm operator, exposure to research and extension results, etc.

[4] There is no a priori reason why the ω_{ij} weights must be linear, except for ease of estimation. Equation (1.2) could just as easily be specified as

$$(1.2a) \qquad X_i = \phi_j(Z_{ij}).$$

Hall and Winsten have argued strongly that extreme care must be taken in this type of endeavor (*18*, pp. 71–86). Especially when judgments about the relative performance of managers are being made they insist that allowance must be made for the nature of the physical environment facing each manager. If different managers face different constraints on their maximizing behavior, even though all face the same general production function, judgments about their relative performance will be useless unless these constraints are understood. Only after correction for environmental differences is made does the responsibility for technical efficiency belong to management.

The nonmanagerial determinants of technical efficiency are many. A fairly lengthy list follows, and although an attempt has been made to be inclusive there are certainly factors that have been left out. The factors, as will become obvious, are mostly related to agriculture.

Physical factors.—Soil characteristics and climate are the two factors of most importance in this category. They are to a large extent jointly determined because the type of soil resulting from breakdown of primal mineral matter is a function of long-run precipitation, wind, temperature, and sun, i.e., of climate. These factors are fixed in the very short run for both the firm and society and are fixed in the long run for the firm. Short-run impacts of precipitation, wind, etc., or weather, are also important in determining technical efficiency. The weather factor is variable in the short run but is assumed to have a zero average impact in the long run. The variation is assumed to be random.

The impact of these physical factors on productivity in agriculture is likely to be substantial. In an attempt to correct for this impact, and thus to free the ultimate index of technical efficiency from differences in physical environments, land value rather than land area is taken as a factor of production.

Social and political factors.—Perhaps the most important social factor affecting productivity, especially in agriculture, is population density. Particularly when output per acre is taken as an indication of productivity or efficiency in production the type of nearby market is crucial. The nearer a farm is to a large metropolitan area the greater is the potential for small acreage–high value truck cropping. The same land a thousand miles from nowhere might only be profitable for low intensity–dry land grain cropping. So demographic factors play an important role in determining cropping patterns and intensity of land (and other factor) use.

Political intervention in agriculture has a history many centuries long. The range of programs that affect farmers' productivity is wide. Government-sponsored agricultural research and extension directly affect the farmer's productivity —the superabundance of farm goods in the United States is attributable in part to the success of research and extension services. Differential availability of these services to farmers is very likely to cause corresponding differentials in productivity.[5]

Other government programs also affect productivity in agriculture. Soil bank schemes remove marginal land from production and encourage more intensive

[5] Griliches has already presented some preliminary but convincing evidence on this score (*15*, pp. 961–74).

cultivation on the remaining, higher quality land. Crop subsidies, especially when highly favorable to a crop with limited geographic distribution (e.g., cotton and tobacco), can affect "normal" cropping patterns and also upset equilibrium pricing. Rational values of total output are difficult to construct when some crops are heavily subsidized and some are not. A farmer who produces a high value of output of tobacco may not be nearly as efficient in a broad social sense as a farmer who produces a smaller value of soybeans or chickens. The government artificially supports the price of the former and not of the latter. Equilibrium prices in a social welfare model should be used instead of actual prices received by farmers. In the real world, of course, such distortions are almost impossible to remove (using world free market prices might help, where these are available).

Random factors.—Weather is probably the most important random factor affecting agriculture, and its role has already been treated. Some lesser factors are loss due to fire and theft,[6] accidents in the production process (overdoses of anhydrous ammonia or some weed killers are not uncommon), unexpected delays in delivery and repair of equipment (a combine in the repair shop instead of the field can easily cost half a wheat crop), and random variation in seed and livestock quality. Livestock in particular are subject to extraneous factors, the most important of which is probably disease. But the "sheep's in the meadow, the cow's in the corn" situation can also be damaging. A large herd of mad steers can wreak unbelievable destruction on a mature field crop.

Random factors are impossible to fit into a determinate model of production, and their existence is a nuisance if such a model is desired. But they are essential if some stochastic model is being built. Since estimation of a stochastic model is the desired goal here random factors affecting production create no problems.

[6] Cattle rustling was once important, and episodes are still reported occasionally. But the thought of someone stealing 5,000 bushels of soybeans is ludicrous.

The received body of microeconomic production theory holds that a firm's production function specifies the maximum output attainable from a set of inputs, given the technology available to the firm (*20*, p. 44).[7]

> The production function differs from the technology in that it presupposes technical efficiency and states the *maximum* output obtainable from every possible input combination. The best utilization of any particular input combination is a technical, not an economic, problem. The selection of the best input combination for the production of a particular output level depends upon input and output prices and is the subject of economic analysis.

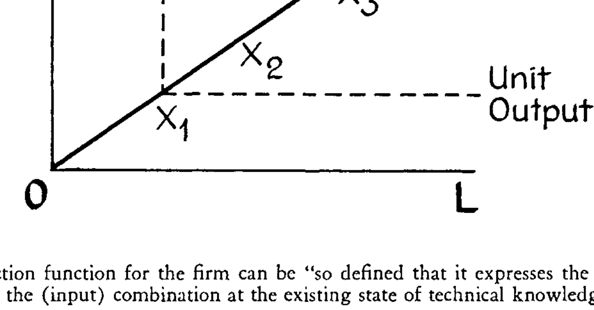
It was argued in Chapter 1 that there are actual differences between firms in technical efficiency, that is, in the manager's ability to achieve the technical maximum. Economic theory dictates that these differences should be measured relative to the technical frontier rather than relative to some "average" firm. This is most clearly seen in a two-factor Leontief-type world.

In Chart 2–1 all four firms are producing only one unit of output. They use differing amounts of the factors of production, although in fixed proportions. It is natural in this case to say that firm X_1 is 100 per cent efficient (relative to the observed performance of these four firms—some engineer with better knowledge of *potential* techniques in this industry might say that none of these firms is efficient), or has an efficiency index of $\frac{OX_1}{OX_1} = 1.0$. The performance of firms X_2, X_3, and X_4 can be measured relative to that of X_1; thus

$$1 >> \frac{OX_1}{OX_4} < \frac{OX_1}{OX_3} < \frac{OX_1}{OX_2} < \frac{OX_1}{OX_1} = 1.$$

These ratios have a straightforward economic interpretation. If $\frac{OX_1}{OX_2} = 0.6$ then

CHART 2–1



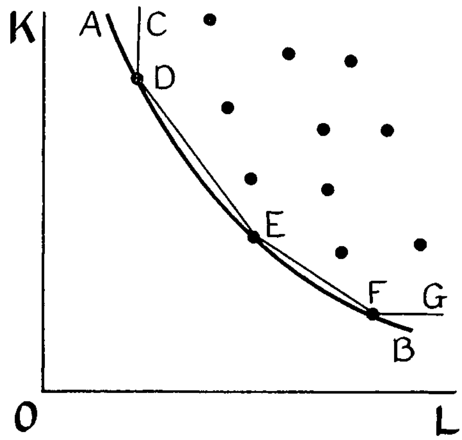
[7] A production function for the firm can be "so defined that it expresses the *maximum product* obtainable from the (input) combination at the existing state of technical knowledge" (7, pp. 14–15).

the inputs of capital and labor could be reduced in the ratio of 1.0 to 0.6 in firm X_2 with output remaining the same, provided that firm X_2 used precisely the same "technique" (in the broadest possible sense) as X_1. This, intuitively, is what efficiency means.

In 1957 M. J. Farrell generalized this Leontief single-process example to many processes and *n* inputs while retaining the linearity assumptions (*11*, pp. 253–81). The technique yields estimates of technical efficiency and price efficiency. Summaries of the method have appeared in Nerlove and Bressler (*37*, pp. 86–100; *5*, pp. 129–36).

Assume that all firms in an industry use two factors of production, labor and capital, have equal access to the most efficient technology, but that some firms are more technically proficient at using this technology than others. All firms utilize a linearly homogeneous production function (LHPF), thus producing with constant returns to scale. The physical environment for all firms is the same. It is then possible, in this two-factor case, to represent the results of each firm's production decision on a unit isoquant diagram similar to that of Chart 2–2.

CHART 2–2

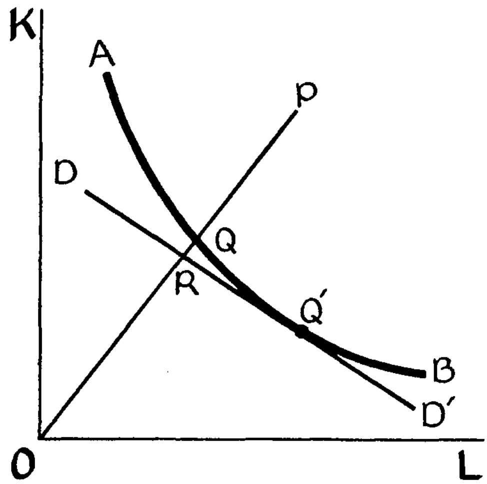


Each observation represents the input combination used by a single firm to generate one unit of output.[8] In a smooth, neoclassical world the efficient frontier would be represented by *AB*. In a discrete, linear combination world the frontier is *CDEFG*. In either case, no firm is able to produce a unit of output with a combination of inputs to the southwest of the frontier. This space is infeasible given present technology.

Farrell's method measures each firm's technical efficiency relative to this achieved efficiency frontier. Thus in Chart 2–3 firms *Q* and *Q′* are both 100 per cent technically efficient, for both produce on the unit isoquant *AB*. Firm *P* is inefficient. By Farrell's measure, its degree of technical inefficiency is the ratio *OQ*

[8] It is not necessary to assume LHPF at this stage. But to relate one observation to another we must assume that the slope of each firm's production function is parallel to any other firm's slope along a ray through the origin. Thus the LHPF assumption becomes necessary before comparisons can be made.

CHART 2–3



to OP, or $\frac{OQ}{OP} \times 100$ on a percentage basis. All firms have a technical efficiency ranking of 100 or less on this basis, with the lower limit being zero if the firm uses inputs but produces no output.[9]

Chart 2–3 also illustrates how price efficiency can be measured, if factor prices facing each firm are known. Assume DD' is the relative price line facing all firms in the industry. Then only Q' is both techcnically efficient *and* price efficient, for DD' is tangent to AB at Q'. Firm Q is technically efficient, but not price efficient, and the degree of its price inefficiency is measured simply by the ratio of OR to OQ, or $\frac{OR}{OQ} \times 100$ on a percentage basis. The justification of this measure is straightforward—DD' is a budget or cost line. Hence it is possible to produce the unit amount at a cost of only OR. Since firm Q spent OQ, the ratio of its inefficiency is OR to OQ. It is clear then that both measures of efficiency are in substance just cost indices, although technical efficiency is measured relative to the production frontier rather than relative to a minimum cost budget line. The use of a production frontier separates the allocative from the technical decision, something that simple cost comparisons cannot do.

The Farrell technique is generalizable to n inputs using modern linear programming techniques. With more than two, or possibly three inputs, however, the increased generality is achieved at the cost of visualizing the production function without constraining it to any algebraic form such as Cobb-Douglas or CES.

[9] This is obviously for gross output only. If net output is the measure considered, then the problem of negative numbers arises, for the firm could use a greater quantity of intermediate inputs (in value) than it produced as gross output. A negative output with positive inputs of the primary factors capital and labor would result. Soligo and Stern found that 23 of 48 industries studied in Pakistan had a negative value added, due either to inefficiencies in the industries in the use of imported inputs or to domestic inputs priced higher than similar inputs abroad (*42*, pp. 250–70).

Then the constraint of a functional form must be balanced against the ease of visualizing the production surface.

A number of theoretical and operational difficulties have been raised with Farrell's frontier approach to measuring efficiency. The assumption of constant returns to scale has been criticized by Nerlove and others (*37*, p. 90; *25*, pp. 282–90; *2*, pp. 826–39). The problem can be surmounted by segregating the data by size, estimating separate frontiers and testing for significant differences.[10] An alternative approach is to consider differences in scale a factor of secondary importance and thus *contributing* to differences in efficiency. Bressler discusses this technique with some enthusiasm, but notes a major theoretical problem. If there is a systematic relationship between scale and factor proportions, then the validity of the original frontier is in doubt (*5*, p. 133). In other words, the question of scale may not be separable from estimating the frontier.

Although the frontier function corresponds closely to the theoretical ideal production function, data problems are severe. In particular, the frontier is determined by the extreme observations in the data set, and thus the position of the frontier is strongly sensitive to errors of observation. While this could bias the frontier in a wildly optimistic fashion, especially if raw firm data were used, there is an offsetting pessimistic bias. The frontier depends only on actual observations contained in the sample. A larger sample cannot contract the frontier, but it can enlarge it. This bias is analogous to that of a sample maximum as an estimator of a population maximum (*2*, p. 827). The two biases will tend to be offsetting, but the extent to which either is dominant is unknown and presumably varies from situation to situation.

A final objection, mentioned by Bressler, is common to all envelope approaches (*5*, p. 136). Only marginal data are used. The vast bulk of the observations do not enter the estimation procedure at all. Of course, the presence of data does not make it relevant. If an efficient envelope is the objective, then the problem is falsely raised, for only the marginal observations are relevant.

It is possible to take account of all observations in fitting a smooth envelope according to some functional form, e.g., Cobb-Douglas. By constraining errors to one sign and fitting either least lines or least squares with linear or quadratic programming techniques, a fitted envelope function is obtained using all observations in the estimation. This procedure is used by Aigner and Chu for a Cobb-Douglas function in output-input space, as contrasted with the efficient isoquant in input-input space introduced by Farrell (*2*, pp. 826–39).

Since efficient frontiers can be estimated in a number of ways, it is necessary to examine in more detail the techniques involved before a decision about which to use is made. The following will be pursued here:

(1) the programming approach in input-input space of Farrell;
(2) the programming approach in output-input space of Aigner and Chu; and
(3) a "chance constrained" frontier or density function approach.

The first method utilizes only extreme observations from the data set; the second two utilize all or a certain proportion of the data.

[10] Although there are no statistical tests presently available for this purpose.

A programming model for calculating a Farrell frontier has best been described by Boles (*4*, pp. 137–42). Consider each of n firms as a separate activity producing a unit of output through the input of m factors of production. The j^{th} activity is completely described by a vector of $m + 1$ elements, and f_{ij} represents the quantity of factor i used in the unit activity j. Call this activity P_j.[11] The objective is to determine the location of each firm j relative to the origin and an envelope of all n firms.

The essential question to be asked about each activity, then, is the following: Given the n activities and the j^{th} list of inputs, what is the maximum amount of output that can be produced? By definition, the j^{th} activity produces one unit of output. If some combination of activities can produce more than one unit while using no more resources than the j^{th} activity, then the j^{th} activity is inefficient, and the efficiency index is defined as the reciprocal of maximum output. Formally, then, there are n distinct linear programming problems in which the n productive activities form a constant coefficient matrix, A, and each of the activities in turn furnishes the coefficients of the "right-hand side," *rhs*. Let V be an $n \times 1$ vector of ones. The j^{th} linear programming problem is:

$$\text{Maximize } X_0 = V'X$$
$$X \geqq 0$$
$$AX \leqq P_j.$$

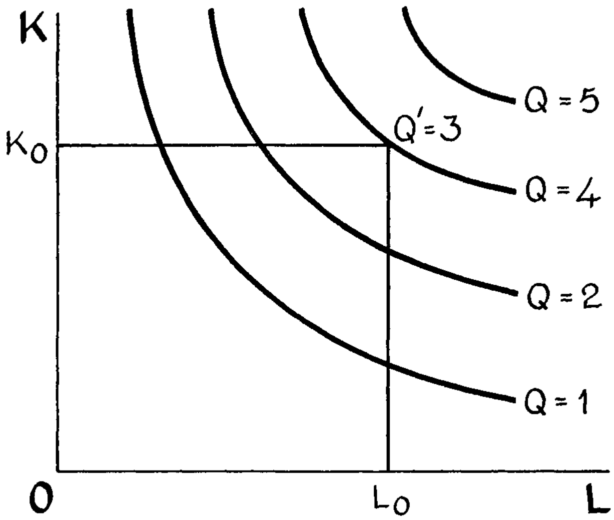
Let $\overline{X}_0$ be the optimum value of the objective function; then the efficiency index is $\frac{1.0}{\overline{X}_0}$. The set of convex combinations of the optimum basis defines one facet of the technically efficient unit isoquant (*4*, pp. 137–38).

The envelope approach used by Aigner and Chu does not operate in isoquant space like the Farrell frontier, but in total output-input space. The advantage of this is that the assumption of constant returns to scale need not be made, and so the output hyperplane is not constrained to lie on straight lines emanating from the origin. This generality is achieved at the cost of specifying a functional form for the hyperplane, in this case Cobb-Douglas.[12] The cost may be small if the form assumed gives a good fit and behaves according to economic logic, as the Cobb-

[11] $P_j = \begin{bmatrix} f_{1j} \\ f_{2j} \\ \cdot \\ \cdot \\ \cdot \\ f_{mj} \end{bmatrix}$

[12] This cost may be needlessly incurred. If linear combinations of points determining the efficient production surface are acceptable instead of a smooth surface, then the efficient facets could be listed in the same fashion as the isoquant model. The efficiency index for each of the n activities would be calculated in a similar manner: the maximum potential output as determined by the output of the efficient envelope at a point corresponding to the jth availability of inputs. A diagram may make this somewhat clearer. Chart 2-4 shows a production function in isoquant space once again, but this time with nonconstant returns to scale, necessitating that all isoquants be drawn rather than only the unit isoquant. These isoquants are efficient—that is, they are derived from the envelope production function in output-input space that Aigner and Chu estimate. Firm Q' uses K_0 capital and L_0 labor, which would be sufficient to produce an output of 4 if Q' were efficient. In fact Q' produces only 3. Thus its efficiency index is ¾ = 0.75. When no functional form is estimated, such as Cobb-Douglas, then this method assumes constant returns to scale *on* a facet of the envelope, but not between facets.

CHART 2–4



Douglas usually does. A benefit of fitting a Cobb-Douglas function to the efficient envelope is that it permits direct comparisons with Cobb-Douglas functions estimated by "average" statistical techniques.

Consider the usual Cobb-Douglas model in general form,

$$y_j = \prod_{i=0}^{m} x_{ij}^{\alpha_i} e_j, \text{ where} \qquad (2.1)$$

y_j = output of firm j
x_{ij} = use of factor i by firm j
α_i = parameter
e_j = a random error term that contains a systematic efficiency term as well.

In logs (capital letters), this can be written as

$$Y_j = \sum_{i=0}^{m} \alpha_i X_{ij} + E_j, \qquad (2.2)$$

where one column of X_{ij} is a vector of ones to allow for an intercept.

