By EDWARD F. DENISON

Some Major Issues in Productivity Analysis: An Examination of Estimates by Jorgenson and Griliches

The Office of Business Economics has been asked by several of the principal users of its data to supplement its established series on national output and its composition (GNP) by consistent measures of factor inputs, so as to facilitate the analysis of economic growth. The OBE is responsive to these requests and considers the preparation of measures of factor inputs an appropriate extension of its work on the national economic accounts. The estimates of business capital stocks and some other studies that have been published in the SURVEY OF CURRENT BUSINESS are important steps leading to the preparation of factor input measures.

The conceptual and statistical problems that are involved in the measurement of factor inputs are unusually difficult, however, and OBE believes that some discussion of these problems is called for before it engages itself to prepare the measures. To elicit such a discussion is a major purpose of publishing this article.

In this study, Edward F. Denison, one of the outstanding experts in the analysis of economic growth, provides a searching comparison of the concepts and statistical procedures that he considers appropriate for input measurement with those recently proposed by the eminent econometricians, Dale W. Jorgenson and Zvi Griliches. The Jorgenson-Griliches proposals differ sharply from those set forth by Denison, and also by many others who have done research in this field. For the convenience of the reader, the *Review of Economic Studies* article in which the Jorgenson-Griliches proposals appeared is reprinted—with some corrections by the authors—in this issue of the SURVEY.

These differences in concepts and procedures yield strikingly different conclusions. According to Denison, a substantial part of the postwar growth of national output has been due to an increase in productivity; according to Jorgenson-Griliches almost all of the increase has been due to an increase in factor inputs.

The issues raised by these opposing conclusions are not only important from the standpoint of basic research but are also likely to have far-reaching implications for the formulation of private and public policies directed at the promotion of economic growth. We believe that the publication of the Denison article and of a reply to it by Jorgenson and Griliches in a later issue of the SURVEY will be of substantial interest to all those concerned with economic growth.

IN a recent article, "The Explanation of Productivity Change," Professors Dale W. Jorgenson and Zvi Griliches found that increases in labor and capital input were responsible for almost all postwar growth in the United States [1]. They concluded that output per unit of input contributed little to the growth rate of output—only 0.10 percentage points, to be exact. This estimate contrasts with much larger amounts obtained in virtually all other studies. I arrived at 1.37 percentage points in *Why Growth Rates Differ: Postwar Experience in Nine Western Countries* (written with the assistance of Jean-Pierre Poullier) [2].

This review is a response to repeated requests to comment upon the article by Jorgenson and Griliches.[1] Do their estimates differ so much from mine because of differences in the time period analyzed, in the definition of output, or in the sector of the economy covered? Does the discrepancy reflect a mere difference in classifying growth sources into those regarded as increasing input and those regarded as raising output per unit of input? Or is it due to differences in statistical procedures? What are the differences in our procedures, what are their quantitative effects, and whose, in my opinion, are preferable? In this article, all of these questions are discussed.

To decompose the discrepancy in results, it is necessary to examine many aspects of the estimates. Section I of this review measures the effects of differences in time period, definition of output, and scope of the economy analyzed, and section II examines a minor difference in procedure. After allowance for these differences, most of the large discrepancy between our measures of output per unit of input remains. Our statistical measures of total output diverge because different price indexes are used for deflation; the effect is examined in section VI. Differences between our total input series for the sector of the economy analyzed by Jorgenson and Griliches are much larger. The input series differ because of (a) differences in the weights we use to combine individual inputs and (b) differences in the way we measure each individual input. In sections III and IV, I consider the change that would be introduced in my series, given my individual input measures, if the Jorgenson-Griliches weights were used. In sections V, VII, and VIII, I measure the effects upon their series, given their weights, of using their measure for each input in place of mine. The two preceding sentences must be qualified

NOTE.—Dr. Denison is Senior Fellow, The Brookings Institution, Washington, D.C. The views expressed in this article are those of the author and do not purport to represent the views of the other staff members, officers, or trustees of The Brookings Institution.

1. Its preparation was the occasion of rather extended communication among us, in the course of which Professors Jorgenson and Griliches clarified certain of their procedures, provided some unpublished data needed for comparison of our estimates, and offered suggestions on presentation. This assistance helped me to isolate the differences between our procedures and focus my discussion on these differences. It is acknowledged with gratitude.

I also benefited greatly from discussions of a draft of this review with George Jaszi, and of certain sections with Murray F. Foss, Guy V. G. Stevens, and Allan H. Young.

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by noting, as I shall at the appropriate points, that lack of data necessitated some departures from this plan. In section IX, I provide a table that summarizes the results of the preceding sections and thus reconciles our output per unit of input series.

An equally important purpose of this article is to examine the merits of alternative procedures. In most sections I therefore discuss differences in procedure that happen not to be important sources of discrepancy in our series during the particular time period discussed as well as those that are, and in sections IX and X offer some general observations.

The section of most general interest may well be section VII, in which I examine the Jorgenson-Griliches capital utilization adjustment. I try there to nudge the theory of growth analysis forward a little. In addition, their capital utilization adjustment is the largest single reason that our output per unit series diverge.

I. Time Period, Definition of Output, and Scope of Economy Covered

THE Jorgenson-Griliches summary result, that output per unit of input contributed only 0.10 percentage points to a 3.59 percent a year increase in output, refers to the 1945–65 period. Use of 1945 as a starting point minimizes their figure. From 1948 to 1965 Jorgenson and Griliches obtain a growth rate of output per unit of input of 0.74.[2] Almost all of this increase came before 1950 and after 1961; the growth rate of their output per unit of input series was 0.01 from 1950 to 1961 and 2.01 from 1961 to 1965 [calculated from 1, table VIII]. Cyclical movements contribute to the difference between these periods, but even so the contrast is remarkable.

My summary estimate, that the increase in output per unit of input contributed 1.37 points to the growth rate, refers to the period from 1950 to 1962. For this timespan, Jorgenson and Griliches obtain 0.30, as against 0.10 for 1945–65. Thus, the difference in time period is responsible for 0.20 points of the difference between our summary estimates. Our estimates for 1950–62 and two subperiods are contrasted in the first two rows of the following table. The third row [from 2, table 21–1] shows my estimates after adjustment to eliminate, as best I could, the effects of differences among terminal years in the intensity of demand (i.e., short-term changes in intensity of utilization of employed resources).

	1950–62	1950–55	1955–62
Unadjusted:			
Jorgenson-Griliches	0.30	0.42	0.22
Denison	1.37	1.93	.97
Adjusted:			
Denison	1.41	1.54	1.31

The Jorgenson-Griliches series refers to real gross national product per unit of input in the private domestic economy; mine, to real national income (also called net national product valued at factor cost) per unit of input in the economy as a whole.

The reason I chose to analyze the growth of net rather than gross product is both fundamental and conventional.

> "Insofar as a large output is a proper goal of society and objective of policy, it is net product that measures the degree of success in achieving this goal. Gross product is larger by the value of capital consumption. There is no more reason to wish to maximize capital consumption—the quantity of capital goods used up in production—than there is to maximize the quantity of any other intermediate product used up in production, such as, say, the metal used in making television sets. It is the television sets, not the metal or machine tools used up in production, that is the objective of the production process" [2, pp. 14–15].

Jorgenson and Griliches confine discussion of their choice of gross product to a single sentence. "Exclusion of depreciation on capital introduces an entirely arbitrary distinction between labour input and capital input, since the corresponding exclusion of depreciation of the stock of labour services is not carried out" [1, p. 256]. (They also cite an article by Domar, but it contains no reference to depreciation of labor.) Their statement is too brief to allow much discussion, particularly since Jorgenson and Griliches do not specify how they would depreciate labor. I am not aware of a definable labor counterpart to capital depreciation as a component of GNP that there is no advantage in increasing because it is not wanted—feeding, clothing, and housing children surely do not fall into this category—but if there be such, the appropriate remedy would be to change the measures of output and labor earnings.

I do not wish to pursue this subject further in this article, but must provide a statistical reconciliation of our estimates. This is facilitated by the fact that, sheerly by chance, conversion of my estimate of output per unit of input in the 1950–62 period to their concepts would scarcely change it because the difference in definition of output happens to be offset by the difference in the scope of the economy covered. The explanation is as follows:

(a) My output series refers to national income, or net national product (NNP) valued at factor cost, measured in 1958 prices. The Jorgenson-Griliches output series refers to gross national product valued at market prices, measured in 1958 prices. The choice between factor cost and market price weights to combine the components of product does not affect comparability of our results, but that between gross and net

2. National accountants would not draw inferences about postwar growth trends from an analysis beginning before 1948, at the earliest, because elimination of price controls distorted the real output measure in 1945–48, and because—in the case of 1945—of the great difference from later years in the composition of output. In addition, special aspects of postwar reconversion greatly affected the 1945–48 period.

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product does. The *absolute* increase in the value of gross product at 1958 factor cost is equal to the increase in net product at 1958 factor cost plus the increase in depreciation valued in 1958 prices. Each year, the change in output per unit of input (and every other growth source except depreciable capital) contributes the same absolute amount to the increase in real GNP at factor cost as to real NNP at factor cost. (Depreciable capital contributes to the increase in real GNP an amount equal to its contribution to the increase in real NNP plus the absolute increase in depreciation at constant prices.) But the same absolute amount contributed by output per unit of input yields a smaller percentage increase in GNP at factor cost than in NNP because the value of GNP is bigger than that of NNP—in 1950 by 11.6 percent, according to my estimates. Hence, output per unit of input contributed less to the growth rate of GNP when measured in percentage points. For 1950–62, my estimates yield a contribution of output per unit of input to the growth rate of GNP of 1.24 percentage points as against 1.37 to the growth rate of NNP.[3]

(b) My output estimates refer to the economy as a whole; the Jorgenson-Griliches estimates, to the private domestic economy. Thus, the latter exclude the net inflow of property income from abroad and GNP originating in general government. However, my estimates imply *no* increase in output per unit of input in the sectors they exclude.[4] The *absolute* contribution of the increase in output per unit of input to the increase in output is therefore the same in the sector covered by the Jorgenson-Griliches estimates as in the whole economy. Because the level of private domestic GNP was smaller than that of total GNP, the contribution of output per unit of input to its growth rate is proportionately larger; it is 1.38.[5]

This is practically the same as my original figure of 1.37; adjustments (a) and (b) are almost exactly offsetting.[6] Thus, differences in definition and scope of output together account for none of the difference between our 1950–62 estimates of the contribution of output per unit of input.[7]

II. Divisia Indexes

JORGENSON and Griliches devote considerable attention in their article to their use of Divisia indexes (which are averages of growth rates, with frequent changes in weights) in their measurement of input and output. I shall not discuss the alleged theoretical superiority of Divisia indexes, but simply note that their substitution has no effect upon the comparisons. When Jorgenson and Griliches introduce them in moving from their table I to table II, the movement from 1950 to 1962 of their series for output, input, and factor productivity is almost unaffected. Indeed, introduction of Divisia indexes has no appreciable effect at other dates except at the very beginning of their period, when price and output patterns were distorted. Moreover, my own procedures for combining inputs are substantially equivalent to the use of Divisia indexes.

III. The Input Weights: Total Labor vs. Total Capital and Land

TO calculate changes in total input, weights to combine the various types of input are required. Our weights, though different, share two characteristics that distinguish them from those of some other investigators. First, we each set the sum of our input weights equal to 100 percent (or 1). This has the effect of classifying gains from economies of scale as a contribution of output per unit of input to the growth of output.[8] Second, we each use the shares of labor, and of capital and land, in total earnings from production as weights to combine these broad types of input, and rely upon data from the national accounts to estimate these shares.[9]

Our actual weights differ as a result of differences in the scope and definition of our output measures and of differences in our estimating procedures. The latter contribute to the discrepancy between our results for growth of GNP per unit of input. During the postwar periods analyzed, capital-land input increased more than labor input so that the greater the weight attached to capital-land, the more a measure of

3. For consistency with OECD estimates, my GNP figures include a small amount for government capital consumption. This comes out again when I move to the private domestic economy in adjustment (b).

4. The entire increase in net property income from abroad is counted as a contribution of capital. Real GNP in general government is measured on the assumption that output per person employed does not change (this statement is only approximately accurate), and for this reason I used procedures that have the effect of measuring inputs in general government by employment [2, pp. 187–188]. Hence, no change in output per unit of input occurs in general government.

5. As indicated in section IV, my estimates imply that the contribution to the growth rate of *net* product at factor cost in the *private domestic* sector was 1.51.

6. This implies, of course, that the levels of total national income and private domestic GNP (both measured in 1958 prices at factor cost) happened to be almost the same at the start of the period (1950).

7. In measuring the effects of differences between us in concepts, scope, or procedures for this review, I often shortcut the calculations by using average weights or rates for the period examined even though we each subdivide the periods in our calculations. The results are accurate enough for the purpose at hand.

8. Throughout this review, I ignore as of no quantitative importance the fact that, in presenting the contributions of the sources to the growth rate, I allocated to output per unit of input 0.01 percentage points of an interaction term. Jorgenson and Griliches do not present contributions as such and hence omit this term, but with their estimates nothing would be allocated to productivity in any case. I also ignore rounding discrepancies that cause their growth rate of output to exceed the sum of the growth rates of input and output per unit of input at intermediate points in their analysis by small amounts varying up to 0.06 (as presented in their table IX).

9. My reasons for using income shares are stated in 2, chapter 4.

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total input increases and the less output per unit of input increases.

Differences related to scope and definition

The weights used in my study refer to the shares of labor and capital-land in total national income. I measure labor earnings as the sum of (1) the compensation of employees and (2) a portion (about three-fifths) of proprietors' income; this portion is derived on the assumption that the labor share of national income originating in proprietorships and partnerships is the same as the labor share of national income originating in nonfinancial corporations [2, p. 37]. My estimate of the total earnings of capital and land is equal to the sum of the following items: the remainder (about two-fifths) of proprietors' net income; corporate profits (before tax) and inventory valuation adjustment; the rental income of persons; and net interest. The labor share plus the capital-land share equals national income. (Whatever is not earned by labor is counted as earnings of capital and land despite the fact that "pure" profit—whether a return to entrepreneurship or monopoly profit—is included.)[10] Depreciation is revalued at replacement cost in the computation of corporate and noncorporate earnings and rental income, and of total national income.[11] On the average in the 1950–62 period, labor earnings represented 78.6 percent and capital and land earnings 21.4 percent of total national income.[12] These percentages are shown in line 1 of the following table. The remainder of the table will help the reader follow the rest of this discussion.

The Jorgenson-Griliches analysis is confined to the private domestic sector. My results imply that labor earnings averaged 74.7 percent and capital and land earnings 25.3 percent of national income in this sector. Jorgenson and Griliches analyze the growth of gross rather than net output; this obviously calls for a difference in procedure somewhere in the calculations. One acceptable possibility is to include depreciation with the earnings of capital and land in the derivation of weights, and this is what Jorgenson and Griliches do.[13] If depreciation is added to national income and to the capital-land share, and the percentages are recomputed, my estimates indicate that labor earnings averaged 67.2 percent of gross domestic product at factor cost in 1950–62 and that capital-land earnings together with depreciation averaged 32.8 percent. (These figures are unaffected by the method of measuring depreciation.) These shares, shown in line 3 of the table, differ from those in line 1 for conceptual reasons. Their use by Jorgenson and Griliches to analyze gross private product would have introduced little or no discrepancy between their estimate of output per unit of input and that which I derived in section I after allowance for differences in the definition and scope of our output measures.

