Volume Title: The Theory and Empirical Analysis of Production

Volume Author/Editor: Murray Brown, editor

Volume Publisher: NBER

Volume ISBN: 0-870-14486-3

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

Conference Date:

Publication Date: 1967

Chapter Title: Production Functions in Manufacturing: Some Preliminary Results

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Chapter URL: http://www.nber.org/chapters/c1480

Chapter pages in book: (p. 275 - 340)

PRODUCTION FUNCTIONS IN MANUFACTURING: SOME PRELIMINARY RESULTS

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I. Introduction

THIS is a first progress report on a research program whose ultimate purpose is to account for the major sources of productivity growth in U.S. manufacturing industries in the post-World War II period. The analytical framework for this research endeavor was developed in my studies of technical change in agriculture, whose main substantive conclusions pointed to improvements in the quality of the labor force, economies of scale, and public investments in research and extension as the major sources of measured "residual" technical change in agriculture.[1] By extending this work to the manufacturing sector I hope to test the analytical framework and the broader relevance of the previous findings and to modify them in light of the different conditions prevailing in the industrial sector of the economy.

This paper, however, has a much narrower scope. It reports on a detailed analysis of cross-sectional data from the 1958 *Census of Manufactures* and the 1960 *Census of Population,* concentrating primarily on the construction and testing of quality-of-labor variables and an investigation of economies of scale.[2] The currently available data for analysis

NOTE: This work is a part of a larger study of the econometrics of technological change supported by grants from the National Science Foundation and the Ford Foundation. Parts of this paper were written during my tenure as a Ford Foundation Faculty Research Fellow at the Econometric Institute, Rotterdam.

[1] See Griliches (1963a) and (1964). Names and dates refer to the list of references at the end of this paper.

[2] The same data have been analyzed in somewhat similar fashion by Bell (1964) and Hildebrand and Liu (1965). Besides using somewhat different variables and procedures, the main difference between these studies and this paper is in purpose

are limited both in scope and quality and can answer only a few of the many interesting questions that could be explored within the production function framework. The work reported here is preliminary not only in the narrow scope of its questions, but also in the sense that it is based almost entirely on only one set of data. As such, it reports results that are an outcome of considerable "fishing" in these data. The findings of this analysis have to and will be eventually tested on new data (from the 1963 *Census of Manufactures*) which will become available shortly.

The major part of this article is devoted to reporting the results of estimating a production relation of the form

$$\log (V/L)_{ij} = a_0 + \alpha \log (K/L)_{ij} + h \log L_{ij} + \sum_h \beta_h Z_{hij} + d_i + d_j + u_{ij},$$

where V is value added, L is a measure of man-hours, K is a measure of capital services, the Z_{hij}'s are various measures of labor and capital quality, d_i and d_j are coefficients of industry and state dummy variables, and u_{ij} is a random disturbance. The index i varies over industries (two-digits, $i = 20, \ldots, 38$) and the index j over 49 states (including the District of Columbia but excluding Alaska and Hawaii). This form is convenient for the estimation of economies of scale, since the coefficient $h = a_k + a_l - 1$ provides a direct measure of it and a direct way of testing its "significance."

Note several old-fashioned aspects of this formulation: (1) I am estimating a Cobb-Douglas form, implicitly assuming that the elasticity of substitution is unity; (2) I will be using "naïve" simple least-squares estimation procedures; (3) I am imposing the *same* α and h coefficients on the whole universe of nineteen industries. Detailed estimates of the elasticity of substitution based both on the ACMS method [3] and on a direct approximation of the CES production function are presented in Section III of this paper. They indicate, overwhelmingly, that at least for these data the Cobb-Douglas assumption is not inappropriate.

It is harder to make an adequate allowance for the simultaneity

and scope. Bell did not test for economies of scale, nor allowed for any quality-of-labor variables. Hildebrand and Liu did try to use, unsuccessfully, a rather poor education variable. Their primary interest, however, was in defining and testing (with what I interpret to be negative results) a particular version of the embodiment-of-technical-change hypothesis. The main difference between this paper and their work is in the attempt that is made here to construct and test a series of specific industry-by-state quality-of-the-labor-force variables. See Griliches (1965) for a more detailed discussion of the Hildebrand and Liu results.

[3] See Arrow *et al.* (1961).

problem without constructing a complete production and input decision behavior model. Assuming profit maximization with random deviations but without any lags, one can estimate the coefficients by indirect least squares, which in this context is a full information method.[4] But this information, while "full," is apparently not very good, as it leads to unreasonable coefficients and very high standard errors. An alternative, and theoretically less demanding procedure, is to use the method of instrumental variables. Unfortunately, in aggregative cross sections of this type, the available instrumental variables such as lagged labor are "too good." They are so highly correlated with the variable they are replacing that there is almost no difference between the least squares and instrumental variables estimates of the major coefficients.[5] Either there is no simultaneity problem or it cannot be cured by the use of lagged endogenous variables as instruments. The possible magnitude of simultaneous equations bias is also somewhat reduced by the use of industry and state dummy variables, eliminating the systematic components of the correlation between the disturbance and the "independent" variables. But only the availability of several consistent cross sections over time will permit a more satisfactory treatment of the simultaneity problem.[6]

Perhaps the most unsatisfactory aspect of the estimates presented below is the imposition of the same coefficients on all two-digit subindustries. There are two answers to this criticism: (1) I have fitted the equations separately, and they are not significantly different from each other. This, however, is more a reflection of the lack of degrees of freedom and poorness of the data than of a true equality of coefficients. (2) I am not interested in individual industry effects but in average relations for manufacturing as a whole. If the coefficients are different, fitting one equation to them will in fact result in estimates that are averages of the corresponding individual industry coefficients.[7]

[4] See Hoch (1962) and Kmenta (1964) for a discussion of this method.

[5] This was also the case in the Hildebrand and Liu (1965) estimates for 1957 using essentially the same data and similar procedures. In none of the fifteen cases (industries) estimated by them is there any significant difference between their least squares and two-stage least squares estimates of the production function coefficients. See also Griliches (1963c).

[6] This would allow us to use Mundlak's (1963) covariance method and less highly serially correlated lagged variables as instruments.

[7] Things are a little bit more complicated than that. The statement in the text is strictly true only in the bivariate case (one independent variable) and in the multivariate case when the slope parameters are distributed independently of the values of the independent variables. More generally, each coefficient is a weighted

Whether these are the "right" averages having the "right" weights for eventual time series comparison is problematic. But clearly the procedure is superior to fitting the same equation to total manufacturing averages.[8] Allowing each industry to have a separate intercept will reduce somewhat the misspecification in the other coefficients. The alternative of using up an additional eighteen degrees of freedom and allowing also the α's to differ between industries leads to no appreciable improvement in the estimates. Again, as several cross sections with more degrees of freedom become available, one will also be able to relax this restriction.

The use of industry and state dummy variables is a mixed blessing. They do take care of various possible specification errors which are either industry or state specific. They also put the various hypotheses to a much more stringent test. On the other hand, they reduce the available variance of the various independent variables greatly, forcing us to estimate relations from the "within"-industry or state variance components. This is likely to lead to more unstable estimates with larger standard errors. Moreover, since it is quite likely that some of our measures are subject to substantial error, using only the "within" variance will magnify the error-to-systematic-component variance ratio and lead to downward bias in the estimated coefficients. Thus, while the equations including all the dummy variables have the highest R^2's, they do not necessarily yield the best estimates of the coefficients of interest.

The plan of the rest of the paper is as follows: Section II discusses data sources, the definitions of various variables and their possible shortcomings. Section III represents a digression devoted to a review of previous estimates of the elasticity of substitution in manufacturing and the presentation of a detailed set of new estimates. Section IV presents the major production function results, using also data from the 1954 *Census of Manufactures* for a further exploration of the problem of economies of scale. Section V indicates how some of the results of this study can be used to account for the growth of manufacturing output in the postwar period.

average of the corresponding microparameters plus a zero-weighted covariance correction term involving the noncorresponding parameters. If there is no particular correlation between the various parameters and the average deviations of the independent variables in the subindustries, the latter terms drop out. See Zellner (1962) and Theil (1954) for more details.

[8] Preliminary estimates using state average per establishment data for *total* manufacturing were presented in Griliches (1963c). The current results can be interpreted as a within-state disaggregation using two-digit industry detail.

The main results of this study can be summarized briefly: (1) There is no substantial evidence against the Cobb-Douglas assumption in manufacturing. (2) Differences in the quality of labor are an important factor in accounting for differences in labor productivity (holding capital-labor ratios constant). (3) There are some indications of mildly increasing returns to scale in manufacturing. The data used, however, are not the most suitable for an investigation of this question. More work and better data are required before the last finding can be considered to be definitive.

II. The Data and the Variables

The *Census of Manufactures, 1958,* presents, for the first time in decades, data on the gross book value of depreciable assets in manufacturing by states and two-digit industries.[9] This is the basic body of data defining the scope of this study and the number of available observations. In addition, the *Census of Manufactures* and the associated *Annual Surveys of Manufactures* provide data on value added, payrolls, man-hours, number of establishments, and other variables. The main limitation of this body of data, besides its being only a one-year cross section, is the extreme paucity of data on the characteristics of the labor force in the various states and industries. The largest expenditure of effort in this study was devoted to the construction of appropriate state-by-industry "quality-of-the-labor-force" variables from the 1960 *Census of Population.*[10]

The basic unit in this study is a per-establishment within-state industry average. Data are not available for all industries in all states. Preliminary investigations were carried out using a total of 440 observations in 49 states (including the District of Columbia) and 19 industries (excluding Industry 39, miscellaneous manufacturing). Most of the final computations were carried out using a reduced sample of 417 observations, excluding Industry 21, tobacco products, and Industry 29, petroleum and coal products. Industry 21 was excluded because I did not succeed in constructing the associated labor quality variables for it, and

[9] Vol. I, Chap. 9. These data were actually collected as part of the 1957 Annual Survey of Manufactures, but are part of the 1958 Census program.
[10] Vol. I, Chap. D, by State.

Industry 29 because the results for it proved to be extremely unstable in some of the preliminary calculations.

V—value added (adjusted) is the main dependent variable in this study and the measure of output used.[11]

K—the flow of capital services is defined as the sum of insurance premiums, rental payments, property taxes paid, depreciation and depletion charged in 1957, and .06 (6 per cent) of gross book value on December 31, 1957. Note that the first four items refer to 1957 instead of 1958. Since they are likely to be quite sticky and hence highly serially correlated, not much error is introduced by using one-year-lagged values (no comparable data are available for 1958).

L—total man-hours equals total payroll divided by the average wage rate per hour of *production* workers. Note that this converts the contribution of nonproduction workers into production worker hour-equivalents, allowing to some extent for quality differences due to a different mix of production and nonproduction workers in different industries or states.[12]

As noted above, all these variables are per establishment (i.e., the state totals are divided by the number of establishments in the industry in 1958).[13]

W—average wage rate of production workers is derived as the ratio of total wages to total man-hours of production workers.

Besides the capital service variable described above, two other capital measures were also tried: gross book value and a capital services concept as above except that the 6 per cent was taken of the net (depreciated) stock of capital rather than of the gross measure as in *K* above.[14] The first of these alternative measures gave similar but somewhat inferior

[11] This and all the other nonlabor-quality variables are taken from the *Census of Manufactures, 1958,* Vol. I, Chap. 9, the appropriate chapters of Vol. II, and the *Annual Survey of Manufactures, 1957.*

[12] One could have used two separate labor inputs here: production and nonproduction workers. The work of Hildebrand and Liu indicates, however, that there is little to be gained from such a division. There is no significant improvement in fit (or in other aspects of their results) when they disaggregate the labor input into these two components. Note, however, that implicitly we are assuming that the entire production-nonproduction wage difference is due to skill differences. But if, for example, the geographic dispersion of skilled worker wages is less than that of the unskilled, this will introduce a certain amount of bias into this labor measure.

[13] The 1957 data are also in per-1958 establishment units.

[14] Net stock was defined as gross book value minus accumulated depreciation to the end of 1956 and minus depreciation in 1957.

results in the preliminary runs. There was little to choose between the second alternative and the measure actually used.[15]

The flow formulation, whenever it is not proportional to the stock, is the more relevant measure of capital services. The variable I use approximates the idea of capital services—capital stock $(\delta + r)$, where δ is the depreciation rate and r is the interest rate. To the extent that the expected life of equipment differs between industries or establishments (either because of physical reasons or because of anticipated obsolescence), the use of current depreciation will approximate differences that are due to differences in δ.

The procedure used assumes, however, that capital services of different vintages are equally productive. This hypothesis can be tested by including R, the ratio of net to gross stock of capital, in the various regressions. R is a measure of the youngness of the capital stock. The higher is net stock relative to gross, the less it has depreciated, the more recent, presumably, is its vintage (on the average). Thus, to the extent that the "embodiment-of-technical-change-in-capital" hypothesis is important, it should show up in a significant and positive coefficient for R.[16] Unfortunately, this is a very weak test due to the main drawback of these capital data—they are all in historical costs rather than in current or constant prices. The embodiment hypothesis says that because of technical change, younger capital is more capital; but at the same time, because of price level changes it is also *less* capital. Thus, if the rate of embodied technical change is no greater than the average rate of inflation, not an unreasonable assumption, the two effects would cancel out. Also, the differences in R may reflect different depreciation policies more than they do age differences. Be that as it may, the R variable was never significant in any of the various combinations tried. Thus, I find no evidence for the embodiment hypothesis, which is consistent with the Hildebrand and Liu results and the Berglas (1965) time series investigation.[17]

The 1960 *Census of Population* provides data by state on the sex,

[15] The simple correlation between these two measures is .999. It is .990 between K as used and gross book value.

[16] In the form as introduced ($\log R = \log NK - \log GK$) it also allows the regression to "choose" a net capital stock concept if it were to fit better.

[17] Hildebrand and Liu introduced $\log R$ as an interaction term. I.e., they use $\log R \times \log K$ as their capital variable, and it is never significantly superior to just $\log K$ as a variable. By introducing $\log R$ separately we allow it a more general and independent role and a better chance at "significance," but with no greater success.

race, and age composition of the labor force by industry.[18] I have utilized the following variables constructed from this data:

Age—median age of employed males
P White—white as a fraction of the total employed males
P Female—females as a fraction of all employees

The Census does not provide a direct estimate of the educational distribution of the labor force by industry. Nor does it provide an education-by-occupation distribution at the state level. It does give, however, information on the occupational distribution of the labor force by industry by state, from which one can construct an occupational mix quality-of-the-labor-force variable. Such a variable will approximate and should be highly correlated with the education variables used in my previous studies. The occupational mix index is constructed as follows:

$$O_{ij} = \sum_k y_k O_{kij}$$

where k, i, and j are indexes for the occupation, industry, and state classifications respectively. O_{kij} is the fraction of total males in the ith industry in state j belonging to the kth occupation category. y_k is the mean income of *all* males , 25 years and over, in this occupational category in 1959. Two sets of y's were used, one for the northern and western states and another one for the southern states.[19] The resulting index can be

[18] Difficulties arise because the *Census of Population* industrial breakdown does not strictly equal the two-digit manufacturing Census classification. Where the population Census breakdown is more detailed, the results were aggregated using total employment in the subindustries as weights. In a few other cases, a population Census industry classification was attributed to several two-digit codes. For example, the *Census of Population* industry categories "primary ferrous" and "primary nonferrous" are added to yield the *Census of Manufactures* two-digit "primary metals" industry. Similarly, the *C of P* category "furniture, lumber, and wood products" is assigned to both the "lumber" and the "furniture" industries.