To make this a frontier function all E_j must be constrained to one side of the estimated production surface. Thus, (2.2) should be estimated such that

$$\sum_{i=0}^{m} \hat{\alpha}_i X_{ij} = \hat{Y}_j \geqq Y_j. \qquad (2.3)$$

Only "efficient" firms satisfy the final equality—all others have a smaller *actual* output than would be achieved if they too were efficient by the standards of the estimated production function.

An infinite number of sets of $\hat{\alpha}_i$ will satisfy (2.3). To force the estimated production surface to lie as closely as possible to the observed set of points a minimizing constraint must be placed on some function of the sum of the resulting error terms. Some flexibility is possible with this constraint. In the context of a system of simultaneous equations it is convenient to

$$\text{Min} \sum_{j=1}^{n} E_j^2.$$

This form is also most convenient for comparing "frontier" estimates of the coefficients of a Cobb-Douglas production function with "average" or ordinary least squares estimates. In the present context, however, a simultaneous system is not necessary to justify use of a Cobb-Douglas function (see Chapter 3), and comparison with "average" estimates, while interesting and useful, is not the main purpose of this monograph. A constraint that diminishes the impact of extreme observations rather than accentuates them is most desirable for fitting a frontier with data subject to observation errors, so the form used here is to minimize the linear sum of the errors,[18] i.e.,

Assuming all $E_j \geqq 0$, Equation (2.3) can be written as an equality:

$$\sum_{i=0}^{m} \hat{\alpha}_i X_{ij} - E_j = Y_j. \tag{2.4}$$

The problem then is to

$$\text{Minimize} \sum_{j=1}^{n} E_j \text{ subject to}$$

$$\sum_{i=0}^{m} \hat{\alpha}_i X_{ij} \geqq Y_j \text{ and } \hat{\alpha}_i \geqq 0.$$

This looks like a linear programming problem with the possible exception of the objective function, which must be translated into a simple linear function of $\hat{\alpha}_i$ and X_{ij}. This can be done. The technique is to sum Equation (2.4) over j. Then

$$\sum_{j=1}^{n} \sum_{i=0}^{m} \hat{\alpha}_i X_{ij} - \sum_{j=1}^{n} E_j = \sum_{j=1}^{n} Y_j. \tag{2.5}$$

Solve for $\sum_{j=1}^{n} E_j$, which is to be minimized:

[18] This is the single-sign analogue to a "least lines" linear regression where the sum of the absolute values of the deviations from the fitted line is minimized. Minimizing

$$\sum_{j=1}^{n} E_j^2$$

is the single-sign analogue to a standard least squares regression.

$$\text{Min} \sum_{j=1}^{n} E_j.$$

$$\sum_{j=1}^{n} E_j = \sum_{j=1}^{n} \sum_{i=0}^{m} \hat{\alpha}_i X_{ij} - \sum_{j=1}^{n} Y_j. \tag{2.6}$$

Consider that for any particular data set $-\sum_{j=1}^{n} Y_j$ is a constant. Any set of $\hat{\alpha}_i$ that minimizes $\sum_{j=1}^{n} E_j$ for one value of $-\Sigma Y_j$ will minimize for any other value, including zero, so the term can be dropped from Equation (2.6) with no consequence. The remainder is entirely suitable as a linear programming objective function. But it is somewhat simpler computationally to divide by n, the number of observations, so that the objective function becomes

$$\text{Minimize} \sum_{i=0}^{m} \hat{\alpha}_i \overline{X}_i, \text{ where} \tag{2.7}$$

$$\overline{X}_i = \text{mean of } X_{ij} \text{ and}$$
$$\overline{X}_0 = \text{one.}$$

Written more fully, Equation (2.7) is

$$\text{Minimize } \hat{\alpha}_0 + \hat{\alpha}_1 \overline{X}_1 + \ldots + \hat{\alpha}_m \overline{X}_m. \tag{2.8}$$

The problem then is to

$$\begin{aligned} &\text{Minimize } \hat{\alpha}_0 + \hat{\alpha}_1 \overline{X}_1 + \ldots + \hat{\alpha}_m \overline{X}_m \\ &\text{Subject to } \hat{\alpha}_0 + \hat{\alpha}_1 X_{11} + \ldots + \hat{\alpha}_m X_{m1} \geqq Y_1 \\ &\qquad \vdots \\ &\hat{\alpha}_0 + \hat{\alpha}_1 X_{1n} + \ldots + \hat{\alpha}_m X_{mn} \geqq Y_n \\ &\hat{\alpha}_i \geqq 0. \end{aligned} \tag{2.9}$$

This can be solved by any linear programming package. The vector $\frac{Y_j}{\hat{Y}_j}$ is the index of efficiencies.

A third approach to frontier estimation builds on either the Farrell or the Aigner and Chu approach, but does not allow the frontier to be determined by the marginal observations alone. Within the Aigner and Chu context, Equation (2.3) would be translated from a deterministic inequality to a probability statement of the form

$$Pr\left(\prod_{i=0}^{m} x_{ij}^{\hat{\alpha}_i} \geqq y_j \right) \geqq P,$$

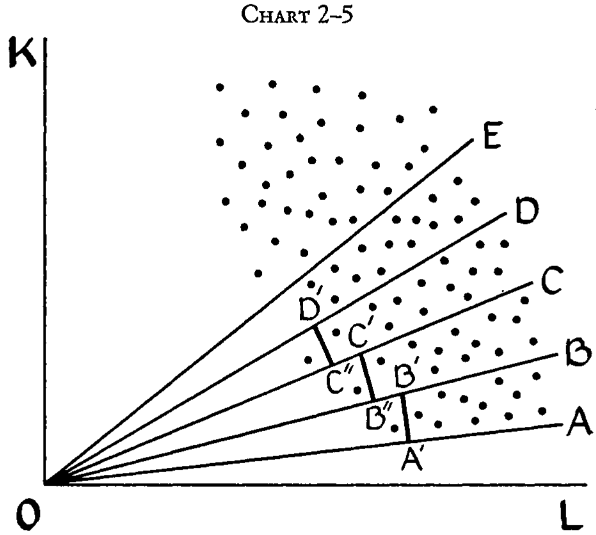
with P an externally specified probability, e.g., 95 per cent, with which the equation is to hold (2, p. 838).

Although it is difficult to get a program to do this automatically, it is relatively easy to do by hand with standard LP programs. The problem in (2.9) is estimated in its entirety and the 100 per cent "efficient" observations noted. There will be as many "efficient" firms as there are factors of production with $\hat{\alpha}_i > 0$, barring ties. This is an inevitable consequence of working in a linear world. These "efficient" firms may be efficient because of errors of observation or other problems. The technique then is to discard the first (100 — P) per cent of "efficient" firms until a prespecified level of P is reached. Thus 5 per cent of the extreme observations might be discarded with 95 per cent of the observations determining the frontier. Alternatively, "efficient" firms might be discarded one at a time until the resulting estimated coefficients stabilize. Either way, the objections to estimating a frontier function because of data problems may be largely overcome in this fashion.[14] This form of probabilistic frontier function, using the single-signed "least lines" fit to a Cobb-Douglas function, is used for much of the empirical work in Chapters 5–7.

A density-type function would seem to be the most logical for a Farrell frontier estimated with probabilistic rather than deterministic intent. If the observations are scattered about the "average" function with the density of observations greatest near the center and diminishing in each direction, then a ray from the origin toward the observations will meet the deterministic frontier first, and then encounter an increasing density of observations. When the density reaches a predetermined level, then the frontier is drawn in. By varying the density it would presumably be possible to have the frontier coincide with the average, or one standard deviation from the average, etc.

An alternative approach is illustrated in Chart 2–5. A narrow cone (say, 5°)

Chart 2–5



[14] It should be noted, however, that the frontier becomes increasingly biased in a pessimistic direction as extreme observations are removed. This bias is of little consequence if the sample is very large and if the extreme observations are in fact the product of data errors rather than reflecting superefficient firms that yield strange production function coefficients.

is drawn from the origin, e.g., AB. All observations falling in the cone are enumerated and the inner, say, 10 per cent are segregated from the rest by a line, e.g., $A'B'$. As the cone is swung from one axis to the other (either in discrete steps or continuously) segments such as $A'B'$, $B''C'$, $C''D'$, etc., are generated. By joining the midpoints (or either endpoint) of such segments a smoothed probabilistic frontier is formed. No programming technique has been developed along these lines as yet, but it seems clear that one could be devised.

CHAPTER 3. "AVERAGE" PRODUCTION FUNCTIONS AND TECHNICAL EFFICIENCY

"Average" production functions, i.e., those estimated by a statistical technique such as least squares that minimizes errors on both sides of the estimated function, have received far more attention than frontier functions. The reasons are numerous, but the most important is the dominance of a statistical theory attuned almost solely to zero average errors.

Only two functional forms have received widespread favor from economists in search of empirical production functions—the Cobb-Douglas and the constant elasticity of substitution, or CES. The CES function can be formulated with an efficiency parameter, but the function is very difficult to estimate and interpret with more than two factors of production. With the six factors of production[15] in the data set used for the empirical work in this study, the CES function becomes unmanageable. No further consideration will be given to it here.

The Cobb-Douglas function is the standard for the profession. Although some of its secondary characteristics are disturbing, especially unitary elasticity of substitution and separability of the contribution of each factor of production, its primary characteristics—ease of handling and generally good fit—continue to recommend it to economists. Of course, a good deal of care must be taken so that the model estimated corresponds to the decision-making model of economic theory. The Cobb-Douglas function, along with most other production functions, suffers from a major difficulty in this respect. Application of ordinary least squares to the single equation Cobb-Douglas function, linear in logarithms, yields biased results. The bias, in general, is due to misspecification of the estimated function. In particular, there are two sources of bias—simultaneous equation bias and management bias.

The techniques used to remove simultaneous equation bias are interesting in their own right but of little relevance here. It is important that some means be used to remove this bias in order that the remainder of the work have value, so a method particularly suited to agricultural production functions will be outlined. The real interest, however, lies in the technique used to remove management bias, for a by-product is an index of efficiency. The interest in estimating an "average" production function is, then, not idle. This function and its resulting index, while not fitting well with traditional theories of the production function, will be amenable to all the standard statistical tests of significance. It is challenging to set off to the frontier, to examine its coefficients, and to construct an index of efficiency from its boundary. But with nothing with which to compare the results, and no tests of significance to form confidence intervals, the trip would be devoid of real meaning. The "average" function, estimated consistently and without bias, will thus serve as a foil to the frontier function.

Removing Simultaneous Equation Bias

The problem is most easily seen in the context of estimating the simple two-factor Cobb-Douglas model. Let

$$y = ak^{\alpha}l^{\beta} \tag{3.1}$$

[15] Chapter 1 talked of five primary factors of production, including management. The empirical work uses six factors of production, excluding management. The difference is caused by splitting the intermediate input into three separate factors—livestock, fertilizer, and seeds and miscellaneous.

be the deterministic production function facing all firms in the industry. It is linear in logarithms.

$$Y = A + \alpha K + \beta L, \tag{3.2}$$

where, as before, capital letters denote logs. This equation cannot be estimated statistically because there is no stochastic term.

Assume some form of random shock affects production in the real world so that the function is instead

$$y_i = ak^{\alpha}l^{\beta}e_i, \tag{3.3}$$

where e_i is lognormally distributed with mean one and contains, among other things, differences in "efficiency" between firms. In logs the equation is

$$Y_i = A + \alpha K + \beta L + E_i. \tag{3.4}$$

This cannot be estimated directly by ordinary least squares without bias and inconsistency in the resulting parameters. The reason is that profit maximization, the normal economic assumption for competitive (as opposed to regulated) industries, imposes additional constraints on (3.3) or (3.4).

Let $r =$ interest rate on capital, $w =$ wage rate, and $p =$ price of output. Then:

$$\begin{cases} p\dfrac{\delta y_i}{\delta k} = p\dfrac{\alpha}{k}ak^{\alpha}l^{\beta}e_i = r \text{ and} \\[2ex] p\dfrac{\delta y_i}{\delta l} = p\dfrac{\beta}{l}ak^{\alpha}l^{\beta}e_i = w. \end{cases} \tag{3.5}$$

Rearranging and converting to logs yields

$$\begin{cases} K = \log\dfrac{\alpha p}{r} + A + \alpha K + \beta L + E_i \\[2ex] L = \log\dfrac{\beta p}{w} + A + \alpha K + \beta L + E_i. \end{cases} \tag{3.6}$$

Thus the level of use of the factors K and L depends not only on exogenous prices of the factors, but also on the error term in the original equation to be estimated, (3.4). Application of ordinary least squares to (3.4) yields biased and inconsistent estimates of α and β (*23*, p. 233; *30*, pp. 143–205; *48*, pp. 1–66; *37*, pp. 86–100).

Several proposals have been made to overcome the problem of simultaneous equation bias, but in the present agricultural context the most relevant is a set of assumptions developed by Hoch (*21*, pp. 34–53).

The basic question is whether the farm decision-maker looks at (3.3) when trying to maximize profits, or at some variant of (3.3). If he differentiates (3.3) in order to equate marginal revenue product of each factor to its cost, then ordinary least squares estimates of (3.4) will be biased. But assume that when the farmer makes his factor input decisions he does not know what the resulting output will be. Some unknown factor, particularly weather, will intervene between the input decision and the output result. What does the farmer do then? Hoch suggests that *anticipated* output rather than current output is differentiated with respect to inputs in the profit-maximizing calculus.

Intuitively the model eliminates the simultaneous equation bias because input decisions are made on the basis of anticipated output, which assumes that e_t equals one and hence has no impact. Formally, the solution is as follows. Let $A(y_{it})$ be anticipated output for firm i in year t, and be defined as $k^{\alpha}l^{\beta}$. Then:

$$p\frac{\delta A\,(y_{it})}{\delta k} = p\frac{\alpha}{k}\,(k^{\alpha}l^{\beta}) = r, \tag{3.7}$$

and it can be seen that e_t does not enter this decision equation (a similar equation naturally results with differentiation by the other factor or factors). This model justifies single equation estimation of (3.4).[16]

Removing Management Bias

The second form of bias is due to the known exclusion of a factor of production—management—from the estimated production function. As Griliches has shown, the bias resulting from this omission depends on the multiple correlation between the true management variable and all the included variables (*17*, pp. 8–20). The estimated coefficients of those included variables for which there exists a positive relationship will be biased upward, and downward bias will exist for those variables with a negative relationship.

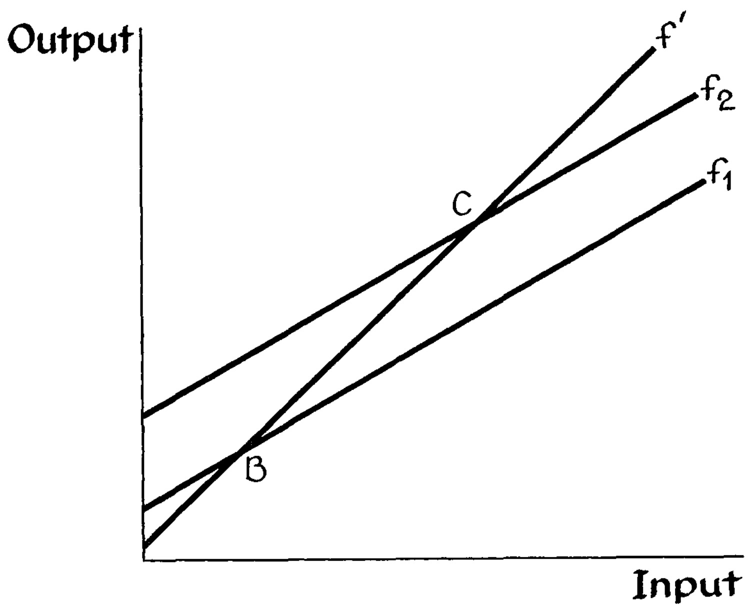
In agriculture it is generally argued that better managers use *more* of most inputs,[17] resulting in a bias similar to that pictured in Chart 3–1. There are two types of managers, good and bad, who use production functions f_2 and f_1 respectively (f_2 and f_1 are linear in logs). The observed data are B and C. In the absence of any knowledge about which observations pertain to good managers and which to bad, the fitted function will be f' rather than either f_1 or f_2. That is, the elasticity of output with respect to the input will be overestimated. The intercept will obviously have little meaning as well.

The solution to the problem requires more data. If management remains equally effective over time, then additional observations on each firm at different times would yield more than one point on f_1 and f_2. This use of a time series of cross-section observations and analysis of covariance estimation to overcome management bias was suggested independently by Hoch and Mundlak (*21*, pp.

[16] Hoch points out that *anticipated* output $A(y_{it})$ does not equal *expected* output $E(y_{it})$ because $E(y_{it}) = A(y_{it})E(e_{it})$. Only in the probability limit does $E(e_{it}) = 1$. It is necessary to assume that the decision-maker understands the difference between $E(y_{it})$ and $A(y_{it})$ when doing his differentiation. Any decision-maker who differentiates a production function to find his profit-maximizing output probably does.

[17] Unfortunately, it is possible to *prove* that better managers use more of all inputs if the managerial factor can be represented as a multiplicative index attached to a Cobb-Douglas function, e.g., $y_j = a_j k^{\alpha} l^{\beta}$ where a_j is larger for better managers. Since $\delta k/\delta a_j > 0$ and $\delta l/\delta a_j > 0$ due to the nature of the Cobb-Douglas, better managers (in this index sense) must use more of each factor. This is too strong a condition on the manager's performance, but it will turn out to be a necessary assumption to work with the analysis of covariance model outlined below.

CHART 3–1



34–53; *32*, pp. 44–56). An extension of the technique to a cross section of multi-product firms for a single time period was developed by Massell (*31*, pp. 495–508).

The assumed production function takes the form

$$y_{jt} = a_0 a_j a_t \prod_{i=0}^{m} x_{jit}^{\alpha_i} e_{jt}, \text{ where} \tag{3.8}$$

a_0 = overall intercept,
a_j = firm intercept, and
a_t = time intercept.

The error term, e_{jt}, is still presumed to be lognormally distributed with mean one, but is now free of any firm specific or time specific factors. Estimation of Equation (3.8), in its linear-in-logarithms form, will yield unbiased estimates of the α_i if the management function can be reasonably approximated by linear shifts in the logarithmic production function.[18] The vector of a_j contains the firm effects that persist over time. These effects determine the position of each firm's production function relative to all other firms. Thus a measure of technical efficiency can easily be generated.

