A SKEPTICAL NOTE ON THE CONSTANCY OF RELATIVE SHARES

By ROBERT M. SOLOW*

Ever since the investigations of Bowley and Douglas it has been widely believed that the share of the national income accruing to labor is one of the great constants of nature, like the velocity of light or the incest taboo. Keynes [12, p. 48] called it "a bit of a miracle." Even if it is sometimes observed that the pattern of distributive shares shows long-run shifts or short-run fluctuations, the former can be explained away and the latter neglected on principle. The residual belief remains that, apart from a slight (and questionable) upward trend and a countercyclical movement, the share of wages in the privately produced national income is unexpectedly stable. Much effort is devoted to exploiting and explaining this fact.

The object of this paper is to suggest that, like most miracles, this one may be an optical illusion. It is not clear what exactly is meant by the phrase: "The wage share in national income is relatively stable" or "historically almost constant." The literature does not abound in precise definitions, but obviously literal constancy is not in question. In any case, what I want to show is that for one internally consistent definition of "relatively stable," the wage share in the United States for the period 1929-1954 (or perhaps longer) has not been relatively stable.

If this contention is accepted, it is not without some general implications for economic theory. Beginning with Ricardo there have been sporadic revivals of interest in macroeconomic theories of distribution.[1] Now it is possible to have an aggregative distribution theory without believing in the historical constancy of relative shares, but the belief certainly reinforces the desire for such a theory. After all, a powerful macroeconomic fact seems to call for a macroeconomic explanation. It need not have one, but that is beside the point. As Kaldor says [9, p. 84]:

* The author is professor of economics at the Massachusetts Institute of Technology. He was a fellow of the Center for Advanced Study in the Behavioral Sciences, Stanford, California, when this paper was written. Mrs. Mary Girshick did the computations.

[1] I suppose the main contributors since Paul Douglas [4] have been M. Kalecki in [10] and, more recently [11, Ch. 2]; K. Boulding [2, Ch. 14]; N. Kaldor [9]; Kaldor's main argument was anticipated five years earlier by F. H. Hahn [6].

> . . . no hypothesis as regards the forces determining distributive shares could be intellectually satisfying unless it succeeds in accounting for the relative stability of these shares in the advanced capitalist economies over the last 100 years or so, despite the phenomenal changes in the techniques of production, in the accumulation of capital relative to labor and in real income per head.

But if, in fact, relative stability of distributive shares is at least partially a mirage, one may feel freer to seek intellectual satisfaction elsewhere. There is still a lot to be explained.

I. *How to be Constant though Variable*

Table 1 shows the share of compensation of employees in a number of different aggregate income totals, so that the reader can see what kind of variability occurs, over the business cycle and over longer periods.

What does an economist mean when he says that the wage share has been relatively stable? Since he does not mean that it has been absolutely constant, he must mean that in some sense or other it has been more nearly constant than one would ordinarily expect.[2] The sentence already quoted from Kaldor suggests that since technique, real capital and real income per head have all changed "phenomenally," you would normally expect distributive shares to have changed "a lot," but they have only changed "a little" and this requires a special explanation. Not to split verbal hairs, it is evident that this is no definition at all. One must have some standard by which to judge whether some particular series of observations has fluctuated widely or narrowly.

Such standards of comparison can arise in a variety of ways. A tight theory may itself provide a benchmark. For example, the fraction of males among live births in a well-defined animal population is subject to statistical fluctuations from year to year. But the theory of sex determination, although perhaps not complete, gives some indication of how variable one ought normally expect the series to be. To say that the series is relatively stable could then simply mean that the observed variance is significantly less than the variance expected from the theory. Something like this does appear to be in the back of some authors' minds when they refer to the stability of the wage share. Take as a starting-point the neoclassical general equilibrium theory of distribution, which is formulated in terms of production functions, input-ratios, and the like. These quantities fluctuate over time. Ought not the pattern of distributive shares show comparable variability, according to the theory?

[2] A sporting colleague of mine once offered to bet that Vincent Impellitteri would get more votes for Mayor of New York City than most people expected.