[19] Separate average-income-by-occupation figures were also computed for the North and West, but they differed only in the second or third place, and it was decided to average the two. The y_k used were

Occupation	*North and West*	*South*
Professional	$8,983	$8,577
Managerial	9,916	8,381
Clerical	5,536	5,205
Sales	7,374	6,149
Craftsmen	5,949	4,907
Operative	5,115	4,108
Service	4,423	3,721
Laborers	4,196	2,913
Occupation not reported	6,032	5,296

SOURCE: *Census of Population, 1960*, PC(2) 7B, Tables 2 and 3.

interpreted as the annual income predicted for this particular labor force given its occupational mix and national (or regional) average incomes by occupation.

Note that of the four labor quality variables, three (age, percentage white, and occupation mix) refer to the *male* labor force. That is, roughly speaking, we distinguish between the male and female labor force components and in addition allow for industry and state differences in the quality of the male labor force (only).[20]

Two types of dummy variables were used:

I_f—an industry dummy which takes the value of one for an observation from the corresponding industry and zero for all other industries. Eighteen such dummies were used, leaving out the dummy for Industry 20, and defining all industry effects as additive to or from the I_{20} level.

S_f—a similar dummy for states. Actually, I did not use a separate dummy for each state but combined several smaller states into quasi-regions. In preliminary investigations, forty such dummies were used. Since many of these did not differ significantly from each other, they were further combined into a total of twenty quasi-regional dummies.[21]

Using dummy variables forces the rest of the coefficients to be estimated from the variance around the respective class means. I.e., it takes out the between-class variance of all the variables. In some cases not much is left when this is taken out, and one should expect both a decline in the "significance" and in the numerical value of certain coefficients (because of an increase in the relative variance of measurement errors). Thus, much of the difference in capital-labor ratios is between industries. Introducing industry dummies is likely to reduce the apparent importance of this variable. Similarly, most of the labor force quality differences are geographical. Introducing state dummies is likely to eliminate much of their effect. Thus, it is not clear that the "dummies inclusive" estimates are necessarily the best. Moreover, since

[20] In principle one could construct also similar variables for the female labor force. This was not done both because of its costliness and because of the expectation that quality variation in the female *industrial* labor force is much narrower than among males.

[21] The "'regions" used were: Maine; (rest of) New England; New York, New Jersey; Pennsylvania; Ohio, Indiana; Illinois; Michigan; Wisconsin; Minnesota; Iowa, Missouri, North Dakota, South Dakota, Nebraska, Kansas; Delaware, Maryland, District of Columbia, Virginia, West Virginia; North Carolina, South Carolina, Georgia, Florida; Kentucky; Tennessee, Alabama; Mississippi, Arkansas; Louisiana, Oklahoma, Texas; Colorado, Montana, Idaho, Wyoming; Arizona, New Mexico; Utah, Nevada; Washington, Oregon. California is the "left out" dummy, or the reference state.

much of the real wage differences that we count on to identify the production function are also geographical, we shall use the state dummies only sparingly.[22]

The quality variables (and the wage rate) are in per-man or in fraction-of-the-labor-force units. All the variables, except for the dummies and the percentage white and percentage female variables are transformed into logarithms of the original units. The latter variables are left in their original fraction form since it is thus easier to interpret them as quality variables.[23]

One of the main shortcomings of these data has already been intimated above: various variables refer to somewhat different points of time. Perhaps the most important drawback is the unavailability of capital figures in constant prices. In addition, the labor force quality data is as of 1960. Also, I use 1957 data from the *Annual Survey of Manufactures* and 1958 data from the *Census of Manufactures* as if they relate to the same universe and observations. Actually, there are some differences in coverage and the possibility of substantial sampling error. Moreover, the year 1958 was a recession year (albeit mild), and some of the industries may have been operating below capacity. To the extent that this affects all establishments in an industry similarly, the use of industry intercept dummies will allow for most of it. I am also relying on the cross-sectional nature of the data and the rather slow changing nature of geographic differentials to reduce the impact of the other shortcomings discussed above.

[22] The state dummies are useful, however, against the hypothesis that there are substantial regional price-of-output differentials which have not been eliminated from our output measure.

[23] Consider two types of labor, which are convertible into each other at a fixed exchange (premium) rate. Thus, the correct component of the total labor input in the production function is, say:

$$(L_1 + cL_2)^\beta$$

This can be factored into

$$[L_1 + L_2 + (c - 1)\,L_2]^\beta = (L_1 + L_2)^\beta[1 + (c - 1)\pi]^\beta$$

where $\pi = L_2/(L_1 + L_2)$ is the fraction that class 2 workers are of the total. Estimating this type of function we would need:

$$\beta \log (L_1 + L_2) + \beta \log (1 + m\pi_2)$$

Not knowing m ($m = c - 1$), it is hard to construct the second term satisfactorily. But since π is a fraction, and m is also likely to be a fraction, we can approximate

$$\log (1 + x) \approx x, \; |x| < 1$$

and $\log (1 + m\pi)$ by a function of π alone, the m and β constants entering into its estimated coefficient.

III. The Elasticity of Substitution in Manufacturing: A Digression

From the point of view of production function estimation and the analysis of sources of productivity growth, the elasticity of substitution is a second-order parameter. Even if it were significantly different from unity, one would have to take this into account in an analysis of growth only if there were very substantial changes in the capital-labor ratio.[24] Nevertheless, by estimating a Cobb-Douglas-type production function we are assuming that this elasticity is equal to one in manufacturing. If this assumption is substantially incorrect, we will be committing a specification error of unknown magnitude and consequences. It is thus of some interest to review the previous evidence on this point and to conduct some additional tests with the data used in this study. Besides, the elasticity of substitution is a parameter of some general interest (particularly for theories of income distribution), and hence additional estimates of it are worth reporting for their own sake.

In reviewing the previous estimates of the elasticity of substitution in U.S. manufacturing industries we are faced with two conflicting sets of estimates. The studies based on cross-sectional data yield estimates which are on the whole not significantly different from unity. The time series studies report, on the average, substantially lower estimates. Almost all of these studies use the ACMS method of estimating the elasticity of substitution from the regression,

$$\log V/L = \sigma \log W + u$$

or a related form. At the two-digit industry level such an equation was fitted to cross-sectional data from the *Annual Survey of Manufactures*

[24] See Nelson (1964) for a more detailed analysis of this point. Roughly speaking, the percentage rate of growth of output can be approximated by the expression

$$y = t + \alpha n + (1 - \alpha)k + (1/2)\alpha(1 - \alpha)[(\sigma - 1)/\sigma](k - n)^2$$

where y, n, k, and t are the percentage rates of growth of output, labor, capital, and "productivity," respectively; α is the elasticity of output with respect to labor (at the particular point), and σ is the elasticity of substitution. The last term reflects the influence of $\sigma \neq 1$. Consider, for illustrative purposes, the following values for these variables: $\alpha = .7$, $\sigma = .5$, $k = .04$, $n = .01$. The halving of the elasticity of substitution would have a depressing effect on the rate of growth of output of only

$$-.5 \times .7 \times .3 \times 1.0\,(.04 - .01)^2 = -.105 \times .0009 = -.0001,$$

or one-hundredth of a percentage point.

by Minasian (1961) and Solow (1964), and for four-digit industries by Ferguson (1964), using data from the various Censuses of Manufactures.[25] Bell (1964) was the only one to use the capital-labor-ratio version of this equation. These estimates are briefly summarized in Table 1. The general impression from a more detailed look at these studies is that, accepting the ACMS model as correct, there is no strong evidence that σ is significantly different from unity. The estimates are often poor and erratic (particularly at the four-digit level), but they cluster around one, with a significant number of them exceeding unity by substantial amounts.

The time series estimates are summarized in Table 2.[26] In general they average below unity, though often not significantly so. In many cases the estimated relationships are also quite erratic and poor. For example, in 11 out of 18 cases the coefficient of log W_t estimated by McKinnon is not significantly different from zero. This implies either that σ is very small, or that the model or the data are not very good in accounting for the annual fluctuations in labor productivity. I shall come back to this point below. Suffice it to say here that in spite of the fact that the more recent Brown and Ferguson studies yield estimates which are closer to their cross-sectional counterparts, the over all impression from the time series results is that the estimates cluster around a σ which is significantly below unity.[27]

In a similar but more detailed recent survey Lucas examines these two conflicting bodies of evidence and concludes (with some reservations) in favor of the time series results. His argument is based on the existence of important biases in cross-sectional data, each of which would tend to bias the estimated elasticities towards unity. The two major sources of this bias are the disregard in these studies of regional price-of-output differentials and quality-of-labor differentials. It can be

[25] The summary of Ferguson's results is based on unpublished tables of the detailed results kindly supplied by him.

[26] These summaries are not really fair to the original papers, particularly those of Brown and Lucas, which contain much more material than is reported here. I have, however, limited myself to results which are comparable across studies.

[27] Ferguson's estimates are an exception to this statement, but given their undeflated nature and the consequent bias toward unity, I would attach less weight to them. Since this was first written the following additional studies have come to my attention: O'Neil (1965) and Sheshinski (1964) based on cross-sectional data and an additional time series study by McKinnon reviewed in Nerlove's survey in this volume. None of these studies is in strong conflict with the conclusions of this section.

TABLE 1

Cross-sectional Estimates of the Elasticity of Substitution in U.S. Manufacturing Industries

Author	Period and Unit	Method	Number of Elasticities Estimated: Total	Significantly (more than 2 standard errors): Below 1	Significantly (more than 2 standard errors): Above 1
Minasian (1961)	1957, states	ACMS	18	2	3
Solow (1964)	1956, regions	ACMS	18	0	1
Ferguson (1964)	1947, 1954, 1958 Selected *four*-digit industries, states	ACMS	129	13	8
Bell (1964)	1958, states	$\log \frac{WL}{V-WL} = a + \frac{(1-\sigma)}{\sigma} \log \frac{K}{L}$	18	0	11

ACMS: $\log V/L = a + \sigma \log W$. See Arrow *et al*. (1961)

TABLE 2

Time Series Estimates of the Elasticity of Substitution in U.S. Manufacturing Industries

Author	Method	Period	Number of σ Coefficients Estimated	"Significantly"	
			Total	Below 1	Above 1
McKinnon (1962)	$\log (V/L)_t = \sigma(1-\gamma) \log W_t + \gamma \log (V/L)_{t-1} + \lambda t$	1947–58	18	12[a]	0
Lucas (1963)	$\log (V/L)_t = \sigma \log W_t + \lambda t$	1931–58	14	13	0
Ferguson (1965)	$\log (V/L)_t = \sigma \log W_t + \lambda t$	1949–61	19	1	2
Brown (1965)[b]	$\log (V/L) = \sigma \log W^*_t + bC + \lambda t$	1948–60	13	3	0

[a]No exact test for σ is provided. The count here is approximate and impressionistic.

[b]W^* are different moving averages of past wage rates. C is a measure of capacity utilization.

shown that under reasonable assumptions each of these omissions will bias the estimate towards unity.[28] On the other hand, he considers the two sources of bias in the time series context—simultaneity and misspecification of the lag structure—and concludes that these do not bias the time series estimates especially in some *particular* direction. The latter conclusion is supported by his finding that trying different lag schemes and a simultaneous equation model does not change his estimates by much.[29]

To my mind the choice is not all that clear-cut. First there is the puzzling frequency of *above*-unity estimates in the cross-sectional studies.

[28] Let the true equation be $\log V/L = \sigma \log W + u$. True W = total payroll/L, but actually we observe only N, the number of employees, while L, the effective labor input measure, is equal to $L = qN$, where q is a quality index. Then the true equation in the observed units can be rewritten as

$$\log V/L = \log V/N - \log q = \sigma \log \hat{W} - \sigma \log q + u$$

or

$$\log V/N = \sigma \log \hat{W} + (1 - \sigma) \log q + u$$

where $\hat{W}$ is the observed wage rate per incorrect unit $\hat{W}$ = Payroll/N, $\log \hat{W} = \log W + \log q$. Leaving out q from the regressions implies that the estimated σ is equal to

$$E\hat{\sigma} = \sigma + (1 - \sigma)b_{q\hat{W}}$$

where $b_{q\hat{W}}$ is the regression coefficient of $\log q$ on $\log \hat{W}$. It will always be positive, since quality as defined will be positively correlated with *measured* wage rates. If q is a random error,

$$b_{qW} = \frac{\mathrm{Var}\,(\log q)}{\mathrm{Var}\,(\log W)} < 1$$

is the fraction that error variance is of the total variance in the observed wage rates. The sign of the bias depends then on the sign of $(1 - \sigma)$, and the estimated $\hat{\sigma}$ is biased toward unity. The expression for bias due to ignored variations in the price of output is similar.

[29] Another possible source of difference between time series and cross-section estimates of σ, pointed out by Ferguson (1965), is the more restrictive definition of output (value added) in the OBE series. The *Census of Manufactures* data, the source of all cross-sectional estimates, includes in value added certain overhead expenses (mainly services) purchased from other industries, while the net-income-originating series of the OBE nets them out. If these inputs do belong in the production function with the same elasticity of substitution, then subtracting them from value added biases the estimated $\hat{\sigma}$ *away* from unity. If the elasticity of substitution of these overhead inputs is zero, which is the implicit assumption behind the subtraction procedure, including them (wrongly) in the value-added concept would reduce the fit but would not bias the estimate of σ (since by assumption these inputs are uncorrelated with differences in the wage level). The first formulation seems to be the more relevant one, which would lead to poorer and more erratic results in the time series estimates.

The significant-bias-toward-unity argument implies then that a number of these elasticities are *under*estimated and are in fact even further above unity. This contradicts the time series results, which rarely exceed unity. But more importantly, Lucas does not consider the major drawback of the time series data: the predominant influence of short-run business cycle phenomena. In cross sections one observes differences in labor productivity and capital intensity which are relatively long run and which change only slowly (this, of course, creates problems of its own). In time series estimates, partly because of the inclusion of the trend variable, almost all of the observations on the net relationship between V/L (labor productivity) and W are short run (deviations from trend) and are dominated by cyclical phenomena. By now it is well recognized that the cyclical behavior of labor productivity does not fit well the standard production function framework and requires a substantially more complicated model to explain it.[30] But it is from these same cyclical observations which are probably not on the production function at all that these studies attempt to derive the properties of the aggregate production function. No wonder the results are meager.

Another way of making this same criticism of the time series studies is to note that the coefficient of W is based on the variance of W net of trend (presumably its main systematic component) and often net of the previous labor productivity level. If, as is quite likely, the measured wage rate is subject to substantial measurement error, estimates that take out much of the systematic component of W magnify the relative error—variance—and result in downward-biased coefficients. Thus the time series estimates are biased toward zero. Since I do not believe that the usual time series data are really relevant to the question asked of them, I would prefer the cross-sectional estimates, particularly since there are ways of getting around their most obvious sources of bias.

In what follows we shall investigate the importance of these biases using three approaches: (1) introducing labor quality variables directly into the regression, (2) using separate regional dummy variables to take into account possible regional price-of-output and labor quality differentials, and (3) allowing for serial correlation in the disturbances due to persistence in the left-out variables.

Since we have observations for two years (1957 and 1958) on the

[30] There is a growing literature on this subject. For a recent view see Solow (1964b).

major variables, we can also investigate the possibility of a distributed lag model or other forms of time dependence in these data.

