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RELATIVE MOVEMENTS OF REAL WAGES AND OUTPUT

AN article by Mr. J. G. Dunlop in this JOURNAL (Sept. 1938, Vol. XLVIII, p. 413) on "The Movement of Real and Money Wage Rates," and the note by Mr. L. Tarshis printed below (p. 150),[1] clearly indicate that a common belief to which I acceded in my "General Theory of Employment" (p. 10) needs to be reconsidered. I there said:

> "It would be interesting to see the results of a statistical enquiry into the actual relationship between changes in money wages and changes in real wages. In the case of a change peculiar to a particular industry one would expect the change in real wages to be in the same direction as the change in money wages. But in the case of changes in the general level of wages, it will be found, I think, that the change in real wages associated with a change in money wages, so far from being usually in the same direction, is almost always in the opposite direction. . . . This is because, in the short period, falling money wages and rising real wages are each, for independent reasons, likely to accompany decreasing employment; labour being readier to accept wage-cuts when employment is falling off, yet real wages inevitably rising in the same circumstances on account of the increasing marginal return to a given capital equipment when output is diminished."

But Mr. Dunlop's investigations into the British statistics appear to show that, when money wages are rising, real wages have usually risen also; whilst, when money wages are falling, real wages are no more likely to rise than to fall. And Mr. Tarshis has reached broadly similar results in respect of recent years in the United States.

In the passage quoted above from my "General Theory" I was accepting, without taking care to check the facts for myself, a belief which has been widely held by British economists up to the last year or two. Since the material on which Mr. Dunlop mainly depends—namely, the indices of real and money wages prepared by Mr. G. H. Wood and Prof. Bowley—have been available to all of us for many years, it is strange that the correction has not been made before.[2] But the underlying

[1] Cf. also his article on "Real Wages in the United States and Great Britain," published in *The Canadian Journal of Economics* for August 1938.

[2] Cf., however, the reference given below (p. 38) to Prof. Pigou's "Industrial Fluctuations."

problem is not simple, and is not completely disposed of by the statistical studies in question.

First of all it is necessary to distinguish between two different problems. In the passage quoted above I was dealing with the reaction of real wages to changes in *output*, and had in mind situations where changes in real and money wages were a reflection of changes in the level of employment caused by changes in effective demand. This is, in fact, the case which, if I understand them rightly, Mr. Dunlop and Mr. Tarshis have primarily in view.[1] But there is also the case where changes in wages reflect changes in prices or in the conditions governing the wage bargain which do not correspond to, or are not primarily the result of, changes in the level of output and employment and are not caused by (though they may cause) changes in effective demand. This question I discussed in a different part of my "General Theory" (namely Chapter 19, "Changes in Money Wages"), where I reached the conclusion that wage changes, which are not in the first instance due to changes in output, have complex reactions on output which may be in either direction according to circumstances and about which it is difficult to generalise. It is with the first problem only that I am concerned in what follows.[2]

The question of the influence on real wages of periods of boom and depression has a long history. But we need not go farther back than the period of the 'eighties and 'nineties of the last century, when it was the subject of investigation by various official bodies before which Marshall gave evidence or in the work of which he took part. I was myself brought up upon the evidence he gave before the Gold and Silver Commission in 1887 and the Indian Currency Committee in 1899.[3] It is not always clear whether Marshall has in mind a rise in money wages associated with a rise in output, or one which merely reflects a change in prices (due, for example, to a change in the standard which was

[1] See, however, the *post-scriptum* to Mr. Tarshis's note to which I refer further below.

[2] In his "Essays in the Theory of Economic Fluctuations," to which I shall have occasion to refer below, Dr. Kalecki deals with the relation between real wages and output in the essay entitled "The Distribution of the National Income." But it is with the other problem that he is primarily concerned in the essay entitled "Money and Real Wages."