	Labor share	Property share
Denison labor estimates:		
1. Whole economy, national income	78.6	21.4
2. Private domestic economy, national income	74.7	25.3
3. Private domestic economy, GNP at factor cost	67.2	32.8
Jorgenson-Griliches labor estimates:		
4. Private domestic economy, GNP at factor cost	70.8	29.2
5. Private domestic economy, GNP at market prices	63.8	36.2

Differences due to estimating procedures

The Jorgenson-Griliches weights differ from these for two reasons. First, although their estimate of labor earnings, like mine, equals compensation of employees plus a portion of proprietors' income, they obtain the latter by a different procedure. They assume that labor earnings of proprietors are equal to the number of proprietors (exclusive of unpaid family workers) times compensation per fulltime equivalent employee in the private domestic economy [1, p. 278]. This procedure allocates approximately all of proprietors' income to labor and none to capital and land. The labor share obtained by this procedure averages 70.8 percent, and the capital-land share 29.2 percent, of private domestic GNP at factor cost instead of 67.2 and 32.8, the percentages at which I arrive. My allocation of proprietors' income seems to me the more reasonable, but admittedly both procedures have substantial precedent. In the nature of the case, there is no way to check the results directly. Their use of a larger estimate of labor earnings would, in itself, lead Jorgenson and Griliches to a *higher* estimate of the contribution of output per unit of input to growth than I obtain. However, it is much more than offset by what I regard as an error in their derivation of capital-land earnings.

Jorgenson and Griliches state in their statistical appendix [1, p. 278] that "total income from property is gross private domestic product in current prices less private domestic labour income." Gross private domestic product was valued at market prices in their calculation. This means that Jorgenson and Griliches count indirect business tax liability minus "subsidies less current surplus of government enterprises" and plus business transfer payments and the "statistical discrepancy" in the national accounts as earnings of capital and land. Jorgenson and Griliches inform me that this inclusion was intentional, not an oversight. Inclusion of these items in the earnings of capital and land raises their capital-land share from 29.2 percent to 36.2 percent, or by almost one-fourth, and lowers their labor share from 70.8 to 63.8.[14] (These shares, shown in row 5 of the preceding text table, were computed from annual

10. Since Jorgenson and Griliches do the same, this does not cause our estimates to diverge.

11. The estimates are based on use of Bulletin F lives and straight-line depreciation. They were prepared before the results of the latest OBE capital stock study for nonresidential structures and equipment became available.

12. I do not actually use weights for the period as a whole in calculations, nor do Jorgenson and Griliches. I use weights for three subperiods, and they change weights annually. The averages provide a convenient summary.

13. This procedure is not necessarily exactly equivalent to that which I used in section I above to adjust my estimates to a gross product basis, but any difference in the end result for output per unit of input is probably trivial.

14. It also has the effect of including indirect taxes, and the other reconciliation items mentioned, in profits *after tax* in the numerator of the "implicit rate of return after taxes" that Jorgenson and Griliches show in table VI, column 4, of their article. Their article gives no hint of this peculiar definition of an after tax rate of return. I doubt that many readers of their article can be aware of it.

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figures given me by Jorgenson and Griliches.)

The principal item at issue, quantitatively, is indirect business tax liability. Jorgenson and Griliches do not explain why they include indirect business taxes in their weights or why, if they are to be included, there is more reason to add them to capital-land earnings than to labor earnings. Possible reasons for their procedures are hard to visualize, and I can only speculate as to what they may have had in mind.

The fact that Jorgenson and Griliches are analyzing the growth of gross product valued at market prices (which, viewed from the "income side," includes indirect taxes), rather than gross product valued at factor cost, surely necessitates no difference in weights. Share weights are used as estimates of the relative response (elasticity) of output to changes in labor input and to capital-land input; for example, use of weights of 30 percent for capital and land and 70 percent for labor to analyze gross product growth would imply that a given percentage increase in every type of capital-land input raises gross product by three-sevenths as large a percentage as does the same percentage increase in every type of labor input. There is no systematic reason for the percentage response of gross product valued at market prices to differ from the percentage response of gross product at factor cost.[15]

Possibly Jorgenson and Griliches mean to challenge the classification of indirect taxes as indirect. The income division that is appropriate for use as weights is the distribution of earnings that would prevail in the absence of taxes, *taking as given* the existing quantities of each input in the sector and period analyzed. To approximate this distribution, analysis is required of what is often called "shortrun" tax incidence (to distinguish it from analysis of incidence when any impact of taxes on the quantities of factors is taken into account). My use of the classification of taxes followed in the national accounts thus implies the following assumptions. First, that personal income and inheritance taxes (and various licenses, minor taxes, and nontax recipts of governments that are classified as personal) do not alter the distribution of earnings before taxes; hence, they need not be deducted from before-tax shares to achieve the desired distribution. Second, that the "shortrun" incidence of payroll taxes is on labor earnings; hence, labor earnings should be measured inclusive of payroll taxes. Third, that the "shortrun" incidence of corporate profit tax accruals is on corporate profits; hence, corporate profits should be measured inclusive of corporate profits taxes. Fourth, that the incidence of taxes classified as indirect is on no particular type of income and their presence does not alter relative shares measured exclusive of such taxes. Taxes classified as indirect, and the average percentage of total "indirect business tax and nontax accruals" represented by each type in 1950–62, are: sales and excise taxes and customs duties, 55 percent; property taxes, 33 percent; business motor vehicle licenses, 2 percent; other business taxes, 7 percent; business nontaxes, 3 percent.

No one supposes this classification of taxes to be precise. For example, I have myself suggested that at least the portion of the corporate income tax that is levied on regulated utilities probably is passed on in higher prices, causing my capital-land share to be overstated relative to labor. But, with some allowance for offsets, I have regarded the national accounts classification as acceptable.

If Jorgenson and Griliches count indirect taxes as earnings of capital and land because of incidence considerations, this implies that they accept the first three assumptions listed above and reject the fourth in favor of an assumption that the shortrun incidence of indirect taxes rests on capital and land.

For one tax classified as indirect, that on real property, this assumption may be preferable.[16] Indeed, in the context of considering the effect of taxes on the allocation of resources among sectors of the economy, I have myself suggested that one should not consider the impact of the corporate income tax, which bears only on the corporate sector, without simultaneously considering the property tax, which bears most heavily on the principal noncorporate sectors of the private economy: housing and farming [3, pp. 186–187]. It is plausible to argue that neither tax is shifted in the short run. But I see no possible reason to suppose that the short-term incidence of the other components of indirect tax and nontax liability rests on capital and land. These represent the bulk of the category, so I regard addition of indirect taxes to capital-land earnings as mainly an error.[17]

Although counting the difference between factor-cost and market prices as property income raises the Jorgenson-Griliches capital-land share of private domestic GNP by 7.0 percentage points in 1950–62, their actual weight averages only 3.4 percentage points higher than the weight implied by my estimates (with depreciation added) because of their smaller allocation of proprietors' income to property income.

My own estimate of output per unit of input is only moderately sensitive to differences in weights of this magnitude. If I were to substitute their weights for mine, my estimate of the contribution of output per unit of input would be lowered by about 0.08 percentage points.[18] I shall use this number to measure the difference in our results that is due to differences in our division of the weights between labor and capital-land as a whole. However, it should be noted that the Jorgenson-Griliches estimates are much more sensitive than mine to differences in weights because they estimate the

15. The movement over time of gross product at 1958 market prices differs from that of gross product at 1958 factor cost only if the composition of output shifts toward or away from products that were taxed (or subsidized) at above- or below-average rates in 1958. Any difference in movement is not related to share weights in the economy as a whole. (In 2, pp. 15–16, I suggest that if, in the output measure whose growth is analyzed, the components of output are weighted by market prices, such shifts should themselves be treated as a statistical "source" of growth.)

16. Even if this is so, it is an open question whether addition of property taxes to capital-land earnings would, on balance, improve the weights in view of the probable overstatement of the capital-land weight in both our estimates that results from counting "pure profit" and all of the corporate income tax in this share.

17. Inclusion of other, smaller reconciliation items between GNP at market prices and GNP at factor cost in property income seems tenable for only one minor subcomponent: corporate contributions to non-profit organizations.

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differential between the increase in capital-land input and labor input to have been far larger than I do. Substitution of my weights for theirs would raise their estimate of output per unit of input much more than 0.08. In the reconciliation I attempt, this extra amount will be reflected in the difference I identify with differences in our measures of changes in inputs.

IV. Allocation of the Total Capital-Land Weight Among Components

THE procedures that Jorgenson and Griliches and I adopt to estimate the contribution of capital and land to growth are similar at the most general level.

The total weight of capital and land is first divided among types of capital and land in proportion to the estimated earnings of each type. In my estimates five types are distinguished. One of these, international assets, does not appear in the portion of the economy analyzed by Jorgenson and Griliches. The others are: residential structures and residential land, nonresidential structures and equipment, nonresidential land, and inventories. Jorgenson and Griliches use a different classification. They distinguish among residential structures, nonresidential structures, equipment, residential and nonresidential land, and inventories.

Once the weights are assigned, each component of capital-land is treated as a separate input. An index measuring the quantity of each input must be developed. The weight is then multiplied by the growth rate of the index to arrive at the contribution of each component to growth.[19] (In my case contributions of international assets and, as explained in section V, residential property are calculated by a different procedure that does not require an input index.) The total capital-land contribution is the sum of the contributions of the components. In this section, I consider the weights. Later sections will examine the input indexes.

Because I analyze net product and my total capital-land weight includes only net (after-depreciation) earnings, my total capital-land weight is allocated among types of assets in proportion to their estimated net earnings. Jorgenson and Griliches allocate earnings in two parts. The portion of their capital-land weight corresponding to net (after-depreciation) earnings is allocated by estimates of net earnings, as in my procedure. To net earnings of each type of *depreciable* asset, they add depreciation (replacement in their terminology) in order to obtain gross earnings. This corresponds to their measurement of gross product and inclusion of depreciation in their total capital-land weight. This difference in our weighting procedure is legitimate because we are analyzing the growth of different output measures.

The preceding description of the Jorgenson-Griliches methodology pertains to their final estimates, which incorporate the adjustments introduced in moving from their table V to table VI. The weighting structure they initially use—in their tables I through V—is a mixture in that the total capital-land weight includes depreciation but is allocated among components by net earnings alone.

Use of asset values to allocate net earnings

The total weight of capital and land (excluding depreciation in the Jorgenson-Griliches estimates) is, as I have indicated, divided among components in proportion to their net earnings. But first the earnings of each component must be estimated, and this requires some assumptions.

The earnings of an enterprise can be measured, but most enterprises use more than one type of capital and land and there is no way to observe directly the earnings of each type. The analyst has no alternative but to assume that the individual enterprise earns the same rate of return on each.[20] Given this assumption, the total net earnings of capital and land in each enterprise can be distributed among different types of assets in proportion to their value to obtain the earnings of each type.

Jorgenson and Griliches introduce a second assumption: that the rate of return is the same in all enterprises. The two assumptions together permit them to allocate the net earnings of capital-land among types of assets by current asset values in the private economy as a whole. Except for a modification for capital gains and taxes, which I shall discuss shortly, this is their procedure.

The second assumption is not required by the nature of the economy.

18. Substitution of their higher estimates of the labor content of proprietors' income for mine, and addition of all the reconciliation items between GNP at factor cost and GNP at market prices to my estimates of capital-land earnings, would lower my labor share of total national income in 1950–62 from 78.6 to 74.1. By my procedures, the difference of 4.5 percentage points would be allocated among nonresidential structures and equipment, nonresidential land, and inventories in proportion to their present weight. (The weight of other capital-land components is independently derived.) Such a shift in weights would lower my estimate of the contribution of labor input by 0.06 percentage points, raise the contribution of capital by 0.14, and hence lower my estimate of the contribution of output per unit of input to the growth rate of national income in the whole economy in 1950–62 by 0.08. The effect on the growth rate of GNP at factor cost per unit of input in the private domestic sector would be the same, for reasons explained in section I.

19. The actual arithmetic of the Jorgenson-Griliches calculation differs from this description, but it is arithmetically equivalent. Suppose, in a year 1, that in current prices total income and output are $100 and earnings of inventories are $5 (equal to 5 percent of the total weight). Suppose that inventory input is measured by its value in 1958 prices, and this value is $100 in year 1 and $110 (10 percent more) in year 2. The more usual procedure would multiply the 10 percent increase in inventory input by its 5 percent weight, and conclude that the increase in inventories had raised output by 0.5 percent. The Jorgenson-Griliches procedure is to divide the $5 of inventory earnings in year 1 by the $100 of constant-price value in year 1 to obtain a "service price" of 5 cents per unit ($1 of value in 1958 prices) of inventories. The 100 units of inventory input in year 1 and the 110 units in year 2 are then multiplied by 5 cents, yielding $5 in year 1 and $5.50 in year 2. The difference of 50 cents is the contribution of the increase in inventories, and is again equal to 0.5 percent of the year-1 value of output.

20. Jorgenson and Griliches and I each assume statistically, subject to some later qualifications about capital gains and taxes, that, if the rate of return is the same for all types of assets, the ratio of net earnings to net value at current prices is also the same. This is not a wholly satisfactory assumption [2, p. 143, and 3, pp. 28, 112–113, 289–294], but it introduces no discrepancy between our results because we both use it.

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If data were available, one could allocate earnings separately for each enterprise and add up the results. If it turned out, for example, that enterprises having a high proportion of their assets in inventories had a higher rate of return, on the average, than enterprises having a high proportion of their assets in fixed capital, this procedure would (I believe appropriately) yield a higher weight for inventories and a lower weight for fixed capital than would a summary allocation of total capital-land earnings in the economy as a whole by the value of different types of assets in the economy as a whole. With the statistics available, this procedure cannot be implemented for individual enterprises. But I have found it possible to introduce what I regard as major improvements in the weighting structure by dealing with groups of enterprises.

(1) The earnings of capital and land used in the provision of housing services—called the "services of dwellings" industry in international compilations—were isolated [2, p. 40].[21] They are almost the same as total earnings in this industry since labor earnings are trivial. Since residential capital and residential land are the only types of capital and land used by this industry, and since (by definition) these assets are not used by any other industry, the earnings of residential capital and land can be unambiguously identified. Actual earnings of residential property are smaller than the estimate that would be obtained if total earnings in the economy as a whole were allocated by asset values, and hence my procedure leaves more weight for the remaining assets.[22]

(2) The net flow of property income from abroad, corresponding to the earnings of international assets, was also isolated; however, once my estimates are adjusted to correspond to the scope of the economy they cover, this procedure does not affect the comparison with Jorgenson and Griliches because income from abroad is outside their sector.

(3) The remaining earnings of capital and land—those arising in the domestic nonhousing sector—were divided between farm and nonfarm components. *Within* each sector, the total was distributed among nonresidential structures and equipment, nonresidential land, and inventories, in proportion to their net value. The estimates for the farm and nonfarm sectors were then added to obtain total earnings for each of these three types of assets. Farming has a lower ratio of earnings to assets than the nonfarm nonresidential sector, and a higher proportion of its assets are in land and a lower proportion in structures and equipment. Hence, the separate attention I give to agriculture results in a lower weight for land and a higher weight for nonresidential structures and equipment than would be obtained if the farm-nonfarm division were not made.

My average weights for the 1950–62 period are shown as percentages of total national income and of total nonlabor income in the first two columns of the following table. The next two columns give similar data for the private domestic sector.

The last column gives a percentage breakdown of the total capital-land weight that corresponds *conceptually* to the percentage distribution of the net (after-depreciation) portion of the Jorgenson-Griliches final weights, except for an adjustment for capital gains and taxes that they introduce. (It also corresponds conceptually to their division of the total gross capital-land weight, including depreciation, used in the construction of their table I.)[23]

Their distributions differ from this statistically, however, because they allocated total net capital-land earnings among components by values in the private domestic economy as a whole, without giving separate attention to the "services of dwellings" and agricultural industries.[24] For this reason, they presumably assigned a much higher proportion than I of the total net capital-land weight to residential structures and to residential and nonresidential land, and a lower proportion to nonresidential structures and equipment and (to a lesser extent) inventories.[25] On balance, the weighting structure for net earnings *within* their capital-land aggregate probably yielded a smaller increase in combined capital-land input, and hence tended to produce a *larger* increase in output per unit of input, than my weights would have done. This is chiefly because land, to which they assign more weight, did not increase.

21. In most Western European countries, the "services of dwellings" is considered a separate industry, for which the necessary data are published. In the United States, this activity is divided between the "real estate" and "farms" industries and not published separately, but it can be approximated from the details of the national accounts worksheets.

22. My precedures avoid the need to further divide the earnings of residential property between structures and sites. If such a breakdown were desired in order to preserve the Jorgenson-Griliches classification of assets, it could be obtained by allocating earnings *within* the housing sector by asset values.