Although this specification of the management function is better than none at all, it is still very troublesome. Management is assumed to shift the whole production function neutrally, with no change in factor elasticities anywhere along the function. While it is true that no factor in the normal Cobb-Douglas function interacts with any other, i.e., the function is separable, the impact of manage-

[18] Several additional papers have confirmed most of the merits of the analysis of covariance model for the present application (*33*, pp. 814–28; *3*, pp. 585–612; *47*, pp. 55–72).

ment almost surely contradicts this. In fact, an a priori specification of how management might have an impact in a production function would most likely be through changes in factor elasticities rather than by neutral shifts in the entire function. If this were the case, then the production function should be specified as follows:

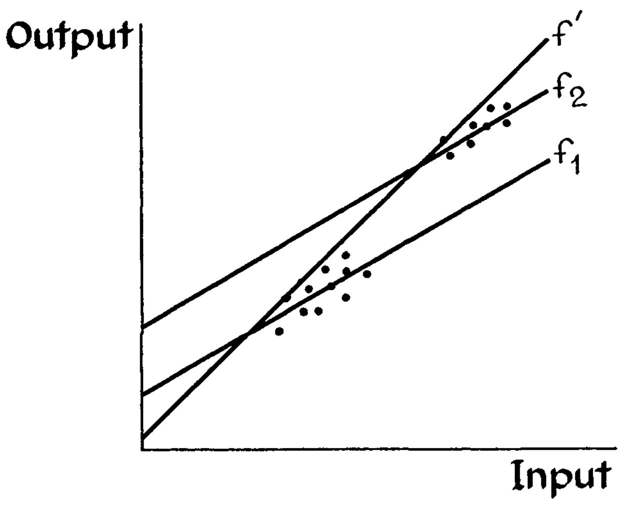
$$y_j = a_j k^{\alpha + \alpha_j} l^{\beta + \beta_j} e_j. \tag{3.9}$$

Each factor's coefficient is composed of an overall elasticity plus a firm specific elasticity.[19] This formulation is particularly appealing when the cross section of firms is wide and includes a number of different types of productive operations. Then the assumption of constant elasticities of output for all firms is tenuous.[20]

It is not clear how technical efficiency is measured in Equation (3.9). A vector of firm effects, a_j, can still be estimated and interpreted as neutral shifts in the function—i.e., as an index of technical efficiency—but the additional firm effects within each factor's elasticity of output confuse the matter considerably. It is quite conceivable, for instance, that $\alpha_j, \beta_j < 0$ for "good" firms; they use far more of the inputs, and declining elasticity of output has set in. On the other hand, one function of good management might be to devise productive techniques that prevent diminishing returns, and then $\alpha_j, \beta_j > 0$. A priori specification fails at this point and there is no empirical evidence available yet.

One final question about management bias arises. Does it occur in frontier production functions? Consider the distribution of data in Chart 3–2. If the data

CHART 3–2



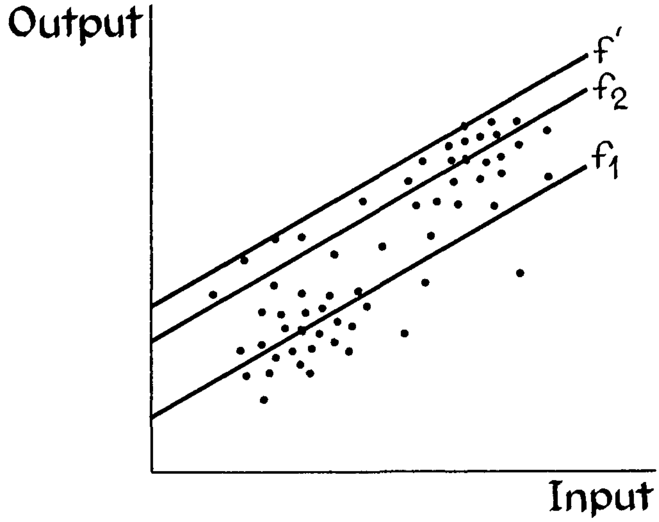
[19] I am indebted to Christopher Sims for suggesting this formulation of the Cobb-Douglas function.

[20] Management is obviously not the only factor that might interact with other factors in the production process. This suggests that it might be useful to add further terms to Equation (3.9), for example,

$$y_j = a_j k^{\alpha + \alpha_j + \alpha' l} l^{\beta + \beta_j + \beta' k} e_j. \tag{3.10}$$

Estimation of Equation (3.9) or Equation (3.10) is straightforward if the time series of cross-section observations is long enough, and if computer facilities, including programs, are adequate.

CHART 3–3



cluster completely around f_1 and f_2, with little dispersion, then the frontier f' will be "biased" as well. What this means is that *no* good managers use small amounts of inputs. An alternative possibility is represented in Chart 3–3. Here good managers *tend* to use more inputs and bad managers *tend* to use fewer inputs, thus biasing an "average" function; but a few good managers use small amounts of inputs and achieve the neutral shift from f_1 to f_2 even at this level. In this case the estimated frontier f' will not be biased but will be approximately parallel to f_1 and f_2. This second alternative seems like a more realistic representation of the real world, *assuming management operates by neutrally shifting the production function*. Should the frontier turn out to show substantial management bias it might cast additional doubt on this assumption.

CHAPTER 4. COMPARING AVERAGE AND FRONTIER PRODUCTION FUNCTIONS

The use of two different methods to generate estimates of parameters with similar economic interpretation invites comparison. The treatment in this chapter is analytical, with the empirical comparison deferred to Chapters 6–8. Three major aspects of the relationship of the two techniques are discussed here: (1) the statistical relationship; (2) the economic relationship; and (3) the relative contribution of each to understanding technical efficiency and change.

Statistical Relationship

No formal statistical relationship holds between an average production function fitted to a functional form such as Cobb-Douglas and a frontier production function enumerated by point sets. The reason is that the frontier function is drawn from a subset of the points that are summarized by the average function. An infinite number of frontiers are conceivable for every average function, and vice versa.[21] Comparison of parameter values (if some functional form is imposed on the frontier) and efficiency indexes only can be done empirically, although intuition might suggest a fairly high correlation between the efficiency indexes, at least by rank.[22]

The relationship between a Cobb-Douglas function fitted by traditional least squares and a similar function fitted to a frontier by single-signed "least lines"[23] should be subject to closer analytical treatment. But again, the reliance on ex-

[21] But note that for any *given* set of points there will be a unique frontier and average function. The statement implies only that for a given average function freed of its data set an infinity of frontier functions is conceivable.

[22] Professor M. G. Kendall, in discussing Farrell's original paper, made the following observations (*25*, pp. 286–87):

> If we take the figures in the first column "Land" of Table 1, and rank the 48 States according to them, we obtain a ranking of the numbers 1 to 48. The same procedure can be followed for the other three columns, and the rank numbers summed for the four variables, giving 48 rank sums. These numbers may then be used to arrange the 48 States in a descending order of "productivity" and the results compared with the ranking obtained from the final column of Table 2.
>
> In the rankings obtained from Mr. Farrell's figures there are a number of ties but this does not seriously affect the comparison. I have carried it out, and find that with a few exceptions the ordering given by the ranking method is very similar to the one given by Mr. Farrell's. The Spearman correlation co-efficient between my ranking and Mr. Farrell's is about 0.76, and, if we exclude six anomalous values, is 0.92.
>
> The anomalous values in question concern New Mexico, New Hampshire, Kentucky, Michigan, North Dakota, and Idaho, and I have looked at these individually. It seems to me that the ranking method gives results which on the face of it are at least as acceptable as those derived by Mr. Farrell. For example, in the case of North Dakota, my ranking makes it the 37th, whereas Mr. Farrell gives it an efficiency of 100 per cent.
>
> On the individual variables, North Dakota is 42nd for land, 25th for labour, second for materials, and 48th for capital. The discrepancy seems to arise from an extraordinarily low figure on materials, and on the whole it seems to me that my ranking is a fairer reflection of the position than Mr. Farrell's, since he gives it an efficiency of 100 per cent, notwithstanding that it is the least efficient in the use of capital and nearly the least efficient in the use of land.
>
> The ranking method, of course, purports to arrange the States of the Union in order and not to quantify the measurement of efficiency. If it is no more, however, it is an easy check on the more elaborate method, and from the examples I have given I think it may well prove to be more than that.

[23] That is, by minimizing the sum of error terms with all error terms constrained to one side of the function.

treme values precludes any meaningful analysis. It is only when the frontier is probabilistically determined that the way is opened for formal comparison, and still the precise relationship depends entirely on the nature of the probability function. Discretion in manipulating statistical models is, if not valorous, at least timesaving and efficient when the results depend so precisely on the assumptions. It is best to await the empirical results.

One formal point is worth mentioning here. A comparison of any of the frontier-derived efficiency indexes and Hoch's efficiency index derived with analysis of covariance shows one obvious and striking difference. The frontier index is determinate. There is no random error term. The Hoch index is estimated along with an error term. All variation from the frontier is due to efficiency differences according to the frontier measure. Only variation that persists *over time* is cast into the Hoch measure; all remaining variation (other than that explained by the time dummy variables) is considered part of the random error term.

The difference is partly one of concept and partly one of data. The Farrell frontier, for example, is designed to use a single year's cross-section observations. All variation not attributable to differential use of the factors of production becomes part of the efficiency index. But what happens if there is a time series of cross-section data as is required for the analysis of covariance efficiency model? Then the frontier model will have as many efficiency estimates for each firm as there are time periods. To be comparable to the analysis of covariance model, which yields just one estimate of efficiency for each firm, some adaptations must be made.

If a separate frontier has been estimated for each time period, then presumably all time-related effects (such as technological change) have been removed, and a simple averaging of the efficiencies would yield a single estimate for each firm that could be compared with the index derived from the analysis of covariance model. If a single frontier was estimated for the entire $(t \times n)$ data set, then any time effects must be removed. A somewhat artificial but straightforward technique would be to fit a simple time trend to each firm's series of efficiency ratings. The advantage of this technique would be that each firm could experience its own rate of "technological change," whereas it must conform to the rate of shift of the frontier if separate frontiers are estimated. Of course, if a firm's efficiency changes over time, the analysis of covariance model breaks down because the assumption that cov $(e_{js}, e_{jt}) = 0$ for $s \neq t$ is violated.

It seems clearer now that when the frontier technique is applied to a $t \times n$ data set the efficiency estimate is not determinate. It is an average figure derived from t observations. The net result is to cast doubt on the value of an efficiency estimate based on any of the frontier techniques that use only one year's data. It is equivalent to having only one year's data in the analysis of covariance model, and still using a dummy variable for each observation. The result, of course, is perfect correlation, with each firm's e_j as the estimate of its efficiency. This is biased because of the management bias problem. To be of more than formal interest, then, the frontier technique should have a data set that would also be suitable for analysis of covariance. Less than this yields a biased estimate of efficiency. Thus the estimate is reduced in practical value.

Economic Relationship

The economic relationship between the two production functions is far more interesting than the statistical relationship. At the heart of the comparison is the difference between average practice in an industry and best practice. The frontier production function represents the best techniques in actual application (as opposed to best potential techniques that no firms have yet adopted). This, of course, is the reason for using the frontier function as a base for judging the efficiency of other firms.

The average production function has a less clear-cut economic interpretation even though it has dominated most empirical work (*2*, pp. 829–30).

> A group of economists did notice the obvious conflict with theory, however, and some rationalization of this position was attempted. What they did was to assume that the function to be estimated, i.e., the conceptual construct, is an "average" production function for the industry. Some firms could therefore produce more than the average; some, less. But the meaning of such an "average" function is not necessarily clear. Average in the sense of what? a conditional median? a mean? or, a mode? More importantly, average *about* what? about output? about some input? about technology? or about something else? Some economists refer to it as the function for a "firm of average size." This interpretation cannot be correct unless it is assumed that the parameters of the function are random variables and have their expectations equal to those of the firm of "average size." Others seem to refer to the average function as reflecting some sort of "average technology." But it would be infeasible to assume that a firm which possesses "average technology" with respect to capital also has an "average technology" with respect to labor.

This last criticism is not quite sound. Technology generally refers to the whole productive structure of the firm rather than only the labor input or only the capital input. Thus the frontier production function at any point in space relates amounts of all inputs to output—in fact, to maximum output attainable from that *particular* combination of inputs. There may be a dozen firms with approximately the same input combination, but only one or two achieve maximum output from those inputs. The other firms achieve less, and it is meaningful to speak of the average attained output for that particular combination of inputs, and for that output to be representative of "average technology." The distinction between "average" and "best" can be justified if the comparison is between production *functions* and not between differential efficiency in the use of single productive *factors*.

The relevance of the distinction between "average" and "best" production functions is seen most clearly with reference to the literature on diffusion of innovations.[24] It is easier in this context to refer to the frontier production function in isoquant (input-input) space and to use as an example the simple capital and labor model used earlier. The argument extends to more than two factors in the

[24] The most recent, and certainly one of the best, contributions to this literature is Paul A. David, "A Contribution to the Theory of Diffusion," Memorandum No. 71, Research Center in Economic Growth, Stanford University, June 1969.

isoquant model and, more generally, to output-input production functions such as Cobb-Douglas fitted as an average and as a frontier.

Both new products and new processes are part of technological change. Only new processes are of interest in the context of estimating "average" and "best" production functions. At any moment in time a stream of innovations in productive technique is existent; some fresh from the inventor's mind and not yet adopted by any firm; some so old that all firms that can profitably use the technique are in fact using it. While traditional economic theory sometimes assumes that the time span between these two situations—call it $\hat{t}$ years—is close to zero,[25] the theoretical and empirical literature on diffusion indicates that $\hat{t}$ is positive and surprisingly large in some cases.

One theoretical argument holds that there is a cost to economic knowledge and that resolving uncertainty takes time. Thus factors that affect the rate of diffusion include not only the profitability of the innovation, but the degree of uncertainty about this profitability and the means by which the uncertainty can be resolved. Many studies have shown that educational level of the decision-maker or number of technically trained people in the work force affects the speed of innovation.[26] This argument holds that the reason is that these people are best suited to appraise the profitability of the technique a priori. Firms without the educational or technical resources must await the results from these innovative firms before being convinced the new technique is profitable. In addition to the uncertainty effect, the normal transition frictions and pressures that work in all less than perfect worlds lead to delays. Thus Mansfield, in a study of the diffusion of 12 innovations in 4 different industries found that the process was generally slow but the rate varied widely (*29*, p. 744).

> Although it sometimes took decades for firms to install a new technique, in other cases they followed the innovator very quickly. For example, it took about 15 years for half of the major pig-iron producers to use the by-product coke oven, but only about 3 years for half of the major coal producers to use the continuous mining machine. The number of years elapsing before half the firms had introduced an innovation varied from 0.9 to 15, the average being 7.8.

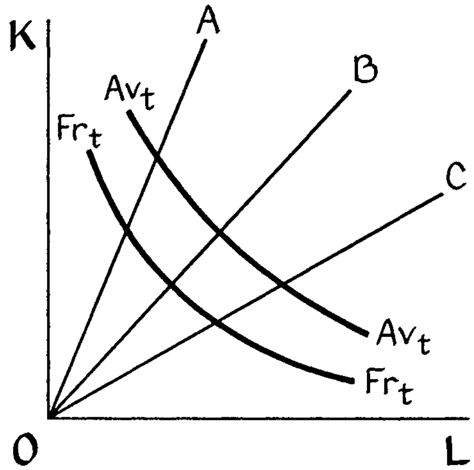
The significant diffusion time required for firms to adopt a new technique suggests a possible interpretation of the "average" and "best" production functions: the "average" function in year t is simply the "best" function of year $t - t^*$, where t^* is the time required for half the firms in the industry to acquire the innovation. Thus for Mansfield's data, t^* averaged 7.8 years.

This interpretation raises some new questions. Whole production functions are now assumed to be involved in the diffusion process rather than single, identifiable innovations. Does the average function shift slowly inward and superimpose itself on the frontier of t^* years earlier? This would involve an absolutely neutral diffusion process with all parts of the function shifting in parallel along rays from the origin, as illustrated in Chart 4–1. Thus the frontier, measured in

[25] All profitable innovations are assumed to be adopted immediately.

[26] For example, see *35*, especially Chapter 5. For a formal model incorporating this view of education, see *36*, pp. 69–75.

CHART 4–1



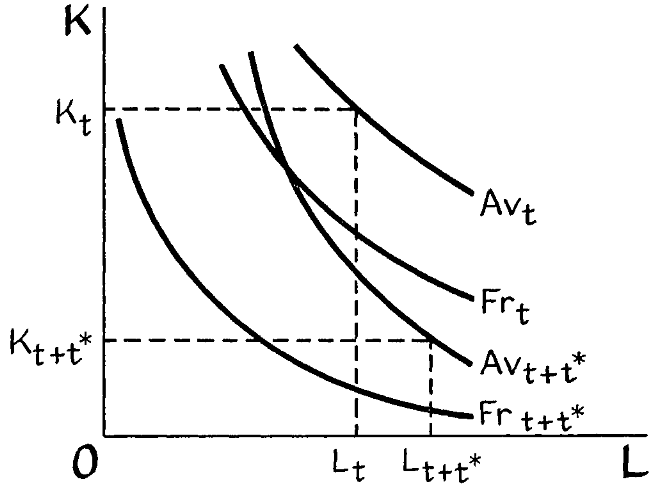
Av_t = Average, year t
Fr_t = Frontier, year t, and average, year $t + t^*$

year t, is parallel to the average function, also measured in year t along any of the rays OA, OB, OC, etc. In year $t + t^*$ the average function is the same as the frontier in year t.

It is possible, of course, that diffusion of the "best" to the "average" proceeds in this neutral fashion. But it seems somewhat unlikely, if for no other reason than that there must be a fairly sizable random element in the process. This is diffusion of a whole *range* of productive possibilities, not just a single innovation. Different relative factor costs, availabilities of natural resources, even transit times for factors and output to reach their respective markets could cause the average function to shift in nonneutral fashion. Institutional factors might also affect the relative speed of diffusion. An industry with a strong, active union might find the labor-intensive side of the function diffusing much more rapidly than the capital-intensive side due to featherbedding and make-work demands. Identical factor prices in the two periods would then result in a lower K/L ratio over time, as seen in exaggerated form in Chart 4–2. If the wage rate rose over time in these circumstances, the K/L ratio could remain stationary or even rise. The situation in industries without significant institutional constraints on labor practices might exhibit opposite tendencies. Particularly in agriculture it might be expected that the capital-intensive techniques would diffuse fastest with a consequent rise in the K/L ratio.

Average and best functions have comparative value even if there is no opportunity to observe the shifts over time. If the average function is a neutral transform of the frontier when both are measured at the same point in time, the implication is that the "average" firms and the "best" firms have similar K/L ratios when faced with identical factor prices. If the two functions are not parallel transforms then the K/L ratios should differ. It is generally thought that in agriculture the "best" firms have a higher K/L ratio than the "average" firms. This implies

CHART 4–2



Av_t = Average, year t
Fr_t = Frontier, year t
Av_{t+t^*} = Average, year $t + t^*$
Fr_{t+t^*} = Frontier, year $t + t^*$

a situation similar to that illustrated in Chart 4–3. Although $\alpha = \beta$ and so the average and frontier firms face the same relative factor prices, $\alpha' > \beta'$ and thus the frontier firm has a higher K/L ratio.

