Factor Shares in the Long Term: Some Theoretical and Statistical Aspects

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> All generous minds have a horror of what are commonly called "facts." They are the brute beasts of the intellectual domain. Who does not know fellows that always have an ill-conditioned fact or two that they lead after them into decent company like so many bull-dogs, ready to let them slip at every ingenious suggestion, or convenient generalization or pleasant fancy?—HOLMES, *The Autocrat of the Breakfast Table*

RATIONAL regularities pervade economic theory. But how rarely are they revealed in empirical records. And how resigned economists have become to it all, knowing full well that any constancy —whether of theory or the real world—will be hidden by the coarse irregularities of the published statistics. So well have they learned this depressing lesson, however, that they are quite unprepared on the rare occasions when a rigid constancy is reported. The share of wages in the national income seems to be such a constant, and a particularly dubious one—for it appears not only to lack a basis in theory, but even to be in conflict with it. Not surprisingly some of the most distinguished economists have been bemused by this. Keynes, with Anglo-Saxon understatement, spoke of the constancy as "a bit of a miracle." Schumpeter decided that it was "a mystery"—as did Joan Robinson. Solow has found that the miracle "may be an optical illusion," but still ranks it as "an interesting problem."[1] And Reder's recent review of the problem concludes that we are still in the dark.[2]

We consider here both some theoretical and statistical aspects

NOTE: I am deeply indebted to Bert Hickman for raising more penetrating questions on an earlier draft than are answered in this one.

[1] J. M. Keynes, "Relative Movements of Real Wages and Output," *Economic Journal*, March 1939, p. 48. J. A. Schumpeter, *Business Cycles; a Theoretical, Historical, and Statistical Analysis of the Capitalist Process*, Vol. II, 1939, pp. 575–76. R. M. Solow, "A Skeptical Note on the Constancy of Relative Shares," *American Economic Review*, September 1958, pp. 618, 628. The longer term stability in manufacturing was indicated by data for 1914–26 in an odd little study by Jurgen Kuczynski and Margaret Steinfeld, *Wages and Labor's Share*, 1927, pp. 54–55, but no analysis was really made of the results.

[2] Melvin W. Reder, in a perceptive (unpublished) paper read at the 1960 Annual Meeting of the American Economic Association.

of the question explicitly raised by Keynes: why, over so long a period in which the relative amounts of labor and capital changed so drastically, did the share of labor and capital in the national income remain relatively so stable? In Part I we discuss the forces determining factor shares, and conclude that "relative constancy" should exist, given the market mechanisms that determine the shape of the underlying production functions.

In Part II we consider how apt have been the previous statistical series used for studying changes in the U.S. ratio of labor income to national income from 1850 or 1900 to date. We find that these data, while suitable for a great many other purposes, were originally estimated with so many constancies stipulated in the estimation procedure that we can safely conclude nothing from them about the constancy of labor's share.

In Part III we consider the difficulty of the entrepreneurial share—compounded as it is of earnings on capital and earnings by labor. We conclude that the conceptual problem of disentangling capital from labor returns to entrepreneurs makes useless a discussion of labor's share in total national income—as well as in any industry dominated by entrepreneurial activity, such as agriculture, construction, and trade and service.

PART I

The Medusa-like fascination of the constant share of labor income to national income springs from an apparently glaring contrast between the patterns of neoclassical distribution theory and the brute statistics on distributive shares.[3] Theory begins from the reasonable stipulation that the respective returns to capital and labor are fixed by the relative prices and quantities of each. But since the past century has witnessed phenomenal changes in "the techniques of production, in the accumulation of capital relative to

[3] We do not consider interests beyond distribution theory. A number of authors have, however, reviewed the data with the thought that such numbers cast light on "the significance of union power . . . economic development, egalitarian movements." (The quotation is from an admirable review by Paul Davidson, *Theories of Aggregate Income Distribution*, 1960, p. 1. See too, Allan Cartter, *Theory of Wages and Employment*, 1959, Ch. 11.) Simon Kuznets has some concise and conclusive remarks on the limitations of the data for any such discussions. See his "Quantitative Aspects of the Economic Growth of Nations," *Economic Development and Cultural Change*, April 1959, p. 56.

labor and in real income per head,"[4] should we not expect marked changes in the ratio of labor to capital returns? Most writers at this point have gone on to contrast the statistics with this apparent implication of theory. Solow, however, has properly raised the question: can one skip so simply from one to the other? What a gulf separates the individual firm as described in neoclassical theory from the compiled numbers on factor shares for the economy. Between the two, writes Solow, lie "a whole string of intermediate variables: elasticities of substitution, commodity demand and factor supply conditions, markets of different degrees of competitiveness and monopoly, far from neutral taxes" and so on.[5] Without necessarily agreeing that a look at these complicating forces leads to "an expectation of 'relative stability' if anything,"[6] we take his perceptive caution to mark precisely the point for initial inquiry—well before the statistics themselves are reviewed.

Let us begin with a definition: The ratio of wages to property income (R) is a function of the price and quantity ratios for labor services (w) and capital service (k):

$$R = f\left(\frac{P_w}{P_k}, \frac{Q_w}{Q_k}\right) \tag{1}$$

A constancy in the wage ratio must follow inexorably if every movement in the price ratio is neatly offset by a contrary change in the quantity ratio. Solow has demonstrated that such an offset is virtually assured if only we assume a reasonable figure of one-third for the elasticity of substitution.[7] Kravis finds, similarly, that a fairly constant share did appear for the U.S. because "under conditions of rapid expansion in production labor was relatively

[4] N. Kaldor, "Alternative Theories of Distribution," *Review of Economic Studies*, February 1956, p. 84. The recent discussion of excessive stability appears to stem from Keynes' 1939 article. In an earlier look at the same U.K. figures from Bowley—though not the U.S. figures—Hicks (*Theory of Wages*, 1948, pp. 130–133) concluded that the capital share fell significantly from 1880 to 1913 (34 to 31 per cent) because the elasticity of substitution fell. His frame of reference was the rise in the share from medieval times.

[5] Solow, "A Skeptical Note," p. 620. Martin Bronfenbrenner, in "A Contribution to the Aggregate Theory of Wages," *Journal of Political Economy* (December 1956) systematically reviews the most important of these factors in his study of the question of whether in recent American history wage rate increases have added to real demand.

[6] Solow, "A Skeptical Note," pp. 620–621.

[7] *Ibid.*, p. 629. See also Irving B. Kravis, "Relative Income Shares in Fact and Theory," in *American Economic Review*, December 1959, p. 940.

inelastic in supply and rising rapidly in price" while "capital was apparently much more elastic or at any rate rapidly growing in supply."[8] But we only push the question back a stage, if we thus demonstrate that a palatable figure for the elasticity of substitution will yield numbers within the bounds of our historical measures of labor's share.

Why this particular substitution rate? The elasticity of substitution that may be hypothesized—whether one-third, one-fourth or any other figure—is surely not a mysterious new constant, given by forces outside the market economy. Why this proportion, and why this elasticity, rather than any other?[9] Should we not hesitate to rely on a particular number as an explanation?

Kaldor premises stability as a result of a constant saving—output ratio, his corollary being that changes in the propensity to save out of profits compensate for changes in the propensity to save out of wages.[10] This question has been examined carefully by Melvin Reder who concludes that historical data are consistent with this theory—but likewise with quite different ones.[11] In a recent telling essay, Kaldor stipulates that "a capitalist economy, after a certain period of adaptation, will tend to settle down to a rate of economic growth and accumulation where the growth rate of capital is the same as the growth rate of output, since at this point the . . . rate of profit on capital will be neither rising nor falling. The historical constancy of the capital output ratio, of the share of profit in income . . . in advanced capitalist economies is thus explicable in terms of forces which tend to bring these two growth rates (of capital and output) into equality with

[8] *Ibid.*, pp. 943–944. Sidney Weintraub (*An Approach to the Theory of Income Distribution*, 1958, p. 82) finds that "either the *M/A* ratios (marginal to average physical product) and the *Ed* magnitudes (price elasticity of demand for specific products) must have remained constant or they must have operated systematically and fortuitously to neutralize each other's variation when the stock of equipment, the level of employment, and the nature of the product-mix underwent change."

[9] Premising a constant elasticity, in turn, implies that the historical curve relating the quantity to the price ratio must have been a straight line. But it is difficult to see an a priori reason why the line should be straight throughout time, or why a curvilinear relationship would not be equally reasonable.

[10] N. Kaldor, "Alternative Theories," p. 84. Allan Cartter (*Theory of Wages*, Ch. 11) presents a theory similar to Kaldor's in emphasizing the marginal propensities to save of laborers and capitalists, but does not premise stability of each, nor compensating movements.

[11] Melvin W. Reder, "Alternative Theories of Labor's Share," *The Allocation of Economic Resources*, 1959, Moses Abramovitz, ed.

one another." He goes on to state that "wages and profits form a constant proportion of output" in "any steadily growing economy where the proportion of output devoted to investment is constant" provided "the propensities to save out of profits and wages are assumed to be given."[12] But why these propensities need be stable through time is another matter, and requires some further demonstration. Indeed in a subsequent discussion of laggard growth he points out that "if the savings propensities were halved, the share of profit in income would be doubled at any given ratio of investment to output," and then goes on to discuss how "the process of accumulation and growth is periodically interrupted."[13] This surely implies that the saving propensities are not necessarily fixed.

We believe it possible to seek a solution in the theory of distribution; and suggest that a market mechanism does exist which works in the direction of long-run constancy—quite apart from any specific ratio of wages to national income, or elasticity of substitution, that may seem most reasonable to us. Agreeing with the emphasis placed on the elasticity of substitution by Solow and Kravis, we go on to consider the forces that determine the level of that elasticity over long periods, constant or not. Let us consider the quantity ratio in equation (1) above. What determines the size of this ratio? The answer, for a broadly competitive economy, is that the ratio is a function of two variables—the price ratio in the present period, and the quantity ratio in the previous period:

$$\frac{Q_w}{Q_k} = f\left(\frac{P_w}{P_k}, \frac{Q_{w_o}}{Q_{k_o}}\right) \tag{2}$$

That the price ratio in the current period is one forceful determinant we can hardly doubt: where entrepreneurs have free access to capital and labor markets we should expect shifts in the ratio of prices to induce shifts in the quantity of each used.

However, only limited substitution possibilities can be grasped within the short term. The production coefficients are therefore destined to appear invariant to many price changes. It is for this reason that we include the quantity ratios of the prior period as a

[12] N. Kaldor, "Economic Growth and the Problem of Inflation," *Economica,* August 1959, pp. 223, 225.

[13] N. Kaldor, "Economic Growth and the Problem of Inflation, Part II," *Economica,* November 1959, p. 290.

second variable—for they reflect the coefficients in being. (Thus, once a bank of machines has been installed, as Johansen has recently emphasized, it will require a fixed complement of manpower throughout the life of the machines.[14] While there are some qualifications to this generalization, they do not warrant ignoring this variable.[15])

But the fixed-complement technology itself is not really a given: for the ratio of manpower to machine inputs at time zero is in turn a function of relative prices in the previous period(s), when the machine-man ratio was adopted from a spectrum of alternatives. Substituting this earlier price ratio for the quantity ratio in the second equation gives us:

$$\frac{Q_w}{Q_k} = f\left(\frac{P_w}{P_k}, \frac{P_{w_o}}{P_{k_o}}\right) \tag{3}$$

Substituting (3) in (1) we get:

$$R = f\left(\frac{P_w}{P_k}, \frac{P_{w_o}}{P_{k_o}}\right) \tag{4}$$

The ratio of wage to property income is therefore a function of the price ratio of labor to capital in the current and preceding period(s).[16]

If this sequence of price ratios could vary randomly we would have precious little reason to anticipate any great stability of the wage-property income proportion. And if the ratios systematically rose or fell, we would expect that proportion to move remorselessly up or down. Only if basic forces made for a stability in the price ratios would we anticipate a stability in the income

[14] Lief Johansen, "Substitution versus Fixed Production Coefficients in the Theory of Economic Growth: a Synthesis," *Econometrica*, April 1959, p. 158.