23. Note, however, that Jorgenson and Griliches classify residential land with other land rather than with dwellings. They also subdivide nonresidential structures and equipment.

24. And possibly also because of differences in data used.

25. In their table I, they presumably also assigned a lower proportion of their total weight than I to structures and equipment and a higher proportion to land and inventories because, to arrive at the current value of structures and equipment, they use the double declining balance formula which yields lower values for such assets than the straight-line formula I adopted. In their final gross earnings weights, this difference is more than offset since depreciation is added back to the capital component to which it pertains.

	Whole economy		Private domestic economy	
	Percent of national income	Percent of capital-land earnings*	Percent of national income*	Percent of capital-land earnings*
International assets	0.6	3	.....	.....
Residential structures and land	3.5	16	4.3	17
Nonresidential structures and equipment	11.2	52	13.6	54
Nonresidential land	2.9	14	3.5	14
Inventories	3.2	15	3.9	15
Total capital and land	21.4	100	25.3	100

*Approximate.

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Capital gains

Anticipated capital gains or losses and taxes on income may bias earnings weights derived in the ways I have described if their presence causes the percentage distribution of asset values to diverge from that of earnings within a sector of the economy where the distributions have been assumed to be the same [3, p. 28]. I believe any such bias in my estimates to be trivial, but must devote extended discussion to the topic because Jorgenson and Griliches assign it a central place in their analysis.

I shall consider capital gains first. Jorgenson and Griliches believe the presence of capital gains or losses affects the validity of the assumption that earnings are distributed like asset values. They state: "Asset prices for different investment goods are not proportional to service prices because of differences in . . . rates of capital gain or loss among capital goods" [1, p. 267]. Their idea is that current asset values are proportional to the sum of earnings and capital gains so that allocation of earnings by asset values assigns too much to assets producing large capital gains and too little to assets producing small capital gains or capital losses. They do not discuss the timespan over which capital gains and losses must be cumulated to secure this proportionality, but I presume it is the discounted value of the anticipated stream of earnings and capital gains that would be supposed pertinent.

The relevance of this idea to the actual data we both use must now be explored. It is necessary, I believe, to distinguish sharply between land and reproducible capital. The current value of land is estimated market value; Jorgenson and Griliches and I rely upon Raymond Goldsmith for data. Land prices may and often do reflect not only current earnings related to current marginal products but also the expectation that marginal products will be higher in the future because of increasing land scarcity (relative to other factors). Land is also an inflation hedge and may reflect the expectation of a rise in the general price level as well. Hence, the ratio of current earnings to value may be lower for land than for capital, and allocation of earnings by value may overweight land and underweight capital.

The case of land has no counterpart within the reproducible capital aggregate. The values Jorgenson and Griliches and I use for capital components are their current replacement costs, estimated by use of price indexes for new equipment, structures, and goods held in inventory. These values are firmly anchored to the present price level and present production costs of capital goods and are not affected by capital gains. (Actually, I doubt that it would matter if the values were true market values, since there is no general reason for these to depart from reproduction costs.) Therefore I see no reason to suppose the allocation of weights among structures, equipment, and inventories is biased by capital gains.

As indicated, land may be overweighted and all the capital components correspondingly underweighted because of capital gains. But if this is true of my weights, the bias must be slight. My weight for dwellings and dwelling sites (including vacant lots, which yield no current income) is completely unaffected because it is based directly on earnings, excluding capital gains, and my procedure does not require a division of this weight between dwellings and their sites. Division of total earnings between farm and nonfarm industries greatly reduces any possible overweighting of private *nonresidential* land. In addition, I used conservative estimates of the value of land (Goldsmith's earlier, rather than later and higher, estimates). Finally, the weight I assigned nonresidential land is so small that it could be reduced even radically with no great effect. If it were cut 40 percent, for example, and this weight reassigned to nonresidential structures, equipment, and inventories, my estimate of the contribution of output per unit of input would fall by only 0.04 percentage points in 1950–62.

If capital gains bias weights obtained from a distribution by asset values, the Jorgenson-Griliches weights, prior to their attempted correction, are subject to larger error than mine because they do not isolate earnings in the "services of dwellings" and agricultural industries in which land is very important.

Jorgenson and Griliches attempt to eliminate the bias that they presume would otherwise enter their weights by introducing a formula that is based on the assumption that, each year, values of types of capital and land are proportional to the sum of the earnings and capital gains derived from them in that year.

The formula can best be understood with the aid of an arithmetic example. Assume for some year the arbitrarily selected data for the private domestic economy shown in the following table. (The table will be used again, and includes some numbers not needed as yet.) For simplicity, I let the data refer to the base year for deflation so that asset values are the same in current and constant prices. The first column gives data based on "true" depreciation (replacement) as estimated by Jorgenson and Griliches; the second, on capital consumption as shown in the national income estimates. Only two types of capital—equipment and inventories—are present, and each has a value of $50,000. (Residential and nonresidential structures are handled like equipment in the formula, and land, like inventories.) During the year, there is a capital gain (realized and unrealized) of $1,500 on the stock of equipment and $500 on inventories. The problem is to divide the total

	Jorgenson-Griliches basis	National accounts basis
Income and product account:		
Sales (equal GNP at market prices)	$60,000	$60,000
Labor earnings	45,000	45,000
Gross capital earnings [a]	15,000	15,000
Depreciation on equipment	7,000	5,000
Interest and profit [a]	8,000	10,000
Interest	1,000	1,000
Profit before tax [a]	7,000	9,000
Corporate income tax [b]	3,333	3,333
Profit less corporate income tax [a]	3,667	5,667
Addenda:		
Value of capital	100,000	----------
Equipment	50,000	----------
Inventories	50,000	----------
Capital gains	2,000	----------
Equipment	1,500	----------
Inventories	500	----------

[a] Includes indirect business taxes and other reconciliation items between factor cost and market price valuation for consistency with the Jorgenson-Griliches classification.
[b] Includes tax on capital gains.

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Jorgenson-Griliches gross capital earnings weight of $15,000 (or 25 percent of the total input weight of $60,000) between equipment and inventories when the Jorgenson-Griliches estimate of "true" depreciation is accepted.

The usual procedure would assign to equipment the $7,000 of depreciation on equipment, and divide the $8,000 of net earnings between equipment and inventories in proportion to their values—in the example, $4,000 each.[26] The total weight of equipment is then $11,000 and of inventories $4,000.

In the absence of a corporation income tax, Jorgenson and Griliches would compute the weight (they call it the "service price") for the $50,000 value of each of the two assets by the following formula [1, p. 256]:

$$p_k = q_k\left[r+\delta_k-\frac{\dot{q}_k}{q_k}\right]$$

where p_k is the price of the k^{th} capital service, q_k is the price of the k^{th} investment good, r is the rate of return, net of "true" depreciation but inclusive of capital gains, on all capital, δ_k is the "instantaneous rate of replacement of the k^{th} investment good" (i.e., the ratio of depreciation to net value), and $\frac{\dot{q}_k}{q_k}$ is the ratio of the capital gain on the k^{th} investment good to the value of that good.

If there were no capital gains in my example ($\dot{q}_k$ would then be zero for both equipment and inventories), this formula would yield the same weights as the simple procedure: $11,000 for equipment and $4,000 for inventories. The price of $50,000 of equipment would be calculated as

$$\$50{,}000\left[\frac{8{,}000}{100{,}000}+\frac{7{,}000}{50{,}000}-\frac{0}{50{,}000}\right]$$

or $11,000.

The price of $50,000 of inventories would be calculated as

$$\$50{,}000\left[\frac{8{,}000}{100{,}000}+\frac{0}{50{,}000}-\frac{0}{50{,}000}\right]$$

or $4,000.

The example actually assumes capital gains of $2,000, of which $1,500 is on equipment holdings and $500 on inventory holdings. When these are introduced, the weights (service prices) shift toward inventories, which have a lower rate of capital gain. The estimated price (earnings) of $50,000 of equipment becomes

$$\$50{,}000\left[\frac{8{,}000+2{,}000}{100{,}000}+\frac{7{,}000}{50{,}000}-\frac{1{,}500}{50{,}000}\right]$$

or $10,500.

The price of $50,000 of inventories becomes

$$\$50{,}000\left[\frac{8{,}000+2{,}000}{100{,}000}+\frac{0}{50{,}000}-\frac{500}{50{,}000}\right]$$

or $4,500.

The assumption of the calculation is that asset values each year are proportional to the sum of net (after-depreciation) earnings and capital gains in that year.[27] Jorgenson and Griliches base their weights (service prices) for each year on such a calculation (or rather a more complicated one to which I shall come shortly) for that year.

I find it impossible to believe that the procedure adopted by Jorgenson and Griliches actually improves the weights. It might be appropriate to apply the Jorgenson-Griliches assumption that values are proportional to the sum of net earnings and capital gains—but only with the use of average capital gains over long periods of time to adjust earlier years—if (1) asset values used in the calculations were independently obtained sales values and (2) substantially different rates of capital gain on different types of capital were forecast by firms and (3) their forecasts were accurate. But the second condition is unlikely and the third so restrictive that I doubt the procedure would be an improvement even if the first condition were met. Actually, the first condition is not met; as already noted, the capital stock values used are not market values but current reproduction costs that are not affected (except very indirectly and irrelevantly) by prospective capital gains. Consequently, the bias that Jorgenson and Griliches seek to eliminate is not present in the original data.[28] Their capital gains adjustment thus introduces a bias in the opposite direction—that is, it overweights capital assets on which capital gains are small.

Even if all three conditions were met, the relevance of an annual calculation would elude me. Since capital gains are highly erratic from year to year, the weights must also change erratically from year to year. It could hardly be argued that market prices of capital goods and land fluctuate annually so as to maintain proportionality between capital values and the sum of earnings and capital gains each year, nor could firms adjust the composition of their real assets annually even if they could foresee the pattern of each year's capital gains and losses. The supposed error in the use of asset values to derive weights for a year could have no relationship at all to the size of capital gains in that year.

Tax on corporate profits

I turn now from capital gains to taxes on income. Jorgenson and Griliches consider only the tax on corporate profits. It is sometimes argued that the presence of this tax leads to allocation of resources in such a way as to cause the after-tax rate of return in the corporate sector to be the same as, and hence the before-tax rate of return higher than, that in the noncorporate sector.

Because earnings from all types of capital and land used by corporations are taxed alike, it is easy to avoid any bias from this source in the distribution of capital-land earnings (which include this tax) among types of assets if asset values are available separately for corporations. One need only allocate earnings of capital and land in the taxed corporate sector in proportion to asset values in corporations, to allocate earnings in the untaxed noncorporate sector in proportion to noncorporate asset values, and then to add the two

26. I follow here the Jorgenson-Griliches procedure of counting indirect taxes, etc., as part of the net earnings component.

27. The calculation implies net earnings of $3,500 and capital gain of $1,500 for equipment, and net earnings of $4,500 and capital gain of $500 for inventories.

28. Except perhaps for the division of the weight between land, on the one hand, and the four capital components as a group, on the other.

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distributions to secure the final earnings estimates for use as weights. This procedure avoids any bias from the tax whether the tax diverts resources from the corporate to the noncorporate sector or does not.

My estimates do treat separately two sectors that are overwhelmingly noncorporate: housing and agriculture. However, the combined earnings of corporate and noncorporate firms within the nonfarm nonhousing sector were allocated by their combined asset values. This introduces an error into my weights for nonresidential structures and equipment, inventories, and nonresidential land if both (1) the rate of return after tax (rather than before tax) was the same for corporate and noncorporate firms, and (2) the percentage distribution of assets among the three types was different in corporate and noncorporate firms. The first condition would mean that before-tax earnings per dollar of value of each type of capital and land are higher in corporations than in noncorporate firms. If this is so, and if the second condition is also met, failure to allocate capital-land earnings of corporate and noncorporate firms (within the nonfarm nonhousing sector) separately would yield too large an estimate for earnings of types of assets used most by noncorporate firms and too small an estimate for types used most by corporations. However, the distribution of assets in noncorporate nonfarm firms could scarcely differ enough from that in nonfarm corporations to introduce an error of appreciable size.

Because Jorgenson and Griliches make a single allocation for the whole private domestic economy, without isolating housing and agriculture, the potential bias in their estimates is much larger and extends to residential as well as nonresidential capital and land. The direct way for them to remove the potential bias would be to make separate allocations of earnings in corporate and noncorporate sectors. An indirect way, having no advantage because it requires the same information, would be to increase the weight attached to corporate assets by (1) raising the value of corporate holdings of each type of asset by the ratio of after-tax earnings to before-tax earnings in corporations; (2) adding the resulting adjusted value of corporate holdings to the unadjusted value of noncorporate holdings of each type of asset; and (3) allocating combined corporate and noncorporate before-tax capital-land earnings among types of capital and land in proportion to the adjusted asset values so obtained. I surmise that Jorgenson and Griliches may have had this in mind when they introduced their formula for the determination of service prices in the presence of a direct tax on income.

This formula, which is used in their actual calculations in place of the simpler formula already discussed, is quite complex because it tries to deal simultaneously with capital gains and the corporate income tax, including the effects of differential taxation of capital gains. I believe the formula is intended to allocate earnings among types of capital and land on the assumption that asset values each year are proportional to the sum of net (after depreciation) earnings and capital gains in that year when earnings and capital gains from each type of asset are each measured after deduction of the corporate income tax applicable to them.

The formula, which I shall now describe, does not actually do this. In fact, it does nothing at all to remove the bias, just discussed, that allocative effects of the corporate income tax may be presumed to introduce. The reason is that Jorgenson and Griliches apply the *same* ratio of before-tax earnings to after-tax earnings (the average ratio for the whole private economy) to both corporate and noncorporate assets instead of using the corporate ratio for corporate assets and a ratio of one for noncorporate assets.

Introduction of new terms does not improve the results obtained by the simpler no-tax formula already described but instead compounds the errors. In particular, it accentuates the erroneous shift of the weights from capital-land components on which capital gain is high to those on which capital gain is small. In addition, it shifts weight from depreciable assets to land and inventories if (as is the case) "true" depreciation as measured by Jorgenson and Griliches exceeds capital consumption allowances as measured in the national accounts (which they use as a proxy for depreciation allowable for tax purposes). I presume their purpose in doing this is to allow for supposed effects of taxing depreciable assets on amounts that represent recovery of capital rather than true earnings, but defects in their formula and measurements make the amounts shifted haphazard.

The formula [1, p. 267, formula 11] is:

$$p_k = q_k\left[\frac{1-uv}{1-u}r + \frac{1-uw}{1-u}\delta_k - \frac{1-ux}{1-u}\frac{\dot{q}_k}{q_k}\right].$$

The definitions of the terms [as given in 1, pp. 256, 267, and 277–279 and in correspondence from the authors] and their values for equipment and for inventories in my example above are as follows:

p_k is the price of the k^{th} capital service. In using the example, I let it refer for convenience to the price of the service of $50,000 worth of equipment, and of $50,000 worth of inventories.

q_k is the price of the k^{th} investment good. In the example, it is $50,000 for equipment and $50,000 for inventories.

u is the ratio of corporate profits tax liability to profits before taxes in the private domestic sector of the economy.

Corporate profits tax liability is taken from the national accounts. It includes tax liability incurred because of inventory profits and other capital gains.

"Profits before taxes" in the private domestic sector are measured as property income (Jorgenson-Griliches definition) less capital consumption allowances and private domestic net interest, both taken from the national accounts. Profits before taxes are therefore equal to the sum of

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"corporate profits and inventory valuation adjustment" in the domestic sector, the proportion of "proprietors' income" not allocated to labor, the "rental income of persons," "indirect business tax and nontax liability," "business transfer payments," and "statistical discrepancy," minus "subsidies less current surplus of government enterprises." [29]

If the reason that Jorgenson and Griliches count indirect taxes as capital-land earnings is a belief that their shortrun incidence is on this share, one would also expect indirect taxes to be counted as taxes on these earnings. This is not done; indirect taxes are not counted as taxes on income but as part of income after tax.

