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PROBLEM OF ACHIEVING AND MAINTAINING A STABLE PRICE LEVEL

ANALYTICAL ASPECTS OF ANTI-INFLATION POLICY

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I

Just as generals are said to be always fighting the wrong war, economists have been accused of fighting the wrong inflation. Thus, at the time of the 1946-48 rise in American prices, much attention was focused on the successive rounds of wage increases resulting from collective bargaining. Yet probably most economists are now agreed that this first postwar rise in prices was primarily attributable to the pull of demand that resulted from wartime accumulations of liquid assets and deferred needs.

This emphasis on demand-pull was somewhat reinforced by the Korean war run-up of prices after mid-1950. But just by the time that cost-push was becoming discredited as a theory of inflation, we ran into the rather puzzling phenomenon of the 1955-58 upward creep of prices, which seemed to take place in the last part of the period despite growing overcapacity, slack labor markets, slow real growth, and no apparent great buoyancy in over-all demand.

It is no wonder then that economists have been debating the possible causations involved in inflation: demand-pull versus cost-push; wage-push versus more general Lerner "seller's inflation"; and the new Charles Schultze theory of "demand-shift" inflation. We propose to give a brief survey of the issues. Rather than pronounce on the terribly difficult question as to exactly which is the best model to use in explaining the recent past and predicting the likely future, we shall try to emphasize the types of evidence which can help decide between the conflicting theories. And we shall be concerned with some policy implications that arise from the different analytical hypotheses.

History of the Debate: The Quantity Theory and Demand-Pull. The preclassical economists grew up in an environment of secularly rising prices. And even prior to Adam Smith there had grown up the belief in at least a simplified quantity theory. But it was in the neoclassical thought of Walras, Marshall, Fisher, and others that this special version of demand determination of the absolute level of money prices and costs reached its most developed form.

We can oversimplify the doctrine as follows. The real outputs, inputs, and relative prices of goods and factors can be thought of as determined by a set of competitive equations which are independent of the absolute level of prices. As in a barter system, the absolute level of all prices is indeterminate and inessential because of the "relative homogeneity" properties of these market relations. To fix the absolute scale factor, we can if we like bring in a neutral money. Such money, unlike coffee or soap, being valued only for what it will buy and not for its intrinsic utility, will be exactly doubled in demand if there is an exact doubling of all prices. Because of this important "scale homogeneity," fixing the total of such money will, when applied to our already determined real system of outputs, factors, and relative prices, fix the absolute level of all prices; and changes in the total of such money must necessarily correspond to new equilibria of absolute prices that have moved in exact proportion, with relative prices and all real magnitudes being quite unaffected.[1]

As Patinkin and others have shown, the above doctrines are rather oversimplified, for they do not fully analyze the intricacies involved in the demand for money; instead they ignore important (and predictable) changes in such proportionality coefficients as velocity of circulation. But by World War I, this particular, narrow version of demand-pull inflation had more or less triumphed. The wartime rise in prices was usually analyzed in terms of rises in the over-all money supply. And the postwar German inflation was understood by non-German economists in similar terms.

But not all economists ever agree on anything. Just as Tooke had eclectically explained the Napoleonic rise in prices partially in terms of the war-induced increase in tax, shipping, and other costs, so did Harold G. Moulton and others choose to attribute the World War I price rises to prior rises in cost of production. And it is not without significance that the great neoclassical Wicksell expressed in the last years of his life some misgivings over the usual version of wartime price movements, placing great emphasis on movements in money's velocity induced by wartime shortages of goods.

Of course, the neoclassical writers would not have denied the necessary equality of competitive costs and prices. But they would have regarded it as superficial to take the level of money costs as a predetermined variable. Instead, they would argue, prices and factor costs are

[1] But as Hume had early recognized, the periods of rising prices seemed to give rise to at least transient stimulus to the economy as active profit seekers gained an advantage at the expense of the more inert fixed-income, creditor, and wage sectors. The other side of this Hume thesis is perhaps exemplified by the fact that the post-Civil War decades of deflation were also periods of strong social unrest and of relatively weak booms and long periods of heavier-than-average depressions—as earlier National Bureau studies have suggested.

simultaneously determinable in interdependent competitive markets; and if the level of over-all money supply were kept sufficiently in check, then the price level could be stabilized, with any increases in real costs or any decreases in output being offset by enough backward pressure on factor prices so as to leave final money costs and prices on the average unchanged.