[15] Should there occur a marked variation in the ratio of the price of labor to that of capital, marginal equipment can be sold in the market and new equipment bought—and bought until a point is reached where the technical ratio has actually changed. Over a longer period technical substitution is, of course, still more likely to take place as old equipment is fully written off.

[16] Since the entrepreneurial choice is really made with respect to price trends over the useful life of the capital investment under consideration, it is the expected price ratio that is relevant. We assume that to be some function of the past and present ratios. This is probably insufficient. An allowance should also be made for the change in these ratios: entrepreneurs would recognize the forces in this country making for a long-run rise in wage rates, the long-run accumulation of capital, and the decline in the risk component of capital cost. Such an allowance would lead to a bias in favor of a higher capital-labor ratio. However, this bias, if steadily exercised, would tend to bring back the relative price of capital.

ratio. At first sight we have no reason to expect such stability. (Indeed, if we attached the same importance to monopolistic elements as Kalecki and Mitra did when explaining the wage proportion, we would premise so solid a stability in the price trend for one factor as could only result in greater instability in the ratio of that price series to the other.[17])

True, if we assumed a fixed elasticity of substitution all would be magically simplified. For Bronfenbrenner's analysis demonstrates that any one in a wide choice of elasticities would all tend to give us factor shares that varied within only a narrow range.[18] But what warrant do we have for creating any such *numerus ex machina*, a new constant—unwavering and unyielding amid all the forces of economic change? Furthermore, a look at the Creamer-Kendrick data for manufacturing suggests that in this major sector the capital-labor ratio went in one direction from 1899 to 1919, and in the other from 1919 to 1953. Meanwhile (according to our estimates), factor shares remained almost unchanged. Hence, the elasticity of substitution must have changed significantly through time. It is, therefore, not by any arbitrary positing of a fixed elasticity that we can achieve a satisfying explanation of the relative constancy in the sequence of price ratios discussed above.

What can be said about the determinants of the price ratio? We simplify our problem by taking the price of labor as given in all periods. (Its determinants do not matter here: they are fixed as part of a general equilibrium solution. We need only deal with changes in the price of capital vis-à-vis that of labor.) What, then, fixes the price of capital used in production relative to those forces that determine the price of labor?

DEMAND FOR CAPITAL

Consider a manufacturing company: its demand for capital reflects the uses to which the capital will be put—investment in machinery, inventories, land, etc. Now while machinery, for example, is often treated as technically complementary to labor in the production of most goods (on terms set by the engineers) this basically

[17] Michal Kalecki, *Theory of Economic Dynamics*, 1954. Ashok Mitra, *The Share of Wages in National Income*, The Hague, 1954. Keynes, in "Relative Movements," speculated on the role of monopolistic elements, but did not accept them as a real explanatory force.

[18] Bronfenbrenner, "A Contribution to the Aggregate Theory."

reflects the underlying price relationship.[19] Were labor to become a free good while machinery continued to command a price, then over time one would expect a marked increase in the ratio of labor to machinery as capital saving methods were introduced. "Technical" requirements might still forbid complete substitution; sociological ones would certainly do so. But, within the limits set by these constraints, there would still be ample room for wide variation determined by the relative profitability of using one factor instead of another. But that profitability would be set by the changing price ratio of one to the other—assuming an initial long-run competitive equilibrium position in which the marginal revenues from each were equal.

SUPPLY OF CAPITAL

The supply forces that work to fix the cost of capital include the interest rate and the price of a physical unit of capital. Variations in the former appear to have had small impact on the long-period share of return to capital in the national income—small not in terms of economic importance, but as compared to the much greater effects of variation in the prices of machinery and plant. Over the past sixty years producers' durable equipment has risen about 300 per cent in price, while ten-year bond yields have changed about 10 per cent, from 3.2 to 2.9 per cent.[20] Moreover, the ratio of interest to total costs of machinery service is relatively small over the life of most equipment. Thus, while variations of high economic significance have occurred in interest rates, their contribution to changing the numbers on factor shares has been feeble in comparison with the effect of variations in the prices of machinery and other capital items.

Let us look to the determinants of the price of these services.[21]

[19] Variations in the demand for capital as a purely technical complement to labor will not tend to change the relative price of one to the other and hence can be ignored here. To simplify discussion we convert the problem to a two-factor one, treating land costs henceforth as commutable into machinery and construction costs. We label the latter as machinery for convenience.

[20] Kuznets' implicit price index for producers' durables just about doubles from 1897–1901 to 1927–31, and the Department of Commerce series more than double from 1929 to 1957. See Bureau of the Census, *Historical Statistics of the United States, Colonial Times to 1957*, pp. 142–144. Bond yield data from *ibid.*, p. 657.

[21] We consider first the unit price of equipment assuming no change in its productive capacity. But in the real world, of course, the manufacturer may cheapen machinery service either by cutting price, raising productive capacity or a combination of the two.

The supply forces which fix the price of machinery are those involved in: the cost of labor; the cost of materials; "normal profits"; and rents, advertising and other costs.

In 1957 the machinery industries sold something like $26 billion worth of goods,[22] of which: $10.0 billion was used for wages and salaries; $8.0 billion was used for materials purchases; $0.3 billion was used for rent and interest; $1.1 billion was used for net profit (after tax); and $1.7 billion was used for taxes.

We assume that variations in rent, taxes and corporate dividend policy are numerically unimportant in determining price variation, given their small share in total cost, a generally competitive industry, and their limited dollar range. Hence, it is the fluctuating level of wage and of material costs that dominates the change of final product charges by machinery industries.

Material (and component) costs are determined on the demand side first by variations in total production; but these, involving technical complementaries, do not alter the capital-labor price ratio.[23] Secondly, these costs are determined by the buyer's option of producing in a different fashion, using a different level of fabrication as a partial substitute for raw materials or components. The use of the latter option, of course, is influenced by the supply schedules for the industries that provide materials to the components and materials industries *per se*, and by the wages in the latter industries. On the supply side, these will reflect, to an important extent, wage costs in steel, hand tools, coal, railroads, and other industries that directly or ultimately provide materials to the machinery industries.

[22] For machinery except transport and electrical, data on sales, rent, interest, profit, and taxes from IRS, *Statistics of Income, Corporation Income Tax Returns, 1957–58*, p. 27. For compensation of employees we use *U.S. Income and Output*, p. 200. For materials, we take the IRS total for cost of goods sold, deduct wages, and round the figures. The major problem in this simple procedure is a possible significant change in inventory holdings. From IRS, *Business Statistics*, 1959, p. 20, we see that inventory change over the taxable year was in fact quite small, assuming most tax years to end June through January.

[23] This statement is something of an exaggeration: at markedly different levels of output there could be economies of scale greater for one factor than another. (Eric Schiff's penetrating discussion in his paper "Factor Substitution and the Composition of Input" in *Output, Input, and Productivity Measurement*, Studies in Income and Wealth 25, Princeton for NBER, 1961, emphasized the scale effect and pointed out that production processes are not necessarily so "input homogeneous" as to permit easy substitution throughout the scale range.) We would expect that, in the long run, changes in size of production run (and possible plant) consequent upon scale economies would be explicable for present purposes by the mechanism discussed below.

Hence, it is wage costs in the machinery industries, and in the suppliers to these industries, that substantially shape the supply price of machinery.[24]

If changes in wage costs to the machinery industries bore no necessary relationship to changes in other industries this would not get us forward; but in fact they do. Wage rates for unskilled labor in the machinery industry, for example, must broadly move together with those for unskilled labor in steel. If machinery industries begin paying more, then the steel industry and others must match the rate change for the same quality of labor, or begin to lose labor.[25] The same is true for cranemen, machinists, carpenters, truck drivers, etc. And there must be a similar correspondence even for jobs that seem peculiar to one industry: were machinery industries to double the rate paid on a simple assembly operation characteristic of these industries, the steel industry and others must begin to raise their rates for broadly similar skills or see their expert semiskilled people begin moving out to jobs in machinery (and vice versa if steel or coal moves first). In a competitive labor market over the long term, therefore, we expect to see wage rates for major occupations change similarly in machinery industries and in their supplying industries.[26]

There remains one step. If productivity advances in the machinery industries had been negatively correlated with those in the supplying industries, the broad correspondence of wage *rate* trends might have been so nullified as to make each wage *cost* series take a different path. We see no reason to assume so unlikely a negative relationship. Because of the endless problems of measuring deflated output in the machinery industries, we have little empirical

[24] I am indebted to Bert Hickman for calling my attention to the similar point made by Robert Grosse in his valuable "The Structure of Capital" in *Studies in the Structure of the American Economy*, edited by W. W. Leontief, 1953, p. 186. Grosse finds that "substitution occurs chiefly when there are technical improvements in capital goods production which result in a fall in the ratio of the price of capital goods to the price of labor. But with a given technique, there will be relatively little price substitution." Since the choice of technique itself is determined by relative prices and returns there is no need for the latter assertion.

[25] We are speaking here of correspondence of movement, not identity of level.

[26] We assume that the role of union and government intervention, however great, does not require significant modification of this statement. In a recent review of the considerable literature on the role of the union, and an extended analysis of his own, Gregg Lewis concluded that the absolute influence of unions was relatively small. See H. Gregg Lewis, "The Effects of Unions on Industrial Wage Differentials," in *Aspects of Labor Economics*, Princeton for NBER, 1962. A fortiori, the differential effect as between industries using the same type of labor is likely to be still less.

material. But if we may take such other metal manipulating industries as transport equipment to give us a suggestion and relevant indication, we find that their productivity advanced over the decades in the same direction as that for steel, coal mining, lumber manufacturing, etc.[27]

Surely the thrust of productivity advance was also at work in the machinery industry. If so, one would expect the price of machinery services to fall in relation to wage rates.[28] This result, however, would depend on the source from which this productivity advance derived. If it involved the use of machinery, then the machine-producing industry must needs be defined as, in this respect, a machine-using industry. In that event, however, we are promptly returned to the original question on the forces that make for the substitution between capital and labor.

But suppose the productivity advance in machinery occurred in the industry *qua* producer. It would then have had to result from the other modes by which the entrepreneurial function is exercised. For example, highly skilled tasks would be broken down to use less skilled labor. Noncompeting groups in the labor market (immigrants, women, nonwhites) would be hired in increasing measure to replace higher cost groups. Maintenance, stand-by, finishing, clerical operations would be cut out or cut down. Each of these steps would cut wage *costs* without touching wage *rate* trends. New plants would be established in states with lower labor costs, and in areas closer to the optimum location between new market concentrations and factor sources. New control systems and revised layouts would make more efficient use of existing stocks and input flow patterns. In general, this range of procedures could induce productivity advances, and in significant volume, without proportionate increases of capital or labor inputs. Many would require no increase of inputs.[29]

[27] Solomon Fabricant, *Employment in Manufacturing, 1899–1939: An Analysis of its Relation to the Volume of Production*, New York, NBER, 1942. Harold Barger and Sam H. Schurr, *The Mining Industries, 1899–1939: A Study of Output, Employment and Productivity*, New York, NBER, 1944.

[28] We consider here factors making for a decline in the price of machinery services. Producers may pass on the decrease in their costs either by reducing machinery prices, by improving the output capacity of machinery while keeping prices rigid, or by a combination of the two. Hence, reported machinery price series without an adequate allowance for changes in machinery productivity do not measure changes in the price of machinery services.