This variable is the same for each type of asset, regardless of its distribution between the corporate and noncorporate sectors. In the example,

$$u=\frac{3,333}{9,000}=.3704.$$

r is the ratio of (a) total income from property less profits tax liability less the current value of replacement plus the current value of capital gain to (b) the current value of capital stock. It is the same for all types of capital and land. In the example,

$$r=\frac{15,000-3,333-7,000+2,000}{100,000}=.06667.$$

v is the ratio of private domestic net interest to the after-tax rate of return, r, multiplied by the current value of the capital stock. It is the same for all types of capital and land. In the example,

$$v=\frac{1,000}{.06667\times 100,000}=.15.$$

w is the proportion of "true" replacement (depreciation) that is allowable for tax purposes. Jorgenson and Griliches obtain this proportion as the ratio of capital consumption allowances, as measured in the national accounts, to their estimates of depreciation (replacement). They use the same ratio for all types of depreciable assets (residential structures, nonresidential structures, and equipment). For equipment in the example,

$$w=\frac{5,000}{7,000}=.7143.$$

No value is needed for inventories (or land).

δ_k is the rate of replacement (depreciation) of the k^{th} investment good. For equipment in the example,

$$\delta_k=\frac{7,000}{50,000}=.14.$$

No value is needed for inventories.

x is defined as the proportion of capital gains included in income for tax purposes. However, Jorgenson and Griliches inform me that, in their calculations, x actually was assumed to be zero for all types of assets.[30]

$\frac{\dot{q}_k}{q_k}$ is the rate of capital gain on the k^{th} investment good. I defer a description of the derivation of $\dot{q}_k$. In the example, the ratio is

$$\frac{1,500}{50,000}=.03 \text{ for equipment,}$$

and

$$\frac{500}{50,000}=.01 \text{ for inventories.}$$

When the values derived from the example are inserted, weights of \$10,794 for equipment and \$4,206 for inventories are obtained. For equipment p_k equals:

$$\$50,000\left[\frac{1-(.3704\times.15)}{1-.3704}\times.06667+\frac{1-(.3704\times.7143)}{1-.3704}\times.14-\frac{1-(.3704\times 0)}{1-.3704}\times.03\right]=\$10,794.$$

For inventories, p_k equals:

$$\$50,000\left[\frac{1-(.3704\times.15)}{1-.3704}\times.06667+.00-\frac{1-(.3704\times 0)}{1-.3704}\times.01\right]=\$4,206.$$

Effects of the formula

It is informative to recapitulate results from the example, and insert the results of one additional calculation. When no account was taken of capital gains or taxes, weights of \$11,000 for equipment and \$4,000 for inventories were obtained. Use of the no-tax formula to allow for capital gains shifted the weights to \$10,500 and \$4,500. If tax depreciation had been the same as true depreciation in the example, substitution of the formula with taxes present would have further shifted the weights to \$10,046 and \$4,954, this change reflecting the Jorgenson-Griliches assumption that capital gains are tax free.[31] With allowance, in addition, for taxation of part of "true" depreciation on equipment, the weight of equipment is raised to \$10,794 and that of inventories reduced to \$4,206. The particular numbers reflect only the figures assumed in the example, of course, but the direction of the changes at each

29. As originally printed, the Jorgenson-Griliches article stated that "the variable u, the rate of direct taxation, is the ratio of profits tax liability to profits before taxes for the corporate sector. These data are from the U.S. national accounts" [1, p. 277]. This definition, though logical if u were to be used only for corporate assets, would make the equation as it stands wholly inconsistent.

30. In their article this is not really clear. They write only that "the proportion of capital gains included in income is zero by the conventions of the U.S. national accounts" [1, p. 267]. This must be interpreted to mean that "the variable x, the proportion of capital gains included in income for tax purposes (but not the value of capital gains as they appear elsewhere in the formula) is zero." The two statements are unrelated, and while the first is true, the second is not. Some capital gains (the inventory valuation adjustment in particular) are fully, and others partly, taxed. Jorgenson and Griliches include these taxes in the numerator of u, which has the effect of charging them to earnings instead of to capital gains. With x equal to zero, $-ux$ in the numerator of the last term of the formula could be omitted without changing the results.

31. This calculation uses only the column in the example headed "Jorgenson-Griliches." The values of the variables are the same as those just given except that u is .4761 instead of .3704, and w (for equipment) is 1 instead of .7143.

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step helps to explain just what the formula does to the weights. I have already pointed out the main consequences.

The Jorgenson-Griliches formula may have theoretical interest.[32] But as they have applied it, it is hardly to be taken seriously as a tool for statistical analysis. The alterations in weights, away from assets with large capital gains, that would be introduced by their simple "tax-absent" formula are untenable. If they were tenable, the additional changes introduced by their "tax-present" formula would not be. The only bias potentially introduced by the corporate income tax (except by differential taxation of earnings and capital gains) is not affected. The overall corporate tax rate, u, as measured, is meaningless. It also is obviously wrong to assume that this tax bears as heavily upon dwellings and land as upon other assets. How indirect taxes can be counted as part of before-tax capital-land earnings but not as a tax on these earnings defies my understanding. Capital gains are not actually taxed at zero, as is assumed; they are taxed at a wide range of effective rates, ranging up to full taxation of the nonfarm inventory valuation adjustment. The fraction of depreciation (replacement) as measured by Jorgenson and Griliches that is taxable is not the same for all types of depreciable assets, as is assumed; the ratio of reproduction cost to original cost varies greatly between long-lived structures and short-lived equipment, and the proportions of these assets on which fast depreciation is allowed also varies greatly in the later years of their period.[33] Furthermore, much of the depreciation in the national accounts (particularly that on most dwellings) has no tax relevance at all (and farm depreciation is already on a replacement-cost basis). But these objections are, of course, largely superfluous if I am correct in asserting that the capital gains adjustment is itself a mistake.

Estimates of capital gains

The estimates of capital gains used by Jorgenson and Griliches that underlie the whole analysis are themselves subject to considerable criticism. The capital gain on any type of asset in a year is properly the difference between (a) the change in the value of holdings of the asset from the beginning to the end of the year, and (b) the value of the change in the quantity of the asset, measured in current prices. This figure can be approximated within an acceptable error by multiplying the value of the asset at the beginning of the year by the percentage change during the year in a price index for the stock of the asset.

Jorgenson and Griliches inform me that they used the former of these methods to secure capital gains on land, utilizing data from Raymond W. Goldsmith. For the capital items, however, they use neither of these measures. They write: "The capital gain for each asset is the product of the rate of growth of the corresponding *investment* deflator and the value of the asset in constant prices of 1958" [1, p. 279, italics added]. This differs from proper procedure in two respects. First, they measure changes in prices from the average of one year to the average of the next, instead of from the beginning to the end of the year. This is important for their annual series, but probably washes out over a period of years. Second, and more important, they use the implicit deflator for investment instead of the implicit deflator for the capital stock. This procedure yields an accurate approximation of the capital gain only if the two deflators are the same. They are the same if, but only if, the composition of the stock of an asset is the same as the composition of investment in it during each of the years compared—gross investment in the case of depreciable assets, net investment in the case of inventories. Only in this case are the weights appropriate for a capital stock price index the same as those that underlie the investment price index.

In the national accounts framework, this condition is met only for residential structures, which are treated as a single commodity both in deflation of investment and in building up a capital stock series. It is not met for nonresidential structures or for producers' durables, for each of which deflation is performed in considerable detail.[34] It is wildly not met for inventories; the composition of inventory change is usually very different from that of the stock of inventories. Moreover, the composition of inventory change varies greatly from year to year. As a consequence of this (together with the fact that, on a 1958 base, the levels of price indexes for different inventory components diverge greatly as one moves away from 1958), the implicit deflator for the change in inventories properly moves very erratically, especially in years far removed from 1958, even though the deflator for the stock of inventories moves smoothly. Jorgenson and Griliches note and dislike these wild movements. But instead of correcting their method to use the deflator for the stock of inventories instead of inventory change, they arbitrarily alter the deflator for inventory change by substituting the consumption deflator.

Depreciation

When an investment yielding a positive gross return is made, gross output is increased, depreciation is increased, and net output is increased by the difference between the two, which is the net product of the investment. If one were interested in analyzing the growth of both gross and net product, he could proceed in any of three ways. (1) He could analyze the growth of net product using net earnings weights (as I did in *Why Growth Rates Differ*), and add constant-price depreciation to output and to the contribution of capital in order to analyze gross product (as I did in section I of this paper). When I apply this method to the private domestic sector covered by Jorgenson and Griliches, my estimates yield the following results:

	Growth rate of output	Contribution of inputs	Contribution of output per unit of input
Net product	3.23	1.72	1.51
Gross product	3.35	1.97	1.38

32. However, if the formula is viewed as a theoretical construct rather than a description of their procedures, u, v, w, and x should all carry the subscript k since they differ for each asset type.

33. Tax depreciation differs from the Jorgenson-Griliches estimate of true depreciation chiefly because original cost is not the same as reproduction cost and because double declining balance depreciation is not allowed or, if allowed, is not used by taxpayers because they do not think it to be to their advantage.

34. The fact that Jorgenson and Griliches treat each of these as a single commodity, with a single service life, in constructing capital stock series does not suffice to remove the objection.

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(2) He could analyze the growth of gross product using gross earnings weights (as Jorgenson and Griliches do), and subtract constant-price depreciation from output and from the contribution of capital in order to analyze net product. (3) He could analyze the growth of net product using net earnings weights and the growth of gross product using gross earnings weights. The three procedures are exactly equivalent only in special circumstances, but their results are not likely, in practice, to diverge very much. To explore the considerations involved in the choice would take me far afield, and I content myself with the assertion that, to measure net product, it is better to use net product weights than to follow the second alternative.

Jorgenson and Griliches [1, p. 257] criticize John W. Kendrick for not using service prices as his weights. They are wrong. Kendrick analyzed growth of net product and appropriately used net earnings weights. To include depreciation in the weights in an analysis of the growth of net product, as Jorgenson and Griliches insist he should do, would be a plain error that would lead to overstatement of the contribution of capital to growth.[35] That the other aspect of their service prices—their capital gains and tax adjustment—would have improved his estimates is just not credible on the basis of my preceding discussion.

Effect of differences in weights

When Jorgenson and Griliches adjust their initial estimates to use what they call "prices of capital services" in their calculations, they raise their 1950–62 growth rate of total input, and lower that of output per unit of input, by 0.35 percentage points [computed from 1, tables V and VI]. This number combines the effects of two changes from their initial estimates. First, Jorgenson and Griliches remove an error present in their initial weights. Whereas they initially allocate the depreciation component of their gross capital-land earnings weight like net earnings, they now allocate it correctly by depreciation. Second, they introduce the adjustment for capital gains and corporate income tax that I have described. The portion of the 0.35 percentage points that results from the reallocation of depreciation does not represent a discrepancy between their estimates and mine of the contribution of output per unit of input to GNP growth in the private domestic sector. I cannot isolate this portion but it is clearly substantial and, like the combined adjustment, positive. The portion that results from the adjustment for capital gains and taxes does cause a discrepancy, but I cannot isolate the amount nor even be sure whether it is positive or negative.[36] Neither can I calculate the discrepancy between our results (not necessarily included in the 0.35) that is introduced by my according separate treatment to housing and agriculture. Hence, I cannot measure the difference in our output per unit of input series that resulted from the difference in our allocation of the total capital-land weight among components, and this introduces a gap into the reconciliation table I provide in section IX.[37]

Consideration of the bearing of the Jorgenson-Griliches discussion of service prices upon my own estimates suggests only one qualification of my procedures. This is the possibility, already examined, that I may slightly bias my results by overweighting nonresidential land.

V. The Measurement of Capital-Land Inputs (Excluding the "Utilization" Adjustment)

I turn now to input series for the various types of capital and land. This section compares my estimates with those of Jorgenson and Griliches after their adjustment for what they call "errors" in investment goods prices, but not for changes in "utilization." Their "utilization" adjustment will be discussed separately in section VII.

Nonresidential land

Jorgenson and Griliches and I each estimate the input of nonresidential land to have been constant over the period.[38] Its contribution to growth is therefore zero in both series.[39]

Inventories

To measure inventory input, I use the OBE series for the value of farm and nonfarm inventories in 1958 prices; this is the series that is consistent with the annual changes published in the national accounts. The growth rate of this series times the inventory share of national income equals the contribution of inventories to growth.

Jorgenson and Griliches initially use a conceptually similar, but statistically different, series obtained by starting with a base-year value and cumulating annual changes published in the national accounts. They then introduce a certainly erroneous change in the price deflator; they substitute for the inventory deflator the deflator for personal consumption expenditures. This error is apparently a byproduct of their faulty procedure for measuring capital

35. Unless the second alternative listed above were to be adopted, which Jorgenson and Griliches do not suggest.

There have been some studies of gross product that have included depreciation in the weight of capital and land as a whole but have allocated it among components by value of the stock. The Jorgenson-Griliches criticism of this procedure (which corresponds to theirs in construction of their table 1) is correct.

36. The percentage division of the Jorgenson-Griliches gross capital-land earnings weight between net earnings and depreciation also affects the results. It may or may not differ appreciably from mine. Their depreciation is presumably larger because they use the double declining balance instead of the straight-line formula. But their net earnings are also larger because they include indirect taxes.

37. The combined effect of this and certain other differences is estimated in section IX to be 0.33 percentage points.

38. Their estimates combine residential with nonresidential land. Perhaps they would assume some slight decrease in nonresidential land and an increase in residential land if they were to make the distinction.

39. Because of differences in the *weight* assigned to this nongrowing factor, already discussed, this does *not* mean that land does not affect our results.

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gains, which I have already discussed.

Growth rates of the stock of inventories from 1950 to 1962 are 3.00 for my series [2, p. 190], 4.06 for their initial series, and 4.14 for their series after the price substitution (both computed from 1950 and 1962 values in 1958 prices provided by Jorgenson and Griliches). The initial Jorgenson-Griliches inventory series increases by about the same *absolute* number of 1958 dollars as mine. Its much larger *percentage* change and growth rate reflect a much lower figure for the base-year value of the stock; their series for total inventories runs at a bit lower level than the OBE series for nonfarm inventories alone. The data they use for level and change are evidently inconsistent.

The difference of 1.14 points between their final inventory growth rate and mine accounts for 0.04 percentage points of the difference between our estimates of output per unit of input growth, based on my share weights; the amount based on their share weights would probably be about the same. Of the divergence, 0.03 is due to the low level of their inventory series; this is raised to 0.04 by their price adjustment.

Nonresidential structures and equipment: Denison series

One's choice of a capital stock series to measure input of nonresidential structures and equipment necessarily depends on his judgment as to whether or not the ability of a capital good to contribute to production declines during its actual service life because it performs less well, requires more maintenance, or is installed in a less optimal use than it was initially as a result of demand shifts and the like; and, if it does decline, by how much and in what time pattern. Gross stock (the value of the stock without deduction for accumulated depreciation) provides an appropriate measure if there is no decline. Use of a net stock series is always inappropriate on theoretical grounds; net value drops as the length of the remaining service life declines, and this has no relevance to ability to contribute to production currently. In *Why Growth Rates Differ*, I assumed that the ability of capital goods to contribute to production typically does decline during their service lives but not very much. I suggested [2, pp. 140–141] that if one weighted the growth rate of gross stock about 3, and that of net stock based on straight-line depreciation about 1, he would obtain a series that might reasonably approximate the decline in the ability of capital goods to contribute to production as they grow older. To give some weight to net stock in this way is merely a convenient method of introducing a declining pattern.

In my actual estimates, I gave *equal* weight to gross stock, based on Bulletin F lives, and to net stock, based on Bulletin F lives and straight-line depreciation. (For the 1950–62 period, but not the subperiods, estimates of the contribution of capital to growth with the capital stock data I had were actually the same whether gross stock or net stock was used, so that the weights actually did not matter for the whole period.) I did so partly because I feared the gross stock series then available to me was unduly sensitive to possible errors in estimated service lives as a result of its construction with but little detail and without a distribution of retirements, and I wished to reduce this sensitivity; and partly because of the needs of international comparisons [2, pp. 140–141].

