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A Survey of Some Theories of Income Distribution

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THE theory of income distribution is in a highly unsatisfactory and controversial state. Further thinking on the subject can be facilitated by a survey that does the tedious but necessary preliminary work of reviewing the field, putting it into some kind of order, and pointing out the more obvious strengths and flaws, connections and inconsistencies. It is in this spirit that the following comments are offered.

There are at least four possible subjects that a theory of income distribution could cover: first, the level and changes in the level of incomes earned in particular occupations; second, the distribution and changes in the distribution of personal incomes by size; third, the functional distribution of income among the owners of the different productive factors; and fourth, the relative size and changes in the relative size of the various components of the official personal income accounts.

The first, being the least ambitious, is probably also the most promising, but so little work has been done in this area that there is virtually nothing to review. On the second subject, the most important work, Champernowne's,[1] is beyond my grasp, and apart from that very little has been done. The available data cover too short a period of time to base theories on, and we have not yet progressed beyond the simple notions of the classical economists who looked to death duties and increasing education to diminish inequalities of income. The effectiveness of this last factor has recently been questioned,[2] but there is still plenty of scope for more work on this subject. Another equally obvious influence on income distribution by size has never been mentioned and may well be worth looking into. There is evidence of a secular trend toward increasing centralization and an increasingly large-scale organization of economic, social, and political life; this means a changing pattern of demand for people, with fewer positions available in the higher and more in the lower echelons. The demand for skilled people has declined relative

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[1] D. G. Champernowne, "The Graduation of Income Distributions," *Econometrica*, October 1952, and "A Model of Income Distribution," *Economic Journal*, June 1953.

[2] H. P. Miller, "Income in Relation to Education," *American Economic Review*, December 1960.

to that for the unskilled, as has the demand for executives relative to that for clerks and for generals relative to privates. Automation may well arrest or reverse this trend in the future, but in the past such changes in the pattern of demand may well have exerted on income distribution an equalizing force no less important than the influences exerted on the supply side. The rising interest in the economic returns to education, viewed as investment in human resources, is likely to stimulate work on this aspect of income distribution; but so far there is little or nothing to report on.

Whereas on the first two subjects there has been too little theorizing, on the third—the functional distribution of income—there are a large number of theories, using a variety of approaches and explaining a variety of phenomena; and hence it is in this area that there is the greatest scope as well as the greatest need for a survey. Almost all of this work is concerned with the twofold division between the share of labor in national income and the share of all other factors, loosely called capital. The classical system of three factors of production has been abandoned, largely because such categories as rent, interest, and profit in the national income accounts bear little relation and cannot easily be made to bear a meaningful relation to the factors land and capital. This is also the reason why the fourth subject—distribution by components of the national income accounts—has received little attention and tends to merge with the third subject.

The numerous theories of the division of income between labor and capital can be classified according to their approach and according to what they are trying to explain. As to the former, the distributive shares depend partly on the behavior of individual decision-makers, entrepreneurs, and consumers, and partly on the magnitude and pattern of demand and the relative supply of the different factors. The theories that center attention on the individual decision-maker's behavior are known as microeconomic theories; those that explain shifts in the distributive shares by changes in demand, supply, or institutional factors we shall call macroeconomic explanations. Those that treat demand and the supplies of factors as endogenous variables, both determined by and determining the distributive shares, we propose to call macroeconomic theories.

As to the second method of classification, we shall distinguish

between theories that explain the constancy and those that account for changes in the share of labor in national income. This, obviously, is not an exhaustive classification, but it covers most theories and all those we propose to deal with. Our discussion begins with the second group of theories, all of which happen to be macroeconomic explanations.

Short-Run Macroeconomic Explanations

The U.S. data show a pronounced anticyclical movement in labor's share of national income according to almost any of the customary U.S. definitions of labor's share—i.e., wages and salaries, employee compensation, or this plus some part of the earnings of the self-employed.[3] The U.K. data show less regularity, primarily, I believe, because labor's share traditionally means wages only; and whereas the share of wages in wages and profits moves *against* the cycle, the share in total income (which also comprises salaries, rent, and interest) of wages and profits combined moves *with* the cycle, and these two contrary tendencies are mutually offsetting in their influence on the share of wages in national income.[4]

The main explanation of the anticyclical behavior of labor's share (according to the U.S. definition) is, I believe, what Burkhead calls the capacity effect.[5] The greater the utilization of capacity, the larger the output over which fixed costs are spread and the smaller therefore the fixed costs per unit of output. The importance of this effect is probably increasing with the secularly increasing share of fixed in total costs and of supervisory and administrative workers in the total labor force.[6]

A second explanation of this anticyclical behavior is what some writers call the lag effect,[7] i.e., the alleged lag of wages behind price increases in times of prosperity and inflationary pressures.

[3] James C. Beck, "Labor's Share and the Degree of Utilization of Capacity," *Southern Economic Journal*, April 1956; Jesse Burkhead, "Changes in the Functional Distribution of Income," *Journal of the American Statistical Association*, June 1953; Edward F. Denison, "Distribution of National Income since 1929," *Survey of Current Business*, June 1952; Joseph Phillips, "Labor's Share and 'Wage Parity,'" *Review of Economics and Statistics*, May 1960.

[4] E. H. Phelps Brown and P. E. Hart, "The Share of Wages in National Income," *Economic Journal*, June 1952, pp. 253–277.

[5] Burkhead, *JASA*, June 1953, p. 209.

[6] Charles Schultze, *Recent Inflation in the United States*, Chap. 4, Study Paper No. 1, Joint Economic Committee, 86th Congress, 2nd Session, Washington, 1959.

[7] Burkhead, *JASA*, June 1953, pp. 209–210.

That such a lag exists has for a long time been assumed by most economists, on the basis partly of a priori reasoning and partly of the evidence of the more spectacular hyperinflations of history.[8] All the recent evidence, however, seems to go counter to this notion and to show that prolonged inflation other than hyperinflation does not diminish labor's share in the national income, except in some cases of repressed inflation.[9] This need not imply that a wage lag and consequent income redistribution cannot and do not exist in the short run, at the peak of cyclical prosperity, during temporary bottlenecks and inflationary pressures. This subject, however, needs further investigation and one must keep an open mind in the interim.

A third explanation given by Burkhead is the compounding effect:[10] the cyclical shift to profits is also a shift to saving, and the capitalists' higher savings give rise to higher incomes. I find this explanation unconvincing, since the earnings of capital move presumably with the stock of capital rather than its rate of accumulation.

A fourth explanation, which seems rather obvious but is not mentioned anywhere, might have to do with the composition of output. Cyclical fluctuations in demand and hence output are fluctuations not only in magnitude but also in composition. In times of prosperity, demand and output are not only higher but also different in composition, with investment goods and consumer durables and luxuries accounting for a larger share of the total. If labor's share in the income generated by these industries is different from what it is in the rest of the economy, then their increased importance will also lead to a redistribution of income.[11]

We must also mention here the so-called Keynesian theory

[8] C. Bresciani-Turroni, *The Economics of Inflation*, London, 1937, especially Chap. 8.

[9] G. L. Bach and A. Ando, "The Redistributional Effects of Inflation," *Review of Economics and Statistics*, February 1957; *Economic Survey of Europe in 1956*, Geneva, United Nations, 1957, Chaps. 8 and 9; E. H. Phelps Brown and M. H. Browne, "Distribution and Productivity under Inflation, 1947–57," *Economic Journal*, December 1960.

[10] Burkhead, *JASA*, June 1953, pp. 209–210.

[11] This factor, however, may well pull in the opposite direction. A rough calculation based on recent U.S. data shows that in investment goods industries (construction and producer durables) the share of employee compensation in total income generated was 75 per cent, higher than the 69 per cent average for the economy as a whole. The inclusion of consumer durables and luxuries is likely to diminish the disparity but not to reverse it.

of distribution, the rudiments of which are contained in the *Treatise on Money* and which has been further developed by Boulding, Hahn, Kaldor, Kalecki, and Robinson.[12] This is an implicit theory, which links investment and income distribution by analyzing the latter's effect on the community's propensity to save, postulating the equality of saving and investment as an equilibrium condition, and tacitly taking for granted that a rise in investment will somehow redistribute income in favor of capital. It has been criticized, even ridiculed, for taking for granted what a proper theory of income distribution ought to explain explicitly;[13] it may be fairer, however, and more fruitful to inquire into the causal factors that it takes for granted. The theory is supposed to apply equally to underemployment and full-employment situations (though Kaldor and Robinson would restrict it to the latter); at least as far as the former are concerned, the U.S. data certainly bear out the theory, and the macroeconomic explanations just discussed can serve as the causal factors implicit in it. If labor's share in national income is inversely correlated with the level of national income within the business cycle, it must be similarly correlated—*and for the same reasons*— with the share of net investment in net national income, which we know to be directly correlated with the level of income. One thinks first of the capacity effect as the most obvious reason why a rise in investment should, through its stimulating effect on the level of income and capacity utilization, reduce the relative share of labor; and the lag effect seems a possible further explanation too.