The standard Koyck-type distributed lag model,

$$\log (V/L)_t = \sigma(1 - \gamma) \log W_t + \gamma \log (V/L)_{t-1} + v_t,$$

when applied to this type of cross-sectional data yields usually very high γ's and implies a very slow rate of adjustment to wage changes.[81] But given only two years of data such a model would fit even if there were no disequilibrium or lagged adjustment problem with the estimated γ approaching unity, except for errors of measurement and other transitory variations. An alternative hypothesis, which would also explain the relatively good fit of the above form, is that the CES form holds without any appreciable lag

$$\log (V/L)_t = \sigma \log W_t + u_t,$$

but that there is a substantial first-order serial correlation in the residuals, due to the persistence of the various possible mis-specifications, such as regional quality-of-labor differentials. This correlation can be formalized as

$$u_t = \rho u_{t-1} + v_t$$

implying the estimation of

$$\log (V/L)_t = \sigma \log W_t + \rho \log (V/L)_{t-1} - \sigma\rho \log W_{t-1} + v_t$$

Thus, we can distinguish between the two models by adding the lagged wage rate term to the Koyck form of the CES function. The distributed lag model implies that the coefficient of the lagged wage rate should be zero or positive. The serial correlation model implies that it should be negative and of the same magnitude as the *product* of the coefficients of the current wage rate and the lagged productivity term.

Such a computation has been performed on the pooled seventeen-industry set of data for 1958, containing a total of 417 observations. A direct estimate of the elasticity of substitution gives

$$\log (V/L)_{58} = A_1 + 1.198 \log W_{58};\ R^2 = .606$$

The partial adjustment model results in

$$\log (V/L)_{58} = A_2 + .233 \log W_{58} + .827 \log (V/L)_{57};\ R^2 = .890$$

[81] E.g., see the labor-demand-equation results in Hildebrand and Liu.

with a "highly significant" coefficient for the lagged dependent variable and a much improved fit (the estimated σ is still above unity: $\sigma = .233/(1 - .827) = 1.35$. But it implies a distressingly low rate of adjustment of only 17 per cent per year. Adding the lagged wage rate term, we have

$$\log (V/L)_{58} = A_3 + \underset{(.089)}{1.056} \log W_{58} + \underset{(.022)}{.855} \log (V/L)_{57} \underset{(.022)}{-.900} \log W_{57}; \; R^2 = .918$$

If the serial correlation model is right, the third coefficient should equal minus the product of the first two, which it does approximately ($1.056 \times .855 = .903 \approx .900$). Since there is no obvious alternative explanation for a significant negative coefficient of the lagged wage rate variable, I reject the partial adjustment model and accept the serial correlation one.[32]

Table 3 presents similar results for individual industries. The first set of σ estimates is comparable to, though substantially better than (in terms of fit and t ratios), the Minasian and Solow estimates and is generally of the same order of magnitude. Only one of these σ's (out of 17) is significantly different from unity, and that one is above unity.[33] The second set of σ estimates is based on the partial adjustment equation, while the third is based on the serial correlation model. In 12 out of the 17 cases the latter model is the one consistent with the data. In general, all the estimated σ's are not very (statistically) different from unity, the significant deviations if anything occurring above unity rather than below it.

[32] The matter should not rest here. "Serial correlation" does not explain anything. The next step is to find out what is the mis-specification that is causing it. A small attempt along these lines will be reported below, but the topic as a whole is outside the range of this paper.

Two more observations are worth making about these results: (1) In general one can interpret an equation of the form $y_t = ax_t + bx_{t-1} + cy_{t-1}$ as a distributed lag even if b is negative (assuming $a > 0$). But if $b \simeq -ac$, the implied lag is very short, and there is little gain from the more complicated interpretation. (2) Note (as pointed out to me by R. Solow) that *since* the estimated coefficient of w_t is about 1.0 and the others are about .9 (or $-.9$) the equation as a whole can be interpreted as saying that

$$\log (WL/V)_{58} = .9 \log (WL/V)_{57} + \text{random term.}$$

[33] This count excludes Industry 29, petroleum and coal products, for which no satisfactory estimates were obtained in either of the models. The regressions reported in this table and elsewhere in this section underwent almost no pretesting, and hence the estimated standard errors are applicable, subject to the conventional caveats.

TABLE 3

Estimates of the Elasticity of Substitution at the Two-Digit Manufacturing Industries Level, 1958

(varying number of states as observations)

Industry and SIC Number	σ_1	R^2	N	σ_2 from Lag Function	σ_3 from Transformed Function	R_3^2
20. Food	0.908 (.097)	.694	41	1.014	c.	.861
22. Textile	0.938 (.170)	.615	21	1.113	1.094 (.540)	.748
23. Apparel	1.055 (.194)	.572	24	0.835	0.628 (.212)	.899
24. Lumber	1.069 (.055)	.948	23	1.107	1.175 (.202)	.963
25. Furniture	1.039 (.074)	.908	22	0.989	c.	.968
26. Paper	1.667 (.302)	.522	30	1.300	c.	.901
27. Printing	0.827 (.177)	.593	17	0.450	0.678 (.277)	.862
28. Chemicals	0.714 (.219)	.268	31	0.592	0.700 (.396)	.671
29. Petroleum	n.s.		17	n.s.	n.s.	
30. Rubber and plastics	1.281 (.416)	.422	15	2.208	0.902 (.434)	.847
31. Leather	0.839 (.257)	.470	14	1.603	1.164 (.401)	.852
32. Stone, clay, glass	0.908 (.187)	.496	26	0.774	1.877 (.494)	.780
33. Primary metals	1.407 (.422)	.299	28	3.491	2.374 (.473)	.891
34. Fabricated metals	0.849 (.144)	.530	33	1.167	1.203 (.283)	.740
35. Machinery, exc. electrical	1.240 (.383)	.272	30	2.400	2.004 (.294)	.866
36. Electrical machinery	0.662 (.314)	.162	25	0.397	1.533 (.529)	.592
37. Transportation equipment	0.961 (.547)	.110	27	1.087	c.	.617
38. Instruments	0.752 (.427)	.256	11	0.823	c.	.721

Notes to Table 3

Source: 1958 data from the *Census of Manufactures;* 1957 data from the *Annual Survey of Manufactures*.

Models:

1. $\log(V/L) = A + \sigma \log W$
2. $\log(V/L) = A + (1-\gamma)\ \sigma \log W + \gamma \log (V/L)_{t-1}$
3. $\log(V/L) = A + \sigma \log W + \rho \log (V/L)_{t-1} - \rho\sigma \log W_{t-1}$

c. – contradicts Model 3. The coefficient of $\log W_{t-1}$ is not significantly different from zero. In these cases R_3^2 is very close to R_2^2.

n.s. – no significant relationships found in either of the models.

V/L – value added (adjusted) per man-hour.

W – wage per man-hour of production workers.

Total man-hours – total payroll divided by W.

The figures in parentheses are the estimated standard errors of the coefficients.

Table 4 presents pooled estimates for total manufacturing, allowing for two possible sources of bias: (1) regional price variation through the introduction of regional dummies and (2) labor quality biases through the introduction of specific labor quality variables. Note that by pooling the data from all the industries we assume that elasticity of substitution is the same in all industries, an assumption which is not contradicted by the results presented in Table 2. But this does not imply that we have to assume the same CES form for the production function as a whole. By allowing different intercepts in this equation for different industries (using industry dummy variables) we can allow the distribution parameters in the CES form to differ between industries.[34]

The same is true also of the exponents in the Cobb-Douglas form. A finding of a unitary elasticity of substitution does not imply that all industries are characterized by the *same* Cobb-Douglas form. That assumption we shall have to make in the next section, but it is not used

[34] If we write the CES production function as

$$V = A[\delta L^{-\rho} + (1 - \delta)K^{-\rho}]^{-1/\rho}$$

where $\sigma = 1/(1 + \rho)$, we can allow the A and δ parameters to differ between industries, by allowing the

$$\log (V/L) = I_i + \sigma \log W + u$$

equation to have different intercepts for different industries.

TABLE 4

Estimates of Elasticity of Substitution Equations in U.S. Manufacturing, 1958

(N = 417)

	Regression Numbers							
	1	2	3	4	5	6	7	8
Coefficients of:								
W_{58}	1.198	1.056	1.258	.996	1.035	.993	1.122	1.039
	(.047)	(.089)	(.074)	(.053)	(.096)	(.095)	(.070)	(.099)
$(V/L)_{57}$		0.855			0.661	.703		0.634
		(.022)			(.029)	(.028)		(.030)
W_{57}		−0.900			−0.686	−.698		−0.677
		(.090)			(.094)	(.097)		(.095)
Occupation			0.043		−0.067			−0.213
			(.129)		(.058)			(.114)
Age			−0.512					−0.207
			(.146)					(.095)
P White			−0.156		0.026			
			(.062)		(.029)			
P Female			−0.015		−0.024			−0.046
			(.036)		(.042)			(.039)
Industry dummies				yes	yes	yes	yes	yes
State dummies							yes	yes
R^2	.606	.916	.629	.843	.935	.931	.860	.941

Note: V/L is the dependent variable. All variables are logarithms of the original units except for P White, P Female, and the dummies. Regressions 6 and 7 are based on 440 observations.

here. Note also that if the labor quality variables are of a multiplicative nature in the original form, their coefficients will contain terms involving $(1 - \sigma)$ and thus we would not expect them to be significant if σ is close to unity.[35]

Table 4 shows clearly that the labor quality variables, which we shall find to be important in the production function framework in the next section, contribute little in the elasticity-of-substitution estimation context. This is as one would expect, given a σ close to unity. Moreover, their introduction or the introduction of state (quasi-region) dummies does not change the estimated elasticity of substitution significantly. It is still around unity. And now this result cannot be attributed to labor quality differentials or to other mis-specifications (such as price-of-output differences) which have a predominantly regional component. The only other alternative possible interpretation of these results is one which would deny entirely the possibility of estimating the elasticity of substitution from cross-sectional data, asserting that there are no real wage differences in the United States, all of the observed wage differentials reflecting "quality" differentials. This is unlikely, but cannot be disproved in the extreme form of the statement.[36]

All of the above estimates were based on estimates from a derived demand equation, using the hypothesis of profit maximization, rather than on estimates of the production function itself. Since the CES *production function* form is highly nonlinear in the parameters, it is rather difficult to estimate, and very few direct estimates of this function have been reported in the literature.[37] Recently, however, both Kmenta (1964b) and Nelson (1964) have shown that one can think of the Cobb-Douglas form as a first-order approximation to the CES, and that the second-order approximation can be written as

$$\log V = A + \alpha_1 \log K + \alpha_2 \log L - (1/2)\rho\alpha_1\alpha_2[\log K - \log L]^2,$$

where ρ is again related to $\sigma = 1/(1 + \rho)$. Thus if ρ is significantly different from zero (σ different from one) this should show up in a

[35] Among our quality-of-labor variables only the occupation mix is of this form.

[36] The quality variables used here can account for only about 66 per cent of the observed variance of wage rates. Allowing for industry differences raises this to 82 per cent. Even adding regional variables still leaves unexplained about 12 per cent of the observed wage rate variation.

[37] None, as far as I know, for manufacturing. In most of the other cases some of the parameters were estimated from other data, e.g., such as we explored above, using therefore the profit maximization assumption.

significant coefficient for the square of the (log) capital-labor ratio. This allows a direct test, one that does not depend on the correct specification of the maximization equations and the right expected input and output prices.[38] Using our data (for 417 observations) we estimate

$$\log\left(\frac{V}{L}\right) = \underset{(.03)}{.64} + \underset{(.037)}{.442}\log\frac{K}{L} + \underset{(.014)}{.050}\log L + \underset{(.018)}{.030}\left[\log\frac{K}{L}\right]^2;$$

$$R^2 = .550$$

The $[\log K/L]^2$ term is not significantly different from 0 at conventional significance levels, and this remains also true when industry dummies are added to the above equation. Given our data we cannot reject the hypothesis that the Cobb-Douglas form is an adequate representation up to a second-order approximation.[39] If anything, these results imply the possibility that the "true" σ is actually *above* unity, with a point estimate of $\hat{\sigma} = 1.29$ ($\rho = -.23$).[40]

I do not intend to argue that these results prove that the Cobb-Douglas is the right form for the manufacturing production function, only that there is no strong evidence against it. Until better evidence appears, there is no reason to give it up as the maintained hypothesis.

IV. Production Function Estimates

After the long digression on the form of the production function, we can summarize the main substantive results of this paper relatively briefly. The estimates using the combined set of observations over states

[38] It is, however, a fairly weak test. The expected coefficient of the $[\log K/L]^2$ term is quite small, e.g., if $\alpha_1 = .4$, $\alpha_2 = .6$, and $\rho = 1.0$ (i.e., $\sigma = .5$), this coefficient would be equal to $-.12$. For σ's closer to unity (e.g., $\sigma = .75$) this coefficient would be much smaller ($-.04$). Given the usual standard errors in such studies, it is not likely that these coefficients will be "significant." This is another reflection of the second-order nature of this question. Thus the results below should not surprise us. Unless one has much better data (in terms of the observed range of K/L) and more observations, one may not be able to detect by such procedures even substantial deviations of the true σ from unity.

[39] Since the estimates of the α_1 and α_2 coefficients are *not* invariant to the choice of units in which K and L are measured, the coefficients of log (K/L) should not be interpreted as the comparable Cobb-Douglas coefficients. These are given in the next section.

[40] Note that small differences in the estimated coefficient of $[\log K/L]^2$ imply large differences in the estimated σ. Thus this is not a very good way of estimating it, and one should not be surprised when rather wide swings result. This is an illustration of the point made by Domar at this conference.

and (two-digit) industries are presented in Table 5. As indicated in this table the estimated capital coefficient is always "highly significant." [41] It varies from about .39 in the "no dummies" regression to about .23 in the regressions which eliminate the between-industries differences in capital intensity.[42] These results are consistent with a priori notions about the order of magnitude of this coefficient and with estimates of factor shares at the aggregate level.[43] The coefficient of the labor variable, which indicates the excess of the sum of the coefficients over unity, and is thus a measure of economies of scale, is always significant and positive, albeit small. It indicates, roughly, that a 10 per cent expansion in the scale of the average enterprise in manufacturing would result in a 10.5 per cent increase in output. These results are consistent with the Hildebrand and Liu (1965) findings for 1957 for the separate two-digit industries using similar data. I shall say more about this finding below. The age of capital or embodiment variables is never significant and sometimes has even the wrong sign. The poor performance of this variable indicates that either it is a poor approximation to the relevant embodiment variable or that embodiment in capital is not an important force in manufacturing.

All the coefficients of the labor quality variables have the expected signs and are in general significant at the conventional levels, but the contribution of these variables becomes small if all the between-industry and between-regions variance is eliminated using the dummy variables procedure. This is as one would expect, since most of the important quality variations are likely to be associated with interregional or interindustry

[41] Since there was substantial pretesting of the estimates reported in this section, "significance" statements should be taken with more than usually large chunks of salt. In no case, however, are the statements in the text seriously in conflict with some of the preliminary results not reported here. For example, while the above statement about the numerical value of the capital coefficient is not exact for the other two versions of capital tried, the statement taken as an order of magnitude is correct. The alternative measures were also always "significant" at conventional significance levels. In what follows I shall drop the quotation marks around significant, assuming that they will be supplied by the reader.

[42] The decline in the capital coefficient arises from the introduction of the industry dummies rather than from the use of state dummies.