My estimates were made before the latest OBE capital stock study was completed. Before I continue this section, the change that use of the new OBE data would introduce into my estimates needs examination. Had the OBE study been completed, I would have used OBE capital stock series based on Bulletin F lives, on use of the Winfrey distribution for retirements, and on use of the OBE "price deflation II."

Growth rates of the stock of nonresidential structures and equipment from 1950 to 1962 computed from five measures, and my estimates of the contribution of structures and equipment to the growth rate based on each, are as follows: [40]

Nonresidential structures and equipment capital stock series	Growth rate (percent)	Contribution to growth rate of national income (percentage points)
Average of gross and net stock series, equal weights:		
1. Used in *Why Growth Rates Differ*	3.74	0.43
2. OBE revised—Deflation I	3.24	.37
3. OBE revised—Deflation II	3.51	.40
Average of gross stock (weighted 3) and net stock (weighted 1):		
4. OBE revised—Deflation II	3.40	.39

Row 1 shows the estimates I actually used. Row 2 shows that the incorporation of revised OBE data, based on Bulletin F lives, straight line depreciation, and the Winfrey distribution, but retaining the same deflators (OBE Deflation I) as the estimates I actually used, would lower my estimate of the contribution of capital to growth by 0.06 percentage points. The change is due mainly to the use of much more detail in the calculation of stocks. Row 3 shows that substitution of OBE's series based on their Deflation II for nonresidential structures would yield a contribution of capital 0.03 percentage points higher than does use of their Deflation I series. (I shall comment on the difference shortly.) After this substitution, the contribution of nonresidential structures and equipment based on revised data remains 0.03 points lower than the estimate I actually used.

Given estimates incorporating the Winfrey distribution and the use of considerable commodity detail, and in the absence of international comparisons, I would weight gross stock about three and net stock (based on straight line depreciation) one, instead of assigning equal weights. This would yield a contribution of 0.39 points (row 4) and would lower the estimates I actually used for the contribution of capital by 0.04. My estimate for the contribution of output per unit of input is thus 0.04 points too low by reference to the estimate I would now secure by use of the data presently available.

40. The revised OBE data were provided by letter on December 19, 1967. My average 1950–62 weight for nonresidential structures and equipment is 11.2 percent of total input.

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Nonresidential structures and equipment: Jorgenson-Griliches series

Jorgenson and Griliches treat nonresidential structures and producers' durables as separate inputs in their estimates. For each, they use the double declining balance formula to obtain a capital stock series. No detail is used for either calculation.

Capital stock series obtained by the double declining balance formula have always heretofore been described as "net stock" series. Estimates of the value of net stock obtained by this formula assume that net value declines rapidly—much more rapidly than the straight line formula assumes. Justification of so rapid a decline in net value has relied on the argument that obsolescence is rapid; this justification seems to require that obsolescence not only shortens service lives (this is reflected in all capital stock series) but also *greatly* accelerates the loss of value during the shortened service life.

Although their method is the same, Jorgenson and Griliches sometimes appear to regard the series they obtain by the double declining balance formula not as a net stock series but as a gross stock series. Thus, in describing the derivation of a capital series, they state [1, p. 255]: "The quantity of new investment goods reduced by the quantity of old investment goods *replaced* must be added to accumulated stocks." And, again: "We assume that the proportion of an investment *replaced* in a given interval of time declines exponentially over time." [Both italics mine.] And they usually (though not on page 277) refer to the value eliminated from the stock each year as "replacement" rather than as depreciation. *If* they mean "replacement" to be construed as equal to discards, they are indeed trying to construct a gross stock series. But if this *is* their intent, their method is certainly odd. I do not know what evidence they would muster to support the assumption (which is also applied, even more improbably, to dwellings) that discards decline exponentially (i.e., are greatest in the first year after purchase or installation and thereafter decline each year). But even if it were true that discards decline exponentially, their exponents (because they use *double* declining balance) apparently are about twice too big to retain the (Bulletin F) average service lives that they initially accept and from which they begin the calculation [1, p. 277]; that is, they greatly cut their own average service lives. Starting with a 15.1-year average service life for equipment, for example, they estimate half the stock has vanished after 5 years, and seven-eighths after 15 years.

Whatever the intent, changing the name does not change the data, and I shall regard the series constructed by Jorgenson and Griliches as measuring what such series have always been regarded as measuring—the net stock based on the double declining balance formula—and what they call "replacement" as an estimate of depreciation. A series based on this formula makes the ability of an individual capital good to contribute to current production drop much faster than seems to me at all plausible. Whatever can be said to justify its use in measuring net value has no relevance to measurement of changes in ability to contribute to current production.

I have puzzled over the Jorgenson-Griliches discussion of why they use their formula [1, p. 255] but have been unable to discern its relevance to the choice of a capital stock series to measure changes in capital input.[41]

It may be necessary to note here that the choice of a particular formula to measure capital depreciation (or "replacement") in the process of computing income share weights, including the net capital values used to allocate total net capital-land earnings among components, in no way dictates that the same formula should be used to construct the capital stock series that is used to indicate changes in capital input over time. Different series not only can be used for the two purposes but, conceptually, must be. For weighting, value must decline as remaining service life diminishes whereas a measure of current services must not do so. Thus, it is entirely consistent to use net stock values to determine weights, and whatever series seems most suitable (including, in particular, gross stock) to measure changes in capital input (or services) over time. Jorgenson and Griliches themselves accept this view when they adjust their capital services for changes in utilization (section VII below) without changing their depreciation.

I wish to stress that the choice of depreciation or replacement formula appropriate for measurement of changes in capital input has nothing to do with "vintages," that is, with the way one wishes to treat quality differences in capital goods that do not reflect a difference in costs and that result in "unmeasured" quality change (or "embodied" technical progress) as time goes on. Use of a fast depreciation formula is not a method of making an allowance for unmeasured quality change. This can be readily seen from the fact that, with any continuous rate of quality improvement in capital goods, net capital stock based on double declining balance depreciation can rise either more or less than gross stock or net stock based on straight line depreciation. From 1950 to 1962, for example, data from the OBE capital stock study show identical percentage changes for net stock when straight line depreciation is used and when the double declining balance method is used.[42]

Jorgenson and Griliches employ series they themselves derive by use of the double declining balance formula. They assign a single service life to all nonresidential structures and to all producers' durables, whereas OBE assigns different lives to each of a large number of components. The growth rate of their value of nonresidential structures and equipment (from the beginning of 1950 to the beginning of 1962) is 0.17 higher than that of the corresponding OBE series. Even so,

41. The Jorgenson-Griliches discussion seems to visualize steady growth of replacement investment, and their rationalization seems to require, in addition, steady growth of new investment. But if gross capital investment grew at a steady rate (and service lives were not changed over time), it would make little or no difference whether an index of gross stock (in the usual sense of the term) or of net stock computed by any of the usual formulas were used to measure capital input. It is only because investment has been irregular—particularly because of depression and war—that the problem of selection has any importance.

42. This is the case whether "constant cost I" or "constant cost II" estimates are compared. Changes are computed from the average of the beginning and end of 1950 to the similar figure for 1962.

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in the period examined, their series is not radically different from other measures. The 1950–62 growth rates of the capital stock series they initially obtained (prior to their price substitution) and used in constructing their table I, are 4.11 for equipment, 3.42 for nonresidential structures, and 3.72 for nonresidential structures and equipment combined (computed from data for the value of the stock in 1958 prices provided by Jorgenson and Griliches).

However, in moving from their table II to table IV, Jorgenson and Griliches greatly accelerate the rise in the growth of the equipment stock by deflating past gross investment in producers' durables by the price deflator for consumers' durables instead of that for producers' durables. This substitution raises the 1950–62 growth rate of their equipment stock alone by 1.49 points, to 5.60, and the growth rate of nonresidential structures and equipment combined by 0.62 points, to 4.34 (computed from capital stock data provided by Jorgenson and Griliches).

To justify the substitution, Jorgenson and Griliches state that, for items that appear in both the BLS consumers' price index and the BLS wholesale price index, the retail and wholesale series diverge by roughly the same amount as the composite indexes. They further state that the consumers' price index is better because more money is spent on it.

It is desirable to deflate common components of consumers' expenditures for durable goods and producers' purchases of durable goods by the same deflator, the best available—at least when they are sold by the same outlets on similar terms. But automobiles are the only important common component (as well as the only component of the consumer and wholesale price indexes that is mentioned by Jorgenson and Griliches).[43] And OBE already uses the same (consumers') price series to deflate consumer and business purchases of automobiles. The sharp divergence between the implicit deflators for all consumers' durables and all producers' durables is ascribable to commodities *not* common to the two series. Production processes for the two sets of goods are very different. Consumers' durables, which had the smallest price rise of any sizable product group, are dominated by mass-produced, standardized products. Their exceptional price behavior was due to radio and television receivers, "kitchen and other household appliances," and automobile "tires, tubes, accessories, and parts." Producers' durables, in contrast, are dominated by items produced in small volume, including a large element of individualized, built-to-order items most akin to custom services. I do not see how any inference about changes in prices of producers' durables can be drawn from prices of consumers' durables, or that the latter provide a more relevant comparison with the former than any other prices.

The OBE deflator for producers' durables is, to be sure, subject to substantial error in either direction because the data entering it are incomplete and their reliability low—mainly because so many components are *not* standardized. But there is no a priori presumption that the series is biased upward by reference to the usual price index criteria. I regard this substitution as unwarranted.

It must be stressed that this price substitution cannot be rationalized as an attempt to allow for quality change not involving a difference in costs at a common date ("unmeasured" quality change). Neither the CPI nor the WPI makes any such allowance (nor do any of the GNP deflators).[44]

In contrast to producers' durables, there is a presumption that the deflator for the nonresidential structures portion of GNP is biased upward by reference to usual price index criteria. This is because most components are based on prices of construction materials and labor, rather than on output prices, and hence do not allow for changes in output per man-hour in on-site construction work. This bias has long been recognized, but its size has been hard to appraise.

For use in its capital stock study, OBE developed an alternative nonresidential construction price series that attempts to eliminate this bias, and used it as an alternative to the GNP nonresidential construction price deflator to derive its Deflation II capital stock estimates that I have already mentioned. These estimates differ from OBE's Deflation I estimates only because of the use of a different construction deflator. Jorgenson and Griliches make the same substitution in moving from their table II to table IV. This raises the 1950–62 growth rate of their nonresidential structures series by 0.50 percentage points, from 3.42 to 3.92, and the growth rate of nonresidential structures and equipment combined by 0.28 points, from 3.72 to 4.00 (computed from data provided by Jorgenson and Griliches).[45] The effect on the combined series is almost identical to that (0.27 points) introduced when the similar substitution was made between lines 2 and 3 of the text table above, and the effect upon the growth rate of *total* input when my weights are used is also the same, 0.03 percentage points.[46]

The 4.00 growth rate of the stock of nonresidential structures and equipment obtained by Jorgenson and Griliches when their construction price substitution but not their equipment price substitution is introduced may be compared with the 3.40 growth rate I obtain by use of the revised OBE data with use of Deflation II (text table above). The 0.60 difference reflects both a difference in choice of capital stock series and OBE's greater use of commodity detail. Based on my weights, it accounts for 0.07 percentage points of the difference between us in output per unit of input.

Residential structures and land

My methodology does not require an input series for residential structures

43. Some types of office furniture might be regarded as having a household counterpart, and there are items of trivial importance.

44. In my view, there is no way to do so. But this is a controversial matter that need not be discussed here.

45. With *both* the equipment and construction price substitutions, the 1950–62 growth rate of the Jorgenson-Griliches series for nonresidential structures and equipment is 4.65.

46. Robert J. Gordon has also attempted to construct a series for deflation of nonresidential construction from which the bias has been eliminated. Data he has generously provided me show that substitution of his series for the OBE nonresidential construction deflator would raise the growth rate of a series for the stock of nonresidential structures and equipment (specifically, the gross stock based on Bulletin F lives) by 0.40 percentage points. A change of this size would raise the growth rate of a total input series, based on my weights, by 0.04 percentage points as against the 0.03 indicated by the OBE Deflation II series.

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and land. Instead, I isolate the amounts of national income, measured in constant prices, that originated in the "services of dwellings" industry in the same way as the current dollar figures were obtained in deriving share weights. The same procedure can be followed for GNP at factor cost. I find [2, pp. 123–126, 413] that the increase in the stock of dwellings and residential land contributed 0.25 percentage points to the growth rate of national income and 0.32 points to the growth rate of GNP at factor cost from 1950 to 1962.[47] This method of direct measurement, which I first used in [2], is, in my opinion, an important advance in growth analysis. It provides a measure for the contribution of this very large part of the capital-land stock to the growth of output as actually measured that is entirely accurate, except for some slight statistical difficulty in the United States in disentangling the details of the national product estimates. An incidental advantage, it may be noted, is that the figure for the contribution to GNP makes no use of, and consequently cannot be affected by, errors in the price index for residential construction.

Jorgenson and Griliches measure the contribution of residential structures as the growth rate of the dwellings stock times the weight assigned to dwellings—the procedure I used in an earlier study [3]. However, instead of using a gross stock series to measure changes in the services of dwellings, as I did then, they use net stock calculated by the double declining balance formula. It seems to me impossible to suppose that this pattern remotely resembles that of the flow of services of dwellings during their service life. The 1950–62 growth rate of the dwellings stock computed by this formula, as they initially estimate it for use in their table I, is 4.53 (computed from data provided by Jorgenson and Griliches).

The deflator for residential construction may be presumed to have an upward bias for the same reason as the deflator for nonresidential construction. Jorgenson and Griliches attempt to allow for this by deflating residential construction expenditures by the OBE Deflation II series for nonresidential construction in place of the residential construction deflator. This raises the 1950–62 growth rate of their dwellings stock by 0.39 points, from 4.53 to 4.92.[48]

Residential land is combined with other land in the Jorgenson-Griliches procedure. As already indicated, their combined growth rate (and contribution to growth) is zero.

If I had used the Jorgenson-Griliches growth rate for the net stock of dwellings, and multiplied it by *my* share weights, I would have obtained a much lower figure than I did for the contribution of dwellings to growth of total national income: probably around 0.13 percentage points instead of 0.25.[49] My output per unit of input series would then have been raised by about 0.12 points. I am not, unfortunately, able to quantify the effect upon *their* estimates of the difference between us in the measurement of the contribution of housing.

Summary comment

The Jorgenson-Griliches estimates of the contribution of capital and land to GNP growth differ from mine because of (1) differences in weights; (2) differences in the initial method of measuring capital and land inputs, including the difference in method of estimating the contribution of dwellings; (3) their substitutions of price indexes; and (4) a utilization adjustment they introduce. I have already examined the weights (1); discussion of the utilization adjustment (4) is deferred to section VII.

The total effect of all their price substitutions (3) was to raise their 1950–62 growth rate of total input, and lower that of output per unit of input, by 0.23 percentage points [computed from 1, tables II and IV]. This calculation is based on use of their weights. Of this amount, in the neighborhood of 0.07 points derives from adjustment of construction. The remaining 0.16 points are due to substitutions of price series for producers' durables and inventories (almost entirely the former), which I regard as illegitimate. (It is partly offset by an output adjustment described in section VI below.)

The effect of (2), differences in measures of input (*other* than price substitutions for producers' durables and inventories), I can calculate only with the use of my weights—that is, the numbers refer to the change in my series that use of their input indexes would introduce. Of the difference between us in total input and output per unit of input, the difference in our measure of inventory input (excluding their price substitution) accounts for about 0.03 percentage points, and land indexes for none. Their nonresidential structures and equipment series rises enough more than the revised OBE series I would use to account for 0.07 points; both are based on the OBE II construction deflator. The difference in residential structures accounts for *minus* 0.12 points. The difference in capital stock measures (or their equivalent, in the case of dwellings) thus accounts for *minus* 0.02 points of the difference in our output per unit of input measures, based on my weights and apart from the effects of their price substitutions for producers' durables and inventories.

My incorporation of revised OBE data for nonresidential structures and equipment would *add* 0.04 points to the difference between us.