As to the validity of the theory in full-employment situations, I know of no statistical evidence; but if the capacity effect and the lag effect adequately explain a fall in labor's share during cyclical upswings of falling unemployment, they should also explain a fall in labor's share resulting from rising investment at times when the labor force is fully employed. This is certainly so when a rise in investment at a time of full employment implies

[12] K. E. Boulding, *A Reconstruction of Economics*, New York, 1950, Chap. 14; F. Hahn, "The Share of Wages in the National Income," *Oxford Economic Papers*, June 1951; N. Kaldor, "Alternative Theories of Distribution," *Review of Economic Studies*, No. 61, 1955–56; M. Kalecki, "A Theory of Profits," *Economic Journal*, June-September, 1942, pp. 258–261; J. Robinson, *The Accumulation of Capital*, London, 1956.

[13] J. Tobin, "Towards a General Kaldorian Theory of Distribution," *Review of Economics and Statistics*, February 1960.

an increase in hours worked and an expansion of the labor force. The argument becomes more complex when the rise in investment involves a transfer of resources from other sectors of the economy; but the capacity effect and the lag effect are operative and may exert the dominant influence even then. Some adherents of the Keynesian theory seem aware of its limitations (Robinson's "inflation barrier" and Kaldor's "subsistence minimum" indicate some distrust of the lag effect), and within such limits its validity should not be denied merely because one is annoyed by the way in which it is stated.

Long-Run Macroeconomic Explanations

In addition to countercyclical changes, the U.S. data also show a secular increase in labor's share of the national income; all the economists who have worked with these data have sought and found special explanations to account for this trend.[14] One of these is the increasing importance of the government contribution to the national product, since, according to present accounting practice, this consists solely of employee compensation. Another explanation is the secularly diminishing importance of agriculture, where labor's share in value added is especially low and where, in addition, labor's real income is understated in view of the lower-than-average prices paid by farm workers. A third explanation is the secularly rising proportion of wage and salary earners and the diminishing proportion of small (i.e., unincorporated) businessmen in the total labor force.

It is to isolate and eliminate the effect of this last factor that it has become customary in the United States to define the labor share or service share as employee compensation plus all or part of the earnings of unincorporated business, regarded as "entrepreneurial labor income."[15] The effect of the diminishing importance of agriculture is isolated by comparing the actual share of labor to what it would be if the weights of the different sectors had remained constant;[16] the effect of government's in-

[14] Denison, *Survey of Current Business*, June 1952; Phillips, *RES*, May 1960; Dale H. Johnson, "The Functional Distribution of Income in the United States, 1850–1952," *Review of Economics and Statistics*, May 1954, pp. 175–182; I. B. Kravis, "Relative Income Shares in Fact and Theory," *American Economic Review*, December 1959, pp. 917–949.

[15] Johnson, *RES*, May 1954; Kravis, *AER*, December 1959; Burkhead, *JASA*, June 1953; Edward C. Budd, "Factor Shares, 1850–1910," in *Trends in the American Economy in the Nineteenth Century*, Studies in Income and Wealth, 24, Princeton for NBER, 1960.

[16] Johnson, *RES*, May 1954, p. 180; Kravis, *AER*, December 1959, pp. 934–935.

creasing importance is eliminated by excluding the government contribution from both national income and labor's share.[17]

There are a number of other factors as well, such as the influence of war and the great depression on the decade estimates and the change in the source and nature of the data at the very date (1929) when they seem to show the greatest change in labor's share. We shall pay no more attention to these factors, however, because the first three explanations between them deal, to the satisfaction of most writers on the subject, with just about all the secular change in labor's share of the national income in the United States.[18]

These conclusions are at least as interesting for what they imply as for what they actually say. Economists who try to explain changes in distributive shares by invoking special factors and particular trends seem to believe in the constancy of distributive shares, which would obtain if it were not for these special factors. And if they succeed in explaining fully the changes in relative factor shares by these special factors, then they have come close to proving indirectly the existence of other factors at work which tend to maintain the stability of distributive shares. Further support for this line of reasoning comes from a comparison of the American and the British data. The latter show that the share in the national income of wages alone was virtually constant over the period 1870–1950, with swings no greater than 8 per cent away from the average.[19] When one recalls that in Britain agriculture was much less important throughout this period, and that the other two factors—the increasing importance of government and the rising proportion of employees—both affect salaries and not wages, then the British findings appear to confirm rather than to contradict the U.S. data; and they strengthen the feeling that there must be other factors making for the constancy of distributive shares.[20] Some people may also wish to attach importance to the fact that all the factors invoked to explain the secular change in

[17] Denison, *Survey of Current Business*, June 1952; Phillips, *RES*, May 1960, pp. 177 ff.; Kravis, *AER*, December 1959, p. 927; George J. Schuller, "The Secular Trend in Income Distribution by Type, 1869–1948: A Preliminary Estimate," *Review of Economics and Statistics*, November 1953; Johnson, *RES*, May 1954, pp. 179–180.

[18] Kravis alone feels differently and, for an earlier period, Budd. Unfortunately, the latter's paper reached me too late for more than a cursory glance.

[19] Phelps Brown and Hart, *Economic Journal*, June 1952.

[20] It is worth noting in this connection that Budd, dealing with U.S. data for 1850–1910, finds a greater stability in the share of wages than in that of wages and salaries or of services.

distributive shares in the United States are macroeconomic. While this proves nothing, it might rule out *some* macroeconomic explanations of constancy and suggest that we should at least begin our search for the explanation of the constancy of distributive shares among the microeconomic factors.

Microeconomic Theories

The first of these is the marginal productivity theory of distribution. It is worth recalling that parts of this theory were, to some extent, originally developed to provide a rebuttal of Marx's theory of exploitation and an ethical basis for the distribution of income in a free enterprise economy. This explains the argument, on the assumption of perfect competition, that each factor's rate of remuneration equals the value of its marginal contribution to output, and the related argument, based on the assumption of long-run competitive equilibrium, that the capitalist gains nothing from being in the seemingly advantageous bargaining position of the entrepreneur who does the hiring and firing.[21]

Today, we no longer seek ethical content in economic theories, and many of us have become reluctant to assume perfect competition and long-run equilibrium; nevertheless, most people still adhere to the marginal productivity theory. For one thing, its generality and elegance has considerable appeal; for another, it fits in best with our marginalist approach to economics; for a third, the acceptance of the marginal productivity theory of income distribution is closely bound up with the assumption of an aggregate production function whose analytic convenience has enticed many economists to slur over or disregard the objections to it. Its modern form, however, is a very much watered-down version of the neoclassical marginal productivity theory and amounts to little more than a market theory of income distribution. By this we mean a theory that stresses merely that the rates of remuneration of productive factors are market prices determined by supply and demand, and influencing in turn the quantities and proportions in which individual entrepreneurs demand the services of the productive factors.

The main purpose to which the theory is put nowadays is to explain the relative stability of factor shares. Given a market the-

[21] I am here referring, of course, to the use of Euler's theorem for solving the adding-up problem.

ory of income distribution, it is enough for demand curves to slope downward and for the main disturbances to come from the supply side in order to assure contrary and hence offsetting movements in factor supplies and factor prices. These, in turn, assure the relative stability of factor shares, in the sense that factor shares in total income fluctuate relatively less than either factor supplies or factor prices. That such stability exists becomes obvious on considering that the supply of reproducible capital has increased twice as fast as the supply of labor;[22] and there is no need at this stage to enter the controversy over how constant something must be to be called constant. Those who believe in the constancy of relative factor shares have further assumed unit or near-unit elasticity of substitution between labor and capital, and also neutral technical progress—this latter to explain the constancy of shares even in the face of some disturbances (technical progress) coming from the demand side. These assumptions, however, are not an integral part of the marginal productivity or market theory of income distribution and may be left aside while we analyze the theory.

The theory that rates of factor remuneration are and behave like market prices implies the assertions, first, that the quantities and proportions in which the services of the productive factors are demanded depend on relative prices; and second, that total demand for productive services influences their prices. We propose to offer some critical comments on both these assertions.

As to the first, factor substitution in response to a change in relative factor prices can be direct and indirect. Direct substitution is made by entrepreneurs; and it is well known, though not stressed often enough, that the scope for this is extremely limited. The proportions in which the entrepreneur combines labor and capital in his day-to-day production decisions are largely determined by the nature of his productive equipment and methods, which have been fixed for the useful lifetime of his plant at the time when he built his factory and bought his equipment. Changing relative prices influence the entrepreneur's factor proportions mainly through their impact on his investment decisions; and these affect factor proportions only in a small fraction of the total productive system.[23] The manufacturing capacity created

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[22] Kravis, *AER*, December 1959, p. 918.

[23] The fact that disused capacity can be brought back into production does not really modify this statement. For the change in factor proportions that

by gross investment in the average year is only about one-tenth of total manufacturing capacity.