Many writers have gone erroneously beyond the above argument to untenable conclusions such as the following: A rise in defense expenditure matched by, say, excise taxes cannot raise the price level if the quantity of money is held constant; instead it must result in enough decrease in wage and other factor costs to offset exactly the rise in tax costs. Actually, however, such a fiscal policy change could be interpreted as a reduction in the combined public and private thriftiness; with M constant, it would tend to swell the volume of total spending, putting upward pressure on interest rates and inducing a rise in money velocity, and presumably resulting in a higher equilibrium level of prices. To roll back prices to their previous level would take, even within the framework of a strictly competitive neoclassical model, a determined reduction in previous money supply. (This illustrates the danger of going from the innocent hypothesis, that a balanced change in all prices might in the long run be consistent with no substantive changes in real relations, to an overly simple interpretation of a complicated change that is actually taking place in historical reality.)

While the above example of a tax-induced price rise that takes place within a strict neoclassical model might be termed a case of cost-push rather than demand-pull, it does not really represent quite the same phenomena that we shall meet in our later discussion of cost-push. This can perhaps be most easily seen from the remark that, if one insisted on holding prices steady, conventional demand reduction methods would work very well, within the neoclassical model, to offset such cost-push.

Demand-Pull à la Keynes. Aside from the neoclassical quantity theory, there is a second version of demand-pull associated with the theories of Keynes. Before and during the Great Depression, economists had become impressd with the institutional frictions and rigidities that made for downward inflexibilities in wages and prices and which made any such deflationary movements socially painful. Keynes's *General Theory* can, if we are willing to oversimplify, be thought of as a systematic model which uses downward inflexibility of wages and prices to convert any reduction in money spending into a real reduction in output and employment rather than a balanced reduction in all prices and factor costs. (This is overly simple for at least the following reasons: in the pessimistic, depression version of some Keynesians, a hyperdeflation of wages and prices would not have had substantive effects in re-

storing employment and output, because of infinite elasticity of liquidity preference and/or zero elasticity of investment demand; in the general form of the *General Theory,* and particularly after Pigou effects of the real value of money had been built in, if you could engineer a massive reduction in wages and costs, there would have been some stimulating effects on consumption, investment, and on real output; finally, a careful neoclassical theory, which took proper account of rigidities and which analyzed induced shifts of velocity in a sophisticated way, might also have emerged with similar valid conclusions.)

While the Keynesian theories can be said to differ from the neoclassical theories with respect to analysis of deflation, Keynes himself was willing to asume that attainment of full employment would make prices and wages flexible upward. In *How to Pay for the War* (1939), he developed a theory of inflation which was quite like the neoclassical theory in its emphasis upon the demand-pull of aggregate spending even though it differed from that theory in its emphasis on total spending flow rather than on the stock of money. His theory of "demanders' inflation" stemmed primarily from the fact that government plus investors plus consumers want, in real terms among them, more than 100 per cent of the wartime or boomtime available produceable output. So prices have to rise to cheat the slow-to-spend of their desired shares. But the price rise closes the inflationary gap only temporarily, as the higher price level breeds higher incomes all around and the real gap reopens itself continually. And so the inflation goes on, at a rate determined by the degree of shifts to profit, the rapidity and extent of wage adjustments to the rising cost of living, and ultimately by the extent to which progressive tax receipts rise enough to close the gap. And, we may add, that firmness by the central bank in limiting the money supply might ultimately so increase credit tightness and so lower real balances as to bring consumption and investment spending into equilibrium with available civilian resources at some higher plateau of prices.

Cost-Push and Demand-Shift Theories of Inflation. In its most rigid form, the neoclassical model would require that wages fall whenever there is unemployment of labor and that prices fall whenever excess capacity exists in the sense that marginal cost of the output that firms sell is less than the prices they receive. A more eclectic model of imperfect competition in the factor and commodity markets is needed to explain the fact of price and wage rises before full employment and full capacity have been reached.

Similarly, the Keynes model, which assumes stickiness of wages even in the face of underemployment equilibrium, rests on various assumptions of imperfect competition. And when we recognize that, considerably before full employment of labor and plants has been reached,

modern prices and wages seem to show a tendency to drift upward irreversibly, we see that the simple Keynesian system must be modified even further in the direction of an imperfect competition model.

Now the fact that an economic model in some degree involves imperfect competition does not necessarily imply that the concepts of competitive markets give little insight into the behavior of relative prices, resources allocations, and profitabilities. To some degree of approximation, the competitive model may cast light on these important real magnitudes, and for this purpose we might be content to use the competitive model. But to explain possible cost-push inflation, it would seem more economical from the very beginning to recognize that imperfect competition is the essence of the problem and to drop the perfect competition assumptions.

Once this is done, we recognize the qualitative possibility of cost-push inflation. Just as wages and prices may be sticky in the face of unemployment and overcapacity, so may they be pushing upward beyond what can be explained in terms of levels and shifts in demand. But to what degree these elements are important in explaining price behavior of any period becomes an important quantitative question. It is by no means always to be expected that by observing an economy's behavior over a given period will we be able to make a very good separation of its price rise into demand and cost elements. We simply cannot perform the controlled experiments necessary to make such a separation; and Mother Nature may not have economically given us the scatter and variation needed as a substitute for controlled experiments if we are to make approximate identification of the casual forces at work.