Examining Technological Change

Technological change can conveniently be broken into two types, neutral and nonneutral (6, p. 27).[27]

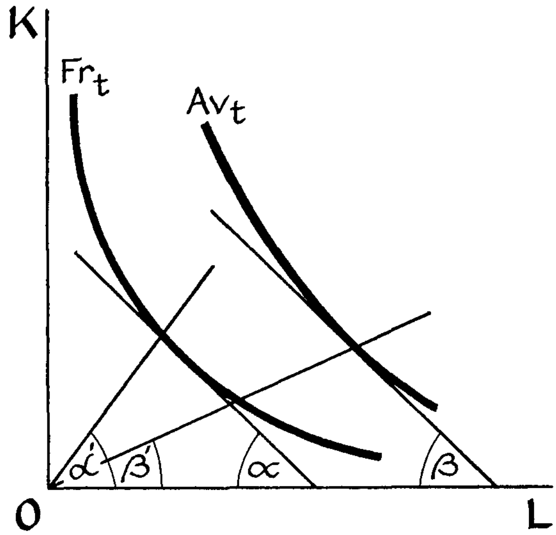
> A *neutral change* alters the production function but does not affect the marginal rate of substitution. A *non-neutral* change does affect the marginal rate of substitution. If the marginal product of capital increases relative to that of labour, for given labour-capital combinations, then a *labour-saving* or *capital-using* change raises the marginal product of labour, relative to that of capital, *cet. par.*
>
> Neutral technological changes include a change in the efficiency of a technology and/or a change in technologically determined economies of scale. A non-neutral change is associated with variations in capital intensity and the elasticity of substitution.

The two types of technological change are most easily seen in the familiar two-factor isoquant diagram, e.g., Chart 4–4. Thus AB is a frontier isoquant at time zero and $A'B'$ is the frontier after neutral technological change has taken place. That is, $A'B'$ is parallel to AB along any ray from the origin. The neutral shift can be attributed to a simple change in efficiency (affecting equally all K/L ratios) or to a change in technically determined economies of scale.

It is clear that $A''B''$ does not represent neutral change from the original AB

[27] It might be noted that Brown defines a production function as "the relation between a *maximum* amount of output and the inputs required to produce it" (6, p. 26) [emphasis added].

CHART 4–3



Av_t = Average, year t
Fr_t = Frontier, year t

frontier. The capital-intensive part of the production function has shifted relatively little and the labor-intensive part has shifted a good deal. The result has been a capital-saving technological change: at identical factor prices relatively (and possibly absolutely) more labor and less capital are used after the change than before.

The estimation of frontier isoquants by either the Farrell technique or any of the others is a natural first step in examining the neutrality of technical change. As different frontiers are estimated for different time periods and the results plotted relative to one another, both the speed and direction of shift are easily observed. The elasticity of substitution[28] can vary from isoquant to isoquant, and along a particular isoquant. There is no artificial constraint requiring a constant elasticity of substitution either on a single isoquant or before and after technical change.[29] Since there is no a priori reason for supposing technical change to be neutral, a technique for judging the extent and direction of any nonneutrality is

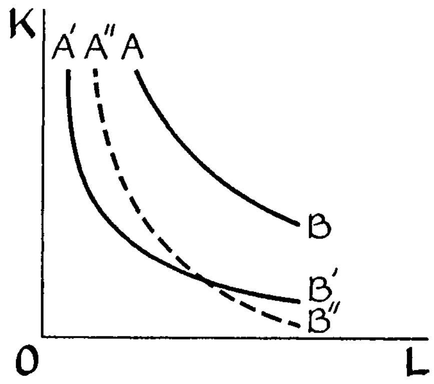
[28] Defined as

$$\frac{(K/L)\ \delta\ (K/L)}{(f_L/f_K)\ \delta\ (f_K/f_L)} = \sigma,$$

where f_L is the marginal product of labor $(\delta Q/\delta L)$ and f_K is the marginal product of capital $(\delta Q/\delta K)$. "The ratio of the marginal product of capital to the marginal product of labour is the marginal rate of substitution of labour for capital . . . . the elasticity of substitution as defined in the formula relates the proportional change in the relative factor inputs to a proportional change in the marginal rate of substitution between labour and capital (or the proportional change in the relative factor price ratio). Intuitively, it can be thought of as a measure of the ease of substitution of labour for capital; it can also be conceived of as a measure of the 'similarity' of factors of production from a technological point of view" (*6*, p. 18).

[29] Brown's entire book is ". . . *concerned only with technologies that can be characterized by constant elasticities of substitution*" (*6*, p. 19). This is a price that must be paid for working in a Cobb-Douglas or CES world. The CES function, of course, permits non-unitary values of σ but still requires that it be constant.

CHART 4-4



welcome indeed. Unfortunately, the value of the Farrell technique in this regard holds only in a two-factor world. With more than two factors it becomes almost essential to fit the frontier to some functional form, thus placing constraints on the elasticity of substitution.

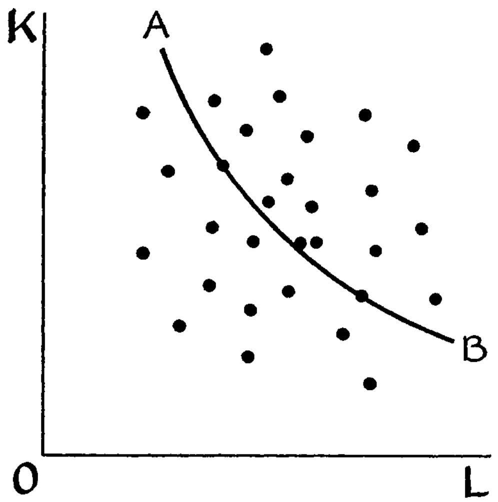
An additional benefit of the frontier model in understanding technological change relates to the manner in which it is estimated. By using only firms that are representative of the technical state of the arts to determine the frontier no confusion is introduced between a shift in technical achievement as represented by what the best firms are doing and the diffusion time required to transmit this technical knowledge (and equipment) out to average firms. Traditionally measured technical change[30] can occur due to both of these factors, i.e., the rate of generation and first use of technical knowledge may speed up (due, perhaps, to larger investments in research and development efforts) or the speed of diffusion may increase (due to better education of workers and management). While both would result in similar Solow-type estimates of technical change, the two processes are separate in theory and in social application. Investment to speed up Solow-measured technical change might be completely misdirected if spent in one direction while the actual change was coming from the other. In conjunction with the analysis of covariance model (to estimate "average" technology) the frontier model will generate the necessary data to allow society to make such investments somewhat more rationally.

This is an exciting potential, and the means by which it might be achieved deserves to be spelled out in further detail. A $t \times n$ data set is required—that is, a time series of cross-section observations. A Farrell-type frontier is estimated using the first time period (or first two or three if some averaging in the time series is desired and possible).[31] The whole $t \times n$ set of data is then plotted relative to the period one frontier. Graphically this might look like the representation in Chart 4-5.

[30] Neither a review of the traditional technique nor of the relevant literature is necessary here. The basic article is by Robert M. Solow (*43*, pp. 312–20). A good bibliography appears in *24*, pp. 280–83.

[31] Alternatively, census data in five-year intervals might be used.

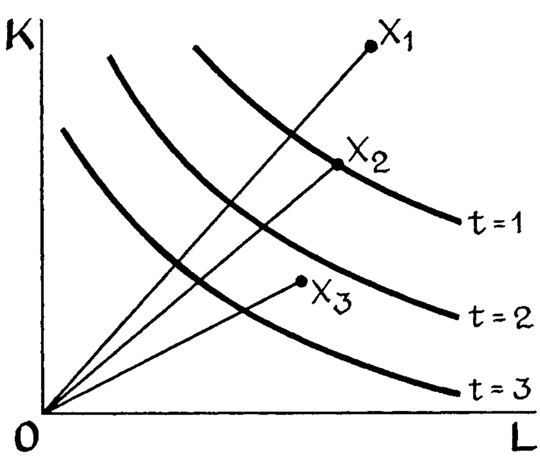
CHART 4–5



Each observation has both a firm and a time identification index. A $t \times n$ set of efficiencies is calculated relative to efficiency frontier *AB*—that is, relative to the production frontier in the first time period. In this model, however, some firms will likely have calculated efficiencies of over 100 per cent (all such firms must have a time index greater than period one). A time series of efficiencies is generated, with all firms' indexes starting at or below 100 and probably rising slowly and somewhat unevenly over time. In Chart 4–6, firm *X*'s efficiency rises from 75 at time 1 to 100 at time 2 to 150 at time 3, all relative to the period one frontier. At no time does firm *X* become efficient with reference to current frontier technology.

Now a search proceeds for factors that explain these efficiencies. Instead of

CHART 4–6



looking just for cross-sectional differences in educational factors, economies of scale, use of modern inputs, research and development expenditures, etc., to explain differences between firm efficiency at a single point in time, all of the explanatory factors now also have time subscripts. Thus different firms use these efficiency factors in different proportions at any point in time (thus explaining their cross-sectional efficiency differences at that time). But they also change these proportions over time, both with reference to their own input activities and *in relation to other firms*. A firm may gain in efficiency relative to a firm on the frontier by concentrating on a particular combination of "modern" inputs. This can happen even if the reference firm stays on the frontier in all time periods (as is likely if the unmeasured physical environment makes a major contribution in determining efficiency—this environment, of course, remains constant over time). Thus firm X may go from 75 per cent efficiency at time 1 to 150 per cent efficiency at time 2 while firm Y goes from 100 per cent efficiency to 160 per cent efficiency: X's gain is relatively greater although it does not achieve technical parity with Y in either time period.

Society is interested in X's success in catching up as well as in Y's absolute superiority. How did X catch up? Why is Y so technically proficient in both time periods? The answer probably has very little to do with technological change drifting down in neutral, disembodied form over firms X and Y. The answer more likely lies in differential use of inputs, especially those modern, nontraditional inputs that Denison and others have indicated have potential for generating economic growth (*10*).[32] The secondary equation just estimated helps identify these inputs and, more importantly, makes a start at assessing their quantitative impact. The particular form of the equation may or may not be critical in this context. In a sense the estimate is a "supply function for technical change," and the form of this function is anyone's guess. It was only a little more than a decade ago when the necessity to think about such a function was even called to the attention of economists.[33] The amorphous nature of the form of the function is not, then, very surprising. Nelson notes that since the work of Abramovitz and Solow many more variables than just capital and labor have been treated explicitly, with a consequent gain to both theory and policy making. "However, our knowledge still would appear to be quite weak with respect to the functional form of the relationships, and very weak with respect to the size of certain key parameters" (*34*, p. 481). A frontier model using a time series of cross-section data can be used to attack these problems head-on. Without artificially constraining functional forms, yet within the body and spirit of economic theory, the frontier model seems capable of repaying large dividends on its high data costs.[34]

[32] A particularly relevant piece of work in this context is an article by Zvi Griliches (*16*, pp. 331–46). The Jorgenson and Griliches article mentioned earlier is also useful, but see a rather scathing rebuttal by Robert J. Gordon (*13*).

[33] "This result is surprising in the lopsided importance which it appears to give to productivity increase [the 'residual' of technological change], and it should be, in a sense, sobering, if not discouraging, to students of economic growth. Since we know little about the causes of productivity increase, the indicated importance of this element may be taken to be some sort of measure of our ignorance about the causes of economic growth in the United States and some sort of indication of where we need to concentrate our attention" (*1*, p. 11).

[34] This is true only if a two-factor world makes sense.

Evaluating the Physical Environment

Differences in physical environment have been all but ignored so far. A frequent criticism of the application of the frontier model to agriculture is that almost all differences in efficiency will be due to soils and climates. The effects of these fixed factors may be so large as to swamp the effects from the secondary inputs like educational levels or scale economies.[35]

Some attempt, therefore, should be made to introduce the physical environment directly into the estimation of the frontier. Two different approaches are possible. Outside estimates of the effect of physical environment, independent of any of the output or input data being used to test the model, might be generated. At heart this is an ecological-biological problem somewhat beyond the expertise of economists. Work is being done in this area[36] but the results are still not suitable for use in the present model. A crude approximation is to use land value instead of area as a factor of production. This approach is used in the first rounds of the empirical work of Chapters 5–7.

A less satisfactory but more amenable approach is to combine the results of the analysis of covariance estimation and the frontier estimation. The procedure, which requires observations on individual firms to be meaningful, might work as follows: Using the entire $t \times n$ data set required for the analysis of covariance model, fit the Cobb-Douglas frontier in m-dimensional space and calculate the index of efficiencies, $Y_j/\hat{Y}_j$. This index will contain differences in efficiency due to physical environment as well as management techniques.

Next, the vector of efficiencies should be regressed on all the secondary explanatory variables that might relate to management—education of the operator, exposure to research and extension, scale factors, etc. No attempt is made to correct for the physical environment. Consequently this regression will be biased if there is any correlation between a farmer's management skill and the physical environment he works in.[37] Since this correlation is likely to be positive the estimated coefficients of the "management" variables will thus be biased upward. The omission of innate ability would have the opposite effect. The biases will be offsetting, but which is dominant will vary from situation to situation.

The secondary equation permits the calculation of an estimated vector of efficiencies Y_j^* that, except for the bias, is free of impact of the physical environment, which falls into the error term. Thus Y_j^* is that part of the efficiency index accounted for by "management" factors.

The analysis of covariance model can now be used to estimate an unbiased production function in $m + 1$ dimensional space, where the extra input is the management vector Y_j^* generated by the secondary equation above. The vector a_j will still contain a time-free "firm effect," but this "firm effect" should now also

[35] Nerlove, Hall and Winsten, and Sturrock all make this point in direct or indirect fashion (*37*, p. 90; *18*, pp. 71–86; *25*, p. 285). The strongest position taken on this subject is by Helen C. Farnsworth and Holbrook Working in informal communication with the author. The correctness or incorrectness of this position, especially with respect to the "swamping effect," is an empirical question unrelated to the theoretical development of the model, which has relevance outside agriculture.

[36] See, for instance, *38*, for a crop-specific study, and *39*, for a more general approach.

[37] It might be noted again that this secondary equation is a "production function" for efficiency (*17*).

be free of any management effect, which is removed separately as a factor of production. The vector a_j then is a measure of the physical environment.[88]

The vector of physical environment can now be inserted in the estimation of the original frontier to eliminate any specification bias. An iterative procedure can then be followed if the coefficients of the frontier production function change substantially due to introduction of the new physical environment variables. The second-round secondary equation should now have a substantially better fit, but not a perfect one. The random variation due to weather, etc., remains. Only the "explained" variation should be considered part of efficiency; the random element has little economic significance.

The differing physical environments, although mostly unchangeable,[89] contain important economic information. By estimating the marginal revenue productivity of each firm's physical environment, it is possible to determine if input resources are being allocated efficiently with reference to environmental productivity. It is possible, for instance, that transferring resources out of low productivity firms into high productivity firms would result in greater output, even though the high productivity firms already have substantially higher levels of input use. On the other hand, attempts by farmers to be located only on high productivity farms may result in underutilization of low productivity areas.

[88] The similarity of the first round of this technique to two-stage least squares is striking.

[89] This is especially true at the micro-decision level. Thus individual farmers can do little to affect climate or basic soil type, but collective action may accomplish substantially more. The TVA project, for instance, was a form of altering the physical environment.

The Basic Data Set

To test the models developed in Chapters 1–4 a time series of cross-section observations on inputs and outputs of individual firms is required. In addition, information on the firm's decision-maker is required if the secondary goal of the model, estimating the relationship between technical efficiency and management, is to be reached. Thus the data requirements are extraordinarily severe. Some compromises were inevitable from the beginning, but the nature of the data set ultimately used is distressing nonetheless.

The basic data set is an 8×48 matrix, where each of the 48 contiguous states is considered a "farm firm" and the observations are over the eight-year interval 1960–67. What will be estimated, then, is an aggregate agricultural production function, thus raising all the familiar problems of aggregation of both input and output over firms into state data (*48*, pp. 8–11).

> 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?
>
> . . .
>
> 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.

An additional problem is that even when aggregation makes sense and an additively separable production function, such as the Cobb-Douglas, is to be fit,[40] entrepreneurship remains firm specific and does not seem capable of aggregation. The implications for the model here are severe: differences in technical efficiency due to management will be washed out in the aggregation. It will not be possible to carry the empirical work as far as the theoretical model would indicate. A new sample of individual firms is necessary if the relationship between technical efficiency and management is to be explored. Such a sample has been located and is now being analyzed.

Some troublesome assumptions must be made. The United States agricultural sector is assumed describable by a single aggregate production function. In addition, the fact that firm data are aggregated linearly and not geometrically is assumed to be of negligible consequence. These assumptions are necessary just to use the data set for the first round of production function estimations. While unfortunate, these assumptions have mostly been made before by other workers in the field and to good effect. It is wise to be aware of the limitations of data, but it is also necessary to realize that even the worst data may contain valuable information if the proper search techniques are used.

[40] For sensible aggregation, the production function must be additively separable. See, for example, *48*, p. 9.

The main source of data was *Farm Income: State Estimates, 1949–1967* (*46*). Since this source reports data back to 1949 it would have been possible to use 19 years in the time series rather than 8. The shorter, more recent set was used for several reasons:

1) Some of the labor data were not available prior to 1961. Extrapolations were made for several observations for 1960 but these are subject to sizable error, and further extrapolation was deemed unwise.

2) Substantial technological change, if nonneutral or nonuniform by state, would have invalidated the analysis of covariance model. Even neutral, uniform technical change would have meant that several estimates of frontier-determined efficiency would be needed. By restricting the sample to the 1960–67 period it was hoped to keep all forms of disembodied technological change to a minimum. The empirical results strongly suggest this was achieved.

3) The 8 × 48 data matrix yields 384 observations. When separate firm and year dummy variables are introduced along with the six normal factors of production in the analysis of covariance model, the estimating equation contains 62 variables. The available computer facilities, both hardware and software, were strained at this level. A larger data matrix would have been impossible to handle.

Gross Output

The dependent variable, Y_{jt}, is gross agricultural output of state j in year t, divided by the number of farms in state j in year t. All factors of production are also on a *per farm* basis. Each Y_{jt} is built up from livestock, crop, and government payments components which are deflated separately. The deflators, shown in Table 5–1, are year and component specific, but not state specific. Thus, they are of the form D_{kt} rather than D_{kjt}, where $k =$ livestock, crops, and government payments. It is possible, of course, to calculate a D_{kjt} and to divide the k components into further subclasses. But the magnitude of the task and the strongly diminishing returns to improving any one variable when all the others remain of low quality suggested the effort would be unrewarding.