[3] Marshall's contributions to official inquiries from 1886 to 1903 we used to regard as constituting, together with the "Principles," his most important and valuable work. Re-reading his "Official Papers" to-day, I find this confirmed. Yet his "Official Papers," published by the Royal Economic Society in 1926 (still obtainable by members at 5*s*.), has had a negligible circulation compared with any of his other works.

the particular subject on which he was giving evidence); but in some passages it is evident that he is dealing with changes in real wages at times when output is expanding. It *is* clear, however, that his conclusion is based, not like some later arguments on *à priori* grounds arising out of increasing marginal cost in the short period, but on statistical grounds which showed—so he thought—that in the short period wages were stickier than prices. In his preliminary memorandum for the Gold and Silver Commission ("Official Papers," p. 19) he wrote: "(During a slow and gradual fall of prices) a powerful friction tends to prevent money wages in most trades from falling as fast as prices; and this tends almost imperceptibly to establish a higher standard of living among the working classes, and to diminish the inequalities of wealth. These benefits are often ignored; but in my opinion they are often nearly as important as the evils which result from that gradual fall of prices which is sometimes called a depression of trade." And when Mr. Chaplin asked him (*op. cit.*, p. 99), "You think that during a period of depression the employed working classes have been getting more than they did before?" he replied, "More than they did before, on the average."

Subsequently, as appears from an important letter of April 1897 (hitherto unpublished) to Foxwell,[1] who held somewhat strongly the opposite opinion, Marshall's opinion became rather more tentative; though the following extract refers more to his general attitude towards rising prices than to their particular effect on real wages :—

> "You know, my views on this matter are (*a*) not very confident, (*b*) not very warmly advocated by me, (*c*) not very old, (*d*) based entirely on non-academic arguments & observation.
>
> In the years 68 to 77 I was strongly on the side you now advocate. The observation of events in Bristol made me doubt. In 85, or 86 I wrote a Mem^{n} for the Com^{n} on Depression showing a slight preference for rising prices. But in the following two years I studied the matter closely, I read and analysed the evidence of business men before that Commission; & by the time the Gold & Silver Commission came, I had just turned the corner.
>
> Since then I have read a great deal, but almost exclusively of a non-academic order on the subject: & was thinking about it during a great part of the evidence given by business men & working men before the Labour Commission. I

[1] Endorsed by Foxwell—"*Marshall*, a very characteristic letter on the question of rising and falling prices, among other matters."

have found a good deal that is new to strengthen my new conviction, nothing to shake it. I am far from certain I am right. I am absolutely certain that the evidence brought forward in print to the contrary in England and America (I have not read largely for other countries) does not prove what it claims to, & does not meet or anticipate my arguments, in the simple way you seem to imagine."

Shortly afterwards he began to work at his evidence for the Indian Currency Committee which seems to have had the effect of confirming him in his previous opinion. His final considered opinion is given in Question 11,781 :—[1]

" I will confess that, for ten or fifteen years after I began to study political economy, I held the common doctrine, that a rise of prices was generally beneficial to business men directly, and indirectly to the working classes. But, after that time, I changed my views, and I have been confirmed in my new opinions by finding that they are largely held in America, which has recently passed through experiences somewhat similar to those of England early in the century. The reasons for the change in my opinion are rather long, and I gave them at some length before the Gold and Silver Commission. I think, perhaps, I had better content myself now with calling your attention to the fact that the statistical aspect of the matter is in a different position now. The assertions that a rise in prices increased the real wages of the worker were so consonant with the common opinion of people who had not specially studied the matter, that it was accepted almost as an axiom; but, within the last ten years, the statistics of wages have been carried so far in certain countries, and especially in England and America, that we are able to bring it to the test. I have accumulated a great number of facts, but nearly everything I have accumulated is implied in this table. It is copied from the article by Mr. Bowley in the ECONOMIC JOURNAL for last December. It is the result of work that has been going on for a number of years, and seems to me to be practically decisive. It collects the average wages in England from the year 1844 to the year 1891, and then calculates what purchasing power the wages would give at the different times, and it shows that the rise of real wages after 1873 when prices were falling was greater than before 1873 when prices were rising."

Here follows a table from Prof. Bowley's article in this JOURNAL for Dec. 1898. Marshall's final conclusion was crystallised in a passage in the " Principles " (Bk. VI, Ch. viii, § 6) :—

(When prices rise the employer) " will therefore be more able and more willing to pay the high wages; and wages will tend upwards. But experience shows that (whether they

[1] " Official Papers," pp. 284–288.

are governed by sliding scales or not) they seldom rise as much in proportion as prices; and therefore they do not rise nearly as much in proportion as profits."

Although Marshall's evidence before the Indian Currency Committee was given in 1899, Prof. Bowley's statistics on which he was relying do not relate effectively to a date later than 1891 (or 1893 at latest). It is clear, I think, that Marshall's generalisation was based on experience from 1880 to 1886 which did in fact bear it out. If we divide the years from 1880 to 1914 into successive periods of recovery and depression, the broad result, allowing for trend, appears to be as follows :—

		Real Wages.
1880–1884.	Recovery	Falling.
1884–1886.	Depression	Rising.
1886–1890.	Recovery	Rising.
1890–1896.	Depression	Falling.
1896–1899.	Recovery	Rising.
1899–1905.	Depression	Falling.
1905–1907.	Recovery	Rising.
1907–1910.	Depression	Falling.
1910–1914.	Recovery	Rising.