Many economists have argued that cost-push was important in the prosperous 1951-53 period, but that its effects on average prices were masked by the drop in flexible raw material prices. But again in 1955-58, it showed itself despite the fact that in a good deal of this period there seemed little evidence of over-all high employment and excess demand. Some holders of this view attribute the push to wage boosts engineered unilaterally by strong unions. But others give as much or more weight to the co-operative action of all sellers—organized and unorganized labor, semimonopsonistic managements, oligopolistic sellers in imperfect commodity markets—who raise prices and costs in an attempt by each to maintain or raise his share of national income, and who, among themselves, by trying to get more than 100 per cent of the available output, create "seller's inflation." →

A variant of cost-push is provided by Charles Schultze's "demand-shift" theory of inflation. Strength of demand in certain sectors of the economy—e.g., capital goods industries in 1955-57—raises prices and

wages there. But elsewhere, even though demand is not particularly strong, downward inflexibility keeps prices from falling, and market power may even engineer a price-wage movement imitative in a degree of the sectors with strong demand. The result is an upward drift in average prices—with the suggestion that monetary and fiscal policies restrictive enough to prevent an average price rise would have to be so very restrictive as to produce a considerable level of unemployment and a significant drop in production.

II

Truths and Consequences: The Problem of Identification. The competing (although imperfectly competing) theories of inflation appear to be genuinely different hypotheses about observable facts. In that case one ought to be able to distinguish empirically between cost and demand inflation. What are the earmarks? If I believe in cost-push, what should I expect to find in the facts that I would not expect to find were I a believer in demand-pull? The last clause is important. It will not do to point to circumstances which will accompany any inflation, however caused. A test must have what statisticians call power against the main alternative hypotheses.

Trite as these remarks may seem, they need to be made. The clichés of popular discussion fall into the trap again and again. Although they have been trampled often enough by experts, the errors revive. We will take the time to point the finger once more. We do this because we want to go one step further and argue that this problem of identification is exceedingly difficult. What appear at first to be subtle and reliable ways of distinguishing cost-induced from demand-induced inflation turn out to be far from airtight. In fact we are driven to the belief that aggregate data, recording the *ex post* details of completed transactions, may in most circumstances be quite insufficient. It may be necessary first to disaggregate.

Common Fallacies. The simplest mistake—to be found in almost any newspaper discussion of the subject—is the belief that if money wages rise faster than productivity, we have a sure sign of cost-inflation. Of course the truth is that in the purest of excess-demand inflation wages will rise faster than productivity; the only alternative is for the full increase in the value of a fixed output to be siphoned off into profits, without this spilling over into the labor market to drive wages up still further. This error is sometimes mixed with the belief that it is possible over long periods for industries with rapid productivity increase to pay higher and increasingly higher wages than those where output per man-hour grows slowly. Such a persistent and growing differential is likely eventually to alter the skill- or quality-mix of the labor force in

the different industries, which casts doubt on the original productivity comparison.

One sometimes sees statements to the effect that increases in expenditure more rapid than increases in real output necessarily spell demand inflation. It is simple arithmetic that expenditure outrunning output by itself spells only price increases and provides no evidence at all about the source or cause of the inflation. Much of the talk about "too much money chasing too few goods" is of this kind.

A more solemn version of the fallacy goes: An increase in expenditure can come about only through an increase in the stock of money or an increase in the velocity of circulation. Therefore the only possible causes of inflation are M and V and we need look no further.

Further Difficulties. It is more disconcerting to realize that even some of the empirical tests suggested in the professional literature may have little or no cutting power in distinguishing cost from demand inflation.

One thinks automatically of looking at the timing relationships. Do wage increases seem to precede price increases? Then the general rise in prices is caused by the wage-push. Do price increases seem to precede wage increases? Then more likely the inflation is of the excess-demand variety, and wages are being pulled up by a brisk demand for labor or they are responding to prior increases in the cost of living. There are at least three difficulties with this argument. The first is suggested by replacing "wage increase" by "chicken" and "price increase" by "egg." The trouble is that we have no normal initial standard from which to measure, no price level which has always existed and to which everyone has adjusted; so that a wage increase, if one occurs, must be autonomous and not a response to some prior change in the demand for labor. As an illustration of the difficulty of inference, consider average hourly earnings in the basic steel industry. They rose, relative to all manufacturing from 1950 on, including some periods when labor markets were not tight. Did this represent an autonomous wage-push? Or was it rather a delayed adjustment to the decline in steel wages relative to all manufacturing, which took place during the war, presumably as a consequence of the differential efficiency of wage control? And why should we take 1939 or 1941 as a standard for relative wages? And so on.