47. The increase in gross product at factor cost, valued in 1958 prices, was put at $15.7 billion.

48. From 1950 to 1962, the Deflation II series rises less than the residential construction deflator, so the substitution implies that the bias in the deflator is *downward* in this period. This accounts for the negative adjustment in the growth rate of output that the following section shows is introduced by this price substitution. Over the longer time span reflected in the capital stock series, the adjustment is in the right direction.

49. This calculation supposes that about one-fourth of the weight I assign to dwellings pertains to sites, as distinguished from structures.

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VI. Effect of Price Index Alterations on Output

JORGENSON and Griliches substitute investment price indexes in deflating the investment components of GNP as well as in measuring capital stock. The 1950–62 growth rate of their private domestic GNP is raised by 0.09 percentage points [calculated from 1, tables II and IV] and this partially offsets the deduction from output per unit of input they introduced by substituting prices in capital stock measurement.

To isolate the separate effects of their price substitutions on output, I duplicated their calculations. The breakdown of their adjustment is: producers' durable equipment 0.10; nonresidential structures 0.03; residential structures, −0.03; and inventories, 0.00. (The total, 0.10, presumably differs from their 0.09 because of rounding.) Thus, their entire output adjustment stems, on balance, from the use of consumers' durables prices to deflate producers' durables; none of it results from the legitimate attempt to adjust construction prices.

VII. The Utilization Adjustment for Capital and Land

MORE than half of the difference between our output per unit of input growth rates in 1950–62 results from an adjustment that Jorgenson and Griliches introduce for changes in utilization of capital and land. Their general idea is that the hours per year that capital is used have increased secularly, and that a given percentage increase in capital hours per dollar of capital has the same effect on output as a similar percentage increase in the quantity of capital. Their capital utilization adjustment raises the contribution of their total input series by 0.60 percentage points in their full 1945–65 period and by about 0.58 points in the 1950–62 period.[50] Their method of deriving this adjustment is theoretically unsound, and the statistical procedures they followed to obtain their estimates are altogether untenable. In my view, their capital utilization adjustment should be discarded.

Series for manufacturing equipment powered by electric motors

The starting point for the adjustment was a series contained in a 1963 SURVEY OF CURRENT BUSINESS article by Murray F. Foss [4]. Most production equipment in manufacturing is powered by electric motors. Foss used Census data for electric power consumption and the horsepower of electric motors to estimate the average number of hours per year that electric-power-driven equipment in manufacturing establishments was utilized. He concluded that its utilization increased by an amount on the order of one-third to one-half from the 1920's to the mid-1950's. The dates for which he made actual calculations were the Census years 1929, 1939, and 1954 [4, table 2, line 7]. Growth rates of average equipment hours calculated from his utilization estimates for these years are −0.45 from 1929 to 1939, 2.15 from 1939 to 1954, and 1.10 from 1929 to 1954. Jorgenson and Griliches made a similar comparison of the years 1954 and 1962 [1, table X, line 6]. From 1954 to 1962, the growth rate was 1.33. Jorgenson and Griliches used the 1939–54 rate for all annual changes in the 1945–54 period and the 1954–62 rate for all annual changes after 1954. They thus obtained average rates of increase in utilization of about 1.72 for 1945–65 and 1.60 for 1950–62.

These rates almost certainly are much higher than the trend rate, which is what Jorgenson and Griliches are seeking, or the rate that would be obtained if calculations could be made directly from the terminal years of these periods. The average rate from the depression year 1939 to 1954 must have been greatly raised by the difference in cyclical position; the rate from 1945 or 1950 to 1954 must have been much smaller than the rate over the 1939–54 period as a whole.[51] The rate from 1954, itself a recession year, to 1962 or 1965 probably was also raised by cyclical influences.[52] A minimal downward adjustment of their estimates to eliminate cyclical incomparability in the pre-1954 period could be made by substituting the 1929–54 rate where they use the 1939–54 rate. This would lower the 1945–65 growth rate of utilization from 1.72 to 1.22, and the 1950–62 rate from 1.60 to 1.25. Probably a better procedure would be to use the 1929–62 rate, which is 1.16, as representative of the trend throughout the period, hence for both the 1945–65 and 1950-62 periods; this would cut their 1950–62 rate by more than one-fourth and their

50. The 1945–65 figure of 0.60 points was provided by Jorgenson and Griliches; it can also be approximated from their published data.

The average growth rate of their capital utilization series itself was 1.72 in 1945–65 and 1.60 in 1950–62. (See the following text paragraph.) Multiplication of their 1950–62 growth rate of 1.60 by their average 1950–62 capital-land share of 0.36175 yields an estimated contribution of 0.58 percentage points.

(In this period, the combined contribution of their capital utilization adjustment and the labor hours adjustment was 0.52, thus the contribution of the labor adjustment was apparently about −0.06. I use this figure in section VIII.)

51. Foss himself wrote: "In fact, some of the illustrations in this article suggest that the major change in relative equipment utilization took place during and immediately after World War II, and that changes since then (aside from cyclical movements) have been relatively small" [4, p. 8].

52. Because Jorgenson and Griliches interpolate between far-removed dates rather than use annual estimates, the capital utilization adjustment obviously cannot purport to adjust capital input for shortrun variations in utilization. Jorgenson and Griliches note this and state that it "allows only for the trend in the relative utilization of capital" [1, p. 266]. My objection to their procedure is the same whether one construes their series as representing the trend rate in 1945–65 and 1950–62 or the actual changes from 1945 to 1965 and from 1950 to 1962.

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1945–65 rate even more. Overstatement of the increase in this series from the absence of any procedure to deal with the cycle is, however, among the least of my objections to their utilization adjustment, and there is no need to pursue it further.

A second limitation is that the weights used to construct the all-manufacturing utilization series are inappropriate for the use to which Jorgenson and Griliches put it. "Available kilowatt hours of motors" were used as weights to combine utilization ratios for the component industries in obtaining the all-manufacturing utilization series.[53] For use in converting a series for the value of power-driven equipment in manufacturing establishments to a capital input series, the utilization ratios for all manufacturing should be based on the use of the *value* of power-driven equipment in each industry as that industry's weight. This was noted by Foss [4, p. 11] but is not mentioned by Jorgenson and Griliches. A series so constructed is not available for comparison, nor are the value data for power-driven equipment that its construction would require. Perhaps the two sets of weights would yield tolerably similar results; at the 2-digit level, Foss finds, with some exceptions, fair correspondence between distributions of *total fixed* capital and installed horsepower. Nevertheless, the possibility of appreciable error is present in the manufacturing series.

Equipment values are not available for mining either, but similar utilization ratios for the five mineral industries were published separately by Foss. Solely as an illustration that weights *may* matter, I calculated all-mining utilization ratios with alternative proxies for capital values. Use of "available kilowatt hours" as weights yields a 4 percent increase in utilization from 1929 to 1954, whereas use of "electric power consumed by motors" would yield a 16 percent decline. Like the manufacturing series, these calculations used 1929 weights for 1929 and 1954 weights for 1954. I argue subsequently that fixed weight indexes would be more appropriate. I calculated fixed weight indexes using four alternative sets of 1929 weights. Use of "value of machinery and equipment installed during 1929" yields a 14 percent increase in utilization from 1929 to 1954; "available kilowatt hours of motors" a 12 percent increase; "national income originating," a 2 percent increase; and "electric power consumed by motors," a 1 percent decrease. Probably the first two are better proxies than the last two for equipment values, but differences are large and investigation is needed.

In the absence of tests of its effects, the inappropriate weighting of the manufacturing equipment series adds to the reservations about the Jorgenson-Griliches use of this series that is created by their failure to allow for cyclical differences. But there is a fundamental conceptual objection to their use of this series to adjust capital input that would remain if value weights were used and cyclical adjustments were made. To develop this point, I shall proceed as if this had been done.

Conceptual problem of incorporating utilization data

The trend rate of capital utilization provides interesting information. But to integrate this information into the type of classification of growth sources that Jorgenson and Griliches or I employ, one must know the *reasons* that utilization increased and the *amount* due to each reason. Even if one knew exactly how much utilization had changed, in the absence of this additional information he still would not know the amount of the increase in output that (prior to any utilization adjustment) is included in the contribution of input (or any component of input) and the amount that is included in the contribution of output per unit of input. This is a subject that Jorgenson and Griliches do not discuss at all. However, their procedures imply that, prior to the introduction of their capital utilization adjustment, the effects of an increase in capital utilization necessarily appear only in their output per unit of input series.

The average hours "worked" by power-driven equipment in manufacturing establishments (adjusted to eliminate short-term fluctuations) may actually change for quite varied reasons, and these have altogether different implications for the analysis.[54]

1. The effects of some types of change are fully measured by the increase in the capital stock, so that any additional allowance for increased utilization duplicates the change in the capital stock measure. These types can be described as changes in composition of capital, of which three main categories can be distinguished.

(a) At any point in time, producers can select among varieties of equipment with different characteristics that sell at different prices. One characteristic that can be purchased at a higher price is greater reliability: longer use without downtime for regular maintenance or to replace worn-out or defective components or the entire machine. If producers shift to higher priced equipment, average "hours worked" will increase but so will the capital stock series. A priori there is reason to suppose that, as capital has become more abundant relative to labor, the use of more expensive equipment has been one aspect of the rising capital-labor ratio.

(b) At any point in time, different manufacturing industries vary in the hours they use capital. On the assumptions that Jorgenson and Griliches and I accept, the rate of return, as measured by the ratio of net earnings to net value, is, nevertheless, the same in each manufacturing industry. If hours in each industry are unchanged, but the weights of the industries alter, the average hours in manufacturing as a whole will change but capital input should not.

Suppose Industry A and Industry B each have $1 million of equipment, but

53. Foss confirms this statement, which the reader can check by use of Foss's ratios for mineral industries [4, table 5], for which the procedure was similar and for which industry data are shown. For minerals industries, Foss shows a five-industry breakdown. The all-industry utilization ratio in his column 6 is equal to the ratios for the individual industry groups weighted by "available kilowatt hours of motors" as shown in column 2.

54. Not all of these possibilities had occurred to me when I discussed capital utilization in *Why Growth Rates Differ* [2, pp. 154–155]. I would now word that section somewhat differently.

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Industry A operates on three labor shifts, or 120 hours a week, and Industry B on one shift of 40 hours, and capital is used during the same time periods. Equilibrium requires the same rate of return in the two industries; otherwise, there would be an incentive for capital to move from one industry to the other. If the rate of return is 10 percent, the product (as indicated by earnings) of the $1 million of equipment in each industry is $100,000. The product of $1 million of equipment per hour it is used in a week must then be three times as high in Industry B as in Industry A ($2,500 against $833.33). This must be the case, or the rates of return would differ. If (because of changes in demand patterns or for other reasons) Industry B gets bigger relative to Industry A, average hours worked by equipment in the two industries combined will decline, whereas if Industry A gets bigger average hours will increase, because Jorgenson and Griliches use a capital utilization series that is constructed with shifting industry weights. They would therefore measure the former development as a decline in equipment input, the latter as an increase. This is a simple "error of aggregation." It results from giving an hour worked by $1 million of equipment in each industry the same weight.

To illustrate, suppose that in a second year the total value of equipment is $2,000,000, as before, but Industry A now has $1,500,000 and Industry B $500,000. Based on the use of capital stock to measure input, without a utilization adjustment, the contribution of equipment to output (in first-year values) remains $200,000; only the division between industries has changed—to $150,000 in Industry A and $50,000 in Industry B. This correct result could also be obtained by correctly weighting hours: The value of equipment (in millions) in each industry is multiplied by average weekly hours, and the contribution to output of an hour worked by $1 million of equipment is counted as $833.33 in Industry A and $2,500 in Industry B. In Industry A, equipment value times hours increased from 120 to 180; multiplication by $833.33 yields an increase in equipment's contribution from $100,000 to $150,000. In Industry B, equipment value times hours dropped from 40 to 20; multiplication by $2,500 yields a drop in the contribution of equipment from $100,000 to $50,000. The total contribution of equipment at first-year values is again $200,000 in both years.

In this example, the Jorgenson-Griliches procedure would erroneously yield an increase in equipment input of 25 percent, instead of no change, because it assigns equal weight to an hour worked in each industry.

Foss has investigated the effects of changes in industry weights in selected periods and concluded that the change in the all-manufacturing utilization ratio he observed chiefly reflected changes in individual industries rather than in industry mix, although he did note that there probably *was* a shift toward continuous process manufacturing industries, particularly aluminum, refined petroleum, and chemicals.

(c) At any point in time, the number of hours that different types of equipment are used varies widely *within* any establishment, firm, or industry. If the composition of assets changes, the average hours worked by all combined will rise or fall even though there is no change for any particular type. The hours for the same type of equipment may also vary among uses, and this distribution may change over time. These cases are identical to that discussed in (b). Greater use does not imply larger earnings per dollar of capital value. Two machines of different types (or of the same type in different uses) must be assumed to contribute equal amounts to production per dollar of value, not per dollar of value mutliplied by hours worked. If this assumption is invalid, rates of return vary and the economic unit is not in equilibrium. The sensitivity of a conglomerate average-hours-worked series to changes in weights of different types of machines, and to changes in weights of different uses of machines, must be high because the range of hours is large. Shifts of this type could well dominate the long-term movement of "average hours" series for individual firms, establishments, and industries.

Unless a capital utilization series can be standardized to eliminate the effects of *all three* types of "mix" changes, it is useless for the purpose to which Jorgenson and Griliches put it. I cannot imagine how such standardization could be achieved. But even if it could, this would surmount only one of the difficulties.

2. The amount of downtime of machines depends in part on the number of workers who operate them (which affects, among other things, the speed of machine operation), their skill, and the care they exercise. It depends also upon the number and skill of the workers who repair machines. The skill of engineers and others employed by equipment suppliers to service customers is often a crucial determinant of the amount of time lost from breakdowns. If machine hours increase because of an increase in the quantity or an improvement in the quality of labor, this is already counted in principle, and one hopes in practice, as a contribution of labor.

3. The amount of downtime depends in part on expenditures for maintenance. A firm presumably attempts to allocate expenditures among maintenance, purchases of new capital goods for replacement, and production labor in such a way as to minimize total cost. Maintenance expenditures may change because the price of maintenance changes relative to prices of capital goods and production workers; in this case, there is no ascertainable contribution to growth. Maintenance expenditures may also change because management devises a better procedure to determine the minimum cost combination. If they increase for this reason, only the *net* benefit remaining after deducting the increase in maintenance costs from the saving in capital and labor costs contributes to an increase in output.[55] Classification of any net benefit is discussed in case 7 below.

4. Downtime depends in part on the inventory of spare parts; any change is already covered as a contribution of

55. Unless output is measured on the Scandinavian "gross-gross-product" basis, which double counts maintenance as well as capital consumption.

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inventories. It depends also on the speed with which parts and servicemen can be obtained; this, in turn, depends on capital and labor in the transportation industries, which are already counted as capital and labor input.[56]

5. The hours that machines are used may change because of a change in the average hours worked per worker; in my study I allow, in principle, for this effect in my adjustment of labor input for changes in labor hours of full-time workers [2, p. 61, n. 11]. (I found no significant change in labor hours of full-time workers in the economy as a whole over the period analyzed so this case did not actually affect my estimates.)

6. Machine hours may also change because shift work becomes more or less prevalent *in particular activities*. In my estimates, such a development was regarded as a component source of the change in output per unit of input [2, pp. 152–154, 173–174], and in my international comparisons, I made a specific estimate for this determinant. However, I found no evidence of a significant change in shift work in the United States in 1950–62, and therefore estimated the contribution of changes in shift work to be zero [2, pp. 152–154, 173–174].

7. The hours worked by machines may rise, or in some cases fall, because of advances of knowledge and its dispersion. These may:

(a) Provide more reliable machines without increasing their cost—a development variously described as "unmeasured" quality change in capital goods or "embodied" technical progress. (In practice, "measured" quality change covered in case 1(a) above and "unmeasured" quality change are often intertwined.)