[29] Summarizing data in John W. Kendrick, *Productivity Trends in the United States*, Princeton for NBER, 1961, Fabricant estimates that the physical output of

But each of these entrepreneurial gambits is available to machine-using as well as to machine-producing industries. And at any time they have an equal incentive to reduce costs by adopting such alternatives. It is, of course, unnecessary and unreasonable to assume that more efficient techniques would be seized upon at precisely equal rates in machine-using and machine-producing industries. We do assume, however, that (apart from input increases) the long term forces making for productivity advance in the machinery industries, and thereby making for lower machinery service prices, will find their parallel in similar forces that lower labor costs in the machine-using industries. Hence, the obvious fact of productivity advance in the machinery industries does not *per se* imply any changing ratio of capital to labor price—that ratio whose approximate long-term stability we have inferred above as confronting the machine-using industries.

A second component of capital costs, beyond that for machinery and equipment, is for buildings. By the same line of reasoning as that used above, the bulk of building service price variation will be determined by wage rate variation in the building industry and those industries (cement, steel, mechanical items) that supply the construction industry—with a lesser contribution from variations in profit margins and productivity differentials.

A third component of capital costs is that for financing inventories. The price of inventories will vary with the price of the final product which, in turn, is largely dominated by the course of wage costs in the given industry and those preceding it in the production sequence.

Finally, the decision to hold funds in working capital—whether currency, deposits, securities, or receivables—must be made in terms of the opportunity cost of using such funds for actual investment in inventories or productive equipment, and hence be relatable to the same factors as determine the cost of such investment.

A significant qualification to the above would appear to be the fact that contracts are made at one point in time for the acquisi-

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the economy rose by 3.5 per cent a year from 1889 to 1957, whereas man-hours and tangible capital inputs jointly rose by only 1.7 per cent (Solomon Fabricant, *Basic Facts on Productivity Change*, New York, NBER, 1959, p. 19). From the numbers we infer a significant contribution by forces other than these tangible inputs—among which we give pride of place to the entrepreneurial function.

tion of capital, while their terms are constant for years afterwards despite subsequent variations in the wage cost figures. In one respect this argues for a distributed lag function, recognizing not merely current trends in wages but previous trends—with a diminishing distribution of weights through time. But a substantial link to current trends is still maintained for most contracts by virtue of the fact that the contracts can be broken by refinancing. The opportunity cost involved in continuing old financing is largely set by the cost of breaking old contracts and the cost of borrowing under current terms. Because of the costs and difficulties of refinancing, there will be no instant and proportionate response in contractual changes as current changes in investment opportunity occur. And the increasing reliance on internal financing for capital expansion and replacement, rather than resort to borrowing or the use of senior equity securities, tends to diminish further the scope for such response. However such response as does occur through refinancing has a clear bias: since it occurs at the option of the borrower, it will take place only when current costs of new capital are below those for which he has contracted. Old borrowings bring a bias to the capital-labor price ratio since old contracts are broken at the option of the borrower (i.e., via paying off and refinancing), and that option is exercised more when the price of capital is declining relative to that of labor than when the ratio is going the other way.

It must be noted that not all data on the comparative trend of the price of capital and labor in the U.S. would be suited to the question as we have posed it. The required measure of the price of capital is one that has meaning only for those industries in which the entrepreneur can, in principle, substitute between his capital and labor inputs. Hence, the valuable Kendrick series, which includes imputed rent of owner-occupied houses and an entreprenurial allocation of a kind not suited to our immediate concern, is unfortunately not usable here.[30]

[30] His series includes the imputed rent of owner-occupied residences in the capital return. Variations in the "price" of such capital, which will reflect variations in property taxation and building maintenance costs, will not cause owner occupiers to change the proportions of labor to capital in their occupancy activity. No more suitable, are series where no market measure of the capital input to the sector existed—as, for example, those computed by deducting an arbitrarily estimated "labor compensation" of proprietors from their total return. (The residual estimate of capital compensation to proprietors is then divided by a capital input series to get a price of capital series.) Such a pro-

The above model is intended to describe only longer-term changes in relative prices and factor shares for individual industries, or more accurately for those constellations of products in which sets of firms tend to specialize and, by virtue of that specialization, tend to be termed industries. The shifting of the boundaries for these product groups over time surely tends to blur the boundaries of what is defined as an industry. But even if individual industries continue clearly delimitable through time, the combination of these industries into a grand total for the economy is another matter. The changing weight of products and "industries" in that total over the years involves a significant aggregation problem. In theory we would expect a nation to shift toward those industries that use its most abundant resources and away from those using scarcer resources; i.e., to change a specialization whenever what was once a high cost resource becomes a low cost one, and vice versa. The unsettled controversy over the Leontief paradox suggests, however, that empirical verification in this area is still significantly incomplete.[31]

In summary, we define the share of national income flowing to wages as against capital as a function of the quantity and price ratios of each factor. We find that in the long run the quantity ratio is in turn a function of the changing price ratios. Taking the price of labor service as given, we contend that the changing price of capital service must bear a constant long-term proportionality to that of labor. This proportionality derives from the fact that the supply forces working to fix the price of capital are dominantly wage costs in the capital-producing industries and those that supply them. In a competitive market these wage costs parallel wage-cost changes in capital-using industries because wage changes for identical occupations must bear a parity to one another in all employing industries; while historical experience suggests that productivity trends in the supplying and using industries are not so negatively related as to make costs take a different course from rates.

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cedure does not deal with measures separately available to the proprietor, as would be necessary if his substitution between capital and labor inputs were a function of price ratios.

[31] We refer to Leontief's conclusion that the U.S. tends to export goods using relatively greater amounts of labor, to import those using relatively greater amounts of capital, and to the extensive literature by Diab, Swerling, Hoffmeyer, *et al.*, on this proposition.

PART II

What basis do we have for asserting that the share of wages in the national income prior to 1919 was in fact stable?[32] The answer, in brief, is: very little. Most of the studies in the field rely on the estimates of R. F. Martin, W. I. King and Gale Johnson (Martin relies largely on King for the period 1909–19).[33] We consider each before turning to the data since 1919.

KING'S ESTIMATES FOR 1850–1900[34]

To assess the adequacy of King's data for the study of factor share changes prior to 1900, we review some of the ratios and averages implicit in his figures. The result of that review is not one to encourage our use of his totals for reaching any conclusion as to the stability of factor shares. We infer this from a consideration of data for three sectors—agriculture, government, and commerce—which together account for well over half of his income totals in these decades.[35]

Agriculture

King's figures show an unreasonably high share of farm product going to wages for 1870 and 1880 (his data imply almost 50 per cent of product in wages), then decline to nearly 20 per cent by

[32] We do not discuss the variety of data available for other countries: to understand the derivation of their estimates is a major project in itself. The interested reader will find a variety of comparisons for several countries, with data adjusted as best may be for comparability, in Livio Livi, *Primo Computo Del Reddito Distributto al Fattori Della Produzione*, 1958, Ch. 11, especially p. 172. Professor Livi's data show great stability for some nations, great change for others, over the period from 1938 to about 1954.

[33] R. F. Martin, *National Income in the United States, 1799–1938*, 1939; W. I. King, *The Wealth and Income of the People of the United States*, 1915; and D. Gale Johnson, "The Functional Distribution of Income in the United States, 1850–1952," in *Review of Economics and Statistics*, May 1954. A well known extensive study is E. C. Budd, "Factor Shares, 1850–1910," in *Trends in the American Economy in the Nineteenth Century*, Income and Wealth 24, Princeton for NBER, 1960. Among the significant briefer studies is the discussion in William Fellner, *Trends and Cycles in Economic Activity*, 1956, Appendix to Part 3, Parts 7 and 5; Simon Kuznets, *National Income: A Summary of Findings*, New York, NBER, 1946, p. 50, and Kuznets' review, more in accord with the conclusion of stability, in his "Long Term Changes in the National Income of the United States of America Since 1870" in *Income and Wealth of the United States* (International Association for Research in Income and Wealth, 1952, p. 85).

[34] I am most grateful to Elizabeth Jenks for comments on my interpretation of King's procedures, and for unpublished data from his "data books."

[35] King, *Wealth and Income*, p. 140.

1910.[36] The latter ratio is reasonably close to the Department of Agriculture estimates for that year,[37] but the ratios in the early decades are startlingly high. The enormous 1870–1910 decline is difficult to credit for a period during which the ratio of hired labor to entrepreneurs actually rose.

A second, no less disturbing aspect of King's figures is their implication that earnings per farmer (including imputed value of food and home) were less than those per farm employee.[38] Contemporary materials, as well as most data for later years, suggests quite the reverse.

Commercial and Professional Services

King estimated the total product of trade, service, finance, insurance and real estate—accounting for 30 per cent of the national product in 1910—"on the basis of a constant ratio to the product of urban population and average income."[39] What is important for present purpose is not that this large sector was estimated in this quite arbitrary, if reasonable, fashion. Rather our concern is that the wage component is likely to have been estimated with equal (or greater) arbitrariness: King gives no information from which his procedure could be deduced. Hence, we have little basis for using the trend in the factor-share ratio for total national income (including as it does a large allowance for wages) to deduce yeasty conclusions about the trend in factor shares.[40]

[36] *Ibid.*, pp. 138, 260.

[37] U.S. Department of Agriculture, *Major Statistical Series of the U.S. Department of Agriculture*, Vol. 3, p. 46, 1957.

[38] King, *Wealth and Income*, p. 150, refers to the Census of 1900, *Occupations* (p. 1), as his source for persons employed in agriculture. Referring to that volume we see, e.g., that his six million for 1870 is identical with the reported Census total for farmers and employees in agricultural pursuits. We therefore divide the Census 3.0 million farmers into King's entrepreneurial earnings (p. 263); then divide the residual 2.9 millions (for employees) into King's wage total for farming employees (p. 260). The result is about $240 per employee, $190 per entrepreneur. If, moreover, we assume that no money wage should have been set down for unpaid family workers, this $50 gap—in the wrong direction—would have been still greater.

[39] King, *Wealth and Income*, p. 138.

[40] King reports money earnings per employee for all industries combined (*ibid.*, p. 168). If we divide that average into his wage aggregates for the trade, service, finance group (p. 260), the result shows no rise in the number of employees in this group from 1850 to 1910. In other words, his implicit trend for earnings per employee in this category would have had to decline substantially over the decades, or to have risen significantly less than that for all employees, to give a reasonable employment trend for the group.

Government

King's description of how he estimated the product total for this group reads in its entirety, "The services of the government were assumed to be worth the amount paid for running the government."[41] We follow this thin lead by comparing, e.g., his $437 million for government product in 1870 with the $588 million total for federal expenditures plus state and local taxes.[42] Hence, a state and local surplus carry-over of about $150 millions—more than half again as great as the $280 millions of taxes they raised—would have had to exist if King's totals were to be reconciled with these expenditure figures.

No information is given on how the wage component of this government was estimated. If we refer to the same population census source that King cites as the basis for his employment estimates, add up the occupations shown there as associated with government employment, and divide the total into his wage bill, we get an average of $5,000 per employee in 1870.[43] This figure is over twelve times the reasonable average ($397) that he estimates for all nonfarm employees.[44] But since most government employees prior to 1900 were wage earners and lower salaried personnel, an average of at most $500 would be more like the true figure than $5,000.

MARTIN'S ESTIMATES, 1900–20[45]

Entrepreneurial income plus salaries and wages account for nearly 90 per cent of Martin's income totals in these years.[46] Hence, to assess the constancy of factor share ratios from these data must largely require that his estimates for the two shares have been independent of one another.

[41] *Ibid.*, p. 129.

[42] State and local taxes from a Census source cited in some of King's notes: 1870 Census, Part III, *The Wealth and Industry of the People of the United States*, p. 11. Federal expenditures are from Bureau of the Census, *Historical Statistics*, p. 710.