A related problem is that in a closely interdependent economy, effects can precede causes. Prices may begin to ease up because wage rates are expected to. And more important, as wage and price increases ripple through the economy, aggregation may easily distort the apparent timing relations.

But even if we could find the appearance of a controlled experiment, if after a period of stability in both we were to notice a wage increase

to a new plateau followed by a price increase, what could we safely conclude? It would be immensely tempting to make the obvious diagnosis of wage-push. But consider the following hypothetical chain of events: Prices in imperfect commodity markets respond only to changes in costs. Labor markets are perfectly competitive in effect, and the money wage moves rapidly in response to shifts in the demand for labor. So any burst of excess demand, government expenditure, say, would cause an increased demand for labor; wages would be pulled up; and only then would prices of commodities rise in response to the cost increase. So the obvious diagnosis might be wrong. In between, if we were clever, we might notice a temporary narrowing of margins, and with this information we might piece together the story.

Consider another sophisticated inference. In a single market, price may rise either because the demand curve shifts to the right or because the supply curve shifts to the left in consequence of cost increases. But in the first case, output should increase; in the second case, decline. Could we not reason, then, that if prices rise, sector by sector, with outputs, demand-pull must be at work? Very likely we can, but not with certainty. In the first place, as Schultze has argued, it is possible that certain sectors face excess demand, without there being aggregate pressure; those sectors will indeed show strong price increases and increases in output (or pressure on capacity). But in a real sense, the source of inflation is the failure of other sectors, in which excess capacity develops, to decrease their prices sufficiently. And this may be a consequence of "administered pricing," rigid markups, rigid wages and all the paraphernalia of the "new" inflation.

To go deeper, the reasoning we are scrutinizing may fail because it is illegitimate, even in this industry-by-industry way, to use partial equilibrium reasoning. Suppose wages rise. We are led to expect a decrease in output. But in the modern world, all or most wages are increasing. Nor is this the first time they have done so. And in the past, general wage and price increases have not resulted in any decrease in aggregate real demand—perhaps the contrary. So that even in a single industry supply and demand curves may not be independent. The shift in costs is accompanied by, indeed may bring about, a compensating shift in the subjectively-viewed demand curve facing the industry. And so prices may rise with no decline and possibly an increase in output. If there is anything in this line of thought, it may be that one of the important causes of inflation is—inflation.

The Need for Detail. In these last few paragraphs we have been arguing against the attempt to diagnose the source of inflation from aggregates. We have also suggested that sometimes the tell-tale symptoms can be discovered if we look not at the totals but at the parts. This

suggestion gains force when we recognize, as we must, that the same general price increase can easily be the consequence of different causes in different sectors. A monolithic theory may have its simplicity and style riddled by exceptions. Is there any reason, other than a desire for symmetry, for us to believe that the same reasoning must account for the above-average increase in the price of services and the above-average increase in the price of machinery since 1951 or since 1949? Public utility prices undoubtedly were held down during the war, by the regulatory process; and services ride along on income-elastic demand accompanied by a slower-than-average recorded productivity increase. A faster-than-average price increase amounts to the corrective relative-price change one would expect. The main factor in the machinery case, according to a recent Joint Economic Committee study, appears to have been a burst of excess demand occasioned by the investment boom of the mid-fifties. And to give still a third variant, Eckstein and Fromm in another Joint Economic Committee study suggest that the above-average rise in the wages of steelworkers and the prices of steel products took place in the face of a somewhat less tight labor and product market than in machinery. They attribute it to a joint exercise of market power by the union and the industry. Right or wrong, it is mistaken theoretical tactics to deny this possibility on the grounds that it cannot account for the price history in other sectors.

Some Things It Would Be Good to Know. There are at least two classical questions which are relevant to our problem and on which surprisingly little work has been done: One is the behavior of real demand under inflationary conditions and the other is the behavior of money wages with respect to the level of employment. We comment briefly on these two questions because there seems to us to be some doubt that ordinary reversible behavior equations can be found, and this very difficulty points up an important question we have mentioned earlier: that a period of high demand and rising prices molds attitudes, expectations, even institutions in such a way as to bias the future in favor of further inflation. Unlike some other economists, we do not draw the firm conclusion that unless a firm stop is put, the rate of price increase must accelerate. We leave it as an open question: It may be that creeping inflation leads only to creeping inflation.