[43] Using the data provided in the October 1962 issue of the *Survey of Current Business* (pp. 6–18) on the share of payments to labor in total GNP originating in manufacturing (after an adjustment for indirect business taxes) gives .24 as the "share of capital" in 1958. However, 1958 is the low point for this variable in the postwar period. The "average" capital share for 1947–60 is .27 (from these same data).

TABLE 5

Production Function Estimates, U.S. Manufacturing, 1958

	440 Observations		417 Observations								
	1	2	3	4	5	6	7	8	9	10	11
Coefficients of:											
K/L	.358	.297	.392	.388	.382	.351	.252	.258	.261	.235	.229
	(.023)	(.023)	(.018)	(.018)	(.075)	(.019)	(.022)	(.021)	(.020)	(.021)	(.021)
L	.058			.056	.070	.078	.032	.047	.055	.058	.054
	(n.c.)			(.013)	(.012)	(.012)	(.015)	(.015)	(.016)	(.016)	(.015)
R (Age K)	.053	.013									
	(.058)	(.043)									
Occupation					.952	.992	.352		.391	.419	.289
					(.079)	(.098)	(.092)		(.177)	(.174)	(.180)
Age						-.672					.067
						(.128)					(.139)
P White						.092	.196				.134
						(.056)	(.046)				(.047)
P Female						-.120	-.261			-.238	-.245
						(.030)	(.069)			(.066)	(.059)
Industry dummies		yes					yes	yes	yes	yes	yes
State dummies								yes	yes	yes	yes
R^2	n.c.	.781	.528	.547	.665	.697	.823	.852	.854	.860	.862
σ_u	.1090	.0772	.1073	.1052	.0907	.0865	.0672	.0629	.0626	.0616	.0611

n.c. – not computed. These estimates are from log V dependent regressions. While the coefficients are the same or related, the R^2's are not comparable with the log (V/L) forms.

See section II for definitions of the variables.

differentials. The coefficient of the occupation mix variable should be, if the variable were measured correctly, of the same order of magnitude as the coefficient of labor in the production function (about .7).[44] Its "nondummy" estimates are close (see especially the estimate in column 6 of Table 5), but the introduction of industry dummy variables reduces it substantially below this level, though not significantly so.[45] The introduction of regional dummies does not greatly affect the order of magnitude of this coefficient (or of the coefficients of the other quality variables) but substantially reduces its precision.

The age variable is significant in the simple regressions but does not survive the elimination of the interindustry and interregional variance. Its coefficient is negative, indicating a higher productivity in the establishments with a younger labor force. The differences in this variable may, however, be more a reflection of differences in the age of establishments or the age of (more finely defined) industries, rather than of the age of the labor force. Also, the median may be a poor measure for a variable whose effect is known to be u-shaped. Since we do not expect the average age distribution to change by much, and since the main purpose of this study is to derive conclusions which will be useful in an eventual analysis of aggregate time series, a more detailed analysis of the effects of this variable was not undertaken.

The race and sex variables are highly significant and have the expected signs. Since we are holding the occupational mix constant (though perhaps not in a completely satisfactory fashion), these coefficients reflect the well-known fact that women and Negroes are paid less even if one controls for differences in industrial and occupational composition. But these results also show that there are real productivity differences associated with these differentials. A finer occupational and educational breakdown would perhaps reduce these differentials, but is unlikely to eliminate them entirely.[46]

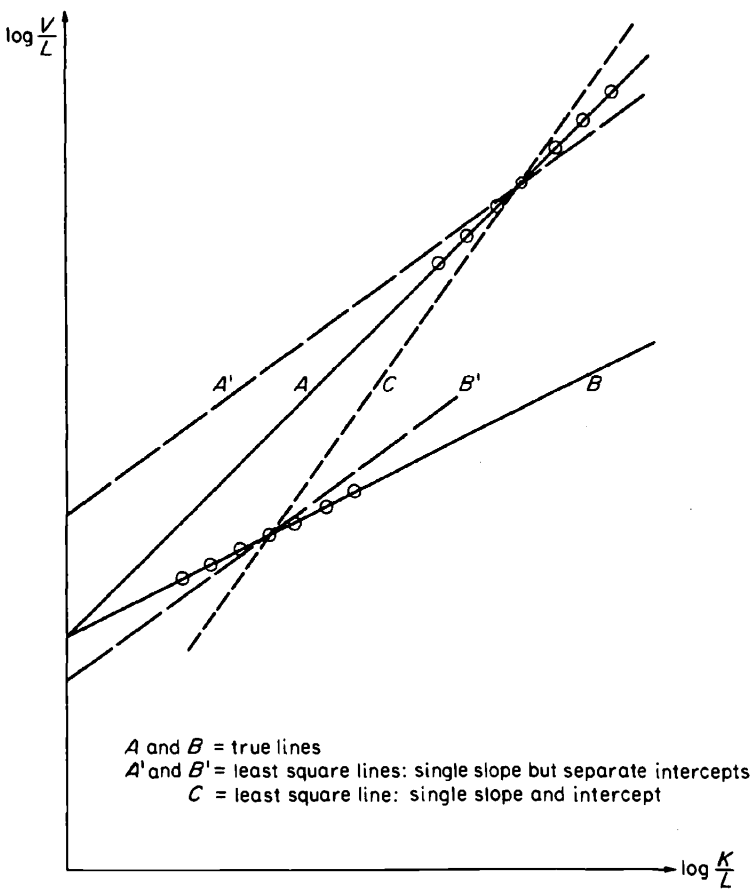
[44] This statement assumes that the correct labor measure to be entered into the production function is a quality-held-constant man-hours figure, constructed by weighting different classes (occupations) of workers by their relative prices (wages) in a base period. The occupation mix variable can be thought of as the result of factoring this measure into two components: number of workers and quality per worker, each entering the production function with the same coefficient (the coefficient of "correct" labor). The occupation mix variable is such a quality measure based on the division of the labor force into occupational categories and the use of 1959 prices (incomes) as weights.

[45] For closer estimates see pages 305 and 306, and footnote 57.

[46] For a detailed analysis of this problem in the income distribution context, see Hanoch (1965).

The industry dummies account for a substantial part of the residual variance. Their introduction reduces both the capital and occupational mix coefficients, leaving the other coefficients largely unchanged. Their significance implies either substantial interindustry differences in rates of return or more likely interindustry differences in the capital coefficients. Allowing for different industry intercepts reduces somewhat the misspecification consequences of assuming that all the industry slopes are the same. This is illustrated in a highly exaggerated fashion in Figure 1. If the true production functions are different, forcing one line onto the data may result in a substantial upward bias in the estimated coefficient (this of course depends on the particular distribution of observations in the sample). Allowing for different intercepts both improves the fit of the

FIGURE 1



over-all relationship and brings the estimated slope relationship closer to the "average."

Attempts to allow for such interindustry differences in the slope coefficient were not particularly successful. Estimating the production functions (without the quality variables) separately for each of the industries resulted in "reasonable" coefficients (as far as order of magnitudes are concerned) in 15 out of the 18 possible cases. But only in 9 of these cases was the capital coefficient significantly different from zero at the conventional significance levels.[47] This is not surprising, since in most of these cases we were trying to identify this coefficient from the *within*-industry across-states variance in capital intensity, using only 20 or so degrees of freedom. To discover the effect of this variable we must either allow it to vary more (i.e., *across* industries) or obtain a much larger sample by pooling the data from different industries.

It is possible to pursue this subject a little bit further through the introduction of industry–capital-intensity interaction dummies. The use of such dummies allows for different slopes with respect to a particular variable while imposing the same coefficients on all the other variables. Table 6 provides an example of such an analysis, introducing an additional set of $19(K/L)_i$ variables, one for each two-digit industry. These variables take the value of the capital-labor ratio in the appropriate industry and state, and are zero for all noncorresponding industries. Two such regressions are presented in Table 6. The first one allows only for differences in the slope coefficient, imposing the same intercept on all industries but allowing separate regional effects. The results are "reasonable," and all the individual slope coefficients (except for Industry 29) are significantly different from zero. Introducing separate industry intercepts improves the over-all fit, but leads to much more erratic results. Generally it reduces the magnitude of most of the coefficients and increases their standard error.

The second set of results is similar to the results obtained by estimating these coefficients separately for each industry. The procedure used here differs only in imposing the same level of returns to scale on all the industries and utilizing the regional dummies (which is not possible

[47] It may be worth reporting that the capital coefficient appears to be above "average" in the food, lumber, pulp, and the stone, clay, glass industries, and below "average" in the textile, printing, leather, fabricated metals, and machinery industries. These results are roughly consistent with those obtained by Bell (1964) using similar data.

TABLE 6

Estimates of the Production Function, U.S. Manufacturing, 1958, Allowing the Capital Coefficients to Differ Among Industries

	440 Observations			
	Coefficient (1)	Estimated Standard Error (1)	Coefficient (2)	Estimated Standard Error (2)
$(K/L)_i$ Variables by Industry				
20. Food	.329	(.056)	.384	(.139)
21. Tobacco	.136	(.061)	.818	(.125)
22. Textile	.650	(.046)	.135	(.151)
23. Apparel	.360	(.029)	.279	(.078)
24. Lumber	.729	(.059)	.363	(.076)
25. Furniture	.435	(.043)	.277	(.114)
26. Pulp and paper	.482	(.069)	.378	(.067)
27. Printing	.167	(.061)	.049	(.195)
28. Chemicals	.460	(.079)	.217	(.068)
29. Petroleum	.071	(.057)	-.198	(.110)
30. Rubber	.204	(.042)	.362	(.089)
31. Leather	.346	(.033)	.044	(.125)
32. Stone, clay, glass	.344	(.080)	.247	(.109)
33. Primary metals	.352	(.076)	.298	(.064)
34. Fabricated metals	.296	(.045)	.024	(.096)
35. Machinery	.349	(.056)	.009	(.084)
36. Electrical machinery	.197	(.032)	-.019	(.081)
37. Transportation equipment	.220	(.032)	.165	(.051)
38. Instruments	.248	(.049)	.047	(.162)
L	.028	(.014)	.039	(.014)
Industry dummies			yes	
State dummies	yes		yes	
R^2	.789		.873	
σ_u	.0782		.0622	

NOTE: $(K/L)_i$ equals K/L for the corresponding industry and zero for all others.

when they are estimated separately). One conclusion that I draw from this table is that the estimates not containing the industry dummies (in Table 5) may be the better ones, even if they do have a higher residual variance.

Another important conclusion which comes out of this table is that our estimate of economies of scale survives the industrial disaggregation. It is somewhat smaller but of the same order of magnitude as the estimates which impose the same slope in the output-capital relationship in Table 5. Thus, it is not a product of the disregard of industrial differences. Nevertheless, this finding of economies of scale is subject to an important reservation: we have very little relevant variation in scale in our data to determine this effect. We get it from interstate differences in the average size of establishment within industries. The average size does not differ much, and we do not have direct observations on large and small plants separately.[48] A definitive treatment of this issue awaits the availability of better data.[49]

Another difficulty arises from the fact that the Cobb-Douglas form of the production function is not very well suited to the analysis of economies-of-scale problems. It imposes one particular degree of economy over the whole range of sizes and observations. But this is not what one would expect either on theoretical grounds or on the basis of previous evidence. Most of the empirical evidence on cost functions seems to imply a slowing down of the rate of increasing returns with size.[50] To investigate this possibility one needs data, however, which can be separated into size classes.

Data of this sort are available from the 1954 *Census of Manufactures,* which provides a cross-tabulation by ten size-of-establishment classes, "size" being measured by the average number of employees. The main drawback of these data is the absence of relevant capital data. The Census does provide information on a variable, "aggregate horsepower of power equipment," which may be taken as a proxy for capital, but one should not expect very strong results using it. Nevertheless, since we are interested at this point mainly in scale relationships, we shall over-

[48] Data by establishment size (number of employees) are available in 1958 for value added and labor but not for the capital variable.

[49] If and when the Census Bureau completes its Time Series of Establishments project and provides some access to it, it will open up a new and very valuable set of data bearing directly on this question.

[50] See, for example, Johnston (1960) and Nerlove (1963).

look the possible poorness of this capital proxy variable, and carry out an analysis similar to the one performed on 1958 data, but using the size dimension instead of the interregional dimension to provide us with variation within industries.[51]

The results for 1954 are presented in Table 7. As expected, the horsepower coefficient is lower than the capital coefficient in 1958. But the size dummies are the interesting part of this table. Without any size dummies we get an estimate of economies of scale of about the same order of magnitude as in the 1958 data (.043). Substituting the size dummies for the economies-of-scale variable, allowing thereby nonlinear and abrupt changes in the size effect, we get the interesting finding of no significant size effect over the lower range of size classes, but a rising and significant size effect from about the 250–499 employees class and on. This result survives the introduction of industry dummies and $(HP/L)_i$ industry interaction dummies, both separately and together. The first four size coefficients are almost never significantly different from zero, while in all forms there is a consistent and significant increase in the size coefficients above the 100–249 employees class. This indicates that a constant rate of increasing returns is not a bad approximation for the upper size classes which account for most of the value added in manufacturing. But it does not fit the lowest size classes, which do not exhibit as large diseconomies as would be predicted by this constant rate assumption.[52] In any case, there is no evidence for a sharp J-shaped form. The implied average cost function is shaped more like half of a saucer with a relatively wide brim.

The third regression in Table 7 confirms this impression. It introduces both a constant economies-of-scale variable (L) *and* size class dummies. In this case the estimated average rate of increasing returns is higher, .089 instead of .043, but now the larger size class coefficients are all not significantly different from zero, while the smallest ones are significant and *positive*. This implies that the .089 rate reflects quite well the rate of increasing returns in the upper size classes, but under-

[51] The data are taken from *Census of Manufactures, 1954,* Vol. I, Chap. 3, Table 1. All the variables are defined as in Section II, except that installed horsepower is used instead of the various capital measures available in 1958. Data on ten size classes and twenty industries (including Industry 39) are used to generate a total of 182 observations. Since there were few observations in the largest-size class, the largest-size dummy is defined to correspond to the two largest-size classes.

[52] This finding may possibly reflect the distinctly poorer quality of data for the small establishments.

TABLE 7

Production Function, U.S. Manufacturing, 1954

(N = 182)

Coefficients of:	Regression Containing: No Size Dummies		No Other Dummies				Industry Dummies		$(HP/L)_i$ Interaction Dummies		Both Industry and Industry $(HP/L)_i$ Interaction Dummies	
HP/L	.104	(.015)	.102	(.016)	.100	(.016)	.029	(.018)				
L	.043	(.007)			.089	(.038)						
Size dummies:												
1-4			-.009	(.032)	.126	(.076)	.024	(.021)	.025	(.022)	.055	(.064)
5-9			.040	(.032)	.131	(.058)	.017	(.020)	.020	(.020)	.015	(.046)
10-19			.075	(.031)	.071	(.047)	-.003	(.020)	-.002	(.020)	-.006	(.034)
20-49			-.018	(.031)	.012	(.036)	-.019	(.019)	-.019	(.019)	-.012	(.022)
50-99			.000[a]		.000[a]		.000[a]		.000[a]		.000[a]	
100-249			.029	(.031)	.000[b]		.037	(.019)	.038	(.020)	.042	(.022)
250-499			.060	(.031)	.000[b]		.070	(.019)	.072	(.020)	.089	(.032)
500-999			.078	(.031)	-.007	(.034)	.092	(.019)	.094	(.020)	.111	(.042)
1000 +			.127	(.028)	-.006	(.049)	.131	(.018)	.134	(.018)	.146	(.062)
R^2	.338		.387		.392		.788		.784		.854	
σ_u	.0972		.0954		.0947		.0589		.0599		.0522	

Note: $(HP/L)_i = (HP/L) \times I_i$ where I_i is the appropriate industry dummy.