According to this, Marshall's generalisation holds for the periods from 1880 to 1884 and from 1884 to 1886, but for no subsequent periods.[1] It seems that we have been living all these years on a generalisation which held good, by exception, in the years 1880–86, which was the formative period in Marshall's thought in this matter, but has never once held good in the fifty years since he crystallised it! For Marshall's view mainly prevailed, and Foxwell's contrary opinion was discarded as the heresy of an inflationist. It is to be observed that Marshall offered his generalisation merely as an observed statistical fact, and, beyond explaining it as probably due to wages being stickier than prices, he did not attempt to support it by *à priori* reasoning. The fact that it has survived as a dogma confidently accepted by my generation must be explained, I think, by the more theoretical support which it has subsequently received.

To my statement that Marshall's generalisation has remained uncorrected until recently there is, however, an important exception. In his "Industrial Fluctuations," published in 1927, Professor Pigou pointed out (p. 217) that "the upper halves of trade cycles have, on the whole, been associated with higher rates of real wages than the lower halves," and he printed in

[1] I compiled this table, as a check, independently of Mr. Dunlop's table, *loc. cit.*, p. 419. But it only serves to confirm his more accurate version. According to him, trend eliminated, real wages fell 3 per cent. in the recovery culminating in 1883 or 1884 and rose 2·7 per cent. in the depression from 1884 to 1886.

support of this a large scale chart for the period from 1850 to 1910. Subsequently, however, he seems to have reverted to the Marshallian tradition, and in his "Theory of Unemployment," published in 1933, he writes (p. 296) :—

> "In general, the translation of inertia from real wage-rates to money wage-rates causes real rates to move in a manner not compensatory, but complementary, to movements in the real demand function. Real wage-rates not merely fail to fall when the real demand for labour is falling, but actually rise; and, in like manner, when the real demand for labour is expanding, real wage-rates fall."

About that time M. Rueff had attracted much attention by the publication of statistics which purported to show that a rise in real wages tended to go with an increase in unemployment. Prof. Pigou points out that these statistics are vitiated by the fact that M. Rueff divided money wages by the wholesale index instead of by the cost-of-living index, and he does not agree with M. Rueff that the observed rise in real wages was the main *cause* of the increased unemployment with which it was associated. But he concludes, nevertheless (p. 300), on a balance of considerations, that "there can be little doubt that in modern industrial communities this latter tendency (*i.e.*, for shifts in real demand to be associated with shifts in the *opposite* sense in the rate of real wages for which work people stipulate) is predominant."

Like Marshall, Prof. Pigou based his conclusion primarily on the stickiness of money wages relatively to prices. But my own readiness to accept the prevailing generalisation, at the time when I was writing my "General Theory," was much influenced by an *à priori* argument, which had recently won wide acceptance, to be found in Mr. R. F. Kahn's article on "The Relation of Home Investment to Employment," published in the ECONOMIC JOURNAL for June, 1931.[1] The supposed empirical fact, that in the short period real wages tend to move in the opposite direction to the level of output, appeared, that is to say, to be in conformity with the more fundamental generalisations that industry is subject to increasing marginal cost in the short period, that for a closed[2] system as a whole marginal cost in the short period is substantially the same thing as marginal wage cost, and that in competitive conditions prices

[1] *Passim*; see particularly pp. 178, 182. It was Mr. Kahn who first attacked the relation of the general level of prices to wages in the same way as that in which that of particular prices has always been handled, namely as a problem of demand and supply in the short period rather than as a result to be derived from monetary factors.

[2] The qualifications required, if the system is not closed, are dealt with below.

are governed by marginal cost; all this being subject, of course, to various qualifications in particular cases, but remaining a reliable generalisation by and large.