[43] 1900 Census, *Occupations* (p. 1) checked against 1870 Census, *The Statistics of the Population of the United States*, 1872, pp. 764–765. We add: employees of government, clerks, officials of government, army officers and soldiers. (Excluding the military would make matters worse.) The government employee average in 1900 was much the same as that for all employees.

[44] King, *Wealth and Income*, p. 168.

[45] Martin, *National Income*.

[46] *Ibid.*, p. 22.

Mining

Since Martin relies largely on King, the latter's lack of notes means that one cannot assert too flatly how the Martin estimates were derived. We believe, however, that the King-Martin procedure for these decades amounts to assuming that the trend of average wage per employee was identical with that for average earnings per entrepreneur.[47] Hence, no matter how great the actual variations in the relative earnings of each, these estimates would not show any change in the ratio of entrepreneurial earnings to wages and salaries.

Manufacturing and Trade

Martin assumed that the trend of earnings per employee and per entrepreneur were the same for 1899–1919. Indeed, for manufacturing, he used the same figures.[48]

Construction, Transportation, Service

For each of these industries Martin used the trend in the wage-salary bill to extrapolate that of entrepreneurial earnings, implicitly assuming (1) a constant ratio of average wage-salary to average entrepreneurial earnings, and (2) a constant ratio of employees to entrepreneurs.[49]

[47] Martin, *ibid.*, p. 119, adopts King's average entrepreneurial income for the years, 1909–19, and uses Douglas' earnings in coal mining to run back to 1899. King, of course, gives no description but his average wage in all mining (Martin, p. 93) appears to be much the same as his coal mining wage (Martin, p. 319—weighting anthracite and bituminous together). For employees, Martin uses Douglas' average earnings in coal mining also, to interpolate between 1902 and 1909 Census earnings figures. But, since Douglas interpolated between Census benchmarks to begin with, this means that Martin used the same average earnings trend for employees and entrepreneurs. (Martin actually uses coal plus oil mining, stipulating that oil wages varied with the value of petroleum production. But since oil wages account for 10 per cent or less of the combined total this variation makes little difference.)

[48] Martin, *National Income,* pp. 120, 121. For trade, he implicitly assumed a constant ratio of entrepreneurs to employees in the Census of Population occupation category "wholesale and retail merchants and dealers." However, in actual Census reporting a proportion of that category was actually employees—i.e., salesmen and store managers. See Alba Edwards, *Comparative Occupation Statistics,* 1943, p. 110.

[49] Martin, *National Income,* pp. 120, 121. He may have implicitly assumed a downward (or variable) trend in one set of ratios that was precisely offset by an upward (or variable) trend in the other. This seems most unlikely.

Conclusion

For industries which (in 1910) accounted for approximately two-thirds of Martin's total for realized private production income, he estimates 1900–20 trends in entrepreneurial income by assuming a constant ratio of employee earnings to earnings by entrepreneurs. For a substantial group, he assumed a constant ratio of employees to entrepreneurs as well. His method of estimate thus precludes variation in a large area. Within this area, variations in the ratio of entrepreneurial earnings to wages and salaries, and hence, in the ratio of wages and salaries to national income, could occur. We conclude that the use of such data for discussing stability in the ratio of labor income to national income is futile, adding little to our knowledge of the subject.

JOHNSON'S ESTIMATES, 1900 AND LATER

In his well known study, Gale Johnson has developed a series for factor income distribution from 1900 on. "As is generally known," he summarizes, "employee compensation has increased in relative significance since 1900—from about 53–55 per cent of the total in the first two decades to about 64 per cent in recent years . . . the total share of labor (i.e., including entrepreneurial labor) increased consistently for each overlapping decade beginning in 1915 . . ."[50] The overlapping decade figures, however, hide what the annual data show: that this basic advance to a new level was largely achieved in two jumps, from 1917 to 1918, and again from 1919 to 1920 (see Table 1 in which we summarize some of Johnson's results, utilizing unpublished annual data kindly provided by him). A look at the underlying data indicates the sources of this gain—decline in corporate profits, decline in entrepreneurial earnings, and a marked rise in wages.[51] How reasonable are such declines and the changing ratio that they produce?

1. For 1917–18 Johnson relies on a series published as "Department of Commerce" but concerning whose method of estimate we have no information.[52] That series shows a 25 per cent fall in

[50] D. Gale Johnson, "Functional Distribution," pp. 177–178.

[51] We rely on unpublished data kindly provided by Professor Johnson.

[52] The series was taken from the NICB, *The Economic Almanac 1951–1952* (page 208). Though labeled there as "Department of Commerce" it has apparently never appeared in any Department of Commerce publication and that Department has no information on its source.

TABLE 1

SHARE OF EMPLOYEE COMPENSATION IN NATIONAL INCOME

Year	Per Cent
1900–09	55.0
1910–14	55.4
1915–19	51.8
1920–24	61.7
1925–29	59.6
1915	53.5
1916	52.3
1917	46.7
1918	54.2
1919	52.6
1920	60.9
1921	66.9
1922	59.9
1923	60.4
1924	61.5

SOURCE: Johnson, "Functional Distribution."

nonfarm entrepreneurial income from 1917 to 1918—an almost incredible change under wartime conditions, particularly when the available estimates show no decline in the number of these entrepreneurs. King's estimates, as well as the Martin estimates based largely on them, show a mild gain over the period.[53] Until there is some basis for knowing how this "Department of Commerce" series was derived we find little plausibility in these particular figures. They imply that a sizable decline of average entrepreneurial earnings in trade, service, and manufacturing occurred at a time when the wage rates and annual earnings of employees in these sectors were rising significantly. It is most improbable, however, that the earnings of self-employed carpenters were declining while those of hired carpenters were soaring, or that the incomes of store owners were falling while those of store managers were rising. *A fortiori,* the labor component of nonfarm entrepreneurial income should certainly not be assumed to have declined (as is implicitly done in Johnson's computation of the service share, which takes that component as a flat percentage of the total).

2. It is for 1919–20, however, that the major problem arises. The ratio in 1918–19 did not differ significantly from that pre-

[53] Martin, *National Income,* p. 39; King, *Wealth and Income,* p. 108; and Kuznets, *National Income,* p. 463.

TABLE 2
FACTOR INCOMES AND SHARES, 1919 AND 1920
(dollar figures in billions)

	Johnson Data	Revised Data
Nonfarm entrepreneurial		
1919	9.4	7.8
1920	7.2	7.2
Farm entrepreneurial		
1919	8.8	9.5
1920	7.4	7.1
Corporate profits		
1919	7.7	8.4
1920	5.5	9.4
Interest and rent		
1919	7.5	7.5
1920	8.2	8.2
Employee compensation		
1919	37.1	37.7
1920	43.9	43.9
National income		
1919	70.5	70.9
1920	72.1	78.1
Share of wages in national income (per cent)		
1919	52.6	53.2
1920	60.9	56.2
Share of wages in nonfarm income (per cent)		
1919		60.1
1920		60.1

vailing over the first decade of the century. The real jump—to the level for the 1920's as a whole—came from 1919 to 1920. An explanation involves several elements. In Table 2, we show the Johnson estimates for major income categories for these two years, and also show our own revised estimates. We turn now to a description of the basis for our revision.

For nonfarm entrepreneurial income the Kuznets total (which Johnson uses without change) falls by $2.2 billion from 1919 to 1920. Of that decline, $1.6 billion is in trade alone.[54] Kuznets actually estimates a rise for the earnings withdrawn by trade entrepreneurs, for he assumes they gained as did the average earn-

[54] Withdrawal and savings data from Kuznets, *National Income*, pp. 312, 316.

ings of trade employees. The decline in trade income that he reports derives from his estimate of a still greater decline in the net savings of trade entrepreneurs.[55] The latter is computed on the assumption that the profit rate on sales of trade entrepreneurs paralleled that of trade corporations.[56] The following is a summary of the implicit averages from Kuznets, and our estimate of King's implicit figures.[57]

		1919	*1920*	*1921*	*Change, 1919 to 1920*
Average annual entrepreneurial income:	Kuznets	$4,023	$2,556	$1,420	−$1,477
	King	2,535	2,460		− 75
Average annual wage-salary:	Kuznets A	1,399	1,418	1,354	+ 19
	Kuznets B	1,506	1,664	1,451	+ 158

King's entrepreneurial income series declines trivially, while his wage-salary figures, and both Kuznets' wage-salary averages, show rises from 1919 to 1920. Kuznets' entrepreneurial average, however, shows a marked decline. If we follow the procedure used in later years of national income estimation we would expect a rough concordance of change between the total income of the average entrepreneur in trade and the income of the average employee. Had Kuznets followed this procedure for his trade (and manufacturing) estimate no marked change would appear in his

[55] The procedure for estimating the trade withdrawals is outlined in *ibid.*, p. 724.

[56] *Ibid.*, pp. 628, 726. Kuznets outlines a more complex procedure which we here define roughly as "profit rate on sales."

[57] Kuznets data from *ibid.*, pp. 718–719. The implicit King figures were derived as follows. Realized income drawn by entrepreneurs and other property owners is given in King (*Wealth and Income*, p. 108). From this total we deduct dividends, interest and rent to derive his implicit entrepreneurial income. King's dividend and interest figures appear in Simon Kuznets, *National Product in Wartime*, New York, NBER, 1945, p. 141. His rent total can be computed by deducting from his realized income total (W. I. King, *The National Income and its Purchasing Power*, New York, NBER, 1930, p. 94) Kuznets' estimates of the same total minus rent (*Simon Kuznets, National Income and Its Composition 1919–1938*, New York, NBER, 1941, Table 86). We then divided this total by King's estimate for the number of entrepreneurs (*King, National Income*, p. 62). The results will differ from those appearing in Kuznets' *National Product*, p. 141, because of adjustments made by the latter in the King data (*ibid.*, p. 144).

implicit, or in Johnson's explicit, figures on the U.S. total wage share from 1919 to 1920.

Alternatively one can do as Kuznets did, and stipulate that the withdrawn portion of entrepreneurial earnings did move with wages and salaries, but that, in addition, net savings of the enterprise moved in relationship to sales, as did the corporate profit ratio. Our only empirical evidence is from a different period: since the middle 1930's, when independent data begin, there appears to be a rough relationship between the trend in total income per entrepreneur in trade and the average employee in trade.[58]

We prefer to assume that the alternative incomes that link the entrepreneurial and labor market, and that affect the flow of manpower from one to the other, are total incomes in each category, rather than that one component of entrepreneurial income parallels wages while the other may pursue its separate path. We estimate a revised figure for nonfarm entrepreneurial income by assuming that the average income of trade entrepreneurs remained unchanged from 1919 to 1920, while wages rose.[59]

For farm entrepreneurial income we use the latest Department of Agriculture estimates[60] rather than earlier ones. These figures show a greater decline than those embodied in the Kuznets figures—apparently because of a far greater fall in cotton marketings.

For corporate profits we take the recent estimates of Goldsmith.[61] The gain shown would be even more marked if the basic Ebersole data on corporate profits were superseded by the NBER corporate sample for these years.[62]

[58] Department of Commerce estimates prior to the Bureau of Internal Revenue figures for entrepreneurial earnings in the late 1930's cannot be considered independent evidence on this point. The two move in the same direction in 1945–46 as well. They do not do so for 1946–48, as was pointed out in a comment by Selma Goldsmith. We attribute the entrepreneurial income decline in 1946–48 to the rise of about 10 per cent a year in the number of trade entrepreneurs, largely under the stimulus of GI loans, etc. No equal pressure on entrepreneurial earnings, however, is suggested for 1919–20, when the count of entrepreneurs rises only 3 per cent according to King. (Cyclical forces predominate in 1920–21 and 1948–49 and overwhelm the factors we are distinguishing here.)

[59] Kuznets (*National Income*, p. 718) shows a fall of $1.6 billion, with essentially no change in the count of entrepreneurs. We take 1920 as given and deduct $1.6 billion from Johnson's 1919 figure for nonfarm entrepreneurs, so that the implicit component for trade stays constant.