It is also worth asking exactly how a change in prices influences the proportions in which labor and capital are combined in investment projects. The standard argument is that a rise in wages relative to product prices will lower the profitability of investment on *all investment projects*, but to a lesser extent on the more capital-intensive ones, which is why demand will shift toward capital. The tacit assumption underlying the argument is that the total volume of investment remains unchanged despite the reduced profitability of all investment (and despite the higher wage level, which may affect total consumption), and that total income and employment also remain unchanged. When this assumption is false, the effects of changed levels of investment and income on the nature of investment and on income distribution must also be taken into account; and these effects may modify or change the argument. Because the scope for direct factor substitution by entrepreneurs is so limited, economists have stressed the importance of indirect factor substitution by consumers. A rise in the relative price of labor raises the prices of labor-intensive goods relative to those of capital-intensive goods; this prompts consumers to shift their demand from the former to the latter, thereby indirectly bringing about a shift in demand from labor to capital. The argument is impeccable, but its importance must be judged by empirical results. For indirect factor substitution by consumers, in contrast to direct factor substitution by entrepreneurs, exerts its influence on factor shares through interindustry shifts, and these should be separable from the forces that operate within an industry. Suggestive in this connection is Solow's finding that interindustry and intersector shifts in demand and output have had no stabilizing effect on labor's share in aggregate income;[24] but it

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this implies is usually incidental and comes about in response to a rise in demand, whichever way relative factor prices have shifted. With demand unchanged, relative factor prices would have to change drastically indeed to bring disused capacity back into use.

[24] Robert M. Solow, "A Skeptical Note on the Constancy of Relative Shares," *American Economic Review*, September 1958, pp. 618–631. Solow presents his findings as an argument against relying on macroeconomic explanations of the stability of factor shares. I am not familiar, however, with any macroeconomic theory that would invoke interindustry shifts as a stabilizing factor, except for the argument of this paragraph, which is an integral part of the marginal productivity or market theory of income distribution.

is only suggestive, partly because his data relate to industries and sectors too broadly defined to have too much bearing on the present argument, and partly also because his conclusions, even if based on a finer industry classification, would prove the unimportance of indirect factor substitution by consumers only in the (to my mind likely) case that the elasticity of direct and indirect substitution combined should be nearer unity than the elasticity of direct substitution alone.[25]

We can now proceed to examine the second assertion, which is that the change in factor proportions wrought by individual decision-makers in response to a change in relative prices will change total market demand for the different factors and thus influence, in its turn, relative factor prices. Two observations need to be made in this connection. One is that this part of the mechanism will operate only at high activity levels, for it is difficult to imagine the forces of demand and supply in factor markets influencing prices at times when the labor force, existing plant capacity, and the potential supply of savings are all under-utilized.

Our other observation is that to analyze the effectiveness of this part of the mechanism would require a detailed comparative study of price and wage behavior both between the capital goods and other sectors and between periods when plant capacity is more fully utilized than the labor force and periods when the reverse is true. Some such studies are being made as part of the current analysis of inflation, an important by-product of which may well be additional information on and insight into the workings of factor markets.

To sum up, our criticism of the market theory of income distribution is that it is based too little on direct evidence about how factor markets operate and too much on analogy with the very much simpler operation of market supply and demand and substitution in markets for consumer goods. We do not deny that market forces operate in factor markets; but they may be much weaker and much more sluggish than is generally supposed; and for all we know they may, in some respects, operate quite differently from those that operate in the market for bread.

At best the market theory explains not constancy but merely

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[25] I am indebted to Professor M. Reder for cautioning me against reading too much into Solow's elegant argument.

a fair degree of stability in factor shares, and even that only in the face of disturbances coming from the supply side. To explain the constancy of relative factor shares would require unit elasticity of factor substitution. There is quite a literature, to which we can only refer here, to warn against accepting unit elasticity on the evidence that the Cobb-Douglas function gives a good fit to time-series data.[26] On the other hand, several people have pointed out recently that what constancy there is in relative factor shares in the United States is compatible not only with unit elasticity but also with a whole range of values of the elasticity of substitution if this is not too far removed from unity, and especially if it is above unity.[27] The first reaction to this argument is to point out that the elasticity of substitution would be expected to be below rather than above unity,[28] and to ask what is "not too far removed." We have neither statistical estimates of the elasticity of substitution nor intuition to tell us what are reasonable or likely values.

As to disturbances emanating from the demand side, the discussion in the previous two sections has shown that factor shares *are* affected by quite a variety of changes that emanate from the demand side; there is only one such change, though a major one, that seems to have left relative shares unaffected—technological progress. The kind of progress that leaves relative factor shares unchanged has been defined as neutral; and the fact that progress in our economy has been so close to neutral needs an explanation and can hardly be regarded as an accident of history. No satisfactory explanation has been offered as yet by the upholders of the marginal productivity theory; some tentative explanations offered by others will be discussed in the next section.

A second microeconomic theory of income distribution is Kalecki's theory.[29] This is a first cousin of the marginal pro-

[26] E. H. Phelps Brown, "The Meaning of the Fitted Cobb-Douglas Function," *Quarterly Journal of Economics*, November 1957; and some of the literature referred to there.

[27] M. Bronfenbrenner, "A Note on Relative Shares and the Elasticity of Substitution," *Journal of Political Economy*, June 1960; also Solow, *AER*, September 1958, p. 629; and Kravis, *AER*, December 1959, p. 940.

[28] After all, the relative supply of labor has been diminishing and its relative share has, if anything, increased.

[29] M. Kalecki, "The Determinants of Distribution of the National Income," *Econometrica*, April 1938, reprinted with significant changes in his *Essays in the Theory of Economic Fluctuations*, London, 1939, and also in his *Theory of Economic Dynamics*, London, 1954.

ductivity theory, from which it differs, in a formal sense, by dropping the assumptions of perfect competition and long-run equilibrium, and making instead the simplifying assumption of constant returns to scale (horizontal marginal and average direct cost curves)—at least within the range of underemployment, which is Kalecki's sole concern. The main determinant of income distribution in his theory is the percentage gross profit margin entrepreneurs add to marginal or average direct cost in setting their prices, and Kalecki regards this as an index of the degree of monopoly. In early versions of his theory, this is determined by (and stands in a definite relation to) the price elasticity of demand facing the entrepreneur; in the last version, the degree of monopoly is more generally defined in order to include oligopoly as well.

The great merit of Kalecki's theory is that it relates income distribution to the entrepreneur's pricing policy and makes the stability of distributive shares partly dependent on the stability of entrepreneurial profit margins. This means that it is a short-run theory, which has to do with the firm's day-to-day pricing decisions. Thus it contrasts with (and perhaps complements) the market theory, which is a long-run theory and mainly has to do with the entrepreneur's investment decisions and very little with his day-to-day decisions on the relation of prices to wages and the determination of output in the face of given prices and wages. Kalecki's theory suffers from the lack of a satisfactory and integrated theory of monopolistic and oligopolistic competition; and it may well be that a fully satisfactory explanation of distributive shares will have to await the development of such a theory. His simplifying assumption of horizontal marginal cost curves has been justly criticized;[30] but its abandonment only complicates without destroying his theory. It should also be stressed that the theory explains distributive shares in the gross product, not in net income, and hence allows for cyclical fluctuations in the *income* shares; that labor's share in the theory means the share of wages only and excludes salaries that do not enter prime costs; and that vertical differentiation also plays an important role. Kalecki himself believes in a secular increase in the degree of concentration and monopoly and provides a somewhat unconvincing

[30] Melvin W. Reder, "Rehabilitation of Partial Equilibrium Theory," *American Economic Review*, May 1952, pp. 191–192.

argument for reconciling this with the constancy of distributive shares; but others, testing his model, have found the degree of monopoly to be constant.[31] Further testing of his model should investigate the importance and implications of the alleged secular shift from wage-earning production workers to salaried supervisory and administrative employees. With Kalecki's theory linking distributive shares to the firm's pricing policy and the market theory linking it to the firm's investment decisions, one should hope that future work on the theory of investment by the firm, and on the influence of the firm's monopoly power and pricing policy on its investment decisions, will provide the means to integrate these two microeconomic theories of income distribution.

Macroeconomic Theories

The classical example of this group of theories is Ricardo's subsistence theory of wages. This asserts that the wage rate always tends toward the subsistence level, because a higher wage will raise the birth rate and a lower one will raise the death rate of workers, thereby increasing or diminishing the supply of labor and thus depressing or raising wages until they return to the subsistence level and equilibrium in the supply of labor is restored.

The beginnings of a modern macroeconomic theory of income distribution can be found in—or read into—Hicks' *Theory of Wages*, where he introduces the idea of induced technical change in an attempt to account for the stability of factor shares. He knew that the market theory of distribution explains the relative stability of factor shares only in the face of disturbances emanating from the supply side, and even then only when the elasticity of substitution is near unity. This accounts for his attempt to make the nature of innovation depend on economic factors, but his argument is hard to follow and incomplete. It is similar, however, to part of the theory to be discussed presently and will be referred to below in that connection.

Another macroeconomic theory, and perhaps the most satisfactory one, can be pieced together from an article by E. H. Phelps Brown and B. Weber and from several papers by N. Kaldor.[32] The first two authors have shown that in England not

[31] Ashok Mitra, *The Share of Wages in National Income*, The Hague, 1954.
[32] E. H. Phelps Brown and Bernard Weber, "Accumulation, Productivity and Distribution in the British Economy, 1870–1938," *Economic Journal*, June 1953,

only the share of capital in income but also the rate of return on capital invested and the level of output per unit of capital have all shown a remarkable constancy over time.   Any two of these imply a third, so that if the constancy of two is explained, the constancy of the third follows.   Accordingly, Phelps Brown and Weber, in a tentative theoretical explanation of their findings, argue the existence of economic forces that keep stable the first and the third relations mentioned above.   Kaldor, in his turn, explains the same findings in terms of economic forces stabilizing the first and second relations and obtains the stability of the third as a consequence.   We shall argue presently that while neither approach is fully convincing, the two combined provide a better theory than either of them singly.