TABLE 1.—SHARE OF COMPENSATION OF EMPLOYEES IN VARIOUS INCOME TOTALS, 1929–1955

Year	As Per Cent of National Income	As Per Cent of Privately Produced Income	As Per Cent of Income Originating in Corporate Business	As Per Cent of Income Originating in Manufacturing
1929	58.2	55.6	74.6	74.2
1930	61.8	57.3	78.7	76.7
1931	66.5	63.2	87.9	88.0
1932	73.2	69.3	101.0	108.0
1933	73.4	69.5	101.6	104.7
1934	70.0	69.6	88.3	89.4
1935	65.3	60.8	83.8	82.6
1936	66.1	61.3	80.0	78.3
1937	65.1	61.0	79.9	78.7
1938	66.6	61.8	83.0	83.3
1939	66.1	61.6	80.9	79.9
1940	63.8	59.5	76.2	73.4
1941	61.9	57.6	72.7	69.0
1942	61.9	56.8	71.7	71.1
1943	64.4	57.6	72.2	73.4
1944	66.4	58.8	73.8	74.8
1945	68.0	59.8	77.0	77.3
1946	65.5	60.6	79.9	78.8
1947	65.3	61.7	77.5	75.9
1948	63.6	60.0	74.8	72.9
1949	65.2	61.2	75.7	73.5
1950	64.3	60.4	73.6	70.8
1951	65.1	60.8	74.0	71.1
1952	67.2	62.8	76.7	75.4
1953	68.9	64.5	78.5	77.5
1954	64.4	65.0	79.6	79.3
1955	68.9	64.7	77.4	76.5

Source: Department of Commerce, *Surv. Curr. Bus., National Income* Supplement, 1954, and July 1956.

But there is a world of difference between this case and the genetic illustration. The general equilibrium theory is in the first instance a microeconomic one. Between production functions and factor-ratios on the one hand, and aggregate distributive shares on the other lies 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. It is hard to believe that the theory offers any grip at all on the variability of relative shares as the data change—in fact this may be viewed by some as a symptom of its emptiness. A license to speculate, maybe, but hardly a firm standard. As a matter of speculation, the theory might be taken to imply that the aggregate shares come about through a kind of averaging process, in which many approximately independently changing

parameters intervene. From this view would follow an expectation of "relative stability," if anything.

A second possible source of a standard of variability is suggested by the analogy of statistical quality control. There the problem is also one of detecting "excessive" variability (or sometimes even deficient variability). But in the absence of some outside specification, the standard is usually given by the past behavior of the process itself. Clearly if the wage share had once oscillated between 50 and 80 per cent and now moved only in the range from 60 to 70 per cent, we could speak of relative stability. But it is not claimed that this is the case.

Third, the contrast between micro- and macroeconomic theories suggests that it might be possible to formulate an *internal* standard of variability. A hint in that direction is contained in a remark of Phelps Brown and Hart [14]: "Yet it still remains true that the changes in the share of wages in national incomes are not so great as we should expect when we look at the often wide swings of the corresponding shares within particular industries, and this relative stability also calls for explanation." Indeed it does; if the calorie contents of breakfast, lunch, and supper each varies widely, while the 24-hour total remains constant, we at once suspect a master hand at the controls. Similarly if wide swings within industries yield only narrow swings in the aggregate, this points to some specifically interindustrial or macroeconomic force.

But relative shares have denominators as well as numerators. However we subdivide the economy, the over-all share will be a weighted average, not a sum, of the respective shares for the subdivisions. This does not automatically entail that the over-all share will have a smaller variance than the sector shares. That all depends on the intersector correlations, i.e., on the macroeconomic forces. Note an interesting consequence: it is *negative* correlations between sectors which reduce the variance of the weighted average.

Here we have something empirically testable. Suppose, to take the simplest possible case, the economy is divided into k equal-sized sectors, in each of which the wage share is equally variable through time. Then if the sector shares fluctuate independently, the aggregate wage share will have a variance only $1/k$ times the common sector variance. If this were in fact the picture, it would be hard to claim that the relative stability of the aggregate shares required a specifically macroeconomic explanation. It might still be claimed that the aggregate share is more stable than it ought to be on this hypothesis, but now the explanation would have to be sought in the excessive stability of the individual sector shares. I suppose it could be plausibly argued that there are macroeconomic reasons for such microeconomic stability, but this is not the form that current theories take.