[60] *USDA Agriculture Handbook* No. 118, Vol. 3, p. 43.

[61] Raymond W. Goldsmith, *A Study of Saving in the United States*, Princeton University Press, Vol. III, 1956, p. 435 for tax liability and inventory valuation adjustment; Vol. I, 1955, p. 939 for net earnings.

[62] Goldsmith, *ibid.*, used the original estimates of J. F. Ebersole, S. S. Burr, and

For interest and rent we use the Johnson figures.

For employee compensation we adopt, with only one change, the Kuznets figures used by Johnson. The construction sector shows a marked rise; we believe a decline to be more reasonable.[63] The reported rise was estimated as follows: Ohio and Pennsylvania ratios of wages and salaries to gross construction totals were applied to U.S. gross construction activity totals. The results were then divided by an average wage-salary whose 1919–20 change is given by that for Ohio and Pennsylvania.[64] As thus estimated, the activity per person engaged falls slightly from 1919 to 1920 in current dollars, and a 20 per cent decline in constant dollars is implied.[65] We prefer here to assume that the constant dollar volume of work per employee did not decline from 1919 to 1920.[66] Working from employment and earnings data that we have derived elsewhere, we deduce a 1919–20 rise of $50 million instead of the $650 million rise implicit in Johnson's figures.[67]

To summarize, our revisions of Johnson's estimates of the share of wages in national income (see Table 2) are the result of: our revision of Kuznets' trade entrepreneurial total; the adoption of the latest revision of the USDA figures for farm entrepreneurial incomes rather than the earlier one available to Johnson; the use of corporate profits data derived from Goldsmith's recent study

G. M. Peterson, "Income Forecasting by the Use of Statistics of Income Data," *Review of Economic Statistics*, November 1929. The NBER data appear in *Historical Statistics*, p. 591. Because they relate only to large corporations it seems inappropriate to use them. However, examination of the Ebersole estimates indicates that $15.7 billion was added to the reported *Statistics of Income* figures for 1919, largely on the basis of fitting trends to the number of returns, total depreciation reported, etc., for various sets of years. With a profit rate of 8.7 per cent implicit in the reported figures this comes to a fairly arbitrary implicit correction of the reported figures by the addition of $1.4 billion to the 1919 net income total, leading to a greater 1919–20 decline.

[63] For the rise see Kuznets, *National Income*, p. 641.

[64] *Ibid.*, pp. 646, 653.

[65] For gross per person engaged, we use data from *ibid.*, pp. 641, 643. The gross per employee, while not directly estimated, should, we assume, have moved in the same way. The implicit deflator is that derived from the constant and current price construction totals in Simon Kuznets, *Capital in the American Economy, Its Formation and Financing*, Princeton for NBER, 1961, Vol. II, Tables 4 and 5.

[66] Our only independent evidence on this point relates to the period since 1940, when our construction employment figures are derived independently of the volume totals. For these years, a rise in the real output per employee takes place even when the rate of gain in construction is checked.

[67] We work essentially from the same activity figures as used by Kuznets, but deflate somewhat differently. Most important, however, we have an employment benchmark prior to 1929, and also interpolate between a 1900 and 1929 benchmark for activity per employee in constant dollars.

rather than those in the quasi "Department of Commerce" series; and a minor change in the employee compensation figures of Kuznets that underlie Johnson's figures. As can be seen from Table 2, the gain in the share of wages as shown in Johnson's original figures was 8.3 percentage points, and in our revised figures, 3 percentage points. The gain in the revised figures is thus half as great. Johnson links his series for the years 1900–19 by a ratio link in 1919. But the level of these earlier years would be much closer to that of the 1920's if the revised data were used for 1919.

One further comment must be made. If one takes the revised figures, which reflect a marked fall in farm entrepreneurial earnings, and computes the wage share only for nonfarm income, the result is the following:

	1919	*1920*	*Gain*
Wage ratio in nonfarm income	60.0	60.1	0.1

The gain for the nonfarm economy was trivial. Hence, the massive upward shift shown in the original estimates diminishes considerably in the revised figures. Of the gain that does appear, virtually all reflects a shift within the farm sector.

KING'S ESTIMATES, 1909 AND LATER

For 1909 and subsequent years, King provides annual estimates that constitute the ultimate basis of most of the later work in the field.[68] They have been used by the Department of Agriculture to extrapolate national income back to 1909. They appear to be the most likely source for the "Department of Commerce" series published in the *Economic Almanac*, and were also used by Johnson to run his data back to 1909. These estimates likewise constitute the basis for Martin's extrapolation of most of his components back to 1909 from 1919. Since King's post-1919 figures have been clearly superseded by Kuznets' work, only their use for 1909–19 extrapolation remains to be considered.

King's results can be summarized simply. From 1909 to 1917 he notes little change in the ratio of wages and salaries to the national income, while for 1918 he estimates a gain.[69] Hence the rise he shows in labor's share from 1909 to 1919 derives from his

[68] King, *National Income*.

[69] King's data as summarized and adjusted in Kuznets, *National Income*, Table 94. If we relate King's employee compensation to his realized income (Kuznets, *National Income*, p. 471) we arrive at the same trend.

estimate that a marked gain took place in 1917–18. How does this gain arise? It comes rather simply from his estimate that entrepreneurial withdrawals rose a trivial 5 per cent, while employee compensation rose by 25 per cent.[70] Since he shows virtually no change in the number of entrepreneurs over this period, and a decline in the number of employees, he implies an even greater discrepancy on a per-earner basis. Moreover, he assumes a $2 billion rise in realized income drawn from farming. Therefore he implicitly estimates no rise (or an actual decline) in income per nonfarm entrepreneur during a period when wages were skyrocketing.[71]

Labor's share, in consequence, jumps. Such a change is wholly unreasonable. First, had such a differential developed, a substantial movement from entrepreneurial pursuits into wage work should have taken place—as it did in 1941–42. But King stipulates no change in the number of entrepreneurs. Second, experience in World War II (for which we have data rather than reasoned surmise to guide us) indicates that entrepreneurial income rose by as much as, or more than, wages. From 1940 to 1941, for example, wages and salaries rose by about 30 per cent—or much the same as from 1917 to 1918—but income per entrepreneur rose by nearly 40 per cent.[72] If the wage worker were buying cotton shirts in 1917 and silk ones in 1918, something must have happened to the income of the trade entrepreneur selling him the shirts. And with construction wage rates rising (according to King) and construction booming, would the incomes of the independent construction entrepreneur have remained unchanged?

ESTIMATES SINCE 1919

For the period since 1919, estimates derivable by known procedures become available. The Kuznets figures for 1919–38 have already been considered in connection with Johnson's estimates. We concluded that the gain from 1919 to later years (and by extension, from 1919–20 or 1919–24 to later years) reflects a significant 1919–20 rise in the estimates. After reducing this estimated rise by adopting alternative estimating techniques, we con-

[70] Data summarized in Kuznets, *National Income*, Table 93.

[71] King's count of entrepreneurs appears in his *National Income*, p. 62; his realized income figures from *ibid.*, p. 108.

[72] U.S. Department of Commerce, *National Income*, 1954 edition, Tables 14, 17, 25 and 28.

cluded that the remainder is equivalent to the decline in farm entrepreneurial earnings. Hence, the share of wages in nonfarm income originating did not change in the 1919–29 period. Given the tremendous impact of the depression on the data for the 1930's, they are not very useful in considering long-term trends.[73] We therefore show no rise in the share of labor in nonfarm income originating.

For the period since 1929 the Department of Commerce figures have been precisely and comprehensively analyzed by Edward Denison.[74] After excluding household and government sectors (presumably because of the lack of a property income counterpart to wages for these sectors), Denison finds that a small gain in the employee percentage in the ordinary business sector took place from 1929 to 1950.[75] The well known sizable rise of the employee share in over-all national income over this period, therefore, proves to be primarily a reflection of changes in imputed rent on owned homes, profits on investments abroad, and other flows that are irrelevant to an interest in the substitution of one productive factor for another in the process of economic change.

It would be pleasant to conclude that the brief run of these two sets of data, for 1919–29 and 1929–50, are fortunate confirmations of the economic process outlined in Part I above. However, it is necessary to pursue one further analytic issue—that concerning the entrepreneurial share. Unless we do so, we shall not be clear about precisely what the empirical data on factor shares can actually tell us concerning changes in the production function, the elasticity of substitution, and the relative flows of capital and labor.

PART III

What basis is there for judging how closely the real world conforms to the pattern outlined in Part I of this paper? The obvious source—national income statistics—is subject to a number of deci-

[73] "The relative share of income from work in national income as a whole shows, of course, appreciable short-run variations in the course of the business cycle . . . . Consequently, when we engage in long-run analysis it is essential to select for comparison periods during which the cyclical factor may be assumed to have canceled out." Fellner, *Trends and Cycles*, p. 264.

[74] "Distribution of National Income," *Survey of Current Business*, June 1952.

[75] Denison (*ibid.*) reports a gain of under 1 per cent. If we use revised data from *U.S. Income and Output* (p. 134), the figure is 1.4 per cent. Hence there seems to be no basis for the 5 per cent figure referred to in conference discussions.

sive qualifications. Many of these, fortunately, have recently been canvassed by Irving Kravis, who demonstrates that varying treatment of these elements, numerous and significant though they are, would distort our reading of final results by very little.[76] However, a single major factor remains, and it makes inappropriate any extended attention to the statistics on labor's share in total national income, its share of income originating in agriculture, trade and service, or any combination that includes these sectors.

The problem here is the entrepreneurial puzzle. At the end of the year the entrepreneur finds in his till a sum—or sustains a loss —reflecting the return on his personal abilities as well as return on his capital invested. To discuss the relative return to the capital and to the labor which he uses in his activities, we must disentangle the contributions made by each. If we cannot do so, we must eschew the apparently relevant data for those industry sectors dominated by entrepreneurs. What are our choices? (1) We may ask the farmer, or businessman, but even if he could give us reliable figures, one may doubt whether a rational entrepreneur would attempt to do so: what the market has joined together he does not separate. (2) We might make some guesses from an analysis of the production function. But theory tells us nothing about how to allocate the joint product of two or more factors so as to reveal the average contributions of each factor. (Deductions as to the contribution of marginal increments of each factor to marginal changes in product do not help much for allocating the entire output.) (3) We might see what indications the market gives us—but the market does not operate in these terms and provides us with no information.

Failing all else, most analysts have made different types of arbitrary allocations. One is to stipulate a rate of return to entrepreneurial labor, with the balance of the entrepreneur's income considered the return to his capital.[77] Conversely, others have

[76] See Kravis' lucid discussion of market-nonmarket activity shifts, the role of government debt and of historical cost depreciation, etc., in "Relative Income," pp. 926–930.

[77] This procedure has been widely used, by W. J. Spillman, the Department of Agriculture and others. Its most recent important use is in the study by John W. Kendrick, *Productivity Trends: Capital and Labor*, New York, NBER 1956. Kendrick estimates labor input by weighting manhours in each major industry group, inclusive of those worked by proprietors and unpaid family workers, by base period average hourly employee compensation.

stipulated a rate of return to his capital and thence inferred the return to his labor. These procedures, of course, give significantly different results—but neither has any better theoretical justification than the other. A more thoughtful proposal allocates entrepreneurial income between capital and labor in accordance with the ratio of property to wage income in the portion of the economy outside the entrepreneurial sector, changing when that ratio changes.[78] While more precise, it does not seem to solve the problem.