Gross output rather than net output is used because intermediate inputs are included in the estimated production. If all intermediate inputs were used to the point where their marginal revenue product equaled marginal cost, then working

TABLE 5–1.—DEFLATORS FOR GROSS OUTPUT*

Year	Livestock and products	All crops	Government payments[a]
1960	100	100	100
1961	99	102	100
1962	101	105	102
1963	97	108	102
1964	93	108	99
1965	103	105	104
1966	115	107	112
1967	109	101	106

* Indexes of prices received by farmers, from *44*, 1968, p. 470.
[a] Index for all farm products.

TABLE 5-2.—AVERAGE DAYS WORKED AT FARM WAGE WORK, U.S.*

Year	Days	Year	Days
1960	139	1964	129
1961	134	1965	137
1962	134	1966	138
1963	138	1967	142

* Data are from *44*.

with net output would be equally correct. But there is widespread feeling, based on a good deal of empirical work, that for a number of inputs marginal revenue product is not equal to marginal cost.[41] To discover their real contribution to production these inputs must be included in the estimated function.

Labor

The labor input variable is an unsophisticated measure of total man-days worked in agriculture. The basic data, reported in *Agricultural Statistics,* are for total farm employment for both family and hired workers. Family workers include farm operators doing one or more hours of farm work and members of their families working 15 hours or more during the survey week without cash wages. The number of hired workers includes all persons doing farm work for pay during one survey week each month. Survey weeks were selected to be the latest calendar week that excluded the last day of the month.

In order to convert the number of bodies of workers into a slightly more representative indicator of time worked, the numbers employed on farms for each state in any year were weighted by the average number of days worked at farm work in each year. This index is shown in Table 5-2.

No attempt is made to correct the labor input for quality differences, e.g., age, sex, or educational level. The data necessary to do this are not available on a year-by-year basis. Any cross-sectional differences that persist over time will be cast into the residual of technical efficiency.

Capital

The capital variable almost always presents the greatest difficulty in production function estimation. The problem is both theoretical and empirical. Even a theoretical measure of the infinite variety of capital items is not to be had, and a simplified measure assuming all capital equipment is alike can usually not be constructed due to lack of data.

Traditionally the answer has been to construct some measure of capital stock and assume that this stock was *proportional* to the flow contribution of the stock into the productive process. Yotopoulos has shown that this approach is generally not satisfactory and that it is much better to work with the flows themselves (*49*, pp. 476-91).

The capital input variable used here is such a flow construct, or at least a proxy to it. It is a USDA-reported current farm-operating expense that includes repairs

[41] See especially *15*, pp. 961-74.

TABLE 5-3.—DEFLATORS FOR CURRENT EXPENSES ON CAPITAL ITEMS*

Year	Index	Year	Index
1960	100	1964	102
1961	101	1965	104
1962	101	1966	106
1963	102	1967	109

* Constructed from data in *44*.

and maintenance of buildings, repairs and operation of motor vehicles and other machinery, and petroleum fuel and oil used in the farm business. This repairs-and-operation-of-capital-items variable is assumed proportional to the total capital flow variable. This assumption would be very bad if individual farm data were used, because the repairs component is so lumpy. But the lumpiness should smooth out when farm data are aggregated to state data and then converted to an "average farm" basis. For once, aggregation is a benefit rather than a problem.

The repairs-and-operation-of-capital-items variable is deflated by an index of prices paid by farmers for such things as gas and oil, building materials, and garage services. This is shown in Table 5-3.

Land and Buildings

Unless land area used in agricultural production is weighted by some measure of its productivity, the results tend to be meaningless. An acre of rich Iowa soil is simply not the same factor of production as a rocky, barren acre of grazing land in New Mexico. The solution taken here is the usual one—to assume that the real estate market does its job well, and so quality differences in land are reflected by differences in the sales value. A further assumption that must be made is that *marginal* revenue product (equal to sales value) is proportional to *average* revenue product for sales value to be a good proxy for productivity. No evidence one way or the other is available to test this assumption. Although some site value is going to be included in the sales value measure as well as productivity differences, the bias seems to be less this way than just using unweighted acres.[42]

The flow input of land in state j for year t is thus defined as:

$$D_{jt} = .05\, LV_j\, [\mathrm{I}_{j64} + \mathrm{II}_{j64} + \mathrm{III}_{jt}] + .02\, BV_{j64},$$

where

LV_j = Value per acre of farmland in state j. Averages of the 1959 and 1964 Censuses of Agriculture data for value per acre are used. No time subscript is attached.

BV_{j64} = Total value of buildings in state j in 1964; taken from *Farm Real Estate Market Developments.*

[42] A very serious objection to this procedure arises when the data set contains time series as well as cross-section observations. The partially local nature of agricultural demand and the immobility of land combine to make *changes* in land value a good predictor of output variations. Christopher Sims has emphasized, in a personal communication, that this explanatory power has nothing to do with technology or land productivity. The criticism loses much of its force, however, if *changes* in land value are not permitted. Thus in the land specification used here a single unit land value is used as a proxy weight for natural productivity differences, and *the same weight is used for all eight years in the data set.*

I_{j64} = Total cropland used for crops in 1964 plus cropland used for soil improvement or idle in 1964 minus USDA-reported harvested acreage of 59 crops in 1964.

II_{j64} = Cropland used for pasture in 1964 plus open permanent pasture in 1964 plus pastured woodland and forest in 1964.

III_{jt} = Harvested acreage of 59 crops in year *t*.

Coefficients of 5 per cent of land value and 2 per cent of building value are used to translate the stocks into flow variables. The building coefficient is purposefully low for two reasons: (1) building repairs have been counted in the capital input, so this component should cover only the depreciation factor—2 per cent of the value of buildings seems reasonable—and (2) farm buildings tend to be as much a consumption good as a factor of production. My experience growing up in rural Ohio was that farmers put up expensive buildings to prove to the community that they were successful, and not that expensive buildings led to success.

Only one part of the land variable has a time subscript: the harvested acreage of 59 crops. There are no annual data for the other components, so the 1964 Census data must be used for all 8 years.

Fertilizer

Relative to the previous factors of production, the fertilizer variable is simple conceptually and empirically. Actual physical quantities of each nutrient, N, P_2O_5, and K_2O, applied in each crop year (July 1 [$t-1$] to June 30 [t]) in each state are available. The only difficulty is how to weight the different nutrients according to their relative productivities. One means is to use relative prices as an indicator. Griliches reported a set for 1955 as follows, in dollars per 20 pounds of nutrient (*15*, p. 967):

Nutrient	Price
N	1.62
P_2O_5	0.93
K_2O	0.45

Since 1955 a major change has taken place in the pattern of fertilizer consumption in the United States. Nitrogen application has risen substantially while the other two nutrients have not kept pace. Marginal productivity of nitrogen has probably declined substantially since 1955, so a new set of weights is desirable. To obtain them, the following equation was estimated:

$$\text{Total price per ton} = A + (P_N)\ (\%N) + (P_{P_2O_5})\ (\%P_2O_5) + (P_K)\ (\%K_2O).$$

The data used in this regression were prices paid by farmers for different analysis fertilizers, e.g., 10-10-10, 10-15-10, etc., for the years 1960–67. No attempt was made to weight each type of fertilizer by the amounts used, but only popular types were included. The results are reported in Table 5–4.

Comparison with the Griliches weights is interesting. The implicit productivity of nitrogen is still the highest, but relatively much less so than in 1955. The reasons are twofold: (1) the increased use of high nitrogen fertilizer has indeed reduced its marginal productivity, and (2) technology has reduced the price of

TABLE 5-4.—IMPLICIT FERTILIZER PRICES*

Nutrient	Coefficient	(t-value)	
N	1.779	(7.2)	
P_2O_5	1.355	(14.2)	
K_2O	0.984	(6.6)	
Constant	17.439	(6.7)	$R^2 = .89$

* Original data from *44*.

high nitrogen fertilizer. This latter reason is "bad" for getting productivity weights if farmers do not react immediately to the lower prices, and it is virtually certain that they do not. However, the weights seem reasonable compared to the 1955 Griliches weights, and are clearly better than no weighting at all.

Livestock

The definition of the livestock variable is:

$$V_{jt} = (\text{Feed}_{jt})\,(W_{60}^{1}) + (CLX_{jt})\,(W_{60}^{2}) + 0.08\,(\text{Cattle}_{jt})\,(P_{60}^{1}) + (\text{Hogs}_{jt})\,(P_{60}^{2}) + (\text{Sheep}_{jt})\,(P_{60}^{3}) + (\text{Chickens}_{jt})\,(P_{60}^{4}) + (\text{Turkeys}_{jt})\,(P_{60}^{5}),$$

where

Feed_{jt} = Current feed expenses for state j in year t.
CLX_{jt} = Current livestock expenses (purchases) for j, t.
W_{60}^{i} = Price deflators for feed and livestock expenses.
Cattle_{jt} . . . Turkeys_{jt} = Numbers of such animals on farms January 1 of year t in state j.
P_{60}^{i} = Average value per animal in 1960. This value is the average for the whole country and thus is not state specific.

The 8 per cent discount rate to convert livestock capital values into flows is the same as that used by Griliches (*15*, p. 967). So far three *different* rates of return have been used to convert stock values to flows: 8 per cent for livestock, 5 per cent for land, and 2 per cent for buildings. Neglecting the building case as a special situation, the two different rates for livestock and land presume that farmers are not equating marginal returns from the last dollar invested in land and livestock. But some account must be taken of the trade-off between average return and variance. The rates used here assume the variance is higher for returns to livestock and lower for land, an assumption that seems perfectly realistic. Thus farmers must earn a higher *average* rate of return from livestock to be indifferent between investing in livestock and investing in land.

Seed and Miscellaneous

This variable is the sum of two current expense items reported in the *Farm Income: State Estimates* publication. The seed component also includes minor amounts for bulbs, plants, and trees. It is deflated by the seed section of the index of prices paid by farmers. The miscellaneous component includes a large variety of items and is deflated by the index for all commodities bought. Of special relevance in the miscellaneous index are charges for pesticides, electricity, irrigation, and veterinary services and medicines. These modern inputs are probably used most heavily by "efficient" farmers. The coefficient of this variable may thus turn out to be quite important.

The Production Functions

Given the quality of the data revealed in Chapter 5 it is perhaps surprising that there is any empirical production function at all. But Table 6–1 shows that there is. The coefficients are those estimated for a Cobb-Douglas function linear in logs using the entire 8×48 data matrix.

Average functions.—Equation 1 reports the simplest possible production function estimated by ordinary least squares (OLS), using all six factors of production.[43] The fit is amazingly good. The six factors of production explain 97 per cent of the variation in output. Prior warnings that aggregation would subsume most differences in technical efficiency were entirely justified.

Equation 1 is not subject to simultaneous equation bias if farmers maximize *anticipated* output, as assumed, but it is subject to management bias. No management variable is introduced explicitly and no analysis of covariance is performed. The size and significance of all the coefficients, then, must be interpreted in that context.[44]

Equation 2 still makes no attempt to remove management bias, but instead tests the possibility that no single aggregate production function holds for all of U.S. agriculture. Due to the large number of variables involved and the good fit of the simplest form of the Cobb-Douglas function it is impossible to estimate a production function of the form

$$y_j = ak^{\alpha + \alpha_j} l^{\beta + \beta_j} e_j. \tag{6.1}$$

Equation (6.1) would not remove management bias in the sense of neutral shifts in the production function for better and worse firms, but it would permit, perhaps more importantly, the elasticity of output for each input to be different for each firm (or group of firms, if j represents, say, geographic or "production" regions). Although it is not possible to fit Equation (6.1), it is possible to fit a proxy for it. The most drastic differences in input elasticities of output are likely to occur between largely arable farms and largely livestock operations. Thus Equation 2 in Table 6–1 introduces p_j, the proportion of livestock in total output, as a continuous variable within four of the six input coefficients. The production function is of the form

$$y_j = al^{a} k^{\beta + \beta_1 p_j} d^{\gamma + \gamma_1 p_j} f^{\delta + \delta_1 p_j} v^{\epsilon + \epsilon_1 p_j} m^{\zeta} e_j. \tag{6.2}$$

No differences were apparent for the labor or seed and miscellaneous variables.

In general, the introduction of p_j modifies the elasticities of output in plausible directions. The land and fertilizer coefficients, for example, drop significantly as livestock form a larger proportion of output. The livestock variable tends to be slightly higher as p_j increases. The capital variable loses all significance by itself,

[43] The reader is reminded that intermediate inputs have been split into three categories, and no management input is included.

[44] A t-value greater than 1.65 indicates significance at the 95 per cent confidence level, and greater than 2.34, at the 99 per cent level, for one-tail tests. A two-tail test should be used to test the significance of the p coefficients.

TABLE 6–1.—PRODUCTION FUNCTIONS

Equation	Technique	Coefficients (t-values)								R^2
		C	L	K	D	F	V	M	E	
1	OLS	1.7350	0.1919	0.3726	0.0458	0.1484	0.2510	0.1579		0.970
		(53.8)	(6.7)	(11.7)	(4.2)	(16.0)	(19.5)	(5.4)		
2	OLS	1.7804	0.0764	0.0844	0.1840	0.3243	0.2971	0.0849		0.978
		(60.0)	(2.8)	(1.1)	(4.2)	(12.0)	(7.7)	(3.3)		
				+0.6795 p	−0.3284 p	−0.3738 p	+0.0938 p			
				(4.6)	(−4.4)	(−8.0)	(1.6)			
3	OLS	a_t	0.2206	0.3832	0.0487	0.1444	0.2567	0.1336		0.971
			(6.4)	(11.9)	(4.3)	(14.1)	(19.6)	(4.1)		
4	OLS	$a_t a_j$	0.1231		0.3443	0.0481	0.3103	0.1222		0.994
			(2.7)		(4.2)	(2.2)	(8.3)	(2.8)		
5a	LP_{100}	1.6693	0.6015	0.4887		0.1334	0.2347	0.1043		
5b	LP_{98}	1.8578	0.3287	0.3689	0.0298	0.1428	0.2045	0.2243		
5c	LP_{97}	1.8828	0.2679	0.4842	0.0099	0.1693	0.1885	0.1712		
6a	LP_{100}	0.7089	0.3366	0.2556	0.1402	0.1022	0.2928	0.0977	0.6200	
6b	LP_{98}	0.3826	0.0532		0.2640	0.0574	0.2341	0.3270	0.9613	
6c	LP_{96}	0.4000	0.1115	0.0189	0.2861	0.0710	0.2617	0.2405	0.8723	

C = constant
L = labor
K = capital
D = land
F = fertilizer
V = livestock
M = seed and miscellaneous
E = "entrepreneurship"

and its total contribution to output is through the p_j modifier, which is large and positive. This result is somewhat surprising, but is due, perhaps, to the different nature of arable and livestock farming. Two other changes are also of interest—both the labor and the seed and miscellaneous coefficients drop substantially in size (and somewhat in significance, although both remain significant above the 99 per cent confidence level). This reduction in the relative role of the labor input, especially when allowances for differences in output mix are made, is the same phenomenon as reported by Griliches for a totally different data set for U.S. agriculture (*14*, pp. 419–28).

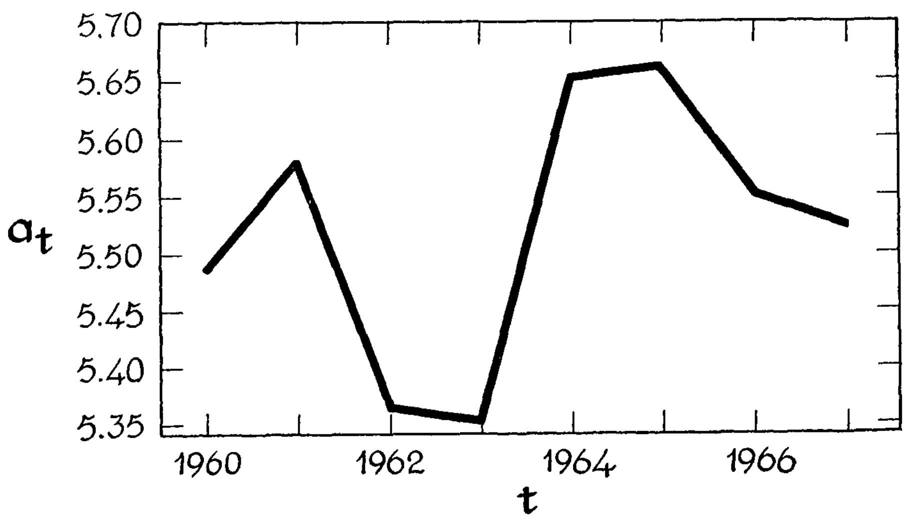
Equation 3 introduces a set of year effect variables to remove any neutral weather effects, deflation problems, etc. These time coefficients are significant at only the 90 per cent confidence level and show no trend at all, as can be seen in Chart 6–1. They are included in the full analysis of covariance model (Equation 4), even though their significance is small, to ensure that no time-related bias affects the other coefficients.

With Equation 4 the heart of the matter is reached. This equation introduces 48 firm effect variables, one for each state, to remove management bias (to the extent that this bias can be removed by neutral shifts of the intercept of the log-linear form of the production function). It would have been desirable to retain some of the features of Equation 2, where p_j was introduced into various input coefficients to allow for different production functions as the crop-livestock mix changed. But the high R^2 of .994 for Equation 4 prevented this—obviously the major part of the significance of p_j was picked up by the firm dummy variables.

What happens to the elasticities of output when management bias is removed? There are three very striking changes:

(1) The output elasticity of capital becomes completely insignificant, and capital is dropped from the production function altogether. The movement of change is certainly as expected although the magnitude is surprising indeed. Many people

Chart 6–1



have argued that the best farmers use a great deal of capital—the argument is advanced, in fact, that many are "overcapitalized." Thus it is to be expected that management and use of capital would be collinear and the capital coefficient should drop when the effect of management is introduced separately. The fact that it drops to zero is presumably more a function of the data quality than of the true productivity of capital.

(2) The output elasticity of fertilizer drops sharply as well, but not to complete insignificance. The use of fertilizer is also thought to be heaviest by good farmers. This result tends to confirm that judgment. It is worth noting, in addition, that since the elasticity of output for fertilizer in the unbiased regression is only about one-third that in the biased equation (Equation 1) the marginal revenue product is reduced correspondingly. Griliches found a ratio of marginal revenue product to marginal cost of between 3 and 5 for fertilizer (*15*, pp. 968–69). Elimination of management bias reduces this range to between 1 and 2. This ratio is still high enough to indicate some disequilibrium in the use of fertilizer, and thus to explain its growth in consumption, but is not so high as to strain one's belief in the rationality of American farmers.

(3) The coefficient of land in the unbiased production function becomes very large relative to other inputs and relative to its previous size in the biased equation. Thus the use of land seems inversely related to management. Exclusion of management from the production function negatively biases the estimated returns to land. In fact, it seems to play a very small part in producing agricultural output. But when the management effect is removed land assumes a more understandable role in the production function. The implications of this seem to be that good farmers do not spread their talents too widely over large farms (large in a value sense). This finding gives some impetus to the feeling that the "family farm" is still the most efficient in agriculture, although this conclusion obviously needs much more study before being accepted.