(b) Enable management to make more continuous use of machines. Foss writes:

> "Also of importance over the long run has been the advance in knowledge acquired by management in making more efficient use of machines. One example of this has been the efforts by many firms to smooth out within the year the production peaks which come from seasonal or other short-lived peak loads and which frequently entail the use of standby equipment with relatively low annual utilization. . . . Within particular industries there have undoubtedly been efforts to introduce continuous, automatic operations in which machines tend to be used with a high degree of intensity."

(c) Improve communications and speed transportation of parts and of key personnel needed for repairs, notably by air.

(d) Improve the decisionmaking process generally—notably with respect to determination of the trade-off among costs incurred for maintenance, replacement, downtime, speed of operating machines, waste of materials, and quality of product.

This list of possible reasons for changes in average machine hours may not be exhaustive. But it suffices to make clear that, unless the reasons for changes in capital utilization are known and their effects can be isolated and quantified, data on capital utilization cannot be integrated into a classification of growth sources of the type Jorgenson and Griliches and I use. It is possible that the entire change indicated by the Jorgenson-Griliches series is already reflected in capital and labor input or counterbalanced by higher maintenance costs, and is not a component of the Jorgenson-Griliches output per unit of input series prior to their utilization adjustment, or of my series. Or any or all of it may be a component. Jorgenson and Griliches never mention, and appear unaware of, the range of possibilities.

Among the possible reasons for an increase in capital hours that I have listed, two would or might contribute to a change in output per unit of input as I measure it, and as Jorgenson and Griliches do prior to introduction of their utilization adjustment. The effects of one of these, changes in shift work in particular activities, I estimated [2, pp. 152–154] to be zero in the economy as a whole in 1950–62, though admittedly on the basis of inadequate information; better data may permit more reliable estimation in future years. The other is advances in knowledge and their dispersion. There is no clear presumption that these led to an increase in the hours that capital goods are utilized or that, if they did, the net saving in unit costs bore any systematic relationship to the change in machine hours. But if there was such an effect, it appears in the "advances of knowledge" component of my output per unit of input series. I see scant possibility that it will ever be possible to isolate this effect.

If one could isolate and measure this effect and the shift-work effect, one would have a choice of transferring them to the contribution of capital (evidently the Jorgenson-Griliches preference) or of classifying them as component sources of the growth of output per unit of input. The latter would be my preference because it is not the saving-investment process that governs these income determinants [2, p. 144], and I shall say a little more about this at the end of this article. But it would really make little difference to the sophisticated reader where they were shown because he could move them at will.

The Jorgenson-Griliches estimates

The Jorgenson-Griliches estimates implicitly assume (1) that the utilization series would be unchanged if weighted by value of power-driven machinery and (2) that the entire effect of increased utilization appears in their productivity measure until they make their utilization adjustment, hence that *only* advances in knowledge and changes in shift work *within industries* affected utilization of manufacturing equipment driven by electric motors. Since they do not diminish the growth of their capital stock series by

56. Parts of points 2 to 4 are nicely illustrated by an advertising letter that happened to reach me as I was writing this section. It states:

"Are you aware that the . . . Corporation has for the past fifteen years been providing preventive and corrective maintenance to a growing number of manufacturers and users of electronic and electromechanical devices?

"Our experience in performing both scheduled and emergency service (supported by factory-trained personnel, local stocking of replacement parts, and quick response to emergency calls) aims to improve your operation in terms of lower 'down-time' and higher reliability."

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shortening service lives as they increase capital utilization, they also assume (3) that increased utilization does not cause equipment to wear out more rapidly. (If there is such a user cost, the utilization adjustment duplicates their original estimate of the contribution of capital for this reason.)

I know of no reason to accept this set of assumptions. But it is instructive to calculate what the quantitative importance of the change in utilization of power-driven equipment in manufacturing would be if by chance all these assumptions were correct. First, the weight in total input must be calculated. *All* nonresidential structures and equipment represented 13.6 percent of total input in the private domestic economy in 1950–62, according to my net earnings weights. *All* producers' durables in manufacturing establishments represented about 14 percent of the value of the total stock of private nonresidential structures and equipment, hence 1.9 percent of total input. Machinery in manufacturing establishments driven by electric motors represented at the outside 70 percent of the value of the stock of producers' durables in manufacturing establishments in 1950–62, hence at most 1.4 percent of total input. If the utilization of such machinery increased 1.16 percent a year (the figure I suggested earlier as the trend rate of the utilization series), and if an increase in utilization is treated (as Jorgenson and Griliches *do* treat it) as equivalent to the same percentage increase in the quantity of such equipment, this raises the growth rate of total input (net product basis) in the private domestic economy by 0.016 percentage points (1.4 percent of 1.16 percent) and lowers that of output per unit of input by the same amount. This would be my estimate if I were to accept the Jorgenson-Griliches utilization estimates and their three implicit assumptions mentioned in the preceding paragraph (which, of course, I do not). Even with the Jorgenson-Griliches utilization increase of 1.60 percent a year, the contribution is only 0.022 percentage points in 1950–62. If, as in the Jorgenson-Griliches estimates, depreciation is added to the weights, the calculated contribution to gross product growth would probably come up to 0.03.

How do Jorgenson and Griliches get from 0.03 to 0.58? By introducing the "very strong assumption" (their language) that utilization of *all* types of capital and land in *all* activities increased at the same rate as did machinery in manufacturing establishments driven by electric motors! This assumption is not only "very strong"; it is truly magnificent in its implausibility. Utilization of structures, sites, furniture, and office equipment in manufacturing, of office buildings, of physicians' automobiles, of houses and their sites, of railroad stations, of farmland (have the seasons changed?), of inventories (whatever this may mean), of literally everything has increased, and at the same rate as machinery driven by electric motors in manufacturing establishments!

If one is willing to assume that the change in machinery hours in manufacturing was due only to advances in knowledge and changes in shift work within industries, he might perhaps, I suppose, go even further and assume there was some net increase in *machinery* hours outside manufacturing after 1950, and thus raise the figure derived from the manufacturing series a little. Foss found some examples of machinery in nonmanufacturing industries in which utilization increased from the 1920's to the 1950's as well as some where it did not. For example, in two of five mining industries, utilization of power-driven equipment increased from 1929 to 1954 while in three it declined, although it should be noted again that these years are not cyclically comparable.[57] Locomotive use increased while freight car use decreased. Utilization in electric utilities increased from the late 1930's to 1948, but not from 1948 to 1958. And so on. But even doubling the manufacturing figure would yield no more than 0.06 points in their gross product growth rate. Jorgenson and Griliches have applied the increase in utilization not only to all machinery but to all other types of capital and to land. Since all capital and land received 36.2 percent of their total input weight (inclusive of depreciation as well as indirect taxes), this raised the contribution of the utilization adjustment from 0.03 to 0.58 (36.2 percent of 1.60).

The conclusion to be drawn from the preceding discussion—it seems to me inescapable—is that the Jorgenson-Griliches utilization adjustment must be rejected.

After this summation, it may seem superfluous to mention that the Jorgenson-Griliches procedures also contain an important inconsistency. Houses and sites represent a huge part of the stock of capital and land, and much of the capital utilization adjustment reflects the assumption that the hours houses are used have increased. Even if Jorgenson and Griliches were right to assume that people have been spending an increasing amount of time in their houses, per dollar value in constant prices of house, this would not affect their output measure because (fortunately) OBE does not adjust its deflated consumer expenditure series for housing to allow for the supposed increased utilization, and Jorgenson and Griliches do not adjust the OBE series on this account. Hence, Jorgenson and Griliches are arithmetically wrong to subtract the utilization adjustment for residential structures and the residential portion of their land input from the growth of productivity.[58]

57. The Foss series for all mineral industries rises (but its 1929–54 growth rate is only 0.17 as compared with 1.10 for manufacturing) because of a very sharp increase in nonmetal mining, which receives a rather heavy weight (20 percent of the total in 1929 and 27 in 1954) based on available kilowatt hours of motors.

58. Let me stress that my criticisms of the Jorgenson-Griliches utilization adjustment do not extend to the article by Foss, which I have praised in print on several occasions. Nor do I mean to deny the value and relevance to growth studies of series of the type that Foss prepared for power-driven equipment in manufacturing and mining industries and a few other types of fixed capital and that might be prepared for additional types. Indeed, like Jorgenson and Griliches, I should be very glad to see such studies extended. I believe Foss is correct in suggesting [4, p. 10] their importance for analysis of long-term changes in capital-output ratios. Studies of shift work would be immediately useful. More generally, the fact that capital utilization series do not easily fit into the type of classification discussed in this article does not imply that one cannot fruitfully explore the relationship between changes in capital utilization and economic growth. There may be a valid analogy with studies, obviously valuable, of such questions as: "How does transportation affect growth?" or "How did high wages in the United States affect American as compared with European growth in the nineteenth century?" Studies of these questions, too, do not yield results that fit into the type of classification of growth sources that is examined here.

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VIII. The Measurement of Labor Input

JORGENSON and Griliches and I measure labor input in ways that are similar in spirit and general approach. Both our input series take into account employment; hours worked, with an allowance for a productivity offset as hours change; and the education of the labor force. My series allows, in addition, for changes in the distribution of total hours worked among age-sex groups whereas theirs does not, but Jorgenson and Griliches agree that this should be done [1, p. 269].[59] Thus a comparison does not raise major conceptual issues.

However, the data and procedures we actually use to measure labor input differ at almost every step, and it is necessary to consider whether this introduces a difference into our estimates of productivity change. My conclusion is that our labor input series are in rather close agreement with respect to the common elements of our estimates, after allowance for my inclusion of government employees.[60] Their omission of an age-sex measure contributes to their higher estimate of the growth of output per unit of input.

Employment, hours, and education

Because of a difference in classification with respect to employment and hours effects, it is desirable to combine the two for comparison. It is also necessary to build up a comparison in several parts.

My employment series is based on household survey data from the *Monthly Report on the Labor Force.* Jorgenson and Griliches rely on the OBE series for persons engaged in production, which is the sum of its full-time equivalent employees and active proprietors of unincorporated enterprises. This series is mainly constructed from establishment reports.

I have attempted to compare data from the two sources at the all-civilian-employment level to try to determine whether movements of the two series are statistically consistent from 1950 to 1962. My series for civilian employment has a 1950–62 growth rate of 1.03.[61] To obtain a conceptually similar series for comparison, I start with OBE series on persons engaged in production, excluding military employment; substitute the OBE series for full-time and part-time employees for full-time equivalent employees; add my estimates for unpaid family workers; and adjust the 1962 figure to exclude Alaska and Hawaii by application of a 1960 overlap ratio. The resulting series has a 1950–62 growth rate of 1.00. For this timespan, the *statistical* difference between MRLF and OBE data would, by this test, make the Jorgenson-Griliches employment series grow 0.03 less than mine. However, Jorgenson and Griliches omit unpaid family workers. The 1950–62 growth rate of their employment series for private industries would be lowered by 0.06 if my estimates for unpaid family workers were added to their estimates. The two differences together would make their series grow 0.03 more than mine.

We each estimate the effect of changes in hours worked by measuring changes in average hours, and allowing for a productivity offset as hours of full-time workers decline. For civilian workers, my resulting series for the effect of changes in hours upon the work done in a year of employment has a growth rate of −0.25 from 1950 to 1962 [2, table 6–6, and an adjustment to exclude military personnel]. This figure includes the effect of a major increase in part-time employment; in fact, it mainly reflects the effect on hours of an increasing part-time component of employment, as distinguished from changes in hours of full-time workers. Two figures from the Jorgenson-Griliches estimates must be combined for comparison. Their series for the effect of hours on the work done in a year of *full-time* employment has a growth rate of about −0.09 from 1950 to 1962.[62] The increase in part-time work is reflected in the employment component of the Jorgenson-Griliches labor input series because their employment series is computed on a full-time equivalent basis. The 1950–62 growth rate of the OBE persons engaged series for private industries is lower by 0.23 than that of an otherwise similar series in which the OBE series for full-time and part-time employees is substituted for full-time equivalent employees. Thus, the combined effect of changes in full-time hours and increased part-time employment on the Jorgenson-Griliches labor input series is −0.32 (−0.09 plus −0.23), which compares with my −0.25. When the difference of −0.07 is added to the 0.03 difference in the employment growth rates, it appears that the difference between our employment and hours series makes their labor input series grow 0.04 points less than mine. Based on their 1950–62 average labor share, this would make their estimate of the contribution of total input 0.03 points lower, and of output per unit of input 0.03 higher, than use of my series.[63]

59. They also say that the labor input series should, in addition, be standardized by occupation and industry. In my view, this is a conceptual error, but since they did not do this, no discrepancy between our estimates is introduced.

60. To adjust for the difference in the scope of our employment estimates, I use OBE data for general government employment. This is appropriate because these data are consistent with the government product data used in Section I above to reconcile productivity estimates. The difference in the scope of our estimates causes little difficulty in comparing other components of our labor input series because, with unimportant exceptions, we each assume that changes are the same for total private employment as for total civilian employment.

61. Computed from 2, tables 5-1A, 5-1C, 5-1D, and C-1. In my estimates, all series are linked at 1960 to eliminate the effect of adding Alaska and Hawaii to coverage of the data.

62. In footnote 50, I calculated that their hours adjustment for labor amounted to −0.06 percentage points in the growth rate of total input. Division of this amount by their average labor share of 0.638 in 1950–62 yields −0.09.

63. I have not isolated the effect of one of their procedures in this reconciliation of our estimates. Although unpaid family workers are excluded from the Jorgenson-Griliches employment series, they do affect total labor input via the hours estimates. Jorgenson and Griliches inform me that they obtained average hours by dividing the BLS establishment-based series for total manhours worked in the private economy (which includes unpaid family workers) by persons engaged in production (which excludes unpaid family workers). Hence, the decline in the ratio of unpaid family workers to total employment presumably intensifies the decline in their average hours series. This reduces the growth in labor input insofar as it was not offset by their efficiency adjustment.

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We each estimate the effect of the rise in education upon the quality of labor. The growth rate of my "education quality" series for civilian employment is 0.75 [2, table 8–5]. Despite procedural differences, their rate is also 0.75 [computed from 1, table VII]. No discrepancy in our labor input series is introduced by education.

Age-sex composition

My "quality index" for changes in the age and sex composition of hours worked by civilian employees has a −0.15 growth rate from 1950 to 1962 [2, table 7–7, and an adjustment to exclude military personnel]. Jorgenson and Griliches omit this labor characteristic from their measure. Based on their average 1950–62 labor share, the omission causes their total input series to grow 0.11 points more than mine from 1950 to 1962, and their output per unit of input series 0.11 points less.

IX. Summary of Statistical Review

AN approximate reconciliation of our output per unit of input estimates can now be compiled. It is provided in table 1.

The initial difference between our estimates is 1.27 percentage points (line 3). When my estimates are adjusted to conform to the definition and scope of output used by Jorgenson and Griliches, and their estimates are adjusted to my time period, the difference is reduced to 1.08 (line 6). If my estimates are adjusted to incorporate revised OBE data for the stock of nonresidential structures and equipment, including use of the OBE Deflation II series for nonresidential structures, the difference between us is widened to 1.12 percentage points (line 9).

I found only one significant difference in our classifications of growth sources, as between input and output per unit of input. My input series is broader in that it includes the effect on labor "quality" of shifts in the age-sex composition of hours worked, whereas such shifts affect the Jorgenson-Griliches series for output per unit of input. This source made a negative contribution to growth in 1950–62, so that adjustment of their output per unit of input series to my classification narrows the difference between us from 1.12 to 1.01 percentage points (line 12).

The remaining 1.01 points, which are divided among components in lines 13 to 20, result from differences in statistical procedures. These are of two types: differences in weights and differences in input measures.

Not all of the difference between our weights is relevant here; the portion that is due to inclusion by Jorgenson and Griliches of depreciation and the portion that is due to their exclusion of government and the international sector are related to the difference in output measures, and their effects were previously eliminated in moving from line 3 to line 6. (There is one exception: The effect on the capital utilization adjustment of including depreciation in the weights was not eliminated and is included in the effect of the capital utilization adjustment in line 18.)