Phelps Brown and Weber explain the stability of the first relation—the share of capital in total income—by a propensity of businessmen to maintain, at least in the short run, a fixed proportional relation between prices and direct costs.   In other words, they accept Kalecki's theory in its simplest form, finding statistical confirmation of this constancy by Phelps Brown and Hart.[33]   Kaldor relies on essentially the same argument, although his explanation is very differently worded.

The existence of forces stabilizing the second relation—the rate of return on capital invested—is developed in detail by Kaldor along lines reminiscent of Ricardo's subsistence theory of wages.[34] He argues that the expected rate of earnings on capital must exceed the market rate of interest by a minimum margin in order to repay the entrepreneur for his risk and trouble; that when expected margins exceed this minimum, the resulting faster rate of capital accumulation will soon depress rates of return and bring expected margins closer to the minimum; whereas if expected margins fall below the minimum, the consequent choking off of capital accumulation will soon raise the margin again if population continues to grow and technology to progress.   Kaldor's argument explains

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pp. 263–288; N. Kaldor, "A Model of Economic Growth," *Economic Journal,* December 1957, pp. 591-624; Kaldor, "Economic Growth and the Problem of Inflation," *Economica,* August 1959, pp. 212–226 and 287–298; Kaldor, "Capital Accumulation and Economic Growth," in *The Theory of Capital,* New York, 1961.

[33] Phelps Brown and Hart, *Economic Journal,* June 1952, pp. 267–268.

[34] Phelps Brown and Weber also mention this as a possibility but do not elaborate it.

a long-run stability not so much of the rate of return on capital as of the margin between this rate and the market rate of interest on riskless securities; but in view of the latter's historical behavior, Kaldor's theorem fits Phelps Brown's data very well. It might be added that there is nothing marginal about Kaldor's expected future rate of profit on capital invested, since this is based on the past actual rate of profit, which in turn is the ratio of profit per output (the net profit margin) to capital per output.

The long-run stability of the third relation, the capital-output ratio, is again argued by Phelps Brown and Weber, or rather they outline and refer to an argument found in more detail elsewhere. As is well known, when the return on capital is constant, a proportional rise in output and the stock of capital means neutral technical progress. And neutral technical progress can be equated, as a rough first approximation, to parallel productivity increases in consumer goods and capital goods industries; for productivity increases in the former are labor-saving, those in the latter capital-saving improvements. Phelps Brown and Weber expect the neutrality of technical progress to be assured by the forces that tend to keep productivity increases in these two groups of industries parallel. These forces have been better described in the literature on economic development, especially by the critics of the doctrine of balanced growth.[35] A productivity increase in one industry generates pressures on other industries to raise their productivity too. These pressures take a variety of forms: the squeezing of profits in competing and the creation of bottlenecks in related and complementary industries. An earlier generation of theorists would have concluded that a change in relative prices would lead to a gradual shrinking of the former and expansion of the latter industries; but a closer look at the historical evidence (and perhaps also a new fashion in theorizing) suggests that this will only happen as a last resort. Established firms will do their utmost to improve productivity and lower costs before accepting the death sentence of dwindling profits; and bottlenecks are usually broken not by a gradual expansion of the factor in short supply but by a technical breakthrough that dispenses with or greatly reduces the need for this factor. There

[35] Albert O. Hirschman, *The Strategy of Economic Development*, New Haven, 1958, especially Chap. 4; also my "Growth—Balanced or Unbalanced?" in M. Abramovitz *et al.*, *The Allocation of Economic Resources*, Stanford, 1959.

is mounting evidence that one innovation leads to another through economic pressure as much as through the similarity of technical conditions and problems; and if all this leads to parallel increases in labor productivity, it also assures neutral technical progress. Needless to say, this is no exact equilibrating mechanism but a very rough and approximate equalizing force.

As already mentioned, Phelps Brown and Weber rely on this last and on the first equilibrating force to account for the stability of the three relations; Kaldor relies on the first two and deduces the neutrality of technical progress as an indirect consequence.[36] There is no need, however, to choose between these two explanations. If any two of the three equilibrating forces were strong, the third would indeed be unnecessary for explaining the stability of the three relations; but this is not the case. Parallel increases of productivity in different industries are at best a very long-run and approximate tendency; and equally long run is the tendency of capital accumulation to influence the profitability of capital. By contrast, the entrepreneur's tendency to maintain a stable relation between prices and direct costs is a short-run stabilizing influence, which may be subject to secular change. The three stabilizing forces therefore should be recognized as mutually reinforcing, and the theories of Kaldor and Phelps Brown and Weber as complementary. They need to be developed further and restated more carefully but are nevertheless the most, perhaps the only, satisfactory macroeconomic theory of income distribution. It should be noted in closing that this theory does not hinge on interindustry shifts, is therefore not subject to Solow's criticism,[37] and can accommodate the changes in income distribution brought about by interindustry shifts, which are discussed among the macroeconomic explanations.

COMMENT

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Professor Scitovsky provides a short survey of distribution theories that I found useful, enlightening, thought-provoking, and,

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[36] The most nearly complete and satisfactory statement of Kaldor's theory is contained in the unpublished paper mentioned in footnote 32. His presentation is more suggestive and persuasive than this account, owing primarily to the use of his "investment function" as an explanatory device.

[37] Cf. footnote 24.

happily, presented without a single mathematical symbol. For this I salute him.

Insofar as Scitovsky is simply reporting the views of others, he must, in all fairness, be considered immune to criticism. However, he also classifies the various theories, and points out their most important strengths and flaws, connections and inconsistencies. Here he is fair game and if we can use the pretext of his comments to strike a blow at any of the authors he cites, so much the better. In the latter part of my comment I shall attempt to do so.

Scitovsky devotes almost all of his paper to the functional distribution of income. However, he gives only the barest mention to the distribution of national income by type of income, as published by the Commerce Department. This series reports the form in which income initially accrues to suppliers of the factors of production, before government steps in to take and give via taxes and transfer payments. This is one of the most interesting distributions, and not only because we have some data that we can try to explain. It is, after all, the distribution that most of the shouting is about. The public outcry is not over the functional distribution as economists define it but about how the income of farm proprietors is too low, or corporate profits have been squeezed, or are too big, and so on. Work by Charles Schultze, among others, has shown how illuminating analysis based on this classification can be in examining business fluctuations. I suspect it would also be useful to try to bring the tools of economic theory and empirical investigation to bear upon the reasons that this distribution is what it is, and has changed as it has over longer periods. Those who think the division of income between corporations and persons has something to do with the saving rate, and this includes most economists, should be interested in such a study. Some work has been done for individual shares, such as the income of farm proprietors and independent professionals, but not, I think, comprehensively. Any general theory would have to include both the determinants of the various legal forms of organization and the composition of the resources going into the mixed shares in terms of the classifications used in economic theory.

The theories that Scitovsky does discuss, insofar as they rely on data at all, reverse this process. Instead of trying to develop a theory to explain the data, they try to adjust the data to a classification that theory can explain, or restrict themselves to only a

portion of the economy where statistics and the shares of theory correspond, or else slur over the differences. Only infrequently do they seriously try to go beyond a simple two-way division between labor income and all other income.

For the study of such a classification, the economy can be broken down into three parts. In one—private corporations organized for profit—the actual shares correspond tolerably well to those of theory. In a second part—proprietorships and partnerships—any division of income into the categories of economic theory is simply a reflection of the assumption on which it is based. In the rest of the economy—government, households and institutions, noncorporate ownership of real property, etc.—each economic unit gives rise almost entirely to either labor or nonlabor income so that within it there is little problem of the distribution of income. But this does not mean these economic units can be ignored, nor that theory can simply deal with the rest of the economy as if they didn't exist. In 1957 they contributed one-fifth of labor income. They contributed almost one-third of non-labor income if corporate profits are measured before tax and almost one-half of nonlabor income if corporate profits are measured after tax,[1] as would seem appropriate for those theories relying on the rate of return or some relation between saving and after-tax labor and nonlabor incomes. They absorb labor or capital, as the case may be, and create income that is spent or saved. Most of the theoretical models, as distinguished from the statistical studies, that Scitovsky discusses seem incapable of accommodating these entities.

Scitovsky's classification of theories by what they are trying to explain seems to me inadvertently to bias and limit the subsequent discussion in a way that is very much to the disadvantage of the marginal productivity theory. He says, "we shall distinguish between theories that explain the constancy and those that account for changes in the share of labor in national income." That is to say, he deals with theories that will explain why, if labor income is 70 per cent, it will remain at 70 per cent or will grow to 75 per cent. But he does not consider why it was 70 per cent rather than 10 or 100 per cent in the first place.