All of the differences reported between biased and unbiased functions should be viewed as qualitative rather than as concretely quantitative. Analysis of covariance has some tendency to bias the estimated elasticities of output downward if there are any errors of measurement in the variables.[45] There certainly are in the variables used here. The separate intercepts remove the firm specific means as information, leaving only the variance about the mean to estimate the coefficients. Any errors are thus magnified. A large sample would seem to dampen this effect.

Frontier functions.—The next stage is to examine frontier estimates of the same production functions discussed above. The "average" functions were presented first in order to have some familiar concepts and statistical tests of significance in hand when looking at the frontier estimates. This way there is some confidence that, at least in the traditional sense, a production surface exists for U.S. agriculture and that the real problem is to find the most meaningful way to estimate it.

Equations 5abc–6abc report the results of fitting the linear form of the Cobb-

[45] This problem was pointed out to me by Zvi Griliches. No published reference seems to be available, and in fact the direction of the bias is part hunch.

Douglas production function using the linear programming model outlined in Chapter 2. They are frontier production functions. Equation 5a, labeled LP_{100}, is the result of fitting a deterministic frontier function to the same data set as used in Equation 1. The results seem strange—especially the very large labor coefficient. Before reading much significance into these results it is wise to be sure that a few errors of observation are not the cause rather than earthshaking economic discoveries. Thus Equations 5b and 5c (LP_{98} and LP_{97}) report what happens as the first 2 per cent of the "efficient" firms from Equation 5a, and then the next 1 per cent, are removed from the data deck.

A rather remarkable transformation takes place. With just 2 per cent of the observations removed the estimated equation looks remarkably like Equation 1, which is an average function estimated with ordinary least squares. The similarity remains when another 1 per cent of the data is discarded, i.e., the coefficients seem to have stabilized. And these coefficients are, with minor exception, very similar to those of the analogous average function. The most obvious difference is that the intercept of the frontier function is about 14 per cent higher (when the antilogs are compared) than for the average function. This is certainly not surprising. Except for the labor coefficient, the factor elasticities are so nearly identical as to be safely considered insignificantly different.[46]

The 40–70 per cent rise in the labor coefficient is puzzling. The only reasonable explanation that comes to mind suggests that efficient farmers achieve their output with relatively less labor input than less efficient farmers, and that consequently the marginal productivity of labor is higher. The other factors have similar output elasticities because the amounts used increase proportionately, or approximately so, with output. If this is true, and it is at best a very tentative hypothesis, then earlier attempts to measure "efficiency" by looking only at output per worker were not as far from the mark as theory would have suggested.

In summary to this point, the frontier production function seems to have shifted almost neutrally outward from an average function, with the possible exception that the labor elasticity of output may have increased on the order of a half.

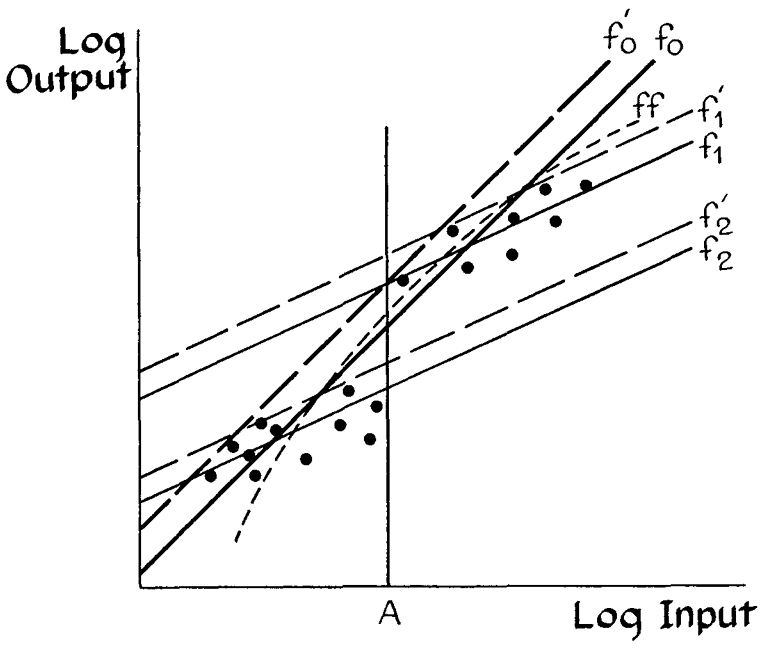
Management Bias

Equations 6abc report the results of an ad hoc experiment to remove management bias from the frontier function of Equation 5b or 5c. Consider that Equation 1 was shown to suffer a rather bad case of management bias—the capital and fertilizer coefficients were biased strongly upward and the land coefficient was biased downward. If the frontier coefficients of a similar function are nearly identical except for the intercept (and possibly the labor variable) then the frontier function must also suffer from management bias. That means the fears expressed at the end of Chapter 3 were well founded. The data set, if it could be represented in two dimensions, might look something like Chart 6–2.

No "good" farmers use a small amount of inputs, and so there are no observations on the actual frontier f_1' below an input level of A. The estimated frontier, f_0', is thus biased in a fashion similar to the estimated average function f_0. Fitting separate intercepts for each firm eliminated the bias in f_0, with the resulting

[46] There are no formal tests of significance with which this assertion can be checked, however.

CHART 6–2



average functions being f_1 for good farmers and f_2 for bad. What procedure might be used to eliminate management bias in the frontier?

The technique used here is at best a stab in the dark and is not to be taken very seriously. But the results are interesting enough to report. In Equations 6abc a new factor of production, labeled E (for entrepreneurship), is added to the data set.[47] The values of this variable are the separate intercept coefficients of Equation 4, the analysis of covariance model, which are assumed to serve as a proxy for management. There is only one value for each state, so each is repeated eight times so that there is an observation for each time period.

Once again the deterministic frontier, Equation 6a or LP_{100}, is somewhat strange and difficult to interpret. And once again the probabilistic form, with 2 per cent or 4 per cent of the observations removed, makes more sense.[48] Thus Equation 6b or 6c bears a noticeable resemblance to Equation 4, i.e., the production function free of management bias, *if the* E *variable is considered as a separate intercept term for each state.* The coefficient of *E* is very close to 1.0. Thus the separate intercepts of the analysis of covariance function are also the separate intercepts in this frontier function. It shifts up and down neutrally for good and bad farmers.

If this is accepted, then the rest of the coefficients of Equation 6b or 6c fall

[47] *M*, for management, was already taken by the seed and miscellaneous variable.

[48] The careful reader may note that 3 per cent of the observations were removed from Equation 5c and 4 per cent from Equation 6c. The difference is due to the number of factors of production with positive coefficients in the "b" runs and the fact that all "efficient" firms in the "b" runs were removed in the "c" runs. The results are very insensitive to changes in the number of observations removed, so no significance is attached to the difference.

neatly in place. The capital and fertilizer coefficients drop dramatically and the land coefficient increases. Even the magnitudes of the estimated coefficients are the same as in Equation 4. Removing management bias from the frontier *has precisely the same effect as removing it from the average function.*

The implications of this are rather disturbing. It makes little sense to estimate f_1 and f_2 (in Chart 6–2) as separate linear (in logs) production functions. It is quite meaningless to extrapolate f_1 beyond A because it seems to be impossible to get "efficient" levels of output with fewer than A inputs. There is either some interaction or scale effect not captured by the Cobb-Douglas function, or the actual production function is non-linear in logs, perhaps like ff in Chart 6–2. Either way the usual method of eliminating management bias by analysis of covariance techniques, while perhaps a practical approximation, is not very satisfactory for present purposes. A production function that allows firm effects and possibly even other factor effects within each factor's coefficient would be far more desirable.[49] It is a major disappointment that the nature of the data presented here does not permit estimation of such a function.

Technical Efficiency

The production function estimates are very interesting in their own right, but they are only a means to an end. Technical efficiency is the Holy Grail in this quest. The reason for estimating the production functions is to find the "right" way to correct for differential use of the factors of production—otherwise there is no way to judge one state's performance relative to that of another when different factor amounts and proportions are used.

Four different vectors of technical efficiency can be generated from the production function results of Table 6–1. They are reported in Table 6–2, along with rankings for each vector.

The most important vector, because it measures technical efficiency relative to a probabilistic frontier production function, is headed "Biased 98% LP." It is calculated from Equation 5b of Table 6–1 according to the following formula:

$$\text{Efficiency of state } j = \frac{1}{8}\sum_{t=1}^{8}\frac{y_{jt}}{\hat{y}_{jt}}.^{50}$$

The high degree of technical efficiency at the state level and relative to six factors of production is readily apparent from Table 6–2. Three-quarters of the states have measured efficiencies within 10 per cent of the frontier function. The least efficient state (West Virginia) is less than 20 per cent away from the frontier. And if differences in cropping patterns, crop-livestock ratios, and more accurate soil-climate productivity differentials could be allowed for, the measured inefficiencies would be smaller. Even so, most of the measured differences are not related to either the physical or management factors outlined in Chapter 1. As Chapter 7 will show in some detail, at least half the variation in observed tech-

[49] An example would be the function of Equation (3.10).

[50] For those states that contain one of the extreme observations which is dropped, the averaging is only over seven observations rather than eight.

TABLE 6–2.—TECHNICAL EFFICIENCY RATINGS

State	Biased 98% LP		Unbiased 98% LP		Analysis of covariance		Residuals	
	Efficiency rating	Rank	Efficiency rating	Rank	Efficiency rating	Rank	Efficiency rating	Rank
South Dakota	0.991	1	0.9663	11	1.736	17	0.1423	4
Iowa	0.986	2	0.9830	1	1.657	25	0.0936	7
North Dakota	0.984	3	0.9343	40	1.967	4	0.1560	2
Florida	0.978	4	0.9584	20	1.825	14	0.2117	1
North Carolina	0.976	5	0.9734	7	1.967	3	0.1536	3
Delaware	0.970	6	0.9593	19	1.861	11	0.0752	9
Montana	0.965	7	0.9579	22	1.651	26	0.0826	8
Illinois	0.963	8	0.9725	8	1.658	24	0.0362	15
Colorado	0.960	9	0.9736	6	1.607	35	0.0263	18
New Mexico	0.956	10	0.9723	9	1.453	44	0.0545	13
Alabama	0.955	11	0.9538	24	1.694	22	0.0446	14
Kentucky	0.951	12	0.9507	28	1.701	20	0.1000	6
Connecticut	0.948	13	0.9655	13	1.902	5	0.0654	11
California	0.945	14	0.9636	14	1.613	33	0.0707	10
Nebraska	0.945	14	0.9778	3	1.633	27	−0.0270	30
Maine	0.945	14	0.9172	46	2.223	1	0.1110	5
Kansas	0.941	17	0.9783	2	1.605	36	0.0168	22
Wyoming	0.939	18	0.9663	11	1.491	43	−0.0048	25
Georgia	0.939	18	0.9615	16	1.812	15	0.0030	24
Vermont	0.934	20	0.9537	25	1.834	13	0.0216	21
Mississippi	0.932	21	0.9422	35	1.688	23	0.0581	12
Arkansas	0.928	22	0.9535	26	1.631	28	0.0264	17
New Hampshire	0.928	22	0.9409	36	1.893	7	0.0244	20
Massachusetts	0.923	24	0.9394	37	1.976	2	0.0340	16
Minnesota	0.922	25	0.9513	27	1.760	16	−0.0314	33
Texas	0.921	26	0.9583	21	1.444	45	−0.0296	32
New Jersey	0.920	27	0.9463	31	1.870	10	−0.0128	26
Wisconsin	0.920	27	0.9347	39	1.876	9	0.0168	22
Oklahoma	0.920	27	0.9753	4	1.365	48	−0.0403	35
Missouri	0.917	30	0.9614	17	1.579	37	−0.0326	34
Indiana	0.916	31	0.9666	10	1.618	31	−0.0681	38
Idaho	0.913	32	0.9341	41	1.702	19	−0.0237	28
New York	0.909	33	0.9163	47	1.845	12	−0.0258	29
Arizona	0.906	34	0.9619	15	1.625	29	−0.0956	39
Washington	0.903	35	0.9451	33	1.616	32	−0.0472	36
Nevada	0.902	36	0.9564	23	1.360	47	−0.1182	43
South Carolina	0.898	37	0.9298	44	1.895	6	0.0256	19
Oregon	0.896	38	0.9492	30	1.548	40	−0.0568	37
Louisiana	0.890	39	0.9366	38	1.576	38	−0.0282	31
Utah	0.889	40	0.9594	18	1.432	46	−0.1136	41
Rhode Island	0.887	41	0.9210	45	1.891	8	−0.1149	42
Pennsylvania	0.884	42	0.9433	34	1.700	21	−0.1129	40
Maryland	0.883	43	0.9739	5	1.522	42	−0.1632	48
Ohio	0.880	44	0.9494	29	1.570	39	−0.1250	47
Tennessee	0.880	44	0.9304	43	1.608	34	−0.0181	27
Michigan	0.854	46	0.9126	48	1.707	18	−0.1183	44
Virginia	0.848	47	0.9458	32	1.625	30	−0.1232	46
West Virginia	0.810	48	0.9310	42	1.531	41	−0.1187	45

nical efficiency is due to errors of measurement in the variables used for the production function analysis.

This vector of efficiencies, while the best available, is subject to management bias—thus its name. The collinearity of the management factor with several of the "real" factors allows the estimated elasticity of these factors to capture some of management's contribution. The efficiency ratings are thus biased upward to some extent.

Three other measures of technical efficiency are also reported in Table 6–2. The "Unbiased 98% LP" efficiency rating uses Equation 6b as the base. This index is perhaps not very meaningful as a measure of efficiency because another measure of efficiency is introduced as a factor of production. The "Unbiased 98% LP" index is thus a second order measure of efficiency, i.e., it measures how efficient firms are at being efficient. Alternatively, and more likely, it is just a random series.

The "Analysis of Covariance" efficiency vector is simply the firm intercepts of Equation 4. These ratings are not the antilogs—since the rankings are more interesting than the actual values, it seemed unnecessary to make the conversion. Just for comparative purposes, however, antilogs have been taken of the intercepts for the most "efficient" state (Maine) and the least "efficient" state (Oklahoma). The Oklahoma intercept converts to 3.92 and the Maine intercept converts to 9.24, or *2.36 times higher*. Griliches' suspicion that analysis of covariance biases the production function coefficients *downward* seems entirely justified, for it is entirely unreasonable that such large neutral shifts occur in the production functions of different states. What has happened is that the separate state intercepts have captured a substantial proportion of the impact of differential use of inputs, leaving very little for the factor elasticities to explain. As a measure of technical efficiency then, the analysis of covariance estimates is biased strongly downward. The obvious temptation is to *average* the rankings from the "Biased 98% LP" model and the analysis of covariance model to achieve an unbiased set of rankings. That temptation will be firmly resisted because the relative degrees of bias are not known, and the resulting ranking would have no economic meaning whatsoever.

The "Residuals" efficiency vector uses Equation 1, the simplest production function, as a basis for calculations. The process is in two steps: first, the production function is estimated, with the results as shown; and second, the set of 48 dummy variables is regressed on the residuals from the first stage. The ratings shown in Table 6–2 are the coefficients of these dummy variables. The regression on the residuals is highly significant—75 per cent of the variation in the residuals is explained. But what does it mean? To have unbiased estimates of all coefficients in both the first and second regressions requires that the variables in the first be orthogonal to those in the second (*12*, pp. 194–96). The presence of management bias suggests that they are not. The "Residual" efficiency ratings are thus biased estimates of efficiency in the same fashion as the "Biased 98% LP" estimates, because some proportion of the "real" contribution of efficiency has been captured by the factors of production in the first step regression. The extent of this bias can be indicated by comparing the "Analysis of Covariance" index of efficiency with that of the "Residual" index. Casual comparison reveals little relationship, and

TABLE 6–3.—CORRELATION MATRIX OF EFFICIENCY INDEXES

	Biased 98% LP	Unbiased 98% LP	Analysis of covariance	Residual
Biased 98% LP	1.00			
Unbiased 98% LP	0.50	1.00		
Analysis of covariance	0.31	−0.45	1.00	
Residual	0.89	0.15	0.45	1.00

in fact the simple correlation coefficient (r) is only 0.45. Management bias is a powerful force.

Table 6–3 shows the simple correlation matrix for all four indexes of efficiency. The correlation between the "Residual" index and the "Biased 98% LP" index is one further manifestation of the similar nature of frontier and average production functions and the relative neutrality of the shift from average to frontier. But none of the other correlations is significant. At least at the state level of aggregation the relative degree of technical efficiency achieved is highly sensitive to how efficiency is defined and how the production function is specified and estimated.

CHAPTER 7. EXPLAINING TECHNICAL EFFICIENCY

This chapter has about it some of the aura of a snipe hunt. The ratings of technical efficiency generated by different types of production functions turned out to be extremely sensitive to small changes in specification and conceptual approach. A low degree of confidence should be placed on all the ratings then, and the search for "explanations" of a firm's relative position in the ratings is necessarily elusive. It seems justifiable to argue that the shortcomings are not so much in the methodology as in the data. Presumably because of the large size of the sample, the data set withstood a searching round of production function estimations with remarkable success. But because the data are state aggregates, the estimates of technical efficiency contain little or no impact of differences in age, educational level of farm operators, or other potential managerial factors. Estimation of an ad hoc "supply function" for management is thus ruled out.

It is still important to ask what has happened. This implicitly asks what efficiency in agriculture means when measured relative to six factors of production, among which are fertilizer, capital, and a variable (seed and miscellaneous) that may serve as a proxy for other modern inputs. "Good" farmers use these inputs in great quantities and achieve large outputs with them—but the production function makes this distinction. No neutral shifts are necessary to account for "good" farmers' performance when these factors of production are included.

What has happened, then, is that differences in *static technical efficiency* largely disappear when measured at the state level. The result should not be too surprising. The residual of *dynamic technical change* also disappears under similar circumstances—when inputs are measured "properly" and all relevant factors, including intermediate ones, are included in the production function (*16*, pp. 331–46). It is disappointing that the residual of technical efficiency disappears before rather than after the introduction of the research and educational differences that Griliches finds important, but this may be a data problem.

The sets of technical efficiency generated in Chapter 6 may be sensitive to slight changes in specification and subject to a wide range of uncertainty, but they are not totally without useful information. The information turns out to be less relevant to understanding the management function in agriculture than to casting some light on the misspecification of several input variables and possibly the output variable as well.

Table 7–1 lists the factors that were thought might have some power to explain variations in the technical efficiency ratings among states. All are state specific.

Only the "Analysis of Covariance" ($X2$) and "Biased 98% LP" ($X3$) indexes of efficiency were investigated thoroughly. The "Unbiased 98% LP" index is of dubious meaning and the "Residual" index is of relatively little economic significance.