I now recognise that the conclusion is too simple, and does not allow sufficiently for the complexity of the facts. But I still hold to the main structure of the argument, and believe that it needs to be amended rather than discarded. That I was an easy victim of the traditional conclusion because it fitted my theory is the opposite of the truth. For my own theory this conclusion was inconvenient, since it had a tendency to offset the influence of the main forces which I was discussing and made it necessary for me to introduce qualifications, which I need not have troubled with if I could have adopted the contrary generalisation favoured by Foxwell, Mr. Dunlop and Mr. Tarshis. In particular, the traditional conclusion played an important part, it will be remembered, in the discussions, some ten years ago, as to the effect of expansionist policies on employment, at a time when I had not developed my own argument in as complete a form as I did subsequently. I was already arguing at that time that the good effect of an expansionist investment policy on employment, the fact of which no one denied, was due to the stimulant which it gave to effective demand. Prof. Pigou, on the other hand, and many other economists explained the observed result by the reduction in real wages covertly effected by the rise in prices which ensued on the increase in effective demand. It was held that public investment policies (and also an improvement in the trade balance through tariffs) produced their effect by deceiving, so to speak, the working classes into accepting a lower real wage, effecting by this means the same favourable influence on employment which, according to these economists, would have resulted from a more direct attack on real wages (*e.g.*, by reducing money wages whilst enforcing a credit policy calculated to leave prices unchanged). If the falling tendency of real wages in periods of rising demand is denied, this alternative explanation must, of course, fall to the ground. Since I shared at the time the prevailing belief as to the facts, I was not in a position to make this denial. If, however, it proves right to adopt the contrary generalisation, it would be possible to simplify considerably the more complicated version of my fundamental explanation which I have expounded in my "General Theory." [1] My practical conclusions would have, in that case,

[1] Particularly in Chapter 2, which is the portion of my book which most needs to be revised.

à fortiori force. If we can advance farther on the road towards full employment than I had previously supposed without seriously affecting real hourly wages or the rate of profits per unit of output, the warnings of the anti-expansionists need cause us less anxiety.

Nevertheless, we should, I submit, hesitate somewhat and carry our inquiries further before we discard too much of our former conclusions which, subject to the right qualifications, have *à priori* support and have survived for many years the scrutiny of experience and common sense. I offer,[1] therefore, for further statistical investigation an analysis of the elements of the problem with a view to discovering at what points the weaknesses of the former argument emerge. There are five heads which deserve separate consideration.

I

First of all, are the statistics on which Mr. Dunlop and Mr. Tarshis are relying sufficiently accurate and sufficiently uniform in their indications to form the basis of a reliable induction?

For example, in so recent a compilation as the League of Nations "World Economic Survey 1937–38," prepared by Mr. J. E. Meade, the traditional conclusion receives support, not on *à priori* grounds, but on the basis of the most recently available statistics. I quote the following from pp. 54–55 :—

> During the great depression after 1929, the demand for goods and services diminished, and in consequence the price of commodities fell rapidly. In most countries, as can be seen from the graph on page 52, hourly money wages were reduced as the demand for labour fell; but in every case there was a greater fall in prices, so that hourly real wages rose. . . . (It is then explained that the same was not true of weekly wages.) . . . Since the recovery, the opposite movements may be observed. In most countries, increased demand for goods and services has caused commodity prices to rise more rapidly than hourly money wages, and the hourly real wage has fallen. . . . In the United States [2] and France,[3] however, the rise in money wages was so rapid between 1936 and 1937 that the hourly real wage continued to rise. . . . When real hourly wages are raised—*i.e.*, when the margin between commodity prices and the money-wage cost becomes less favourable—employers are likely to diminish the amount of employment which they offer to labour. While there were, no doubt, other influences

[1] In amplification of Mr. Dunlop's useful summary at the end of his article (*loc. cit.*, pp. 431–3).

[2] [Probably as a result of the New Deal.]

[3] Explained as being due to the forty-hour week.

> affecting the demand for labour, the importance of this factor is well illustrated by the graph on page 53. In the case of all the countries represented for which information is available, the fall in commodity prices between 1929 and 1932 caused a rise in the hourly real wage, and this was accompanied by a diminution in employment . . . (it is shown that on the recovery there has been a greater variety of experience). . . .