1. How can we agree on what rate of return to stipulate for entrepreneurial capital—since this must be done, implicitly or explicitly? The risk may be greater, or less, than that which prevails for capital in, say, the corporate sector. Perhaps we could agree that an entrepreneur investing in his own store, or farm, must surely be more confident of earning a return than if he invested randomly in any other store or farm. (Would the typical entrepreneur go into business if he lacked such confidence?) But if so, his risks (as he perceives them) are smaller than those for investors generally in the same industry. To assume that his capital earns the full market rate of return would therefore overstate the rate for an equivalent risk as the entrepreneur himself saw it. But even if there were full agreement on this—which is doubtful —could we agree on the proper reduction to be made in the market rate to give a truer measure of his earnings on capital?

2. Can we proceed any more successfully by first intuiting the labor earnings of the entrepreneur? The massive decline of self-employment in trade, service and agriculture during 1941–42 suggests that much disguised unemployment existed in those industries—with many a self-employed person whose capacities were below the average for employees in the same industries. The market value of the labor services of such entrepreneurs may be zero, but so long as they can pay themselves (out of capital) they will receive more than the market would pay them. (Data showing a short work year for certain categories of entrepreneurs, and the high failure rate for firms in trade and service suggest that this

[78] Kravis, "Relative Income," p. 925. Denison suggests: "if one *must* allocate, a preferable procedure would be to assume the division between labor and property inputs and income to be the same in noncorporate as in corporate firms." See the comment by Edward F. Denison on paper by Edward Budd in *Trends in the American Economy*, p. 402.

is not merely hypothetical.[79]) Conversely, there will unquestionably be many entrepreneurs whose talents reach far above the level for employees in the same industry. How do we rationally estimate the proportion in each category? Surely not by using the simple 50-50 proportion implied when we estimate their average labor income as equal to the average for all employees.

3. The advantages of being one's own master surely exist and are surely positive. If they operated as other equalizing advantages do, they should work to keep down the dollar returns from self-employment. But by how much? And if some mystic with a Monte Carlo method gave us a figure, how much should we deduct from the labor share and how much from the property share? If one economist makes such an assignment, on what basis can he contend his assignment ratio is preferable to any other?

4. But away with these qualifications. Suppose we make an allocation at a point in time. On what basis can we choose the true allocation for the next point in time? The shifting tide of hopefuls that enter, and failures that leave, self-employment suggests that the net returns to self-employment are ever changing relative to the returns to capital and labor elsewhere. But how much of this net change occurs in the return to capital and how much in the return to labor? Without light in this apparently impenetrable maze we cannot know how the relative returns to each change over time. Kravis' solution is surely in the right direction: he stipulates that for the self-employed, the ratio of capital to labor returns will run parallel with that for the rest of the economy. But our present problem still remains—the regress is apparently an infinite one. If the ratio of one to the other is assumed to vary with the rest of the economy, then its assumed variation over time adds nothing to our knowledge of true variation over time: we are simply iterating the changes in the non-self-employ-

[79] In a typically incisive discussion of the allocation of entrepreneurial income between labor and property, Kuznets suggests different markets for entrepreneurial and other capital, as for entrepreneurial and other labor, noting that "a direct estimate of the return on the property component" for U.S. agriculture "leaves a return on labor that is below the going wages of hired labor; and a direct estimate of the return on labor leaves a return on property distinctly below any comparable return rate." Simon Kuznets, "Quantitative Aspects of the Economic Growth of Nations," IV, *Economic Development and Cultural Change*, April 1959, p. 26. Compare Simon Rottenberg, "Note on Economic Progress and Occupational Distribution," *Review of Economics and Statistics*, May 1953, for general comments on the level of entrepreneurial abilities in underdeveloped countries.

ment sector.[80] With the evidence from our competitive economy of flows of men (and money) into and out of enterprise, we have no basis for assuming that the rate of return to men (or money) in entrepreneurial pursuits remains stolidly constant relative to the return in corporate enterprise, or government bonds (or enlistment in the army).[81]

5. We conclude that nothing useful can be learned by dealing with relative shares for: (a) the economy as a whole, (b) the nonfarm economy, (c) agriculture, distribution or any industry (or combination of industries) in which entrepreneurial income plays a significant part. If we stipulate an unchanged ratio of labor to capital income for entrepreneurs within the entrepreneurial industries, we clearly add nothing to our knowledge of the changing rate of return to each factor. If we stipulate that the returns moved parallel to those in the rest of economy or industry, we simply iterate what we know already. If we are free to stipulate a changing ratio—and in the mobile, competitive real world changes surely must take place—we can discover empirical relationships of any type, largely given by our initial arbitrary stipulation of how these changes took place. Hence such limited bits of information on total entrepreneurial income as exist are of little service in our quest.

GOVERNMENT

Without descending to modern instances there is clearly no basis for distinguishing what portion of the ancient Roman Senator's salary derived from his florid oratory (labor) and what from the shining toga and elegant ivory chairs (capital) in which he reposed. Nor would one want to deduce, even with data, that the man with the larger desk received a higher income because he had a larger desk (more capital): his commanding presence alone might account for that income. Moreover, our income estimates include no allowance for the services of government capital. As estimates have been made hitherto, a rising level of government

[80] Kravis does not suggest that the absolute level of return here adds to our knowledge, seeking only to deal with the central issue of changing relative income shares. He notes the desirability of allocation not in proportion to economy-wide shares but those within the same industry (see "Relative Income," p. 926). But even if this were done, it would not meet the point we raise.

[81] And without identical changes of capital stock-labor ratios in entrepreneurial and those in non-entrepreneurial sectors we would have still not solved the problem.

employment automatically worked to bring an increase in labor's share—whether government assets quintupled over the same period, were reduced to zero, or remained unchanged.[82]

We conclude that for measuring long-term trends in relative shares there is little to be learned about changes in relative shares resulting even from massive changes in the production functions, if we concentrate our view on changes in total national income, or income inclusive of any significant entrepreneurial components. For the U.S., this leaves as industries not so dominated primarily manufacturing, and individual industries in transport, communications, and mining.[83]

Conclusion

In Section I we proposed a long-term mechanism of factor substitution that would work to bring an approximate stability in factor shares. In Section II we rejected existing long-term national income estimates as a basis for asserting that empirical data do demonstrate that such stability was in fact achieved in the U.S. during the past half century or so. Future work with empirical data, it would appear, must relate to the direct process of factor use and reward where it takes place—within individual industries. It is this process rather than any pretty constancy that is of substantial analytic interest.

We have asserted in Section III that the entrepreneurial puzzle makes it meaningless to consider trends for any sector where the entrepreneurial share is a great one—agriculture, trade, service. And a variety of authors have demonstrated the signal effects of changing industrial composition on any gross aggregates.[84] We

[82] Solow excludes the government from his detailed analysis for this reason ("A Skeptical Note," p. 623). Kravis ("Relative Income," p. 928) and Denison ("Distribution of National Income," p. 17) each note the lack of a property component in government income originating. The difficulty of estimating the current value and return on government assets, particularly military assets, is, of course, the reason why we have no such data.

[83] We exclude government for reasons noted above. Construction, even without the great role of self-employment, has little reliable data on shares prior to 1939. The existing estimates are sensible reconstructions and do not rely on any independent measure.

[84] One of the earlier studies, albeit only for the period since 1929, was the lucid review by Edward Denison, "Distribution of National Income," in *Survey of Current Business*. Recently Cartter (*Theory of Wages*, pp. 161–167) noted that intersector shifts in large measure account for the change from the 1920's to the 1950's in the share for the private business sector. See too, Budd's detailed study ("Factor Shares," pp. 381–391); his more recent comments (U.S. Congress, Joint

conclude that the relevant U.S. data for studying factor substitution and return relate primarily to manufacturing, mining, the utilities, and railroads.[85]

Examination of some of these data at an aggregative level suggests, as one would expect, both constancy and variation. For example, adjusted data for manufacturing show that payrolls as a percentage of value added did show a long-term stability:[86]

1889–99	54.0
1919–29	51.5
1947–54	53.9

(Even the lower figure for the 1920's may reflect only the inclusion of contract work in these data, unlike those for the other periods.) A more precise measurement, using direct estimates of property income (incorporating an allowance for current value depreciation) can be made from the data of Wooden and Wasson,[87] who show an approximate constancy over the shorter period from 1929 to the early 1950's.[88] But an examination of data for

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Economic Committee, *Employment, Growth, and Price Levels,* Part 8, 1959, pp. 2520, 2524); and Jesse Burkhead, "Changes in the Functional Distribution of Income," *Journal of the American Statistical Association,* June 1953.

[85] Conceptual and empirical problems make it useless to consider long-term trends for most components of finance and transportation.

[86] For 1899, the 1900 Census of Manufactures, I:59, provides data from which we can adjust to exclude contract work, hand trades, and firms grossing under $500. For the 1889–99 average, we compute ratios from the 1954 Census of Manufactures, Part I, pp. 2–3; then use the ratio of the 1899 average to the average of the two to raise the adjusted 1899 estimate. (1919–29 data on contract work for smaller firms are not available.) Data for later years are from the 1954 Census, Part I, pp. 2–3. Data prior to 1889 are not used because of major differences in coverage of small firms.

Solow ("A Skeptical Note," p. 627) notes a small increase after 1899 which he considers may be due to the changing character of output. Our data show no rise. The difference may stem from the fact that reliance on unadjusted Census data, as reported, involves the use of an 1899 figure inclusive of railroad car construction, while later censuses exclude this high wage-sales ratio industry.

[87] Donald Wooden and Robert Wasson, "Manufacturing Investment Since 1929," *Survey of Current Business,* December 1956, p. 20.

[88] An admirable, succinct study by Martin Bronfenbrenner, "A Note on Relative Shares and the Elasticity of Substitution," *Journal of Political Economy,* June 1960, emphasizes how wide a range of substitution elasticities and changes in capital-labor ratios are compatible with what appear to be "small" changes in labor's share. His review on this ground alone throws out much discussion about long-term stability. On the other hand, if we credit the Creamer-Kendrick data on the changes in the capital-labor ratio in manufacturing from 1899 to 1919, and the reversal from 1919 to 1953, then by the use of Bronfenbrenner's formula we can see that the labor-share ratios shown above imply striking changes in the elasticity of substitution from one period to the next. (See John Kendrick,

individual industries is really the area for investigation, and here preliminary study shows both striking regularity and striking variation. With the recent issuance of the long awaited study by Creamer, Dobrovolsky and Borenstein,[89] together with other volumes in the broad study of capital formation and financing it is possible to begin such work. The prospect for an extension of Department of Commerce work on manufacturing investment gives us hope of data carefully adjusted for the complexities of recent changes in depreciation allowances. Given such materials, economists ought to be able to say something more useful about the process of long-term factor substitution in the American economy.

COMMENT

JACK ALTERMAN, Bureau of Labor Statistics

Lebergott has provided us with an interesting and provocative paper to start the Conference. He has also succeeded in presenting his discussants with something of a dilemma. On the one hand, he proposes a theory based on the operation of certain market forces to explain the stability of factor shares. On the other, the paper implies that because of inadequacies in the data and, more important, the insuperable problem of allocating proprietors' income into labor and capital components, we cannot determine at present whether his particular theory, or for that matter, any general theory of factor shares is supported by empirical evidence. The paper implies that economists will have to await future work on individual sectors and industries, not affected by the "proprietor problem," before being able to say anything useful about changes in factor shares, factor substitution, and relative prices.

I should like to comment on both the theoretical and statistical

NOTE: The comments on Lebergott's paper represent my own views and are not to be considered as reflecting the official position of the Bureau of Labor Statistics, U.S. Department of Labor, on the topics under discussion.

EDITOR'S NOTE: Based on original version of the Lebergott paper (see the Lebergott reply).

Productivity Trends in the United States, Princeton for NBER, 1961, Table D-3.) Hence, the reported lack of change in labor's share for manufacturing would appear to be of economic significance.