The original intent was to be able to estimate a function of the form $X2$ or $X3 = \phi\ (X6, \ldots, X39)$. It was hoped, especially, that all the age of operators variables ($X6$–$X11$) and education of operators variables ($X12$–$X18$) could be introduced explicitly rather than by a single weighted "age" or "education" variable. Griliches (and all subsequent workers in the field) used just such a weighted education variable, with the weights 1950 mean income of U.S. males,

TABLE 7-1.—EXPLANATORY FACTORS

Identifying number	Variable	Identifying number	Variable
	Age of operators (proportions)	X22	Number of non-white farm operators
X6	Less than 25	X23	Gross farm income
X7	25–34	X24	Proportion of livestock in output
X8	35–44	X25	Soil Bank payments, 1964
X9	45–54	X26	Proportion of land irrigated
X10	55–64	X27	Population density of state
X11	65 and over		Agricultural research expenditures[a]
	Education of operators (proportions)	X28	1935
	Elementary	X29	1940
X12	0–4 years	X30	1945
X13	5–7 years	X31	1950
X14	8 years	X32	1955
	High school	X33	1960
X15	1–3 years		Agricultural extension expenditures[a]
X16	4 years	X34	1935
	College	X35	1940
X17	1–3 years	X36	1945
X18	4 or more	X37	1950
X19	Number of tenants of all types	X38	1955
X20	Number of days worked off the farm	X39	1960
X21	Number of Negro farm operators		

[a] Data taken from *27*.

TABLE 7–2.—AGE PRODUCTIVITY WEIGHTS

Proportion of farm operators in age group	Simple correlation coefficients with X3 (biased 98% LP)
Less than 25	0.40
25–34	0.44
35–44	0.33
45–54	0.19
55–64	−0.27
65 and over	−0.46

25 years old and over, by school years completed (*16*, pp. 331–46). These weights were reported by Houthakker (*22*, p. 342).

There is no strong a priori reason why these income weights for the entire U.S. male labor force are the proper productivity weights for agricultural workers. Griliches has provided an empirical justification, of course, but a more accurate set of weights might give even better results. If aggregation had not washed out the influence of age or education on agricultural output the large sample (48 observations) could be used to *estimate* productivity weights directly. Thus the proportion of farm operators in each age or educational class could be introduced as a separate variable and the resulting coefficients used to form a more meaningful single age or education variable, if that were needed. A similar procedure might be used for research and extension expenditures over time.

The nature of the estimates of technical efficiency has, of course, largely thwarted that approach. A slight hint of the potential results, however, is shown in Table 7–2.

The distribution of productivity weights, with the younger farmers having relatively high productivity and the older farmers exhibiting declining productivity, is at least plausible. No further claims are made, but at least the method seems worth pursuing with a better data set.

The overall intent of explaining the variation in efficiency need not be entirely abandoned either. Equation (7.1) reports the most successful attempt (*t*-values in parentheses).

$$\begin{aligned} (7.1)\qquad X3 = \underset{(9.2)}{0.8377} + \underset{(1.3)}{0.4386\,X9} + \underset{(2.4)}{0.1329\,X19} - \underset{(-4.1)}{0.6764\,X20} \\ - \underset{(-0.9)}{0.0468\,X21} + \underset{(1.7)}{83.2992\,X25} \end{aligned}$$

$R^2 = 0.48$

t-value	Significance (*Per cent*)
0.68	50
1.68	90
2.02	95
2.42	98
2.70	99

Almost half the variation in the "Biased 98% LP" efficiency index can be "explained." The explanatory factors are not those normally associated with differences in efficiency, but the possible reasons for their significance are interesting nonetheless.

The easiest coefficient to make sense of, and the most significant, is *X*20, the number of days worked off the farm by the farm operator. More days worked off the farm mean lower efficiency. A straightforward interpretation of this would suggest that a farmer lacks the time to look after the important details of farm management as he spends more time at work off the farm. This is perfectly plausible and possibly accounts for some of the significance of this variable. But of greater significance is the likelihood that this variable is correcting some bias introduced by the labor variable. The labor input was a crude measure of man-days worked in agriculture, but correction for time worked by the number of actual laborers present was only on an annual basis for the entire country. There was no state specific weighting for proportion of time spent in farm work. Variable *X*20 makes this correction at the efficiency stage rather than where it should more properly be made—i.e., at the stage of input variable construction.

Next in significance is the relative number of tenants of all types who are farm operators (*X*19). The original motivation for including this variable was to test whether lack of ownership has an adverse effect on efficiency, as is frequently argued, especially for underdeveloped agricultures.[51] Thus the estimated coefficient was expected to be negative. The fact that it is significantly positive might be taken as just another example of the capriciousness of the efficiency index. But the nature of the data suggests another explanation.

The original expectation of a negative response was especially conditioned by the nature of the sharecropping tenure pattern in the South. This form of tenancy, however, does not dominate tenant farming for the United States as a whole. The proportion of farm operators who are tenants of one form or another averages 0.21 for nine Southern states (Virginia, North Carolina, Tennessee, South Carolina, Georgia, Alabama, Mississippi, Arkansas, and Louisiana) and 0.25 for six Corn Belt states (Indiana, Illinois, Iowa, South Dakota, Nebraska, and Kansas). While the Southern states might be suspected of low efficiency, the Corn Belt states generally are not. Some other factor must be at work.

The most likely expectation seems to be that, at least in the non-Southern states, tenants tend to be young farmers who have not saved enough to buy their own farms. They tend to work exceptionally long, hard hours in these early years, a factor not accounted for in the labor variable. Thus *X*19 might be correcting for this extra motivation.

Variable *X*25, 1964 Soil Bank payments, is significant at only the 90 per cent confidence level, but the potential reasons are quite interesting. Two somewhat different forces seem to be at work.

First, the output variable used in all the production function estimations included a component for government payments. In general, these payments are on an output basis, i.e., the more cotton produced the higher the payments. It makes sense to include them under such circumstances. But some payments, pri-

[51] For a review of the arguments, see *40*, pp. 267–314.

TABLE 7–3.—SOIL BANK PAYMENTS AND TECHNICAL EFFICIENCY*

State	Soil Bank payments as % of gross output	Biased 98% LP efficiency rank
North Dakota	2.8	3
New Mexico	2.4	10
South Carolina	1.8	37
South Dakota	1.7	1
Oklahoma	1.5	27
Colorado	1.1	9
Georgia	1.1	18

* Data are from *45*.

marily those under the Soil Bank Program, are made for *not* producing.[52] Those states where the Soil Bank payments form a significant proportion of measured output should then show a high degree of technical efficiency. They are able to "produce" output without inputs.

Seven states received Soil Bank payments that were 1 per cent or more of their gross output in 1964. Table 7–3 shows these seven states, their Soil Bank payments as a percentage of gross output, and their efficiency ranking according to the "Biased 98% LP" index (*X*3).

Four of the seven states are ranked in the top 10 in efficiency. If nothing else, part of the mystery of North and South Dakota's high efficiency ratings seems to be resolved.

The second aspect of the significance of the Soil Bank variable in explaining the efficiency ratings has to do, perhaps, with the tendency of participating farmers to take out of production their worst yielding land and to farm the remainder more intensively. The production function should have no difficulty picking up the intensity of farming so long as it involves extra fertilizer, etc., that is part of the function. But the change to higher quality land will not be picked up by the land variable used in the production functions. Once again the form of a variable seems responsible for variation in "efficiency" rather than real differences in farm managers' performances.

Variable *X*9, the proportion of farm operators 45 to 54 years old, undoubtedly serves as a proxy for something else. It is significant at only the 80 per cent confidence level, which for the type of data being examined here is only high enough to be suggestive. A number of plausible interpretations of the positive coefficient for *X*9 come to mind. The most direct is simply that the productivity of "age" reaches its peak in this age group. There is a trade-off between the physical effort a young man can exert (and the better education he tends to have) and the experience that an older farmer draws upon in his decision-making. Farming is not yet a regimented science; it retains a craftlike aspect that gives scope to the wisdom of years of working the soil. The significance of *X*9 may reflect that the age class of 45 to 54 years is the optimum point in this trade-off.

Another possibility is that a farm operator in this age class is very likely to have

[52] Most payments require some restrictions on output to *qualify* for subsequent payments on the basis of amount produced, but the Soil Bank Program pays for not producing at all.

TABLE 7-4.—CORRELATION OF PROPORTION OF NEGRO FARM OPERATORS WITH OTHER VARIABLES

	Variable name	Simple correlation
X7	Age class 25–34	−0.37
X8	Age class 35–44	−0.49
X10	Age class 55–64	0.50
X11	Age class 65 and over	0.31
X12	0–4 years' elementary education	0.75
X13	5–7 years' elementary education	0.69
X16	4 years of high school	−0.61
X17	1–3 years of college	−0.34
X18	4 or more years of college	−0.18
X19	Number of tenants	0.47

one or more mature sons who may also be working on the farm. There will be two (or more) managers then, one with the wisdom of experience and one with the vigor and modern education of youth. It should not be surprising that such an operation might be more efficient than those run single-handedly.[53]

The last variable that yields even a suggestion of meaning is *X*21, the proportion of farm operators who are Negro. Its significance is only between 60 per cent and 70 per cent. The estimated coefficient is negative, i.e., the higher the proportion of Negro farm operators, the lower the efficiency. Before a judgment of "ability" is read into the negative coefficient for *X*21 it would be well to consider with what other factors *X*21 might be correlated. Table 7-4 shows several simple correlation coefficients.

The population of Negro farm operators is not evenly distributed with respect to age and education. There tend to be few young Negro farm operators and many over age 55, when productivity seems to be rapidly diminishing. They tend to have less than seven years of schooling, with a decided lack of completed high school and college experience. And Negro farm operators tend to be tenants. There seems to be a fairly strong case for identifying the observed lower efficiency of Negro farm operators (of questionable significance at that), not with "ability," but with "opportunity."

Equation (7.1) is not exactly a "management" production function. It is not suitable for removing the effect of the physical environment by the iterative procedure outlined at the end of Chapter 4. That procedure cannot be tested here. And yet the attempt to explain the technical efficiency indexes presented in Chapter 6 has been revealing. Even what little variation that was left for differences in "efficiency" to explain now seems to be due largely to biases introduced by definitional problems in the data rather than by any meaningful determinants of managerial performance. The conclusion stated at the beginning of this chapter, that static cross-sectional differences in technical efficiency disappear if the production function is properly specified, seems all the stronger for these results. The

[53] It is obviously "efficient" only so long as the management input is excluded from the production function. It seems unlikely that two managers on one farm is an efficient use of managerial resources.

argument that Griliches made for dynamic technical change at the state level is thus extended to static differences as well.

Measured Technical Efficiency and Economic Welfare

Judging from the small degree of technical inefficiency observed at the state level, the welfare losses from this particular failure of the decision-making process seem to be quite small. The average state is only 7.6 per cent from the frontier in the "Biased 98% LP" model, and at least half of that distance must be attributed to measurement errors rather than productive inefficiencies. An average loss of about 3 to 4 per cent seems to be the most that is likely—an amount easily subsumed in a year's growth, if such growth were desired in U.S. agriculture.

Are all states, then, equally good producers of agricultural output? Are there really no welfare losses due to poor decision-making in U.S. agriculture? The answer to both questions is no. First, by measuring technical efficiency at the state level all differences among farmers *within* the states were aggregated away. It is entirely possible for substantial productive inefficiencies to exist within states with few observed differences between states. The empirical results reported here can make no judgment at all on that score.

Second, the significance of allocative inefficiencies should not be ignored. To discover the exact extent of such inefficiencies would require a state-by-state analysis, but some rough indications can be drawn short of that. It is fairly safe to assume that all states face similar factor costs and prices for output because to a large extent the U.S. agricultural market is national in scope. Land is fixed and family labor seems to be relatively immobile, so the assumption does not necessarily hold true for these two factors. But with the other inputs costing all farmers about the same, and all farmers receiving about the same prices for their output, output levels per unit of the relatively fixed inputs should be approximately the same. Some differentials will exist due to land fertility, labor skill differences, and differing composition of output, but differentials of over 100 per cent in output per acre or perhaps 50 per cent in output per man-day would seem to indicate substantial allocative inefficiencies.

Table 7–5 shows a small cross section of states and compares their land and labor productivities. West Virginia's gross output per acre is only 30 per cent of California's; its gross output per man-day is just one-fifth as large. The suspicion is very strong that West Virginia does not use a profit-maximizing quantity of purchased inputs and is severely inefficient in its allocative decision-making.

TABLE 7–5.—ILLUSTRATIVE LAND AND LABOR PRODUCTIVITIES, 1964*
(*Dollars*)

State	Gross output per acre	Gross output per man-day
California	102.5	89.9
Iowa	97.2	89.1
Georgia	87.6	48.2
West Virginia	30.7	18.4

* Drawn from original data used for production function estimation.

So what? The Harberger diagram (Chart 1-1 in Chapter 1) demonstrates the inconsequential nature of allocative inefficiencies, even if they are fairly severe. Why draw special attention to the allocative inefficiencies suspected in U.S. agriculture? The answer is that the Harberger diagram assumes full employment of resources throughout the economy to make its point. This is a bad assumption for many of the resources used in agriculture. Land and most buildings are immobile. They cannot move if they are not being utilized to full potential. Farm labor can move, but for a variety of economic and social reasons the self-employed and family component of it tends not to, even in the face of substantial underemployment. Most intermediate inputs probably find alternative uses, either elsewhere in agriculture or in the industrial sector, so poor allocation of these resources probably does lead to marginal losses only. But the losses to the more fixed factors are total and not just marginal because they are not employed elsewhere in the economy. The welfare costs of this type of allocative inefficiency are very great, particularly because the human resource is one that tends to be wasted.

The production functions estimated in Chapter 6 were meant as a means to an end, the calculation of technical efficiency, and the discussion so far has concentrated on that aspect. A number of other interesting issues can also be examined within the production function framework. Two of them, marginal returns to factors of production and economies of scale, reveal significant insights into the structure and performance of U.S. agriculture.

Marginal Returns

The normal model of firm behavior in a competitive environment requires that each firm equate the marginal revenue product of each factor of production (*MRP*) to its marginal cost (*MC*), so that the ratio of *MRP* to *MC* equals one. The Cobb-Douglas production function is very convenient for calculating this ratio, especially when the variables are measured in value flows. In such a situation

$$\frac{MRP_{ij}}{MC_{ij}} = \hat{\alpha}_i \frac{TR_j}{X_{ij}} = 1 \qquad (8.1)$$

if the j^{th} firm is profit maximizing in a competitive world (TR_j = total revenue of the j^{th} firm, X_{ij} = value of the flow input of variable X_i by the j^{th} firm, and $\hat{\alpha}_i$ = estimated coefficient of factor i in the Cobb-Douglas function). Equation (8.1) shows that the calculation can be done for each firm separately,[64] but the normal procedure is to examine the marginal returns for the "average" firm, so that TR_j becomes $\overline{TR}$ and X_{ij} becomes $\overline{X}_i$. Table 8–1 shows the results of calculating $\hat{\alpha}_i \frac{\overline{TR}}{\overline{X}_i}$ for both the "biased" and "unbiased" average production functions reported in Chapter 6 (Equations 1 and 4 in Table 6–1).

TABLE 8–1.—RATIO OF MARGINAL REVENUE PRODUCT TO MARGINAL COST

Factor of production	"Biased" equation: Point estimate	"Biased" equation: 99% confidence interval	"Unbiased" equation: Point estimate	"Unbiased" equation: 99% confidence interval
Labor	1.17	1.10–1.24	0.75	0.63–0.87
Capital	3.76	3.68–3.84	—	—
Land	0.29	0.26–0.32	2.14	1.93–2.35
Fertilizer	4.86	4.84–1.08	1.57	1.51–1.63
Livestock	1.05	1.02–1.08	1.30	1.20–1.40
Miscellaneous	1.62	1.54–1.70	1.25	1.14–1.36

[64] This is the calculation that would have to be performed for each state if the welfare discussion of Chapter 7 were to be pursued in greater detail. The presence of either management bias or analysis of covariance bias in the estimated production functions suggests that the results would still be mostly qualitative rather than concretely quantitative. The general conclusions in Chapter 7 as to the size and nature of the welfare losses due to allocative inefficiency might be firmed up somewhat, but would not be substantially changed.

The capital, land, livestock, and miscellaneous variables are all measured in value flows per year, so the calculations are perfectly straightforward. The labor variable is measured in man-days of work, so a daily wage rate is needed to convert this to a value flow. The average wage rate for agricultural workers for the 1960–67 period for the entire United States was used. The fertilizer variable is almost a value flow variable because the weights used to form a total fertilizer input from the N, P_2O_5, and K_2O quantity inputs were price weights. However, the constant term of the price weight equation was left out, so it is necessary to add this back in to calculate the value of fertilizer use.

The results are not very surprising in view of the continuing transformation taking place in U.S. agriculture. The labor return is slightly greater than one if the biased coefficient is used, but significantly less than one if the unbiased coefficient is used. The unbiased production function is supposed to have more desirable statistical and economic properties than the biased function. Thus the marginal returns estimated from the unbiased function should also be more meaningful. The implications are that farmers are using too much labor at the going wage rate, and the exodus of farm labor to the cities confirms that a disequilibrium exists. It is also possible to turn this argument on its head and argue that the exodus of farm labor suggests returns to labor are low. Thus the unbiased coefficients make more sense than the biased ones, confirming empirically what was argued theoretically in Chapter 3.

The returns to capital are very high in the biased model, but are zero (because capital drops out of the production function) in the unbiased model. The zero return is presumably an artifice—the data were not good enough to sort out the multicollinearity between the separate firm intercepts and the capital variable. The downward bias introduced by analysis of covariance is probably also at fault here. An unbiased estimate of capital return is thus impossible to calculate, although it probably lies between zero and 3.76.

The land coefficient changes radically when management bias is removed from the production function. The ratio of MRP to MC is 0.29 in the biased case —so low that it is not believable. And in the unbiased model the ratio is 2.14, which seems very high for a factor of production whose magnitude of use is declining. Government restrictions have caused most of the decrease in land use, however, and the effect has apparently been to raise land's marginal revenue product well above its marginal cost.

The disequilibrium in the use of fertilizer has been mentioned already with reference to Griliches' findings. The marginal returns calculated from the biased production function are nearly identical to those reported by Griliches, and the reduction of the ratio from about 5 to about 1.5 when the unbiased function is used is reassuring. Farmers may be slow to adjust, but they are not *that* slow.

The livestock variable comes closest to a unitary ratio although the value of 1.30 for the unbiased estimate is significantly greater than one. The extent of any disequilibrium seems to be small, however. This also is true of the seed and miscellaneous variable. A fairly substantial disequilibrium seems to exist in the biased model, but most of this disappears when the bias is removed.