This authoritative study having international scope indicates that the new generalisations must be accepted with reserve. In any case Mr. Tarshis's scatter diagram printed below (p. 150), whilst it shows a definite preponderance in the south-west and north-east compartments and a high coefficient of association, includes a considerable number of divergent cases, and the absolute range of most of the scatter is extremely small, with a marked clustering in the neighbourhood of the zero line for changes in real wages; and much the same is true of Mr. Dunlop's results. The great majority of Mr. Tarshis's observations relate to changes of less than 1·5 per cent. In the introduction to his "Wages and Income in the United Kingdom since 1860," Prof. Bowley indicates that this is probably less than the margin of error for statistics of this kind. This general conclusion is reinforced by the fact that it is *hourly* wages which are relevant in the present context, for which accurate statistics are not available.[1] Moreover, in the *post-scriptum* to his note, Mr. Tarshis explains that whilst real wages tend to move in the same direction as money wages, they move in the opposite direction, though only slightly, to the level of output as measured by man-hours of employment; from which it appears that Mr. Tarshis's final result is in conformity with my original assumption, which is, of course, concerned with hourly wages. It seems possible, therefore, taking account of Mr. Meade's results, that I may not, after all, have been seriously wrong.

Furthermore, for reasons given below, it is important to separate the observations according as the absolute level of employment is distinctly good or only mediocre. It may be that we can analyse our results so as to give two distinct generalisations according to the absolute level reached by employment. If, at the present stage of the inquiry, we are to make any single statistical generalisation, I should prefer one to the effect that, for fluctuations within the range which has been usual in the periods investigated which seldom approach conditions of full employment, short-period

[1] It is possible that Mr. Meade has been more successful than Mr. Dunlop in using hourly wages, and that this explains some discrepancies in their conclusions.

changes in real wages are usually so small compared with the changes in other factors that we shall not often go far wrong if we treat real wages as substantially constant in the short period (a very helpful simplification if it is justified). The conclusion, that changes in real wages are not usually an important factor in short-period fluctuations until the point of full employment is approaching, is one which has been already reached by Dr. Kalecki on the basis of his own investigations.[1]

II

It may be that we have under-estimated the quantitative effect of a factor of which we have always been aware. Our argument assumed that, broadly speaking, labour is remunerated in terms of its own composite product, or at least that the price of wage-goods moves in the same way as the price of output as a whole. But no one has supposed that this was strictly the case or was better than an approximation; and it may be that the proportion of wage-goods, which are not the current product of the labour in question and the prices of which are not governed by the marginal cost of such product, is so great as to interfere with the reliability of our approximation. House-rent and goods imported on changing terms of trade are leading examples of this factor. If in the short period rents are constant and the terms of trade tend to improve when money wages rise and to deteriorate when money wages fall, our conclusion will be upset in practice in spite of the rest of our premisses holding good.

In the case of this country one has been in the habit of supposing that these two factors have in fact tended to offset one another, though the opposite might be the case in the raw-material countries. For whereas rents, being largely fixed, rise and fall less than money wages, the price of imported food-stuffs tends to rise more than money wages in periods of activity and to fall more in periods of depression. At any rate both Mr. Dunlop and Mr. Tarshis claim to show that fluctuations in the terms of trade (terms of foreign trade in Mr. Dunlop's British inquiry and terms of trade between industry and agriculture in Mr. Tarshis's American inquiry) are not sufficient to affect the general tendency of their results, though they clearly modify them quantitatively to a considerable extent.[2] Nevertheless, the effect

[1] "The Determinants of Distribution of the National Income" *Econometrica*, April 1938, p. 102, now reprinted in his "Essays in the Theory of Economic Fluctuations."

[2] Cf. Dunlop, *loc. cit.*, p. 417.

of expenditure on items such as rent, gas, electricity, water, transport, etc., of which the prices do not change materially in the short period, needs to be separately calculated before we can be clear. If it should emerge that it is this factor which explains the results, the rest of our fundamental generalisations would remain undisturbed. It is important, therefore, if we are to understand the situation, that the statisticians should endeavour to calculate wages in terms of the actual product of the labour in question.

III

Has the identification of marginal cost with marginal wage cost introduced a relevant error? In my "General Theory of Employment," chapter 6 (appendix), I have argued that this identification is dangerous in that it ignores a factor which I have called "marginal user cost." It is unlikely, however, that this can help us in the present context. For marginal user cost is likely to increase when output is increasing, so that this factor would work in the opposite direction from that required to explain our present problem, and would be an additional reason for expecting prices to rise more than wages. Indeed, one would, on general grounds, expect marginal total cost to increase more, and not less, than marginal wage cost.