The more general case is no more complicated. Suppose there are k sectors, with shares $S_1, \ldots, S_k$ and weights in the aggregate $w_1, \ldots, w_k$. If the S_i represent the share of wages in the sector value-added, the w_i will represent the share of the sector value-added in the total. Let σ_i^2 be the variance of S_i through time, and let symbols without subscripts represent the aggregate share and its variance. Then in the null case of independence among sectors we would find:

$$\sigma^2 = \sum_1^k w_i^2 \sigma_i^2, \tag{1}$$

and in any case we would have

$$S = \sum_1^k w_i S_i. \tag{2}$$

Predominantly positive correlations among sectors will yield a larger σ^2 and negative correlations a smaller σ^2.

The value-added weights, however, are not constant from year to year. And on the face of it changes in the weights might be expected to be the main intersectoral force accounting for the relative stability of the aggregate share. If in fact the aggregate share fluctuated less than the sector shares would suggest, this might come about through countershifts in the weights: low-share sectors gaining in weight at the expense of high-share sectors when sector shares rise, and vice versa. There are good theoretical reasons why this might occur, but the fact is that it does not.

This subsidiary proposition is easly testable. It is only necessary to recompute the over-all shares using the observed sector shares but some fixed set of base-year weights. This has been done by Kalecki [11, p. 32] for U. S. manufacturing, 1879-1937, and by Edward F. Denison [3, p. 258] for the "ordinary business sector," 1929-1952. In both cases the fixed-weight series showed approximately the same amplitude of fluctuation as the observed series. The same conclusion can be read from the data to be analyzed below. Short- and long-run changes in the importance of the various sectors are important economic facts.[3] but they are not what accounts for the variance or lack of variance of the over-all shares. Thus in making use of formulas 1 and 2 I have in each case recalculated the averages using the value-added weights of a fixed base-year, usually somewhere in the middle of the period.

[3] James W. Beck [1] explicitly investigates short-run changes in over-all shares during the three periods 1930-32, 1941-43, 1950-53. Only in the second of these were weight-shifts a predominant factor. One wonders whether commodity substitution would not prove to be more important in a finer industry classification. John Dunlop, in his pioneering study [5, esp. pp. 163-91], also found weight-shifts to be a significant factor for the period 1929-34.

II. *Empirical Results*

The sector shares in Table 2 were calculated from the 1954 *National Income* Supplement to the *Survey of Current Business* (pp. 176-79). In each case they represent the ratio of "compensation of employees" to "national income originating."[4] The original data are reported for eleven sectors, not the seven used here. The four disappearing sectors are: Rest of the World; Government; Finance, Insurance, and Real Estate; and Services. The Rest of the World is a horse of a wholly different color. Government had to be dropped because our quaint

TABLE 2.—SHARE OF COMPENSATION OF EMPLOYEES IN INCOME ORIGINATING IN SELECTED SECTORS OF THE ECONOMY FOR SELECTED YEARS, 1929–1953

Sector	Weight	1929	1935	1937	1939	1941	1947	1951	1953	Variance
Agriculture, etc.	.113	.170	.134	.153	.185	.162	.170	.162	.206	.0004
Mining	.031	.751	.813	.715	.761	.705	.733	.704	.740	.0013
Contract Construction	.056	.667	.709	.704	.710	.733	.727	.759	.766	.0010
Manufacturing	.441	.742	.826	.787	.799	.690	.759	.711	.711	.0021
Wholesale and Retail Trade	.230	.702	.726	.691	.701	.624	.633	.650	.670	.0013
Transportation	.084	.725	.800	.812	.785	.717	.840	.805	.815	.0018
Communications and Public Utilities	.044	.541	.540	.560	.550	.543	.697	.619	.604	.0030
Total (Current Weights)		.647	.658	.656	.675	.613	.653	.631	.696	.0007
Fixed-Weight Total		.652	.702	.677	.688	.613	.666	.642	.678	.0008

accounting practices measure the value of its product by the compensation of its employees, so that by assumption no income is ever imputed to government-owned capital assets. I dropped the other two noncommodity-producing sectors on the grounds that the value-added concept is rather vague for them, and in many cases probably bears no remotely technological relation to conventional inputs. One could make a similar (but weaker) case for not including Trade, and one could argue that the imputation to wages in Agriculture may depend heavily on shifts between family and hired labor; but I have kept both in an effort to widen the coverage. The sector shares are shown for a selection of eight years between 1929 and 1953 but not for all. This was a perhaps unwise attempt to avoid the deep depression years and the war period.