[a] Assumed to be zero by definition.

[b] Not significantly different from zero. Actual coefficient not estimated, since the program did not enter this variable for failure to meet tolerance requirements.

predicts productivity in the lower size classes (overestimates average costs). Here the lower size dummies adjust for the imposition of a constant rate of scale economies. We conclude, therefore, that the rate of economies of scale is apparently not constant. Rather, it is approximately zero for the lower size classes, but significantly positive for the larger size classes. Imposing the *same* rate on the whole sample *underestimates* the economies of scale in the economically relevant range (in the size classes where most of the output is being produced).[53]

A similar phenomenon can be also detected in the 1958 data. Here we do not have separate data by establishment size, but we do know how many of the establishments, which comprise our averages within a state and industry, have more than 20 employees. A crude size mix variable is given by P Large (number of establishments with 20 or more employees)/(total number of establishments). A believer in economies of scale may anticipate a positive coefficient for this variable, implying a higher average productivity in the larger establishments. But we, having examined the 1954 results, are forewarned.

Introducing the P Large variable into the 1958 regression ($N = 417$) yields:

$$\log(V/L) = \underset{(.020)}{.374} \log(K/L) + \underset{(.019)}{.127} \log L - \underset{(.048)}{.157} \text{ P Large}$$
$$- \underset{(.048)}{.066} \log R + \underset{(.106)}{.848} \text{ Occup.} - \underset{(.131)}{.710} \text{ Age} + \underset{(.055)}{.108} \text{ P White}$$
$$- \underset{(.037)}{.049} \text{ P Female}; \; R^2 = .706$$

and

$$\log(V/L) = \ldots \underset{(.019)}{.348} \log(K/L) + \underset{(.019)}{.109} \log L - \underset{(.047)}{.083} \text{ P Large}$$
$$- \underset{(.047)}{.083} \log R + \underset{(.101)}{1.465} \text{ Occup.} - \underset{(.150)}{.304} \text{ Age} + \underset{(.056)}{.054} \text{ P White}$$
$$- \underset{(.037)}{.069} \text{ P Female} + \text{state dummies}; \; R^2 = .760$$

resulting in an increase in the estimate of the over-all economies of scale from .05 to above .10 and a *negative* coefficient for P Large. Since P

[53] In 1954 establishments with more than 100 employees comprised only 9 per cent of the total number of establishments, but accounted for over 78 per cent of total value added in manufacturing.

Large = 1 – (P Small), this implies a positive coefficient for the below-20-employees size class. This is another indication that there may be no gains to be had from growing from a very small plant to a somewhat larger one. The economies of scale are largely in the medium- to large-scale plant range. If this is true, the estimates of economies of scale presented in Table 5 are probably too low.[54]

Two caveats are worth reiterating before we leave this section. (1) We have done nothing in this paper about the simultaneity problem. Since what we could have done (using W and lagged L as instrumental variables) would have made very little difference, it did not seem worth doing.[55] (2) The year 1958 was one of mild recession, and our results may be affected by differential underutilization of capacity.[56] But our results, to the extent that they are comparable, do not seem out of line with those of Hildebrand and Liu, who used data from the peak year of 1957.[57] Nevertheless, since all econometric results are no better than the data that went into them, these caveats should be kept in mind in evaluating the above findings.

V. Implications for the Measurement of Technical Change in Manufacturing

The conventional measure of residual technical change in an industry is given by

$$\lambda = y - w_k k - (1 - w_k)n$$

[54] Unfortunately, since the P Large variable does not survive the introduction of industry dummies, the above statement is not definitive.

[55] I have used the instrumental variable procedure in analyzing cross-sectional data for total manufacturing without any significant difference in the results. See Griliches (1964c). An attempt to use the indirect least-squares method led to nonsense results.

[56] This may explain the relatively poor performance of the petroleum products industry, which seems to have suffered the largest relative decline in value added from 1957 to 1958. That, however, may also be a reflection of certain incomparabilities between the 1957 Annual Survey and the 1958 Census.

[57] We have comparable data for 372 observations in 1957. They yield:

$$\log (V/L)_{57} = \underset{(.017)}{+.369} \log (K/L)_{57} + \underset{(.012)}{.074} \log L_{57} + \underset{(.103)}{.643} \text{ Occup.} + \underset{(.060)}{.155} \text{ P White}$$

$$- \underset{(.031)}{.105} \text{ P Female}; \; R^2 = .723, \; \sigma_u = .0838,$$

which is not out of line with the more detailed results for 1958 presented in Table 5.

where y, k, n are percentage *rates* of growth in output, capital, and labor respectively, and w_k is the share of capital in total factor payments. This procedure assumes that all the variables are measured correctly, that all the relevant variables are included, and that factor prices represent adequately the marginal productivities of the respective inputs. The last assumption is equivalent to the assumption of competitive equilibrium and constant returns to scale. To analyze λ, the unexplained part of output growth, it is useful to rewrite this equation in terms of a more general underlying production function: [58]

$$\lambda = w_k(k^* - k) + (1 - w_k)(n^* - n) + (w_k^* - w_k)(k^* - n^*) + h[w^*k^* + (1 - w^*)n^* - f] + a_z z + u$$

where $w_k^* = a_k/(a_k + a_n) = a_k/(1 + h)$, with $h = a_k + a_n - 1$; a's are the true elasticities of output with respect to the various inputs; starred magnitudes are the correctly measured versions of the variables; f is the percentage rate of growth in the number of establishments; and z is the rate of growth in inputs which affect the production function but are not included in the standard accounting system. These could be services from the cumulated stock of past private research and development expenditures or services from the cumulated value of public (external) investments in research and extension in agriculture, or measurable disturbances such as weather or earthquakes. The first term measures the effect of errors in the conventional capital measures on the estimated "residual." The second term reflects errors in the definition and measurement of the labor input. The third term reflects errors in the measurement of the relative contribution of labor and capital to output growth. It would be zero if factor shares were in fact proportional to the respective production function elasticities. The fourth term is the economies-of-scale term. It would be zero if the sum of the coefficients were unity *or* if the rate of growth in the number of firms just equaled

[58] See Griliches (1964) and Griliches and Jorgenson (1965) for a more detailed exposition of this approach. The equation in the text can be derived from the production function-based statement

$$y = a_k k + a_n n + (1 - a_k - a_n)f + a_z z + u;$$

and the definitions $h = a_k + a_n - 1$ and $w_k^* = a_k/(1 + h)$. Here it is assumed that z is an external variable, and economies of scale are defined not to include it. It is easy to rewrite this, making the alternative assumption that y is homogeneous of degree $1 + h$ in *k, n, and z.*

the weighted rate of growth of total inputs.[59] The fifth term reflects the contribution of left-out variables, while the last term is the "pure" residual term—the amount of output growth not accounted for by this expanded list of possible sources.

Assuming that the a_i's are constant over time implies the assumption of a Cobb-Douglas form for the underlying production function and of neutral residual technical change. This equation can be also adapted to the CES form by introducing an additional $[k^* - n^*]^2$ term.

Given this framework, the purpose of econometric estimates of the production function is (1) to test or validate a particular way of measuring an input or adjusting it for quality change; (2) to test and estimate the role of left-out inputs such as research and development; (3) to estimate the rate of economies of scale; (4) to check on the possibility of disequilibrium and estimate the deviation of the "true" output elasticities from the observed factor shares; and (5) to check on the appropriateness of the assumed form of the production function and the related implicit assumptions used in constructing the various productivity indexes. In this paper we have only begun to approach some of these goals. We have some evidence on items (1), (3) and (5). We have done nothing about (2), and our evidence on (4)—the relative magnitude of capital and labor elasticities—is too weak to provide us with any useful conclusions at this point.

The effects of the findings we do have on the explanation of growth in U.S. manufacturing during 1947–60 can be illustrated using some figures derived in earlier work.[60] For this period the standard approach yields:

$$\lambda = y - w_k k - w_n n = 3.22 - .272 \times 3.33 - .728 \times .46 = 1.98$$

where .728 is the average share of payments to labor in total GNP originating in manufacturing (after an adjustment for indirect business taxes) during this period, n is a measure of man-hours, and k is a measure (of the rate of growth in) of the net stock of fixed capital in manufacturing.[61] Thus, the conventional productivity measure attributes more than

[59] This emphasis on plant or firm scale economies distinguishes this formulation from several other attempts to discuss this issue at the aggregate level. E.g., both Walters (1963) and Westfield (1964) proceed as if there were economies of nation or industry size, irrespective of the number of establishments over which the particular aggregate output is spread.

[60] See Griliches (1963c).

[61] See the Appendix for the sources and derivation of these and subsequent numbers.

60 per cent of the observed rate of growth in manufacturing output to the unexplained category of technical change.

What more can we say about this on the basis of our econometric results? First, on the basis of the results reported in Section III, the Cobb-Douglas assumption underlying the use of a fixed or average weight is not a bad approximation, though we have no evidence from the one cross section on whether this production function has shifted in a non-neutral way over time. Second, we have some evidence of mildly increasing returns to scale, to which we shall return after we discuss the problem of adjusting the inputs series for quality change. The third and major finding is that the quality-of-the-labor-force dimensions are important and that a base period income-weighted occupation mix variable enters into the production function with a coefficient that is not significantly different from the man-hours coefficient. Hence, it can be treated as a multiplier to the conventional labor force figures.

The other major labor force quality variables—percentage white, percentage female, and median age—changed very little in the aggregate over this time period, and will not be discussed further here.[62] At the aggregate level, however, we do have more detailed data not only on the occupation mix, but also on the educational level of each occupation. From these data we can construct a more detailed index of quality per man. From the data on "education by occupation" and on "occupation by industry" a distribution of employed males in manufacturing by number of school years completed was constructed using the second set as a source of weights for aggregating the first set. The resulting distributions (see Table 8) were weighted using 1959 mean incomes of *all* U.S. males by school years completed as weights to yield a weighted education-per-man index that rose from 100 in 1947 to about 113 in 1960, or at the approximate rate of 1.0 per cent per year.[63]

[62] Between 1950 and 1960, using *Census of Population* data, P Female increased from 24.9 to 25.3 in manufacturing, P White (of males) remained unchanged at 92.3, and the median age of employed males in manufacturing rose from 38.4 to 39.6 years.

[63] The principle behind this index is the same as for any other quality change adjustment. In some period we are able to observe a relationship between differences in some dimension(s) of a commodity and the price that these different dimension bundles fetch in the market place. From this information we are able to derive the "price(s)" of these dimensions, and we can then use these prices (incomes) to adjust for the changes that have occurred in the dimensions (education) of the commodity (labor) over time. That education enters into the production function in this particular form was shown for agriculture in Griliches (1963a). The 1947 figure in the text is based on an interpolation between the 1940 and 1950 values in Table 8.

TABLE 8

Schooling of the Labor Force: U.S. Manufacturing, Employed Males, 18 Years and Over, 1940, 1950, 1952, 1957, 1959, and 1962

Years of School Completed		Distribution by School Years Completed[a] (per cent) 1940	1950	1952	1957	1959	1962	Weights: Mean Income of Males 25 Years and Older, 1959[b]
Elementary	0	3.0	1.6	1.7	1.7	0.9		$2,092
	1-4	7.0	7.2	5.8	4.5	4.1		2,487
	5-7	15.9	15.8	14.1	12.2	10.9		3,552
	8	28.1	20.9	20.2	17.5	16.6		3,893
High school	1-3	19.1	20.9	21.4	22.1	22.7		5,412
	4	16.9	20.7	23.5	27.0	28.2		6,334
College	1-3	4.8	5.8	7.0	7.2	8.0		7,642
	4+	4.3	5.6	6.3	7.8	8.6		10,222
n.r.		0.9	1.5					
Index of school years completed per man[c] (1950=100)		94.7	100.0	103.3	107.7	110.5	113.1[d]	

n.r. = not reporting.

[a]The distributions are constructed by weighting the distributions of school years completed by occupation (usually 8 classes: professional, managers and proprietors, clerical, sales, craftsmen, operatives, service excluding domestic, and laborers excluding farm and mine) using occupation by industry weights. Education by occupation data are taken for 1940 and 1950 from the "Occupational Characteristics" volumes of the respective *Censuses of Population;* 1952 and 1957 from *Current Population Reports,* Series P-50, Nos. 49 and 78; 1959 and 1962 from *Special Labor Force Reports* Nos. 1 and 30. Weights are from the "Occupation by Industry" volumes of the respective *Censuses;* weights for other years interpolated on the bases of "occupation-by-industry" distributions for *all* workers (including female) from the annual *Labor Force Reports.* The "less than 5" class in 1940, 1952, and 1957 was broken down into 0 and 1-4 classes using the distribution by single years of school completed of all urban males, 25 years and over, given in the "Detailed Characteristics" volumes of the respective *Censuses* (1950 Census data were used to break down the 1952 distribution and 1960 Census data for the 1957 distribution.) The 7-8 class given in the 1940 distribution was similarly broken down using the 1940 Census data on school years completed (by single years) by urban males.

Notes to Table 8 (concluded)

[b]Average income of all U.S. males, 25 years old and over, by school years completed. Computed from *Census of Population, 1960,* PC(1)ID, Table 2223, using the midpoints of the income classes and $20,000 for the $10,000 and over class.

[c]The product of the respective column with the weights (average income) column, divided by 100, adjusted for the nonreporting class by dividing through (100 – n.r.) /100, and expressed as an index to the base 1950 ($5,122).

[d]The 1959-1962 comparison is based on a somewhat different and more detailed occupational breakdown. The comparable distributions are:

	School Years Completed								
	Elementary				High School		College		
Year	0	1-4	5-7	8	1-3	4	1-3	4	5+
1959	.9	3.7	10.4	16.6	22.1	29.2	8.4	5.5	3.2
1962	.7	3.4	10.2	14.2	21.7	30.7	9.6	6.0	3.5

For the new classes the mean incomes are $9,386 for the four-year college class and $11,295 for the 5+ class. These were derived by interpolating the figures in the last column on the basis of more detailed data given by H. Miller, *Trends in Income Distribution in the U.S.* (forthcoming Census-Social Science Research Council monograph). The resulting estimate of a 2.3 per cent change in "education per man" in U.S. manufacturing between 1959 and 1962 was linked to the previous results leading to the 113.1 figure given in the body of the table.

Combining this with the man-hours index, we find that total labor inputs (quantity as well as quality) grew at the rate of 1.46 per cent per year.

No mention has yet been made of the very difficult problem of the correct measurement of capital services. Even though our work above throws little light on this subject, we shall digress at some length on this topic now, since it is of great importance to the correct accounting of output growth over time.

Conventional measures of capital, such as the one used in the numerical example above, suffer from several shortcomings: (1) they overdepreciate, that is, they assume that the services derived from a piece of capital equipment deteriorate too rapidly with age; (2) they measure the stock of capital rather than the flow of services from it; and (3), they overdeflate it; they use the wrong price indexes for converting capital in current prices to a measure "in constant prices."