IV

Is it the assumption of increasing marginal real cost in the short period which we ought to suspect? Mr. Tarshis finds part of the explanation here; and Dr. Kalecki is inclined to infer approximately constant marginal real cost.[1] But there is an important distinction which we have to make. We should all agree that if we start from a level of output very greatly below capacity, so that even the most efficient plant and labour are only partially employed, marginal real cost may be expected to decline with increasing output, or, at the worst, remain constant. But a point must surely come, long before plant and labour are fully employed, when less efficient plant and labour have to be brought into commission or the efficient organisation employed beyond the optimum degree of intensiveness. Even if one concedes that the course of the short-period marginal cost curve is downwards in its early reaches, Mr. Kahn's assumption that it eventually turns upwards is, on general common-sense grounds, surely beyond reasonable question; and that this happens, moreover, on a part of the curve which is highly relevant for practical

[1] *Loc. cit.*

purposes. Certainly it would require more convincing evidence than yet exists to persuade me to give up this presumption.

Nevertheless, it is of great practical importance that the statisticians should endeavour to determine at what level of employment and output the short-period marginal-cost curve for the composite product as a whole begins to turn upward and how sharply it rises after the turning-point has been reached. This knowledge is essential for the interpretation of the trade cycle. It is for this reason that I suggested above that the observations of the relative movement of real and money wages should be separately classified according to the average level of employment which had been reached.

It may prove, indeed, at any rate in the case of statistics relating to recent years, that the level of employment has been preponderantly so low that we have been living more often than not on the reaches of the curve before the critical point of upturn has been attained. It should be noticed that Mr. Tarshis's American figures relate only to the period from 1932 to 1938, during the whole of which period there has been such intense unemployment in the United States, both of labour and of plant, that it would be quite plausible to suppose that the critical point of the marginal cost curve had never been reached. If this has been the case, it is important that we should know it. But such an experience must not mislead us into supposing that this must necessarily be the case, or into forgetting the sharply different theory which becomes applicable after the turning-point has been reached.

If, indeed, the shape of the marginal-cost curve proves to be such that we tend to be living, with conditions as they are at present, more often to the left than to the right of its critical point, the practical case for a planned expansionist policy is considerably reinforced; for many *caveats* to which we must attend after this point has been reached can be, in that case, frequently neglected. In taking it as my general assumption that we are often on the right of the critical point, I have been taking the case in which the practical policy which I have advocated needs the most careful handling. In particular the warnings given, quite rightly, by Mr. D. H. Robertson of the dangers which may arise when we encourage or allow the activity of the system to advance too rapidly along the upward slopes of the marginal-cost curve towards the goal of full employment, can be more often neglected, for the time being at least, when the assumption which I have previously admitted as normal and reasonable is abandoned.

V

There remains the question whether the mistake lies in the approximate identification of marginal cost with price, or rather in the assumption that for output as a whole they bear a more or less proportionate relationship to one another irrespective of the intensity of output. For it may be the case that the practical workings of the laws of imperfect competition in the modern quasi-competitive system are such that, when output increases and money wages rise, prices rise less than in proportion to the increase in marginal money cost. It is scarcely likely, perhaps, that the narrowing gap could be sufficient to prevent a decline in real wages in a phase in which marginal real cost was increasing rapidly. But it might be sufficient to offset the effect on real wages of a modest rise in marginal real cost, and even to dominate the situation in the event of the marginal real cost curve proving to be almost horizontal over a substantial portion of its relevant length.

It is evidently possible that some such factor should exist. It might be, in a sense, merely an extension of the stickiness of prices of which we have already taken account in II above. Apart from those prices which are virtually constant in the short period, there are obviously many others which are, for various reasons, more or less sticky. But this factor would be particularly likely to emerge when output increases, in so far as producers are influenced in their practical price policies and in their exploitation of the opportunities given them by the imperfections of competition, by their long-period average cost, and are less attentive than economists to their short-period marginal cost. Indeed, it is rare for anyone but an economist to suppose that price is predominantly governed by marginal cost. Most business men are surprised by the suggestion that it is a close calculation of short-period marginal cost or of marginal revenue which should dominate their price policies. They maintain that such a policy would rapidly land in bankruptcy anyone who practised it. And if it is true that they are producing more often than not on a scale at which marginal cost is falling with an increase in output, they would clearly be right; for it would be only on rare occasions that they would be collecting anything whatever towards their overhead. It is, beyond doubt, the practical assumption of the producer that his price policy ought to be influenced by the fact that he is normally operating subject to decreasing average cost, even if in the short-period his marginal cost is rising. His effort is to maintain prices when output falls and, when output increases, he may raise them by less than the full amount required to offset