It does not seem to me that this question can be answered with-

[1] This calculation divides national income originating in proprietorships and partnerships between labor and property income in the same proportion as income originating in corporations.

out reference to marginal productivity. The marginal productivity theory also purports to explain occupational differentials, which the other theories he discusses do not. The marginal productivity theory itself is consistent with any statistical division of national income between labor and other income, until the quantities, functions, etc., needed to arrive at some numerical division are introduced. Von Thünen was the first to try to take this theory as a point of departure to use observed data to derive a formula that would yield a numerical answer for the statistical distribution of income. Acceptance of the marginal productivity theory did not rest on the validity of von Thünen's formula, and I doubt that it will or should rest on anyone else's formula. Scitovsky asserts that the main purpose to which the theory is put nowadays is to explain the relative stability of factor shares. But the theory cannot itself be tested by seeing whether or not the distribution is stable.

The exclusive focus on changes or stability in the labor share of income, as distinct from its level, avoids any discussion of whether the macroeconomic theories Scitovsky describes can explain, or even are consistent with, equilibrium for individual economic units. I wish Scitovsky had indicated, in each case, whether the theorist was trying to explain why economic forces, operating through the marginal productivity process, lead to some particular change or lack of change in the distribution of national income, or whether he has some other concept of equilibrium or none at all.

I am particularly interested in the part of Scitovsky's paper where he pieces together what he calls "perhaps the most satisfactory macroeconomic theory." I shall try to show that it is not satisfactory at all, and indeed is completely untenable for the United States from the late twenties to the present. In this section, if we follow Scitovsky, we need be concerned with only three numbers, although two more are hiding under the table: (1) nonlabor income, which sometimes gets called income from capital and which I shall therefore call C; (2) total national income or product, which I shall call Y; and (3) total capital input or capital stock, which I shall call K. If we set down the ratio of each of these three numbers to the other two, we get three ratios, and any two ratios determine the third. We can write the equation: C/Y (the nonlabor share of national income) equals C/K (the rate of return on capital) times K/Y, the ratio of capital input

to national product. If two of these ratios happen to be constant over time, the constancy of the third follows automatically. Scitovsky notes that Phelps Brown and Weber argue that economic forces keep the nonlabor share of national income and the capital-output ratio constant, and this makes the rate of return constant. Kaldor thinks that economic forces keep the nonlabor share of national income and the rate of return on capital constant, and this explains constancy in the capital-output ratio. Finally, Scitovsky himself suggests that all three ratios have some tendency toward stability and these three stabilities mutually reinforce one another.

The important thing to note is that the equation holds only if the same numbers are used for nonlabor income, national income, and capital input each time they appear in the ratios. I concede that, taking each ratio separately, there is some definition of the numerator and denominator that will yield a ratio that evidences no clear and pronounced upward or downward long-term trend. This is not a very stringent statistical test of stability, since any series that fluctuates at random will meet it, but I shall waive the question of what stability means. The point I wish to develop is that there is no set of definitions which, if uniformly applied, will allow all three ratios to show long-term stability in the United States by even the loosest definition of stability. If this is so, the whole argument collapses.

Let me note first that, to use only three series and three ratios in the equation, it is necessary to use prices of the same date in all three ratios. Actually, it is customary to use *current* prices in the first two ratios and *constant* prices in the capital-output ratio. In fact, the theoretical rationale for stability in each requires that this be done. On this basis, balance requires that the right side of the equation be multiplied by another ratio, that of capital stock prices to national product prices. Our theorists get around this difficulty by assuming that productivity increases at the same rate in the production of capital goods as in that of consumption goods, and hence that prices will increase at the same rate, so this ratio too is stable. But actually, the ratio of prices of capital goods output to other prices has risen hugely—by about one-third since 1929, implying that productivity has increased vastly less in capital goods production than elsewhere. Maybe the price indexes are no good and this did not really happen, but in the absence of alterna-

tive data there can be no empirical support for the assumption of equal productivity and price changes in capital goods industries and elsewhere. And these are the same price data used in measuring real capital stock.

Second, the ratios must refer to the same part of the economy—either all of it or some clearly defined sector. Most favorable to the argument—since it is the only way a tolerably constant income distribution can be obtained—is to deal with corporations alone, or else with what I have called elsewhere the ordinary business sector, consisting of corporations, proprietorships, and partnerships. This is pretty awkward for the theoretical argument, as I have already suggested, but let us waive that too. For what I really want to stress is the definition of *nonlabor income*, C, and of *capital input*, K. Nonlabor income enters into two ratios. It is absolutely necessary to *include* corporate income taxes to make nonlabor income a constant fraction of national income in corporate or ordinary business. Even then the data for the last few years suggest a downward movement in the nonlabor share but let us waive that.

But to get a stable rate of return on capital it is necessary, and the theoretical argument seems to require, that corporate profits be measured *after* deduction of corporate income taxes. The Machinery and Allied Products Institute (MAPI) estimates for corporations make the point very clearly.[2] With their adjustments, profits *before* tax averaged 19.2 per cent of corporate income produced in 1923–29 and 21.4 per cent from 1950 to 1959. Inclusion of interest would largely eliminate any change. Profits *after* tax dropped from 16.0 to 9.6 per cent of corporate national income, or by two-fifths, and inclusion of interest would further accentuate the change. On the other hand, profits *after* tax were 5.6 per cent of net worth from 1923 to 1929 and 5.5 per cent from 1950 to 1959. But on a *before*-tax basis, the rate of return increased from 6.5 to 11.1 per cent, or by seven-tenths.[3] This general characteristic of the profits record is well known. It pops up particularly in discussions of the incidence of the corporation

[2] Data cited in the following paragraphs are those underlying charts presented in the *Capital Goods Review*, May 1959.

[3] Other estimates do not yield so clear-cut a result as the MAPI estimates. Comparison of Office of Business Economics profits estimates with Raymond W. Goldsmith's net stock figures suggest the before-tax rate of return has risen while the after-tax rate has fallen. On this basis, there is *no* stable ratio to be explained.

income tax. The stability of *before*-tax corporate income in the corporate national income total suggests the tax is not shifted, while the stability of the rate of return computed *after* tax suggests it is shifted.

For the first two ratios, C/Y and C/K, both to be constant when nonlabor income, C, is defined in the same way, total income, the denominator of the first ratio, would have to rise by the same amount as the net capital stock, the denominator of the second ratio. MAPI estimates show the ratio of income produced in corporations to corporate net worth increased from 27.2 per cent in 1923–29 to 47.6 per cent in 1950–59, or by three-fourths. Under these conditions there *cannot* be *any* definition of nonlabor or property income that will yield stability in both the nonlabor share of national income and the rate of return.

This brings me to the measure of capital, K, which appears in the second and third ratios. In the second, the rate of return calculation, the denominator is the net capital stock, and it *must* be the net capital stock if the ratio is to be stable statistically or if the theoretical argument for stability is to be sensible. But as we have just seen, if the net capital stock is used in the third ratio, the capital-output ratio, and it is computed in current dollars, that ratio drops drastically.

Even when we measure the capital-output ratio in constant prices, if we use the net capital stock to measure capital input the ratio drops dramatically from the twenties to the recent period. I supposed this to be well-known from the work of Kendrick, Kuznets, and Goldsmith, and surely so since Arthur Smithies' attempt to explain this change appeared in the *Quarterly Journal of Economics* last May.

The stable constant-dollar capital-output ratio can still be salvaged, or nearly so, if capital input is measured by constant-dollar depreciation on structures and equipment rather than by capital stock. This works out with both the Kendrick and the MAPI estimates for, say, the years 1909, 1929, and 1957. A case can be made that this is the way capital input of structures and equipment ought to be measured, and that the ratio of depreciation to net output is the one that could be expected to be stable on a priori grounds. But if stability in the capital-output ratio is achieved by the use of depreciation to measure capital input, depreciation would also have to be substituted for net stock in the

second ratio, and this would make the "rate of return" drop drastically. Nor would there be any theoretical reason to expect stability.

I do not think that changes of two-fifths to seven-tenths in a ratio meet even the most tolerant ideas of stability. They seem to me sufficient to dispose of any notion that stability in two of the ratios can explain stability in the third, or that, as Scitovsky suggests, stability in all three can be mutually reinforcing.

It is true, of course, that the English authors cited in this section have relied on English as well as United States statistics. But only the article by Phelps Brown and Weber tries to deal simultaneously with the three ratios. And they say that in the 1924–38 period one of the ratios, that of capital to output, was about the same as from 1870 to 1914, while the other two ratios were both very different. Within the 1924–38 period they find that the ratio of real output to real capital rises sharply, while the other two ratios are supposed not to have shown a trend, although this is not very clear from their charts. Neither within nor between periods is stability of all three ratios really claimed. The three ratios are not given for individual years. Nor are any data subsequent to 1938 provided.