What is needed from the production function point of view is a measure of the flow of services of capital in constant prices. One of the main problems to be faced in constructing such a measure is what assumption we are to make about how the services of a given machine behave as it ages. The usual assumption is that they decline rather rapidly, and this assumption is buttressed by the observation that the value (price) of a machine declines rapidly as it ages. But the value of old machines will decline because their expected life span is declining, because better new machines have become available, and because the quality of their services deteriorates as they age. Only the last one is a legitimate deduction to be made from a *service*-oriented measure of capital. It is true that there is less life left in an old machine, but that does not mean that its product during the current year is necessarily any worse for that. It is true that the availability of better new machines will result in capital losses by the owners of old machines, but this does not make the old machines any *worse;* it only makes the new ones better.

Instead of the usual sharp depreciation assumptions, I shall make the opposite assumption that the services of a machine do not decline at all (or very little) as long as it is in operation. To make this assumption a little bit more realistic, I shall use relatively conservative estimates of the length of life of machines. Given this assumption, the flow of capital services is proportional to the *gross* (undepreciated) stock of capital. During the 1947–60 period, the gross stock of capital in manufacturing grew at the rate of 4.2 per cent per year, as compared to the 3.3 per cent per year rate of growth in the net stock of capital.

While services are proportional to gross stock for a machine of a given length of life, this is not true if we want to add together machines with different lengths of life. A $100 machine that will last five years will have roughly twice as large an *annual* flow of services (in dollars) than another $100 machine whose expected length of life is ten years. Thus, the shorter the life expectancy, the higher is the ratio of services to stock. In manufacturing we can identify two major components of capital formation: equipment and structures. Structures have a much longer life than equipment and hence should be given a lower weight in compiling an index of capital *services*. Since the stock of equipment has grown more rapidly recently, this adjustment makes a substantial difference to our measurement of the growth in the total level of capital services. A service flow measure of capital in manufacturing grows at the rate of 4.7 per year, as compared to 4.2 for the gross stock and 3.3 per year for the net capital stock measure.

All of our capital measures are based on a cumulation of deflated investment figures. But some of the price indexes used to deflate investment goods are quite bad.[64] This is particularly true of the indexes used to deflate construction expenditures (our structure's component). These are not even "price" indexes. They are "cost" indexes (input rather than output price indexes), allowing for no improvements in the productivity of the construction industry. They do not price some well-specified factory buildings or houses, but simply average construction worker wage indexes, cement price indexes, lumber price indexes, and so forth. In this whole field there is only one decent price index available at the moment, the one computed by the Bureau of Public Roads, based on bid prices for federally supported highway construction. It prices such well-specified units as "a cubic yard of dirt excavated," "a square foot of concrete laid," and "a pound of structural steel put into place." If we use this index to deflate construction expenditures and recompute our estimate of service flows accordingly, we find that they grew at the rate of 5.6 per cent per year.[65]

Even though equipment price indexes do try to price output rather than inputs, they also do not take quality change into account very satisfactorily. Our equipment price indexes are all components of the Wholesale Price Index, on which much less resources are spent than on the Consumer Price Index, and whose quality change adjustments are much less frequent and looser. If we were to assume that both the rate of quality change and the forces determining the longer-run price levels have been roughly the same for consumer durables as for producer durables, we can get an idea about the possible magnitude of the quality bias in the WPI machinery price indexes by comparing them with the appropriate components of the CPI. From 1947 to 1960, the WPI index for machinery and motive products rose by 38 per cent *relative* to the consumer durables price index, or about 2.5 per cent per year.

[64] See Griliches (1963b) for more details.

[65] Ideally we would want an index pertaining to total construction rather than just to highway construction. An improved *price* index for all contract construction has been recently developed by Dacy (1964). Unfortunately, it covers only the 1947–61 period. During this period, however, it moves very much like the BPR index of highway construction prices.

I think that we are quite safe in making the assumption that the quality of producers equipment has been improving at the rate of *at least* 1.0 per cent per year.[66] If we were to incorporate this assumption into our measure of capital services, we would find that it grew at the rate of 6.2 per cent per year during the 1947–60 period. Nevertheless, since this last adjustment is not based on conclusive and direct evidence we will not use it in the final growth accounting to be presented below.

Having gone as far as we could in the direction of approaching the correct input measures, we can now return to the estimated role of economies of scale. It is quite small. Since we estimate h, the excess of the sum of the coefficients over unity, at about .05, and the rate of growth in the *number* of establishments (f) during this period at 1.2 per cent per year,[67] the total effect is

$$h(w_k k^* + w_n n^* - f) = .05(.27 \times 5.6 + .73 \times 1.46 - 1.2) = .07$$

or less than one-tenth of a percentage point.

These various adjustments are brought together in Table 9. It can be seen from this table that they reduce the contribution of the residual from about two-thirds to less than a fifth of the measured rate of growth in manufacturing output. The single largest adjustment is for the changing quality of labor, accounting for over a third of the measured residual. Thus by a more careful accounting we have been able to eliminate a substantial part of the unknown, assigning the bulk of it to improvements in the quality of labor and capital. This has been accomplished not by just renaming "technical change" as "quality change," but by actually going out and getting independent and nontautological estimates of the various components. This is a real gain in the explained fraction of growth.

Of course I may have overestimated the importance of some of the included factors, and left out some other important sources of growth.

[66] This is quite conservative. The scattered studies of quality change bias, in automobile and tractor prices indexes reviewed in Griliches (1963a and b), support this view. Note also that Solow (1962) estimates the rate of "quality improvement" in new capital at 3 per cent per year.

[67] This 1.2 per cent figure is based on the growth of the total number of establishments in manufacturing between 1947 and 1958 or the growth in the number of establishments with 20 or more employees between 1954 and 1963. The results of either computation are very much the same. The total number of establishments in manufacturing in 1963 was not yet available at the time this was being written.

TABLE 9

Output, Input, and Residual Measures of Technical Change: U.S. Manufacturing, 1947-60

(per cent per year)

Category	Rate of Growth	Weighted Rate of Growth[a]	Residual "Technical Change"[b]	Residual as Per Cent of Output Growth
1. Output	3.22	3.220		
2. Man-hours	0.46	0.335		
3. Net stock of capital	3.33	0.906		
4. Residual (conventional)			1.98	61
5. Service flow, gross stock adjustment	1.36	0.370		
Residual (adjusted)			1.61	50
"Quality" adjustments:				
6. Schooling per man	1.00	0.728		
7. Bias in construction deflators	0.90	0.244		
Residual (adjusted)			0.64	20
8. Economies of scale	1.38	0.069		
Residual (adjusted)			0.57	18

Note: See Appendix for sources and derivations. The adjustments are cumulative; i.e., the gross service flow measures of capital in manufacturing grew at the rate of 4.69 per cent per year. What is recorded in the table is the excess over the rate of growth in the conventional measure: 4.69 - 3.33 = 1.36.

Line 5: See Appendix for details.

Line 6: See Table 8 (1947 value interpolated from figures for 1940 and 1950).

Line 7: Line 5 concept recomputed using the Bureau of Public Roads construction price index to deflate the investment-in-structures component.

Line 8: The weighted rate of growth of total corrected inputs, from lines 2 through 7, is 2.583. Subtracting the estimated rate of growth in the number of establishments of 1.2 per cent per year, leaves 1.383 as the estimated rate of growth in the average scale of enterprises. Applying to it the 0.05 estimated rate of returns to scale, yields 0.069 as its contribution to the rate of growth of output.

[a]Labor measures multiplied by 0.728, capital measures multiplied by 0.272; their respective average factor shares during this period.

[b]Computed from appropriate entries in second column; e.g., residual (conventional) = 3.220 - 0.335 - 0.906 = 1.979.

In particular, I have done nothing in this paper about the possible effects of the growing level of research and development expenditures on output (except to the extent it has already reflected itself in the quality change measures).[68] This paper does not pretend to completeness or definitiveness. It is only a beginning, an attempt to illustrate the possibility and profitability of an alternative approach to the problem of sources of growth. Much hard work still remains to be done to pin down their individual contributions adequately.

The approach outlined above may not appear to be all that different from the conventional one. It also uses a Cobb-Douglas-type production function and the concept of embodiment, the main difference being in that we allow for "embodiment" of technical change in *both* capital and labor. But the use and purpose of the model are different. The Cobb-Douglas type framework (with or without embodiment) is usually used to *estimate* the rate of technical change. In our approach, it is used as an organizing device, as an accounting framework for putting together different *direct* estimates of technical change embodied in *particular* inputs. Thus, by focusing the question on where and how the quality (technical) changes occurred, it provides in some cases a handle for affecting the rate of technical change directly, while in others it at least points to areas where research on the particular sources of these quality changes is likely to pay off. For example, if one accepts the finding that growth in education per man (and not in some other variables that are correlated with education) is responsible for over a quarter of the observed rate of growth in output per man-hour, then we do in fact know how to affect this variable, what it may cost, and what the returns are likely to be (both in absolute terms and in their effect on the rate of

[68] A reasonable adjustment for the contribution of R&D would eliminate all of the remaining residual. The effect of R&D growth can be approximated from the formula

$$a_R \dot{R}/R = rs$$

where R is a measure of the *cumulative* amount of knowledge-capital arising out of R&D investments, a_r is the elasticity of output with respect to an increase in this knowledge measure, r is the gross rate of return to R&D investment, and $\dot{s}$ is the ratio of *net* investment in knowledge to output. The ratio of gross investment (R&D) in knowledge to output in 1958 was about .07. Assuming that half of it was for maintenance and replacement implies $s = .035$. Assuming $r = .2$, which is consistent with whatever scattered work there is on this subject [for the most important contribution and references to other work, see Mansfield (1964)] gives .007 as the contribution of R&D, or more than half of a percentage point of the observed rate of growth of output.

growth). Similarly, the adjustment for bias in capital deflators tells us that we may have been underestimating substantially the actual growth in capital that is the result of a given amount of saving. Moreover, by disaggregating further (beyond just structures and equipment) and finding out which are the items whose quality has been increasing, we may come closer to being able to affect the aggregate rate of productivity growth. This approach is, of course, very much more *ad hoc* and requires much and rather detailed data, but in economics as in most other fields it is difficult to get something for nothing.

VI. Concluding Remarks

The above is an installment from a relatively large and long-range research program. As such it has no clear beginning or end. Most of the findings must be interpreted as maintained hypotheses supported by data recently examined and to be tested further on additional data now being collected. Most of the production function work will be tested and expanded as soon as the complete results of the 1963 Census become available. The work on capital deflators and on the contribution of R&D require entirely different sets of data and possibly an entirely different approach.[69] In the meantime, there is a danger that here, as in much of other research, we may be looking for answers where the data are and not where the questions are important.

Appendix
Data: Sources and Adjustments

The data on manufacturing GNP in constant and current prices and the share of labor costs (after an adjustment for indirect business taxes) are all taken from the October 1962 issue of the *Survey of Current Business* (pp. 6–18).

The man-hours figures are taken from BLS Bulletin 1249, *Trends in Output Per Man-Hour,* and from subsequent BLS releases.

The "conventional" net stock estimates are from Wooden and Wasson, "Manufacturing Investment since 1929," *Survey of Current Business,* November 1956 and from subsequent issues of the *SCB.*

[69] On the contribution of R&D the major work is being done by Mansfield. I have done some work on the deflator problem [see Griliches (1963b)], but a definitive treatment awaits better data and is outside the scope of an individual research project.

The various capital series are derived from unpublished Department of Commerce data underlying the estimates presented in the November 1962 issue of *SCB* (pp. 9–18).

The lengths of life assumed here are the same as in the above cited source: seventeen years for equipment and forty years for structures. A constant 5 per cent per year rate of interest was assumed in converting the gross stock estimates into service flow. The conversion was accomplished using the "annuity" approach; i.e., first it was asked: What is the present value of a $1.00-per-year annuity that lasts for (say) seventeen years? If the discount rate is 5 per cent, the answer is $11.27. Next, one finds the number of such annuities that could be bought for (have the present value of) $100.00. It is 8.87 (100/11.27). Thus, the gross stock estimate for equipment was multiplied by 0.0887 and the gross stock of structures was multiplied by 0.0583 (which answers the same question for a forty-year annuity), and the two resulting series were summed to arrive at an estimate of the *flow* of capital services in constant prices.

The BPR price index of highway construction for 1922–60 is taken from U.S. Department of Commerce, *Price Trends for Federal Highway Construction,* various issues. It is extrapolated back to 1908 on the basis of the implicit GNP deflator.

The derivation of the education variable is given in greater detail in Table 8. It is the result of weighting the distribution of all employed males by school years completed, by the mean 1959 income of males (over 25) in the appropriate educational categories. The resulting variable can be thought of as the income predicted by the current educational distribution and the base period incomes for the respective educational subcategories. Actually, it is very close to a "mean school years per man" concept except for a nonzero weight for the zero education class and somewhat higher weights for the higher education categories.

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COMMENT

RONALD G. BODKIN, University of Western Ontario

I would like to begin my comments by congratulating Professor Griliches on an excellent piece of scholarship. What in my view makes this an excellent paper is Griliches' discontent at allowing the large residual that has been termed "technical change" to go unanalyzed and his consequent attempt to provide some type of explanation for it, in terms of more adequate measures of the conventional inputs, unconventional inputs, and increasing returns to scale. In this paper, he largely confines his analysis of the unconventional inputs to education embodied in the labor force, although there is a suggestion that research and development outlays may explain a large part of the remaining residual, which itself is only 18 per cent (instead of 61 per cent) of the growth in U.S. manufacturing output over the period 1947–60. (Thus the role of research and development expenditures in manufacturing industry may parallel the effects of research and extension expenditures on agricultural productivity, which Griliches found to be of considerable quantitative importance in his recent *American Economic Review* paper on agricultural production functions.) In what follows, I shall, at particular junctures, be mildly critical of some of Professor Griliches' techniques or interpretations. These specific issues should be interpreted as merely mild differences of opinion, for I regard this paper, like his earlier *American Economic Review* article on agricultural production functions, as an excellent piece of research.

The first specific comment I would like to make concerns the size of the coefficients of the occupation variables in Table 5. Professor Griliches' discussion implies that he believes that the universe coefficient of this variable is approximately the same as that of the ordinary labor variable —that is, that quantity and quality of labor enter multiplicatively in the specification of the production function. Griliches asserts that this hypothesis is confirmed until the introduction of the industry and/or state dummies reduces the (relevant) variance of this variable to an amount too small to allow this effect to show up. An alternative hypothesis might be that not all of the improvement of the quality of labor represented by education and embodied in the work force is labor-augmenting technological progress. In the discussions this morning, it was pointed out that technological progress embodied in a factor of production need not result in technological progress that could be described as augmenting for that particular factor of production. Thus technological progress embodied in the labor force might not be labor-augmenting, or at least not all of its effects need show up as a multiplicative factor to quantities of "raw" labor. If this is so, then we have an alternative explanation of the lower coefficient of the occupation variable, which (if the standard errors be accepted at face value) is significantly different (or almost so) from the hypothetical equality with the implied coefficient of the (almost) entirely physical input of labor.