The standard way for an inflationary gap to burn itself out short of hyperinflation is for the very process of inflation to reduce real demands. The mechanisms, some dubious, some not, are well known: the shift to profit, real-balance effects, tax progression, squeeze on fixed incomes. If price and wage increases have this effect, then a cost-push inflation in the absence of excess demand inflicts unemployment and excess capacity on the system. The willingness to bear the reduced real

demand is a measure of the imperfectness of markets permitting the cost-push. But suppose real demands do not behave in this way? Suppose a wage-price rise has no effect on real demand, or a negligible one, or even a slight positive one? Then not only will the infliction not materialize, but the whole distinction between cost-push and demand-pull begins to evaporate. But is this possible? The older quantity theorists would certainly have denied it; but the increase in velocity between 1955 and 1957 would have surprised an older quantity theorist.

We do not know whether real demand behaves this way or not. But we think it important to realize that the more the recent past is dominated by inflation, by high employment, and by the belief that both will continue, the more likely is it that the process of inflation will preserve or even increase real demand, or the more heavily the monetary and fiscal authorities may have to bear down on demand in the interests of price stabilization. Real-income consciousness is a powerful force. The pressure on real balances from high prices will be partly relieved by the expectation of rising prices, as long as interest rates in an imperfect capital market fail to keep pace. The same expectations will induce schoolteachers, pensioners, and others to try to devise institutions to protect their real incomes from erosion by higher prices. To the extent that they succeed, their real demands will be unimpaired. As the fear of prolonged unemployment disappears and the experience of past full employment builds up accumulated savings, wage earners may also maintain their real expenditures; and the same forces may substantially increase the marginal propensity to spend out of profits, including retained earnings. If there is anything to this line of thought, the empirical problem of verification may be very difficult, because much of the experience of the past is irrelevant to the hypothesis. But it would be good to know.

The Fundamental Phillips Schedule Relating Unemployment and Wage Changes. Consider also the question of the relation between money wage changes and the degree of unemployment. We have A. W. Phillips' interesting paper on the U. K. history since the Civil War (our Civil War, that is!). His findings are remarkable, even if one disagrees with his interpretations.

In the first place, the period 1861-1913, during which the trade-union movement was rather weak, shows a fairly close relationship between the per cent change in wage rates and the fraction of the labor force unemployed. Due allowance must be made for sharp import-price-induced changes in the cost of living, and for the normal expectation that wages will be rising faster when an unemployment rate of 5 per cent is reached on the upswing than when it is reached on the downswing. In the second place, with minor exceptions, the same relation-

ship that fits for 1861-1913 also seems to fit about as well for 1913-48 and 1948-57. And finally Phillips concludes that the money wage level would stabilize with 5 per cent unemployment; and the rate of increase of money wages would be held down to the 2-3 per cent rate of productivity increase with about 2½ per cent of the labor force unemployed.

Strangely enough, no comparably careful study has been made for the U.S. Garbarino's 1950 note is hardly a full-scale analysis, and Schultze's treatment in his first-class Joint Committee monograph is much too casual. There is some evidence that the U.S. differs from the U.K. on at least two counts. If there is any such relationship characterizing the American labor market, it may have shifted somewhat in the last fifty to sixty years. Secondly, there is a suggestion that in this country it might take 8 to 10 per cent unemployment to stabilize money wages.

But would it take 8 to 10 per cent unemployment forever to stabilize the money wage? Is not this kind of relationship also one which depends heavily on remembered experience? We suspect that this is another way in which a past characterized by rising prices, high employment, and mild, short recessions is likely to breed an inflationary bias—by making the money wage more rigid downward, maybe even perversely inclined to rise during recessions on the grounds that things will soon be different.

There may be no such relation for this country. If there is, why does it not seem to have the same degree of long-run invariance as Phillips' curve for the U.K.? What geographical, economic, sociological facts account for the difference between the two countries? Is there a difference in labor mobility in the two countries? Do the different tolerances for unemployment reflect differences in income level, union organization, or what? What policy decisions might conceivably lead to a decrease in the critical unemployment rate at which wages begin to rise or to rise too fast? Clearly a careful study of this problem might pay handsome dividends.

III

A Closer Look at the American Data. In spite of all its deficiencies, we think the accompanying scatter diagram in Figure 1 is useful. Where it does not provide answers, it at least asks interesting questions. We have plotted the yearly percentage changes of average hourly earnings in manufacturing, including supplements (Rees's data) against the annual average percentage of the labor force unemployed.