[89] Daniel Creamer, Sergei P. Dobrovolsky, and Israel Borenstein, *Capital in Manufacturing and Mining*, Princeton for NBER, 1960.

aspects of the paper and indicate some reservations regarding the findings. Briefly stated, Lebergott's theory, considered as theory and without regard to empirical verification, is incomplete and can be considered an explanation of stability in factor shares only if certain other conditions are stipulated. Second, I would disagree with his finding that at present economists can say little that is useful regarding factor shares, even for areas of the economy not particularly affected by the proprietor allocation problem. I have attempted to provide some analysis of the change in factor shares in the corporate sector of the economy.

Lebergott's attempt to provide an explanation for stability in factor shares stems from his reluctance to accept the proposition that "reasonable" figures for elasticity of substitution or of propensities to save will yield numbers assuring relative stability in shares. As Lebergott asks, what is reasonable and why choose a particular figure? The theory developed in the paper suggests that a market mechanism exists which works to produce long-run constancy, apart from any estimate of elasticity of substitution that may seem reasonable.

Lebergott's theoretical explanation for expecting stability in factor shares is as follows:

> In summary, we define the share of national income flowing to wages as against capital as a function of the quantity and price ratios of each factor. We find that in the long run the quantity ratio is in turn a function of the changing price ratios. Taking the price of labor as given, we contend that the changing price of capital must bear a constant long-term proportionality to that of labor. This proportionality derives from the fact that the supply forces working to fix the price of capital are dominantly wage costs in the capital-producing industries and those that supply them. In a competitive market these wage costs parallel wage-cost changes in capital-using industries because wage changes for identical occupations must bear a parity to one another in all employing industries. On the other hand, historical experience does not suggest that productivity trends in the supplying and using industries are so negatively related as to make costs take a different course from rates.

It seems to me that the explanation for stability of income shares in terms of the general "proportionality" of changes in wage rates and in productivity for capital producing and using industries is

incomplete. It is incomplete in that it does not really explain how the assumed correspondence in movement of over-all unit labor costs and capital goods prices is supposed to result in stability of income shares. The gap in the theory is due to the fact that the initial explanation of stability of income shares in terms of the relationship of the price of labor and capital, in which the price of labor is defined as wage rate and the price of capital may be defined as property income per unit of capital services, becomes an analysis of the relationship of the price of labor and the price of capital goods, primarily machinery. The price of capital goods is not the same as the price of capital and until Mr. Lebergott's thesis is expanded to explain the relationship between unit labor costs, the price of capital goods, and the price of capital, we do not know whether the theory does provide an explanation for stability in factor shares or perhaps only a tendency towards stability.

It may be useful as background for further discussion and clarification to indicate the conditions under which proportionate changes in over-all unit labor costs and the price of capital goods would result in stability in factor shares.

The conditions for stability in factor shares can be stated in the following set of propositions, in which the various items are to be interpreted as indexes of change.

Indexes of Change

W = total labor income (current dollars)
R = total property income (current dollars)
O = total output (constant dollars)
K = capital stock (constant dollars)
C = capital stock (current dollars)
$\frac{C}{K}$ = price of capital goods
$\frac{R}{C}$ = rate of return on capital

Factor shares will remain stable if labor and property income increase in the same proportion.

$$\frac{W}{R} = 1 \tag{1}$$

$$W = \text{[illegible}R \tag{2}$$

It follows that factor shares will also remain stable if labor income per unit and property income per unit increase in the same proportion.

$$\frac{W}{O} = \frac{R}{O} \tag{3}$$

Under what conditions will the change in unit labor and unit property costs be equal?

Property income per unit of output is equal to property income per unit of capital input, times the amount of capital per unit of output (the reciprocal of the capital productivity ratio).

$$\frac{R}{O} = \frac{R}{K} \times \frac{K}{O} \tag{4}$$

What is the relationship between the price of capital (R/K) and the price of capital goods (C/K), a relationship which is not explicitly stated in the Lebergott theory?

$$\frac{R}{K} = \frac{C}{K} \times \frac{R}{C} \tag{5}$$

The change in the price of capital is equal to the change in the price of capital goods times the change in the rate of return on capital.

We can now substitute step (5) in (4)

$$\frac{R}{O} = \frac{C}{K} \times \frac{R}{C} \times \frac{K}{O} \tag{6}$$

There is one further step needed to complete the statement of relationships. That is to introduce into the equation Lebergott's proposition regarding the parallelism in unit labor costs and the price of capital goods.

$$\frac{W}{O} = \frac{C}{K} \tag{7}$$

$$\frac{R}{O} = \frac{W}{O} \times \frac{R}{C} \times \frac{K}{O} \tag{8}$$

It follows that if the rate of return in capital (R/C) and the productivity of capital (O/K) are constant, or if they change by the same proportion, then the change in unit property income will

equal the change in unit labor income and income shares will be stable.

$$\frac{R}{C} \times \frac{K}{O} = 1 \text{ or } \frac{R}{C} \div \frac{O}{K} = 1 \tag{9}$$

$$\frac{R}{O} = \frac{W}{O} \times 1 \tag{10}$$

$$R = W \tag{11}$$

To summarize, income shares will remain constant if the price of capital goods increases in proportion to the unit labor costs, only on the condition that the rate of return on capital changes in proportion to the average change in the productivity of capital. It follows from the above that before Lebergott's thesis regarding similar changes in unit labor costs and capital goods prices can be considered a complete explanation, even in a formal sense, of stability in factor shares, it needs to be expanded to provide an explanation of why the real rate of return on capital should be expected to increase in proportion to the increase in output per unit of capital.

There still remains the question of the extent to which the data and empirical research support the general notion of stability in factor shares and, more specifically, the validity of Lebergott's explanation. The paper indicates that the question of stability in factor shares cannot be answered at the level of the total economy because the particular method or assumption used to allocate the income of proprietors into labor and property components would determine the result. The paper concludes that we must await further work in those sectors of the economy where proprietors are not a major factor, e.g., mining, manufacturing, utilities, before we can provide useful answers to questions regarding long-run trends in factor shares.

Presumably because of the negative findings regarding the relevance of over-all data to the question of factor shares, the paper does not even attempt to determine whether there is any empirical evidence which supports some of the propositions developed in the theoretical section, particularly the relationship of changes in wage rates, output per man-hour, unit labor costs, and capital goods prices.

I would agree with Lebergott that we need more research on factor shares, factor substitution, and price at the sector and in-

dustry level, but I do not agree with the implication that pending further research in the sectors not affected by the "proprietor allocation problem," we can say little that is useful regarding changes in factor shares. A considerable amount of work has already been done by those who have, in a sense, anticipated Lebergott and argued that because over-all factor shares reflect varying labor-property relationships in substantially different types of legal and institutional organizations, the analysis of factor shares for the economy as a whole has relatively little meaning. Meaningful analysis, therefore, involves an attempt to determine what has happened to the various categories of factor income within relatively homogeneous groupings in the economy and also how much of the change in shares may be due to shifts within these homogeneous groupings. Denison's work in analyzing changes in factor shares in terms of homogeneous sectors and the further decomposition of changes in factor shares into inter- and intraindustry changes is well known and need not be elaborated upon here.[1] This analysis has been further refined by the work of Osborne and Epstein[2] for the corporate sector, and by Fitzwilliams[3] for the nonfarm-noncorporate sector to determine how much of the change in income, by type of income, has been due to interindustry shifts within the two major types of business. These studies have indicated that interindustry shifts within the homogeneous sectors mentioned above have had little effect on secular distribution of factor shares.

In terms of Lebergott's basic question: whether we can say anything useful regarding factor shares which is not subject to the criticism of being dependent on the method used to allocate proprietors' income, we must turn to the data for the corporate sector of the economy.

The estimates developed by the National Income Division, Department of Commerce, of factor shares originating in the domestic corporate sector of the economy represent a basic source of information for such an analysis. These estimates are available for the entire period since 1922—almost forty years. In order to

[1] Edward F. Denison, "Distribution of National Income," *Survey of Current Business*, June 1952. Also, "Income Types and the Size Distribution," *American Economic Review*, May 1954.

[2] Harlowe Osborne and Joseph Epstein, "Corporate Profits Since World War II," *Survey of Current Business*, January 1956.

[3] Jeannette Fitzwilliams, "Employment in Corporate and Noncorporate Production," *Survey of Current Business*, November 1959.

obtain an indication of the secular trend in factor shares over the period, a comparison of the percentage distribution in 1922–29 and 1947–59 has been made, omitting the abnormal years of the depression and World War II.

These estimates need to be adjusted to exclude the effect on corporate income originating and property income of the accelerated amortization program which started in 1950 and the changing methods of depreciation permitted under the 1954 Internal Revenue Code. The adjustments are necessary in order to provide comparability over time and to indicate what the corporate income and the property share would have been if straight-line depreciation had been used throughout the period. Estimates of the Machinery and Allied Products Institute were used to make the adjustments.[4] These adjustments yield results which seem to be roughly in line with similar estimates given in general terms in various articles in the *Survey of Current Business*. It should be noted that these adjustments are quite substantial (about $4 billion in 1959) and may become larger and, therefore, more important in the analysis of factor shares as the amount of depreciation under the 1954 Internal Revenue Code begins to cumulate, although this will be offset for a while by the decline in depreciation resulting from the 1950 accelerated amortization program.

Another adjustment to the estimates probably should be made in order to reflect replacement-cost depreciation rather than the book-value depreciation actually used. Here, it is not clear whether replacement-cost depreciation should reflect the increase in the productivity of capital goods as well as the increase in the price of capital goods or merely the latter. Pending clarification of this point, I have taken the figures as published except for the adjustments previously mentioned.

The percentage distribution of factor shares for subperiods, shown in Table 1, indicates relative stability between the two terminal periods, with some indication of a slight upward drift in the labor share during the postwar period, although this is moderated after adjustments for accelerated amortization and changing methods of depreciation.

How does the estimated change in factor shares for the corporate sector compare with the change for the total domestic economy? Estimates for the total domestic economy have been developed by

[4] Machinery and Allied Products Institute, *Capital Goods Review No. 38.*

TABLE 1

FACTOR SHARES IN CORPORATE SECTOR,[a] PERCENTAGE DISTRIBUTION

	Unadjusted		*Adjusted*	
Year	Labor[b]	Property[c]	Labor[b]	Property[c]
1922–29	77.0	23.0	77.0	23.0
1925–29	75.7	24.3	75.7	24.3
1947–59	77.9	22.1	76.8	23.2
1950–54	76.5	23.5	76.1	23.9
1955–59	79.4	20.6	77.9	22.1

[a] Excludes income from abroad.
[b] Compensation of all employees includes supplements, wages, and salaries.
[c] Profits (before Federal corporate income tax) and net interest.

John W. Kendrick in his book on productivity trends in the United States.[5] Kendrick allocated income of proprietors by assuming that they received the same compensation per hour as employees in the same industry. Kendrick's figures are available for selected years only. The comparison of the change between 1929 and 1957 indicates a substantially greater decline in the property share for the total domestic economy than that shown for the corporate sector (Table 2).

TABLE 2

CHANGE IN FACTOR SHARES, CORPORATE VERSUS TOTAL DOMESTIC ECONOMY

	Corporate		*Total Domestic Economy*	
Year	Labor	Property	Labor	Property
1929	74.6	25.4	72.3	27.7
1957	78.5	21.5	81.4	18.6
Per cent change	5.2	−15.4	12.6	−32.9

The change in factor shares for the corporate sector are less than half that indicated by Kendrick's figures for the total domestic economy. This underscores the need to analyze changes of factor shares in terms of homogeneous categories within the economy.