I have some reservations about one of the tests that Professor Griliches employed in deciding to fit a Cobb-Douglas production function, instead of the slightly more general constant-elasticity-of-substitution (CES) form. The regression of output (value added) per man-hour on the wage implies an equality, possibly disturbed by stochastic perturbations or lags in the adjustment process, between the marginal product of labor and its real wage. It seems to me that this sort of assumption is more appropriate in agriculture, the sector studied in Griliches' earlier *American Economic Review* paper, than in manufacturing, which is generally thought to be subject to imperfect competition and positions of market power in a number of subsectors. I personally would have preferred a test in which only cost minimization is assumed, that is, one in which the capital-labor ratio is regressed against the relative prices of these two factors, despite the fact that the data problems may be more severe with this kind of test. In any case, this shortcoming (if it be one) is not very severe, as Griliches also performs a direct test of the unitary elasticity of substitution implied by the Cobb-Douglas form, a test which is based not on a marginal productivity side condition but makes use of only a linear approximation to the CES form. Since the results of this test agree with those of the other test, one can agree that the Cobb-Douglas variant of the production function seems adequate to describe these data.[1]

Several other aspects of this very interesting paper deserve comment. Professor Griliches argues that time series studies of the production function deserve little weight in forming our considered view of what the world is "really" like, and this apart from the usual time series problems of autocorrelated residuals, errors of observation (including possible aggregation errors), and simultaneity of the relationships. Basically, the argument is that most time series studies of production functions remove the trend from the conventional variables, usually by including it (the time trend) as a separate explanatory variable to proxy for technical

[1] Griliches mentions that, because of the severe nonlinearity of the CES form, there are very few direct estimates of it that do not make use of a marginal productivity side condition. I cannot resist the opportunity to engage in a little self-advertising and mention that Professor Lawrence Klein and I will be presenting a paper at the meetings of the Econometric Society this winter in which we present some direct estimates of the parameters of this form, fitted to aggregative data for the U.S. economy. (This paper will be published in the *Review of Economics and Statistics.*) Professor Murray Brown has also obtained some direct estimates of the parameters of this type of production function, in a paper given at last winter's Econometric Society meetings, in which he tried to get directly at this thorny problem of the degree of monopoly power. But, again, one can hardly criticize Professor Griliches for failing to have knowledge of unpublished research.

change, and so the remaining relationship is dominated by short-term (cyclical) influences, which are likely to be quite different from those which are the subject of the standard production function model. I should like to emphasize that, if true, this conclusion would lead us to reject (or at least give very little weight to) most, if not all, of the studies of aggregate production functions of the past. As an invited discussant, I have felt an obligation to grapple with this conclusion, despite something of a self-preservative instinct to duck it. My present view is that Griliches is largely correct, although the situation may not be that bad when the sample period underlying the aggregative production function study is both long in term and homogeneous in character. It is also possible that the use of annual data may "wash out" some or most of the very short-term or transitory influences affecting the production relationships.

Another interesting feature of this paper is the attempt to test for technological progress that is embodied in capital goods of current (or recent) vintage, by including the ratio of the net stock of capital to the gross (a proxy for the average age of capital) as an explanatory variable in the production relations fitted. This variable was never statistically significant, thus providing little support for the embodiment hypothesis. However, the power of this test may not be very great, for reasons elucidated by Griliches. Still, it is interesting to note that Lithwick, Post, and Rymes, in their paper on Canadian production relations presented this morning, also tried a similar vintage variable (the average age of the net stock of capital) in attempting to explain investment behavior. They found no evidence of vintage effects on investment, which is of course not the same type of result as Griliches', but which is broadly consistent with his.

Professor Griliches also finds evidence of mild but statistically significant increasing returns to scale, which, as he notes, agrees rather well with the evidence on this point from aggregate production function studies. It is also interesting to observe that the evidence on this issue also survives a foray into data that are close to the individual establishments, although the Cobb-Douglas form of the production function does not appear to permit an adequate description of variations in returns to scale. (Here, as elsewhere, the Cobb-Douglas form would appear to be only an approximation, although better approximations are difficult to obtain!) If one accepts the existence of increasing returns to scale, especially in the range (that of the larger size classes) in which

Griliches found this phenomenon to be most important, this reinforces the view that pure competition is not likely to be very widespread in the manufacturing sector. Griliches also points out that the evidence from aggregate production functions with regard to increasing returns to scale in the individual firms or establishments (plants) of the economy is very indirect, as these aggregate production functions only measure (if they do that) changes in the scale of the economy, without making a correction for the number of firms or establishments operating within the economy. It is of course true that what Marshall used to call "internal technological economies" will be experienced (on average) only if the growth in the scale of the economy exceeds the growth in the number of producing units, i.e., only if the average producing unit grows in scale. However, to the extent that these aggregate production functions measure the Marshallian concept of "external [technological] economies," no correction for a changing number of producing units is necessary or even desirable.

Finally, I have some mild reservations about the "education per man" variable used in Table 9 to account for a portion of the "technical change" residual. The weighting of education classes by base-period income levels is appropriate only if these base-period income levels do actually measure relative (marginal) productivities among the groups so distinguished. But the correlation between education and income is rather tricky: those who have inherited large property incomes are also likely to have inherited a college education as a status right, which may have little to do with the bulk of the income that they receive. Even if one merely focuses on the correlation between education and earnings (labor income), it is not clear that we are dealing with a causal relationship. As Richard Nelson (following Burton Weisbrod) has argued in a paper to be given at tomorrow's session, education may merely serve to label an employee as possessing certain native characteristics that he required to complete his education (e.g., intelligence, docility, or industry) but may make little difference to his performance on the job. In this case, education simply serves to open up job opportunities which would have been closed if the individual did not possess the requisite number of years of formal schooling completed but which he presumably was capable of doing, nevertheless. The drift of these remarks might be an implication that Griliches has overestimated the growth of the relevant education variable for Table 9, and so the contribution of this factor to the explanation of "technical change," the growth in the residual, is too high.[2] (It might also be too high because the weight—taken to be the same as the labor coefficient—is too large, as I have discussed earlier.) On the other hand, this table takes no account of the external effects of education (that is, those that cannot be described as labor-augmenting effects), and these might well be substantial. Hence, I am unwilling to guess whether Griliches' estimate of the contribution of the education embodied in the manufacturing work force, to "technical change" in the manufacturing sector over the period examined, is too large or too small.

As I said at the outset, none of these remarks should be interpreted to mean that Professor Griliches' paper is anything other than excellent. My approach as a discussant has been the sandwich approach—a solid statement at the beginning, solid material at the end, with the Bologna in the middle.

JOEL POPKIN, Office of Business Economics [1]

In recent years, there has been a substantial increase in the resources we have devoted to attempting to quantify the factors causing technological change. We seem to have made considerable progress in the relatively short time since the appearance in 1958 of the studies of Niitamo and Wolfson, who first introduced into production analysis explicit measures of factors thought to underlie productivity change.[2] This excellent paper of Mr. Griliches, which focuses on manufacturing, together with his work in the agricultural sector undoubtedly represent an important contribution to this progress.

There are two brief points about this paper which I want to mention at the outset. First, I wish that Mr. Griliches could have shown more conclusively that the coefficient on his labor quality index did not differ significantly from estimates of labor's share. Certainly the introduction

[2] Edward F. Denison, in his *The Sources of Economic Growth in the United States and the Alternatives Before Us,* Washington, D.C., 1962, attempted to take a rough account of these factors by including only 60 per cent of the rise in a similar education variable as contributing directly to the growth of real output.

[1] These comments do not necessarily reflect the opinion of the Office of Business Economics.

[2] O. Niitamo, "The Development of Productivity in Finnish Industry, 1925–1952," *Productivity Measurement Review,* 1958, pp. 1–12; and R. J. Wolfson, "An Econometric Investigation of Regional Differentials in American Agricultural Wages," *Econometrica,* 1958, pp. 225–51.

of the dummy variables required for the analysis has served to reduce the coefficient below labor's share, but conclusive evidence that the coefficient would have taken on the appropriate value, in the absence of collinearity, has not been presented.

The second point is a conceptual one and probably does not bias Mr. Griliches' findings in any important way. From 1948 through 1960 there has been a net in-leasing of capital by manufacturers.[3] While slight, failure to account for this biases downward capital's contribution to the growth of output. In the time series analysis for certain manufacturing industries, failure to include changes in leased assets can lead to significant bias in the appraisal of the role of capital in the production process. Similarly, in cross-section work, bias can result from the substantial differences in the mix of leased and owned assets among industries. Perhaps, differences in asset leasing have partly contributed to the range of estimates of the elasticity of substitution which Mr. Mansfield has shown in his discussion of Mr. Nerlove's paper.

I would now like to focus the remainder of my discussion on Mr. Griliches' appraisal of the contribution of education to the growth of manufacturing output. Owing to the lack of suitable data, Mr. Griliches has had to use in his cross-section estimates an occupational mix variable as a proxy for educational mix. He feels that the two should be highly correlated but offers no evidence on the point. Data I have looked at cast some doubt on the strength of this correlation, certainly enough doubt to give us reason to suspect that the coefficient on the educational mix variable might not be close to labor's share. In 1959 the occupation "managers" had a mean income 7 per cent above that of "professionals," yet had 24 per cent less education (measured by the median) than professionals.[4] Craftsmen earned 13 per cent more than clerical workers, yet had 16 per cent less education. This last result suggests that other forms of education, such as apprenticeship programs, should be considered.

The ultimate point at issue here is the way in which formal education enters the production process. If one were to assume, quite conservatively, that rising educational levels influence the production process

[3] J. Popkin, "The Use of Wealth Data in Quantitative Economic Analysis," *1964 Proceedings of the Business and Economic Statistics Section, American Statistical Association*, pp. 346–51.

[4] Derived from *Census of Population, 1960,* PC(2)5B, Table 8, and *ibid.,* PC(1)1D, Table 208.

only through the changes which occur in the occupational mix—an assumption which Mr. Griliches' results clearly support since he used occupational mix as his quality variable—the influence of formal education on growth would be altered considerably. Indexes of occupational mix (percentage of employment accounted for by each occupation category weighted by 1959 mean income for that category) show only a 5 per cent shift toward higher skilled occupations between 1950 and 1960. These data are found in Table 1. This is less than half of the increase which Mr. Griliches found in his education index over time.

This apparently slower change in occupational mix than in the level of formal education has been recognized by other researchers. In particular, Folger and Nam find that an increasing amount of the rise in the level of formal education has been manifested in "within-occupation" educational advances, rather than in shifting the occupational mix toward

TABLE 1

Occupational Mix of Manufacturing Employees, 1950 and 1960, and Mean Income by Occupation, 1959

Occupation	Per Cent of Manufacturing Employment 1960 (1)	1950 (2)	Mean Income 1959 (3)
Professional, technical, and kindred workers	9.2	5.7	$8,601
Managers, officials, and proprietors, except farm	6.3	6.0	9,169
Clerical and kindred workers	6.5	6.6	4,926
Sales workers	4.5	3.6	6,337
Craftsmen, foremen, kindred workers	25.1	24.8	5,586
Operatives and kindred workers	37.2	39.5	4,396
Service workers, except private household	1.8	2.2	3,529
Laborers, except farm and mine	7.5	11.1	3,118
Occupation not reported	1.8	0.4	4,808

Source: Col. 1: *Census of Population, 1960,* PC(1)1D, Table 209; col. 2: *ibid., 1950,* P-C1, Table 134; col. 3: *ibid., 1960,* PC(1)1D, Table 208.

the higher skills. Obviously this finding need not weaken the hypothesis that all formal education abets economic growth if one can show that within-occupations income varies with formal educational achievement. However, this cannot be conclusively shown. In the 1960 population census, data were collected on the "within-occupation," "within-age-group" distribution of income by level of formal education.[5] These data show that the relationship between education and income is not as strong as might have been suspected from Griliches' Table 8, which is based on highly aggregative data. It is not uncommon to find, for instance, that within many occupations, workers who earn over $10,000 per year have less education than their occupational colleagues who earn $7,500 to $10,000. I was, also, quite surprised to find that the education-income relationship is weaker among salaried managers than among individual proprietors both of whom are in the managerial class. I originally had thought that the higher mean income of the managerial class than of the professional class reflected the influence of the individual entrepreneurs who pulled themselves up by their proverbial boot straps. Apparently the productivity of the captains of manufacturing industries, as reflected by their income, is not influenced by education to the degree to which the income of the owner of the corner store is.

The foregoing comments were not meant to question what Mr. Griliches feels is a major finding of his study—that differences in labor quality account for differences in labor productivity. Rather their purpose has been to point out that in the time series analysis of growth in Section V of his paper, the use of the growth of formal education may have resulted in an overestimation of the amount of economic growth explained by labor quality; and in a corresponding underestimate of the amount of growth rate residual which remains unaccounted for. Mr. Griliches has promised to tackle this residual in his future work, and it seems to me that it may be larger than he is willing to admit. I am sure that we all eagerly await future installments of this important research undertaking.

EVSEY D. DOMAR

Griliches claims that a part of the rate of growth of the residual can be explained by the improvements in the quality of capital left undetected by the deflation of capital formation in money terms by some conven-

[5] Derived from *ibid.*, PC(2)5B, Table 9.

tional price index of capital goods. A "correct" price index would rise less rapidly; hence the deflated capital formation and the capital stock would grow faster. I do not object to this procedure, except to point out that the resulting higher rate of growth of currently produced capital goods would increase the rate of growth of output as well. Of course, the textile industry does not produce its own capital goods; here Griliches is safe. But when his adjustment is applied to machine building the effect of the increase in the rate of growth of its output should be substantial and, I would expect, greater than that of the adjustment in its capital stock. So the unexplained part of the residual in this industry will increase rather than diminish. I do not know what the outcome might be in American manufacturing taken as a whole, but for the whole American economy (with a Cobb-Douglas function) the two adjustments (in capital formation and in the stock of capital) almost cancel each other, leaving a small difference with an uncertain sign.[1]

Hans Neisser

The term "rental payment" as used by Griliches in Section II, in defining *K*, clearly is a net term, not the price of the service of the capital good, or "gross rental," as charged by the owner of the equipment piece to the user. This follows from Griliches' adding to the rental value the items depreciation and depletion, insurance premium, and property taxes. These three items would be covered in equilibrium by the market price of the service (gross rental), and the net rental would equal the interest on the capital value of the piece plus a risk premium.

It may be noted that there is a type of contract in which the stipulated rent is the net rental. I refer to the contracts between owner and tenant of a large agricultural estate, as they have been usual in Europe for many centuries. In such contracts the tenant has two obligations: (1) to pay a stipulated annual rent, and (2) at the end of the contract, say after twelve years, to return the property in the same state in which he received it. The second obligation does not refer only to the state of the soil, but to anything received when he took over the estate (livestock, equipment, seed, buildings, etc.). Since the obligation rarely could be fulfilled for buildings because of the depreciation during the contract's lifetime, meticulous accounting was necessary at the end of the contract,

[1] See E. D. Domar, "Total Productivity and the Quality of Capital," *Journal of Political Economy*, December 1963, pp. 586–88.

in which any improvements made by the tenant would find their place (see e.g., the German Civil Code of 1900, paragraphs 582, 586–589).

Griliches uses the gross rental also for measuring the contribution of the capital service to output in the production function. I consider this procedure erroneous. Griliches' formula would be correct only if (1) as mentioned above, the owner has to pay insurance premium and property taxes and (2) if the depreciation as charged precisely measured the actual wear and tear, as far as it affects performance. Possibly Griliches had this case in mind when he developed his model. But on page 314 we read: "Instead of the usual sharp depreciation assumptions, I shall make the opposite assumption that the services of a machine do not decline at all (or very little) as long as it is in operation." Nevertheless in the next paragraph he retains the "gross rental" approach: "A $100 machine that will last five years will have roughly twice as large an *annual* [Griliches' italics] flow of services (in dollars) than another $100 machine whose expected length of life is ten years." In the first quotation, Griliches assumes that the service of the machine in the technical sense is the same at the beginning and at the end of the year; in the second quotation he denies it. The first quotation develops the implications of an assumption concerning the behavior of the machine in rendering services year by year; hence, it is inconsistent with the second one. It scarcely needs proof that Ricardo's rent concept was a net term, but it referred only to the soil. In Walras, too, the necessity of distinguishing between the market price of the service (in equilibrium) and what we call the net rental is acknowledged. To obtain the equilibrium price for the machine, Walras deducts from the equilibrium price of the service the depreciation allowance and the insurance premium; the remainder is to be capitalized by the equilibrium rate of interest. Walras obviously refers to new machines; the price for second-hand machines will be influenced by the fact that the seller of the machine does not hand over to the buyer the accumulated depreciation reserve.