The first defect to note is the different coverages represented in the two axes. Duesenberry has argued that postwar wage increases in manufacturing on the one hand and in trade, services, etc., on the other, may have quite different explanations: union power in manufacturing and

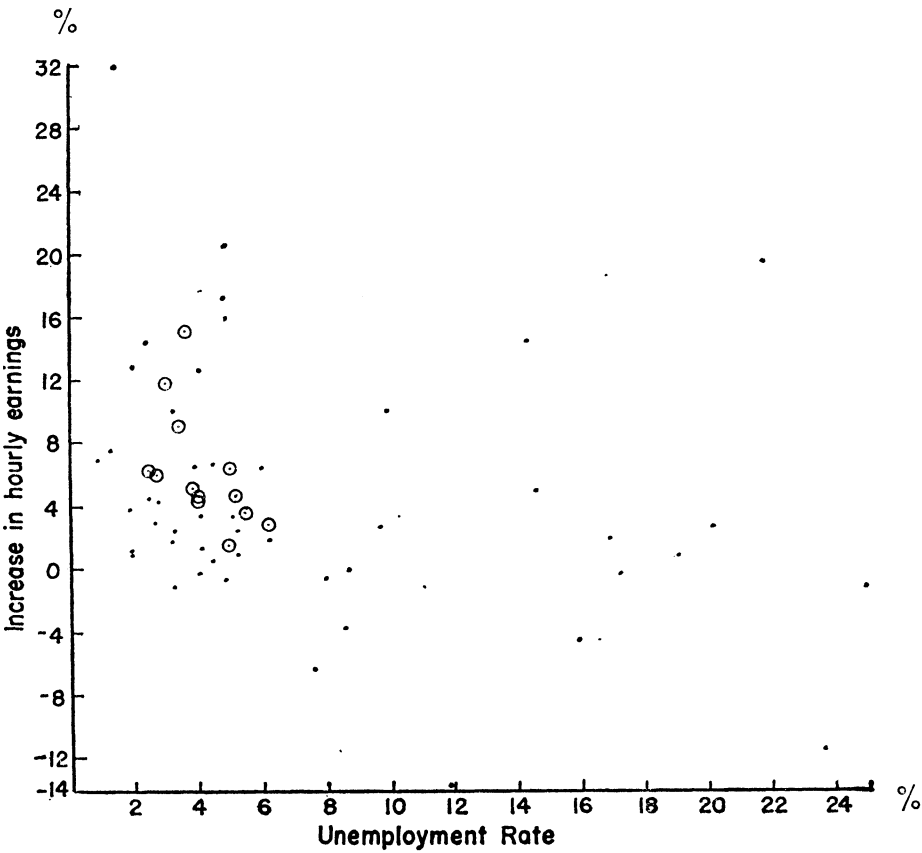


FIGURE 1

PHILLIPS SCATTER DIAGRAM FOR U.S.
(The circled points are for recent years.)

simple excess demand in the other sectors. It is probably true that if we had an unemployment rate for manufacturing alone, it would be somewhat higher during the postwar years than the aggregate figure shown. Even if a qualitative statement like this held true over the whole period, the increasing weight of services in the total might still create a bias. Another defect is our use of annual increments and averages, when a full-scale study would have to look carefully into the nuances of timing.

A first look at the scatter is discouraging; there are points all over the place. But perhaps one can notice some systematic effects. In the first place, the years from 1933 to 1941 appear to be *sui generis:* money wages rose or failed to fall in the face of massive unemployment. One may attribute this to the workings of the New Deal (the 20 per cent wage increase of 1934 must represent the NRA codes); or alternatively

one could argue that by 1933 much of the unemployment had become structural, insulated from the functioning labor market, so that in effect the vertical axis ought to be moved over to the right. This would leave something more like the normal pattern.

The early years of the first World War also behave atypically although not so much so as 1933-39. This may reflect cost-of-living increases, the rapidity of the increase in demand, a special tightness in manufacturing, or all three.

But the bulk of the observations—the period between the turn of the century and the first war, the decade between the end of that war and the Great Depression, and the most recent ten or twelve years—all show a rather consistent pattern. Wage rates do tend to rise when the labor market is tight, and the tighter the faster. What is most interesting is the strong suggestion that the relation, such as it is, has shifted upward slightly but noticeably in the forties and fifties. On the one hand, the first decade of the century and the twenties seem to fit the same pattern. Manufacturing wages seem to stabilize absolutely when 4 or 5 per cent of the labor force is unemployed; and wage increases equal to the productivity increase of 2 to 3 per cent per year is the normal pattern at about 3 per cent unemployment. This is not so terribly different from Phillips' results for the U.K., although the relation holds there with a greater consistency. We comment on this below.

On the other hand, from 1946 to the present, the pattern is fairly consistent and consistently different from the earlier period. The annual unemployment rate ranged only narrowly, from 2.5 per cent in 1953 to 6.2 per cent in 1958. Within that range, as might be expected, wages rose faster the lower the unemployment rate. But one would judge now that it would take more like 8 per cent unemployment to keep money wages from rising. And they would rise at 2 to 3 per cent per year with 5 or 6 per cent of the labor force unemployed.