In summary, the only major disequilibria in which there is any confidence is

TABLE 8–2.—ECONOMIES OF SCALE

	Equation number and form		Value of e
1	Biased OLS	Average	1.168
4	Unbiased OLS	Average	0.948
5b	Biased 98% LP	Frontier	1.299
6b	Unbiased 98% LP	Frontier	0.936

that the average farmer uses too much labor, not enough fertilizer and land, and possibly not enough livestock and the components of the seed and miscellaneous variable.[55] The fact that the *MRP*-to-*MC* ratio is uniformly greater than one, except for one or two factors, might reflect constraints on decision-making not normally considered in the usual free competition model. Hoch found that the ratio *should* exceed one if a farmer faced an output restriction due, perhaps, to government intervention, capital rationing, or risk aversion (*21*, p. 36). Under one or more of these circumstances (all of which were probably important factors in the 1960–67 period) the ratios of *MRP* to *MC* for livestock and seed and miscellaneous are likely to be about "right."

Economies of Scale

As George Judge has pointed out, "when productivity coefficients are estimated, the urge to add them is a strong one" (*26*, p. 432). The rationale seems strong, too: the sum of the individual factor elasticities in a Cobb-Douglas production function is the elasticity of output, frequently labeled "e." If $e < 1$ then a 10 per cent increase in all inputs increases output by less than 10 per cent; if $e > 1$ a 10 per cent increase in inputs yields more than 10 per cent more output. If there is no pause to consider what aspect of the productive process might cause e to be less than or greater than one, then the temptation to look for economies of scale in the production functions of Chapter 6 is irresistible. At least it will not be resisted here.

Table 8–2 shows the value of e for the four most important equations in Table 6–1.

The sum of the biased OLS coefficients is nearly identical to the value reported by Griliches for a similar equation (*15*, p. 966). It is significantly greater than one, and seems to imply substantial economies of scale. The biased frontier function indicates even greater scale economies—the value of e is 1.299 (but there are no tests of significance for this).

The results change drastically, however, when management bias is removed. The sum of the coefficients should be biased upward if each of the factors of production is used more intensively by "good" farmers. If some factors are used less intensively then the bias can be in either direction, although there are good reasons for suspecting that the upward bias will tend to outweigh the downward bias. This has happened here. The elimination of management bias reduces the value of e to slightly less than one in both the average and the frontier functions. This

[55] Farmers use too little land only if government constraints on land allocation are ignored.

conclusion is conditioned by the presence of analysis of covariance bias. If the net effect of this bias is negative, as suspected, then the analysis of covariance estimate of *e* is biased downward.

Good farmers use more of most inputs, so the overall impact of the collinearity between management and input use is positive. In the management biased production function doubling the measured inputs also raises the management input as well, although not by twice as much. Thus the management factor is *not* held constant in the biased function, and the finding of substantial economies of scale is subject to the proviso that the management factor be allowed to increase as the scale of operations increases. The implications for the average farmer are that he will have to perform more managerial functions if he expands his farm to benefit from the scale economies. If his managerial abilities are underemployed on his present farm, this may be possible with little cost; but if the farmer is already managing to capacity, other managerial talent will be required in order to capture the scale economies. If the managerial input is held constant the unbiased equations suggest that expanding the scale of operations is subject to slightly decreasing or constant returns.

Nearly constant returns to scale holding one factor of production fixed is a most surprising finding. It suggests that the factor is of minor importance in the productive process, at least over the observed range of the sample. But this is true only if constant returns to scale are expected when *all* factors are increased proportionately, and this seems not to be true, judging from the elasticities of output reported in Table 8–2. Thus the management factor is important because it is needed to make the substantial economies of scale possible.

A major criticism of estimating a Cobb-Douglas production function and summing the coefficients to determine the elasticity of output is that this elasticity of output is constant for the entire range of the function. Thus the smallest firms have the same value of *e* as the largest. This contradicts the implications of U- or L-shaped cost curves, which are well established in the theoretical and empirical literature. The L-shaped cost curve implies *increasing* returns to scale for small firms up to a point, and then *constant* returns thereafter. The U-shaped curve implies increasing returns, possibly a range of constant returns, and then decreasing returns as scale increases. The empirical finding of *increasing* returns over the entire scale of operations means the average cost curve must be forever decreasing, a situation that violates conditions necessary for reaching a stable equilibrium in a competitive economy. The finding should thus be viewed with some skepticism.

The constant elasticity of output over the entire range of the Cobb-Douglas function can be circumvented by breaking the data into a number of sets and estimating separate functions. Thus it is possible to calculate *e* for small and large firms separately. A difficulty with this procedure is that biased estimates of the coefficients result if the sample is split according to a criterion which depends on the dependent variable. The criterion used here for splitting the sample is USDA-reported gross output (undeflated) per farm for each state in 1964. This is obviously related to the dependent variable in the production function analysis—deflated gross output per farm from 1960 to 1967. There is some variation between the dependent variable and the splitting criterion, so the bias is not as strong as

TABLE 8–3.—COBB-DOUGLAS PRODUCTION FUNCTIONS WITH SPLIT SAMPLE

Equation	Technique	Coefficients (*t*-values) C	L	K	D	F	V	M	R^2	*e*
		SMALL FARMS (*Observations 1–192*)								
1′	OLS	1.7692	0.1726	0.4319	0.0143	0.1541	0.1982	0.1575	0.943	0.956
		(32.0)	(3.5)	(10.1)	(0.8)	(11.3)	(10.0)	(3.4)		
4′	OLS	$a_t a_j$	0.1313	−0.1887	0.2027	0.0061	0.2611	0.4283	0.993	1.030
			(2.0)	(−1.5)	(1.8)	(0.2)	(4.5)	(5.6)		(0.841)
5a′	LP_{100}	1.9753	0.2023	0.4791		0.1201	0.1208	0.1551		1.077
5c′	LP_{97}	1.9753	0.1891	0.4778		0.1252	0.1127	0.1611		1.066
		LARGE FARMS (*Observations 193–384*)								
1″	OLS	1.8430	0.1412	0.1581	0.0835	0.1344	0.2614	0.2118	0.943	0.990
		(42.7)	(3.5)	(2.8)	(5.7)	(10.6)	(13.6)	(5.5)		
4″	OLS	$a_t a_j$	0.1632	0.0165	0.4780	0.1059	0.3275	−0.0188	0.985	1.091
			(2.3)	(0.1)	(3.6)	(2.9)	(5.6)	(−0.3)		(1.072)
5a″	LP_{100}	1.9743	0.3558	0.1866	0.0175	0.1252	0.2282	0.1482		1.061
5c″	LP_{97}	2.0586	0.2078	0.2770		0.1438	0.1750	0.2567		1.060

C = constant
L = labor
K = capital
D = land
F = fertilizer
V = livestock
M = seed and miscellaneous

it might be. Still, the following results should be cautiously interpreted. Table 8–3 reports the estimated coefficients of the production functions for small and large farms separately.

There is no evidence of economies of scale when the sample is split into large and small firms, even for "biased" functions. The value of *e* is not significantly different from one for any of the eight functions reported in Table 8–3. (The value of *e* for Equation 4′ is less than one, but only if the negative capital coefficient is added in, which makes little sense. Ideally, capital should be dropped from the function and the regression estimated again.) Both the biased and unbiased values of *e*, for both the large and small firm size samples, are almost identically one.[56]

Where did the economies of scale go? Despite the insignificance of several of the coefficients in Table 8–3 and the bias resulting from splitting the sample, the answer seems relatively clear. There are two separate production functions for U.S. agriculture—one for small farms and one for large. This confirms the finding about the structure of management bias reported in Chapter 6. There the conclusion was that no "good" managers were to be found operating small farms, and the data set was effectively partitioned into large and small farms with a separate function for each. Comparing Equations 1′ and 1″ from Table 8–3 effectively proves the point. The elasticity of output is unity for both. The factor elasticities are nearly identical, except for capital. The intercept is higher for the large farms than for the small. Large farms are basically neutral multiplications of small farms, except that they use relatively more capital. The result is a higher overall intercept but a lower elasticity of output for capital.[57] Thus "small" farms can presumably become "large" farms—with their resulting higher productivity—only by a massive injection of capital.

The significant economies of scale reported in Table 8–2, then, were due to fitting a single production surface to two discrete surfaces.[58] The larger sized farms, although producing with constant returns to scale, are more "productive" than the smaller farms, which also have constant returns to scale. The overall function thus reports the higher productivity of the large farms relative to the small farms as economies of scale, when in fact the difference is due to a different production function being used by large farms. The overall effect is the same, but the implications for an individual farmer are not. As long as a farmer remains small he experiences constant returns, and once he has become large he experiences constant returns. The only increasing returns occur in the transfer from one production function to the other.

Breaking the sample into large and small farms yields some other interesting results. Once again the frontier seems to be a neutrally shifted average function. This is true of even the LP_{100} estimate for the small farm sample, suggesting that the errors of observation are concentrated in the large farm sample.

[56] It is worth noting that "analysis of covariance bias" does *not* show up when the sample is split in two. That is, the value of *e* is not lower for analysis of covariance estimates than for ordinary least squares estimates. The impact of any *bias* due to the technique of analysis of covariance in the whole sample seems, therefore, not to be of substantial magnitude.

[57] This is due, presumably, to the setting in of diminishing returns.

[58] It should be noted that more than two surfaces may possibly exist. The analysis so far has shown that *at least* two exist.

The removal of management bias by analysis of covariance once again changes the estimated coefficients significantly. In the small farm sample the results are nearly identical to what happened for the overall sample. The capital and fertilizer coefficients drop drastically (to insignificance in this case) and the land coefficient increases. These are familiar results. But, in addition, the seed and miscellaneous coefficient increases almost threefold and increases in significance. Evidently in the small farm sample use of seed and other miscellaneous factors is negatively correlated with management. The most likely reason is that this variable, at low levels, is mostly a catchall for nonproductive expenditures. These are relatively important for poor managers on small farms, but become less important, relatively, for better managers on larger farms (although still in the small farm classification).

Management bias in the large farm sample has somewhat different effects. The capital coefficient drops as before, but the fertilizer coefficient drops only slightly. On large farms the use of capital continues to be collinear with management, but fertilizer no longer is. By the time a farmer has reached "large" scale he uses the correct amount of fertilizer no matter what the level of management. But now the seed and miscellaneous variable becomes highly collinear with management—the coefficient drops to (less than) zero when management bias is removed. This variable thus seems to play an entirely different role in the large and small farm samples. It was a catchall for nonproductive expenses on small farms, but on large farms the "modern" aspects of the variable, such as herbicides and pesticides, seem to take over. Then it becomes strongly collinear with management.

CHAPTER 9. SUMMARY AND CONCLUSIONS

Economics is in many ways a science of efficiency. The decision-making rules postulated in most economic models require some sort of optimizing on the part of the participants, be they consumers or producers. And optimum means efficient. Thus a Pareto optimum is the highest degree of allocative efficiency. Underlying allocative efficiency are several other types of efficiency that, because they are building blocks for the rest of the economic structure, are as important or more so than allocative efficiency in determining how much society gets for its scarce resources. Chief among these is technical or productive efficiency, roughly defined as the extent to which the greatest possible output is achieved from any given combination of inputs. It is a problem almost completely internal to the firm, and for this reason technical efficiency has only recently entered the realm of economics. Previously it was considered solely an engineering or management problem not subject to economic analysis.

Two factors are primarily responsible for the new interest of economists in technical efficiency. First, linear programming techniques revealed a great deal of similarity between allocation of resources among firms and allocation within the firm. Second, in the 1950s economic growth became a major field of study for economists. The determinants of growth are closely related to those factors that cause a firm to use the "best" practices in an industry rather than "average" practices. The distinction between best and average is partly a matter of technical efficiency.

The integration of technical efficiency into economics has been largely ad hoc, with little attempt to place the concept in the context of economic theory. Following the lead of a handful of workers, primarily Farrell, Nerlove, Bressler, and Aigner and Chu, this study makes that attempt. A measure of technical efficiency is constructed that is consistent with accepted microeconomic theory of the firm, and the measure is used in an empirical application to U.S. agriculture.

The innovation introduced by Farrell, that technical efficiency be measured relative to a frontier production function, is the basis for the model used here. The Farrell technique of constructing a frontier unit isoquant has a great many virtues in a two- or three-factor world because no artificially constraining functional forms need to be imposed on the data. But in a more-than-three–factor world the technique, while still yielding satisfactory estimates of technical efficiency, is very cumbersome as a means of examining what the production surface looks like. The empirical example used here has six factors of production, so the fitting of a functional form to the frontier was more convenient and revealing. A side benefit was the ease with which the frontier function could be compared to the average function. Thus the measure of technical efficiency ultimately used here relates the actual output of a firm to the output achievable with that firm's inputs if the frontier production function were used.

The data set used in the empirical part of this monograph is an 8×48 matrix, where each of the 48 contiguous states is considered a "farm firm" and the observations are over the eight-year interval 1960–67. The suspected presence of management bias in any sample of firm input and output data meant a time series of cross-section observations was necessary to construct an unbiased measure of technical efficiency. There are few such data sets available, and the one used here is not suitable for testing the full implications of the theoretical model developed earlier. The sample size (384 observations) was large enough to yield good estimates of the production functions, but a secondary goal, relating the resulting technical efficiency ratings to characteristics of the firm and firm operator, was largely thwarted. The reasons turned out to be twofold: (1) the six-factor production function was capable of explaining most of the variation in output so that little remained for technical inefficiencies to explain, and (2) the level of aggregation of the data washed out most firm-level differences in managerial effectiveness.

The removal of management bias from the production functions through the use of analysis of covariance turned out to have important consequences. The strong positive collinearity of management with several factors of production, especially capital and fertilizer, caused the estimated coefficients of these factors to drop sharply when management bias was removed, although analysis of covariance bias may also have been partly responsible. Capital dropped completely out of the production function, probably because of the quality of the data. The elasticity of output for fertilizer dropped to a much more reasonable value when compared with the cost of fertilizer. The ratio of marginal revenue product to marginal cost dropped from about 5 to about 1.5. The latter value is still high enough to indicate substantial disequilibrium with respect to fertilizer use and thus to account for the continuing rapid growth in consumption, but a value of 1.5 is not so high as to throw suspicion on the rationality of American farmers.

The frontier production functions bore little relationship to the average functions when determined by the entire data set. But when 2 per cent of the "extreme" observations were removed, the frontier production functions turned out to be almost neutral transformations of the average production functions, i.e., the factor coefficients were almost identical, but the overall intercept was higher for the frontier. When a simpleminded attempt was made to remove management bias from the frontier function a similar result occurred.

The presence of management bias in the frontier function was disturbing, for the implications were that two separate production functions existed rather than one. This was confirmed when the sample was divided into large and small farms and the functions reestimated. The small farm production function was similar to the large farm function except for a higher elasticity of output for capital and a lower overall intercept. More importantly, both the small and large farm functions exhibited constant returns to scale, in marked contrast to the significant increasing returns found when the function was estimated for all farms together. The source of the scale economies was clearly the shift between the small and large functions (which seem only to be brought about with a large capital investment) and not continuous scale economies over the length of the function. The bias introduced by splitting the sample and reestimating the functions is not likely to be strong enough to invalidate completely these findings.

The empirical findings about the importance of technical inefficiencies at the state level were interesting although mostly negative. A simple six-factor average production function explained 97 per cent of the observed variation in output. The average state was only 7.6 per cent away from the technical frontier, and at least half of this distance was accounted for by measurement errors and definitional problems. This was taken as evidence that static, cross-sectional differences in technical efficiency at the state level disappear when the production function contains all relevant input factors, particularly such "modern" factors as fertilizer, hybrid seeds, and pesticides.

Thus Griliches' finding that the residual of technological change in U.S. agriculture disappeared when the production function and inputs were properly specified is extended to static cross-section differences in technical efficiency as well. This is, perhaps, not a surprising result for a competitive sector of the economy that faces a national market. Whether a similar situation existed 30, 50, or 100 years ago is a question worthy of further research. The answer would reveal a great deal about the regional nature of agricultural development and the evolution of the present structure of U.S. agriculture.

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THE FOOD RESEARCH INSTITUTE DEDICATES A NEW BUILDING

On April 6, 1970, the Food Research Institute and Stanford University celebrated the dedication of a newly reconstructed building on the Stanford campus for use by scholars of the Institute. The occasion was marked by three formal addresses and a short dedication ceremony conducted on the building terrace by Stanford Board of Trustees Chairman W. Parmer Fuller III and University President Kenneth S. Pitzer. Invited guests were entertained at the Institute for luncheon and the Stanford Faculty Club at dinner.

Speakers at the dedication emphasized the history, traditions, and significance of the Institute and the cause of applied economic research. Dr. Joseph S. Davis, one of the three founding Directors, traced the development of the Institute from its conception in 1921 through its formative years. His description of the activities and events surrounding the original scholars is a delightful recounting of the excitement involved in launching a new enterprise. His talk was followed by that of Dr. Robert D. Calkins, Vice-Chancellor, Division of Social Science, at the University of California at Santa Cruz, former long-time President of the Brookings Institution at Washington, D.C., and product of the Food Research Institute during Davis' tenure as co-Director. Calkins spoke of the high purposes and early accomplishments of applied economic research, particularly in reaction to and as a product of the great challenges posed by twentieth century problems. Whitney MacMillan, Vice President of Cargill, concluded the addresses with a summary of the events leading to the federal farm policy of 1963, and the role of the Institute's scholarship in clarifying the issues involved in that controversy. A pamphlet containing these addresses and including a statement by Institute Director, Dr. William O. Jones, is available on request to the Institute.

The Food Research Building is a reconstruction of the west wing of Encina Hall. Built in 1893, Encina was the home of generations of Stanford freshman men until the early 1950s when they were displaced by University administrative offices. The original sandstone walls, left intact by the renovation, retain Encina's traditional California flavor while the elegantly carved initials still etched on the window ledges are a reminder of the building's colorful history. Recent excavations on the site uncovered long forgotten steam tunnels rumored by the early occupants to lead across campus to a freshman women's residence.

Inside, the four floors, basement, and attic provide modern working space for the Institute's research staff, students, and Library. The Food Research Library, located on the ground floor and basement levels, particularly benefited from the move. Its research holdings, well known for strength in statistical compendia, commodity reports, and sub-Saharan African publications, had been scattered in three locations, two of which were unavailable for browsing. The new location provides housing, research, and study space adequate to current needs as well as provision for the Institute Librarian and his staff. Four seminar rooms and a classroom facilitate the teaching mission of the Institute. Faculty studies, student workrooms, a large faculty reading room, and other supporting facilities complete the accommodations.

The $1,750,000 remodelling project began in January 1968 when the Office of Education provided a matching grant to supplement gifts from more than a dozen foundations and corporations. A plaque commemorating these generous offers is located in the entrance lobby.