[4] National income originating is a slightly more net concept than value added, since it excludes depreciation charges, indirect business taxes, and business transfers. Compensation of employees is the sum of wages, salaries, and the usual supplements. The figures no doubt exclude certain payments which logically ought to be imputed to labor, particularly part of the earnings of unincorporated enterprise. Cf. [3, p. 256] Probably the salary data also catch certain payments which function more like profits. I doubt that these "errors of observation" can influence the broad results substantially.

The table shows both the current-weighted over-all labor share and a fixed-weight series using the weights of 1941. In only one year does the use of fixed weights result in a change in the aggregate share of more than 2 percentage points, and the variability, as measured by the variance, is affected hardly at all. In part this is because the weights do not change radically, the main shift being a decrease in the relative weights of Agriculture and Transportation between 1929 and 1953, with Manufacturing gaining.

The last column shows the variance of each sector share and of the two aggregate-share series. The fixed-weight aggregate has a variance of .0008. If formula 1 is used to calculate a theoretical variance on the assumption that the sector shares moved independently in a statistical sense, it turns out to be .0005. This difference is almost certainly not statistically significant. We would have to conclude that the aggregate share varied just about as much as it would vary if the individual sector shares fluctuated independently, with positive and negative intercorrelations approximately offsetting each other. If anything, the aggregate share fluctuated a bit *more* than the hypothesis of independence would indicate. Anyone who believes that the aggregate share over this period was unexpectedly stable must believe the same of the sector shares and presumably seek the explanation there.

In Table 3, data from the Census of Manufacturing are analyzed in the same way. With the exception of 1941 and the substitution of 1954 for 1953, the same years are represented. Now the ratios give the share of wages only ("production workers' wages") in value added.[5] The fixed-weight average is calculated with weights equal to the 1947 fraction of each industry group in the aggregate value added. Once again the use of fixed weights makes only a negligible difference. In no year do the shares with fixed and current weights differ by as much as 1 per cent. The seven-year variance of the observed aggregate shares is .00028, and for the fixed-weight aggregate it is slightly increased to .00036.

But there is a striking difference between the behavior of the Manufacturing data and the wider Commerce figures. When a theoretical variance is calculated from formula 1, i.e., on the assumption that industry shares are statistically independent, it turns out to be only .00007. This is one-quarter of the observed share variance and one-fifth of the variance of the fixed-weight over-all share. And this substantial difference is in the "wrong" direction. The share of wages in manufacturing value-added fluctuates noticeably *more* than it would if the

[5] There are plenty of anomalies as between Table 2 and Table 3. Presumably they reflect the differences in concept between Census and Commerce data, as well as sheer observational error.

industry shares were mutually uncorrelated. This implies that there is predominantly positive intercorrelation among the wage shares in the separate industries. Instead of a special explanation of the relative stability of the over-all wage share in manufacturing, we appear to need just the reverse: an accounting for its tendency to fluctuate too much.

There are various ways of explaining the facts. Perhaps it is a fair

TABLE 3.—SHARE OF PRODUCTION WORKERS' WAGES IN VALUE ADDED, SELECTED MANUFACTURING INDUSTRY GROUPS, SELECTED YEARS, 1929–1934

Industry Group	Weight	1929	1935	1937	1939	1947	1951	1954	Variance
Food	.121	.268	.287	.291	.257	.285	.297	.281	.00019
Tobacco	.009	.238	.208	.215	.194	.273	.224	.222	.00064
Textile Mill	.072	.475	.575	.545	.499	.459	.540	.532	.00173
Apparel, etc.	.060	.355	.483	.483	.474	.454	.488	.490	.00233
Lumber	.034	.483	.541	.536	.502	.473	.493	.503	.00065
Furniture, etc.	.019	.422	.466	.470	.438	.475	.453	.454	.00020
Paper	.039	.359	.370	.360	.356	.352	.332	.362	.00014
Printing and Publishing	.057	.284	.287	.297	.279	.309	.339	.338	.00063
Chemicals	.072	.199	.206	.212	.189	.232	.212	.212	.00018
Petroleum and Coal	.027	.207	.237	.300	.256	.276	.265	.301	.00114
Rubber	.018	.385	.432	.465	.397	.472	.425	.407	.00109
Leather	.021	.464	.526	.528	.504	.473	.521	.509	.00066
Stone, Clay, Glass	.031	.417	.380	.389	.361	.431	.410	.392	.00056
Metals and Products	.158	.414	.450	.462	.427	.479	.424	.415	.00045
Nonelectrical Machinery	.105	.392	.446	.410	.380	.460	.438	.404	.00081
Electrical Machinery	.052	.341	.350	.369	.335	.423	.396	.357	.00109
Transportation Equipment	.079	.399	.497	.518	.494	.501	.477	.431	.00185
Miscellaneous	.028	.243	.370	.410	.372	.441	.434	.416	.00461
Total (Current Weights)		.358	.395	.402	.383	.407	.398	.382	.00028
Fixed-Weight Total		.357	.403	.406	.376	.408	.401	.389	.00036