I do not assert, of course, that in reality the performance of a machine is constant over its lifetime. Leaving aside the complications arising for the production function from the possibility of maintaining it constant by putting in more maintenance labor, I suggest the following theoretical rule for the measurement of a machine's performance in the production function: market price of the service (gross rental) minus depreciation plus estimated wear and tear per unit of time. If there is no market price for the service, we may measure the contribution by current costs plus estimated wear and tear (as defined above); current costs are the sum of interest, property tax, insurance premium, and an estimated risk premium.

MURRAY BROWN

1. There is difficulty in Professor Griliches' specification of the capital variable. It will be shown that his specification is a mixed embodied-disembodied model, and that it is impossible to specify a "service" concept of capital using gross stock without, at the same time, generating this inconsistency. This does not bias his results in Section IV, since the various specifications of capital are highly collinear, but it does affect his implementation of the results in Section V.

Let depreciation charges, calculated by declining balance, i.e., exponentially declining values over time, be

$$e^{-w}C_N = e^{-w}\int_{t-n}^{t} I(v)e^{w(v-t)}\,dv,$$

where w is the depreciation rate which includes *obsolescence, n* is the service life, and I (v) is gross investment of vintage v.[1] Also, let

$$C_G = \int_{t-n}^{t} I(v)\,dv,$$

be gross stock. Then, ignoring the insurance and rental items, Griliches' capital variable K, in Section II, is

$$\begin{aligned} K &= e^{-w}C_N + .06\ C_G \\ &= e^{-w}\int_{t-n}^{t} I(v)e^{w(v-t)}dv + .06\int_{t-n}^{t} I(v)\,dv \end{aligned}$$

His production model, stripped to the essentials, is

$$\begin{aligned} (1)\qquad V &= AL^{\alpha}G^{\beta}K^{\gamma} \\ &= AL^{\alpha}G^{\beta}\left(e^{-w}\int_{t-n}^{t} I(v)e^{w(v-t)}dv + .06\int_{t-n}^{t} I(v)dv\right)^{\gamma}, \end{aligned}$$

where G is, say, education.

[1] I have used the declining-balance method of writing off stocks, since this conforms more nearly to actual practice than the other major alternative, the straight-line method. Only if service lives are reduced below Bulletin F lives could one represent book value depreciation charges by the straight-line method. Cf. my "Depreciation and Corporate Profits," *Survey of Current Business,* October 1963, p. 12.

What is the marginal rate of substitution of new investment goods $I(T)$ to vintage investment goods $I(v)$ in (1)? It can be shown to be,

$$\frac{\partial V/\partial I(v)}{\partial V/\partial I(T)} = \frac{e^{w(v-t)-1} + .06}{e^{w(T-t)-1} + .06} \tag{2}$$

This represents the increase in the Tth investment goods required to compensate for a reduction in the vth investment goods in order to keep output constant. Clearly, it is not unity, as Griliches maintains on page 281. ("The procedure used assumes, however, that capital services of different vintages are equally productive.") In fact, the more time that elapses between T and v, the smaller is (2).

It can be shown that w, the depreciation rate, which includes an obsolescence factor, is the same as Solow's productivity improvement factor in his embodied model, under the assumptions of competition and smooth deterioration of economic values.[2] Hence Griliches has specified a hybrid model, containing embodied and disembodied components.

His attempt to test what he terms the "embodied model" (*loc. cit.*) against his own specification is a misspent one, since he is comparing a hybrid embodied-disembodied model with another hybrid. Of course the proportions of the two components may differ between the two models, but the data is insufficiently precise to pick up that order of difference.

2. Griliches maintains that the estimated sigmas by industry in his Table 3 do not significantly differ from each other. There is insufficient data presented in the paper to perform an analysis of variance to test the assertion (could we request that he perform it) on his estimates of σ_3, but a casual inspection reveals to my eye that they may indeed differ. If they do, then his specification of the Cobb-Douglas production function yields biased estimates of the elasticities of production. In particular, if capital is growing more rapidly than labor, and the true sigma exceeds unity, then Griliches' estimated elasticity of production with respect to capital is biased upward.

3. On page 296, Griliches asserts: "The only other alternative possible interpretation of these results is one which would deny entirely

[2] R. Solow, "Investment and Technical Progress," in K. J. Arrow, S. Karlin and P. Suppes (ed.), *Mathematical Methods in the Social Sciences, 1959,* Stanford, 1960, p. 100; and my, "An Iconoclastic View of the New View of Investment," Econometric Institute Report, 1963.

the possibility of estimating the elasticity of substitution from cross-sectional data . . ." The assertion would be correct if his "serial correlation" model contains no specification error. There are several misspecifications that could yield the results in question: (1) the sigmas for each industry may indeed differ; (2) there is no utilization adjustment, a factor all of which would not be accounted for in the serial correlation terms; (3) not everyone agrees that cross-section estimates of sigma are identified; (4) value added is a misspecified output measure, which causes trouble in time series, but the misspecification may be compounded in cross-section data; (5) if w, the wage rate, is not deflated by the industry price, there is a problem because of the specification of the lag term in Griliches' model; the omission of the deflator could be justified only if prices were constant between 1957 and 1958 in all industries.

4. Griliches specifies the education variable, following Denison, as labor-augmenting. As a general specification, this cannot be correct, since education probably augments all factors.

5. It is well known that, unless certain conditions are met, the Cobb-Douglas production function is not identified in cross-section data; i.e., the estimated elasticities are functions of relative factor prices. Are these conditions satisfied in the Liu-Hildebrand–Griliches' data and in Griliches' model? In view of the importance of the problem, a brief discussion is certainly warranted.

REPLY by Griliches

Dr. Popkin raises several questions about the adequacy of the occupational mix index as a proxy for an education index and about the more general relevance of "education" to a quality-of-labor measure. The occupation index is not a very good approximation to the variable I really wanted but did not have. The correlation coefficient between such two measures for the urban population of forty-eight states is only about .8. It would probably be higher for better-defined groups and a more detailed occupational breakdown. But no doubt it is not as good a measure as I would wish. This, of course, may also explain why its ultimate coefficients are somewhat lower than expected (as noted by Professor Bodkin). Popkin also questions whether there is a net effect of education holding occupation constant. On this we now have the evidence of

Hanoch's (1965) dissertation based on the 1-in-a-1,000 sample from the 1959 Census. He shows unequivocally that there is a substantial, positive, and significant effect of school years completed on income, holding occupation constant (i.e., using the within-occupations variance).[1] The estimated education effect is larger when occupation is not held constant, but that is as one would expect. Since education affects occupation, the correct weight for it in an analysis such as I performed above is not its coefficient holding occupation constant, but its "reduced form" coefficient based on the solving out of the endogenous occupation variable.

This point can be illustrated by a very simple model. Let Y stand for income, O for occupation, E for education; and u and v are random variables representing such forces as ability and luck. Then we have *two* relations:

$$O = \alpha E + u \tag{1}$$

$$Y = \beta O + \gamma E + v \tag{2}$$

from which we can derive the reduced form:

$$Y = (\beta\alpha + \gamma)E + v + \beta u \tag{3}$$

The last coefficient $(\beta\alpha + \gamma)$ is the one we are interested in. If it is estimated from (2) it will be underestimated. If, as I have done in the first part of my paper, it is estimated from a regression of Y on O, it will be underestimated if E and O are poorly correlated. Thus the coefficient may be too low, but the variable E used in the time series context is right, and does not overestimate its contribution.

Actually, the occupation-education dichotomy is an artifact of the data. The finer the occupational classification, the less difference it will make. What we really want is to allow for the changing mix of the labor force. This is a problem of aggregation error. In allowing for it what kinds of classes of workers should we distinguish? Given the data restrictions we want a classification which will maximize the between variance and minimize the within. None of these dimensions are perfect in this respect, but the educational one is better than the (major) occupational one. If and when the data come with enough detail, I will use both.

There is a certain inconsistency in the way capital is measured in the first and last parts of the paper, which has unfortunately led to some

[1] See his Table 1.

confusion and justifiable queries from Professors Brown and Neisser. This is partly the result of differences in data sources, but it also reflects the fact that much of the last part of the paper was written about two years earlier than the first. There are several different ways in which age and dating effects enter in and get mixed up in measuring services. First, there is the question of how the physical services of a machine age with calendar time. This is the question of what depreciation assumptions one should use. Second, there is the question of how one should aggregate *different* new machines (or their services) having different expected *life spans* (e.g., equipment and buildings). This leads to the distinction that I make between service flow (rental) and stock (purchase price) weights in aggregating the two types of capital that I can distinguish in the time series. Thirdly, there is the question of whether this year's *new* machine of the same type is as productive as last year's *new* machine. This can be viewed either as a question about the correctness of the price deflators for gross investment or as a question about embodied technical change, about the changing quality of *new* machines of roughly the same type (with similar expected life spans). Now these are three very different questions, though one can find examples where they all appear simultaneously. Thus it is not inconsistent as charged by Neisser to assume that (1) the annual flow of services from a given machine does not decline with age as long as the machine is alive (the one-hoss-shay assumption) and at the same time also to contemplate (2) the presence in the market of several types of machines having *different* life spans (10-year one-hoss shays and 100-year one-hoss shays). These are simply different questions.

In the time series data I assumed no deterioration in the service flow with age, and concentrated on the implications of different weights for construction and equipment and the possibility that the conventional deflators were wrong. When I came to the cross-section data I did not have the ingredients to construct the same type of measure. I chose to work with the rather strange measure of depreciation plus 6 per cent of gross stock plus insurance and rentals for the following reasons: I wanted to approximate my previous measure, but I did not have the structure-equipment breakdown. Hence I fell back on the depreciation data to help me along these lines. If, as was prevalent in 1957, the depreciation formula used is a straight-line one, the annual dollar flow will be proportional to gross stock for the same type of machine but will differ (in the right direction) for machines with different expected life spans. This is the difference I wanted to catch, and therefore I used this variable in spite of its questionable connection with the "truth." The next step was to apply the arbitrary (but hopefully reasonable) rate of interest of 6 per cent to the *gross* stock measure and add the result to the depreciation component. *If* the depreciation figures were in fact based on straight-line methods, and if the insurance and rentals component is ignored, the result is still proportional to gross stock and implies the same sort of no-declining-productivity-with-age assumption. It does, however, weight different gross stocks with different expected life spans differently. If there were only two types of capital on hand, the resulting measure would be approximately equal to

$$SK_t = \left(\frac{1}{n_1} + .06\right) K_1{}^G + \left(\frac{1}{n_2} + .06\right) K_2{}^G$$

where n_1 and n_2 are the expected life spans of machines of type 1 and 2 respectively. This is quite similar to the measure I use in the last part of the paper. Neither of these measures is subject to the embodiment objection raised by Brown.

An inconsistency may, however, arise here because the reported depreciation flows may not be based on straight-line assumptions or even if they were, the assumed life spans may include an allowance for obsolescence. Also, it is not really necessary to carry along at this stage the assumption of no deterioration of service quality with age. Either of these possibilities would suggest using the current value (net) of the stock (in constant prices) to compute the interest component of the capital service measure. By incorporating allowances for obsolescence, such a measure would contain some aspects of embodiment. Actually, as brought out in the text, I used such a measure also, but with almost no perceptible effect on the results.

While one can convert in some cases the problem of embodiment into the problem of what is the correct rate of depreciation to be used, what I had in mind under the embodiment label was the question: How many new machines of constant quality did a $100 in investment funds buy us this year as against the same amount spent on new machines last year? This is a question about the correctness of our estimate of gross *investment* in fixed prices, which logically precedes the question of what depreciation rates should be applied to it in constructing a capital stock or services measure. If my gross capital and depreciation data were in fixed prices, eliminating the general inflationary effects, the R measure used to catch the investment specific embodiment effect (net stock/gross stock) would be adequate for the job. Unfortunately, since these data are in historical prices, this measure is quite poor, and therefore the poor results obtained by using it are not conclusive.

Professor Domar makes the very important point that in considering embodiment or quality change in the context of productivity analysis one should be careful also to adjust the output side, since the mismeasured inputs are also outputs somewhere else in the economy. But he is wrong in applying the criticism to the computations presented in Table 9 of my paper and in his estimate of the order of magnitude of this adjustment for the economy at large. As far as this study is concerned, the only quality adjustment made is to the price deflator of gross investment in structures. Since structures are an output of the construction industry, the point made is not applicable to the analysis of sources of output growth in manufacturing to which I have restricted myself in this paper. Also, I believe that Domar reached the wrong general conclusion in his cited paper about the probability that the adjustments on the output and input side would tend to cancel out. If the rate of growth in the error of the investment deflator is constant, and the rate of growth of capital is constant, then the rate of growth of error in the measurement of investment is the same as the rate of growth of error in the measurement of capital. But in this case the adjustment on the output side is multiplied by the value share of gross investment in total output while on the input side it is multiplied by the share of capital in total costs. The second is usually significantly larger than the first (this is true for the entire 1929–64 period for the private domestic U.S. economy), and hence the two adjustments do *not* cancel out.[2] Thus there is merit and profit in pursuing the possibility that our investment deflators are not all that they should be.

The issue of whether the individual industry estimates of the elasticity of substitution are significantly different from each other, raised by Brown, is difficult to resolve because of the generally poor fit of the

[2] See D. W. Jorgenson, "The Embodiment Hypothesis," *Journal of Political Economy,* February 1966, for a more detailed exposition of this point and the above-cited Griliches and Jorgenson (1966) paper for an application to the total private domestic U.S. economy.

individual industry relations. Also I do not have exactly comparable results for the appropriate F tests (the aggregate I ran for the 17 industries contained 415 observations, 3 less than the 418 contained in the individual regressions for these industries). The results I have are roughly as follows: Allowing each industry in the aggregate equation to have a constant term of its own, we cannot reject the hypothesis that the slope coefficient (σ) is the same for all industries. In fact, the residual variance is the same to two decimal places (.0040) for the residuals from the 17 individual industry regressions (with 384 degrees of freedom) and the residuals from the aggregate equation (with 399 degrees of freedom). Switching to the serial correlation model, and testing now simultaneously the between-industry differences in the estimates of *three* parameters (σ, $\rho\sigma$, and ρ), we cannot reject at the 5 per cent level the hypothesis that as a *set* they are different for different industries. It is clear from inspection of the individual estimates that the difference arises from different estimates of ρ, the serial correlation coefficient. On the basis of our specification and data we can therefore reject the hypothesis that (1) the serial correlation properties of the disturbances are the same for all industries, and (2) that the distribution parameters are the same for all industries, but (3) we cannot reject the hypothesis that the elasticity of substitution is the same for all industries. Also, once (3) is accepted, we cannot reject the hypothesis that σ is equal to unity. An attempt to deal with the finding about significant differences in the distribution parameters is described in my paper in Table 6 and the associated text. This problem, and the simultaneity problem raised by Brown, are also discussed (perhaps unsatisfactorily) in the introduction to my paper.