One might suggest that the model builders, in particular, would do well to look a little more carefully at the numbers before they set out to rationalize them. But even to the extent they do, it sometimes seems to matter but little. Thus Kaldor, in one of the articles cited by Scitovsky, appends a footnote that Phelps Brown and Weber indicate a rising capital-output ratio in England from 1900 to 1914 and a falling ratio for 1924–38, and that in the United States the ratio rose from the 1880's to 1909–18, ignoring the depression period, has shown a falling trend since, and is not significantly different now from what it was sixty years ago. In other words, I interject, the ratio has behaved in a perfectly random fashion. But not to Kaldor. Returning to the text, he criticizes existing theories because they cannot explain such constancies except (and I quote) as the result of "some coincidence—as, e.g., that 'capital saving' and 'labour saving' inventions happened (historically) to have *precisely* offset one another."[4] In a similar vein, Scitovsky remarks in his present paper that the marginal

⁴ Nicholas Kaldor, "A Model of Economic Growth," *Economic Journal*, December 1957, pp. 592–593 (my italics).

productivity theory "at best explains not constancy but merely a fair degree of stability in factor shares." One can only wonder how any theory can be criticized on this ground!

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1. Scitovsky's valuable and stimulating survey offers quite a tempting range of topics to pick from for a discussant. But to keep this comment within bearable limits, I shall have to confine myself to a criticism of the last section of his survey, in which he reviews and elaborates upon various "macroeconomic theories" designed to provide an explanation for the "observed" long-run stability of the three ratios. These are: (1) the share of property (i.e., nonlabor) income P in total income Y, or P/Y; (2) the ratio of the stock of capital K to income, or capital coefficient, K/Y; (3) the rate of return on capital, $r = P/K$. As Scitovsky reminds us, these three ratios are not independent so that the constancy of any two implies the constancy of the third.

I shall pass by the issue of whether the empirical evidence adequately supports the long-run stability of these ratios. For one thing, even if these ratios have not remained absolutely constant, it would seem at the very least that their long-run fluctuations have been quite moderate by comparison with the very large changes which have occurred in each of the numerators and denominators separately, as well as in such ratios as output per employed or capital per employed. Besides, the task of explaining the stability is such a fascinating and challenging game that it is hard to resist, even if the stability is after all a figment of somebody's imagination!

My comments fall into two parts. First, I should like to offer some criticism of Scitovsky's largely favorable review of the theories advanced by Kaldor and by Phelps Brown and Weber. I propose to argue that neither of these models, at least as interpreted by Scitovsky, is satisfactory, and more specifically that the first, though logically correct, is unconvincing, while the second is neither convincing nor logically correct. Having thus cleared the ground of opponents, I shall proceed to summarize an alternative explanation which has been advanced in some earlier work of mine, and which Scitovsky did not include in his survey.

2. Kaldor's theory, as interpreted by Scitovsky, is an exceedingly simple and ingenious one. Also, contrary to most other ex-

planations, it succeeds in explaining the stability of the capital-output ratio with no explicit reference to, or reliance on, the nature of technological progress, or, for that matter, on marginal productivity and the price mechanism. Stated very simply, it relies on the identity between the ratio of capital to output and the ratio of the rate of return on sales to the rate of return on capital, or,

$$K/Y \equiv (P/Y)/(P/K), \qquad (1)$$

and on two mechanisms accounting respectively for the long-run stability of the numerator and the denominator ratio.

The stability of the numerator, according to Scitovsky, is explained by the hypothesis that producers set prices by adding a constant percentage mark-up, say, $m/(1-m)$ to unit labor cost. As is well known, such a constant mark-up policy implies that total labor cost, or labor income, will tend to represent a constant fraction $(1-m)$ of total product Y, and therefore the rate of return on sales P/Y will tend to be m—a result of which Weintraub[1] has made a good deal.

This mechanism is supplemented by a second one which Scitovsky hardly discusses and which insures that the rate of return on capital r will gravitate toward a long-run equilibrium value $\bar{r}$, pulling in turn the capital output ratio toward an "equilibrium" value $\bar{k} = m/\bar{r}$. The essential ingredient of this mechanism is the hypothesis that the rate of investment depends both on the rate of growth of income (the conventional acceleration principle) and on the rate of return on capital. The postulated relation is such that when $r = \bar{r}$, and hence $k = \bar{k}$, the rate of growth of capital just equals the rate of growth of income, with the result that K/Y stays put at $\bar{k}$ and hence also r at $\bar{r}$.

If, however, at any point K/Y should happen to fall short of $\bar{k}$, say, because of technical change or other disturbance, then as a result of the unchanged mark-up m, the rate of return on capital would rise above $\bar{r}$. The increase in r in turn leads to a step-up in the rate of investment, which causes capital to grow faster than income, so that K/Y will rise back toward $\bar{k}$, thereby also pulling r back toward $\bar{r}$.

I should add at this point that my interpretation of Kaldor's model, based on his published work,[2] is somewhat different in the

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[1] S. Weintraub, *A General Theory of the Price Level, Output, Income Distribution and Economic Growth*, Philadelphia, 1959.

[2] I am referring in particular to "A Model of Economic Growth," *Economic Journal*, December 1957, pp. 591–624.

details, if not in the broad outline. As I see it, the constancy of the capital-output ratio is based on the interaction of his investment function with his "technical progress function" (stating that the rate of change of output is a function of the lagged rate of growth of capital). These two functions insure that the rate of growth of *both* income *and* capital must gravitate toward an equilibrium rate G, and hence income and capital must tend to become proportional. Given this rate of growth, the share of property income P/Y and the capital coefficient are simultaneously determined with the help of the investment function and by relying on Kaldor's peculiar theory of distribution, according to which the factor shares are determined, of all things, by the ratio of investment to income.

However, these differences of interpretation are perhaps a matter of detail. What is essential is that, in either interpretation, the cornerstone of Kaldor's model is the assumption that, even at full employment, the rate of investment is completely determined by the investment demand; whatever this demand, the level of saving and consumption passively adjusts to it through the intermediary of Kaldor's amazing theory of distribution. Hence, even without criticizing other aspects of his model, such as his nondescript "technical progress function," anyone who, like myself, is inclined to share Tobin's dim view of Kaldor's theory of distribution and saving behavior must regard his whole explanation as unconvincing, in spite of its undeniable ingenuity.

3. The Phelps Brown and Weber model again accounts for the stability of the labor share in terms of a stable mark-up on labor costs; but, in contrast to the Kaldor model, it endeavors to explain directly the stability of the capital coefficient—the stability of the rate of return being thus accounted for as a necessary consequence of the other two. Unfortunately, just how Phelps Brown and Weber propose to explain the stability of the capital coefficient is not very clear, either in their original work or in Scitovsky's interpretation. Scitovsky seems to suggest that this stability is derived from the constancy of the shares plus the hypothesis that technological change has been neutral on balance, which is itself explained by a variety of forces. There are unfortunately two serious difficulties with the passage in which Scitovsky advances this proposition. In the first place, neutral technical change has been defined in a variety of ways, and Scitovsky fails to make clear which concept he is adopting, although from the context

one must conclude that he refers to Harrod's definition.[3]   Second, and more important, it is in general *not* true that constancy of the shares and Harrod-neutral technical change imply constancy of the capital-output ratio.

In order to establish this point, let us recall that Harrod's definition can be paraphrased as follows: technical change between some initial date 0 and some later date t is neutral if, the rate of interest being unchanged between the two dates, the most economical
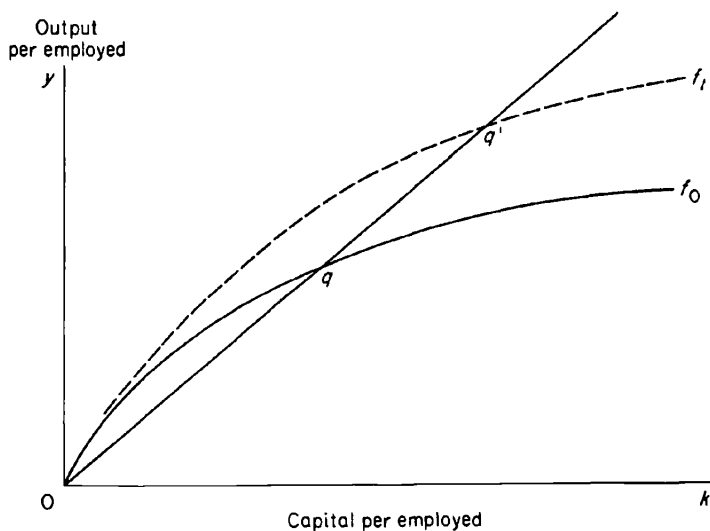

FIGURE 1.

(least-cost) input combination implies an unchanged capital output ratio, i.e., if $r_0 = r_t$ implies $K_t/Y_t = K_0/Y_0$ (it being assumed that the least-cost combination in fact prevails).   It will be useful, for later reference, to exhibit analytically and graphically just what Harrod-neutral technical change implies with respect to the shift in time of the production function.   Let $Y = F_0(K,E)$ denote the production function (homogeneous of first degree) at the initial date, E standing for total employment.   Making use of the homogeneity properties, it can be rewritten as: $y = f(k)$, where $y = Y/E$ and $k = K/E$ denote respectively output per employed and capital per employed.   This function can be represented by a curve in the (y,k) plane, such as the solid curve of Figure 1.

[3] R. Harrod, *Towards a Dynamic Economics*, London, 1948, p. 23.

It should be noted that the slope $f'_0(k)$ of this curve for any given value of k represents the marginal productivity of capital for the given input combinations, and therefore also the rate of interest for which k is the least-cost combination.