idealization that the several industries buy their labor and capital inputs in the same or similar markets, so they can be imagined to face the same factor prices. If it is further assumed that each industry produces a single commodity with a technology describable by a smooth production function, then everything will depend on the distribution of elasticities of substitution among industries. If nearly all elasticities of substitution are on the same side of unity, then the wage shares will go up and down together in nearly all industries and there will be strong positive correlation. If elasticities of substitution are evenly divided on both sides of unity, there will be two groups of industries whose wage shares will move in opposed phase. Whether the net result is to increase

or reduce the variance of the aggregate wage share as compared with the hypothetical zero-correlation value will depend in a complicated way on the arrangement of weights and elasticities.

A special case occurs if each industry is imagined to produce a single commodity with a single fixed-proportions technique. Then every elasticity of substitution is zero and all wage shares move together. It is more interesting to recognize that each "industry" in Table 3 produces many commodities, some of which are complementary with each other in consumption and some of which are rival. Even if each commodity within an industry is produced by a single technique, it is no longer certain that the industry's wage share will rise and fall with the wage rate. The wage share for each commodity will rise with the wage rate, but those commodities whose production is labor-intensive will rise in price relative to others (assuming some degree of competition) and the intra-industry commodity-mix may shift in favor of capital-intensive commodities enough to decrease the wage share. The outcome depends in an easily calculable way on the factor proportions required by each technique and on the elasticities of substitution in consumption. If in addition commodities are producible with varying factor proportions, then once again the elasticities of substitution in production will play a role along with the other parameters [7, p. 8].

It must be admitted that none of this is very informative. It is all too static, too inattentive to technical change, too free with unknown and unknowable parameters—in a word, too neoclassical. It would be nice to have a single aggregative bulldozer principle with which to crash through the hedge of microeconomic interconnections and analogies. It is not inconceivable that the bulldozer may yet clank into view; but it is by no means inevitable either.

It is not clear how the newly popular widow's cruse theories (according to which the share of profits in income depends, given full employment, essentially on the rate of investment) can be made to apply on the somewhat disaggregative level to which my empirical results seem to force me. The stickiness of money wages, which forms the short-run side of Kaldor's theory [9, p. 95], may indeed have something to do with the results of Table 3, although that can hardly be the whole story. [The data next to be presented confirm the suggestion that Table 3's peculiarities are short-run in character.]

There are still other short-run facts that might help to explain the tendency of Table 3's industry shares to move together. An inclination to hoard skilled labor when output declines is one; the longer duration of collective bargaining agreements is another. In Table 4 the attempt is made to wash out some of the short-run effects by using decennial census data over a longer period of time. The layout is the same as that

of Table 3, but the coverage is necessarily poorer and the industrial breakdown cruder, because of changes in classification over the years. Broadly speaking, expectations are confirmed.