higher costs including higher wages. He would admit that this, regarded by him as the reasonable, prudent and far-sighted policy, goes by the board when, at the height of the boom, he is overwhelmed by more orders than he can supply; but even so he is filled with foreboding as to the ultimate consequences of his being forced so far from the right and reasonable policy of fixing his prices by reference to his long-period overhead as well as his current costs. Rightly ordered competition consists, in his opinion, in a proper pressure to secure an adjustment of prices to changes in long-period average cost; and the suggestion that he is becoming a dangerous and anti-social monopolist whenever, by open or tacit agreement with his competitors, he endeavours to prevent prices from following short-period marginal cost, however much this may fall away from long-period average cost, strikes him as disastrous. (It is the failure of the latest phase of the New Deal in the United States, in contrast to the earliest phase, of which the opposite is true, to distinguish between price agreements for maintaining prices in right relation to average long-period cost and those which aim at obtaining a monopolistic profit in excess of average long-period cost which strikes him as particularly unfair.)

Thus, since it is the avowed policy of industrialists to be content with a smaller gross profit per unit of output when output increases than when it declines, it is not unlikely that this policy may be, at least partially, operative. It would be of great interest if the statisticians could show in detail in what way gross profit per unit of output changes in different industries with a changing ratio between actual and capacity output. Such an investigation should distinguish, if possible, between the effect of increasing output on unit-profit and that of higher costs in the shape of higher money wages and other expenses. If it should appear that increasing output as such has a tendency to decrease unit-profit, it would follow that the policy suggested above is actual as well as professed. If, however, the decline in unit-profit appears to be mainly the result of a tendency of prices to offset higher costs incompletely, irrespective of changes in the level of output, then we have merely an example of the stickiness of prices arising out of the imperfection of competition intrinsic to the market conditions. Unfortunately it is often difficult or impossible to distinguish clearly between the effects of the two influences, since higher money costs and increasing output will generally go together.

A well-known statistical phenomenon which ought to have

put me on my guard confirms the probability of constant or diminishing, rather than increasing, profit per unit of output when output increases. I mean the stability of the proportion of the national dividend accruing to labour, irrespective apparently of the level of output as a whole and of the phase of the trade cycle. This is one of the most surprising, yet best-established, facts in the whole range of economic statistics, both for Great Britain and for the United States. The following figures summarise briefly what are, I believe, the undisputed facts [1]:—

Relative Share of Manual Labour in the National Income of Great Britain.[2]

1911	40·7	1924	43·0	1928	43·0	1932	43·0
		1925	40·8	1929	42·4	1933	42·7
		1926	42·0	1930	41·1	1934	42·0
		1927	43·0	1931	43·7	1935	41·8

Relative Share of Manual Labour in the National Income of U.S.A.[3]

1919	34·9	1923	39·3	1927	37·0	1931	34·9
1920	37·4	1924	37·6	1928	35·8	1932	36·0
1921	35·0	1925	37·1	1929	36·1	1933	37·2
1922	37·0	1926	36·7	1930	35·0	1934	35·8

The fluctuations in these figures from year to year appear to be of a random character, and certainly give no significant indications of any tendency to move against labour in years of increasing output. It is the stability of the ratio for each country which is chiefly remarkable, and this appears to be a long-run, and not merely a short-period, phenomenon.[4] Moreover, it would be interesting to discover whether the difference between the British and the American ratio is due to a discrepancy in the basis of reckoning adopted in the two sets of statistics or to a significant difference in the degrees of monopoly prevalent in the two countries or to technical conditions.

In any case, these facts do not support the recently prevailing assumptions as to the relative movements of real wages and

[1] The British figures are based on Mr. Colin Clark's "National Income and Outlay," and the American figures on Dr. King's "The National Income and its Purchasing Power, 1909–1928," and Dr. Kuznet's "National Income and Capital Formation, 1919–1935." But in both cases I have used the slightly adjusted version of the figures prepared by Dr. Kalecki and given by him in his "Essays in the Theory of Economic Fluctuations" pp. 16, 17.

[2] Shop assistants excluded.

[3] Shop assistants included.