Now, let $X = F_t(K,E)$ denote the production function at date t. It can be readily established that if the shift in the production between 0 and t is Harrod-neutral throughout, F_t must be related to F_0 by the equation

$$F_t(K,E) = F_0[K,H(t)E], \qquad (2)$$

where $H(t) > 1$ if in fact technology has improved. In other words, for any given combination of inputs, the output with the new production function will be the same as if the production function had remained unchanged but labor input had been increased by a factor $H(t)$, or each worker had become the equivalent of $H(t)$ workers. Because of the homogeneity, the new production function at time t can be restated in the form

$$Y/[H(t)E] = f_0[K/H(t)E], \text{ or } y = f_t(k) = H(t)f_0[k/H(t)]. \qquad (3)$$

In terms of Figure 1, the new production function f_t is represented by a curve such as the dotted one, which can be derived from the f_0 curve as follows: through any point q of the old function, draw the radius vector $0q$ and extend it to the point q', such that $0q'/0q = H(t)$. Then q' is on f_t, and f_t itself is the locus of such points obtained by sliding q along f_0. To verify that this locus represents a Harrod-neutral shift of the locus f_0, we need only to observe that, by construction, the slope of f_t at q' is the same as the slope of f_0 at q. In general, therefore, for any given rate of interest or slope, the point chosen on f_t and that chosen on f_0 will lie on the same radius vector. But this in turn implies that these two points will be characterized by the same capital coefficient, since along any radius vector we have $y/k = (Y/E)/(K/E) = Y/K = $ constant. In fact, the slope of the radius vector through any point q, when referred to the y axis, represents precisely the capital coefficient at q. It should be noted that the shift in the production function between 0 and t might be Harrod-neutral in a neighborhood without being so throughout.

We may add in passing that Harrod's definition of neutral change

is quite different from the most common or "classical" one, according to which a change is neutral if, and only if, for every combination of inputs, the ratio of the marginal product of any two inputs is the same for F_t as for F_0; that is, the marginal productivity of all factors rises in the same proportion. This definition implies that, if the shift between 0 and t is classical-neutral, then F_t is related to F_0 by the equation $F_t(K,E) = C(t)F_0(K,E)$, with $C(t) > 1$. Similarly f_t is related to f_0 by the relation $x = f_t(k) = C(t)f_0(k)$. Thus f_t can be obtained graphically from f_0 by multiplying the ordinate of each point on f_0 by the factor $C(t)$.

Now it is immediately apparent from the definition and from our graph that, if technical change is Harrod-neutral, constancy of the capital output ratio implies constancy of the shares. For, in this case, we know we are moving from a point such as q on f_0 to a point q' on f_t lying on the radius vector $0q$, and since the slopes at q' and q are the same, we must have $r_t = r_0$. But this condition, together with the relation $K_t/Y_t = K_0/Y_0$, assumed *ex hypothesis*, implies $r_tK_t/Y_t = r_0K_0/Y_0$, i.e., constancy of the shares. Unfortunately the converse proposition, on which Scitovsky seems to rely, is *not* true: that is, constancy of the shares does *not* necessarily imply constancy of the capital-output ratio. The easiest way of disproving Scitovsky's inference is to provide a counter example. Suppose, then, that the production function F_0 is of the Cobb-Douglas variety, or, say, $X = F_0(K,E) = AK^aE^{1-a}$. Suppose next that F_t is another Cobb-Douglas function of the form

$$Y = F_t(K,E) = AK^a[H(t)E]^{1-a} = F_0[K,H(t)E]$$
$$= AH(t)^{1-a}K^aE^{1-a}. \quad (4)$$

By comparing equation (4) with equation (2), we see that the shift from 0 to t satisfies the definition of Harrod-neutrality. At the same time, from a well-known property of the Cobb-Douglas function we know that, if factors are paid their marginal product, the share of capital is given by a, the exponent of K, independently of the input combination chosen. Since the exponent of K does not change between 0 and t, we must have $r_0K_0/Y_0 \equiv P_0/Y_0 = a = P_t/Y_t$, i.e., the shares are constant. And yet from this information, we can make no inference whatever about the capital-output ratios which are respectively $K_0/Y_0 = (1/A)(K_0/E_0)^{1-a}$ and $K_t/Y_t = (1/A)(K_t/H(t)E_t)^{1-a}$, and will be equal if, and

only if, $K_t/E_t = H(t)(K_0/E_0)$. In other words, at least in this particular example, the constancy of the shares, plus Harrod-neutrality, does not entitle us to draw any conclusion about the behavior of the capital-output ratio.

The shortcoming in Scitovsky's argument which we have just brought out could no doubt be remedied by imposing some further specifications on the nature of the production function and/or technical progress. But even if this were done, another and more fundamental flaw would remain in the proposed explanation of the constancies. To see that this must indeed be the case, we need only observe that in the model under discussion we find no reference, explicit or implicit, to the supply side of the capital market, i.e., to the set of forces or mechanisms controlling the community's willingness to accumulate capital through the process of saving. This is an important departure from the Kaldor model, in which, at least, such a mechanism was provided for, even if not very convincingly. Surely there must be something wrong with a line of reasoning that purports to draw conclusions about the stock of capital and its behavior in time merely from information about technical knowledge and pricing behavior. Fortunately, the flaw is not too difficult to ferret out: the root of the difficulty lies in an unholy marriage of a marginalistic model of factor remuneration, with a mark-up—and hence nonmarginalistic—model of price determination. To be more specific, in order to make inferences about factor shares from the hypothesis of Harrod-neutral technical change, we must conceive of the capital share or rate of return on sales P/Y as determined by r and by the capital-labor ratio chosen by producers in response to r, which in turn implies a unique value for the capital-output ratio K/Y, and hence finally for $rK/Y = P/Y$. But then we cannot simultaneously postulate that P/Y is determined independently by an exogenously given "customary" mark-up on labor costs. In brief, the explanations used to establish the constancy of P/Y and that of K/Y are inconsistent with each other!

4. Let me now move on to the more constructive task of outlining an alternative model which can also account for the historical constancies and which, at the same time, meets Scitovsky's challenge of accomplishing this task within the framework of the marginal productivity approach. The argument developed below is to some extent a summary of parts of a recent paper by Albert

Ando and myself,[4] although it places much greater stress on the role of technical progress (which was treated only in passing in our paper) and relies on less restrictive assumptions.

The point of departure of the model is a theory of saving behavior, the main elements of which were first presented in my joint paper with R. Brumberg, "Utility Analysis and the Consumption Function: An Interpretation of Cross-Section Data."[5] As shown in the essay I wrote with Ando, cited above, and in other papers,[6] this model implies an aggregate consumption function of the form $C_t = cY^*_t + bW_t$, where C_t denotes the current rate of consumption at time t and Y^*_t the (anticipated) rate of nonproperty income, both measured in terms of current prices, and W_t represents the aggregate current market value of households' net worth. It has also been shown that the coefficient c (a pure number) and b (with dimension 1/time) will tend to be roughly constant in time, at least as long as the rate of return on capital, r, and the growth trend of Y^* are roughly constant. For purposes of long-run analysis, which is our present concern, we can identify Y^*_t with current labor income, to be denoted by X_t. In the interest of brevity, we may also ignore here the existence of nonreproducible tangible wealth and of government operations (including the national debt). Under these conditions W can be identified with reproducible tangible wealth, or K, and private income, $X + rK$ coincides with net output, Y.[7] The long-run aggregate consumption function thus becomes:

$$C_t = cX_t + bK_t = c(X_t + r_tK_t)$$
$$+ (b - r_tc)K_t = cY_t + (b - r_tc)K_t. \quad (5)$$

[4] "Growth, Fluctuations and Stability," *American Economic Review*, May 1959, pp. 501–524.

[5] In *Post-Keynesian Economics*, K. Kurihara, editor, New Brunswick, N.J., 1954.

[6] F. Modigliani and R. Brumberg, "Utility Analysis and the Aggregate Consumption Function," mimeographed; A. K. Ando, "*A Contribution to the Theory of Economic Fluctuations and Growth*," Ph.D. thesis, Graduate School of Industrial Administration, Carnegie Institute of Technology, 1959; A. K. Ando and F. Modigliani, "The 'Life Cycle' Hypothesis of Saving: Aggregative Implications and Tests," *American Economic Review*, March 1963, pp. 55–84.

[7] For an analysis of the economic implications of the national debt within this framework, see my recent paper, "Long-Run Implications of Alternative Fiscal Policies and the Burden of the National Debt," *Economic Journal*, December 1961. The effect of nonreproducible tangible wealth and of the discrepancy between private net income and output will be examined in a forthcoming paper.