Once again, the use of fixed (1929) value-added weights results in only a slight increase in the variance of the aggregate wage share as compared with the observed totals. The variance of the observed totals is .0003, that of the fixed-weight totals is .0004. (Note that the difference between standard deviations, in natural units, is only the difference between .017 and .020.) Moreover, a good part of this small increase is due to the single very high observed wage share in transportation equip-

TABLE 4.—SHARE OF PRODUCTION WORKERS' WAGES IN VALUE ADDED, SELECTED MANUFACTURING INDUSTRY GROUPS, SELECTED YEARS, 1899–1951

Industry Group	Weight	1899	1909	1919	1929	1939	1951	Variance
Food	.121	.223	.212	.291	.268	.257	.297	.001
Textiles	.150	.462	.449	.368	.420	.488	.515	.002
Metals, etc.	.320	.453	.456	.476	.395	.400	.424	.001
Lumber	.073	.452	.488	.495	.465	.470	.480	.0002
Leather	.027	.532	.480	.405	.464	.504	.521	.002
Paper and Printing	.109	.357	.332	.331	.304	.304	.336	.0004
Chemicals	.064	.223	.216	.265	.119	.189	.212	.001
Stone, Clay, Glass	.038	.548	.543	.486	.417	.361	.410	.006
Tobacco	.014	.284	.288	.234	.238	.194	.224	.001
Transportation Equipment	.086	.671	.474	.440	.399	.494	.477	.009
Total (Current Weights)		.412	.389	.395	.368	.370	.400	.0003
Fixed-Weight Total		.424	.404	.404	.367	.384	.409	.0004

ment in 1899, together with the fact that the weight of this industry increased from 1899 to 1929. It seems just possible that the character of the output of the industry was changing around the turn of the century. Although this effect does not appear to be very strong in the data here analyzed, I suspect that analysis on a finer commodity classification might well show that shifts in the composition of output do have an effect in reducing fluctuations in aggregate shares.

The theoretical variance, calculated from formula 1 on the assumption of the independence of industry shares, is .00025. This is less than the observed figure of .00040, but probably not significantly so. (The standard deviations are .016 and .020.) In any case the wide discrepancy found in Table 3 has all but disappeared. This confirms the belief that the positive association of industry shares in Table 3 was essentially short-run in nature. For long periods in manufacturing, and even for short periods in the grosser sector breakdown of Table 2, the data are compatible with the hypothesis that subgroup shares fluctuate approximately independently through time; or, more accur-

ately, that positive and negative intercorrelations approximately cancel out.

In general, the data we have examined suggest the following: if by the "historical constancy" of labor's share it is meant that the share of the total social product imputed to wages has shown a marked absence of fluctuation as compared with the fluctuations of its industrial components, then this belief is probably wrong. Whatever exceptional stability there has been in the pattern of relative shares appears attributable to the components. This in turn suggests that there is no need for a special theory to explain how a number of unruly microeconomic markets are willy-nilly squeezed into a tight-fitting size .65 strait-jacket. A theory which wishes to produce the magic number among its consequences may have to say something about the component sectors among its premises.

III. *The Character of Trends*

There are still some interesting problems to be found among the sectors and in the aggregates. One such—and some economists would no doubt prefer to phrase the whole "historical constancy" question in these terms—is the mildness of the observable trends in the sector shares and in the aggregate relative shares. The history of western capitalism is supposed to be characterized by a long-run accumulation of capital relative to labor. We expect this trend to result in *some* trend in the distribution of the product. Why do we not observe a stronger one?

First, let us look at the orders of magnitude involved. No great accuracy is possible because of the difficulty of finding a reasonable measure of capital stock, because no two available time series are conceptually identical, and finally because of the imputation problem involved. Roughly speaking, during the first half of this century the capital/labor ratio for the private nonfarm sector rose by about 60 per cent. But most or all of the increase took place before 1929. Between 1929 and 1949 there was little change, possibly even a decline. In manufacturing the contours were broadly similar, although the initial increase in the capital/labor ratio during the period 1909-1929 was considerably greater.[6]

So far as distributive shares are concerned, it is generally accepted that there has been a slight tendency for the labor share to increase secularly. But before 1929 the trend was approximately horizontal[7]

[6] I am leaving aside the period since 1949, which saw a new burst of net capital formation together with an approximately normal growth of the labor force.

[7] See for instance S. Kuznets [13, p. 86]. D. Gale Johnson's calculations [8, p. 178] show the labor share rising from 69.4 per cent in the decade 1900-1909 to 75.2 per cent

(with some short-run movements); between 1929 and 1949 there is a more pronounced upward tilt in the wage and salary share as Table 1 shows.