It would be overhasty to conclude that the relation we have been discussing represents a reversible supply curve for labor along which an aggregate demand curve slides. If that were so, then movements along the curve might be dubbed standard demand-pull, and shifts of the curve might represent the institutional changes on which cost-push theories rest. The apparent shift in our Phillips' curve might be attributed by some economists to the new market power of trade-unions. Others might be more inclined to believe that the expectation of continued full employment, or at least high employment, is enough to explain both the shift in the supply curve, if it is that, and the willingness of employers (conscious that what they get from a work force is partly dependent on its morale and its turnover) to pay wage increases in periods of temporarily slack demand.

This latter consideration, however, casts real doubt on the facile identification of the relationship as merely a supply-of-labor phenomenon. There are two parties to a wage bargain.

U.S. and U.K. Compared. A comparison of the American position with Phillips' findings for the U.K. is interesting for itself and also as a possible guide to policy. Anything which will shift the relationship downward decreases the price in unemployment that must be paid when a policy is followed of holding down the rate of wage and price increase by pressure on aggregate demand.

One possibility is that the trade-union leadership is more "responsible" in the U.K.; indeed the postwar policy of wage restraint seems visible in Phillips' data. But there are other interpretations. It is clear that the more fractionated and imperfect a labor market is, the higher the over-all excess supply of labor may have to be before the average wage rate becomes stable and the less tight the relation will be in any case. Even a touch of downward inflexibility (and trade-unionism and administered wages surely means at least this) will make this immobility effect more pronounced. It would seem plausible that the sheer geographical compactness of the English economy makes its labor market more perfect than ours in this sense. Moreover, the British have pursued a more deliberate policy of relocation of industry to mop up pockets of structural unemployment.

This suggests that any governmental policy which increases the mobility of labor (geographical and industrial) or improves the flow of information in the labor market will have anti-inflationary effects as well as being desirable for other reasons. A quicker but in the long run probably less efficient approach might be for the government to direct the regional distribution of its expenditures more deliberately in terms of the existence of local unemployment and excess capacity.

The English data show a quite clearly nonlinear (hyperbolic) relation between wage changes and unemployment, reflecting the much discussed downward inflexibility. Our American figures do not contradict this, although they do not tell as plain a story as the English. To the extent that this nonlinearity exists, as Duesenberry has remarked, a given average level of unemployment over the cycle will be compatible with a slower rate of wage increase (and presumably price increase) the less wide the cyclical swings from top to bottom.

A less obvious implication of this point of view is that a deliberate low-pressure policy to stabilize the price level may have a certain self-defeating aspect. It is clear from experience that interregional and inter-industrial mobility of labor depends heavily on the pull of job opportunities elsewhere, more so than on the push of local unemployment. In effect the imperfection of the labor market is increased, with the consequences we have sketched.

IV

We have concluded that it is not possible on the basis of a priori reasoning to reject either the demand-pull or cost-push hypothesis, or the variants of the latter such as demand-shift. We have also argued that the empirical identifications needed to distinguish between these hypotheses may be quite impossible from the experience of macrodata that is available to us; and that, while use of microdata might throw additional light on the problem, even here identification is fraught with difficulties and ambiguities.

Nevertheless, there is one area where policy interest and the desire for scientific understanding for its own sake come together. If by deliberate policy one engineered a sizable reduction of demand or refused to permit the increase in demand that would be needed to preserve high employment, one would have an experiment that could hope to distinguish between the validity of the demand-pull and the cost-push theory as we would operationally reformulate those theories. If a small relaxation of demand were followed by great moderations in the march of wages and other costs so that the social cost of a stable price index turned out to be very small in terms of sacrificed high-level employment and output, then the demand-pull hypothesis would have received its most important confirmation. On the other hand, if mild demand repression checked cost and price increases not at all or only mildly, so that considerable unemployment would have to be engineered before the price level updrift could be prevented, then the cost-push hypothesis would have received its most important confirmation. If the outcome of this experience turned out to be in between these extreme cases—as we ourselves would rather expect—then an element of validity would have to be conceded to both views; and dull as it is to have to embrace eclectic theories, scholars who wished to be realistic would have to steel themselves to doing so.

Of course, we have been talking glibly of a vast experiment. Actually such an operation would be fraught with implications for social welfare. Naturally, since they are confident that it would be a success, the believers in demand-pull ought to welcome such an experiment. But, equally naturally, the believers in cost-push would be dead set against such an engineered low-pressure economy, since they are equally convinced that it will be a dismal failure involving much needless social pain. (A third school, who believes in cost-push but think it can be cured or minimized by orthodox depressing of demand, think that our failure to make this experiment would be fraught with social evil by virtue of the fact that they expect a creep in prices to snowball into a trot and then a gallop.)