Now it can be readily shown that this consumption function has the following basic implication: if Y_t fluctuates around an exponential growth trend with a stable rate of growth of, say, n per cent per year, and if r_t fluctuates similarly in the neighborhood of a constant level r, then the ratio of wealth to income K/Y will gravitate around a stable "equilibrium" value, say, a. This equilibrium value is related to the parameters of the consumption function by the equation:

$$a = (1 - c)/(n + b - rc). \qquad (6)$$

To prove this proposition, let us note that eq. (5) implies the saving function $S_t = Y_t - C_t = (1 - c)Y_t - (b - rc)K_t$, i.e., saving is an increasing linear function of income and a decreasing function of the stock of wealth (since the model itself, as well as empirical verifications thereof, indicate that $b > r$ and $c < 1$). But S_t is simply the rate of growth of wealth, or $dK/dt \equiv \dot{K}_t$. Making the substitution, dividing by K_t, rearranging terms, and using eq. (6), we get:

$$\dot{K}_t/K_t = (1 - c)(Y_t/K_t) - (b - rc) = n - \frac{n + b - rc}{K_t}$$
$$\left[K_t - \frac{1 - c}{n + b - rc} Y_t \right] = n - \frac{n + b - rc}{K_t} [K_t - aY_t]. \quad (7)$$

From this equation, we can immediately infer that K_t/Y_t must gravitate toward the value a given by eq. (6). For if K_t/Y_t exceeds this value, i.e., $K_t > aY_t$, then we see from eq. (7) that $\dot{K}_t/K_t$ will be smaller than n, the rate of growth of Y_t. But then Y_t must be rising faster than K_t, and hence, K_t/Y_t must be decreasing in time and approaching a. Conversely, if at any point K_t/Y_t falls short of the equilibrium value, then the rate of accumulation will become sufficiently brisk to cause K_t to rise faster than Y_t, until equilibrium is re-established.

We can thus conclude that, *if* income exhibits an exponential growth trend and r is stable in time, then the stability over time of the ratio of wealth to income, or capital coefficient K/Y, can be accounted for by the supply side of the capital markets, as an implication of the consumption function (5). In order to complete our explanation of the "constancies," we need to introduce some further hypothesis on the nature of technical progress and the demand side of the capital markets, which insure an expo-

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nential growth trend and the stability of r. In particular, the following two assumptions can be shown to be sufficient: (a) that technical progress, for the reasons mentioned by Scitovsky and related mechanisms stressed by Fellner,[8] tends on balance to be Harrod-neutral, thus satisfying equation (2); and (b) that technical progress tends to shift the production function outward at a roughly constant proportional rate, or more precisely, that the time path of the quantity $H(t)$ of equation (2) tends to follow an exponential trend of the form $H(0)e^{pt}$.

To show that assumptions (a) and (b), together with the consumption function (5), imply the three constancies, it is convenient to proceed in two steps. Let us suppose at first that the labor force is constant and equal to E. Then $Y_t = Ey_t$ is proportional to y_t, and its growth is entirely due to rising output per capita, or productivity. Now assumption (a) (Harrod-neutral change) implies that, if the available capital is proportional to income, i.e., K/Y is constant, then the rate of return r needed to clear the capital markets will tend to remain constant; and the consumption function insures that, with a constant r, the capital-output ratio will be constant, *provided* Y grows exponentially. To complete the argument, it is therefore sufficient to show that if the capital-output ratio is constant, i.e.,

$$K = aY \text{ or } k = ay, \tag{8}$$

then assumption (b) insures that income will in fact follow an exponential growth trend. To verify this last conclusion, we only need to substitute eq. (8) into eq. (3), obtaining

$$y = H(t)f_0[ay/H(t)].$$

The value of y satisfying this equation, say, y_t, can be written in the form

$$y_t = y_0H(t) = y_0H(0)e^{pt},$$

where y_0 is a constant satisfying the condition $y_0 = f_0(ay_0)$. Thus y, and hence Y, will tend to grow at the rate p, and hence also eq. (8) will hold, with the constant a given by eq. (6), after setting $n = p$.

This result can be illustrated in terms of Figure 1. We can think of y_0 as the y coordinate of the point q at the intersection of

[8] See, e.g., William Fellner, *Trends and Cycles in Economic Activity*, New York, 1956, Chs. 7, 8, and 9.

the "initial" production function f_0 with the radius vector $0q$, having equation $k = ay$. Then y_t is represented by points at the intersection of this radius vector with a succession of outward shifting production functions f_t. These successive points of intersection are moving outward on the radius vector at the exponential rate p.

The reasoning is essentially unchanged if we drop the assumption that population is constant and assume instead that the labor force is itself growing at the exponential rate p'—that is, $E = E_0 e^{p' t}$. The only difference is that now total income, $Y_t = y_t E_t = y_0 e^{pt} E_0 e^{p' t} = y_0 E_0 e^{(p+p') t}$, will grow at the rate $p + p'$, and hence, the equilibrium value of the capital coefficient a will be given by eq. (6) with $n = p + p'$.

We have thus shown that the three constancies can be explained by the $M - B$ consumption function, plus the hypothesis that technical progress has tended, on balance, to be of the Harrod-neutral variety and to take place at a roughly exponential rate. One significant implication of this model is that the rate of growth of income is completely determined by the rate of technical progress p and the rate of population growth p'—although it reflects in part also the growth of capital per employee, which is brought about by the consumption function, and occurs at the same rate as the growth in productivity.[9] Hence, the growth trend of income is independent of the marginal propensity to consume c or, for that matter, of the saving ratio S/Y; these quantities determine only the equilibrium capital output ratio and hence the ray on which we move, but not the speed of movement.

Unfortunately, it is not possible to expatiate here on this implication of the model, nor to bring out other interesting properties and possible generalizations, including the adaptability of the model

[9] In other words, the growth p in per capita output is due only partly to the outward shift in the production function, the remaining part being due to the growth of k, or in capital per man. It is this increase in capital per man which enables output to grow along the ray $0q$ of Figure 1. If instead k were to remain constant, we would not be moving along the ray $0q$, but instead along a vertical line through q, and the growth of output would be smaller, and might not even be exponential. To illustrate, if the production function were of the Cobb-Douglas type (4), we would have $Y_t = AK^a[H(t)E]^{1-a}$, or $y_t = [H(t)]^{1-a}k^a$. Hence, if k were constant and $H(t) = H(0)e^{pt}$, the rate of growth of y_t would be only $p(1 - a)$. We might also note that the constancy of the three ratios requires only the constancy of the *over-all* rate of growth of income n; we can therefore relax the assumption that p and p' are individually constant as long as their sum is reasonably stable.

to an explanation of cycles as well as long-run trends.[10]   There are, however, two brief concluding comments I should like to indulge in.   First, the alternative model I have presented does not imply that the historical constancies will necessarily be maintained in the future.   I can see no strong ground for confidence that technical progress will forever remain Harrod-neutral on balance, or that it will continue to occur at a roughly constant rate (or that the sum of p and p' will remain constant); and a change in any one of these conditions will in turn tend to disturb the constancy of the capital-output ratio.   However, partly on intuitive grounds and partly for reasons too complex to report here, I would be inclined to expect that the (full-employment) rate of growth n and the capital-output ratio are likely to change at best very slowly.   My second remark is that whether or not I have persuaded the reader of the usefulness of the specific model reported here, I hope at least to have made a convincing case for the central importance of the much-neglected supply side of the capital market for an under-standing of the behavior of the capital-output and the capital-labor ratios, and more generally, of the phenomena associated with growth.

In summary, I have tried to bring to light some shortcomings of the macroeconomic theories reviewed by Scitovsky.   I have also presented an alternative model in the hope of undermining his contention that "no satisfactory explanation [of the constancies] has been offered as yet by the upholders of the marginal productivity theory."   Whether or not I have been successful in this last respect is left for the reader to decide.

REPLY by Tibor Scitovsky

Let me say first of all how pleased I am that my survey should have called forth such interesting and constructive comments.   I feel apologetic toward Modigliani for having missed his and Ando's very relevant and interesting paper, especially because I quite

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[10] Some of these implications are spelled out in Ando and Modigliani, *American Economic Review*, May 1959, and in "Long-Run Implications of Alternative Fiscal Policies and the Burden of the National Debt," *Economic Journal*, December 1961.   It should also be noted that the model can be generalized to allow for monopolistic and oligopolistic market structures, provided the ratio of price to (long-run) average cost remains reasonably constant in time.

agree that the role of the supply side of capital needs and deserves to be more fully explored.

As to the critical comments on my survey, let me begin by defending myself against Denison's charge of my having been unduly critical of the marginal productivity theory. I am sorry if I have created that impression, because I fully agree that marginal productivity is the basic building block for explaining both income distribution and many other things in economics; and my criticism was only aimed at too exclusive and too naive a reliance on it.

The other criticisms of both Denison and Modigliani have to do with the tail end of my survey, concerned with Kaldor's contribution and that of Phelps Brown and Weber. I was impressed more with their approach than with their arguments, and with the general proposition that when economic forces tend to stabilize each of several interdependent variables, they mutually reinforce each other. This seemed and still seems to me an idea worth exploring further, especially in the area under discussion, where so much remains yet to be explained. Denison's point that changes in tax rates and prices make it impossible for all the interdependent variables stabilized by market forces to be stabilized simultaneously is perfectly correct; but instead of destroying the original argument, it merely introduces a range of indeterminacy into it. I stand corrected by Modigliani's criticism of my understanding of Phelps Brown and Weber's argument, but find unconvincing his objection to Kaldor's argument as a complete and even more as a partial explanation. Modigliani's main objection seems to be "the assumption that, even at full employment, the rate of investment is completely determined by the investment demand"; and I should like to remind him that all Kaldor does is to shift the limit, up to which he assumes this, from full employment to what Joan Robinson calls the inflation barrier.

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