What lends mystery to this picture is that in the first quarter-century, when capital accumulates much more rapidly than the labor force grows, the distributive share picture shows little or no trend. But in the second quarter-century, when the growth of capital relative to labor slows down or ceases, the wage share begins to rise. It seems likely that the difference between the two periods may be tied up with a slightly higher rate of technical progress in the years since 1929.

But let us accept the notion that economic history shows us a strong tendency for capital to grow relative to labor. We are then led to expect a strong trend in relative shares. But which way? The neoclassical answer is that this depends on "the" elasticity of substitution, or rather on the distribution of elasticities of substitution on either side of unity.[8]

Here we run up against the same kind of verbal question that occupied us earlier. What is a "strong" trend in relative shares? And what constitutes an elasticity of substitution "substantially" different from unity in terms of common-sense expectations? And how different from unity need the elasticity of substitution be in order that it convert a strong trend in the capital/labor ratio into a strong trend in relative shares? For the case of a two-factor, constant-returns-to-scale production function, it is not hard to calculate that the elasticity of the labor share with respect to the capital/labor ratio is $-S_K(1 - \frac{1}{\tau})$ where S_K is the share of property in income and τ is the elasticity of substitution. Is an elasticity of substitution of $\frac{2}{3}$ substantially different from unity? It means that a 10 per cent change in the relative costs of capital and labor services will induce a 6.7 per cent change in the capital/labor ratio. If $\tau = \frac{2}{3}$ and $S_K = .30$, the elasticity of the labor share with respect to the capital/labor ratio is .15. Thus if the capital/labor ratio rises by 60 per cent (with $\tau = \frac{2}{3}$) the labor share should rise by 9 per cent. And since the labor share hovers around .70, this means a rise of about 6 or 7 percentage points. But this is just the order of magnitude observed!

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for 1940-1949, with nearly all the change coming after 1915-1924. Johnson's figures are for the whole economy and include, besides the direct compensation of employees, an allowance for the labor content of entrepreneurial earnings. The corresponding figures for compensation of employees are 55 per cent and 64.3 per cent. When restricted to the private sector, compensation of employees amounts to 53 per cent of privately produced income in 1900-1909, and 59 per cent in 1940-1949. When the allowance for entrepreneurial earnings is made on the private sector basis the figures are 68 per cent and 71.5 per cent.

[8] Remember that shifts in the weights of different sectors in the total appear not to count for very much.

I don't mean to conclude from this example that yet another problem evaporates. But before deciding that observation contradicts expectation, there is some point in deciding what it is we expect. In this case what needs precision is the notion of substitutability, and the problem is complicated further by the need to consider changes occurring over varying periods of time.

There are even more fundamental obstacles to a clear evaluation of the argument about trends. An unknown fraction of society's capital takes the form of the improvement of human abilities and skills. Casual observation suggests that this fraction has been increasing over time. Correspondingly an unknown fraction of what we call wages, even "production workers' wages," no doubt constitutes a rent on that human capital. So the true quantitative picture is far from clear. If it were possible to separate out the part of nominal wages and salaries which is really a return on investment, the share of property income in the total might be found to be steadily increasing. An alternative way of looking at it is to say that investment in education, training, public health, etc., has the effect of increasing the efficiency of the human agent, so that a measurement in man-hours underestimates the rate at which the labor force grows as properly measured in efficiency units. In this case it might be found that the accumulation of nonhuman capital does not proceed at a faster rate than the labor force grows. These are intrinsically difficult distinctions to draw empirically, but they hold much theoretical and practical importance.

There are of course still other discrepancies between the data we have and the analytical concepts to which we pretend they correspond. The problem of imputing to labor a proper share of the income of unincorporated enterprises has received some attention. But even in the corporate sector possibilities exist for converting what is "really" property income into nominal labor income, and vice versa, and there are often tax reasons for doing so. If this were a random effect in time it would do no great harm, but in fact it may behave more systematically than that.[9]

To complete the catalog of uncertainties about trends, I ought to mention the intrusion of technical change between the simple facts of factor ratios and factor rewards. About the incidence of historical changes in techniques little is known, and without this it is difficult to know what residual remains to be accounted for.

[9] Johnson [8, pp. 180-82] shows that some part of the apparent increase in the labor share is to be attributed to such statistical artifacts as the growing importance of government-produced income, all of which is conventionally imputed to labor, and the declining importance of agriculture and therewith of home-produced and home-consumed goods.

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