Our own view will by now have become evident. When we translate the Phillips' diagram showing the American pattern of wage increase

against degree of unemployment into a related diagram showing the different levels of unemployment that would be "needed" for each degree of price level change, we come out with guesses like the following:

1. In order to have wages increase at no more than the 2½ per cent per annum characteristic of our productivity growth, the American economy would seem on the basis of twentieth-century and postwar experience to have to undergo something like 5 to 6 per cent of the civilian labor force's being unemployed. That much unemployment would appear to be the cost of price stability in the years immediately ahead.

2. In order to achieve the nonperfectionist's goal of high enough output to give us no more than 3 per cent unemployment, the price index might have to rise by as much as 4 to 5 per cent per year. That much price rise would seem to be the necessary cost of high employment and production in the years immediately ahead.

All this is shown in our price-level modification of the Phillips curve, Figure 2. The point *A*, corresponding to price stability, is seen to involve about 5½ per cent unemployment; whereas the point *B*, corre-

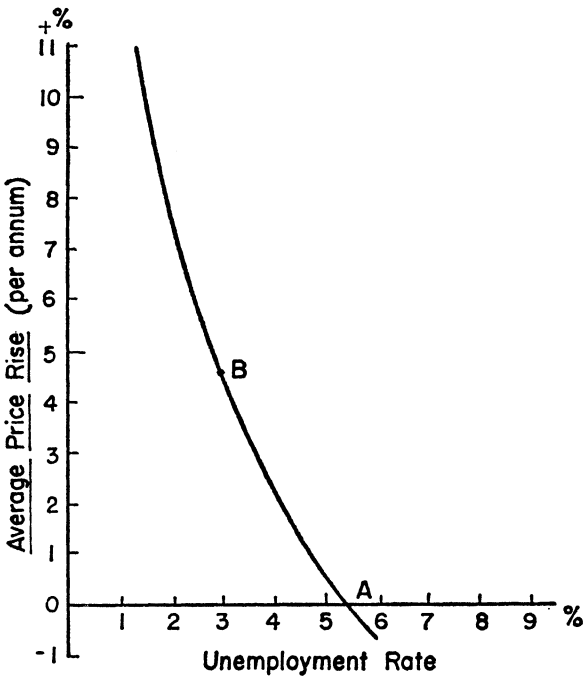


FIGURE 2

MODIFIED PHILLIPS CURVE FOR U.S.
This shows the menu of choice between different degrees of unemployment and price stability, as roughly estimated from last twenty-five years of American data.

sponding to 3 per cent unemployment, is seen to involve a price rise of about 4½ per cent per annum. We rather expect that the tug of war of politics will end us up in the next few years somewhere in between these selected points. We shall probably have some price rise and some excess unemployment.

Aside from the usual warning that these are simply our best guesses we must give another caution. All of our discussion has been phrased in short-run terms, dealing with what might happen in the next few years. It would be wrong, though, to think that our Figure 2 menu that relates obtainable price and unemployment behavior will maintain its same shape in the longer run. What we do in a policy way during the next few years might cause it to shift in a definite way.

Thus, it is conceivable that after they had produced a low-pressure economy, the believers in demand-pull might be disappointed in the short run; i.e., prices might continue to rise even though unemployment was considerable. Nevertheless, it might be that the low-pressure demand would so act upon wage and other expectations as to shift the curve downward in the longer run—so that over a decade, the economy might enjoy higher employment with price stability than our present-day estimate would indicate.

But also the opposite is conceivable. A low-pressure economy might build up within itself over the years larger and larger amounts of structural unemployment (the reverse of what happened from 1941 to 1953 as a result of strong war and postwar demands). The result would be an upward shift of our menu of choice, with more and more unemployment being needed just to keep prices stable.

Since we have no conclusive or suggestive evidence on these conflicting issues, we shall not attempt to give judgment on them. Instead we venture the reminder that, in the years just ahead, the level of attained growth will be highly correlated with the degree of full employment and high-capacity output.

But what about the longer run? If the per annum rate of technical progress were about the same in a low- and high-pressure economy, then the initial loss in output in going to the low-pressure state would never be made up; however, in relative terms, the initial gap would not grow but would remain constant as time goes by. If a low-pressure economy could succeed in improving the efficiency of our productive factors, some of the loss of growth might be gradually made up and could in long enough time even be more than wiped out. On the other hand, if such an economy produced class warfare and social conflict and depressed the level of research and technical progress, the loss in growth would be compounded in the long run.

A final disclaimer is in order. We have not here entered upon the

important question of what feasible institutional reforms might be introduced to lessen the degree of disharmony between full employment and price stability. These could of course involve such wide-ranging issues as direct price and wage controls, antiunion and antitrust legislation, and a host of other measures hopefully designed to move the American Phillips' curves downward and to the left.