The division of the 1.01 points in lines 13 to 20 is, in principle, that which results from first measuring the effect upon my series of substituting their weights for mine and then measuring the effects of substituting their

Table 1.—Reconciliation of Denison and Jorgenson-Griliches Estimates of the Growth Rate (or Contribution to Growth) of Output per Unit of Input (Percentage points)

Reported output per unit of input growth rates:	
1. Denison, total national income, 1950–62 (p. 1)	1.37
2. Jorgenson-Griliches, private domestic GNP, 1945–65 (p. 1)	.10
3. Difference 1–2	1.27
Rates adjusted for definition and scope of output and time period:	
4. Denison, private domestic GNP, 1950–62 (p. 3)	1.38
5. Jorgenson-Griliches, private domestic GNP, 1950–62 (p. 2)	.30
6. Difference 4–5	1.08
Rate adjusted for new data:	
7. Adjustment of Denison series to incorporate new "structures and equipment" data (p. 14)	.04
8. Denison, private domestic GNP, 1950–62, adjusted, 4+7	1.42
9. Difference 8–5	1.12
Rate adjusted for difference in classification:	
10. Adjustment of Jorgenson-Griliches series to eliminate effect of changes in "labor quality" due to shift in age-sex composition of hours worked [a,c] (p. 24)	−.11
11. Jorgenson-Griliches, private domestic GNP, 1950–62, classification adjusted 5–10	.41
12. Difference 8–11	1.01
Breakdown of remaining difference of 1.01:	
13. Difference in division of input weights between labor and capital-land [b,c] (p. 5)	.08
14. Difference in inventory capital stock series [d] (p. 14)	.03
15. Difference in nonresidential structures and equipment capital stock series [d] (p. 16)	.07
16. Difference in residential structures procedure [d] (p. 17)	−.12
17. Jorgenson-Griliches substitutions of price indexes for equipment and inventories, net effect [e]	.07
Effect via output	−0.09 (p. 18)
Effect via input [a]	.16 (p. 17)
18. Jorgenson-Griliches capital-land utilization adjustment [a] (p. 18)	.58
19. Difference in estimates of employment and hours (p. 23)	−.03
20. Other differences [f]	.33

[a] Amount calculated with Jorgenson-Griliches weights.
[b] Reflects the net effect on the Jorgenson-Griliches weights of (1) counting as capital-land earnings all indirect taxes and other reconciliation items between factor cost and market price measures and (2) allocating to capital-land earnings a smaller portion than Denison of proprietors' income.
[c] Calculation based on Denison input series.
[d] Amount calculated with Denison weights.
[e] The construction price substitutions had no effect on output. Their effect on input is already taken into account in lines 7, 15, and 16.
[f] This estimate was obtained as a residual.

To obtain a full reconciliation it would have been necessary after line 9 to measure (1) the changes in my estimates that would have been introduced by my use of the Jorgenson-Griliches weights (except for depreciation) and (2) to measure the effect on their estimates, based on their weights, of the differences between us in measuring inputs. The first could be done for the division of weights between labor and capital-land, but not within the capital-land aggregate. The second could be done for most differences, but lines 14 to 16 were calculated by use of my weights instead of theirs. Line 20 therefore includes:

1. The effects of differences in the allocation of the total capital-land weight among components, including the consequences of the Denison division of the economy among sectors and the Jorgenson-Griliches adjustment for capital gains and taxes.
2. The difference between the amounts shown in lines 14, 15, and 16 and the amounts that would be obtained in these lines if Jorgenson-Griliches weights were used in the calculation instead of the Denison weights.
3. Possible errors in the calculations of amounts shown in several other lines of this table resulting from my use of average 1950–62 weights instead of annual weights (in the case of Jorgenson-Griliches estimates) or 1950–54, 1955–59, and 1960–62 weights (in the case of the Denison estimates) to calculate differences.
4. Rounding discrepancies.

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input measures for mine when their weights are used; the breakdown would be different if the order were reversed. Two departures from this principle should be noted. The effect of a different allocation of total net capital-land earnings among components, the principal subject of section IV, was not measured and is included in "other differences" in line 20. Also, the effect of using different capital stock series (or a different method in the case of dwellings) could be measured only with the use of my weights (lines 14, 15, 16), and the difference between these results and those that would be obtained with their weights is also included in "other differences" in line 20.

The difference between us of 1.01 points shown in line 12 would be 1.04 were it not for a small offset (line 19) flowing from a difference in our estimates of employment and hours, which I did not evaluate. I have presented what I regard as compelling reasons to consider each of their procedures that contributes to this discrepancy as inferior. Nothing in their article suggests to me a change in my estimates.

Well over half of the entire statistical difference stems from the Jorgenson-Griliches utilization adjustment for capital and land (line 18). If increased utilization of capital and land resulting from advances in knowledge had really contributed 0.58 percentage points to the growth rate, then this amount would be regarded as due to classification rather than to statistical procedure. I have stressed my reasons for concluding that this is not the case. Although the portion of the total gains from advances in knowledge that is transmitted to higher productivity by the mechanism of lengthening capital hours simply cannot be estimated from available information, an amount larger than, say, 0.02 or 0.03 points in the 1950–62 growth rate seems improbable. I therefore classify the Jorgenson-Griliches utilization adjustment of 0.58 as resulting from differences in statistical procedure rather than in classification.

X. Some General Observations

JORGENSON and Griliches draw certain conclusions from their results that I believe to be unsupported and unsupportable.

To introduce this discussion, let me first recall that, in the framework of my estimates, output per unit of input in the private domestic economy may rise, or fall if changes are adverse, for any of a large number of reasons. Seven are perhaps worth listing. Having concluded that Jorgenson and Griliches do not have a broader classification of inputs than mine, I consider that all apply equally to their estimates.

1. Advances in technical, managerial, and organizational knowledge permit more output to be obtained with a given quantity of inputs. The gains may take the form of making possible production of more efficient capital goods at the same cost (resulting in "embodied" technological progress) or they may take any other form. Advances in knowledge, whether transmitted through improvements in capital goods or not, may result from expensive research at one extreme or from completely cost-free accidental discoveries at the other.

2. Knowledge may become more quickly or widely dispersed.

3. Expansion of markets may permit economies of scale.

4. The allocation of resources may move closer to the allocation that would maximize output. Allocation has a myriad of aspects ranging from the distribution of total resources among industries, products, and firms of different size to the placement of each individual worker in the particular job in which his contribution is greatest.

5. Obstacles deliberately imposed by governments, business, or labor unions against the most efficient utilization of resources in the use to which they are put may weaken.

6. The adequacy of government services (roads, police, courts, etc.) that affect private productivity may change.

7. The intensity of utilization of resources may change cyclically with variations in the pressure of demand [2, pp. 273–277, 441–442]. (I try to eliminate the effects in presenting "adjusted" growth rates of output per unit of input.)

My statistical estimates of output per unit of input may also rise or fall because my measures of input are incomplete (for example, I could not measure how hard people work) or inexact. In presenting my estimates, I have always tried to stress the limitations of information and technique, and the fact that one cannot proceed with growth analysis without introducing some assumptions. He can only try to adopt assumptions that are as realistic as he can make them. In this article, I have considered only differences between the Jorgenson-Griliches techniques, data, and assumptions and my own. I have not considered the limitations of techniques and assumptions that we share.

Interpretation of Jorgenson-Griliches results

Jorgenson and Griliches introduce their article by stating that its purpose is to test the hypothesis that "if real product and real factor input are accurately accounted for, the observed growth in total factor productivity is negligible." [1, p. 249] Their small estimate of the rise in total output per unit of input leads them to "conclude that our hypothesis is consistent with the facts." From this conclusion, they draw sweeping inferences. My conclusion is that they obtain their strikingly low estimate of productivity growth not by eliminating errors made in other research but by introducing new errors of their own. If so, the inferences they draw from this finding are also wrong.

I have stressed that the determinants of changes in output per unit of input are the same for the Jorgenson-Griliches series as for mine.[64] I am unable to find anything in their procedures that would have the effect of reclassifying a growth

64. Except that they also include changes in labor quality due to changes in age-sex composition.

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source that I consider to be a component of output per unit of input into a component of input except their wholly unwarranted capital utilization adjustment. Nevertheless, their theoretical discussion suggests that Jorgenson and Griliches would like to reclassify growth sources from productivity to input. Some readers of their article have supposed that they have actually done so; this is understandable because Jorgenson and Griliches are not very clear on this matter.

Their discussion [1, p. 260] of "vintages" of capital goods is likely to mislead the unwary reader. This discussion is concerned with the fact that the design of capital goods improves as time passes. For this reason, an investment of a given sum this year buys a bundle of capital goods that is more productive than the bundle that could have been purchased this year with the same sum of money if capital goods of designs known 10 or 20 years ago were now being produced and were the only types known and available.

Jorgenson and Griliches indicate that, to aggregate capital goods in the capital stock, they would like to treat capital goods of different vintages as different commodities and weight them by their marginal products at a common date, rather than weight them by their costs at a common date as is the general practice in existing capital stock series. This procedure would be equivalent to adjusting existing capital stock series to reflect "unmeasured" quality change; "unmeasured" quality change in the capital stock is defined as the difference in movement between a capital stock series constructed by weighting components by marginal products and a series in which costs are used as weights [2, pp. 134–135, 144–145]. The contribution of "unmeasured" quality change to growth is "embodied technical progress." Thus, the procedure Jorgenson and Griliches recommend would have the effect of transferring "embodied technical progress" from the productivity to the input measure.[65]

It is difficult to read their article without supposing that they actually do make such a transfer.[66] But they stop short of making this claim explicit. In actual fact, I find nothing in their procedures that has the effect of adjusting capital input for the type of quality change that is not reflected in cost differences at a common date, and thus of "embodying" technical progress (nor am I aware of any statistical procedure that could be introduced to do this). I have taken pains to point out that neither their price substitutions nor their use of a fast depreciation (replacement) formula in measuring capital stock has any such effect.

It should also be noted that a distinction they introduce between costly and "costless" advances in "applied technology, managerial efficiency, and industrial organization" [1, p. 250] plays no role in their estimating procedure. They do not capitalize the costs or benefits of research and development, of reallocation of labor, or of any other action that would contribute to an increase in output per unit. Thus, they have transferred none of the gains from costly research or from other expenditures or costly actions out of their estimates of output per unit of input.

Given the characteristics of their productivity estimates that I have described, how is one to interpret the following passage, which appears after their empirical results are presented?

> "Our results suggest that the residual change in total factor productivity, which Denison attributes to Advance in knowledge, is small.[67] Our conclusion is not that advances in knowledge are negligible, but that the accumulation of knowledge is governed by the same economic laws as any other process of capital accumulation. Costs must be incurred if benefits are to be achieved. Although we have made no attempt to isolate the effects of expenditures on research and development from expenditures on other types of current inputs or investment goods, our results suggest that social rates of return to this type of investment are comparable to rates of return on other types of investment. Another implication of our results is that discrepancies between private and social returns to investment in physical capital may play a relatively minor role in explaining economic growth." [1, p. 274]

This quotation seems to contain four statements. Even if the Jorgenson-Griliches statistical results were accurate, they would not, I believe, support all of these statements. Indeed, the interpretation of their residual productivity estimate that is required for it to support the first statement seems directly contrary to the interpretation that would be required for it to lend any support to the other three statements.

The first statement is that the small Jorgenson-Griliches residual does not imply a small contribution to growth from advances in knowledge. This statement could be correct *only* if their procedures *have* the effect of reclassifying much of what I regard as the contribution of output per unit of input to an input contribution. In the absence of such a reclassification, a tiny figure for growth of output per unit of input *would* in fact leave little room for a contribution from advances in knowledge—or from economics of scale, reallocation of resources, or any of the

65. Jorgenson and Griliches would like to allow for "unmeasured quality change" of capital goods in computing the fixed investment components of GNP at constant prices as well as in constructing capital stock series. This would not affect the amount transferred from "GNP per unit of input" to input as "embodied technical progress," but by raising the growth rate of gross product, it would offset to some degree the reduction of the productivity series. However, three points should be noted. (1) The addition to growth of GNP per unit of input would tend to be much smaller, on the average, than the deduction because the ratio of gross fixed investment to GNP is much smaller than the fixed investment share of gross earnings, especially when the latter includes indirect taxes. [See 1, p. 262.] (2) In an analysis of *net* product growth, most of the addition to productivity (but not of the subtraction) would disappear because the increase in the growth rate of gross output in constant prices would be accompanied by a corresponding increase in the growth rate of depreciation in constant prices. (3) The relative size of the positive and negative adjustments to GNP per unit of input would change from time to time unless (a) the rate of "unmeasured quality improvement" were constant over a long period (from the installation date of the oldest capital in the stock when output is first measured to the last date that output is measured) and (b) changes in the share of fixed investment in output synchronized with changes in the share of fixed investment in earnings in some very special way.

66. Their footnote 1 on p. 254, does not contradict this. It merely states that they do not measure embodied technical progress in such a way as to make the change in output per unit of input zero by definition. Their footnote 1, p. 274, refers to errors in capital goods prices, which they try to correct, as "analogous to embodied technical change."

67. Footnote by Denison: Actually, I have attributed to advances in knowledge only part of my estimate of the contribution of output per unit of input.

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other sources I have listed as contributing to changes in output per unit of input.

The second statement is that, to obtain important advances in knowledge, commensurate costs must be incurred; costs must be incurred if benefits are to be achieved. This implies that a comparison of costs and gains has been made. Actually, Jorgenson and Griliches provide no estimates at all of the *costs* of obtaining knowledge—e.g., costs of research or exploration. The fact that their residual productivity estimate is small can indicate that *gains* from advances in knowledge—whether costly or costless—are small *only* if Jorgenson and Griliches *have not* transferred gains from advances in knowledge from productivity to input. I would regard as implausible a finding that advances in knowledge have contributed to growth an amount as small as their residual.[68] I have tried to show that their estimate actually results from procedural and statistical errors. But, although I have argued that Jorgenson and Griliches have made no *valid* transfers of growth sources from productivity to input, the actual reason their residual is so very small is their introduction of the capital utilization adjustment. If this adjustment were really accurate and appropriate, they would have counted gains (their estimate implies *most* of the gains) resulting from advances in knowledge as a contribution of capital. If they had succeeded in adjusting capital stock series for unmeasured quality change by their "vintage" approach, this too would have counted gains resulting from advances in knowledge as a contribution of capital.[69]

The third statement is that social rates of return on research and development are comparable to those on other types of investment. This statement, too, does not follow from their results. As just indicated, they provide neither measures of the costs of research and development for comparison with costs of tangible investment, nor measures of the benefits of research and development and of tangible investment.

As to their fourth point, I do not understand how their results could possibly show that discrepancies between private and social returns to investment in physical capital are small. Jorgenson and Griliches must somehow have drawn this inference from the size of their residual. But their introduction of a capital utilization adjustment renders use of their residual for inferences about social rates of return conceptually invalid, just as it does for inferences about returns to research. And even their small residual would be big enough to add greatly to the private rate of return on investment if (improbably) it arose entirely from the discrepancy between public and private returns to investment.

Part of the difficulty with the quotation I have just analyzed stems from the preference of Jorgenson and Griliches for what I regard as an inconvenient classification of growth sources, and this leads me to a final comment on this topic. I believe there is an advantage in matching growth sources with the reasons that income changes, and I have tried to adhere to this principle in my own work. In particular, confusion and misinterpretation are avoided if the contribution of capital is identified with changes in income that result from investment, and that can be altered by changing the amount of investment, and the contribution of advances in knowledge is identified with changes in income that result from advances in technical and managerial knowledge, and that can be altered by changing the state of knowledge. Confusion is hard to avoid if the consequences of advances in knowledge are classified as contributions of capital. This is why I believe it would be unwise, even if they could be isolated, to count as contributions of capital the gains made possible because someone has devised improved designs of capital goods, or found ways to make possible more continuous use of capital goods. Such a classification is an invitation to misinterpretation.

68. It may be noted that Jorgenson and Griliches have estimated that the increase in output per unit of input was negligible over the whole 1929–64 period as well as during the postwar period [5, p. 61]. They clearly believe this to be the typical situation.

69. If the superiority of later "vintages" of capital goods was that they could be used longer hours, the same gains would actually be transferred twice—once by the capital utilization adjustment, and once by the adjustment of the quality of capital.

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