[4] Dr. Bowley has given a figure of 41·4 for Great Britain in 1880. Dr. Kalecki tells me that, if this was adjusted so as to be comparable with the figures given above, it would be about 42·7—which would show an extraordinary stability for the ratio over a period of no less than fifty-five years during which almost everything else changed out of knowledge.

output, and are inconsistent with the idea of there being any marked tendency to increasing unit-profit with increasing output. Indeed, even in the light of the above considerations, the result remains a bit of a miracle. For even if price policies are such as to cause unit-profit to decrease in the same circumstances as those in which marginal real cost is increasing, why should the two quantities be so related that, regardless of other conditions, the movement of the one *almost exactly* offsets the movement of the other? I recently offered the problem of explaining this ἀπορία, as Edgeworth would have called it, to the research students at Cambridge. The only solution was offered by Dr. Kalecki in the brilliant article which has been published in *Econometrica*.[1] Dr. Kalecki here employs a highly original technique of analysis into the distributional problem between the factors of production in conditions of imperfect competition, which may prove to be an important piece of pioneer work. But the main upshot is what I have indicated above, and Dr. Kalecki makes, to the best of my understanding, no definite progress towards explaining why, when there is a change in the ratio of actual to capacity output, the corresponding changes in the degree of the imperfection of competition should *so exactly* offset other changes. Nor does he explain why the distribution of the product between capital and labour should be stable in the long run, beyond suggesting that changes of one kind always just serve to offset changes of another; yet it is very surprising that on balance there should have been a constant degree of monopoly over the last twenty years or longer. His own explanation is based on the assumptions that marginal real costs are constant, that the degree of the imperfection of the market changes in the opposite direction to output, but that this change is precisely offset by the fact that the prices of basic raw materials (purchased by the system from outside) relatively to money wages increase and decrease with output. Yet there is no obvious reason why these changes should so nearly offset one another; and it would seem safer not to assume that marginal real costs are constant, but to conclude that in actual fact, when output changes, the change in the degree of the imperfection of the market is such as to offset the combined effect of changes in marginal costs and of changes in the prices of materials bought from outside the system relatively to money wages. It may be noticed that Dr. Kalecki's argument assumes the existence of an opposite change in the degree of the imperfection of

[1] April, 1938, "The Determinants of Distribution of the National Income," and now reprinted in his book referred to above.

competition (or in the degree in which producers take advantage of it) when output increases from that expected by Mr. R. F. Harrod in his study on "The Trade Cycle." There Mr. Harrod expects an increase; here constancy or a decrease seems to be indicated. Since Mr. Harrod gives grounds for his conclusions which are *prima facie* plausible, this is a further reason for an attempt to put the issue to a more decisive statistical test.[1]

To state the case more exactly, we have five factors which fluctate in the short period with the level of output :—

(1) The price of wage-goods relatively to the price of the product;

(2) The price of goods bought from outside the system relatively to money wages;

(3) The marginal wage cost;

(4) The marginal user cost (I attach importance to including this factor because it helps to bridge the discontinuity between an increase of output up to short-period capacity and an increase of output involving an increase beyond the capacity assumed in short-period conditions); and

(5) The degree of the imperfection of competition.

And it appears that, for reasons which are not yet clear, these factors taken in conjunction have no significant influence on the distribution between labour and capital of the income resulting from the output. Whatever a more complete inquiry into the problem may bring forth, it is evident that Mr. Dunlop, Mr. Tarshis and Dr. Kalecki have given us much to think about, and have seriously shaken the fundamental assumptions on which the short-period theory of distribution has been based hitherto;—it seems that for practical purposes a different set of simplifications from those adopted hitherto are preferable. Meanwhile I am comforted by the fact that their conclusions tend to confirm the idea that the causes of short-period fluctuation are to be found in changes in the demand for labour, and not in changes in its real-supply price; though I complain a little that I in particular should be criticised for conceding a little to the other view by admitting that, when the changes in effective demand to which I myself attach importance have brought about a change in the level of output, the real-supply price for labour would in fact change in the direction assumed by the theory I am opposing—as if I was the

[1] Dr. Kalecki's conclusion is in conformity with Prof. Pigou's argument in "Industrial Fluctuations," Bk I, chap. xviii, where reasons are given for expecting more imperfection of competition in depressions.

first to have entertained the fifty-year-old generalisation that, trend eliminated, increasing output is usually associated with a falling real wage.

I urge, nevertheless, that we should not be too hasty in our revisions, and that further statistical enquiry is necessary before we have a firm foundation of fact on which to reconstruct our theory of the short period. In particular we need to know:

(i) How the real hourly wage changes in the short period, not merely in relation to the money wage, but in relation to the percentage which actual output bears to capacity output;

(ii) How the purchasing power of the industrial money wage in terms of its own product changes when output changes; and

(iii) How gross profit per unit of output changes (*a*) when money costs change and (*b*) when output changes.

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