Having provided some indication of what the change in factor shares has been for the sector of the economy not affected by the proprietor allocation problem, we can turn to the question of whether there is any empirical basis for Lebergott's explanation

[5] John W. Kendrick, *Productivity Trends in the United States*, Princeton for NBER, 1961.

of stability in factor shares. Lebergott's theory assumes that increases in wage rates will be relatively evenly distributed throughout the economy, and that the resulting attempt to offset these wage increases by increased output per man-hour will produce an increase in unit labor costs for capital goods and supplying industries in about the same proportion as unit labor costs of capital using industries. This is a rather big assumption.

It would be difficult to determine this empirically. We can only approach the problem by trying to ascertain, following Lebergott's theory, whether prices of capital goods have moved in proportion to the change in unit labor costs. Here, we must also keep in mind Lebergott's admonition that for this type of comparison, unit labor costs cannot be based on any assumption as to the allocation of proprietors' income to labor and property components.

We have, therefore, used the data for the corporate sector of the economy, supplemented by other information, to try to fill in some of the numbers in the formulation developed as an expansion of Lebergott's theory.

Estimates of the change in labor income per unit of output and property income per unit of output for the corporate sector of the economy can be derived by dividing indexes of labor income and property income by an index of corporate output. Corporate output, in turn, can be derived by dividing estimated corporate gross product by the implicit price deflator for the private nonfarm business sector of the economy.

The variables underlying the change in property income per unit of output can be derived if we can fill in two missing pieces, the change in price of capital goods (an essential element in Lebergott's theory) and the increase in the physical quantity of capital inputs. Estimates for these items are not available for the total corporate sector so we have used the available estimates for total manufacturing as an indicator of the change in capital goods, prices, and capital inputs for the corporate sector. It should be noted that the estimates of capital input cover the depreciated value of plant and equipment, plus inventories. The stock of net plant and equipment refers to capital available rather than capital used. It can reasonably be argued that for some purposes the capital estimate should be adjusted to refer to capital used rather than available.

The labor percentage of corporate income changed from 74.6 per cent in 1929 to 77.7 per cent in 1959. The changes between

1929 and 1959 in factor shares and related variables are shown in the following indexes (1929 = 100).

Unit labor cost	194
Unit property cost	163
Capital goods prices	252
Equipment	243
Structures	318
Equipment and structures	287
Inventories	186

Unit property cost (163) equals price of capital goods (252) times rate of return (97) divided by capital productivity (151).

The relationships indicated by these figures do not seem to be consistent with Lebergott's explanation for relative secular stability in factor shares. The period covered is thirty years, and the increase in the labor share is relatively modest—3 percentage points. The price of capital goods, however, increased substantially more than unit labor costs. The rate of return did not increase in proportion to the increase in the productivity of capital but instead was relatively constant. The relative stability in factor shares was due largely to the offsetting changes among the variables rather than stability in the relationships as suggested by Lebergott's theory.

The results for the 1929–59 comparison are puzzling in at least one respect; the relative stability in the rate of return on the net value of capital available is quite inconsistent with a similar estimate for the corporate sector developed by the Machinery and Allied Products Institute. The MAPI figures indicate an increase of about 60 per cent between 1923–29 and 1959 in corporate profits, including intercorporate dividends, as a percentage of corporate net worth. The increase between 1923–29 and 1950–59 was 70 per cent. The MAPI estimates of net worth are based primarily on *Statistics of Income* data, adjusted to convert book value of plant and equipment to current cost, and further adjusted to reflect what net worth would be if straight-line depreciation had been used throughout. The MAPI figures have an additional adjustment to profits, not included in my estimate, to reflect current cost valuation of depreciation rather than book value depreciation. A similar adjustment to my estimate would have widened the gap rather than narrowed it. Recognizing all these differences, it seems dif-

ficult to believe that both estimates are "correct" and the big disparity between the estimates of rate of return can be attributed primarily to differences in basic data or methodology. This major disparity in findings needs to be clarified before we can describe, in empirical terms, the process by which stability in factor shares for the corporate sector of the economy has been achieved.[6]

GEORGE GARVY

The first part of Lebergott's paper suggests an explanation for the relative stability of the labor share in national income, which has been the subject of a good deal of research and theorizing ever since Keynes called it "a bit of a miracle." Lebergott's reasoning is simple and elegant. In ultimate analysis, the cost of capital goods can be reduced in essence to wage costs incurred in previous periods. Therefore, in the long run, the price of capital goods must bear a constant long-term relation to that of labor. Lebergott's theory includes a number of simplified assumptions which, however, do not diminish its usefulness in providing quite a satisfactory rationalization of the relative long-term stability of factor shares. I find it more realistic and therefore more appealing than reliance on the fact that, given the ratio in which labor and capital are typically combined in advanced economies, quite a wide range of elasticities of substitution are consistent with observed ratios of the labor share in national income.

The second part of the paper deals with several statistical series on which much of the analysis on long-run changes in factor shares rests; it is a skillful and imaginative contribution in the much neglected area of statistical criticism. Indeed, economists are always on the lookout for time series and cross sectional data that seem appropriate for testing theories. Statisticians are continuously striving to widen the scope of statistical data and to improve and refine current series. Perhaps not enough effort is devoted by either economists or statisticians to a critical review of synthetic

[6] Estimates of capital stock, which have been published since these comments were made (Simon Kuznets, *Capital in the American Economy*, Princeton for NBER, 1961; John W. Kendrick, *Productivity Trends*), would indicate that part of the disparity regarding the change in the rate of return may be due to the fact that the use of the change in manufacturing capital stock as an indicator of movement in total corporate capital stock probably overstated the increase in capital stock and correspondingly understated the increase in the rate of return. Preliminary calculations based on the new estimates indicate, however, that this would explain only part of the disparity, and that further research in this area is still required.

time series which have been constructed for earlier periods for which no data derived from records or estimates prepared by qualified statistical agencies are available. Lebergott's critical analysis casts considerable doubt on the validity of several widely accepted generalizations as to long-run changes in factor shares.

I find myself in complete agreement with Lebergott on the two main conclusions of the final section. One is his strong strictures against any attempts to split entrepreneurial income into a labor income and a return on capital component. I have made very similar points in commenting on a paper by Irving B. Kravis[1] (since they have been published in the meantime, I need not repeat them). I concur, in particular, with Lebergott that allocation of entrepreneurial income on the assumption that relative shares of labor and capital in this component move in the same way as in the non-entrepreneurial part of national income, does not add anything of analytical significance. I, therefore, fully agree with his second and related conclusion that an analysis of the labor share should be limited to the sector of the economy where both labor and property income originate; this limitation excludes by definition the government and the household sectors, and also suggests that sectors dominated by entrepreneurial activity, such as agriculture, retail trade and service industry, should be excluded.

While I find Lebergott's general position and conclusions very congenial to my own views, I remain unconvinced that the effort spent in recent years to measure and explain long-term changes in factor shares has yielded significant insights into the dynamics of our economy. The tenuous and changing relationship which exists between theoretical concepts of factors of production and the income streams actually measured in national accounts, precludes establishing a firm link between statistical findings based on the sort of data we have to work with and theoretical generalizations which they are supposed to support or to question. In recent studies of long-run changes in factor shares, a variety of statistical results have been obtained, depending largely on the assumptions made to narrow the gap between theoretical categories of factors of production and the type distribution of income. Empirical and theoretical work has been almost entirely concentrated—as again attested to by Lebergott's paper—

[1] *Proceedings of the Conference on Consumption and Saving* (University of Pennsylvania, 1960), Vol. II, p. 477 ff.

on the relative shares of labor and capital; little effort has been made to estimate the share of land. There is, furthermore, still little recognition of the fact that wages and salaries include a return on investment in human capital. Yet, the importance of human capital has been so clearly demonstrated by many recent economic developments, including the rapid resumption by Germany of its place as a leading industrial nation after a widespread destruction of its physical capital in World War II.

It is perhaps possible, following Lebergott's excellent suggestions, to make further progress towards a conceptually more satisfactory and statistically more reliable allocation of a large segment of national income between labor and capital for the period since 1900. But what questions will such a two-way breakdown help us to answer? Suppose that it will be possible to establish beyond statistical doubt that, over half a century, the share of labor compensation in the private sector has risen (fallen) by two percentage points; given the structural changes in our economy since the beginning of the century, what would be, precisely, the analytical value or the policy implications of such a finding?

PAUL STUDENSKI

Stanley Lebergott has ably analyzed some of the conceptual and statistical difficulties involved in the attempts to break down the national income total for the United States into its capital and labor shares and to measure the changes occurring over time in their relative magnitudes. He has emphasized particularly those difficulties which are due to the presence in our national income figures of a large amount of entrepreneurial income which contains both capital and labor shares. But he has not mentioned another difficulty, which, though of much smaller dimensions, is none the less material. I refer to the fact that the remuneration of corporate officers, which in our national income estimates is classified as labor income, contains within it a substantial amount of capital income.

It is a well known fact that due to the high rates of our federal corporation income tax and also the highly graduated rates of our federal personal income tax, corporate officers today prefer to take their shares of the company's income in the form of large salaries, bonuses and pension rights rather than in the form of ownership of stock and receipt of dividends. They prefer this form of remuneration because its payment is deductible from the company's

taxable income and is, therefore, generally provided very liberally to them. In fact, this type of remuneration is generally set at amounts which would insure to the officers substantial net incomes after payment of personal income taxes—a result which cannot be readily obtained by the officers under a stock ownership type of reward. The incomes received by corporate officers in that form, however, are only in part derived from their personal efforts. In large part, they are earned by the company's capital and are paid to the corporate officers just as if these officers were owners of some of this capital except that the payments are made in a different form. These payments contain, therefore, substantial elements of capital income. They are in effect a mixed capital-labor income, rather than pure labor income. Their classification as labor income in our national income estimates, or analyses thereof, is not completely correct and tends somewhat to overstate the labor share and understate the capital share in the total.

This distortion of the relative sizes of the two shares is probably not very large, but its existence none the less cannot be ignored. It must be accounted for particularly, inasmuch as its importance seems to be increasing over time. The Soviet critics of our national income estimates are placing considerable emphasis on this flaw in our national income classification. In fact, they are attributing to it greater importance than is warranted by the facts; and they tend to discredit our estimates in part on this account. It seems to me imperative, therefore, that in all our estimates of the capital and labor shares in our national income total, we should clearly indicate that our figures of labor income contain this particular admixture of capital income and that we should even try to measure the probable extent of this admixture.

I should, therefore, like to ask Lebergott whether in the course of his analysis he has taken account of this particular overstatement of the labor share and understatement of the capital share and has attempted to estimate the magnitude of the resulting errors.

REPLY by Stanley Lebergott

Alterman makes two major points. (1) He asserts that useful conclusions can be drawn concerning factor shares for the corporate universe, and backs this up with an excellent empirical discussion. My own analysis concentrated on the shortcomings of discussing constancy in the aggregate income share simply because

that was the focus of much speculation in recent years. Although I find distribution and the process of factor substitution most comprehensible at an industry level, Alterman's fine positive contribution leads one to hope that he will further illuminate this intermediate level of aggregation. (2) The bulk of his comments derive from an unpardonable laxity in my original phrasing, which stipulated that the machinery producing industries passed the gains of their productivity advance on in the form of lower "machinery prices." It should have read "machinery service prices"—both as being more correct, and because it was irrelevant for my analysis whether these gains were passed on by lower unit machine prices, an increase in productive capacity per unit, or both. Alterman's equation (7) picks up this error, and most of his subsequent extended analysis and computations drive its sorry consequences home. (I take some consolation in having thereby elicited his elegant and equable analysis.)

Mr. Studenski questions the reported figures on labor's share, noting how elements of return to capital are really embedded in the figures for salaries of corporate officials. One might go further to note that some salary receipt is in the form of stock and other property, the return from which appears in the accounts as a return to capital rather than labor. And, of course, stock options are a salary equivalent that does not even appear in our accounts at all. The use of the reported data, after they have passed the purview of tax officials and tax lawyers, unquestionably poses problems as to their precision for settling issues of interest to economists.